Do Nonpartisan Programmatic Policies Have Partisan Electoral Effects? Evidence from Two Large Scale Randomized Experiments*

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Abstract

A vast literature demonstrates that voters around the world who benefit from their governments’ discretionary spending cast more ballots for the incumbent party than those who do not benefit. But contrary to most theories of political accountability, some evidence suggests that voters also reward incumbent parties for implementing “programmatic” spending legislation, over which incumbents have no discretion, and even when passed with support from all major parties. Why voters would attribute responsibility when none exists is unclear, as is why minority party legislators would approve of legislation that will cost them votes. We study the electoral effects of two prominent programmatic policies that fit the ideal type unusually well. For the first, we implement one of the largest randomized social experiments ever, and find that its programmatic policies do not increase voter support for incumbents. For the second, we reanalyze the study cited as claiming the strongest support for the electoral effects of programmatic policies, which is also a very large randomized experiment. We show that its key results vanish after correcting either a simple coding error affecting only two observations or highly unconventional data analysis procedures (or both). Our results may differ from those of prior research because we were able to marshal large scale experiments rather than observational studies or because we analyze relatively pure forms of programmatic policies rather than mixtures of programmatic and clientelistic policies. However, we conjecture that the primary explanation is the differing nature of the politics for which these policies are passed and implemented.

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1 Introduction

Political scientists in American and comparative politics have amassed considerable support for the theory that office-holders target discretionary government spending to gain votes, and voters reward them for doing so. As the magisterial literature review by Golden and Min (2013, p.12) summarizes: “Studies overwhelmingly find that incumbent politicians are rewarded by voters for distributive allocations, and in particular for those that are clientelistic and from which recipients can be excluded.” Yet, perhaps paradoxically, some literature indicates that voters react in the same way to programmatic policies, for which incumbents have no discretion in delivering benefits because citizens receive services based on well known, publicly stated rules (see Hicken, 2011; Kitschelt and Wilkinson, 2007; Stokes et al., 2013, for a definition). This is all the more puzzling in those situations where programmatic policies are passed with support from every major political party, including those which will be hurt electorally by their very action.

Although important efforts have been made to address the “programmatic incumbent support hypothesis” via qualitative argument (Cornelius, 2004; Diaz-Cayeros, Estevez, and Magaloni, 2009, 2016), and via the possibility of publication bias (Golden and Min, 2013), neither the formal theoretical nor quantitative empirical literatures has offered a resolution. We address this puzzle by analyzing two large programmatic policies, both designed to reduce poverty and its effects — Seguro Popular de Salud (SPS) and Progresa. Studying SPS and Progresa together is also advantageous because the literature views each as a highly visible, highly impactful, programmatic policy passed and implemented in a matter that is close to the pure theoretical form (Camp, 2013; Diaz-Cayeros, Estevez, and Magaloni, 2009, 2016).

We are fortunate in being able to study the electoral effects of these policies with two very large scale randomized experiments. In the first experiment, which we designed and implemented originally for the purpose of evaluating SPS in Mexico, is one of the largest social experiments ever conducted, and the largest randomized health policy experiment to date (King, Gakidou, Ravishankar, et al., 2007). The second is the only randomized social experiment previously used to study the electoral effects of programmatic policies,
and the one study regularly cited as giving the strongest support (De La O, 2013, 2015). Like the first experiment, it was also very large, originally designed as a policy evaluation, and in Mexico.

We thus have two cases relatively close to the theoretical ideal for programmatic policies. We are able to analyze each of these policies with an unusually strong experimental framework. As it turns out, our results from neither experiment provides any support for the programmatic incumbent support hypothesis: we find no evidence that either SPS or Progresa has a causal effect on voter turnout or electoral support for the incumbent party.

We also consider reasons for the differences between our results and the largely positive effects of the programmatic incumbent support hypothesis reported in the literature (Labonne, 2013; Larreguy, Marshall, and Trucco, 2015; Manacorda, Miguel, and Vigorito, 2011; C. Pop-Eleches and G. Pop-Eleches, 2012; Zucco, 2013). Although most prior articles employ non-experimental research designs, and study different countries, we conjecture that a key factor leading to our divergent results is the nature of the politics under which each policy was passed, and the ways in which the policies were implemented (Layton and Smith, 2015). Whereas SPS and Progresa are essentially programmatic policies, most policies studied in the literature are partly programmatic and partly clientelistic. To be specific, programmatic policies in general, including those we study and all others in the literature, (a) impose objective rules that give incumbents no discretion over implementation. This condition implies that programmatic policies should (b) lead voters to expect to receive the same services regardless of the party in office. In addition, SPS and Progresa are distinctive in that they also (c) were passed with broad support in the legislature from all major parties and (d) did not lead any single party to attempt to claim exclusive credit for its implementation. These additional conditions may have led to our findings that are different from those in the literature.

As we discuss in the concluding sections of this paper, future research may be able to unify the results of this study and those in the literature by considering the conditions under which different policies are passed in the first place. For one, incumbent chief executives generally prefer clientelistic policies, because they are likely to yield large electoral
benefits for themselves and their parties (Stokes, 2005). However, clientelistic policies can only be implemented when the incumbent can pass them, which usually means unified control of the executive and legislature (De La O, 2015). Then, as the electoral system becomes more competitive, and divided control over the branches of government more frequent, large scale social policies are less likely to pass if they would put veto players in minority parties at future electoral disadvantage. This pattern may then give rise to some of the relationships we observe in the effects of different policy types on incumbent electoral advantage.

We describe our experiments in Section 2 and give our empirical results in Section 3. We then put our analyses in context in Section 4 by detailing the conditions under which our results may apply to other policies studied in the literature. We also provide an Appendix with technical information about merging census and electoral data, and a separate online Supplementary Appendix that offers extensive supporting statistical analyses, robustness checks, alternative measurement strategies, analyses of a formal theory, and other information.

2 A Tale of Two Experiments: Background and Design

We now briefly describe the two huge programmatic policy interventions and the experiments designed to evaluate each that we marshal for this study. The facts that these programmatic policies were large, and the experiments we now describe reveal large effects on their intended social policy outcomes, makes them exceptionally hard tests of our hypothesis that they do not increase incumbent support.

