Do Nonpartisan Programmatic Policies Have Partisan Electoral Effects? Evidence from Two Large Scale Experiments — A Supplementary Appendix

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Abstract

This is a supplementary appendix to Kosuke Imai, Gary King, and Carlos Velasco Rivera,"Do Nonpartisan Programmatic Policies Generate Partisan Electoral Effects? Evidence from Two Large Scale Randomized Experiments" copy at http://j.mp/NonParProg.

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1 Introduction

Most of the figures, tables, and analyses in this Supplementary Appendix are referenced directly in our paper. Items not referenced in the paper are briefly explained here:

- Figures 4 and 5 provide the timeline of the SPS and *Progresa* evaluations, and details about the 2000 and 2006 presidential elections in Mexico.

- When a precinct contains experimental villages belonging to health clusters from different treatment regimes, we delete the precinct from the precinct cluster (although we found that including these, including them with an indicator variable, or excluding these precinct clusters entirely do not affect our conclusions). The map on the left panel of Figure 6 of this Supplementary Appendix illustrates one example of two clusters located in Sonora with villages from different health clusters, assigned to different treatment regimes. In this case we find that village centroids from treatment health clusters (blue dots) and from control health clusters (red dots) appear in some of the same precincts (light green areas). As a result, such "contaminated" precincts have an undefined treatment status and are removed from the analysis.

- Tables 8–10 report regression estimates and sample characteristics for the results under each of the specifications displayed in the paper in Figures 3 and 4. Figure 20 reports estimates for the causal effect of *Progresa* on turnout, measured as the total number of votes cast (valid and invalid votes) over the total number of registered voters.

- In Figures 21 and 22, we examine whether the effects of the *Progresa* poverty alleviation program vary by poverty levels. Tables 18–23 and Figures 25–28 report results when implementing the same specifications discussed in the paper with a sample we generated by mapping the geographic coordinates of villages across precincts, instead of the name-matching procedure adopted in De La O (2013, 2015). These additional specifications and results lead to the same conclusion: *Progresa* had no statistically significant effect on either voter turnout or incumbent support.

- As a robustness check, we repeated our analyses with all available precinct clusters, even when no match was available. The results appear in Tables 2 and 3. These results reveal that our conclusions in this regression framework remain unchanged from those in the text of the paper.

2 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).

2.1 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 1 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 similarly, 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 on page 7 (but not Figure 2 on page 9). Finally, we remove precincts with village centroids spanning health clusters assigned to different treatment regimes.

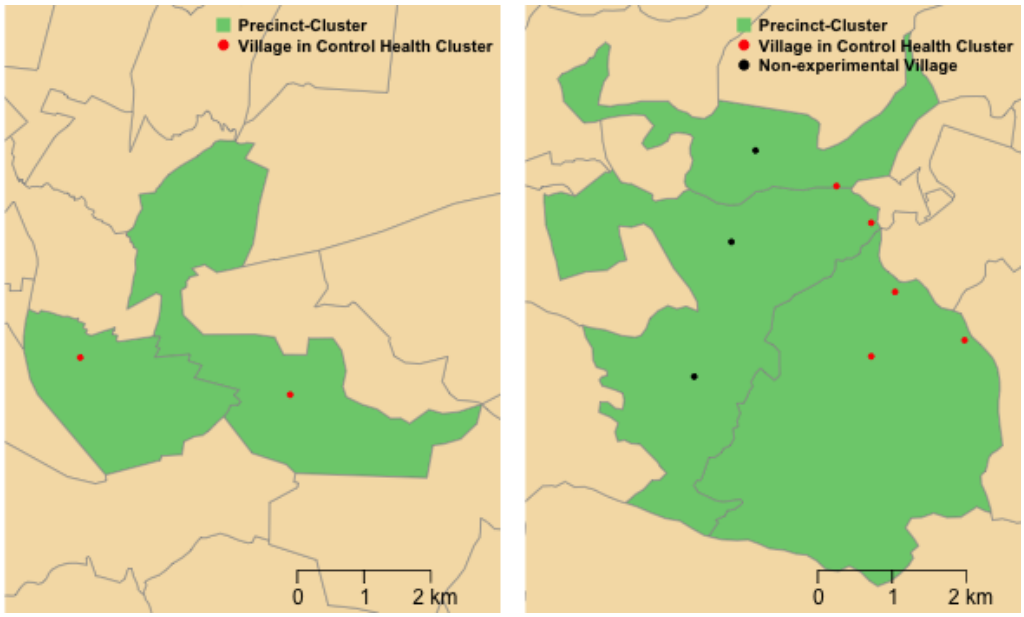

Figure 1: **Defining Precinct Clusters in Rural Areas.** The figure shows how we create precinct clusters, with examples from the municipality of Ixtlahuaca (left panel) and the municipality of Santo Tomás (right panel). In each appears individual precincts (geographically contiguous green areas outlined 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.

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 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 2 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 2: **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.

2.2 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 3 illustrates these errors with the two largest outliers from Figure 7. 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 (ibid.) — 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.

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 3 on the following page).[1] The er-

[1]The shapefile we use to implement the spatial merge of precincts and villages corresponds to boundaries in place during the 2000 presidential election. Electoral officials were initially reluctant to share this file with us because they did not know who had created the file and had no means to ascertain its validity. Instead, they advised us to use for the spatial merge the shapefile with the precinct boundaries in force during the 2003 congressional elections, which is the earliest they had validated. We tried both files and found that the sample of villages of precincts we obtain for our analysis is not sensitive to this choice. In our conversations with electoral officials we also learned that the precision of precinct boundaries in the shapefiles improves over time. Thus, one could be tempted to use a more recent shapefile, where the improvement of boundary precision is more widespread, to implement the spatial merge of villages and precincts. However, this would lead to misleading results because, as we discuss in Section 2.3, boundary changes related to causes other than technological improvements (e.g., population growth) are widespread.

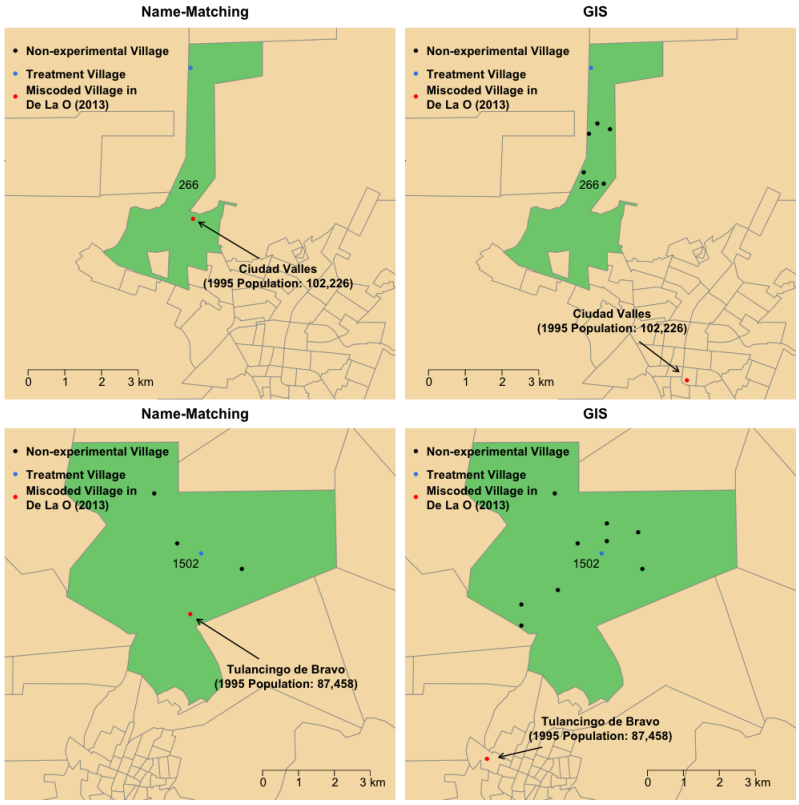

Figure 3: **Name Matching v. Correct GIS Location.** For precincts 266, San Luis Potosí (top row) and 1502, Hidalgo (bottom row), the left column assumes, as De La O (2013, 2015) that census and electoral officials rely on the same village names and numbers of villages per precinct and uses name-matching to locate villages. The right column uses accurate GIS coordinates of census villages.

rors 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 28 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%).[2]

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.

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

2.3 A Brief Guide to Electoral Precinct Boundary Changes in Mexico

Broadly speaking, there are four ways in which precinct boundaries may change. First, electoral officials rely on the “reseccionamiento" procedure. This procedure can be implemented in redistricting years (i.e., roughly every 10 years) or any other year with the exception of those coinciding with elections. The goal of this program is to ensure the size of precincts remains within the eligible voters bounds for precincts ordained by the law (During the period we examine, the lower and upper bounds were 50 and $1,500$ eligible voters respectively). Central authorities of the Electoral Institute initiate the “reseccionamiento" program and it is usually invoked to modify the boundaries of precincts in the periphery of large urban areas that have experienced significant population growth.[3]

Second, after the creation of a municipality in the country (a rare of occurrence), officials carve out precincts for the newly created administrative unit. Third, electoral officials may “fuse" a precinct with a neighboring one if it reports a total number of eligible voters lower than the bare minimum required for the existence of a precinct. In this case, the precinct is fused with a neighboring one such that together they report a total number of eligible voters within the bounds ordained by the law. Finally, electoral officials employed at each of the 300 electoral districts in the country constantly work in coordination with members of the central office of the National Electoral Institute (INE) to update precinct boundaries. Their work brings about changes to precinct boundaries which arise either because of improvements in field work, changes in population, or both. All of these changes accumulate over time and are reflected in the digital maps the government uses for the organization of elections in the country.

Electoral officials have a readily available record of boundary changes resulting from "reseccionamiento," the creation of municipalities, and fusion of precincts in a document titled “tabla de equivalencias seccionales." Unfortunately, this document omits precinct boundary changes related to the work of electoral district authorities. There are paper records with the information necessary to document the date and reason for the modification of boundaries resulting from the labor of district authorities. However, because

[3]Usually, electoral authorities initiate “reseccionamiento" prior to redistricting years to aid their work in the redrawing the boundaries of electoral districts in the country.

of the volume of these files, we learned that government officials are currently incapable of producing a database enlisting all (or selected) boundary changes falling in this category during the period 1994-2006. As such, given the context of widespread changes to the boundaries of precincts, and based on our conversation with government officials, we rely on precinct boundaries temporally close to the election we examine to test the pro-incumbent effect of programmatic policies.

3 Additional Empirical Analyses

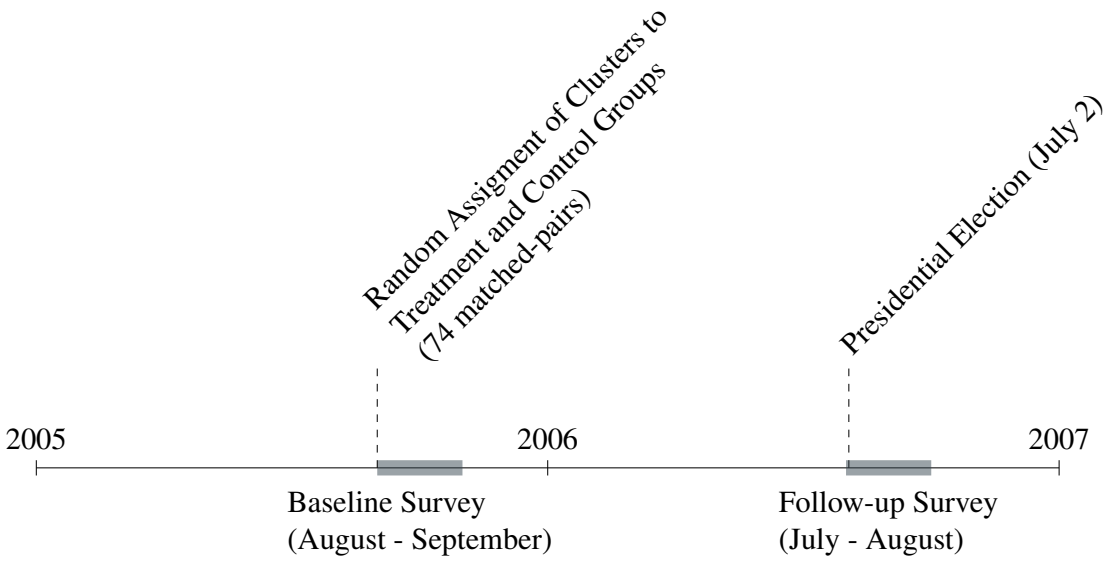

Figure 4: **SPS Evaluation Timeline.** The figure displays the timeline of the SPS evaluation and the date of Mexico's 2006 presidential election. In the presidential contest PAN, the incumbent party, competed against PRI, a leftist coalitiom under the PRD leadership, and two other minor parties (PSD and Nueva Alianza). The PAN candidate was victorious, defeating the PRD candidate by a half percentage point.

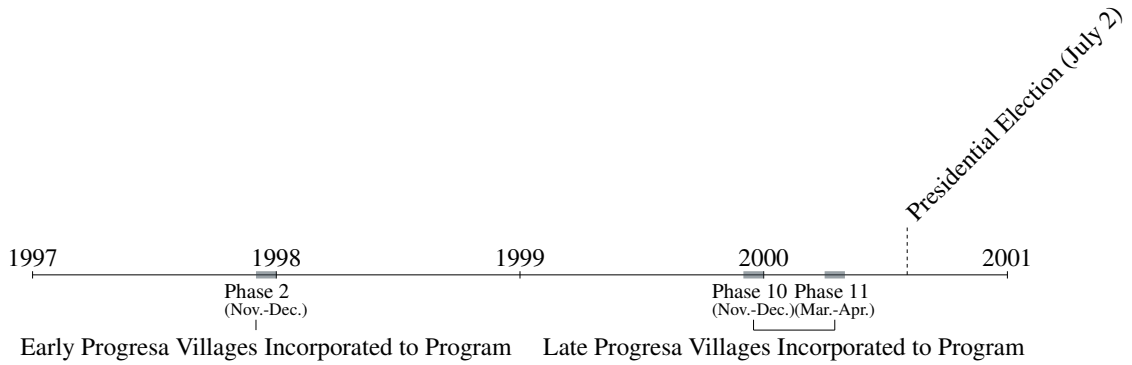

Figure 5: **Progresa Evaluation Timeline.** Researchers sampled 320 villages across seven states that were incorporated to the program by its second phase of expansion, and defined this set as the treatment group. As the control group, the researchers then sampled 186 that would be covered in later phases of the program (phases 10 and 11). (Further detailes of the Progresa evaluation are discussed in Coady (2000) and Skoufias (2005, ch. 3).) The 2000 presidential election took place on July 2nd. PRI was the incumbent party, and competed against a center-right coalition headed by PAN, a center-left coalition headed by PRD, and two smaller parties (PARM and PCD). The PAN-coalition was the winner in this contest, beating the PRI candidate by just over 6 percentage points.

	Rural	**Urban**	**Total**
Precinct Clusters	102	27	129
Precincts	351	71	422
Villages	2012	-	2012
Pairs	47	10	57

Table 1: The table summarizes precinct clusters included in the analysis of the electoral impact of SPS. Out of the original 110 rural clusters, we are able to map precincts for 102. Of these we are able to analyze 47 pairs. In the urban sample we are able to map 27 out of the original 38 clusters, and out of these we are left with 10 pairs. Urban clusters were formed from census tracts instead of villages.

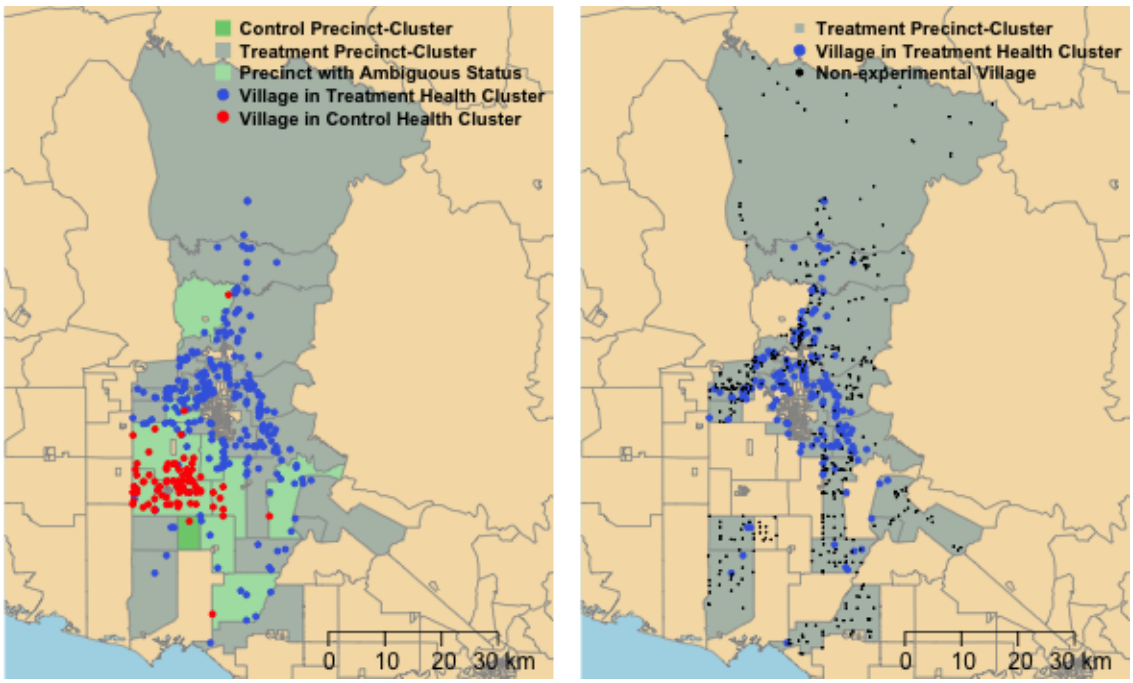

Figure 6: **Avoiding Contamination in Defining Rural Precinct Clusters.** The left panel maps village centroids from a control health cluster (red) and treatment health cluster (blue) in Sonora. Some precincts contain only treatment villages (gray areas with dark gray outlines); one precinct has only control villages (bright green area); and precincts in light green have treatment and control villages. We include in our analysis precincts with village centroids from only a single evaluation cluster. In the case of the treatment cluster, for example, we only keep precincts in grey. The right panel shows the final composition of the treatment precinct cluster in our analysis (the combined gray areas), which include villages from treatment clusters (blue dots), along with village centroids that were not part of the experiment (black dots).

