Chapter 6

The Electoral Payoff of Antipoverty Programs

[Those of the PRI] “are giving the construction material because of the governors’ campaign. That doesn’t matter, we need it, and when the candidate comes we will put banners to show our support and that we are thankful. He promised the material and here it is”, says Petra. “By contrast, those of the PAN gave very little, just 15 bags of cement; with that one can’t do anything. And those of the PRD, poor devils, they give nothing”

Quoted in Masiosare supplement of La Jornada, February 12 2006.

What is the payoff of electoral investments in antipoverty programs? Do voters respond more favorably to transfers that are excludable and particularistic or to public goods and social infrastructure projects? Do clientelistic antipoverty programs generate more votes for incumbent parties than non-clientelistic ones? This chapter answers this questions by analyzing the vote-buying potential of the various antipoverty programs we have analyzed thus far. We build on Wantchekon (2004), who found that voters in Benin responded more favorable to campaigns framed in terms of private goods rather than collective benefit offers. Unlike that study we are in a position to assess how voters respond to actual policies, not just campaign promises.¹

These questions are highly relevant for theorizing about distributive politics. Only by understanding how various types of government transfers shape voting behavior will we be able to truly comprehend why politicians behave the way they do. Our theory in

¹ Although we do not have an experimental setup, we are able to use techniques for observational data that mitigate selection bias effects.
chapter 3 claims that private goods are low-risk investments, while public goods, given their non-excludability, involve high risks on vote yields. A second dimension is related to the institutional design of antipoverty programs –some are discretional in allocation and duration, while others are targeted according to objective criteria. We have claimed that discretion is a critical feature for politicians to establish clientelistic linkages. Programs that employ more objective criteria, following technical and legal procedures, should cancel the possibility of manipulation in the selection of beneficiaries. And since non-discretional programs can’t be capriciously withdrawn, they should presumably liberate beneficiaries to vote their conscience, which can lead them to retrospectively reward parties they credit for their welfare benefits or to support a different political party prospectively.

We analyze the electoral effects of various antipoverty programs in Mexico during the last twenty years. In doing so, we are better prepared to measure the vote buying potential of various forms of government transfers –public versus private goods, delivered through clientelistic versus non-clientelistic programs. Our study of the electoral payoffs of these programs employs both aggregate municipal-level electoral returns and individual-level survey evidence. Since we are interested in understanding the actual electoral yield of these programs, we employ exit polls for the 2000 and 2006 elections presidential elections.\(^2\) For our purposes, exit polls are better than post-electoral surveys, which in Mexico are normally characterized by huge over reporting in favor of the winning candidate, as well as implausible turnout. Pre-electoral surveys, for their part, tend to under represent the winner, because they do not capture last moment

\(^2\) We did not locate any exit poll including questions of program beneficiaries for 1994. We analyzed the Los Angeles Times post-electoral poll for the 1991 congressional election and a Gallup poll for 1993 (results are available upon request).
electoral shits. We supplement our survey research with the systematic analysis of aggregate vote swings in the 1994, 2000 and 2006 presidential elections. Aggregate electoral data becomes necessary to study the electoral payoff of public goods, which benefits are diffused and often not captured in conventional surveys.

1. The 1994, 2000, and 2006 Presidential Elections and Social Programs

The three elections we analyze in this chapter were all dramatic events. The 1994 election was a relatively clean election where the hegemonic party was able to retain power in spite of deep political trouble. The 2000 election marked the watershed of the Mexican transition to democracy with the defeat of the PRI by Vicente Fox. The controversial 2006 election was the closest race in the country’s history, pitting the two parties that had contested PRI hegemony since 1988.

The 1994 Mexican presidential elections took place in a year of significant political turmoil, marked by both the Zapatista uprising in the Southern state of Chiapas that erupted in December of 1993, and the assassination of the PRI’s presidential candidate, Luis Donaldo Colosio, in March of 1994. After the assassination, the president was compelled to improvise in the selection of his successor. He chose Ernesto Zedillo, a technocrat trained in economics.

Initially analysts thought that these events would jeopardize the peaceful transmission of presidential power, which had routinely taken place in Mexico for over 60 years, since the creation of the PRI. For the purpose of this book the Zapatista uprising is particularly noteworthy, since it happened despite Pronasol, which was largely publicized throughout the Salinas’ years as the definitive antipoverty strategy that would
eradicate poverty from Mexico’s map during one sexenio. The Zapatistas made it clear that poverty relief had not reached vast segments of the destitute. Despite massive investments in public good provision, the poorest places in Mexico continued to be condemned to isolation: rural citizens lacked access to health services and their children continued to die of preventable diseases; most of their communities lacked drinking water, electrification, and roads; and they could not send their children to school. The Pronasol program had put in place roads, electrification, health clinics, and drinking water, but these benefits did not reach the poorest.

The 1994 presidential election was also special in that it took place after six years of profound structural transformation. Mexico had joined the North American Free Trade Agreement and the country was more fully integrated into international capital markets. Some political commentators and analysts interpreted these elections as a referendum on economic performance. At the time, it seemed that macroeconomic stabilization had been successful, and domestic consumption was booming. The economy, however, was barely growing. Despite the Chiapas uprising, the Colosio assassination, and the prevailing discontent with the national economy, the electorate reelected the PRI in 1994 by a large margin –the party won 50% of the national vote over 27% that was given to the PAN’s candidate, Diego Fernández de Ceballos. This chapter uncovers the marginal contribution of Pronasol to the PRI’s victory in 1994.

Ernesto Zedillo initiated his presidency with the Tequila Crisis, which sent Mexico into a deep albeit short recession. Poverty reached unprecedented and alarming levels in the first two years of his presidency –according the estimates shown in chapter

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3 Insert cite by Salinas claiming he did more in six years than others in decades...
4 Contrary to these views, Magaloni (2006) finds that the electorate reelected the PRI in 1994 despite holding highly negative retrospective assessments of the national economy.
1, 81% of the rural citizens and 62% of the urban dwellers could not meet “basic capacities” (fulfill a minimal nutrition requirement plus health and education for their children). To a large extent as a result of the economic recession, the PRI began to lose in one local election after another and it also lost the majority in the Chamber of Deputies in the 1997 mid-term elections. Legislative politics were transformed in that for the first time in its history, the PRI was forced to compromise with the opposition to pass legislation. The establishment in 1997 of *Progresa*, as we discussed in the previous chapter, represented a turning point in the design of social policy (Levy, 2006; Levy and Rodríguez, 2004). With *Progresa* Mexico witnessed the advent of social entitlements for its poorest citizens. During the Zedillo administration social infrastructure funds were also decentralized and distribution of these funds was made through a poverty-based formula. By the 2000 presidential elections, the PRI had given up most of its discretion in the manipulation of poverty programs.