2.1 Seguro Popular

Although Seguro Popular de Salud (SPS) translates literally to “universal health insurance,” the Spanish word for “insurance” does not appear in the authorizing legislation, as it is a social welfare (income redistribution) program, not a self-sustaining insurance program. The program was designed to build or improve medical facilities, and provide medical services, preventive care, pharmaceuticals, and financial health protection to the...
50 million Mexicans with no regular access to health care (constituting about half the population of the country). SPS was aimed at those with low incomes; its main purpose is to reduce the devastating effects of catastrophic health expenditures, when, due to illness or injury, greater than 30% of a family’s annual disposable income is spent on health care in one year. Before SPS, about 10% of the poor had catastrophic health expenditures each year. SPS was designed to eventually spend an additional one percentage point in GDP; in 2005, expenditures totaled a substantial $795.5 million. As it turned out, SPS was the most visible accomplishment of the administration of President Vicente Fox Quesada. By all accounts, it was designed, passed, and implemented in a nonpartisan, programmatic fashion.1

Because of Mexico’s term limits, President Fox, along with Health Minister Julio Frenk Mora, decided they needed a way to convince whoever would succeed them to keep SPS in place. How one democratically elected government can “tie the hands” of the next democratically elected government is a classic question of normative political theory (Klarman, 1997; Posner and Vermeule, 2002; Sterk, 2003; Thompson, 2005), formal theory (Alesina and Tabellini, 1990), and empirical political science (Franzese, 2002). In this case, Fox and Frenk’s approach was to commission an independent scientific evaluation by a team we led at Harvard’s Institute for Quantitative Social Science (see http://j.mp/ExpMex). The idea was that if the evaluation favored SPS, it would at least have been more difficult for the next government to eliminate. And to make the evaluation worthwhile, we committed publicly and in print to say so if the evaluation was not favorable in any way.

The Mexican government signed legal contracts that gave us free rein to design and implement whatever evaluation, and spend whatever funds, we judged appropriate. The government gave us unfettered access to government officials, the ability to influence how

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1Julio Frenk, one of SPS’s main architects said “From the beginning I suggested that this [SPS] had to be a proposal from all parties, in other words, that it was not only a PAN, PRI, or PRD project, but a project for the country, for which everyone could claim credit. In a conversation with President Fox, after introducing the bill to Senate, I told him: ‘If we want the reform to move forward, it is extremely important that the project is handled as a shared project and that we give credit to everyone.’ The president agreed, and promised that credit would be given to everyone, which is what happened” (Ortiz, 2006, p.81, our translation). Whether we judge by public statements from minority parties, or from the votes of legislation, there exists near consensus on this point.
SPS was implemented so we could more easily evaluate it, and convening power to speak with the numerous local officials across the country in charge of implementation. We retained the legal right to publish without prior review.

We developed a new experimental research design robust to the interventions by politicians who regularly — indeed usually — derail large public policy experiments, as they choose to be more attentive to the short term desires of their constituents than any longer term benefit of scientific evaluation (King, Gakidou, Ravishankar, et al., 2007). Details of the experiment include discussions of the background and design, published prior to any data analysis (ibid.), novel statistical methods we developed for this design (Imai, King, and Nall, 2009b), empirical results (King et al., 2009a), and publicly available replication data (Imai, King, and Nall, 2009a; King et al., 2009b).

We began by defining 12,284 health clusters, which are new, continuous geographic areas we defined that tile all of Mexico’s 31 states in which fall one health clinic (or potential clinic that we could decide would be built) along with its catchment area (defined as less than a day’s travel time to the clinic, using locally available methods of transportation rather than “as the crow flies”). We recruited 13 of Mexico’s 31 states to participate, including 7,078 health clusters. We matched these health clusters in pairs based on background characteristics and then selected 74 pairs (based on the closeness of the match, likelihood of compliance with the experiment, and necessary political criteria). The experiment thus included 534,457 research subjects, in 118,569 households, within 1,380 localities, nested within 148 health clusters.

Within each of the 74 matched pairs, we randomly selected one health cluster to receive treatment and the other, as the control, to receive no change. “Treatment” included new (or upgraded) hospitals and other medical facilities, doctors, access to medicines and other medical interventions, advertising campaigns to encourage affiliation with the program, individual insurance, and funds to pay for it all. The treatment was applied August-September, 2005, coincident with a baseline survey of 32,515 respondents randomly selected from 50 of the health cluster pairs; the outcome was measured the same way 10 months later, July-August 2006. We measured an extensive array of variables
including individual opinions, attitudes, health status, and financial spending; household level variables, such as assets, wealth, demographics, and others; and physical health measures via three separate blood draws for each person.

The 2006 Mexican presidential election was held on July 2, coinciding with the start of the follow-up survey, which of course happened to be perfect timing for studying the effects of SPS on the election (see Figure 1 in the online Supplemental Appendix). To merge federal election results reported by one governmental office, with census data reported by another, we define a new unit of analysis for this study within the confines of the experimental randomization. This is the precinct cluster, which we define as the largest possible geographic subset of a single health cluster in the SPS experiment for which we can accurately merge all relevant electoral and census information (for details, see the Appendix). When insufficient information is available to identify a precinct cluster, we retained all the benefits of this matched pair cluster randomized design by removing it and the health cluster (and corresponding precinct cluster) to which it was pair matched (as suggested by Imai, King, and Nall, 2009b).² This left us in the end with 57 matched-pairs (47 rural and 10 urban) out of the original 74 and all the benefits of matched pair randomization. Table 1 and Figure 4 in the Supplemental Appendix present full details and descriptive statistics of the sample we use and to which our inferences apply. (As another robustness check, we repeated our analyses with all available precinct clusters even when no match was available, trading off more model dependence for less inefficiency, and found no change in the results we present below. See Tables 2 and 3 in the Supplementary Appendix.)

Especially useful for the present paper is that our evaluation indicated that SPS had a massive effect on its intended outcome variable of financial assistance to the poor — a fact which is all the more impressive because government programs designed to help the poor in most countries typically have no measurable impact on the poor (Gwatkin, Wagstaff, and Yazbeck, 2005). In only 10 months, SPS eliminated about a third of the catastrophic health expenditure problem among the poor in Mexico, and about 60% among experimental compliers (those who would affiliate to SPS when in the treatment group and not

²This decision is not affected by which unit received the treatment within a given pair.
when in the control group). Perhaps even more important for our purposes, the people of Mexico clearly thought the program would help them: Fully 44% of those eligible in treatment areas enrolled in the program in the first month, which for each family involved taking a trip that could last as long a full day to formally affiliate to SPS. Moreover, those who enrolled liked the program a great deal: 69% of those enrolled rated the quality of health services as good or very good, and 97% planned to enroll again in the follow-up period after the experiment.

Some aspects of the program did not have the intended effects, as we reported (King et al., 2009a), but for our purposes this large financial impact makes this an especially strong test of the electoral effects of programmatic policies.

2.2 Progresa

Our second experiment evaluates one of the largest poverty-alleviation programs in Mexico. The program consists of nutritional, educational, and health components. The policy’s key feature consists of cash transfers that eligible households receive on the condition that they attend regular health check-ups, and children enroll and attend school. Program benefits vary according to household composition. The average level of benefits is about 35 USD per-month, which represents about 25% of income in poor rural households (Levy, 2006, p. 23).