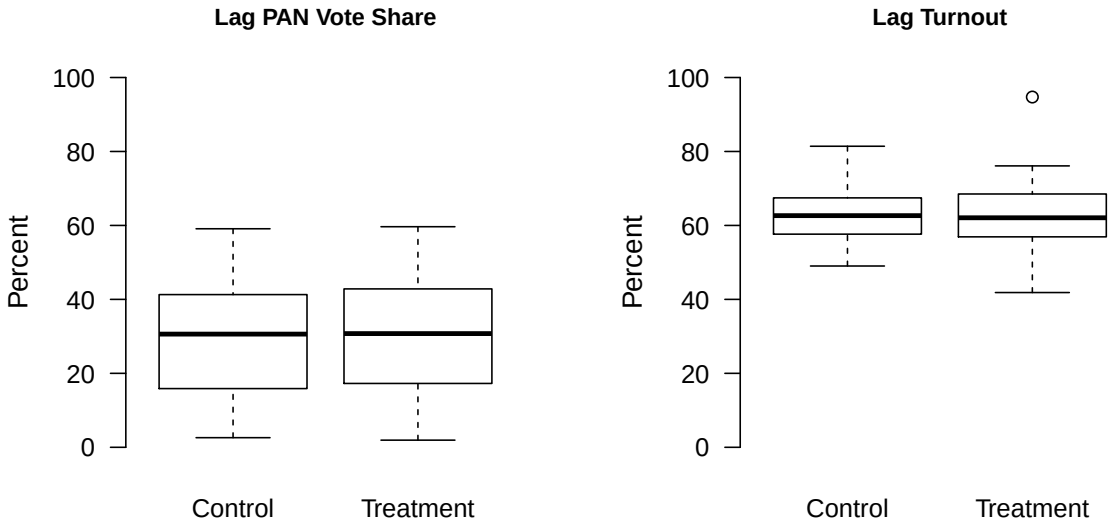

Figure 7: **Distribution of Pre-treatment Covariates Across Treatment Groups in SPS.** The figure shows fairly similar distributions across control and treatment groups in the distribution of lag outcomes. There is a precinct cluster in the treatment group reporting higher turnout, but dropping this observation from our analysis does not affect the main results in the paper.

	PAN (Vote Share)					PAN (Registered Voters)					PAN (Eligible Voters)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment	−0.897	−2.937	−2.616	−2.186	0.651	−0.887	−2.217	−1.860	−1.565	0.348	−0.206	−1.690	−0.963	−0.864	2.397
	(2.177)	(3.131)	(3.094)	(3.072)	(3.132)	(1.445)	(2.246)	(2.190)	(2.172)	(2.240)	(1.971)	(3.235)	(3.093)	(3.108)	(3.025)
Contaminated		1.774	2.437	2.931	4.341		−0.400	0.334	0.626	1.368		−1.552	−0.052	−0.177	1.577
		(3.370)	(3.343)	(3.250)	(3.466)		(2.202)	(2.116)	(2.046)	(2.322)		(2.710)	(2.460)	(2.505)	(2.596)
Contaminated ∗		4.308	3.578	2.626	−2.601		2.838	2.029	1.434	−1.080		3.191	1.539	1.616	−2.712
Treatment		(4.319)	(4.344)	(4.269)	(4.624)		(2.863)	(2.780)	(2.684)	(3.151)		(3.875)	(3.562)	(3.547)	(3.871)
Rural			−2.603	−1.858	0.002			−2.884	−2.388	−1.338			−5.890**	−5.799*	−2.119
			(2.872)	(2.785)	(5.202)			(2.093)	(2.046)	(5.247)			(2.921)	(2.995)	(6.707)
Log(Population)				2.960*	3.036				1.868*	1.099				−0.153	−1.325
				(1.676)	(2.051)				(1.079)	(1.481)				(1.485)	(2.250)
Number of Precincts				−0.332*	−0.425**				−0.237***	−0.245**				−0.120	−0.106
				(0.178)	(0.172)				(0.088)	(0.101)				(0.151)	(0.249)
Assets					40.562***					30.192**					44.193**
					(14.621)					(12.842)					(18.202)
Intercept	28.372***	27.555***	29.316***	6.963	−14.044	16.522***	16.706***	18.657***	4.617	−4.370	17.495***	18.209***	22.194***	23.676*	8.217
	(1.683)	(2.516)	(3.472)	(12.677)	(14.292)	(1.111)	(1.753)	(2.572)	(8.087)	(9.347)	(1.387)	(2.287)	(3.540)	(12.447)	(12.294)
Observations	129	129	129	129	90	129	129	129	129	90	129	129	129	129	90
R^2	−0.006	0.013	0.012	0.027	0.179	−0.005	−0.009	0.004	0.018	0.176	−0.008	−0.019	0.020	0.010	0.188
Note:															*p<0.1; **p<0.05; ***p<0.01

Table 2: **OLS estimates for ITT effect of SPS on Incumbent (PAN) Vote.** The table reports OLS estimates for the ITT effect of SPS on PAN support. PAN support is measured with the total votes it received in the 2006 election as a share of total votes cast (Columns 1-5), registered voters (Columns 6-10), and eligible voters (Columns 11-15). The estimates shows a null of effect of SPS on PAN support across all alternative measures of support and regression specifications. This result is robust to controlling for a cluster's demographic, whether a cluster contains at least one contaminated precinct, population, number of precincts per precinct cluster, and level of cluster assets.

	Turnout (Reg. Voters)					Turnout (Eligible Voters)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	−0.947	−2.006	−1.474	−1.142	0.182	0.239	−0.930	0.675	0.285	3.843
	(1.556)	(2.016)	(2.051)	(2.036)	(2.715)	(3.226)	(4.905)	(4.684)	(4.536)	(5.402)
Contaminated		−3.836*	−2.741	−2.711	−2.984		−5.934	−2.628	−3.761	−2.101
		(2.087)	(2.224)	(2.252)	(2.674)		(4.194)	(3.767)	(3.883)	(4.332)
Contaminated ∗		2.329	1.123	0.825	1.426		2.607	−1.034	0.677	−1.891
Treatment		(3.146)	(3.106)	(3.198)	(3.944)		(6.397)	(5.634)	(5.713)	(7.132)
Rural			−4.301*	−3.844	−3.233			−12.986***	−13.891***	−8.981
			(2.606)	(2.640)	(8.261)			(4.833)	(5.004)	(13.653)
Log(Population)				1.054	−0.903				−5.074**	−8.421**
				(1.274)	(1.882)				(2.201)	(3.631)
Number of Precincts				−0.321	−0.241				0.175	0.333
				(0.294)	(0.387)				(0.209)	(0.631)
Assets					11.919					44.571*
					(13.166)					(26.889)
Intercept	57.160***	58.926***	61.835***	54.354***	63.254***	60.816***	63.548***	72.333***	111.578***	111.062***
	(1.054)	(1.433)	(2.445)	(9.985)	(14.398)	(2.142)	(3.291)	(5.454)	(19.514)	(25.588)
Observations	129	129	129	129	90	129	129	129	129	90
R^2	0.003	0.030	0.068	0.105	0.130	0.00004	0.017	0.099	0.141	0.206
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 3: **OLS estimates for ITT effect of SPS on Turnout.** The table reports OLS estimates of the ITT effect of SPS on turnout. Turnout is measured with total votes cast as a share of registered voters (Columns 1-5) and eligible voters (Columns 6-10). The table show a null effect of SPS on turnout. The finding is robust to controlling for a cluster's demographic, whether a precinct cluster containts at least one contaminated precinct, population, and the number of precincts per precinct cluster.

	PAN (Vote Share)			PAN (Registered Voters)			PAN (Eligible Voters)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.038	0.655	−2.223	−0.265	0.340	−1.312	1.184	3.000	−0.519
	(3.805)	(4.593)	(5.395)	(2.420)	(3.164)	(3.426)	(3.303)	(4.484)	(4.179)
Contaminated		7.012	2.171		3.149	0.231		4.368	−1.415
		(6.279)	(7.529)		(3.767)	(4.632)		(4.003)	(4.609)
Contaminated ∗		1.195	6.822		−0.571	2.641		−4.088	2.811
Treatment		(8.133)	(9.371)		(4.848)	(5.677)		(6.035)	(5.484)
Log(Population)			−8.373			−4.507			−10.537**
			(5.944)			(3.887)			(4.423)
Number of Precincts			3.526			2.354			3.986**
			(2.421)			(1.549)			(1.567)
Intercept	28.596***	25.547***	82.017**	16.815***	15.446***	45.109*	17.381***	15.482***	87.278***
	(2.991)	(3.693)	(40.971)	(1.780)	(2.281)	(26.792)	(1.919)	(2.446)	(30.992)
Observations	50	50	50	50	50	50	50	50	50
R^2	0.00000	0.081	0.136	0.0003	0.028	0.076	0.003	0.019	0.120

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4: **OLS estimates for ITT effect of SPS on PAN Support (Validated Rural Sample).** The table reports OLS estimates for the ITT effect of SPS on PAN support in the set of precinct clusters where the aggregagted level of population from the merging procedure described in Appendix A is within 5 percentage points of the official precinct population. PAN support is measured with the total votes it received in the 2006 election as a share of total votes cast (Columns 1-3), registered voters (Columns 4-6), and eligible voters (Columns 7-9). The estimates shows a null of effect of SPS on PAN support across all alternative measures of support and regression specifications. This result is robust to controlling for whether precinct clusters have at least one contaminated precinct, population, and the number of precincts per precinct cluster.

	Turnout (Reg. Voters)			Turnout (Eligible Voters)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	−1.423	−2.086	−2.652	2.543	6.263	1.262
	(2.461)	(3.083)	(3.315)	(4.656)	(6.434)	(5.164)
Contaminated		−3.557	−4.827		1.500	−6.769
		(3.146)	(3.568)		(3.105)	(4.656)
Contaminated ∗		0.576	1.638		−11.854	−2.055
Treatment		(5.559)	(6.740)		(7.988)	(6.980)
Log(Population)			−0.952			−14.867***
			(4.117)			(5.457)
Number of Precincts			1.453			5.784**
			(2.264)			(2.703)
Intercept	58.377***	59.923***	64.647**	59.718***	59.066***	160.106***
	(1.541)	(2.091)	(28.061)	(1.550)	(2.418)	(37.692)
Observations	50	50	50	50	50	50
R^2	0.007	0.040	0.068	0.006	0.049	0.143
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 5: **OLS estimates for ITT effect of SPS on Turnout (Validated Rural Sample).** The table reports OLS estimates of the ITT effect of SPS on turnout in the set of precinct clusters where the aggregagted level of population from the merging procedure described in Appendix A is within 5 percentage points of the official precinct population. Turnout is measured with total votes cast as a share of registered voters (Columns 1-3) and eligible voters (Columns 4-6). The table show a null effect of SPS on turnout. The finding is robust to controlling for a precinct's demographic, whether a precinct cluster containts at least one contaminated precinct, population, and the number of precincts per precinct cluster.

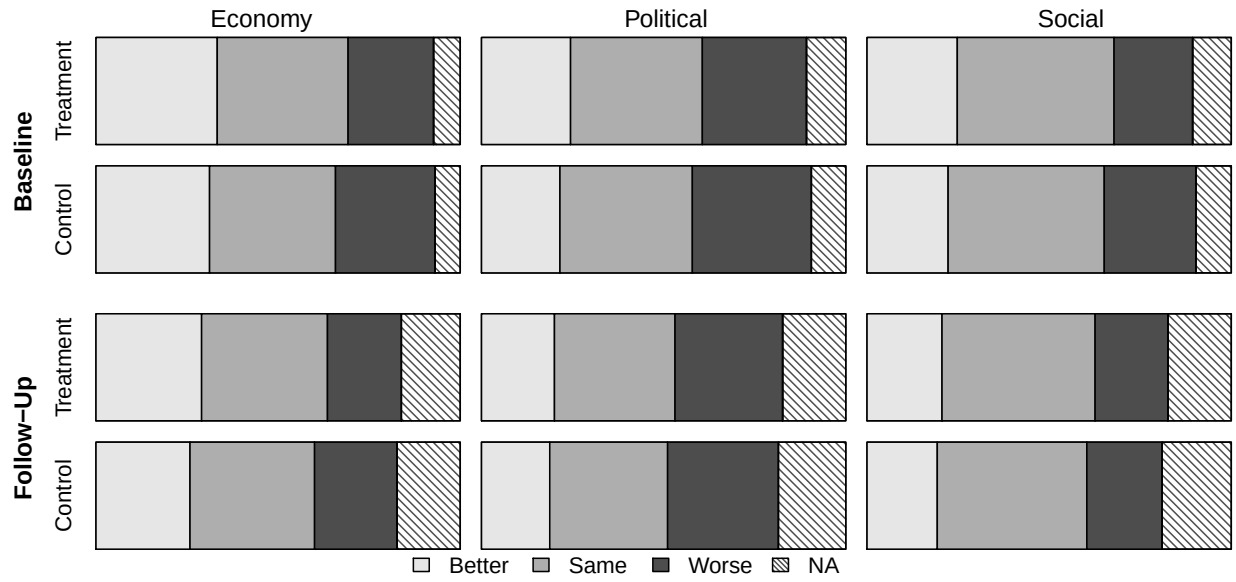

Figure 8: **Distribution of Baseline and Follow-Up Survey Responses to Economic, Political, and Social Retrospective Evaluations.** The figure displays barplots describing the very similar distribution of responses between treated and control groups in economic, political, and social retrospective evaluations of the country across treatment groups in both baseline and follow up surveys.

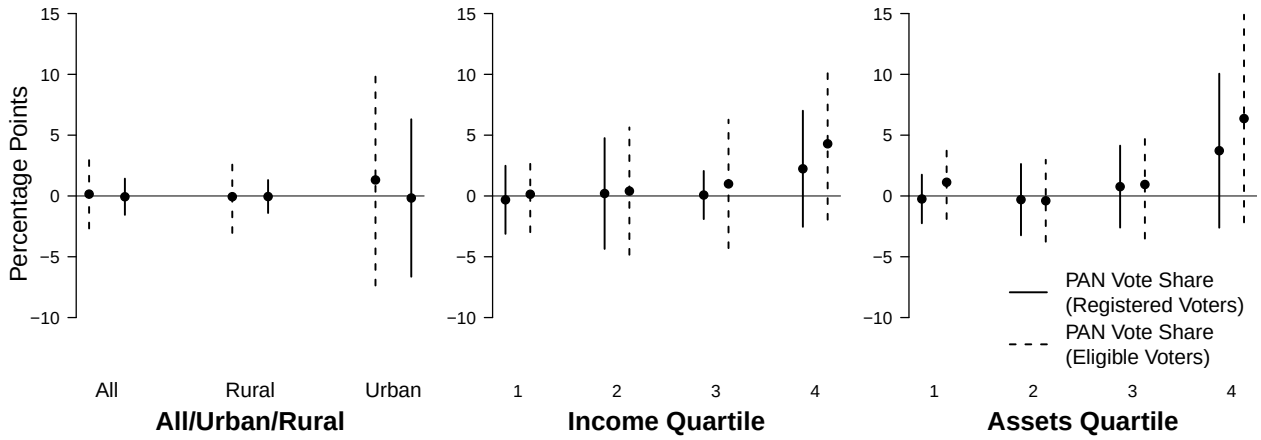

Figure 9: **ITT Estimates of SPS Effect on PAN Support (Votes as a Share of Registered and Eligible Voters).** This figure shows a null Intention-to-Treat (ITT) effect of SPS on PAN support measured with votes as a share of registered voters (vertical solid line) and eligible voters (vertical dashed line). The figure reports point estimates and 95 confidence intervals by cluster urbanicity (left panel), income quartile (middle panel), and household level of asset quartile (right panel).

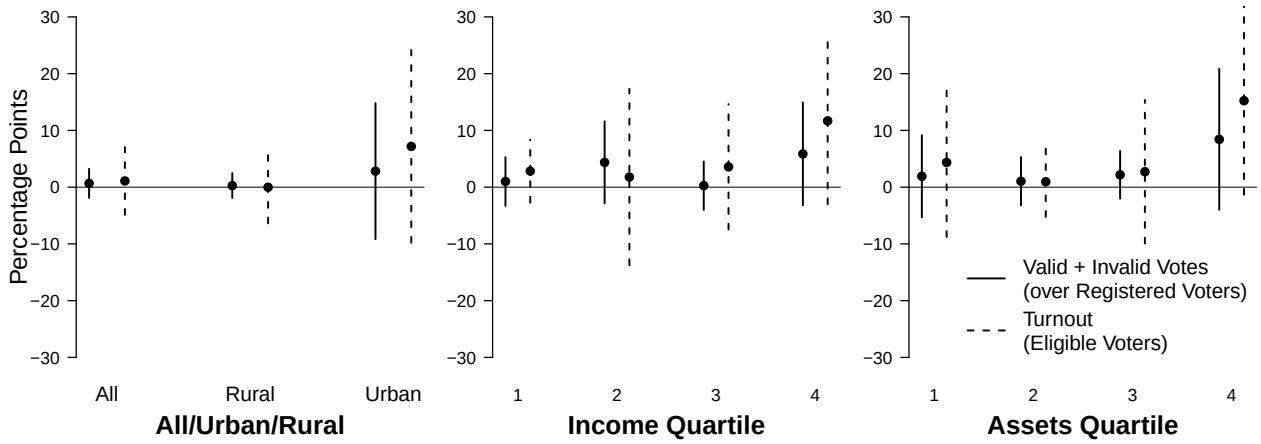

Figure 10: **ITT Estimates of SPS Effect on Alternative Measures of Turnout.** This figure shows a null Intention-to-Treat (ITT) effect of SPS on turnout measured with total valid and invalid votes cast as a share of registered voters, and total (valid) votes cast as a share of eligible voters. The figure reports point estimates and 95 confidence intervals by precinct cluster demographic (left panel), expected household policy usage (middle panel), and household level of income quartiles (right panel). The left panel shows null SPS effect of on turnout across the combined, rural, and urban precinct cluster samples. The center panel shows that the policy's effect does not vary by a household's expected usage of the insurance, and the right panel shows that it does not depend on a household's level of income.

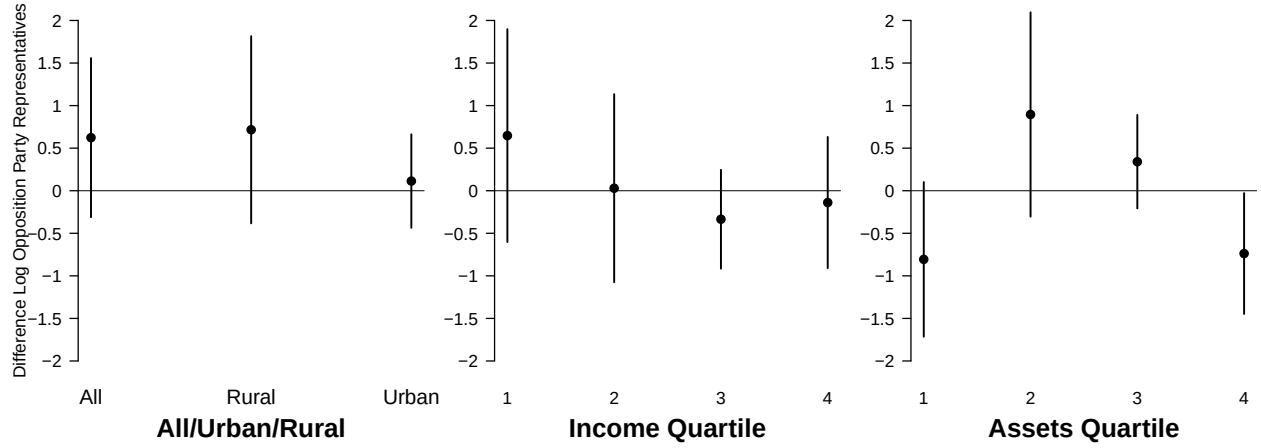

Figure 11: **ITT Estimates of SPS Effect on the Allocation of Opposition Party Resources.** This figure shows a null Intention-to-Treat (ITT) effect of SPS on the difference in the log of opposition party representatives across precinct-clusters. The figure reports point estimates and 95 confidence intervals by precinct cluster demographic (left panel), expected household policy usage (middle panel), and household level of income quartiles (right panel). The left panel shows null SPS effect of on the allocation of party resources across the combined, rural, and urban precinct cluster samples. The center panel shows that the policy's effect does not vary by a household's expected usage of the insurance, and the right panel shows that it does not depend on a household's level of income.