The 2000 presidential elections were a watershed in Mexico’s history because the PRI lost the presidency. The winner was the PAN candidate, Vicente Fox, who obtained 38% of the vote over 28% given to Francisco Labastida, of the PRI. The PRD again came in third, far behind the top parties. Despite outstanding growth rates prior to the presidential elections and high approval rates, the PRI could not win the elections because voters were fed up—they had completely lost confidence in the PRI and perceived that, if reelected, the party would escort the country into another post-electoral recession (Magaloni and Poiré, 2004; Magaloni, 2006). The elections were administered by a highly sophisticated and independent Federal Electoral Institute (IFE), which made electoral fraud practically impossible for the PRI. The average Mexican voter was
euphoric for unseating the PRI. However, the mood must have been different for the poor, and especially for those benefiting from the various government programs such as Procampo and Progresa. The question for the poor was whether the incoming administration of the right-wing PAN would preserve these programs.

After numerous international policy evaluations supporting the effectiveness of Progresa in reducing extreme poverty, the Fox administration decided to continue with the program, but re-baptized it with a new name, Oportunidades, and greatly expanded its rural coverage program while extending it to the cities as well. As we showed in chapter 1, coverage is impressive. At the end of 1999 Progresa reached approximately 2.6 million families, about 40% of all rural households. By the end of 2005, coverage under Oportunidades had doubled to almost 5 million families, two thirds of which are rural households, with the remainder comprising urban and semi-urban ones. Thus, by 2006 more than half of all families living under the poverty line in Mexico were recipients of these transfers.

Figure 6.1 maps the expansion of the programs’ coverage between 2000 and 2006 in each municipality. The map also provides the Ministry of Social Development’s imputation of nutritional poverty (i.e. a poverty line drawn at the minimum caloric intake necessary for survival) at the municipal level. Recall for chapter 2 that the size of each dot represents the absolute number of poor households calculated for each municipality. The program’s expansion incorporated poor households in urban areas. It also engaged in a so-called “densification” in the rural areas (i.e., covering in full very poor communities.
where only a fraction of the inhabitants were covered by Progresa).

**Figure 6.1**

In addition, the Fox administration introduced a new social insurance program to remedy the truncated nature of health care delivery in the country, which grants access only to those working in the formal sector of the economy. According to the 2000 census, some 58 percent of the population was not covered by the social security system. Thus, *Seguro Popular* was an ambitious program created to extend health coverage to the uninsured. The program began in five states in 2001 and by 2005 it had been implemented in all 31 states and the Federal District, covering almost 3 million families. In contrast to *Oportunidades*, which is centrally administered by the federal government,
The 2006 elections were the most contested elections in Mexico’s post-revolutionary history. The front runners were Felipe Calderón from the PAN and Andrés Manuel López Obrador, from the PRD. The margin of victory, after a recount of around a tenth of the votes, was slightly more than half a percentage point. Many interpret the close contest as the outcome of an ideological battle between two radically different visions that divided left from right, rich from poor and North from South. From this perspective, the 2006 election was a prospective exercise, with a clear choice between right-wing continuity and left-wing populism. This chapter will show that the PAN’s triumph would not have materialized without the support of ample sectors of the poor that voted for the right-wing party retrospectively as a result of Oportunidades.6

The PRD did not recognize its defeat, claiming that the election had been rigged. López Obrador based his accusations of electoral fraud on two main arguments. The first was that the computer system had been rigged: the preliminary vote count (PREP) had

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5 He further argues that the piggy-backing of Seguro Popular onto the poverty relief program, allowing for automatic affiliation of families already registered in Oportunidades, can only improve its targeting efficiency for the poor.

6 The Seguro Popular also played a decisive role, as we show elsewhere (Diaz-Cayeros, Estévez, and Magaloni, 2006).
inconsistencies; and in the night of the definitive count, the system showed him with a solid lead, until the results turned around in the last hours. He demanded that each and every vote be recounted (according to the law, the electoral boxes where his own party representatives testified the fairness of the election, by signing the legal documents, should not be opened for a recount). His second accusation was that, throughout the campaign, the PAN had manipulated social transfers to the poor in order to buy their electoral support.\footnote{7}

The accusation that fraud was orchestrated by the PAN in an electronic form by subtracting a few votes from the PRD in each voting booth (casilla) is untenable. Although some scholars detected “anomalies” in the vote results (Mochán, 2006; Mebane, 2006), the election was transparent.\footnote{8} There were thousands of electoral observers monitoring the election. Citizens and parties could verify the vote counts reported in the preliminary vote count system and the large signs placed outside the polling stations. Furthermore, the IFE digitalized all the official documents of the vote count, showing the signatures of party representatives accepting the results in each of the more than 300,000 booths.\footnote{9} Although there were some differences between the preliminary vote counts of the PREP, those of the official count, and those of the recounted booths, these differences were mostly due to random errors.

\footnote{7} A third accusation was related to the IFE being too lenient on the PAN, allowing it to broadcast nasty negative advertising.

\footnote{8} See Diaz-Cayeros’ webpage link (http://www.stanford.edu/~albertod/elections.html) where he discusses allegations of fraud and makes some econometric estimates assessing them as relatively implausible.

But the accusation that the PAN used its access to patronage, pork and clientelism to buy off electoral support, particularly from the poor, is a different matter. It is conceivable that from its position in power, the PAN used the vast resources available to it, following the old practices of the PRI, in order to coerce or bribe poor voters. In particular, Alianza Cívica, one of the most important NGOs carrying out electoral observation, had been warning for months that the PAN was using federal social programs as a currency to buy votes (Alianza Cívica, 2006). This perception was seconded by Fundar, an NGO for citizen oversight in budgetary matters (Fundar, 2006). The accusations could not be taken lightly, to the extent that they came from credible sources. Alianza Cívica must be credited for having created the most important network of electoral observers in Mexico, so vital for the transition to democracy. And Fundar played a key role in improving the transparency and accountability in the federal budgetary process.