The origins of the program date back to the mid 1990s, when the country experienced one of the worst economic crises in its history. The government had previously relied on a myriad of food subsidies to alleviate poverty. However, government officials in the administration of then president Zedillo concluded that subsidies benefitted urban centers at the expense of the poorest rural areas in the country, were regressive, and too costly to administer (ibid., ch. 2). The motivation underlying the new administration’s alternative approach was a recognition that in order to break the cycle of poverty, one had to recognize the relationship between the educational, health and nutritional components of human capital.

The architects of the program were committed to the eradication of poverty in the country, and as a result they implemented a design that would increase its long-term via-
ability. *Progresa* defined a target population and operated under clear, programmatic rules. In the eyes of the policy-makers, this would ensure the political neutrality of the policy.³ Political scientists have also arrived at the same conclusion about the nonpartisan nature of *Progresa*.⁴ Analogous to the experience with SPS, the government hired the International Food Policy Research Institute as a third-party entity to evaluate *Progresa* and bolster the program’s credibility (Levy, 2006, p. 43).

The main evaluation of *Progresa* exploited the phasing of the policy across the country’s localities. The sample consists of 506 rural localities in the country distributed across seven states, with 320 villages drawn from a population of localities eligible to receive the program by November 1997. Eligible localities were then assigned to the treatment group. The remaining 186 villages were randomly drawn from a populations that would receive the policy in one of the later phases (November-December and March-April of 2000) as the control group. Although *Progresa’s* design was “completely randomized”, as distinct from SPS’s more powerful “matched pair randomized” design, both have the advantages of large scale experimental randomization. Then, the government carried out several surveys in each of these villages, first to determine household eligibility and then to measure outcomes over a period of two years. Behrman and Todd (1999) find that treatment and control villages are fairly similar across a large battery of socio-economic indicators.

The results of the evaluation based on this sample of villages shows that *Progresa*...
increases school enrollment, improves the health and nutrition of children and adults, and increases household consumption (largely on food) (Skoufias, 2005, Ch. 5).

The 2000 Mexican presidential election was held on July 2. This means that for the purposes of studying the electoral effects of Progresa, the treatment group is defined as having received the program for 31–32 months before the election whereas the control group is defined as receiving it for only 3–8 months (see the time line in Figure 2 of our Supplementary Appendix). This is not as clean a test of the programmatic hypothesis as with our SPS experiment, since those who received the program recently in the control group may be as grateful as those who received it earlier, but for the purposes of the experiment the distinction between the two groups remains unambiguous.

As with the SPS experiment, analyzing federal election outcomes in Mexico requires a procedure for merging or matching the boundaries of electoral precincts with often overlapping census geography, as the two are generated by different administrative offices that typically do not coordinate. The same issue exists in almost all analyses of electoral data around the world, but the method of dealing with it is crucial. As we explain below, errors in this merging process for Progresa explains some of its unusual results.

These two extraordinary, large-scale experiments serve as an unusually powerful test of whether or not programmatic policies have electoral effects. Given the massive effects each of these policies had on people’s lives, if programmatic policies can electorally benefit incumbents, we should be able to find evidence for it in these experiments.

3 Results

The two sections that follow summarize our results. Numerous auxiliary analyses and robustness checks appear in the Supplementary Appendix that accompanies this paper.

3.1 Experiment 1: Seguro Popular de Salud

We present two sets of outcome variables for the SPS experiment based on analyses of actual electoral results (using aggregation procedures described in the Appendix) and on retrospective survey evaluations (which require no aggregation), respectively. The statisti-
cal methods used in both cases are fully nonparametric, enabling us reap the benefits from our randomized experiment without making modeling assumptions (for a full description, see Imai, King, and Nall, 2009b).

First, we estimate the average causal effect of SPS on electoral outcomes in Figure 1. (This analysis and all other causal effects on electoral outcomes are based on the 57 precinct matched cluster pairs, resulting from mapping the 74 evaluation health cluster pairs into precincts.) A point estimate (black dot) and 95% confidence interval (vertical line) appear for each causal effect, with a horizontal line at zero, indicating no effect. If the literature’s hypothesis is correct, that nonpartisan programmatic policies increase support for incumbents, the points and confidence intervals would appear above the horizontal line. Instead, all the confidence intervals cross the zero, no effect, line, and thus none confirm the hypothesis.

Figure 1: Causal Effect Estimates of SPS on Voter Turnout and Incumbent Party Vote. This figure gives point estimates and 95% confidence intervals for the “Intent-to-Treat” causal effect of SPS on voter turnout (solid line) and the incumbent (PAN) vote share (dashed line), overall (far left of left panel), and in three partitions of the data, including urban/rural (left panel), income quartile (middle), and asset quartile (right).

The overall estimated causal effect of SPS on the percent voting for the incumbent party (the PAN) is the first result, at the far left of Figure 1 (solid vertical line); the estimated effect on federal voter turnout appears next to it (dashed vertical line). In both cases, the estimated causal effect is not distinguishable from zero. Indeed, the confidence intervals are quite narrow for the estimated overall effects, indicating that the experiment
is well powered for this hypothesis and so we should have high confidence that the effect is negligible if not exactly zero.

To further search for possible support for the hypothesis, we partition our sample in three different ways to examine subgroup effects. We give the causal effect within rural and within urban precinct clusters at the right side of the left panel. All results are statistically indistinguishable from zero. The right two panels estimate causal effects by quartile of the proportion of individuals in the first two income deciles (center) and household assets (right). These may be especially relevant since households that report less income will pay less (or nothing if in the first two income declines) for SPS services. These results can also be viewed as predicting compliance with receiving the experimental treatment, since less poor communities were expected to (and actually did) sign up for SPS services less often. This analysis follows Calvo and Murillo (2004), whose account suggests that the largest electoral effect of policies could be found among individuals who benefit most from a given policy. Nevertheless, the estimated causal effect of SPS on voter turnout and on incumbent party vote within every one of these segments of the public is not distinguishable from zero.

Second, we avoid all issues involved in aggregating precincts to precinct clusters (discussed in the Appendix) by estimating the causal effect of SPS on individual survey evaluations. (This analysis, and all causal estimates on individual evaluations, are based on the 50 matched health clusters participating in the survey.) Recall that the beginning of our follow-up survey coincides with the election and hence we are measuring voters’ opinion right after the election. We do this for how well respondents thought the country was doing on economic, political, and social issues, compared to five years before (we provide descriptive statistics in Figure 5 of our Supplemental Appendix).\(^5\) This analysis also allows us to test the channels through which retrospective voting may work, one of the mechanisms hypothesized for why incumbents may benefit from programmatic politics (C. Pop-Eleches and G. Pop-Eleches, 2012).

These results appear in Figure 2 (in a format parallel to Figure 1). For the overall effect

\(^5\)The exact wording of the retrospective evaluation question in the survey was as follows: “Compared to five years ago, do you think Mexico is better, the same, or worse today economically/politically/socially?”
(at the far left) and for each of the three domain areas (economic as a solid line, political as dashed, and social as dotted), the effect is estimated at near zero with a confidence interval that overlaps zero. We find the same essentially zero effect of SPS, and failure of the programmatic policy hypothesis, for individual effects within different subgroups, including rural, urban (left panel), income quartile (center panel), and asset quartile (right panel).