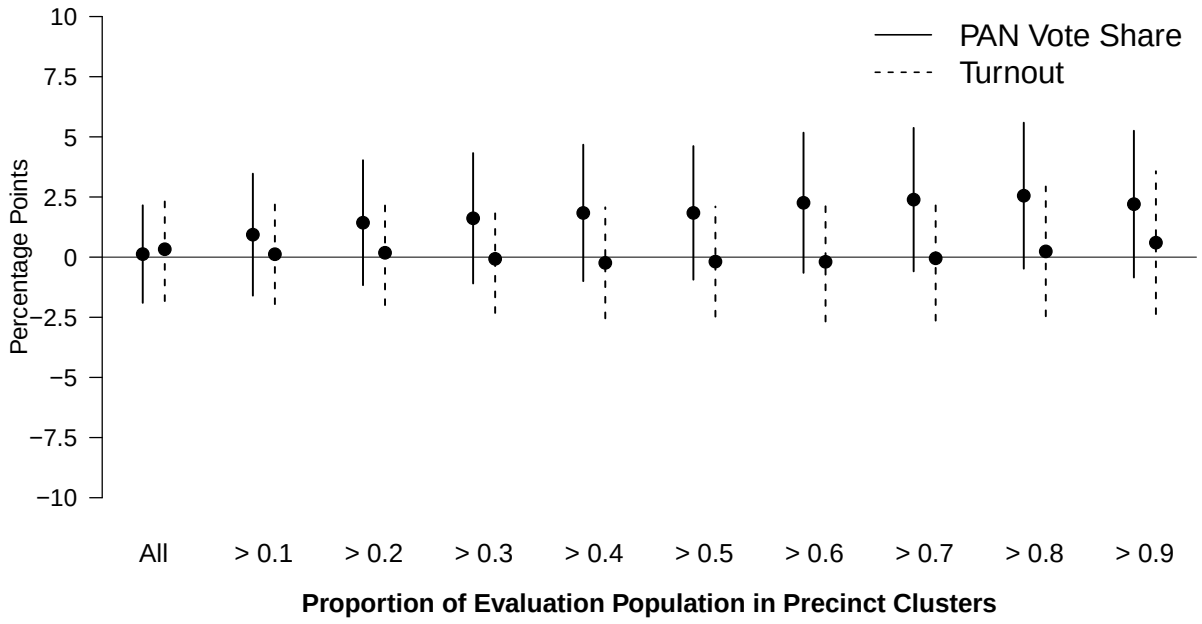

Figure 12: **ITT Estimates of SPS Effect on PAN Support and Turnout by Proportion of Evaluation Population in Precinct Clusters.** To address concerns of attenuation bias, the figure reports point estimates and 95 confidence intervals of the ITT effect of SPS on PAN support (vertical solid line) and turnout (vertical dashed line) by the share of evaluation population to total population across precinct clusters in rural areas. The figure shows a null effect of SPS even when include almost exclusively communities that participated in the evaluation.

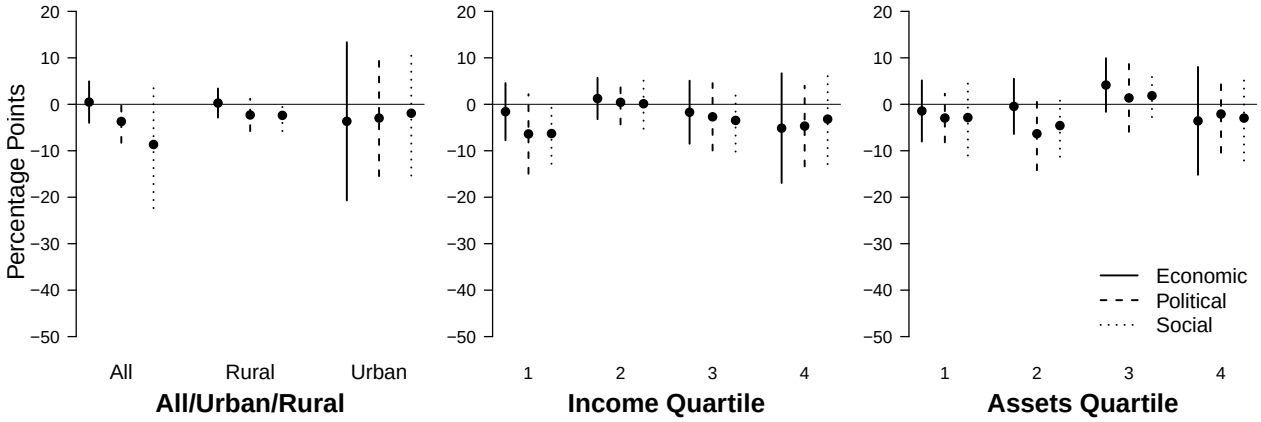

Figure 13: **Differences-in-Differences Estimates of SPS Effect on Survey Responses to Retrospective Evaluations.** The figure shows SPS did not have an effect on the proportion of respondents who reported the country was doing better than five years ago in economic, political, and social domains. The figure reports point estimates and 95 confidence intervals by cluster demographic (left panel), expected household policy usage (middle panel), and household level of income quartiles (right side panel). The left panel shows a null effect in the combined, rural, and urban cluster samples. The other two panels show that the effect does not vary by the expected household compliance with the policy (middle panel) or by the level of household income (right panel). This analysis excludes a matched-cluster pair in Guerrero in which the treatment cluster experienced a significant decline in the proportion of respondents reporting an improvement in the country's social conditions. Including this observation only increases the uncertainty of point estimates in the quartile analysis, but does not change the main substantive results.

	PAN (Vote Share)					PAN (Registered Voters)					PAN (Eligible Voters)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Enrollment	−0.030	−0.026	−0.024	−0.013	−0.016	−0.013	−0.009	−0.009	−0.002	−0.005	0.022	0.028	0.028	0.032	0.028
	(0.066)	(0.065)	(0.065)	(0.064)	(0.061)	(0.046)	(0.045)	(0.046)	(0.045)	(0.042)	(0.061)	(0.060)	(0.060)	(0.060)	(0.054)
Rural		−7.164	−7.817	−6.933	0.522		−7.093	−7.270	−6.789	−1.120		−10.632*	−10.693*	−10.525	−1.521
		(6.862)	(6.765)	(6.287)	(4.624)		(5.902)	(5.827)	(5.720)	(4.687)		(6.339)	(6.443)	(6.478)	(6.080)
Contaminated			2.933	2.567	2.904			0.797	0.530	0.786			0.279	0.005	0.412
			(2.485)	(2.406)	(2.302)			(1.701)	(1.680)	(1.585)			(2.221)	(2.252)	(2.028)
Log(Population)				4.421**	2.816				2.224*	1.003				0.287	−1.653
				(1.994)	(1.970)				(1.347)	(1.434)				(1.803)	(2.210)
Number of Precincts				−0.516***	−0.405***				−0.321**	−0.237***				−0.221**	−0.087
				(0.199)	(0.136)				(0.127)	(0.084)				(0.097)	(0.140)
Assets					39.266***					29.861**					47.427**
					(14.321)					(12.739)					(19.916)
Intercept	29.187***	35.684***	34.829***	1.717	−11.456	16.900***	23.332***	23.099***	6.636	−3.381	16.907***	26.547***	26.466***	24.902*	8.991
	(2.437)	(7.237)	(7.551)	(14.687)	(14.890)	(1.642)	(6.145)	(6.306)	(10.182)	(9.291)	(1.688)	(6.163)	(6.166)	(14.932)	(11.512)
Observations	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90
R^2	0.019	0.042	0.056	0.129	0.237	0.013	0.061	0.062	0.111	0.239	−0.022	0.039	0.039	0.053	0.247
Adjusted R^2	0.008	0.020	0.023	0.077	0.181	0.001	0.039	0.030	0.058	0.184	−0.034	0.017	0.006	−0.003	0.192
Note:															*p<0.1; **p<0.05; ***p<0.01

Table 6: **2SLS Estimates of the Impact of SPS Enrollment on Incumbent Support.** The table reports instrumental variable estimates of the impact of SPS enrollment on incumbent support. Enrollment is defined as the proportion of respondents registered in SPS at the cluster level as measured in the second wave of the SPS evaluation survey. The instrument in the first stage regression is a binary indicator of the treatment status of clusters. PAN support is measured with the total votes it received in the 2006 election as a share of total votes cast (Columns 1-5), registered voters (Columns 6-10), and eligible voters (Columns 11-15). The estimates show a null of effect of SPS enrollment on PAN support across all alternative measures of support and regression specifications. This result is robust to controlling for a cluster's demographic, whether a precinct cluster contains at least one contaminated precinct, population, number of precincts per precinct cluster, and level of cluster assets.

	Turnout (Reg. Voters)					Turnout (Eligible Voters)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enrollment	0.017	0.021	0.019	0.024	0.023	0.079	0.089	0.088	0.081	0.077
	(0.049)	(0.050)	(0.051)	(0.050)	(0.049)	(0.103)	(0.103)	(0.104)	(0.100)	(0.095)
Rural		−6.714	−6.313	−6.171	−3.500		−18.106	−17.499	−18.482	−8.493
		(7.222)	(7.611)	(7.613)	(7.523)		(12.083)	(12.956)	(12.345)	(12.516)
Contaminated			−1.806	−2.213	−2.092			−2.726	−2.945	−2.493
			(2.105)	(2.050)	(2.050)			(4.095)	(3.970)	(3.800)
Log(Population)				−0.237	−0.813				−6.622**	−8.774***
				(1.245)	(1.556)				(2.615)	(3.343)
Number of Precincts				−0.292*	−0.253				0.196	0.345
				(0.177)	(0.155)				(0.178)	(0.301)
Assets					14.065					52.618*
					(13.999)					(30.388)
Intercept	56.566***	62.654***	63.180***	65.950***	61.232***	58.231***	74.649***	75.444***	126.823***	109.171***
	(1.477)	(6.885)	(6.962)	(13.040)	(12.011)	(2.424)	(11.020)	(10.939)	(25.013)	(21.153)
Observations	90	90	90	90	90	90	90	90	90	90
R^2	−0.008	0.031	0.042	0.102	0.128	−0.043	0.020	0.027	0.099	0.186
Adjusted R^2	−0.019	0.009	0.008	0.049	0.065	−0.055	−0.003	−0.007	0.046	0.127
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 7: **2SLS Estimates of the Impact of SPS Enrollment on Turnout.** The table reports instrumental variable estimates of the impact of SPS enrollment on incumbent support. Enrollment is defined as the proportion of respondents registered in SPS at the cluster level as measured in the second wave of the SPS evaluation survey. The instrument in the first stage regression is a binary indicator of the treatment status of clusters. Turnout is measured with total votes cast as a share of registered voters (Columns 1-5) and eligible voters (Columns 6-10). The estimates show a null of effect of SPS enrollment on turnout. This result is robust to controlling for a cluster's demographic, whether a precinct cluster contains at least one contaminated precinct, population, number of precincts per precinct cluster, and level of cluster assets.

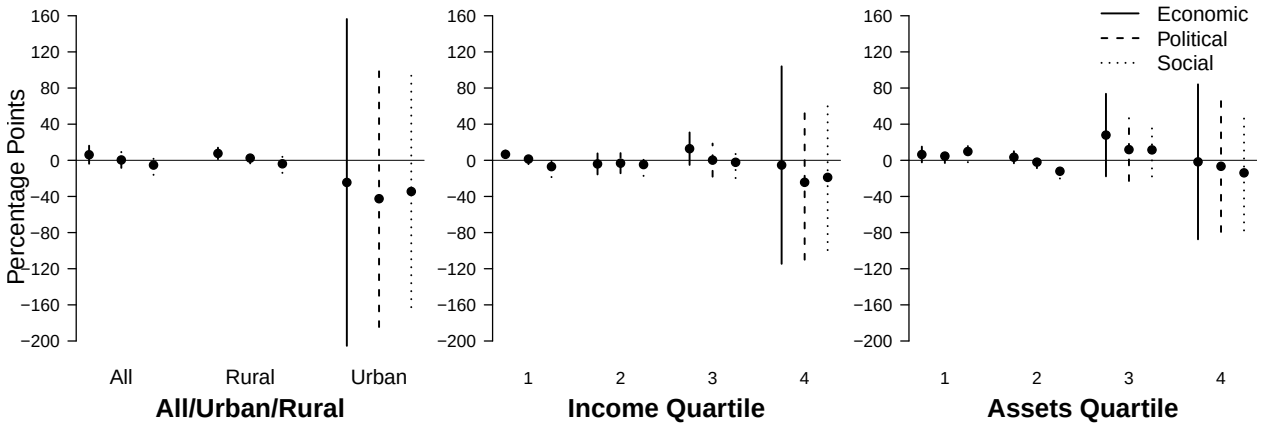

Figure 14: **Complier Average Causal Effect (CACE) Estimates of SPS on Retrospective Survey Evaluations.** The figure reports point estimates and 95% confidence intervals of the CACE of SPS on economic (solid vertical lines), political (dashed), and social (dotted) retrospective evaluations of whether 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).

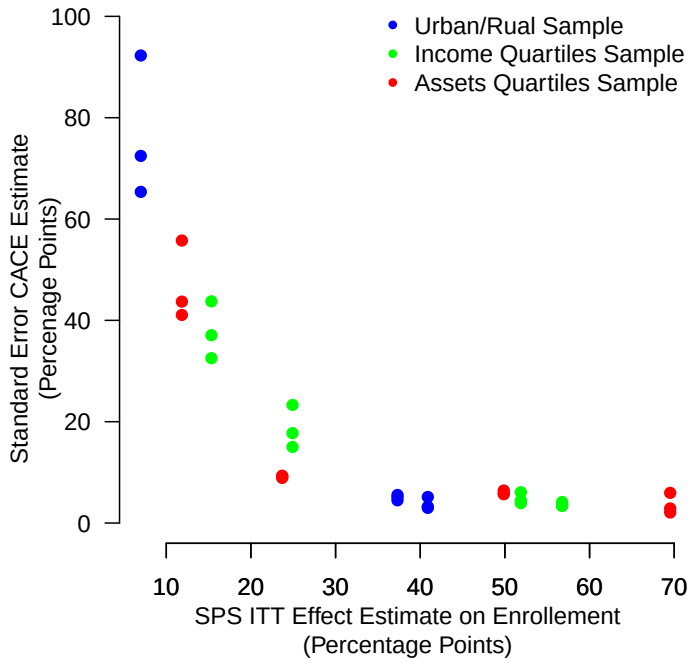

Figure 15: **Standard Errors of CACE Estimates of SPS on Retrospective Evaluations vs. ITT Effect on Enrollment.** Each dot represents the standard error of the SPS CACE estimate on retrospective evaluations and the ITT estimates of the insurance registration encouragement on enrollment across the samples analyzed in Figure 14 (Urban/Rural, Income Quartiles, and Assets Quartiles). The figure shows a negative relationship between the CACE standard errors and the ITT estimates on enrollment. That is, the strata where the SPS registration encouragement had a low impact on enrollment rates reports larger standard erros of SPS CACE on retrospective survey evaluations.

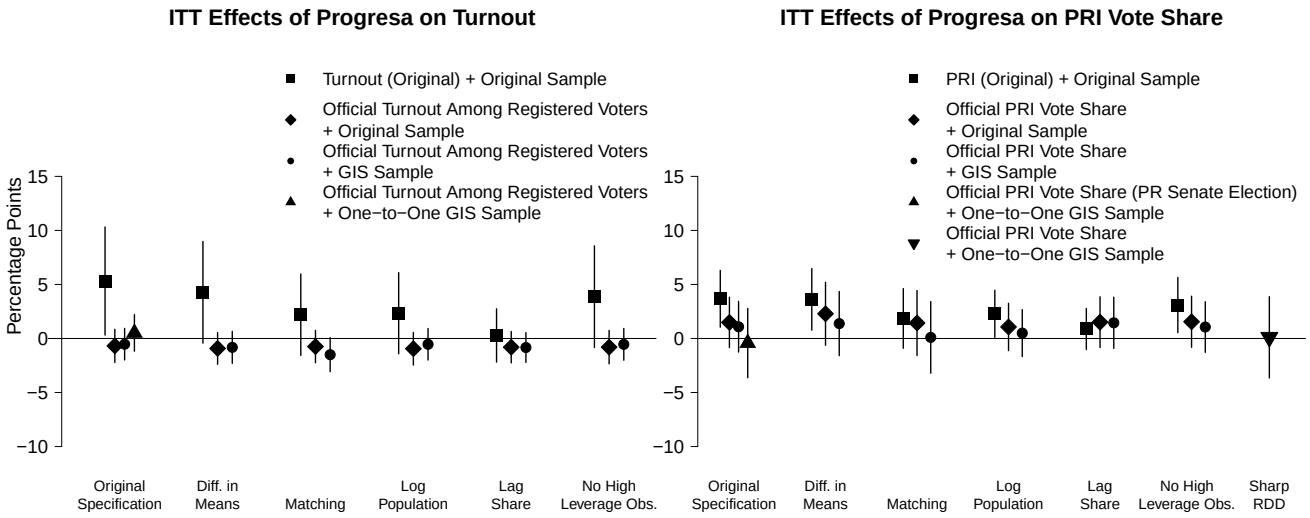

Figure 16: **Intention to Treat Estimates of *Progresa* Effect on Turnout and Incumbent Party Vote.** The left panel reports point estimates and 90% confidence intervals for the total causal effect of *Progresa* on turnout in the 2000 presidential election as originally, and incorrectly, measured in the De La O (2013) sample (squares), for official turnout among registered voters in the same sample (rhombuses), and for official turnout among registered voters in the correct GIS sample (dots) for several different specifications. The panel also replicates Green (2006)'s total causal effect of *Progresa* on turnout in the sample of precincts with only one village under a sharp RD design (triangle). The right panel repeats the same analyses for incumbent (PRI) vote share, including additionally the effect of *Progresa* on both PRI support in the 2000 Proportional Representation (PR) Senate election (triangle), as in Green (2006), and in the presidential election (inverted triangle) under a sharp RDD. Every estimate is indistinguishable from zero, except when using the flawed original measure used in De La O (2013) under the original specification, without controls, and dropping the observations with the highest leverage (first, second, and fourth lines with squares representing the point estimates in the right panel).