President Vicente Fox and the Social Development Ministry (Sedesol) had anticipated these types of accusations, and as a result engaged in a strategy of what was called “Blindaje Electoral”, namely providing safeguards to shield the federal social programs from electoral manipulation (Sedesol, 2005). Some of these safeguards included the prohibition to expand the program’s coverage during a federal election year. Furthermore, Sedesol came to an agreement with the United Nations Development Program (UNDP) to make a thorough study of the way in which social programs worked during the election year, including a survey of beneficiaries and non-beneficiaries of
social programs, in order to assess whether coercion or vote buying through social programs occurred (PNUD, 2007).\textsuperscript{10}

Our position on this debate is that Oportunidades made the PAN’s victory in 2006 possible, but that voters responded to the program freely. Vote-buying and credit-claiming are complementary facets of democratic politics. However, their scope is quite wide, ranging from voters responding freely to programmatic appeals and the benefits that derive from welfare programs and entitlements, to voters supporting a party out of fear of losing their benefits, as in clientelist manipulation (Kitschelt and Wilkinson, 2005). The key difference between these two modes of electoral exchange lies in the implicit threats involved in clientelism, to a large extent made possible, we claim, by the institutional design of a given program.

Oportunidades made an explicit effort at publicizing the principle that benefits could not be made contingent on voting behavior. Every single transaction resulting from the program had the legend quoted at the beginning of chapter 5, which explicitly said that participation in Oportunidades and receipt of benefits were not subject to affiliation with any party and that no candidate running for office could withhold the benefits. Beneficiaries also were reminded that they would receive support if they showed up at their doctor’s visits and kept their children at school. However, in a country with such a long tradition of clientelism, we can not know whether beneficiaries actually believed these statements. Our intuition is that it will take some learning on the part of poor voters in Mexico to feel truly secure of their entitlements. The fact that Vicente Fox did not reverse Progresa after the PRI’s defeat in 2000 must have worked to assure the poor that their welfare benefits were no longer subject to the short-term whims of politicians.

\textsuperscript{10} One of the authors was commissioned to carry out part of the evaluation.

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3. Types of antipoverty programs and their payoffs

Recall from chapter 1 that the Mexican social programs can be categorized according to the degree of discretion and the targeting of benefits. Discretionary transfers of private and club goods are what we have called clientelism. About a third of Pronasol from 1989 to 1994 falls in this category, although the program became increasingly clientelistic through the years—in 1994, almost 40% of the programs’ transfers were private. Discretionary transfers of public goods, when delimited by geographical targeting, are commonly classified as pork or pork-barreling, the larger remainder of Pronasol. Formula-based distribution of private goods is the third combination, which includes Progresa/Oportunidades, starting in 1997. The last mix is formula-based allocation of public goods, which is represented by the FISM program enacted in 1995. We can fit these Mexican programs in a two by two table (figure 6.2) according to their institutional design (discretionary vs. non-discretionary) and the publicness of the transfers involved (private goods vs. public goods).
Our main theoretical expectation is that private goods should exhibit larger electoral returns than public goods, which returns are more risky given their non-excludability. Discretionary programs should also exhibit larger payoffs than formula-based ones because transfers can be given following political priorities. Our intuition is that clientelistic transfers should exhibit highest electoral returns given that they are private and discretionary. Entitlements, in our view, should follow clientelism in terms of their electoral payoffs. Pork-barreling projects should produce medium to low electoral returns. Although pork-barreling projects are discretionary, their benefits are diffuse, which means that they are subject to high voter opportunism. Lastly, formula-based infrastructure projects should produce lowest electoral pay off.
This section employs aggregate vote returns to study the electoral payoffs of these antipoverty programs. Surveys are well suited to study Progresa and Oportunidades but not for Pronasol and FISM. Public good provision benefits are not necessarily captured by polls, which ask whether the respondent benefits from a particular program. Some individuals might classify themselves as beneficiaries of Pronasol, for example, only when they or their close family members received a private transfer, failing to account for public good provision taking place in the locality where they live. Others might respond they are beneficiaries of public goods only when they directly use them—e.g., when they go to the health clinic. But most respondents are likely to fail to report benefiting from public goods because the benefits are diffuse. A survey instrument can be designed seeking to distinguish private and public good benefits, but it is very hard to do this in an exit poll like the ones we use in this chapter.11

A second reason why is hard to study distributive politics only with surveys is that it is practically impossible to obtain some reliable measure of quantities—that is, to rank order beneficiaries according to the pesos per capita they received. We are interested in measuring the electoral yield of pesos per capita investments in public and private goods and this is hard to do using surveys.

Finally, it is often difficult to study clientelistic practices through survey research only because individuals might fail to report that they did receive benefits, when these benefits are perceived to be illegal or to result from corrupt practices from both sides of

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11 The President’s office during the Pronasol years tried to design surveys asking respondents whether they knew that projects were being provided in their community, whether they or their friends had actually witnessed the projects being built and whether they had directly benefited from them. Unfortunately, the overriding concern in the design of those surveys was to measure the image of the president and the effectiveness of the publicity campaigns on the success of the program. Those long questionnaires were not carried out at times of electoral activity, but during the so-called Solidarity Weeks. These surveys are deposited at the Roper center (MXOTAP1992-SOL10992 and MXOTAP1993-SOL10993).
the contract. Specifically for the comparison of programs we seek to make, individuals in surveys might be more willing to respond that they benefited from Progresa and Oportunidades than from Pronasol, creating a problem of underreporting for the early program.

In a survey explicitly designed to assess the public opinion regarding the Solidaridad program in 1992 (MXOTAP1992-SOL10992), 60 percent of respondents who knew about the program reported having seen Pronasol projects. 47 percent said Pronasol projects were carried out in their own neighborhood. However, only 10.3 percent of the respondents reported direct participation in the Solidaridad committees. According to a Los Angeles Times poll carried out by Belden-Rusonello in 1991, 32 percent of the respondents claimed to have benefited from Pronasol (MXLAT1991-258). A Gallup-IMPO poll that same year (MXUSIA1993-I93057) had 19.4 percent reporting having received benefits. In short, it is very unlikely that one can reconstruct through survey data the extent of beneficiaries from Pronasol.12

The main methodological problem we may encounter in our study of the electoral payoffs of antipoverty programs, as discussed in chapter 2, is selection bias. Individuals’ selection into an antipoverty program is non-random but based on certain socio-demographic, geographic, and political traits. These selection criteria might be casually correlated with the object of our inquiry, namely voting decisions. Thus, for example, selection into these programs is given both by the individual’s income and by his or her community’s deprivation, which in turn shape political behavior, including turnout, voting decisions, and the propensity of voters to make retrospective evaluations over

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12 The President’s office surveys filtered respondents: we do not know the attitude of respondents who reported not knowing about the program.
prospective ones. Poor individuals in Mexico have tended to disproportionately vote for
the PRI and to base their voting decisions on pocketbook evaluations.