Figure 2: Causal Effect Estimates of SPS on Retrospective Survey Evaluations. The figure reports point estimates and 95% confidence intervals of the causal effect of SPS on economic (solid vertical lines), political (dashed), and social (dotted) retrospective evaluations of whether the country was doing better today than it was five years ago ($n = 32,515$ individuals in 50 matched health cluster pairs). Results are reported for all respondents and by urban/rural breakdown (left panel), income quartile (center), and asset quartile (right).

In our Supplementary Appendix, we also present a wide array of other analyses of the same data. For example, we reran the analysis from Figures 1 and 2 with different coding rules and statistical techniques. These and many other analyses reveal no noticeable effect. (To be specific, in the Supplementary Appendix, Figure 6 analyzes PAN votes as a share of registered and eligible voters, as alternative measures of incumbent support. Figure 7 measures turnout with total votes cast as a share of eligible voters. Tables 2 and 3 report estimates for the impact of SPS on incumbent support and turnout when we break up precinct clusters pairs and analyze the data in a regression framework. Tables 4 and 5 do the same but for the sample of rural precinct clusters, for which we
know the population measurement error is minimal. Figure 8 shows that the re-allocation of opposition resources following the introduction of SPS does not explain the policy’s null effect. Figure 9 reports intent-to-treat (ITT) estimates by the share of evaluation population in precinct clusters to address concerns of attenuation bias in rural areas. Finally, Figure 10 reports estimates for the impact of SPS on individual retrospective economic, political, and social evaluations of the country when we control for the responses of the same people in a survey we conducted at baseline.) Overall, the results are unambiguous: the highly successful SPS programmatic policy had little effect on turnout or the vote.

3.2 Experiment 2: Progresa

We are fortunate to be in the possibly unprecedented position for political science of having a second large scale randomized experiment to study the same question. The first reanalysis of this experiment to study the programmatic incumbent support hypothesis appeared in Green (2006), who found no effect of Progresa on either incumbent support or voter turnout (with effects close to zero with small confidence intervals).

In this section, we replicate the more recent analyses of the same experiment in an article De La O (2013) and repeated and extended in a book (De La O, 2015), both of which report strong positive effects. We show here that the results in these works supporting the partisan effects of this nonpartisan programmatic policy were due to an unfortunate interaction between simple coding errors and highly unconventional data analysis procedures. When we correct either problem (or both), the results from this experiment look almost identical to the conclusions of Green (2006) and also our analysis of SPS in Section 3.1, indicating little or no effect of programmatic policies on federal voter turnout or incumbent support. Although, in the end, we reach the unavoidable conclusion that the claims of the article and book are not supported by empirical data, De La O deserves credit for highlighting this important issue, thinking of the idea of repurposing a randomized experiment, gathering the necessary data, and making available a replication data set so that further these discoveries became possible.
Coding Errors

In Mexico, as in many countries, electoral and population data are generated by separate government agencies that do not coordinate their work and wind up with different, overlapping geographies. For instance, while electoral authorities assign a given level of population to a precinct, census officials may aggregate part of the same population with a neighboring village that happens to be outside that precinct. Neither office is necessarily correct or incorrect; but they have goals and methods that differ from each other. The problems due to lack of inter-agency coordination is a common problem in the analysis of elections in many countries, but the issue caused particular difficulties here when De La O (2013, p. 5) “overlayed the 506 experimental villages to the smallest unit of outcome measure for which census, precinct, and electoral data roughly coincide: the sección electoral (precinct).” The key problem occurred with the article’s “name matching” procedure used to match precincts based on the textual names of villages in different data files, from organizations that assigned different meanings and geographic locations to the same names.

We verify below (and in Appendix A.2) that 71.3% of observations in De La O (ibid.) had villages incorrectly matched to areas with similar names from places outside of designated treatment and control precincts. The problem was not due to carelessness in matching names; instead, the entire process of name matching in these data is unjustified because names of different areas that happen to match often do not correspond to the same geographical areas. We confirmed that our conclusions about coding errors held generally with formal Mexican Freedom of Information Act requests and multiple conversations with officials at the Dirección Ejecutiva de Organización Electoral (DEOE) and Dirección Ejecutiva del Registro Federal de Electores at Mexico’s National Electoral Institute (INE). More detailed information about this issue appears in the Appendix to this

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6 An important clue comes when De La O (2013, fn.17) tries to account for some of the data with turnout and vote percentages greater than 100 by noting that 10 precincts “contained special voting booths where by law out-of-precinct voters can cast a ballot.” Although such precincts do exist, none are in the author’s data set — a fact which can be verified in data made publicly available by the Mexican government ([http://j.mp/mxife](http://j.mp/mxife)). To be more specific, of the four types of polling stations in Mexico — básicas, contiguas, extraordinarias, and especiales — only especiales allow out-of-precinct voters. Yet, none of the polling stations included in the sample analyzed are of this type.
Of the errors resulting from the name matching procedure, two caused an especially large bias. Correcting only these two units is sufficient to conclude that Progresa’s programmatic policies have little or no impact on turnout or voting for the incumbent. We will also show the same result when correcting all the data.

The large coding errors are apparent in the first column, for voter turnout, and the fourth, for the incumbent (PRI) vote, in Figure 3. Since it is impossible for more than 100% of people to vote, or to vote for any one party, every observation that appears above the dashed line at 100 is in error. Moreover, these include some large errors, cases that extend into the impossible region beyond 50% of the range of the original data, and for turnout more than three times the range. To choose the extreme point, turnout obviously cannot be 375%, as it is in the data analyzed in De La O (2013, 2015). In addition to these known errors, the large number of observations with nearly zero turnout (left column, bottom) in a presidential election with about 65% overall turnout (IFE, 2013) would also seem to be of dubious validity.

Before turning to the consequences of these data errors, we also analyze turnout and

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Figure 3: **Distribution of Turnout and Incumbent Party Vote in the 2000 election.** This figure compares the variables originally constructed in De La O (2013) via name matching (in columns 1 and 4), with the official turnout among registered voters and PRI vote share in the name-matching sample (columns 2 and 5) and in the GIS sample (columns 3 and 6).
incumbent (PRI) vote among those officially registered (using the precincts in De La O (2013) and an alternative GIS-determined sample). These alternative variables appear in Figure 3 in columns 2 and 3 for turnout and 5 and 6 for incumbent vote. These alternative variables are important for two reasons. First, they suffer from none of the problems in De La O (2013, 2015) because all the data needed to compute them come from the same source, with the same geographic boundaries. As a result, we can see in the figure that all the observations for these variables naturally fall within the possible region between 0 and 100%.