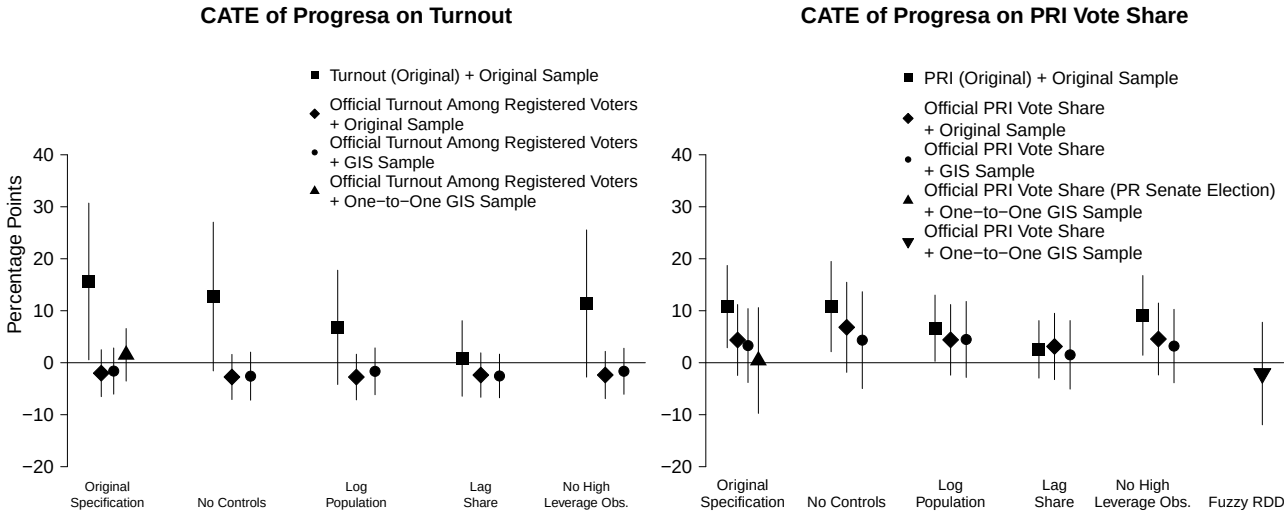

Figure 17: **Complier Average Treatment Estimates of *Progresa* Effect on Turnout and Incumbent Party Vote.** In a manner directly parallel to Figure 16, this figure replicates the instrumental variable estimation from De La O (2013) and the fuzzy RD design from Green (2006). Every estimate is indistinguishable from zero, except when using the wrong measure under the original specification, without controls, and dropping the observations with the highest leverage (first, second, and fourth dotted lines in the right panel).

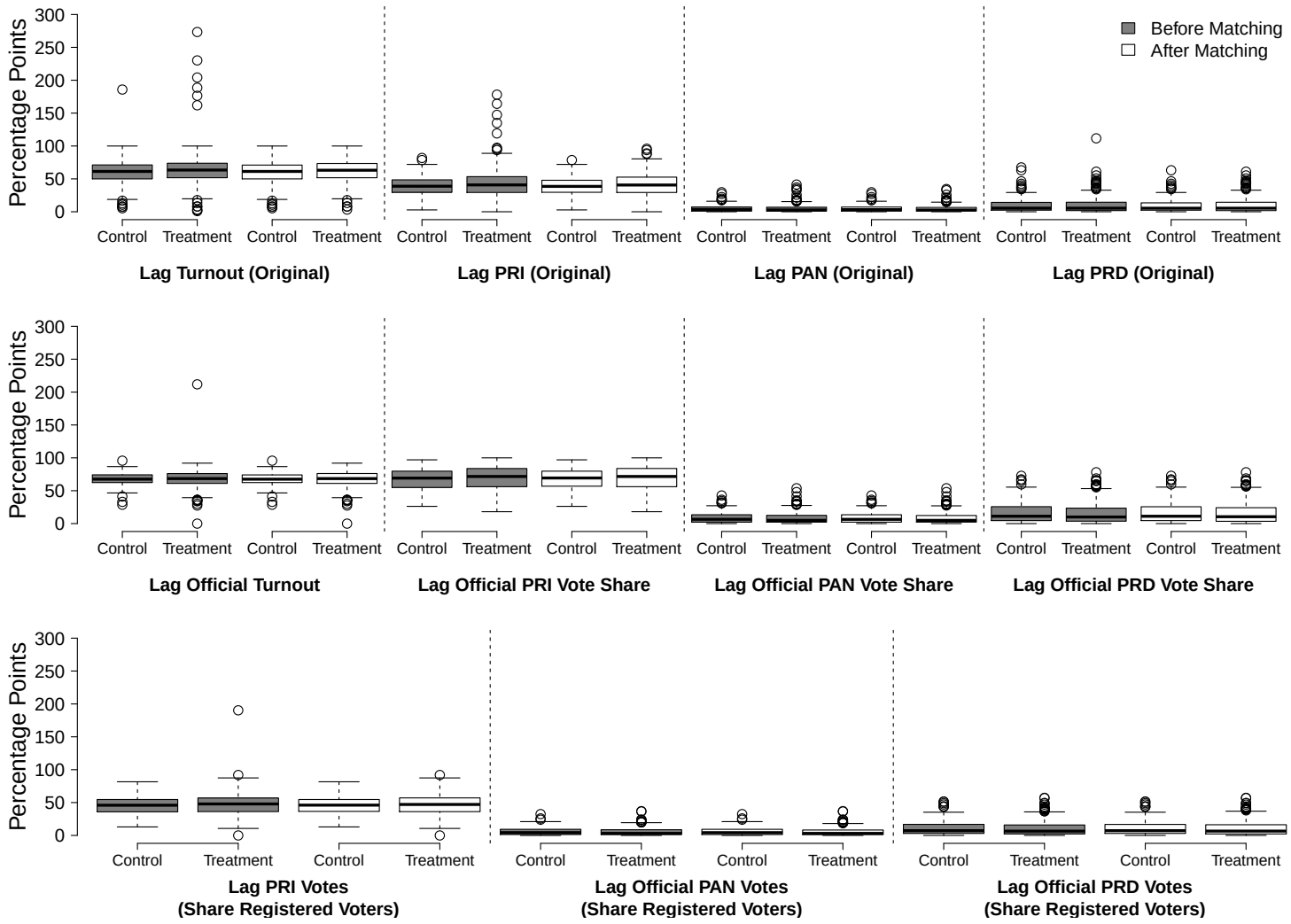

Figure 18: **Balance in Lag Outcomes Before and After Matching.** The figure shows the presence of significant imbalance in the lag outcomes analyzed in De La O (2013, 2015). In particular, the treatment group has several outliers in lag turnout and PRI support (left pair of each panel). This imbalance disappears after Coarsened Exact Matching (CEM) on population and lag outcomes (right pair in each panel).

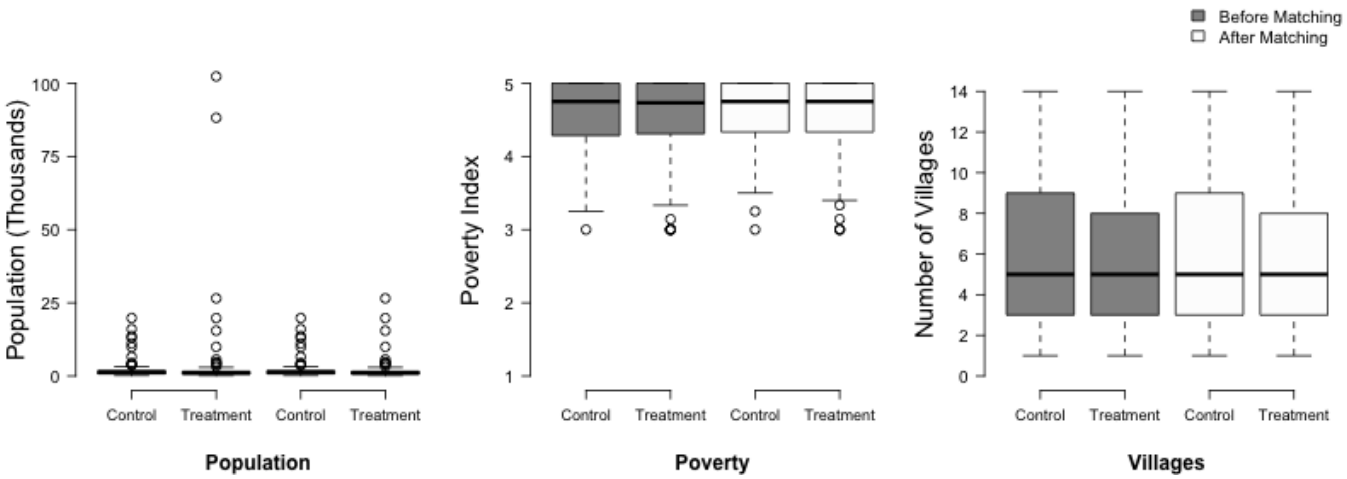

Figure 19: **Balance in Pre-Treatment Socio-Economic Covariates Before and After Matching.** The figure shows the presence of significant imbalance in the pre-treatment covariates in De La O (2013, 2015). In particular, the treatment group has several large population outliers. After Coarsened Exact Matching (CEM) on population and lag outcomes, without affecting the balance in poverty and number of villages across precincts.

	Turnout (Original)						Official Turnout Among Registered Voters					
	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	5.316*	4.270	2.199	2.337	0.278	3.866	−0.685	−0.920	−0.747	−0.947	−0.805	−0.802
	(3.040)	(2.860)	(2.291)	(2.282)	(1.499)	(2.865)	(0.935)	(0.893)	(0.912)	(0.918)	(0.884)	(0.941)
Avg. Poverty	1.401			2.047	0.479	1.198	−1.943**			−1.892**	−0.935	−1.959**
	(3.575)			(3.057)	(2.217)	(3.375)	(0.978)			(0.949)	(1.400)	(0.973)
Population_{1994}	−0.001				0.000	−0.006***	0.000				0.000	0.000
	(0.001)				(0.000)	(0.001)	(0.000)				(0.000)	(0.000)
$\text{Log(Population}_{1994}\text{)}$				−34.962***						−3.092***		
				(5.008)						(0.671)		
Tot. Votes_{1994}	−0.027			0.062***		−0.025	−0.025**			−0.017*		−0.025**
	(0.024)			(0.022)		(0.021)	(0.010)			(0.009)		(0.010)
Turnout_{1994} (Original)					0.958***							
					(0.128)							
Turnout_{1994} (Registered)											0.225*	
											(0.118)	
PRI Votes_{1994}	0.036			0.028	−0.019***	0.044*	0.033***			0.032***	0.002	0.033***
	(0.027)			(0.023)	(0.007)	(0.025)	(0.011)			(0.010)	(0.003)	(0.011)
PAN Votes_{1994}	0.040			0.065	0.021	0.112*	0.044***			0.046***	0.018*	0.049***
	(0.067)			(0.049)	(0.018)	(0.064)	(0.016)			(0.015)	(0.010)	(0.016)
PRD Votes_{1994}	0.025			0.027	−0.033***	0.053	0.024**			0.024**	−0.005	0.027**
	(0.036)			(0.030)	(0.011)	(0.034)	(0.012)			(0.011)	(0.005)	(0.012)
Intercept	61.925***	63.815***		255.681***	13.386	63.564***	67.024***	58.060***		84.180***	47.164***	67.157***
	(17.531)	(2.006)		(23.575)	(15.668)	(16.559)	(5.005)	(0.732)		(6.258)	(13.927)	(4.982)
Village FE	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	417	417	408	417	417	415	417	417	408	417	417	415
R^2	0.116	0.004	-	0.418	0.712	0.197	0.072	0.003	-	0.104	0.173	0.080
Adjusted R^2	0.071	0.002	-	0.388	0.697	0.156	0.025	0.0002	-	0.058	0.131	0.033
RMSE	31.005	30.965	-	25.033	18.011	29.067	8.616	8.554	-	8.47	8.358	8.614

Note: *p<0.1; **p<0.05; ***p<0.01

Table 8: **ITT Estimates of *Progresa* on Turnout**. The table reports a null ITT effect of *Progresa* on turnout. Turnout is measured as in De La O (2013, 2015) (Columns 1-6) and as a share of registered voters (Column 7-12). Column 1 reports the original positive effect found in De La O (2013, 2015). However, this estimate is not robust under a difference-in-means approach (Column 2), matching (Column 3), or under the original regression specification but controlling for log of population (Column 4), lag turnout in ratio (Column 5), or removing the two observations with the largest leverage (Column 6). Similarly, the table reports a null effect when relying on official turnout as the outcome of interest. This estimate is robust across all specifications (Columns 7-12).

	PRI (Original)						Official PRI Vote Share					
	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	3.671**	3.622**	1.853	2.282*	0.882	3.090**	1.486	2.286	1.430	1.511	1.066	1.548
	(1.598)	(1.733)	(1.679)	(1.326)	(1.156)	(1.551)	(1.419)	(1.776)	(1.829)	(1.429)	(1.331)	(1.438)
Avg. Poverty	2.854			3.080**	3.866***	2.774	4.835***			4.851***	5.005***	4.846***
	(1.763)			(1.560)	(1.348)	(1.715)	(1.641)			(1.626)	(1.527)	(1.641)
Population_{1994}	−0.001***				0.000	−0.002**	0.000				0.000	0.000
	(0.000)				(0.000)	(0.001)	(0.000)				(0.000)	(0.000)
$\text{Log(Population}_{1994}\text{)}$				−16.476***						0.344		
				(2.155)						(1.137)		
Tot. Votes_{1994}	−0.031*			0.012	−0.008***	−0.030**	−0.019			−0.020	−0.006**	−0.020
	(0.016)			(0.013)	(0.003)	(0.015)	(0.019)			(0.019)	(0.003)	(0.019)
PRI Votes_{1994}	0.058***			0.055***		0.061***	0.055**			0.055**		0.054**
	(0.019)			(0.015)		(0.017)	(0.022)			(0.022)		(0.022)
PAN Votes_{1994}	−0.047			−0.034		−0.018	−0.119***			−0.120***		−0.123***
	(0.038)			(0.029)		(0.037)	(0.030)			(0.030)		(0.031)
PRD Votes_{1994}	−0.025			−0.024		−0.013	−0.068***			−0.068***		−0.069***
	(0.019)			(0.015)		(0.018)	(0.021)			(0.022)		(0.022)
PRI Votes_{1994} (Share Original)					0.593***							
					(0.080)							
PAN Votes_{1994} (Share Original)					0.014							
					(0.150)							
PRD Votes_{1994} (Share Original)					0.050							
					(0.087)							
PRI Votes_{1994} (Share Official)											0.314***	
											(0.100)	
PAN Votes_{1994} (Share Official)											−0.481***	
											(0.143)	
PRD Votes_{1994} (Share Official)											−0.252**	
											(0.106)	
Intercept	35.680***	34.489***		127.327***	6.198	36.338***	49.936***	56.263***		47.934***	31.902**	49.870***
	(8.560)	(1.266)		(11.896)	(8.470)	(8.308)	(8.091)	(1.410)		(10.510)	(13.609)	(8.097)
Village FE	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	417	417	408	417	417	415	417	417	408	417	416	415
R^2	0.288	0.009	-	0.484	0.609	0.319	0.444	0.004	-	0.444	0.541	0.443
Adjusted R^2	0.252	0.006	-	0.458	0.590	0.285	0.416	0.001	-	0.416	0.518	0.415
RMSE	16.371	18.29	-	13.921	12.278	15.847	13.737	17.573	-	13.738	12.482	13.782
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 9: **ITT Estimates of *Progresa* on PRI Vote Share**. The table reports a null ITT effect of *Progresa* on PRI vote share. PRI vote share is measured as in De La O (2013, 2015) (Columns 1-6) and as a share of total votes cast (Column 7-12). Column 1 reports the original positive effect found in De La O (2013, 2015). However, this estimate is not robust under matching (Column 3), or under the original regression specification but controlling for lag PRI vote share (Column 5). Similarly, we find a null effect of *Progresa* on incumbent support when relying on official PRI vote share as the outcome of interest. This result is robust across all specifications (Columns 7-12).

	PRI (Official Registered Voters)					
	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)
Intention to	0.505	0.742	0.403	0.370	0.121	0.475
Treat (ITT)	(0.940)	(1.103)	(1.127)	(0.940)	(0.865)	(0.948)
Avg. Poverty	1.471			1.510	1.909	1.468
	(1.078)			(1.060)	(1.164)	(1.079)
Population_{1994}	0.000				0.000	0.000
	(0.000)				(0.000)	(0.000)
$\text{Log(Population}_{1994})$				-1.557^{**}		
				(0.657)		
Tot. Votes_{1994}	-0.023^{**}			-0.019^{*}	-0.002	-0.023^{**}
	(0.011)			(0.010)	(0.002)	(0.011)
PRI Votes_{1994}	0.048^{***}			0.047^{***}		0.048^{***}
	(0.012)			(0.012)		(0.012)
PAN Votes_{1994}	-0.048^{***}			-0.047^{***}		-0.047^{***}
	(0.017)			(0.017)		(0.018)
PRD Votes_{1994}	-0.028^{**}			-0.028^{**}		-0.027^{**}
	(0.012)			(0.012)		(0.013)
PRI Votes_{1994}					0.244^{***}	
(Share Registered)					(0.073)	
PAN Votes_{1994}					-0.305^{***}	
(Share Registered)					(0.096)	
PRD Votes_{1994}					-0.242^{***}	
(Share Registered)					(0.067)	
Intercept	35.460^{***}	32.505^{***}		44.041^{***}	24.291^{***}	35.495^{***}
	(5.398)	(0.881)		(6.762)	(9.130)	(5.403)
Village FE	Yes	No	No	Yes	Yes	Yes
Observations	417	417	408	417	417	415
R^2	0.379	0.001	-	0.383	0.465	0.377
Adjusted R^2	0.347	-0.001	-	0.352	0.438	0.346
RMSE	8.936	10.841	-	8.907	8.381	8.96
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 10: **ITT Estimates of *Progresa* on PRI Vote Share (Registered Voters)**. The table reports a null ITT effect of *Progresa* on PRI vote share. PRI vote share is measured as a share of registered voters. This finding is robust across all regression specifications.