To model the electoral effects of these programs, we study vote swings between
PAN) through instrumental variables. Using vote swings between elections takes care of
one of the most serious potential problem arising from omitted variable bias: to the extent
that an independent variable remains constant between the elections, its omission should
not bias the results. Following Tucker (2006) we calculate all the electoral data in log-
odds ratios, in order to keep estimates between the plausible range of 0 and 1 and to
ensure normality in the distributions.

We test whether those vote swings can be attributed to the per capita allocations
of social programs and the municipal-level coverage of the various social assistance
programs. We use instrumental variables to solve problems of selection bias. The
challenge of this approach is to find strong instruments that are good predictors of
municipal-level per capita expenditures in the programs, but that are not causally
correlated with vote swings. Chapter 2 showed that our geographic variables are excellent
instruments because they are powerfully correlated with poverty (and per capita
antipoverty expenditure), yet there is no reason to believe that these variables are causally
related to vote swings.\textsuperscript{13} We test for over-identification of our instruments. The
regressions use state fixed effects to control for state-level patterns of vote swings
stemming from, for example, the partisan label of the incumbent governor, a state’s vote

\textsuperscript{13} An additional advantage of using swings as the dependent variable is that this enables us to use
instruments that although highly correlated with the level of vote support are not correlated with the change
from election to election. Thus, if one were to estimate a model of the level of PRI support, most
geographical variables would not pass an over-identification test because they are highly correlated with
poverty, and PRI support was higher in poorer regions.
mobilization capacity, the concurrency of gubernatorial races, or types of local party organizations.

We model vote swings for 1988-1994 as the product of Pronasol’s instrumented per capital investments in private goods and public goods logged ($Private PC^*$ and $Public PC^*$).\textsuperscript{14} Our expectation is that the return of private goods should be significantly higher than the returns of public goods. The regression controls for the PRI’s vote share in 1988 ($PRI^88$) and the $alpha$ and $decline$ parameters fully discussed in chapter 4. We expect higher vote swings in favor of the PRI in places with higher alphas and lower declines. The signs of these coefficients should thus be positive and negative, respectively. We also expect lower vote swings in places where the PRI performed exceedingly well in 1988 and hence a negative sign of this variable. Results are reported in table 6.1.\textsuperscript{15}

All the coefficients perform as expected and all reach reasonable levels of statistical significance. Pronasol was a decisive factor in accounting for the PRI’s vote swing in the 1994 presidential elections even after controlling for state-fixed effects and municipalities’ divergent electoral histories since the 1970s, as measured by alpha and decline. As expected, the vote buying effect of private goods almost doubles that of public goods. This means that in terms of pesos per capita it was significantly more effective for the PRI to spend in clientelistic transfers such as scholarships, credit, granaries, life stock, mills, or cash transfers targeted to individuals or small groups of

\textsuperscript{14} The instruments we use are the CONAPO poverty index and its square; population; rainfall; and rugged terrain.
\textsuperscript{15} We examined the data of the swings of all the elections for spatial autocorrelation in GeoDA, failing to find a significant concern. Hence we do not include a spatial lag in the estimations.
producers, than in social infrastructure projects or public good provision for communities at large.

**Table 6.1 IV Regression of PRI vote swings, 1988-1994**

<table>
<thead>
<tr>
<th></th>
<th>Coef.</th>
<th>S.E</th>
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<tbody>
<tr>
<td>Private PC ^</td>
<td>0.052</td>
<td>0.008**</td>
</tr>
<tr>
<td>Public PC ^</td>
<td>0.034</td>
<td>0.011*</td>
</tr>
<tr>
<td>PRI 88</td>
<td>-0.890</td>
<td>0.024**</td>
</tr>
<tr>
<td>Alpha</td>
<td>0.043</td>
<td>0.014**</td>
</tr>
<tr>
<td>Decline</td>
<td>-0.022</td>
<td>0.034</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.755</td>
<td>0.034**</td>
</tr>
</tbody>
</table>

N = 2338
F(35,2302) 141.22
Pr>F 0.000
R2 0.619

As instruments we employ the CONAPO index and its square; population; rainfall; and rugged terrain. Regressions are run with robust standard errors and state fixed effects. We tested for overdetermination. ** significant at the 99 percent level. * significant at the 95 percent level.

Our results thus cast doubt on most of the existing empirical analyses on Pronasol. Although the literature has given a great deal of attention to this program as a key instrument in the PRI’s electoral recovery and the PRD’s debacle, most empirical analyses have concluded that PRONASOL’s electoral effectiveness was disappointing. Bruhn (1996: 162) finds no evidence that PRONASOL expenditure helped the PRI and undermined the left. Hiskey (1999) concludes that “the political impact of PRONASOL funds is noteworthy for its insignificance” (128). Although Molinar and Weldon (1994) claim to find positive effects for this program, their results are counterintuitive because they generate vote gains only in states where gubernatorial elections were held in 1991. “In states where elections for governor were not scheduled in 1991, the PRI did
somewhat worse in the federal elections for every extra peso spent on PRONASOL (Molinar and Weldon, 1994: 137).  

Table 6.2 presents analogous results for the election that led to the transition to democracy. The 1994-2000 vote swings are a function of the instrumented municipal level coverage of Progresa in 2000 (Progresa\(^\text{c}\)) and the instrumented per capita expenditure in FISM (FISM\(^\text{c}\)). \(^{17}\) The table also presents the analysis of 2000-2006 vote swings, this time for the PAN candidate since that party became the incumbent, as a function of the instrumented change in the coverage of Oportunidades (Oportunidades\(^\text{c}\)) from 2000 to 2006 and the predicted per capita expenditures in FISM (FISM\(^\text{c}\)). \(^{18}\)

Although we have recognized that transfers within each of these programs were determined by objective formulas that could not be easily manipulated, it is plausible that the phase-in of Progresa and Oportunidades were politically determined. That is, the formulas constrained the politicians to target the poor individually. However, politicians and policy makers were not constrained in terms of deciding the order of expansion of the programs across states and municipalities. \(^{19}\) This leeway to decide the phase-in compels us to instrument change of coverage in Progresa and Oportunidades to eliminate potential problems of endogeneity –that is, that higher increments of coverage might be in part driven by expectations of electoral swings. For the case of FISM, as discussed in the previous chapter, the fact that funding was determined by formulas does not rule out the

\(^{16}\) Magaloni (2006) finds that Pronasol bought votes for the PRI, but she fails to pay attention to selection bias problems.  
\(^{17}\) As instruments for the first of these regressions we employ the CONAPO index; total per capita expenditures in private goods in Pronasol; population; temperature; rivers; distance to rails; municipal size (km2).  
\(^{18}\) Our instruments for the second regression were the Human Development Index; temperature; coast/boarder; rugged terrain; distance to rails; distance to roads; municipal size (km2); and Easting kms.  
\(^{19}\) It is important to note that the randomization trial was only done at the early phase of the program in 1997. Subsequent expansions did not involve random assignment.
possibility that the choice of the specific elements included in the formula, and hence the
distribution resulting from them was the product of political calculations by the
incumbent party.