Second, the causal effect of Progresa is of course zero among all those not registered; yet, these are counted in the turnout and incumbent vote denominators in De La O (2013, 2015), even though they can never contribute to the numerator. In other words, noncitizens, those not registered, etc., cannot vote. Except in the unlikely situation where these ineligible people randomly distribute themselves geographically, causal effects based on these variables can be biased. And even if they were randomly located, this coding strategy adds large and unnecessary inefficiencies in estimation.

Our alternative turnout and incumbent vote share variables, based on officially registered voters, have no such measurement error bias problem since all those in the denominator would appear in the numerator if they choose to vote. If Progresa works in part by increasing levels of registration — as hypothesized in De La O (2013, p.7–8) — then the interpretation of the turnout and vote variables would change, but no post-treatment or other bias or inefficiency would be induced. Since no evidence has previously been offered for this hypothesis, we directly test it and present our results in Tables 12–15 and Tables 22–23 of our Supplementary Appendix. The results, with small confidence intervals around zero, clearly demonstrate that the program had little or no impact on registration rates. These alternative codings for turnout and vote are therefore considerably cleaner tests of the programmatic hypothesis, even if the original data had no coding errors.
**Conditions for Bias**

Thus far, we have revealed the existence of data errors, but that alone is insufficient to change any conclusion; the real question is whether these errors make a difference in the results. We find that this is indeed the case, especially for two huge outliers. The particular impact of these outliers is their correlation with the control variable “lagged population,” which we graph in Figure 4. In the left panel, we graph the raw data, which reveals the two outliers (precincts 266 and 1502), each 90 to 100 times the size of the median and both of which appear in the treated group, with nothing comparable in the control group. De La O (2013, p. 6) was right to check balance between the means of the treated and control groups, but checking solely based on the means led to missing the massive imbalance in the tails of the two distributions evident in this figure. The consequence of imbalance is model dependence (Ho et al., 2007). Since the most important advantage of a correctly analyzed randomized experiment is the absence of model dependence, introducing these coding errors eliminated this important benefit of randomization. Below, we illustrate and correct this problem. As we show in the next section, regardless of the reason for these outliers, when we correct for the imbalance they create, all evidence of significant effects of Progresa on turnout or incumbent support vanish.

We also go a step further and formally evaluate the bias these (incorrect) outlier observations have on the least squares analysis in De La O, 2013. We do this by computing the “statistical leverage” of these observations, with and without the lagged population variable. Observations with larger values of leverage have more influence on the magnitude of coefficient estimates in linear regression.\(^7\) As the right panel in Figure 4 makes clear, the statistical results in ibid. are driven largely by these two observations. Indeed, when controlling for lagged population, the two precincts have leverage that is 15 to 20 times larger than the median (see the vertical axis of the right panel). In contrast, when population is not included as a covariate, the leverage of these precincts is a modest 1.5 to 2.75 times larger than the median (see the horizontal axis). Since leverage is computed solely from the explanatory variables, this result applies identically to both outcome variables, turnout

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\(^7\)The statistical leverage of observation \(i\) is defined as \(x_i^\top (X^\top X)^{-1} x_i\), where \(X\) is an \(n \times k\) matrix of pre-treatment covariates with \(k \times 1\) row \(x_i\).
Figure 4: **Population Outliers and Statistical Leverage.** This figure reveals the extreme degree to which two of the miscoded observations are outliers (left panel) with extremely high statistical leverage (right panel). The left graph is raw data; the right is computed from the least squares analysis in De La O (2013).

and incumbent vote. Particularly unfortunate is that both of these extremely high leverage observations happen to also have extremely (and unrealistically) low reported turnout rates (1.78 and 2.59 percentage points, respectively) and PRI vote shares (0.74 and 1.08 percentage points). Judging from how the data were constructed, the considerably larger vote and turnout results based on official registration, the much larger vote and turnout national figures, or by comparisons with similar precincts, it is likely that these extremely low turnout and vote percentages are incorrect.

We have thus demonstrated all the conditions for bias in the main results: errors in the data that matter, substantial imbalance in the treated and control distributions, huge outliers highly correlated with a covariate included in the regression, and extreme outcome values for the same outlier units.

**Unconventional Model Specifications**

Finally, we consider the unusual model specifications in De La O (2013, 2015). Across electoral studies in American and comparative politics, researchers almost always mea-
sure the vote for an incumbent party as the *incumbent vote share* — the number of ballots cast for the incumbent party divided by the total number of ballots cast. Researchers from most countries then typically model vote share by including a lagged value of vote share as a control variable.

In contrast, De La O (2013, 2015) measured the incumbent vote for a party as the number of voters for that party divided by the *voting age population* — including voters, nonvoters, noncitizens, those ineligible to vote for other reasons, etc. The article then includes a lagged control variable which was not the incumbent vote share, and not the voting age population, but instead the number of people in the *total population* — including the voting aged population as well as all those under 18. As a result of this unusual decision, the outcome variable in this analysis does not add to 100% across parties, or across parties plus nonvoters, and the outcome and control variables are not logically or necessarily related. Although the model is theoretically possible — more noncitizens or infants in an area could in principle lead to more incumbent voting — this idea was not suggested in De La O (2013, 2015); indeed, to our knowledge, no other published statistical model of the electoral politics in any country and election has ever made these assumptions or chosen this type of specification.

More importantly, because counts, unlike percentages, are unbounded, outliers on this scale greatly exacerbate the bias that results from influential outliers. This is why, as we will show, either correcting the coding errors or switching to the dominant method of constructing vote variables eliminates any support for the effects of programmatic policies on partisan outcomes.

**Reanalyses**

We begin our reanalysis with Figure 5, which replicates De La O, 2013’s least squares regression estimate and 95% confidence intervals for the intent-to-treat causal effect of *Progresa* on turnout (left panel) and incumbent vote (right panel). It does this for the data as coded in the original article and book (dotted vertical line) and for data from the official results among registered voters, as in Figure 3 for both the original name-matched sample (dashed vertical line) and a GIS sample (solid vertical line). The horizontal line marking
no effect appears at zero. The only point in including the outcome variables as originally measured (the dotted lines) is to complete our forensic data analysis and to show how correcting the statistical analysis method makes the result vanish; the large number of errors, including the two huge outliers, indicate that the only valid causal estimates of the programmatic incumbent support hypothesis comes from the official vote and turnout figures (dashed or solid lines).

Figure 5: Intent-to-treat Causal Effects of Progresa on Turnout and Incumbent Party Vote. The left panel reports point estimates and 95% confidence intervals for the causal effect of Progresa on turnout as originally, and incorrectly, measured in the De La O (2013) sample (dotted vertical lines), for official turnout among registered voters in the same sample (dashed vertical lines), and for official turnout among registered voters in the correct GIS sample (solid vertical lines) for several different specifications, including in De La O (2013) for the first pair on the left. The right panel repeats the same analyses for incumbent (PRI) vote share. Every estimate is indistinguishable from zero, except when using the flawed original measure without controls (first two dotted lines in the right panel).