	Turnout (Original)					Official Turnout Among Registered Voters				
	Original Specification	No Covariates	Log Population	Share	Leverage	Original Specification	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	15.630*	12.730	6.803	0.819	11.390	−2.013	−2.743	−2.757	−2.372	−2.362
	(9.163)	(8.697)	(6.682)	(4.418)	(8.607)	(2.744)	(2.642)	(2.672)	(2.598)	(2.765)
Avg. Poverty	−1.856		0.673	0.300	−1.173	−1.524		−1.335	−0.425	−1.467
	(3.379)		(3.035)	(2.429)	(3.139)	(1.095)		(1.041)	(1.435)	(1.089)
Population_{1994}	−0.001*			0.00003	−0.006***	−0.0001*			−0.0001	−0.0004**
	(0.001)			(0.0002)	(0.001)	(0.0001)			(0.00004)	(0.0002)
$\text{Log(Population}_{1994}\text{)}$			−35.146***					−3.017***		
			(5.017)					(0.675)		
Tot. Votes_{1994}	−0.038		0.058***		−0.032	−0.024**		−0.016*		−0.024**
	(0.026)		(0.022)		(0.024)	(0.010)		(0.009)		(0.010)
Turnout_{1994} (Original)				0.959***						
				(0.128)						
Turnout_{1994} (Registered)									0.224*	
									(0.117)	
PRI Votes_{1994}	0.046		0.033	−0.019***	0.051*	0.031***		0.030***	0.002	0.032***
	(0.029)		(0.023)	(0.007)	(0.027)	(0.011)		(0.010)	(0.003)	(0.011)
PAN Votes_{1994}	0.061		0.073	0.022	0.127*	0.041**		0.042***	0.016	0.046***
	(0.069)		(0.051)	(0.018)	(0.066)	(0.017)		(0.016)	(0.011)	(0.017)
PRD Votes_{1994}	0.033		0.030	−0.033***	0.059*	0.023*		0.023**	−0.005	0.025**
	(0.037)		(0.031)	(0.011)	(0.036)	(0.012)		(0.012)	(0.005)	(0.012)
Intercept	70.197***	57.112***	260.125***	13.850	69.584***	65.959***	59.504***	82.379***	45.897***	65.909***
	(16.170)	(6.250)	(24.155)	(15.593)	(15.114)	(4.962)	(2.028)	(6.184)	(13.730)	(4.934)
Village FE	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	417	417	417	417	415	417	417	417	417	415
R^2	0.095	−0.009	0.414	0.711	0.183	0.074	0.013	0.104	0.175	0.082
Adjusted R^2	0.049	−0.011	0.384	0.697	0.141	0.027	0.011	0.058	0.133	0.035
RMSE	31.306	31.181	25.107	18.024	29.316	8.612	8.512	8.472	8.348	8.607
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 11: **Instrumental Variable Estimates of *Progresa* on Turnout**. The table reports a null effect of *Progresa* on turnout under an instrumental variable approach. Turnout is measured as in De La O (2013, 2015) (Columns 1-5) and as a share of registered voters (Column 6-10). Column 1 reports the original positive effect found in De La O (2013, 2015). However, this estimate is not robust when not including pre-treatment covariates in the first and second stage regressions (Column 2), or under the original regression specifications but controlling for log of population (Column 3), lag turnout in ratio (Column 4), or removing the two observations with the largest leverage (Column 5). Similarly, the table reports a null effect when relying on official turnout as the outcome of interest. This result is robust across all specifications (Columns 6-10).

	PRI (Original)					Official PRI Vote Share				
	Original Specification	No Covariates	Log Population	Share	Leverage	Original Specification	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	10.794**	10.800**	6.644*	2.571	9.105*	4.370	6.814	4.399	3.129	4.560
	(4.813)	(5.281)	(3.871)	(3.362)	(4.662)	(4.146)	(5.268)	(4.130)	(3.880)	(4.206)
Avg. Poverty	0.605		1.739	3.288**	0.879	3.925**		3.963**	4.338**	3.897**
	(1.897)		(1.707)	(1.531)	(1.849)	(1.843)		(1.812)	(1.704)	(1.846)
Population_{1994}	−0.001**			−0.00005	−0.002***	−0.00001			−0.00003	0.0002
	(0.0003)			(0.0001)	(0.001)	(0.0001)			(0.0001)	(0.0004)
$\text{Log(Population}_{1994})$			−16.655***					0.225		
			(2.127)					(1.120)		
Tot. Votes_{1994}	−0.038**		0.008	−0.009***	−0.036**	−0.022		−0.023	−0.006**	−0.023
	(0.017)		(0.013)	(0.003)	(0.016)	(0.019)		(0.019)	(0.003)	(0.019)
PRI Votes_{1994}	0.065***		0.059***		0.067***	0.057***		0.057***		0.057***
	(0.020)		(0.016)		(0.019)	(0.022)		(0.022)		(0.022)
PAN Votes_{1994}	−0.032		−0.026		−0.007	−0.114***		−0.115***		−0.117***
	(0.038)		(0.029)		(0.038)	(0.030)		(0.030)		(0.030)
PRD Votes_{1994}	−0.019		−0.020		−0.008	−0.066***		−0.066***		−0.067***
	(0.020)		(0.016)		(0.019)	(0.021)		(0.021)		(0.022)
PRI Votes_{1994} (Share Eligible)				0.593***						
				(0.080)						
PAN Votes_{1994} (Share Eligible)				0.021						
				(0.149)						
PRD Votes_{1994} (Share Eligible)				0.050						
				(0.088)						
PRI Votes_{1994} (Share Official)									0.323***	
									(0.099)	
PAN Votes_{1994} (Share Official)									−0.463***	
									(0.144)	
PRD Votes_{1994} (Share Official)									−0.244**	
									(0.104)	
Intercept	41.393***	28.802***	131.667***	7.715	41.151***	52.249***	52.675***	50.808***	32.729**	52.280***
	(8.404)	(3.881)	(12.164)	(8.512)	(8.157)	(8.122)	(3.992)	(10.387)	(13.426)	(8.123)
Village FE	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	417	417	417	417	415	417	417	417	416	415
R^2	0.275	0.006	0.482	0.611	0.309	0.451	0.026	0.451	0.547	0.451
Adjusted R^2	0.238	0.003	0.456	0.591	0.274	0.424	0.024	0.424	0.524	0.423
RMSE	16.499	18.33	13.938	12.266	15.966	13.657	17.383	13.653	12.416	13.688
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 12: **Instrumental Variable Estimates of *Progresa* on PRI Vote Share**. The table reports a null effect of *Progresa* on incumbent support under an instrumental variable approach. PRI vote share is measured as in De La O (2013, 2015) (Columns 1-5) and as a share of total vote cast (Column 6-10). Column 1 reports the original positive effect found in De La O (2013, 2015). However, this estimate is not robust under the original regression specifications but controlling for lag PRI vote share (Column 4). Similarly, the table reports a null effect when relying on official PRI vote share as the outcome of interest. This finding is robust across all specifications (Columns 6-10).

	PRI (Official Registered Voters)				
	Original Specification	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)
Early *Progresa*	1.484	2.212	1.077	0.354	1.401
	(2.754)	(3.284)	(2.728)	(2.537)	(2.784)
Avg. Poverty	1.161		1.293	1.834	1.176
	(1.175)		(1.146)	(1.235)	(1.178)
Population_{1994}	-0.0001			-0.0001^{*}	-0.0001
	(0.00005)			(0.00003)	(0.0002)
$\text{Log(Population}_{1994})$			-1.586^{**}		
			(0.655)		
Tot. Votes_{1994}	-0.023^{**}		-0.019^{*}	-0.002	-0.024^{**}
	(0.011)		(0.010)	(0.002)	(0.011)
PRI Votes_{1994}	0.048^{***}		0.048^{***}		0.049^{***}
	(0.012)		(0.012)		(0.012)
PAN Votes_{1994}	-0.046^{**}		-0.045^{***}		-0.045^{**}
	(0.018)		(0.018)		(0.018)
PRD Votes_{1994}	-0.027^{**}		-0.027^{**}		-0.027^{**}
	(0.012)		(0.012)		(0.013)
PRI Votes_{1994}				0.244^{***}	
(Share Registered)				(0.073)	
PAN Votes_{1994}				-0.303^{***}	
(Share Registered)				(0.098)	
PRD Votes_{1994}				-0.242^{***}	
(Share Registered)				(0.067)	
Intercept	36.245^{***}	31.340^{***}	44.744^{***}	24.465^{***}	36.235^{***}
	(5.379)	(2.493)	(6.725)	(9.051)	(5.385)
Village FE	Yes	No	Yes	Yes	Yes
Observations	417	417	417	417	415
R^2	0.382	0.010	0.386	0.466	0.381
Adjusted R^2	0.351	0.007	0.355	0.439	0.350
RMSE	8.912	10.797	8.888	8.377	8.936
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 13: **Instrumental Variable Estimates of *Progresa* Effect on PRI Vote Share (Registered Voters)**. The table reports a null effect of *Progresa* on PRI vote share under an instrumental variable approach. PRI vote share is measured as a share of registered voters. This finding is robust across all regression specifications.

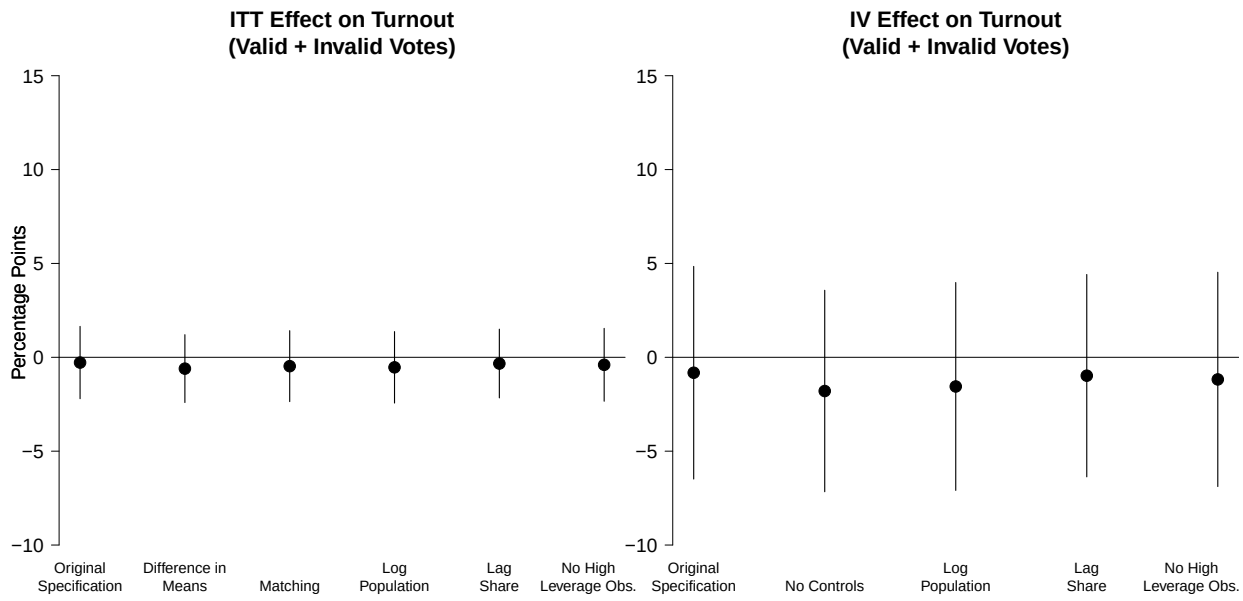

Figure 20: **Intent-to-treat and Instrumental Variable Causal Effects of *Progresa* on Alternative Measure of Turnout.** The left panel reports ITT point estimates and 95% confidence intervals of the causal effect of *Progresa* on turnout, measured as total votes cast (valid and invalid) over the number of registered voters, for several different specifications. The right panel reports instrumental variable estimates relying on the same measure of turnout.

	Registration (Females)						Registration (Male)					
	Counts	Diff. in Means	Matching	Log Population	Share	Leverage	Counts	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	−0.267	4.154*	2.047	−0.342	0.012	−0.335	−0.351	4.418*	2.077	−0.381	−0.034	−0.386
	(1.156)	(2.450)	(1.787)	(1.143)	(1.112)	(1.155)	(1.058)	(2.393)	(1.793)	(1.048)	(1.020)	(1.060)
Avg. Poverty	1.665			1.734	2.683**	1.651	0.459			0.503	1.945*	0.463
	(1.288)			(1.248)	(1.226)	(1.289)	(1.201)			(1.172)	(1.182)	(1.202)
Population_{1994}	0.000				0.000	0.000	0.000				0.000	0.000
	(0.000)				(0.000)	(0.000)	(0.000)				(0.000)	(0.000)
$\text{Log(Population}_{1994}\text{)}$				−1.928						−1.969		
				(1.975)						(1.548)		
Reg. Fem._{1994}	1.968***			1.929***	1.799***	1.951***						
	(0.155)			(0.183)	(0.191)	(0.162)						
Reg. Male._{1994}							2.007***			1.960***	1.806***	1.998***
							(0.131)			(0.146)	(0.164)	(0.145)
Tot. Votes_{1994}	−0.003			0.001		−0.003	−0.008			−0.003		−0.008
	(0.010)			(0.012)		(0.010)	(0.009)			(0.010)		(0.009)
PRI Votes_{1994}	−0.004			−0.004		−0.003	0.000			0.000		0.000
	(0.010)			(0.011)		(0.010)	(0.009)			(0.010)		(0.009)
PAN Votes_{1994}	−0.009			−0.008		−0.004	−0.009			−0.007		−0.007
	(0.019)			(0.019)		(0.020)	(0.017)			(0.018)		(0.018)
PRD Votes_{1994}	−0.003			−0.003		−0.001	−0.003			−0.003		−0.002
	(0.013)			(0.013)		(0.013)	(0.012)			(0.012)		(0.012)
Turnout_{1994}					0.179*						0.172*	
(Eligible)					(0.095)						(0.103)	
PRI Votes_{1994}					−0.090						−0.045	
(Share Eligible)					(0.086)						(0.073)	
PAN Votes_{1994}					−0.144						−0.194	
(Share Eligible)					(0.153)						(0.144)	
PRD Votes_{1994}					−0.051						−0.055	
(Share Eligible)					(0.100)						(0.096)	
Intercept	13.017*	52.867***		24.287	4.996	13.452*	15.015**	53.079***		26.823**	3.926	15.212**
	(7.585)	(1.671)		(16.360)	(6.624)	(7.678)	(6.733)	(1.574)		(12.553)	(6.620)	(6.887)
Village FE	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	417	417	408	417	417	415	417	417	408	417	417	415
R^2	0.875	0.005	-	0.876	0.877	0.873	0.882	0.006	-	0.883	0.883	0.879
Adjusted R^2	0.868	0.003	-	0.869	0.870	0.866	0.875	0.004	-	0.876	0.877	0.873
RMSE	10.4	26.967	-	10.382	10.364	10.396	9.968	26.823	-	9.923	10.009	10.004
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 14: **ITT Estimates of *Progresa* on Voter Registration Rates**. The table reports a null ITT effect of *Progresa* on voter registration rates. Registration rates are measured with the total number of registered voters as a share of the voting eligible population. Columns (1)-(6) and Columns (7)-(12) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications (the only exception is the difference-in-means approach) and across both gender groups.

	Registration (Females)					Registration (Male)				
	Counts	No Covariates	Log Population	Share	Leverage	Counts	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	−0.780	12.385*	−0.990	0.034	−0.980	−1.030	13.172*	−1.109	−0.098	−1.133
	(3.374)	(7.478)	(3.315)	(3.226)	(3.378)	(3.104)	(7.255)	(3.052)	(2.971)	(3.110)
Avg. Poverty	1.827		1.934	2.676*	1.855	0.674		0.727	1.965	0.700
	(1.513)		(1.440)	(1.515)	(1.505)	(1.423)		(1.365)	(1.469)	(1.422)
Population_{1994}	−0.0001			−0.0001	−0.0004	−0.00002			−0.00004	−0.0002
	(0.0001)			(0.0001)	(0.0003)	(0.0001)			(0.0001)	(0.0003)
$\text{Log(Population}_{1994}\text{)}$			−1.916					−1.940		
			(1.990)					(1.579)		
$\text{Registration.}_{1994}$	1.967***		1.928***	1.799***	1.950***	2.007***		1.960***	1.806***	1.998***
	(0.155)		(0.184)	(0.191)	(0.162)	(0.133)		(0.148)	(0.165)	(0.146)
Tot. Votes_{1994}	−0.003		0.002		−0.003	−0.008		−0.003		−0.008
	(0.010)		(0.011)		(0.010)	(0.009)		(0.010)		(0.009)
PRI Votes_{1994}	−0.004		−0.005		−0.004	−0.001		−0.001		−0.0005
	(0.010)		(0.011)		(0.010)	(0.010)		(0.010)		(0.009)
PAN Votes_{1994}	−0.010		−0.009		−0.005	−0.011		−0.008		−0.008
	(0.019)		(0.019)		(0.020)	(0.017)		(0.018)		(0.018)
PRD Votes_{1994}	−0.003		−0.003		−0.001	−0.004		−0.003		−0.002
	(0.013)		(0.013)		(0.013)	(0.011)		(0.011)		(0.011)
Turnout_{1994}				0.179*					0.173*	
(Eligible)				(0.094)					(0.101)	
PRI Votes_{1994}				−0.090					−0.046	
(Share Eligible)				(0.084)					(0.071)	
PAN Votes_{1994}				−0.144					−0.195	
(Share Eligible)				(0.153)					(0.142)	
PRD Votes_{1994}				−0.051					−0.055	
(Share Eligible)				(0.096)					(0.092)	
Intercept	12.620	46.346***	23.742	5.014	12.961*	14.474**	46.144***	26.101**	3.874	14.623**
	(7.729)	(5.314)	(16.547)	(6.872)	(7.759)	(6.858)	(5.074)	(12.809)	(6.846)	(6.976)
Village FE	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	417	417	417	417	415	417	417	417	417	415
R^2	0.875	−0.012	0.876	0.877	0.873	0.881	−0.002	0.882	0.883	0.879
Adjusted R^2	0.868	−0.015	0.869	0.870	0.866	0.875	−0.004	0.876	0.877	0.873
RMSE	10.414	27.208	10.399	10.371	10.412	9.986	26.931	9.944	10.013	10.023
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 15: **Instrumental Variable Estimates of *Progresa* on Voter Registration Rates**. The table reports a null effect of *Progresa* on voter registration rates under an instrumental variable approach. Registration rates are measured with total number of registered voters as a share of the voting eligible population. Columns (1)-(5) and Columns (6)-(10) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications (the only exception is the specification that fails to control for imbalanced pre-treatment covariates in the first and second stage regressions) and across both gender groups.

	Registration (Females)						Registration (Male)					
	Counts	Diff. in Means	Matching	Log Population	Share	Leverage	Counts	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	0.008	0.009	0.008	0.011	0.010	0.008	0.000	0.002	0.000	0.002	0.000	0.001
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Avg. Poverty	0.015*			0.014	0.017**	0.015*	−0.009			−0.010*	−0.010**	−0.009
	(0.009)			(0.009)	(0.008)	(0.009)	(0.006)			(0.006)	(0.005)	(0.006)
Population_{1994}	0.000				0.000	0.000	0.000				0.000	0.000
	(0.000)				(0.000)	(0.000)	(0.000)				(0.000)	(0.000)
$\text{Log(Population}_{1994}\text{)}$				0.037***						0.022***		
				(0.009)						(0.006)		
$\text{Log(Reg. Fem.}_{1994}\text{)}$	0.011			−0.020	0.005	0.012						
	(0.015)			(0.018)	(0.008)	(0.015)						
$\text{Log(Reg. Male.}_{1994}\text{)}$							−0.008			−0.027***	−0.006	−0.009
							(0.009)			(0.010)	(0.005)	(0.009)
Tot. Votes_{1994}	0.000			0.000		0.000	0.000			0.000		0.000
	(0.000)			(0.000)		(0.000)	(0.000)			(0.000)		(0.000)
PRI Votes_{1994}	0.000			0.000		0.000	0.000			0.000		0.000
	(0.000)			(0.000)		(0.000)	(0.000)			(0.000)		(0.000)
PAN Votes_{1994}	0.000			0.000		0.000	0.000			0.000		0.000
	(0.000)			(0.000)		(0.000)	(0.000)			(0.000)		(0.000)
PRD Votes_{1994}	0.000			0.000		0.000	0.000			0.000		0.000
	(0.000)			(0.000)		(0.000)	(0.000)			(0.000)		(0.000)
Turnout_{1994}					0.000						0.000	
(Eligible)					(0.001)						(0.000)	
PRI Votes_{1994}					−0.001						0.000	
(Share Eligible)					(0.001)						(0.000)	
PAN Votes_{1994}					−0.001						−0.001	
(Share Eligible)					(0.001)						(0.001)	
PRD Votes_{1994}					0.000						0.000	
(Share Eligible)					(0.001)						(0.000)	
Intercept	0.022	0.105***	0.105***	−0.058	0.063	0.020	0.165***	0.071***	0.071***	0.121***	0.161***	0.168***
	(0.070)	(0.006)	(0.006)	(0.069)	(0.062)	(0.070)	(0.041)	(0.004)	(0.004)	(0.044)	(0.038)	(0.041)
Village FE	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	417	417	408	417	417	415	417	417	408	417	417	415
R^2	0.087	0.003	-	0.121	0.104	0.084	0.104	0.0004	-	0.109	0.114	0.077
Adjusted R^2	0.038	0.001	-	0.074	0.056	0.035	0.056	−0.002	-	0.061	0.067	0.027
RMSE	0.078	0.077	-	0.076	0.077	0.077	0.051	0.051	-	0.051	0.051	0.051
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 16: **ITT Estimates of *Progresa* on the Difference of Log Number of Registered Voters**. The table reports a null ITT effect of *Progresa* on the difference of the log of number of registered voters. The difference is computed among voters registered by March 2000 (five months before the presidential election and when voters could no longer register) and the end of June 1998 (three months before households in treatment villages in the Progresa evaluation received the conditional cash transfer). Columns (1)-(6) and Columns (7)-(12) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications and across both gender groups.