The results perform as expected. Progresa and Oportunidades both have positive
effects on the incumbent parties’ vote swing in the 2000 and 2006 elections. FISM also
impacts vote swings in the expected direction: more per capita expenditure in social
infrastructure at the local level translates into higher vote swings for the incumbent
party’s candidate in both elections even after controlling for state-fixed effects. This
means that regardless of the partisan label of the incumbent governor or the municipal
president, the incumbent party’s presidential candidate can profit from more expenditures
in social infrastructure projects.

Table 6.2 IV Regression of vote swings, 1994-2000 and 2000-2006

<table>
<thead>
<tr>
<th></th>
<th>PRI vote swing</th>
<th>PAN vote swing</th>
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<tbody>
<tr>
<td>Progresa^</td>
<td>1.886</td>
<td>0.607</td>
</tr>
<tr>
<td>FISM^</td>
<td>0.070</td>
<td>0.069</td>
</tr>
<tr>
<td>PRI 94</td>
<td>-0.605</td>
<td>-0.237</td>
</tr>
<tr>
<td>Aplha</td>
<td>0.076</td>
<td>0.031</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.158</td>
<td>0.252**</td>
</tr>
</tbody>
</table>

As instruments for the first of these regressions we employ the CONAPO index; total per capita
expenditures in private goods in Pronasol; population; temperature; rivers; distance to rails; municipal size
(km2). Our instruments for the second regression were the Human Development Index; temperature;
coast/boarder; rugged terrain; distance to rails; distance to roads; municipal size (km2); and Easting kms.
Regressions are run with robust standard errors and state fixed effects. We tested for overdetermination.
** significant at the 99 percent level. * significant at the 95 percent level.
To have a sense of the range of effects, figure 6.3 simulates the vote buying effects of private goods and public goods in the 1994 and 2006 presidential elections. According to these simulations, if the incumbents do not provide any public goods or private transfers they experience negative swings. Thus, voters do not tolerate incumbents that neglect them, explaining why Carlos Salinas and Vicente Fox decided to expand welfare benefits the way they did.

**Figure 6.3: Simulated Electoral Returns of Various Programs**

The clientelistic provision of public goods under Pronasol induces the most favorable vote swings. Compared to public goods provision, private goods were always better ways for the PRI to buy votes. For example, if the PRI spent 100 pesos in private goods the simulation predicts a swing of around one point; while the same expense in public goods would not generate a positive vote swing. That does not mean, however, that the electoral investment is useless, since any money spent, helps prevent adverse negative swings. Clientelism was more effective at producing electoral support for the PRI throughout the whole range of spending. But this does not mean that public goods
had no electoral payoff for the PRI. Public goods brought swings of considerable magnitude for this party at high enough levels of expenditure.

The relative electoral payoffs of entitlements are very different in that these are not always higher than the payoffs of public goods. In our estimates it is only once more than half of the municipality is receiving Oportunidades that it is predicted to show a positive swing in favor of the PAN, hence Fox’s interest in expanding Oportunidades. At a coverage of more than 60 percent the electoral payoff of a policy of entitlements is very high, in fact, higher than the equivalent benefits of public good provision.

4. Do Entitlements Pay?

This section supplements our findings with survey research. The study of antipoverty programs with individual level data presents challenges of selection bias too. As we discussed in chapter 2, thinking in terms of medical research and experiments helps conceptualize some of the problems involved in studying these types of policy interventions. To estimate the effects of a drug or medical treatment, ideally one would like to have two individuals that are identical in all respects (age, diet, gender, life-style, ethnicity, etc.) but the “treatment” (e.g., the drug). Medical research solves this issue through experimental design wherein a group of individuals of similar characteristics are randomly selected and divided into two groups, the “treated” one receiving the drug and the other “control” group receiving a placebo.

Experiments in social science are harder to design. First of all, most of the outcomes we are interested in cannot be controlled within the limited framework of
Experimental settings. Secondly, experiments always raise questions of external validity. Envisioned by a former academic well trained in economics, Progresa was originally designed to allow for experimental evaluations. Communities with similar characteristics were identified at the onset of the program, but only a randomly selected group began to receive benefits immediately, while the others were incorporated 15 months later.

Green (2005) and De la O (2006) take advantage of the program’s randomization or the delay in incorporation to assess the effects of Progresa on turnout and voting choices. These studies arrive to opposite conclusions. Green (2005) finds that Progresa had no effect on voting choices, while De la O (2006) obtains an effect on turnout of 5 percentage points and 4 percentage points for the incumbent’s vote share. One of the difficulties with reconciling these results is that in order to get leverage from the experimental design these studies end up comparing localities with very specific characteristics, raising issues of external validity. Green’s (2005) highly nuanced study first identifies 3,739 out of 105,749 localities for which electoral polling stations coincide with the boundaries of the locality. Within this sample, she then compares treated localities incorporated into Progresa with untreated ones. Her selection strategy yields highly atypical localities, where the PAN receives very low levels of support (around 20 percent). In her carefully designed study, De la O (2006) takes 505 localities selected by Progresa into the randomized experimental setup, where 300 of them received benefits 21 months before the election while the rest had only received benefits for 6 months. Given this design, before the presidential election of 2000 both groups were receiving transfers.

The conditions of Wantchekon’s (2004) fascinating study of Benin are very hard to replicate. The different findings might also be related to econometric strategies: Green (2005) uses a regression discontinuity framework, while De la O (2006) estimates a first difference regression model.
So the inference she obtains is about the effects of *length of time* of the treatment on voting decisions rather than effects of the treatment itself.

But the greatest problem for our purposes is that there is no experimental setting for Pronasol, FISM, and Oportunidades. This means that if we want to compare the political effects of all the various social assistance programs we must rely on observational data. However, to study the electoral payoffs of social transfers with surveys we can explicitly model the selection process, in order to create something akin to an experimental situation. That is, we contrast treated and untreated individuals by selecting two almost identical persons in terms of the non-random set of characteristics that make them subject of being chosen in the policy intervention, yet one receiving the treatment and the other not. Specifically we use a non-parametric technique, Propensity Score Matching, to match individuals along these lines (Imai, 2003; Rosenbaum and Rubin, 1993). The treatment variable in this quasi-experiment is being a Progresa or Oportunidades recipient.