Reading each panel in Figure 5 from left to right, the different specifications we tried include the linear regression in the original article and book; a simple difference-in-means; a matching estimator⁸; a regression controlling for log of population; a regression with lag

⁸For matching, we did Coarsened Exact Matching (CEM), adjusting the coarsening to deal with the presence of high leverage observations among the pre-treatment covariate. The distributions of covariates before and after matching are reported in Figures 11–12 and 16–17 in the Supplementary Appendix, with full information in our replication data set.
turnout on the same scale as the outcome; and a regression, under the original specification, after removing the two observations with the highest leverage. The panel on the right repeats all the analyses for incumbent (PRI) vote share.

The results in Figure 5 exactly replicate results in De La O (2013, 2015) that give positive point estimates for turnout and vote share for the incumbent party, and a 95% confidence interval that excludes 0 for vote share but is insignificant for turnout. Using the original variable (with errors as is) reveals the same basic results, even using a simple difference in means estimator. However, once we use any of the four methods that each seek to control for the large imbalance induced by the data errors in De La O (2013), the effects vanish, with no statistically significant evidence for the effect of Progresa on either turnout or vote share. Moreover, rerunning any of the six analyses, while dropping the original error ridden variable and switching to official registration data, reveals no evidence of an effect for this nonpartisan programmatic policy on either partisan outcome, regardless of how the data are analyzed. Moreover, with the clean registration data, the confidence intervals are much narrower, and all twelve include zero as a causal effect.

Figure 6 repeats the same analyses, with the same robustness checks, for De La O, 2013, 2015’s instrumental variable analysis estimate of the causal effects. The results here tell essentially the same story, with no statistically significant effect of nonpartisan programmatic policies on voter turnout or vote for incumbent parties. Although again, only the official turnout and vote figures (dashed and solid lines) offer valid causal estimates, and these are not statistically different from zero.

To study the possibility that the null electoral effect of Progresa is due to attenuation bias, resulting from the presence of program beneficiaries and non-beneficiaries across precincts, we estimate the ITT by the average precinct poverty level. The findings, reported in Figures 14 and 15 in the Supplementary Appendix, show that the effect of Progresa is not increasing in poverty levels across precincts, and so attenuation bias does not seem to be a concern. Many other alternative specifications and analyses of the results from De La O, 2013 appear in our Supplementary Appendix, all leading to the same conclusion: Progresa had little or no effect on either voter turnout or the incumbent vote.
Figure 6: Instrumental Variable Estimates of Progresa on Turnout and Incumbent Party Vote. In a manner directly parallel to Figure 5, this figure replicates the instrumental variable estimation from De La O (2013). Every estimate is indistinguishable from zero, except when using the wrong measure without controls (first two dotted lines in the right panel).

4 Generalizing to Partial Programmatic Policies

Randomized experiments enable researchers to estimate causal effects without risky modeling assumptions that are necessary in observational studies. Yet experimental studies share with observational studies the problem of not being automatically representative outside the context in which they are conducted (Imai, King, and Stuart, 2008). We study here the differences in the politics under which policies are adopted and implemented in relatively pure-form and under mixed programmatic-clientelistic policies which we think may account for some of the differences in our findings. We first consider differences the politics of adoption and implementation, and then discuss broader theoretical explanations.

Politics of Policy Adoption  Clientelism has historically been the norm across developing countries because it yields large electoral benefits to incumbents (Golden and Min, 2013). However, adopting clientelistic policies is only possible with unified partisan control of government (De La O, 2015). Under divided partisan control, the only policies
that have a chance of passing are those supported by all parties (or at least all veto players). This explains why programmatic policies — SPS, Progesa, and every other major programmatic anti-poverty program in the developed world — have only arisen under divided control. For example, in ensuring congressional approval for SPS and Progresa, policymakers took great pains to ensure no political actor could claim credit distinct from other parties, and could not use the policies for their own electoral advantage (Levy, 2006; Ortiz, 2006). Voters were also well aware of these facts, and as a result widely believed they would receive program benefits regardless of the incumbent’s partisan identity (Diaz-Cayeros, Estevez, and Magaloni, 2016).

**Politics of Implementation** As relatively pure programmatic policies, SPS and Progresa reduce the chances of partisan credit claiming via a clear policy design and resulting nonpartisan bureaucratic implementation. Progresa is known for having the strongest design and implementation protocol among the universe of conditional cash transfers in Latin America (De La O, 2015), and SPS was quite similar in the period we examine. In contrast, none of the observational studies of the programmatic incumbent support hypothesis analyze pure programmatic policies (Larreguy, Marshall, and Trucco, 2015; Manacorda, Miguel, and Vigorito, 2011; C. Pop-Eleches and G. Pop-Eleches, 2012; Zucco, 2008, 2013), and as such none exactly fit criteria (a)–(d) in Section 1. These observational studies also analyze different policies, political situations, and political contexts than from each other and for our two experiments. Of course, every pair of studies that analyze a different policy or time period differ in an infinite number of ways, any one of which might be the lynch pin that determines any differences in results. As such, future researchers have an important opportunity to help teach us more about how far the programmatic incumbent support hypothesis, even though apparently inapplicable to pure programmatic policies, may work for more mixed programmatic-clientelistic government programs.

Consider that PANES, the temporary emergency relief program analyzed in Manacorda, Miguel, and Vigorito (2011), was a one-time effort. As such, and as discussed by the authors, voters may have associated the policy with the party responsible for its
enactment and implementation. Similarly, in the case of Romania, C. Pop-Eleches and G. Pop-Eleches (2012) explain that the Euro 200 program had a well known partisan intent. In Brazil, Hunter and Power (2007) and Zucco (2008) argue that one of the reasons for Bolsa Família’s electoral success was the president’s ability to claim credit for the program. In addition, A. Hall (2006) notes that the implementation of Bolsa Família was decentralized to municipalities, which led to charges of clientelism in the delivery of the program’s benefits. Finally, Larreguy, Marshall, and Trucco (2015) study an interesting urban titling program crafted in 1973, before Mexico was a democracy. Under this program, incumbents organized events, claiming exclusive credit for the number of land titles granted during a given time period.

Similarly, in a field experiment in Uganda, Blattman, Emeriau, and Fiala (2016) find that a program transferring cash to groups of unemployed youth increased support for and work on behalf of the opposition, among survey respondents. The policy was mostly funded by the World Bank, and a large proportion of survey respondents credited this institution for the creation of the program instead of the country’s autocratic government. The authors hypothesize that this aspect of the program freed voters from clientelistic ties, thereby increasing their propensity to support and work for the opposition. Frey (2015) finds a similar result in Brazil, where local mayors have a limited ability to claim credit for Bolsa Família.