	Registration (Females)					Registration (Male)				
	Counts	No Covariates	Log Population	Share	Leverage	Counts	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	0.023	0.027	0.031	0.030	0.024	0.001	0.006	0.006	−0.00002	0.002
	(0.025)	(0.023)	(0.023)	(0.025)	(0.025)	(0.016)	(0.016)	(0.015)	(0.016)	(0.016)
Avg. Poverty	0.010		0.008	0.011	0.010	−0.010		−0.011*	−0.010	−0.010
	(0.010)		(0.010)	(0.010)	(0.010)	(0.007)		(0.007)	(0.006)	(0.007)
Population_{1994}	0.00000			0.00000	0.00000***	0.00000***			0.00000***	0.00000*
	(0.00000)			(0.00000)	(0.00000)	(0.00000)			(0.00000)	(0.00000)
$\text{Log(Population}_{1994}\text{)}$			0.036***					0.021***		
			(0.010)					(0.006)		
$\text{Log(Registration}_{1994}\text{)}$	0.012		−0.018	0.004	0.012	−0.008		−0.027***	−0.006	−0.009
	(0.015)		(0.018)	(0.008)	(0.015)	(0.009)		(0.010)	(0.005)	(0.009)
Tot. Votes_{1994}	0.0001		0.0001		0.0001	−0.00001		−0.00001		−0.00000
	(0.0001)		(0.0001)		(0.0001)	(0.0001)		(0.0001)		(0.0001)
PRI Votes_{1994}	−0.0001		−0.0001		−0.0001	0.00002		0.00002		0.00001
	(0.0002)		(0.0002)		(0.0002)	(0.0001)		(0.0001)		(0.0001)
PAN Votes_{1994}	−0.0001		−0.0002		−0.0002	−0.0001		−0.0001		−0.0001
	(0.0002)		(0.0002)		(0.0002)	(0.0001)		(0.0001)		(0.0001)
PRD Votes_{1994}	−0.0001		−0.0001		−0.0001	0.00003		0.00002		0.00002
	(0.0002)		(0.0002)		(0.0002)	(0.0001)		(0.0001)		(0.0001)
Turnout_{1994}				−0.00003					−0.0002	
(Eligible)				(0.001)					(0.0004)	
PRI Votes_{1994}				−0.0004					0.0002	
(Share Eligible)				(0.001)					(0.0004)	
PAN Votes_{1994}				−0.001					−0.001	
(Share Eligible)				(0.001)					(0.001)	
PRD Votes_{1994}				−0.0001					0.0004	
(Share Eligible)				(0.001)					(0.0004)	
Intercept	0.032	0.091***	−0.043	0.079	0.030	0.165***	0.067***	0.125***	0.161***	0.169***
	(0.069)	(0.018)	(0.070)	(0.063)	(0.069)	(0.041)	(0.012)	(0.044)	(0.038)	(0.041)
Village FE	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	417	417	417	417	415	417	417	417	417	415
R^2	0.088	0.009	0.119	0.104	0.086	0.104	−0.003	0.109	0.114	0.077
Adjusted R^2	0.040	0.006	0.073	0.056	0.037	0.056	−0.005	0.062	0.067	0.027
RMSE	0.078	0.077	0.076	0.077	0.077	0.051	0.051	0.051	0.051	0.051

Note: *p<0.1; **p<0.05; ***p<0.01

Table 17: **Instrumental Variable Estimates of *Progresa* on the Difference of Log Number of Registered Voters**. The table reports a null effect of *Progresa* on the difference of the log of number of registered voters under an instrumental variable approach. The difference is computed among voters registered by March 2000 (five months before the presidential election and when voters could no longer register) and the end of June 1998 (three months before households in treatment villages in the Progresa evaluation received the conditional cash transfer). Columns (1)-(5) and Columns (6)-(10) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications and across both gender groups.

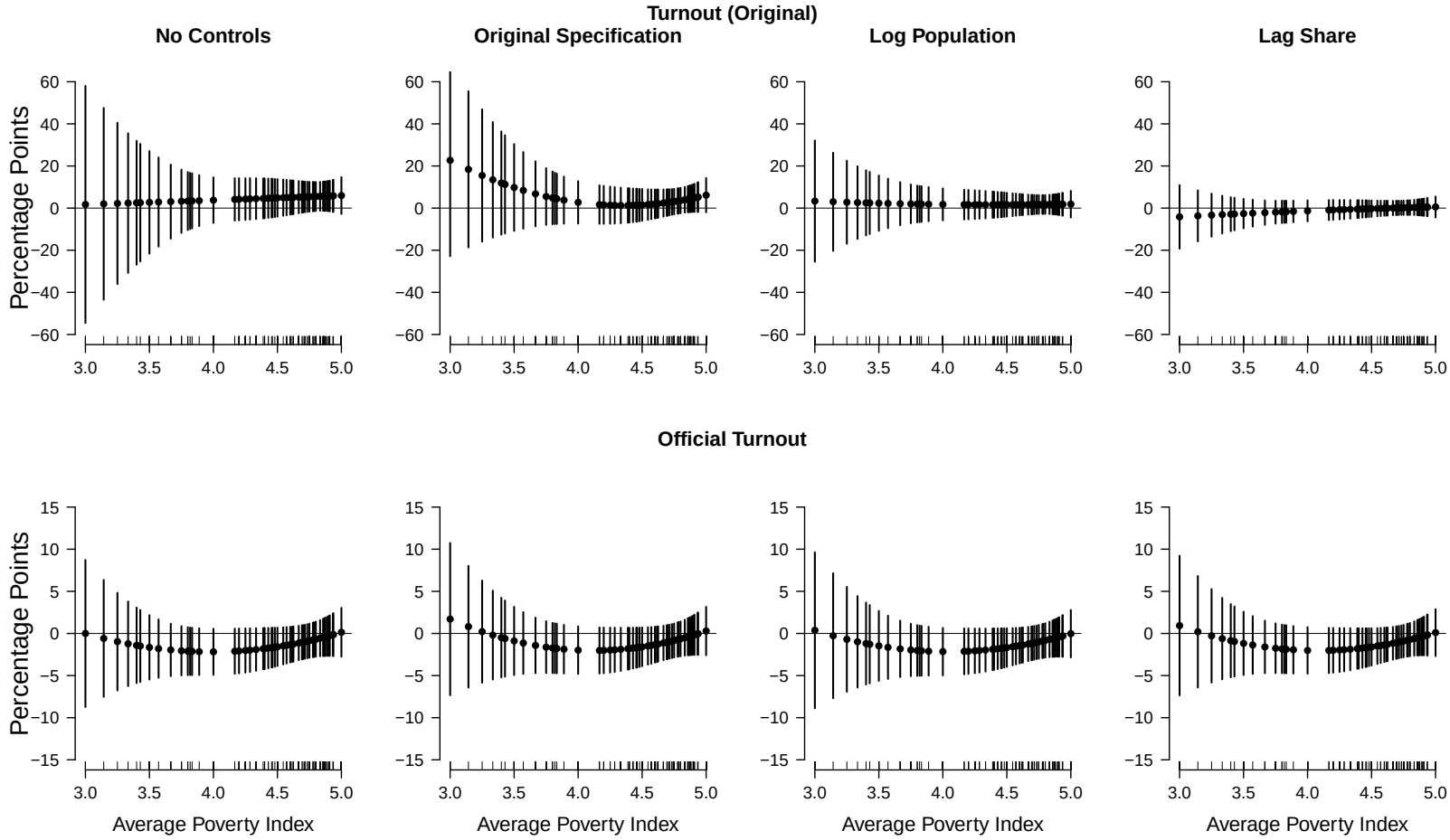

Figure 21: **Heterogenous Effects of *Progresa* on Turnout by Poverty Index.** The figure shows that *Progresa* did not have heterogenous effects on turnout. Turnout is measured using the original outcome in De La O (2013, 2015) and official turnout. For each outcome the figure then reports the estimated treatment effect by observed value of poverty from four different regression specifications, where each specification includes interactions between the treatment, poverty, and poverty squared.

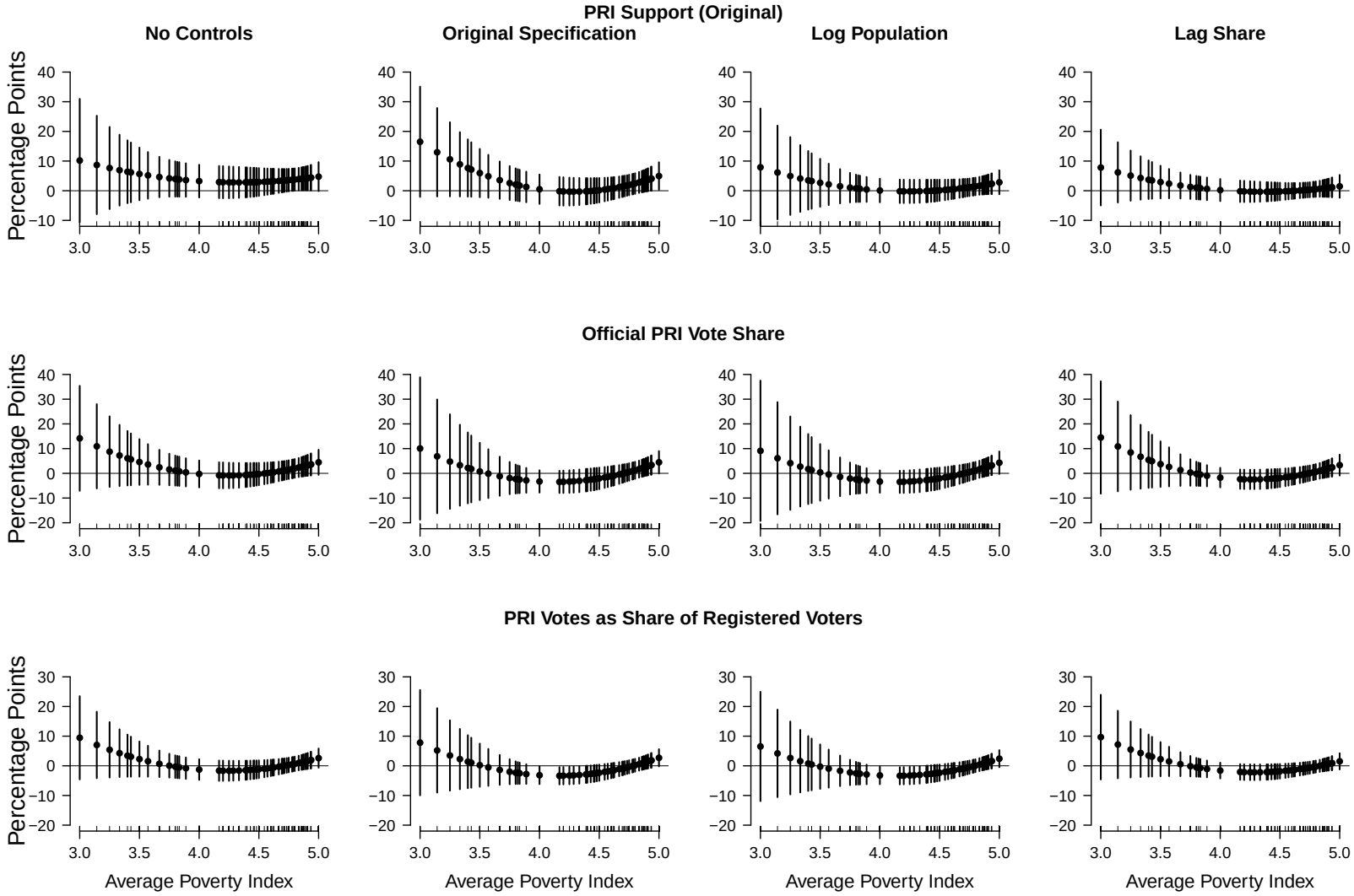

Figure 22: **Heterogenous Effects of *Progresa* on PRI Vote Share by Poverty Index.** The figure shows that *Progresa* did not have heterogenous effects on PRI vote share. PRI vote share is measured using the original outcome in De La O (2013, 2015), the official PRI vote share, and PRI votes as a share of registered voters. For each outcome the figure then reports the estimated treatment effect by observed value of poverty from four different regression specifications, where each specification includes interactions between the treatment, poverty, and poverty squared.

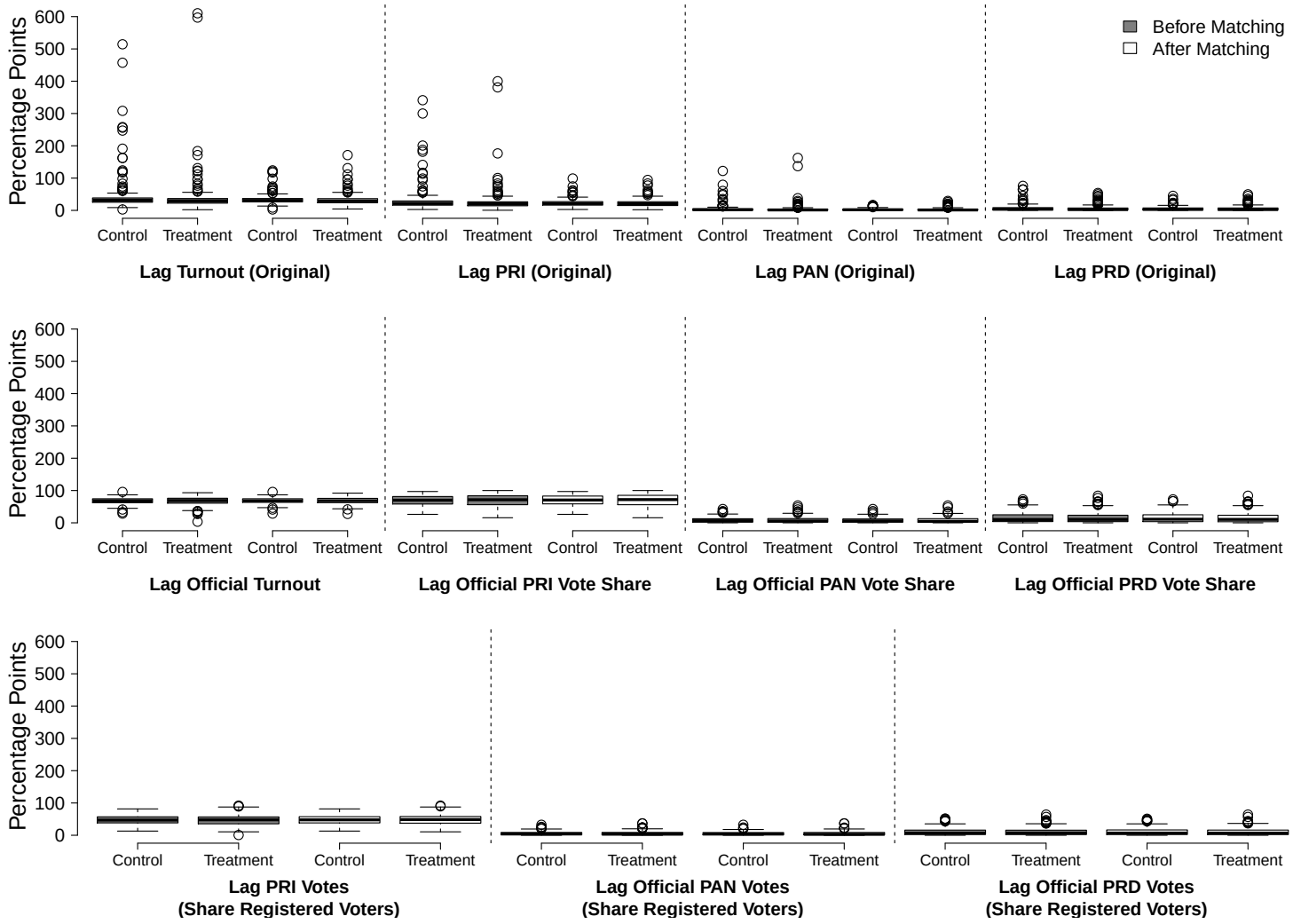

Figure 23: **Balance in Lag Outcomes Before and After Matching (GIS Sample).** The figure shows the presence of significant imbalance in the lag outcomes analyzed in De La O (2013, 2015). In particular, the treatment group has several outliers in lag turnout (as a share of population and registered voters) and PRI vote (as share of population and registered voters), and PAN vote as a share of population. This imbalance disappears after Coarsened Exact Matching (CEM) on population and lag outcomes. Original on left; after matching on right of each panel.

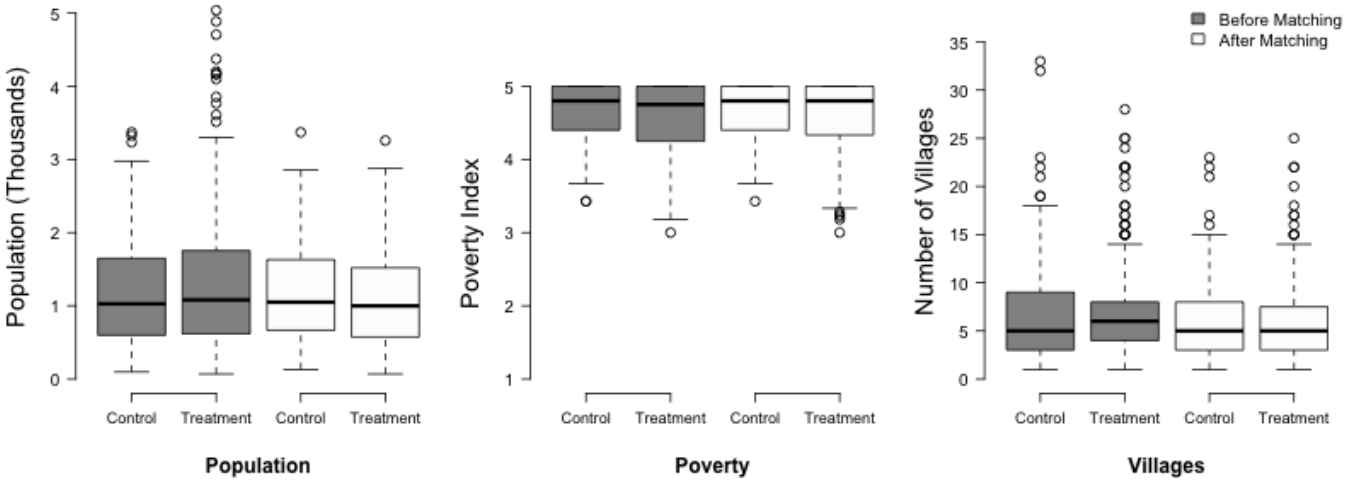

Figure 24: **Balance in Pre-Treatment Socio-Economic Covariates Before and After Matching (GIS Sample).** The figure shows the presence of significant imbalance in the pre-treatment covariates in De La O (2013, 2015). In particular, the treatment group reveals the presence of several population outliers. After Coarsened Exact Matching (CEM) on population and lag outcomes, without affecting the balance in poverty and number of villages across precincts.