In matching “the most common impact indicator of interest is the mean impact of the treatment on the treated. It is also known as the average treatment effect on the treated (ATET)” (Essama-Nssah, 2006). In this case, the comparison of the mean probability of voting for a given party between a treated group and its matched non-treated group is what we are interested in. The method is based on an assumption of unit homogeneity (Holland, 1986). This means that the outcome of the non-participant can be taken as an indication of what would have happened *holding all other relevant variables constant*. In

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22 In this technique, assumptions of linearity are not necessary, because matching is done non-parametrically. In the propensity score the challenge is to find a scale (i.e. the propensity score) under which the assumption of non-confoundedness holds (Imbens, 2003). There is no direct test that can assure this assumption holds. We follow the common practice that is to make sure that the propensity score of the treated and the control groups have a similar distribution (what is known as the balancing test).
this sense, the hypotheses tests do not control for covariates, since they are already incorporated into the choice of observations to be compared.

Propensity score matching is relatively simple to perform. The first-stage involves a probit (or logit) estimation of every individual’s probability of receiving the treatment, i.e., the propensity for being selected as a Progresa/Oportunidades beneficiary. The second stage is a simple test of means, where treated observations sharing similar propensity scores are compared to untreated ones. The estimations reported below were carried out using the PSCORE routine for STATA developed by Becker and Ichino (2002). We use a nearest neighbor match with bootstrapped standard errors.

To study the electoral payoffs of Progresa and Oportunidades, we use two exit polls performed by Reforma newspaper. The Reforma research department is credited with having some of the best electoral surveys in Mexico. The Reforma questionnaires are rather comprehensive, including self reporting of participation in Progresa and Oportunidades. The 2000 exit poll sampled 3380 voters in 119 municipalities (including delegaciones in Mexico City); while the 2006 poll included 5807 voters spread around 122 municipalities or delegaciones throughout the country.

The 2000 poll shows a relatively even distribution of votes for the PRI’s candidate, Francisco Labastida, in rural and urban localities. By contrast, the 2006 poll shows that support for Felipe Calderon varied considerably according to whether voters lived in rural or urban areas. The urban-rural gap in Calderon support is clear from both the survey and the official vote tally: according to the PREP, in rural areas his vote share was 29.7 percent, while in urban areas he reached 38.3 percent of the vote.

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23 We thank Alejandro Moreno for making the polls available to us.
The descriptive statistics of the survey (table 6.3) show that Progresa recipients voted more frequently for the incumbent’s party candidate, Francisco Labastida –there is a 24 percentage point gap in his favor between recipients and non-recipients. Vicente Fox from the PAN seems to be particularly affected by Progresa, producing a 21 percent gap against him. At the same time, supporters of the Similarly, the 2006 poll shows that the PAN’s candidate received more support among beneficiaries of Opportunities –37% of beneficiaries voted for Calderon vs. 32% of non-beneficiaries, a gap of 5 points. If one were to view only the average vote shares for the three main presidential candidates in the 2006 elections among non-beneficiaries of either program, a fierce tie between Calderón and López Obrador emerges, with Roberto Madrazo from the PRI in a distant third place overall. Among beneficiaries of the programs, however, Calderón outpaces López Obrador by double digits. Any one of these spreads is enough to tilt the national election in Calderón’s direction.

<table>
<thead>
<tr>
<th></th>
<th>2000 Elections Progresa</th>
<th>2006 Elections Oportunidades</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Beneficiary</td>
<td>Non-Benef.</td>
</tr>
<tr>
<td>Vote for PAN</td>
<td>25.4%</td>
<td>46.9%</td>
</tr>
<tr>
<td>Vote for PRD</td>
<td>16.4%</td>
<td>17.0%</td>
</tr>
<tr>
<td>Vote for PRI</td>
<td>57.7%</td>
<td>33.5%</td>
</tr>
</tbody>
</table>

But the crucial inference that one wants to know is whether voters in 2000 supported the PRI because they received benefits from Progresa, or because some of the correlates of Francisco Labastida’s vote are also related to the selection criteria of beneficiaries into the social program. The same question arises with respect to the effects of Oportunidades on support for the PAN in 2006.
Table 6.4 First-Stage Probits for Propensity Score Estimation

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>-0.023 (0.016)</td>
<td>-0.046 (0.011)**</td>
</tr>
<tr>
<td>Education</td>
<td>-0.123 (0.035)**</td>
<td>-0.183 (0.025)**</td>
</tr>
<tr>
<td>Income</td>
<td>-0.090 (0.026)**</td>
<td>-0.041 (0.011)**</td>
</tr>
<tr>
<td>Woman</td>
<td>0.146 (0.075) *</td>
<td>0.115 (0.050) *</td>
</tr>
<tr>
<td>Peasant farmer</td>
<td>0.099 (0.119)</td>
<td>0.158 (0.087)</td>
</tr>
<tr>
<td>Family size</td>
<td>0.072 (0.039)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Aggregate Socio-Economic Indicators</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Rural precinct</td>
<td>0.519 (0.098)**</td>
<td>0.577 (0.056)**</td>
</tr>
<tr>
<td>Usos y costumbres</td>
<td>0.213 (0.143)</td>
<td>Indians (municipal share)</td>
</tr>
<tr>
<td>No water supply (%)</td>
<td>0.850 (0.372)**</td>
<td>No water supply (%)</td>
</tr>
<tr>
<td>No electricity (%)</td>
<td>-4.062 (0.721)**</td>
<td>No electricity (%)</td>
</tr>
<tr>
<td>No sewerage (%)</td>
<td>0.708 (0.329) *</td>
<td>No sewerage (%)</td>
</tr>
<tr>
<td>HDI (2000)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Geographical Correlates</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Northing</td>
<td>0.001 (0.000)**</td>
<td>Rainfall</td>
</tr>
<tr>
<td>Easting</td>
<td>0.0002 (0.000)</td>
<td>Easting</td>
</tr>
<tr>
<td>Population density</td>
<td>0.0000 (0.000)**</td>
<td>Temperatura</td>
</tr>
<tr>
<td>Distance to roads</td>
<td>0.028 (0.007)**</td>
<td>Distance to roads</td>
</tr>
<tr>
<td>Distance to rails</td>
<td>0.007 (0.002)**</td>
<td></td>
</tr>
<tr>
<td>Distance to city</td>
<td>0.002 (0.001)</td>
<td></td>
</tr>
<tr>
<td>Landlocked</td>
<td>-0.301 (0.117)**</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Policy-Related Variables</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Progresa coverage, 2000</td>
<td>4.743 (2.556) *</td>
<td>Seguro Popular holder</td>
</tr>
<tr>
<td>Private goods % of funds in Pronasol, 1989-94</td>
<td>-0.347 (0.331) *</td>
<td>Change in Oportunidades coverage, 2000-06</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.857 (0.351)**</td>
<td>Constant</td>
</tr>
</tbody>
</table>

| Observations | 2379                           | Observations | 5019                           |
| LR (20)      | 328.88                         | LR (17)       | 1205.68                        |
| Prob>LR      | 0.0000                         | Prob>LR       | 0.0000                         |
| Pseudo R²    | 0.168                          | Pseudo R²     | 0.246                          |

* Significant at the 95% level; ** at the 99% level.