In the Phillippines, Labonne (2013) finds, based in a randomized evaluation, that a conditional cash transfer (CCT) program modeled after Brazil’s Bolsa Família and Mexico’s Progresa increased support for local incumbents only in competitive (i.e., non-dynastic) municipalities with small federal budgetary transfers. The author does not find similar electoral benefits in competitive municipalities with large transfers. The explanation given for this difference is that in competitive municipalities with large transfers, mayors distribute funds among individuals not receiving the CCT, making the voting behavior of program beneficiaries and non-beneficiaries indistinguishable from each other.

**Theoretical Explanations** The well developed formal theory literature in this area is almost solely concerned with the electoral effects of discretionary, rather than program-
matic, spending policies. For example, Dixit and Londregan (1996, p.1132–1133) devote
the first two pages of their article to clarifying this point. Theoretical results suggest that
targeting tactical or pork barrel spending occurs most often to benefit swing voters (ibid.)
and marginal constituencies (Weingast, Shepsle, and Johnsen, 1981), and this is indeed
what the evidence shows (see Dahlberg and Johansson 2002 on the former and Keefer and
Khemani 2009; Primo and Snyder 2010 on the latter).

Studies have also found that such spending has substantial electoral payoffs (Evans,
2006; Levitt and Snyder, 1997). One clever exception, proposed in De La O (2015), holds
that minority parties will rationally choose to support a programmatic policy that will hurt
them electorally if opposing it imposes even larger costs. Unfortunately, as we show in
Section 3 of our Supplementary Appendix, the proposed formalization of this argument
is consistent with both the programmatic incumbent support hypothesis and the opposite,
and so the theory cannot be used to explain either. We also show there that the parameters
of the theory are impossible to test from the Progresa (or SPS) experiment and so either
way the theory cannot explain the empirical results. However, the foundations of the
model in De La O (ibid.) are of considerable value: when governments try to pass major
public policies to cope with poverty in the presence of divided government they are much
more likely to adhere to the programmatic policy ideal.

A variety of other perspectives might also help explain apparent divergent conclusions
of different studies. For example, as we describe in Section 2, the Progresa experiment
compared those who received the program recently and more distantly, unlike the SPS
experiment which had a clean control group that did not receive the program at all.9 Or,
voters may simply reward incumbents for the implementation of programmatic policies
as a result of reciprocity (Finan and Schechter, 2012; Manacorda, Miguel, and Vigorito,
2011). Incumbents may also signal their commitment to the poor by adopting certain
policies, making it rational for voters to re-elect them (Diaz-Cayeros, Estevez, and Mag-
aloni, 2009). Another possibility is that voters are retrospective in their voting behaviour.
Therefore, welfare improvements associated with government programs prompts them to

9However, Green (2006), using a fuzzy regression discontinuity design, also finds that Progresa does
not have an impact on incumbent support or turnout.
reward incumbents (C. Pop-Eleches and G. Pop-Eleches, 2012). Finally, it may simply be that while incumbents pledge to implement a policy in a non-clientelistic manner, bureaucrats and local-level politicians fail to fulfill this promise (A. Hall, 2006; Rocha-Menocal, 2001).\(^{10}\)

**Future Research**  A valuable area for future research, then, would involve deriving theories that could shed light on when and under which political contexts and policy proposals the programmatic incumbent support hypothesis may or may not hold. Empirical research would also benefit from studies that seek to study the programmatic incumbent support hypothesis in policy and political contexts that are partially programmatic and partially clientelistic (Camp, 2013; De La O, 2013, 2015; Diaz-Cayeros, Estevez, and Magaloni, 2009, 2016).

**5 Concluding Remarks**

When incumbent parties exercise discretion over government spending to benefit specific groups of voters, an impressive literature shows that these voters reward the incumbents with electoral support in subsequent elections. The reason for this powerful result would seem to be called into question by claims that nonpartisan programmatic policies — policies over which incumbents have no discretion — have similar effects on voters making partisan decisions. We sought to contribute to an explanation of why this would happen and, also, why minority party politicians would, in the first place, support policies that would lead voters to oppose them in the next election. It is perhaps fortunate, then, that our results are so strong in concluding that programmatic policies have little or no impact on voter turnout or voter support. Our findings may also suggest why it is difficult for a country to break the cycle of perverse accountability (Stokes, 2005): Since only clientelistic policies yield electoral payoffs, incumbents are willing to adopt programmatic policies only in a context of high political competition.

\(^{10}\)Blind retrospection may yet be another mechanism accounting for a positive effect of programmatic policies on incumbent support (Achen and Bartels, 2004). We consider this possibility unlikely, as other studies have questioned the existence of such phenomenon (see, for example, Fowler and A. B. Hall, 2016).
The evidence we offer may raise other issues as well, such as the fact that parties in Latin America do not seem to have converged to political competition on a left-right dimension as is common in Western democracies. Indeed, the cases of Progresa and Seguro Popular illustrate that certain policy-making domains, which may be of intense political contestation in other countries, have become de-politicized. The consequence of this phenomenon is that no party uniquely claims exclusive representation of poor voters. In line with De La O (2015), however, our view is that the de-politicization of social issues may have to do with the specific context (e.g., divided government during an economic crisis) in which anti-poverty policies have been adopted.

We are in the unusual position for our discipline of being able to test an important hypothesis with two extremely large scale, comparatively high quality randomized experiments. The relatively pure programmatic policies evaluated in both cases were also of such large scale, so highly impactful, and so popular that it is unlikely — given that we found no impact of programmatic policies on voter support in these cases — that it could be found in other circumstances that also have pure programmatic policies. We look forward to future researchers shedding additional light on this important question in other national and policy contexts.

Appendix A Merging Census and Electoral Data

In many democracies, election administration is conducted in an entirely separate office than census operations, vital registration, and other demographic accounting. The result is that the definition of the areal units used in any data analysis that define electoral precincts often overlaps or conflicts with that for census geography. This causes common and well-known data issues, and must be treated carefully by any scholar using aggregate electoral data from many countries around the world (e.g., King and Palmquist, 1998).

A.1 The SPS Experiment

For the SPS experiment, we address problems due to having separate sources for electoral and census geographies in two ways. The first involves defining the “precinct cluster” as
a new geographic entity for our unit of analysis. The second is our large scale, individual-level survey, for which no merging issues arise in the first place. These two data sources are the basis for the analyses in Figures 1 and 2, respectively.

We begin with available information, which includes, in addition to the electoral and census databases, (a) the set of villages that fall within (and help define) each health cluster according to the Health Ministry, (b) the set of villages that fall within (and help define) each electoral precinct according to the Electoral Institute, (c) the complete GIS definition of the precinct boundaries, (d) the geographic centroid for each village, and (e) detailed satellite imagery.

We define the precinct cluster as the set of electoral precincts that contain at least one village belonging to a single health cluster assigned either treatment or control (but not both) within the SPS experiment. We do not use the textual name given to villages since these have different meanings in the two administrative databases.