	Turnout (Original)						Official Turnout Among Registered Voters					
	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	−3.663	−15.178*	−4.112*	−2.720	0.292	−3.244	−0.528	−0.820	−1.497	−0.536	−0.835	−0.544
	(8.303)	(8.569)	(2.278)	(7.939)	(5.495)	(8.364)	(0.889)	(0.899)	(0.960)	(0.888)	(0.837)	(0.893)
Avg. Poverty	25.477*			21.701*	6.214	26.756*	−1.598			−1.695*	0.674	−1.549
	(13.411)			(11.304)	(4.179)	(13.754)	(0.981)			(0.985)	(0.942)	(0.983)
Population_{1995}	−0.048***				0.007	−0.047***	−0.001**				0.0002	−0.001**
	(0.011)				(0.008)	(0.011)	(0.001)				(0.0005)	(0.001)
$\text{Log(Population}_{1995}\text{)}$				−78.745***						−2.094***		
				(18.737)						(0.714)		
Tot. Votes_{1994}	−0.037			0.140**		−0.122	−0.030**			−0.027*		−0.037*
	(0.112)			(0.068)		(0.104)	(0.015)			(0.015)		(0.019)
Turnout_{1994} (Original)					1.272***							
					(0.325)							
Turnout_{1994} (Registered)											0.330***	
											(0.056)	
PRI Votes_{1994}	0.206			0.077	−0.030	0.295**	0.040**			0.037**	0.0001	0.047**
	(0.132)			(0.079)	(0.032)	(0.132)	(0.016)			(0.016)	(0.003)	(0.020)
PAN Votes_{1994}	0.566**			0.248	−0.014	0.726**	0.052***			0.044**	0.018**	0.060***
	(0.269)			(0.164)	(0.049)	(0.305)	(0.019)			(0.019)	(0.008)	(0.023)
PRD Votes_{1994}	0.210			0.068	−0.024	0.296**	0.035**			0.032**	−0.003	0.042**
	(0.133)			(0.079)	(0.037)	(0.132)	(0.016)			(0.016)	(0.004)	(0.021)
Villages	−1.047*			0.278	−0.052	−1.071*	0.028			0.058	0.072	0.027
	(0.572)			(0.513)	(0.303)	(0.579)	(0.092)			(0.092)	(0.084)	(0.092)
Intercept	−74.118	54.787***		402.921***	−35.072	−82.568	64.314***	58.254***		76.973***	31.728***	64.065***
	(64.000)	(7.053)		(68.455)	(26.940)	(66.196)	(4.836)	(0.722)		(6.850)	(7.016)	(4.864)
Observations	408	408	339	408	408	406	408	408	339	408	408	406
R^2	0.278	0.008	-	0.434	0.830	0.289	0.069	0.002	-	0.076	0.204	0.066
Adjusted R^2	0.264	0.006	-	0.423	0.826	0.275	0.050	−0.0004	-	0.057	0.188	0.048
RMSE	71.614	81.048	-	64.456	40.434	71.24	8.52	8.687	-	8.491	7.893	8.541
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 18: **ITT Estimates of *Progresa* on Turnout (GIS Sample)**. The table reports a null ITT effect of *Progresa* on turnout when analyzing the sample created by projecting village coordinates onto precincts as described in Section 2 of the paper. Turnout is measured as in De La O (2013, 2015) (Columns 1-6) and as a share of registered voters (Column 7-12). The null finding is consistent across most regression specifications.

	PRI (Original)						Official PRI Vote Share					
	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	−1.206	−6.332*	−2.273*	−0.814	0.116	−1.056	1.086	1.379	0.101	1.451	0.499	1.061
	(3.440)	(3.506)	(1.354)	(3.281)	(2.533)	(3.474)	(1.425)	(1.805)	(2.015)	(1.440)	(1.322)	(1.426)
Avg. Poverty	11.589**			9.955**	4.539**	12.063**	2.408			2.454	2.023	2.261
	(5.573)			(4.680)	(2.038)	(5.720)	(1.572)			(1.589)	(1.448)	(1.586)
Population_{1994}	−0.021***				0.001	−0.020***	0.003***				0.003***	0.003***
	(0.005)				(0.003)	(0.005)	(0.001)				(0.001)	(0.001)
$\text{Log(Population}_{1994})$				−34.102***						1.835		
				(7.591)						(1.305)		
Tot. Votes_{1994}	−0.031			0.045*	−0.006	−0.063	−0.085***			−0.080***	−0.020***	−0.073***
	(0.045)			(0.027)	(0.013)	(0.046)	(0.023)			(0.024)	(0.004)	(0.027)
PRI Votes_{1994}	0.119**			0.063**		0.152***	0.106***			0.105***		0.093***
	(0.053)			(0.032)		(0.056)	(0.024)			(0.024)		(0.028)
PAN Votes_{1994}	0.194*			0.057		0.254**	−0.047			−0.051		−0.067**
	(0.109)			(0.066)		(0.125)	(0.031)			(0.032)		(0.034)
PRD Votes_{1994}	0.077			0.015		0.109*	−0.019			−0.020		−0.031
	(0.054)			(0.031)		(0.057)	(0.024)			(0.025)		(0.028)
PRI Votes_{1994} (Share Original)					0.653***							
					(0.242)							
PAN Votes_{1994} (Share Original)					0.178							
					(0.663)							
PRD Votes_{1994} (Share Original)					0.285							
					(0.304)							
PRI Votes_{1994} (Share Official)											0.388***	
											(0.100)	
PAN Votes_{1994} (Share Official)											−0.387***	
											(0.141)	
PRD Votes_{1994} (Share Official)											−0.226**	
											(0.107)	
Villages	−0.661***			−0.089	−0.237	−0.670***	−0.566***			−0.544***	−0.363***	−0.563***
	(0.240)			(0.212)	(0.158)	(0.242)	(0.129)			(0.130)	(0.127)	(0.129)
Intercept	−28.267	27.629***		178.312***	−15.837	−31.391	53.471***	56.345***		42.636***	33.701**	54.389***
	(26.714)	(2.825)		(26.916)	(13.547)	(27.655)	(7.577)	(1.382)		(10.906)	(13.471)	(7.638)
Observations	408	408	339	408	408	406	408	408	339	408	408	406
R^2	0.284	0.008	-	0.451	0.764	0.292	0.402	0.001	-	0.392	0.510	0.399
Adjusted R^2	0.269	0.006	-	0.440	0.759	0.278	0.390	−0.001	-	0.380	0.500	0.387
RMSE	29.717	33.797	-	26.441	20.497	29.619	14.223	18.099	-	14.353	12.916	14.231
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 19: **ITT Estimates of *Progresa* on PRI Vote Share (GIS Sample)**. The table reports a null ITT effect of *Progresa* on PRI vote share when analyzing the sample generated by projecting village coordinates onto precincts. PRI vote share is measured as in De La O (2013, 2015) (Columns 1-6) and as a share of total votes cast (Column 7-12). The null finding is consistent across all but one of the regression specifications.

	PRI (Official Registered Voters)					
	Original Specification	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)
Intention to	0.175	0.169	−0.860	0.368	−0.453	0.154
Treat (ITT)	(0.941)	(1.115)	(1.231)	(0.944)	(0.883)	(0.944)
Avg. Poverty	0.383			0.359	2.008*	0.339
	(1.005)			(1.013)	(1.039)	(1.014)
Population_{1994}	0.001*				0.002***	0.001*
	(0.001)				(0.001)	(0.001)
$\text{Log(Population}_{1994}\text{)}$				−0.056		
				(0.807)		
Tot. Votes_{1994}	−0.061***			−0.056***	−0.012***	−0.059***
	(0.013)			(0.013)	(0.003)	(0.018)
PRI Votes_{1994}	0.079***			0.077***		0.076***
	(0.014)			(0.014)		(0.019)
PAN Votes_{1994}	−0.005			−0.011		−0.010
	(0.017)			(0.018)		(0.022)
PRD Votes_{1994}	0.004			0.002		0.002
	(0.014)			(0.014)		(0.019)
PRI Votes_{1994}					0.398***	
(Share Registered)					(0.050)	
PAN Votes_{1994}					−0.137	
(Share Registered)					(0.093)	
PRD Votes_{1994}					−0.100*	
(Share Registered)					(0.052)	
Villages	−0.290***			−0.263***	−0.152*	−0.289***
	(0.087)			(0.088)	(0.085)	(0.087)
Intercept	34.596***	32.679***		35.074***	9.443	34.900***
	(4.917)	(0.875)		(7.013)	(7.020)	(4.961)
Observations	408	408	339	408	408	406
R^2	0.319	0.0001	-	0.316	0.439	0.315
Adjusted R^2	0.306	−0.002	-	0.302	0.428	0.301
RMSE	9.194	10.987	-	9.225	8.383	9.217
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 20: **ITT Estimates of *Progresa* on PRI Vote Share (Registered Voters) in GIS Sample**. The table reports a null ITT effect of *Progresa* on PRI vote share when analyzing the sample generated by implementing the approach described in Section 2 of the paper. PRI vote share is measured as a share of registered voters. This finding is consistent across all regression specifications.

	Turnout (Original)					Official Turnout Among Registered Voters				
	Original Specification	No Covariates	Log Population	Share	Leverage	Original Specification	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	−11.132	−47.802*	−8.392	0.895	−9.792	−1.605	−2.584	−1.655	−2.548	−1.642
	(25.265)	(26.534)	(24.655)	(16.986)	(25.271)	(2.700)	(2.810)	(2.739)	(2.552)	(2.692)
Avg. Poverty	28.050**		23.669**	5.996	29.035**	−1.227		−1.307	1.292	−1.167
	(11.659)		(9.264)	(5.846)	(11.950)	(1.085)		(1.093)	(1.080)	(1.087)
Population_{1994}	−0.047***			0.007	−0.047***	−0.001**			0.0002	−0.001**
	(0.012)			(0.008)	(0.011)	(0.001)			(0.0005)	(0.001)
$\text{Log(Population}_{1994}\text{)}$			−78.230***					−1.992***		
			(19.879)					(0.747)		
Tot. Votes_{1994}	−0.031		0.142**		−0.116	−0.030*		−0.026*		−0.036*
	(0.116)		(0.068)		(0.114)	(0.015)		(0.015)		(0.019)
Turnout_{1994} (Original)				1.272***						
				(0.336)						
Turnout_{1994} (Registered)									0.332***	
									(0.057)	
PRI Votes_{1994}	0.200		0.074	−0.031	0.288**	0.039**		0.036**	0.0001	0.046**
	(0.138)		(0.084)	(0.034)	(0.143)	(0.016)		(0.016)	(0.003)	(0.020)
PAN Votes_{1994}	0.558**		0.245	−0.014	0.720**	0.051***		0.044**	0.018**	0.059**
	(0.280)		(0.169)	(0.051)	(0.316)	(0.019)		(0.019)	(0.008)	(0.023)
PRD Votes_{1994}	0.205		0.065	−0.024	0.290**	0.035**		0.032*	−0.003	0.042**
	(0.137)		(0.081)	(0.040)	(0.141)	(0.016)		(0.016)	(0.004)	(0.021)
Villages	−1.021*		0.284	−0.054	−1.049*	0.031		0.059	0.079	0.030
	(0.561)		(0.503)	(0.281)	(0.570)	(0.093)		(0.093)	(0.084)	(0.093)
Intercept	−81.298	77.409***	394.362***	−34.463	−89.021	63.278***	59.477***	75.285***	29.804***	62.983***
	(56.172)	(18.503)	(84.864)	(23.713)	(58.155)	(4.713)	(1.948)	(7.061)	(7.197)	(4.736)
Observations	408	408	408	408	406	408	408	408	408	406
R^2	0.283	0.017	0.435	0.830	0.294	0.068	0.009	0.074	0.204	0.066
Adjusted R^2	0.268	0.015	0.423	0.826	0.280	0.050	0.007	0.055	0.188	0.047
RMSE	71.608	80.983	64.692	40.825	71.217	8.523	8.657	8.501	7.893	8.544

Note: *p<0.1; **p<0.05; ***p<0.01

Table 21: **Instrumental Variable Estimates of *Progresa* on Turnout (GIS Sample)**. The table reports a null effect of *Progresa* on turnout under an instrumental variable approach. Turnout is measured as in De La O (2013, 2015) (Columns 1-5) and as a share of registered voters (Column 6-10). Across most specifications we find no evidence that the CCT increases turnout. In Column (2) we report a negative and significant effect, but this is the result of the large imbalance across treatment and control precincts.

	PRI (Original)					Official PRI Vote Share				
	Original Specification	No Covariates	Log Population	Share	Leverage	Original Specification	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	−3.666	−19.943*	−2.513	0.355	−3.188	3.299	4.343	4.478	1.516	3.204
	(10.468)	(10.864)	(10.182)	(7.836)	(10.496)	(4.323)	(5.664)	(4.442)	(4.008)	(4.296)
Avg. Poverty	12.436***		10.544***	4.449*	12.805***	1.646		1.404	1.668	1.515
	(4.782)		(3.744)	(2.292)	(4.904)	(1.762)		(1.782)	(1.603)	(1.784)
Population_{1994}	−0.021***			0.001	−0.020***	0.003***			0.003***	0.003***
	(0.005)			(0.004)	(0.005)	(0.001)			(0.001)	(0.001)
$\text{Log(Population}_{1994}\text{)}$			−33.948***					1.560		
			(8.092)					(1.384)		
Tot. Votes_{1994}	−0.029		0.046*	−0.006	−0.061	−0.087***		−0.081***	−0.020***	−0.075***
	(0.047)		(0.027)	(0.014)	(0.049)	(0.023)		(0.024)	(0.004)	(0.027)
PRI Votes_{1994}	0.117**		0.062*		0.150**	0.108***		0.107***		0.095***
	(0.056)		(0.034)		(0.061)	(0.024)		(0.025)		(0.028)
PAN Votes_{1994}	0.191*		0.056		0.252*	−0.045		−0.049		−0.065*
	(0.113)		(0.068)		(0.130)	(0.031)		(0.033)		(0.034)
PRD Votes_{1994}	0.075		0.015		0.107*	−0.017		−0.019		−0.030
	(0.056)		(0.032)		(0.060)	(0.024)		(0.025)		(0.028)
PRI Votes_{1994} (Share Eligible)				0.653***						
				(0.250)						
PAN Votes_{1994} (Share Eligible)				0.177						
				(0.664)						
PRD Votes_{1994} (Share Eligible)				0.285						
				(0.303)						
PRI Votes_{1994} (Share Official)									0.390***	
									(0.100)	
PAN Votes_{1994} (Share Official)									−0.384***	
									(0.142)	
PRD Votes_{1994} (Share Official)									−0.225**	
									(0.107)	
villages	−0.652***		−0.088	−0.238	−0.662***	−0.574***		−0.547***	−0.366***	−0.571***
	(0.236)		(0.208)	(0.153)	(0.239)	(0.130)		(0.130)	(0.128)	(0.130)
Intercept	−30.632	37.066***	175.749***	−15.587	−33.492	55.599***	54.289***	47.203***	34.463***	56.501***
	(23.351)	(7.479)	(34.307)	(11.279)	(24.181)	(7.509)	(3.839)	(11.769)	(13.356)	(7.605)
Observations	408	408	408	408	406	408	408	408	408	406
R^2	0.287	0.012	0.451	0.764	0.296	0.403	0.009	0.393	0.511	0.401
Adjusted R^2	0.273	0.010	0.440	0.759	0.281	0.391	0.006	0.381	0.502	0.389
RMSE	29.724	33.823	26.538	20.647	29.622	14.203	18.036	14.342	12.895	14.206
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 22: **Instrumental Variable Estimates of *Progresa* on PRI Vote Share (GIS Sample)**. The table reports a null effect of *Progresa* on incumbent support under an instrumental variable approach when analyzing the sample created by selecting precincts using the geographic coordinates of villages that participated in the *Progresa* evaluation. PRI vote share is measured as in De La O (2013, 2015) (Columns 1-5) and as a share of total vote cast (Column 6-10). Across most specifications we find no evidence that the CCT increases incumbent support. In Column (2) we report a negative and significant effect, but this is the result of the large imbalance across treatment and control precincts.

	PRI (Official Registered Voters)				
	Original Specification	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)
Early *Progresa*	0.531	0.533	1.134	−1.375	0.466
	(2.857)	(3.510)	(2.910)	(2.694)	(2.845)
Avg. Poverty	0.260		0.093	2.341**	0.230
	(1.146)		(1.157)	(1.168)	(1.158)
Population_{1994}	0.001			0.003***	0.001
	(0.001)			(0.001)	(0.001)
$\text{Log(Population}_{1994})$			−0.125		
			(0.859)		
Tot. Votes_{1994}	−0.061***		−0.056***	−0.012***	−0.059***
	(0.013)		(0.013)	(0.003)	(0.018)
PRI Votes_{1994}	0.079***		0.077***		0.077***
	(0.014)		(0.014)		(0.019)
PAN Votes_{1994}	−0.005		−0.010		−0.010
	(0.017)		(0.018)		(0.022)
PRD Votes_{1994}	0.005		0.002		0.002
	(0.014)		(0.014)		(0.019)
PRI Votes_{1994}				0.398***	
(Share Registered)				(0.050)	
PAN Votes_{1994}				−0.137	
(Share Registered)				(0.093)	
PRD Votes_{1994}				−0.098*	
(Share Registered)				(0.053)	
villages	−0.291***		−0.263***	−0.149*	−0.290***
	(0.088)		(0.088)	(0.086)	(0.088)
Intercept	34.939***	32.427***	36.230***	8.466	35.207***
	(4.917)	(2.408)	(7.600)	(7.010)	(4.978)
Observations	408	408	408	408	406
R^2	0.320	0.001	0.317	0.436	0.315
Adjusted R^2	0.306	−0.001	0.304	0.425	0.301
RMSE	9.186	10.979	9.214	8.408	9.209
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 23: **Instrumental Variable Estimates of *Progresa* Effect on PRI Vote Share (Registered Voters in GIS Sample)**. The table reports a null effect of *Progresa* on PRI vote share under an instrumental variable approach when analyzing the spatially-merged sample. PRI vote share is measured as a share of registered voters. This finding is robust across all regression specifications.