Robust standard errors in parentheses.

Table 6.4 shows first-stage probit estimations of our PSM for Progresa and Oportunidades. In estimating the propensity score, we use individual data from the survey.
merged with municipal-level data. The merge is important in our case because we know that selection into Progresa/Oportunidades was shaped both by income measured at the individual level from household surveys and by locality-level development indicators.\textsuperscript{24} The merger follows the electoral precinct identification at the polling points in the exit poll.\textsuperscript{25}

The combination of variables used to calculate the propensity score was inductive: we included variables with predictive power, while satisfying a balancing property among blocks of observations.\textsuperscript{26} We did not include variables that measure vote choice or are closely related to it in a voting model, such as party ID or left-right scales, since those are the outcomes we are interested in studying.

We use a rather exhaustive specification for predicting beneficiaries. We follow the practice of trying to include variables that are statistically significant predictors of selection into the program and that meet the balancing property. Thus, these models are similar to each other but not identical.

To highlight the issue of balance in the dataset, a common practice is to produce a histogram of the distribution of values of the matched dataset as compared to the unmatched original data.\textsuperscript{27} The four panels in figure 6.2 make those comparisons by

\begin{itemize}
\item \textsuperscript{24} We also employ a municipal-level variable for coverage of the program because this shapes an individuals’ probability to be chosen, as program managers decided to increase the coverage in rural places where the program was already working (the so called densification process).
\item \textsuperscript{25} The balancing property checks whether the propensity score within a range produces random treated and non-treated groups for the independent variables. It formally does not constitute a test of non-confoundedness, but can allow one to reject a propensity score that most likely does not satisfy it. We did not include variables that measure vote choice or are closely related to it in a voting model, such as party ID or left-right scales, since those are the outcomes we are interested in studying.
\item \textsuperscript{26} The balancing property checks whether the propensity score within a range produces random treated and non-treated groups for the independent variables. It formally does not constitute a test of non-confoundedness, but can allow one to reject a propensity score that most likely does not satisfy it.
\item \textsuperscript{27} Propensity score matching produces a dataset in which observations falling under a common support (i.e., observations with similar probabilities of being treated, but with some in the treatment group and others not) should satisfy what is known as the “balancing property”. If a matched dataset is balanced, there
\end{itemize}
showing the kernel density graphs of the distribution of propensity scores for both social programs. On the left side of each pair of graphs one can see that the propensity score of the untreated group is highly unbalanced, since there are very low probabilities predicted for the treatment; while the propensity score of the treated observations tends to be dispersed along the whole range of the propensity score. The panels on the right show the matched observations. Although their kernel densities are not perfectly identical, it is visually clear that the control observations used for matching are quite similar to the distribution of the treated. Although their kernel densities are not perfectly identical, it is visually clear that the control observations used for matching are quite similar to the distribution of the treated ones.\textsuperscript{28} We turn to exploring the political effects of these social assistance programs below.

\textsuperscript{28} As discussed by Ho \textit{et al.} (2006), the multidimensional nature of the covariates means that these graphs does not ensure that balancing is fulfilled, but at least it provides an indication of situations in which the datasets are clearly not balanced. A definitive test of the balancing property does not exist, at least in the current state of the literature. However, the various ways in which we have explored the dataset all suggest that the data are comparable. In addition to propensity score matching, we performed exact matching complemented with the nearest neighbor technique, which generates very similar results.
In table 6.5 we present differences-of-means tests between treated and untreated respondents, which is the program’s impact indicator or the ATET. In our case it corresponds to the comparison of the mean probability of voting for a given party between a treated group and its matched non-treated group. The mean probability of the non-participant can be taken as an indication of how electoral behavior or other political attitudes would have changed holding all other relevant variables constant. For our
purposes, the implication is that it is not necessary to include all relevant controls normally employed in vote models to be able to infer the political effects of a given policy intervention.

**Table 6.5: Effects of Progresa and Oportunidades on Vote Choice and Related Attitudes in 2000 and 2006**

<table>
<thead>
<tr>
<th></th>
<th>Vote in 2000</th>
<th></th>
<th>Vote in 2006</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>National</td>
<td>Rural</td>
<td>Urban</td>
<td>National</td>
</tr>
<tr>
<td>Vote for PRI candidate</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.167</td>
<td>0.286</td>
<td>0.087</td>
<td>-0.049</td>
</tr>
<tr>
<td></td>
<td>(0.058)**</td>
<td>(0.071)**</td>
<td>(0.050)a</td>
<td>(0.027)a</td>
</tr>
<tr>
<td>Vote for PAN candidate</td>
<td>-0.088</td>
<td>-0.195</td>
<td>-0.041</td>
<td>0.113</td>
</tr>
<tr>
<td></td>
<td>(0.043)*</td>
<td>(0.072)**</td>
<td>(0.050)</td>
<td>(0.032)**</td>
</tr>
<tr>
<td>Vote for PRD candidate</td>
<td>-0.060</td>
<td>-0.084</td>
<td>-0.023</td>
<td>-0.065</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.064)</td>
<td>(0.039)</td>
<td>(0.037)a</td>
</tr>
<tr>
<td>Approval</td>
<td>0.228</td>
<td>0.383</td>
<td>0.108</td>
<td>0.370</td>
</tr>
<tr>
<td></td>
<td>(0.082)**</td>
<td>(0.126)**</td>
<td>(0.110)</td>
<td>(0.060)**</td>
</tr>
<tr>
<td>Pocketbook</td>
<td>0.309</td>
<td>0.655</td>
<td>0.207</td>
<td>0.412</td>
</tr>
<tr>
<td></td>
<td>(0.098)**</td>
<td>(0.106)**</td>
<td>(0.134)</td>
<td>(0.051)**</td>
</tr>
<tr>
<td>PID PRI</td>
<td>0.132</td>
<td>0.248</td>
<td>0.098</td>
<td>-0.061</td>
</tr>
<tr>
<td></td>
<td>(0.051)**</td>
<td>(0.079)**</td>
<td>(0.064)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>PID PAN</td>
<td>-0.056</td>
<td>-0.130</td>
<td>-0.031</td>
<td>0.114</td>
</tr>
<tr>
<td></td>
<td>(0.034)a</td>
<td>(0.056)*</td>
<td>(0.047)</td>
<td>(0.030)**</td>
</tr>
<tr>
<td>PID PRD</td>
<td>-0.038</td>
<td>-0.062</td>
<td>-0.054</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.056)</td>
<td>(0.037)</td>
<td>(0.028)</td>
</tr>
</tbody>
</table>