Figure 7 gives examples of how we define precinct clusters in rural areas. In the left panel, two contiguous precincts (green areas with gray boarders) from the municipality of Ixtlahuaca each contain one village centroid (red dots). The precinct cluster in this panel is then the aggregation of both precincts (the entire green area). We also portray, in the right panel, the precinct cluster from the municipality of Santo Tomás; this precinct cluster, with three constituent precincts, is defined in the same way, even though it includes some villages (black dots) not participating in the experiment. We keep these precincts in the analysis, recognizing that including them could slightly attenuate the estimated effect of SPS on electoral outcomes in Figure 1 (but not Figure 2). Finally, we of course remove any precincts that have village centroids that span health clusters assigned to different treatment regimes.

Finally, creating precinct clusters in urban settings is slightly complicated by the fact that the urban health clusters are defined as aggregations of census tracts, and the tracts’ boundaries sometimes overlap precinct boundaries. We overcome this problem with detailed satellite imagery, which we used to check the population distribution across precincts and tracts. This allows us high levels of confidence that the overwhelming ma-
Figure 7: **Defining Precinct Clusters in Rural Areas.** The figure shows how we create precinct clusters, with examples from the municipality of Ixtlahuaca (left panel) and the municipality of Santo Tomás (right panel). In each appears individual precincts (geographically contiguous green areas outlined in gray), village centroids from a health cluster randomly assigned to the control group (red dots), and a precinct cluster (the entire green area). On the right also includes a few village centroids from health clusters that were not part of the SPS experiment.

The majority of the population in the precincts belongs exclusively to the health clusters, and our corresponding precinct clusters, that participated in the evaluation. This also eliminates any potential attenuation bias.

Figure 8 offers an example of the criteria we use to define precinct clusters in urban clusters. The figure displays a satellite image (with more resolution than we can print on the page) of a health cluster in Morelos in the experiment (red boundary) and the precincts, numbered 660 and 661 (blue boundaries), which it overlaps. We assigned a precinct to a health cluster if the population residing in the precinct is found almost exclusively within the area of the health cluster, as is the case in this example. In this particular case, the population reported by census officials in 2005 within the health cluster is 2,996 inhabitants. This figure is very close to the total population of 3,051 inhabitants that census and electoral officials report for precincts 660 and 661 during the same year. Therefore, we include these two precincts in our analysis and use them to define one precinct cluster.
Figure 8: **Defining Precinct Clusters in Urban Areas.** The satellite image in this figure portrays precincts 660 and 661 (blue boundaries) and the overlapping health cluster in our experiment (red boundary). Because virtually all the population in the two precincts also fall within the health cluster, we assign a precinct cluster to be coincident with the health cluster.

### A.2 The *Progresa* Experiment

**Flawed Name Matching**  
The coding errors in De La O (2013) were generated by using the textual name given to a village to try to match electoral and census data. Unfortunately, these names were never normalized or disambiguated, and the data contain no unique identifiers or matching keys. They are, in fact, created by a different person in each office choosing or making up a name and labeling a geographic area, without coordinating with their counterpart in the other office. The result of this process is that government agencies often wind up using different names to refer to the same village or the same name to refer to different villages.

Figure 9 illustrates these errors with the two largest outliers from Figure 3. The goal of this analysis is to locate each village (the geographic centroid of which is portrayed as a dot) within the correct electoral precinct (the aerial unit in green). The left two
panels follow the approach in De La O (2013) — incorrectly assuming that electoral and census officials use identical village names (and distribution of the number of villages per precinct) to refer to the same geographic areas. This “name matching” procedure leads to the inclusion of the village of Ciudad Valles in precinct 266 of San Luis Potosí (top left panel) and Tulancingo in precinct 1502 of Hidalgo (bottom left panel). We now show that these matches are incorrect.

Figure 9: **Name Matching v. Correct GIS Location.** For precincts 266, San Luis Potosí (top row) and 1502, Hidalgo (bottom row), the left column assumes, as De La O (2013, 2015) that census and electoral officials rely on the same village names and numbers of villages per precinct and uses name-matching to locate villages. The right column uses accurate GIS coordinates of census villages.
**Accurate GIS Locations**  To avoid entirely the problem De La O (2013) induced by name matching, we obtained from the Census Bureau the exact village centroids and mapped them with geographic information systems (GIS) technology into the known precinct boundaries (the two right panels in Figure 9). The errors can be seen clearly by the true locations of villages Ciudad Valles and Tulancingo (red dots) entirely outside the precincts (green regions), and instead in areas of high population density (as reflected by the large number of small-area precincts surrounding their respective centroids).

The mistake leading to the errors in De La O (2013, 2015) are not the only coding errors in the data. The article and book also incorrectly assumed that the number of included villages in each precinct by the electoral office was identical to that reported by the census office. For example, the electoral records indicate that precincts 266 and 1502 have 2 and 5 villages respectively. However, because of the population, disambiguation, and name matching problems, this count does not imply that they have 2 and 5 villages according to census records. In fact, the correct numbers, according to the precise GIS coordinates, are 6 and 10 villages, respectively.

These two errors turn out to be extremely consequential. For example, the correct populations of the tiny villages in precinct 266 in 1995 are 4 (people), 82, 1, 2, 1, and 7. Yet, the village incorrectly included in this precinct had over 100,000 inhabitants. Similarly, the villages in precinct 1502 had populations in 1995 of 210, 205, 163, 156, 1998, 83, 97, 19, 41, and 3, whereas the village incorrectly included had 87,458 inhabitants.

Unfortunately, the same types of errors exist throughout the data in De La O (2013, 2015). To show this, we compared the name-matching sample with the sample generated by the GIS procedure. The original sample includes 417 precincts, while the GIS has 410. The two sample have in common 337 precincts, and out of these we are able to replicate the exact village distribution as in De La O (2013, 2015) in just over 80 percent.

As detailed in Table 24 in the Supplementary Appendix, we find that the total population in 71.3% of precincts in the sample from De La O (2013, 2015) differs from the correct GIS sample. This discrepancy is due to three types of mistakes: precincts that include all villages that belong and at least one that does not but coincidentally matches
a village’s name from outside (11.8%); precincts that exclude at least one census village that belong and at least one that does not but coincidentally matches a village’s name from outside (32.7%); and precincts that exclude at least one census village that belongs and no additional villages through name-matching (32.4%).

Finally, we studied the specific choices made in generating the name-matched sample in De La O (2013, 2015). As it turns out, even if name matching made sense (i.e., if census and electoral offices had coordinated in naming villages), many of the choices were unjustified. For instance, in 11.4% of precincts, at least one electoral village had a census village with a matching name that was excluded from the precinct. Another 26.5% of precincts report actual electoral villages without any matching name among census villages.

Along with Mexican officials we talked with, we conclude that the only valid data presently available to study the effects of programmatic policies is from the GIS generated sample we used in this paper.

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11The three elements do not add to the total because of complications with missing census data.


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