Nearest Neighbor Matching Method with bootstrapped standard errors in parentheses. ** Significant at the 99 percent level; * at the 95 percent level; a at the 90 percent level.

The first thing to highlight is that both programs produced significant vote returns for the incumbent parties that implemented them. In 2000, beneficiaries of Progresa had a 17 percent higher probability of supporting the PRI’s candidate. This probability increases to 29 percent for rural beneficiaries, which are the target population. Our results also demonstrate that Progresa hurt the PAN, not the PRD. In fact, Vicente Fox received
scant electoral support from rural voters, who remained loyal to the PRI in 2000 to a large extent because of the benefits they received. Thus, our results contrast Cornelius (2002), who finds positive but rather marginal effects of Progresa in favor of the PRI (around 7%, just a bit more than in De la O, 2006). Cornelius’ (2002) is agnostic of selection bias problems, however.

In the case of Oportunidades, beneficiaries were 11 percent more likely to vote for Calderón from the PAN than non-beneficiaries with very similar propensity scores – that is, individuals with the same socio-demographic and community-level characteristics. Program beneficiaries were, at the same time, 7 percent less likely to vote for López Obrador from the PRD and were indistinguishable from non-beneficiaries in support levels for Madrazo from the PRI. Thus, our results demonstrate that López Obrador’s concerns about Oportunidades were well-founded but not in that individuals were being manipulated to vote for the PAN. Oportunidades worked to create an electoral constituency for the right-wing party among poor voters, who otherwise most probably would have voted for the left. These results contrast to Poiré and Estrada (2007) report no effect of Oportunidades in favor of the PAN, nor do they find a negative impact of the program on support for the left-wing candidate. These authors ignore problems of selection bias.

The last rows of the table report the effects of the program on approval, pocketbook evaluations, and partisan identification. Consistently, the effect of the program on the treated versus untreated electorate is to increase presidential approval, pocketbook evaluations, and partisan identification in favor of the incumbent party.
We highlight three issues about these results. First, our results indicate that Progresa and Oportunidades produced high electoral payoffs for incumbent parties. Although at first sight Progresa seems to have helped the PRI more than Oportunidades, the later seems to have been decisive in this party’s razor-thin victory. The program enabled the PAN’s victory both by giving votes to this party’s candidate among the poor and by taking votes away from the PRD –thus the negative impact of Oportunidades for a vote for López Obrador.

Second, these results provide strong support to our claim that partisan loyalties are conditional, as suggested in chapter 3. If one compares two almost identical individuals in terms of the sociodemographic and municipal-level traits used for selection into the program, one treated and the other not, their partisan identities are likely to be different as a result of these benefits. Welfare benefits, we demonstrate, help to re-create or create partisan identifications. In the Mexican case, Progresa reinforced the rural poor’s loyalties to the former ruling party, and Oportunidades helped the PAN generate new partisan sympathies among this voter group. It is noteworthy that the program works to decrease partisan identification in favor of the PRI, but does not affect identification with the PRD. These results suggest that the new social policies introduced by the Fox administration could possibly be credited one day for a dealignment by the poor from the former ruling party. Yet despite these programs, preexisting political loyalties to the left among the poor might remain unchanged.

Third, a crucial way in which Progresa and Oportunidades shaped voting decisions is by causing higher voter satisfaction with the way the president is handling things (approval) and higher satisfaction with their material well-being (pocketbook
evaluations). The effects of Progresa and Oportunidades on voters’ self-reported improvements in material well-being are impressive. For example, a rural Progresa beneficiary was 60 percent more likely to report that her material well-being had improved than a rural non-beneficiary. Similarly, both programs cause a significant increase in presidential approval. For instance, rural recipients of Oportunidades were 50 percent more likely to approve of Vicente Fox than rural non-recipients.

These patterns of preference beg the question of credit-claiming for Oportunidades, since it is an outgrowth of Progresa, introduced by a rival political party in 1997 and credibly claimed by the PRI as its own. Of course, the inherited program was almost entirely rural in its community coverage in 2000 and more narrowly distributed. The program’s expansion proceeded in two waves. The first occurred in rural municipalities between 2001 and 2004. The second wave began in 2002, in a rapid extension of the program to urban contexts with modified selection procedures (including self-selection by potential beneficiaries with agency review of their applications).

The relative impact of this program’s expansions could possibly be inferred contrasting the columns of the table that separate rural from urban voters when matching propensity scores for voters in and out of the program. If rural voters credited the PRI for Oportunidades, they would show higher levels of electoral support for this party and not the PAN. However, in the rural electorate, Oportunidades spurs higher levels of support for the PAN and incredibly high levels of presidential approval, suggesting that these beneficiaries credited this party and not the PRI.

Thus, we conclude that the delivery of welfare-enhancing benefits through entitlements and sustained policy innovation matters for a governing party’s electoral
prospects. In 2006 crucial votes that represented the margin of triumph at the national level for the PAN’s candidate came from the beneficiaries of the Fox government’s two major social policies, Oportunidades and Seguro Popular (Magaloni, Diaz-Cayeros and Estévez, 2006). The influence on voting decisions exerted by these programs, singly and in combined form, is logically tied to a retrospective calculation that partially neutralized prospective ones in 2006. What is ironic in 2006 is the partisan identity of the two sides of vote-buying, with the right delivering policies unassociated with its historical reputation against the left credibly promising changes in distributive policies in the future. In the end, the average beneficiary reasoned in line with the saying, “Better a bird in the hand than two in the bush”. The Spanish version exaggerates the discount rate of the future: Más vale pájaro en mano que cientos volando. Effective vote-buying, in line with this folk wisdom, is usually based on tangible exchanges from the past rather than welcome promises to the future.