	Registration (Females)						Registration (Male)					
	Counts	Diff. in Means	Matching	Log Population	Share	Leverage	Counts	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	3.063	−12.236	−2.781	3.248	2.681	3.020	3.023	−10.898	−2.630	3.182	2.409	2.993
	(5.461)	(7.518)	(2.070)	(5.705)	(5.107)	(5.452)	(5.383)	(6.748)	(1.880)	(5.640)	(5.115)	(5.377)
Avg. Poverty	3.348			3.299	5.695	3.330	2.213			2.217	6.624	2.221
	(3.002)			(3.012)	(8.044)	(2.968)	(2.664)			(2.729)	(9.016)	(2.724)
Population_{1994}	0.003				0.001	0.003	0.002				0.001	0.002
	(0.007)				(0.003)	(0.007)	(0.008)				(0.003)	(0.008)
$\text{Log(Population}_{1994})$				4.776						2.552		
				(9.084)						(11.854)		
Reg. Fem._{1994}	1.440***			1.459***	1.401	1.441***						
	(0.398)			(0.429)	(1.281)	(0.400)						
Reg. Male._{1994}							1.384***			1.391***	0.845	1.385***
							(0.461)			(0.512)	(1.454)	(0.464)
Tot. Votes_{1994}	−0.028			−0.034		−0.032	−0.024			−0.026		−0.029
	(0.034)			(0.042)		(0.039)	(0.033)			(0.042)		(0.039)
PRI Votes_{1994}	0.015			0.019		0.020	0.014			0.015		0.019
	(0.026)			(0.027)		(0.035)	(0.031)			(0.028)		(0.043)
PAN Votes_{1994}	0.031			0.039		0.033	0.042			0.046		0.046
	(0.054)			(0.055)		(0.070)	(0.067)			(0.061)		(0.088)
PRD Votes_{1994}	0.017			0.021		0.021	0.015			0.016		0.020
	(0.024)			(0.024)		(0.033)	(0.030)			(0.026)		(0.041)
Turnout_{1994} (Eligible)					−0.967						−0.167	
					(2.114)						(1.270)	
PRI Votes_{1994}					0.849						0.406	
(Share Eligible)					(1.865)						(1.595)	
PAN Votes_{1994}					1.626						1.140	
(Share Eligible)					(3.184)						(2.802)	
PRD Votes_{1994}					0.836						0.448	
(Share Eligible)					(2.149)						(1.933)	
Villages	−0.222			−0.269	−0.293	−0.221	−0.218			−0.236	−0.222	−0.218
	(0.273)			(0.275)	(0.362)	(0.273)	(0.267)			(0.300)	(0.406)	(0.266)
Intercept	−20.319	47.035***		−48.492	−27.380	−20.147	−14.636	45.166***		−29.718	−32.126	−14.627
	(23.848)	(6.087)		(72.896)	(50.607)	(23.545)	(21.407)	(5.356)		(83.900)	(49.186)	(21.205)
Observations	408	408	339	408	408	406	408	408	339	408	408	406
R^2	0.816	0.007	-	0.816	0.820	0.816	0.787	0.006	-	0.787	0.797	0.787
Adjusted R^2	0.812	0.004	-	0.812	0.816	0.812	0.782	0.004	-	0.782	0.792	0.782
RMSE	38.032	72.165	-	38.037	43.665	38.15	38.237	65.862	-	38.384	43.134	38.378
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 24: **ITT Estimates of *Progresa* on Voter Registration Rates (GIS Sample)**. The table reports a null ITT effect of *Progresa* on voter registration rates. Registration rates are measured with the total number of registered voters as a share of the voting eligible population. Columns (1)-(6) and Columns (7)-(12) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications and across both gender groups.

	Registration (Females)					Registration (Male)				
	Counts	No Covariates	Log Population	Share	Leverage	Counts	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	9.405	−38.537*	10.070	8.435	9.213	9.281	−34.323	9.862	7.546	9.126
	(16.932)	(23.325)	(17.868)	(16.149)	(16.791)	(16.673)	(20.963)	(17.640)	(16.117)	(16.541)
Avg. Poverty	1.055		0.881	3.552	1.051	−0.064		−0.157	4.692	−0.051
	(3.720)		(3.869)	(4.482)	(3.684)	(3.836)		(4.135)	(6.544)	(3.852)
Population_{1994}	0.003			0.001	0.003	0.002			0.001	0.002
	(0.007)			(0.003)	(0.007)	(0.008)			(0.003)	(0.008)
$\text{Log(Population}_{1994}\text{)}$			4.403					2.176		
			(8.727)					(11.517)		
$\text{Registration}_{1994}$	1.448***		1.464***	1.436	1.450***	1.393***		1.396***	0.874	1.394***
	(0.411)		(0.438)	(1.255)	(0.415)	(0.473)		(0.519)	(1.451)	(0.478)
Tot. Votes_{1994}	−0.033		−0.038		−0.038	−0.029		−0.029		−0.034
	(0.043)		(0.048)		(0.048)	(0.042)		(0.048)		(0.048)
PRI Votes_{1994}	0.019		0.023		0.024	0.018		0.019		0.023
	(0.032)		(0.033)		(0.041)	(0.035)		(0.033)		(0.047)
PAN Votes_{1994}	0.035		0.042		0.035	0.046		0.048		0.048
	(0.059)		(0.059)		(0.072)	(0.072)		(0.066)		(0.090)
PRD Votes_{1994}	0.020		0.024		0.025	0.018		0.019		0.024
	(0.027)		(0.028)		(0.036)	(0.032)		(0.029)		(0.044)
Turnout_{1994}				−0.990					−0.185	
(Eligible)				(2.130)					(1.304)	
PRI Votes_{1994}				0.859					0.414	
(Share Eligible)				(1.885)					(1.620)	
PAN Votes_{1994}				1.598					1.123	
(Share Eligible)				(3.191)					(2.827)	
PRD Votes_{1994}				0.827					0.442	
(Share Eligible)				(2.157)					(1.950)	
Villages	−0.239		−0.278	−0.312	−0.238	−0.236		−0.245	−0.242	−0.235
	(0.256)		(0.269)	(0.355)	(0.256)	(0.251)		(0.295)	(0.400)	(0.251)
Intercept	−13.960	65.272***	−39.538	−21.470	−13.715	−8.312	61.410***	−20.865	−26.796	−8.202
	(16.994)	(16.131)	(62.444)	(40.284)	(16.412)	(14.436)	(14.341)	(74.828)	(39.593)	(13.915)
Observations	408	408	408	408	406	408	408	408	408	406
R^2	0.816	0.015	0.816	0.820	0.816	0.787	0.013	0.787	0.797	0.787
Adjusted R^2	0.812	0.013	0.812	0.816	0.812	0.782	0.010	0.782	0.792	0.782
RMSE	38.352	72.115	38.356	43.855	38.464	38.528	65.872	38.654	43.428	38.666
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 25: **Instrumental Variable Estimates of *Progresa* on Voter Registration Rates (GIS Sample)**. The table reports a null effect of *Progresa* on voter registration rates under an instrumental variable approach. Registration rates are measured with total number of registered voters as a share of the voting eligible population. Columns (1)-(6) and Columns (7)-(12) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications and across both gender groups.

	Registration (Females)						Registration (Male)					
	Counts	Diff. in Means	Matching	Log Population	Share	Leverage	Counts	Diff. in Means	Matching	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intention to Treat (ITT)	0.002	0.004	0.003	0.002	0.002	0.002	−0.005	−0.003	−0.005	−0.003	−0.004	−0.005
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Avg. Poverty	0.027***			0.028***	0.023***	0.026***	−0.001			−0.002	−0.005	−0.001
	(0.008)			(0.008)	(0.008)	(0.008)	(0.006)			(0.006)	(0.006)	(0.006)
Population_{1994}	0.00002***				0.00003***	0.00002***	0.00001				0.00002***	0.00001
	(0.00001)				(0.00001)	(0.00001)	(0.00001)				(0.00001)	(0.00001)
$\text{Log(Population}_{1994})$				0.020**						−0.002		
				(0.010)						(0.008)		
$\text{Log(Registration}_{1994})$	−0.009			−0.016	−0.025**	−0.008	−0.005			−0.001	−0.021***	−0.004
	(0.014)			(0.017)	(0.011)	(0.015)	(0.009)			(0.012)	(0.008)	(0.010)
Tot. Votes_{1994}	−0.0003***			−0.0003***		−0.0003*	−0.0002***			−0.0002***		−0.0002**
	(0.0001)			(0.0001)		(0.0002)	(0.0001)			(0.0001)		(0.0001)
PRI Votes_{1994}	0.0003***			0.0003***		0.0003*	0.0002***			0.0002***		0.0002**
	(0.0001)			(0.0001)		(0.0001)	(0.0001)			(0.0001)		(0.0001)
PAN Votes_{1994}	0.0004***			0.0005***		0.0004**	0.0003***			0.0003***		0.0003**
	(0.0002)			(0.0002)		(0.0002)	(0.0001)			(0.0001)		(0.0001)
PRD Votes_{1994}	0.0004***			0.0004***		0.0003**	0.0003***			0.0003***		0.0003***
	(0.0001)			(0.0001)		(0.0002)	(0.0001)			(0.0001)		(0.0001)
Turnout_{1994} (Eligible)					−0.001						−0.0002	
					(0.002)						(0.001)	
PRI Votes_{1994}					0.001						0.0004	
(Share Eligible)					(0.002)						(0.001)	
PAN Votes_{1994}					0.001						0.0002	
(Share Eligible)					(0.002)						(0.002)	
PRD Votes_{1994}					0.003						0.001	
(Share Eligible)					(0.002)						(0.001)	
Villages	−0.002**			−0.002**	−0.002*	−0.002**	−0.001**			−0.001*	−0.001	−0.001**
	(0.001)			(0.001)	(0.001)	(0.001)	(0.001)			(0.001)	(0.001)	(0.001)
Intercept	0.026	0.108***		−0.068	0.103*	0.027	0.110**	0.076***		0.104**	0.183***	0.107**
	(0.072)	(0.006)		(0.074)	(0.060)	(0.073)	(0.049)	(0.005)		(0.043)	(0.042)	(0.050)
Observations	408	408	408	408	408	406	408	408	408	408	408	406
R^2	0.064	0.001	-	0.057	0.087	0.059	0.053	0.001	-	0.047	0.113	0.048
Adjusted R^2	0.043	−0.002	-	0.036	0.066	0.037	0.032	−0.002	-	0.026	0.093	0.026
RMSE	0.072	0.073	-	0.072	0.071	0.072	0.052	0.052	-	0.052	0.05	0.052
Note:												*p<0.1; **p<0.05; ***p<0.01

Table 26: **ITT Estimates of *Progresa* on the Difference of Log Number of Registered Voters (GIS Sample)**. The table reports a null ITT effect of *Progresa* on the difference of the log of number of registered voters. The difference is computed among voters registered by March 2000 (five months before the presidential election and when voters could no longer register) and the end of June 1998 (three months before households in treatment villages in the Progresa evaluation received the conditional cash transfer). Columns (1)-(6) and Columns (7)-(12) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications and across both gender groups.

	Registration (Females)					Registration (Male)				
	Counts	No Covariates	Log Population	Share	Leverage	Counts	No Covariates	Log Population	Share	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early *Progresa*	0.005	0.013	0.008	0.007	0.006	−0.015	−0.010	−0.011	−0.013	−0.015
	(0.023)	(0.024)	(0.024)	(0.023)	(0.023)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)
Avg. Poverty	0.026**		0.026**	0.021**	0.025**	0.003		0.001	−0.001	0.003
	(0.010)		(0.011)	(0.011)	(0.011)	(0.007)		(0.007)	(0.007)	(0.007)
Population_{1994}	0.00002***			0.00003***	0.00002***	0.00001			0.00002***	0.00001
	(0.00001)			(0.00001)	(0.00001)	(0.00001)			(0.00001)	(0.00001)
$\text{Log(Population}_{1994})$			0.020*					−0.001		
			(0.010)					(0.008)		
$\text{Log(Registration}_{1994})$	−0.009		−0.015	−0.026**	−0.008	−0.005		−0.001	−0.020**	−0.004
	(0.014)		(0.017)	(0.011)	(0.015)	(0.009)		(0.012)	(0.008)	(0.010)
Tot. Votes_{1994}	−0.0003***		−0.0003***		−0.0003*	−0.0002***		−0.0002***		−0.0002**
	(0.0001)		(0.0001)		(0.0001)	(0.0001)		(0.0001)		(0.0001)
PRI Votes_{1994}	0.0003***		0.0003***		0.0003*	0.0002***		0.0002***		0.0002**
	(0.0001)		(0.0001)		(0.0001)	(0.0001)		(0.0001)		(0.0001)
PAN Votes_{1994}	0.0004***		0.0005***		0.0004**	0.0003***		0.0003***		0.0003**
	(0.0002)		(0.0002)		(0.0002)	(0.0001)		(0.0001)		(0.0001)
PRD Votes_{1994}	0.0004***		0.0004***		0.0003**	0.0003***		0.0003***		0.0003***
	(0.0001)		(0.0001)		(0.0002)	(0.0001)		(0.0001)		(0.0001)
Turnout_{1994}				−0.001					−0.0003	
(Eligible)				(0.002)					(0.001)	
PRI Votes_{1994}				0.001					0.0005	
(Share Eligible)				(0.002)					(0.001)	
PAN Votes_{1994}				0.001					0.0004	
(Share Eligible)				(0.002)					(0.001)	
PRD Votes_{1994}				0.002					0.001	
(Share Eligible)				(0.002)					(0.001)	
Villages	−0.002**		−0.002**	−0.002*	−0.002**	−0.001**		−0.001*	−0.001	−0.001**
	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Intercept	0.029	0.102***	−0.061	0.109*	0.030	0.098**	0.080***	0.094**	0.171***	0.096*
	(0.073)	(0.017)	(0.076)	(0.061)	(0.074)	(0.050)	(0.012)	(0.045)	(0.043)	(0.050)
Observations	408	408	408	408	406	408	408	408	408	406
R^2	0.065	0.005	0.057	0.088	0.059	0.051	0.005	0.046	0.109	0.045
Adjusted R^2	0.043	0.002	0.036	0.067	0.038	0.030	0.003	0.025	0.089	0.024
RMSE	0.072	0.072	0.072	0.071	0.072	0.052	0.052	0.052	0.051	0.052
Note:										*p<0.1; **p<0.05; ***p<0.01

Table 27: **Instrumental Variable Estimates of *Progresa* on the Difference of Log Number of Registered Voters (GIS Sample)**. The table reports a null effect of *Progresa* on the difference of the log of number of registered voters under an instrumental variable approach. The difference is computed among voters registered by March 2000 (five months before the presidential election and when voters could no longer register) and the end of June 1998 (three months before households in treatment villages in the Progresa evaluation received the conditional cash transfer). Columns (1)-(5) and Columns (6)-(10) report estimates for females and males respectively. The estimates are statistically indistinguishable from zero across all specifications and across both gender groups.

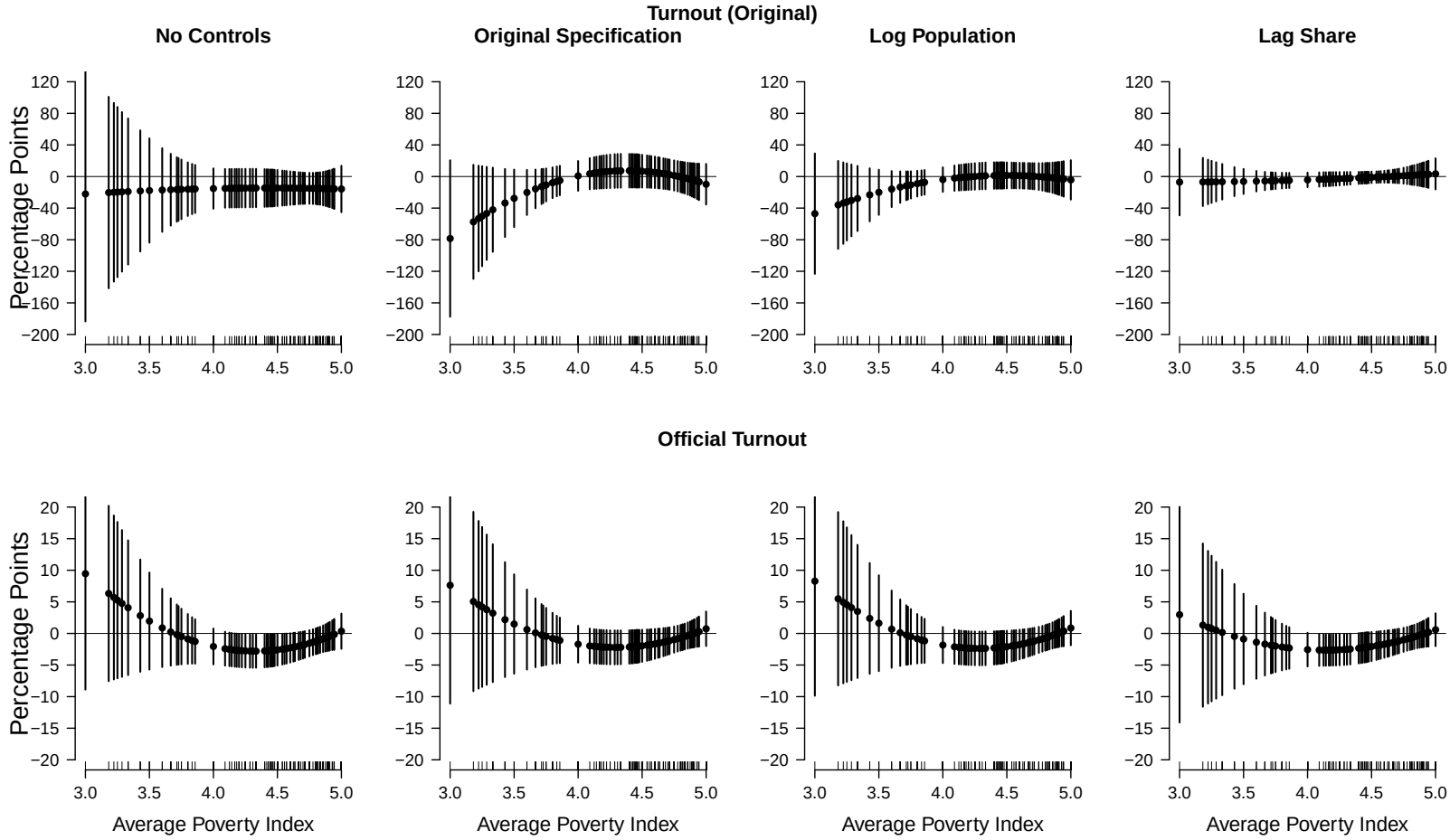

Figure 25: **Heterogenous Effects of *Progresa* on Turnout by Poverty Index in GIS Sample.** The figure shows that *Progresa* did not have heterogenous effects on turnout. Turnout is measured using the original outcome in De La O (2013, 2015) and official turnout. For each outcome the figure then reports the estimated treatment effect by the observed value of poverty from four different regression specifications, where each specification includes interactions between the treatment, poverty, and poverty squared.

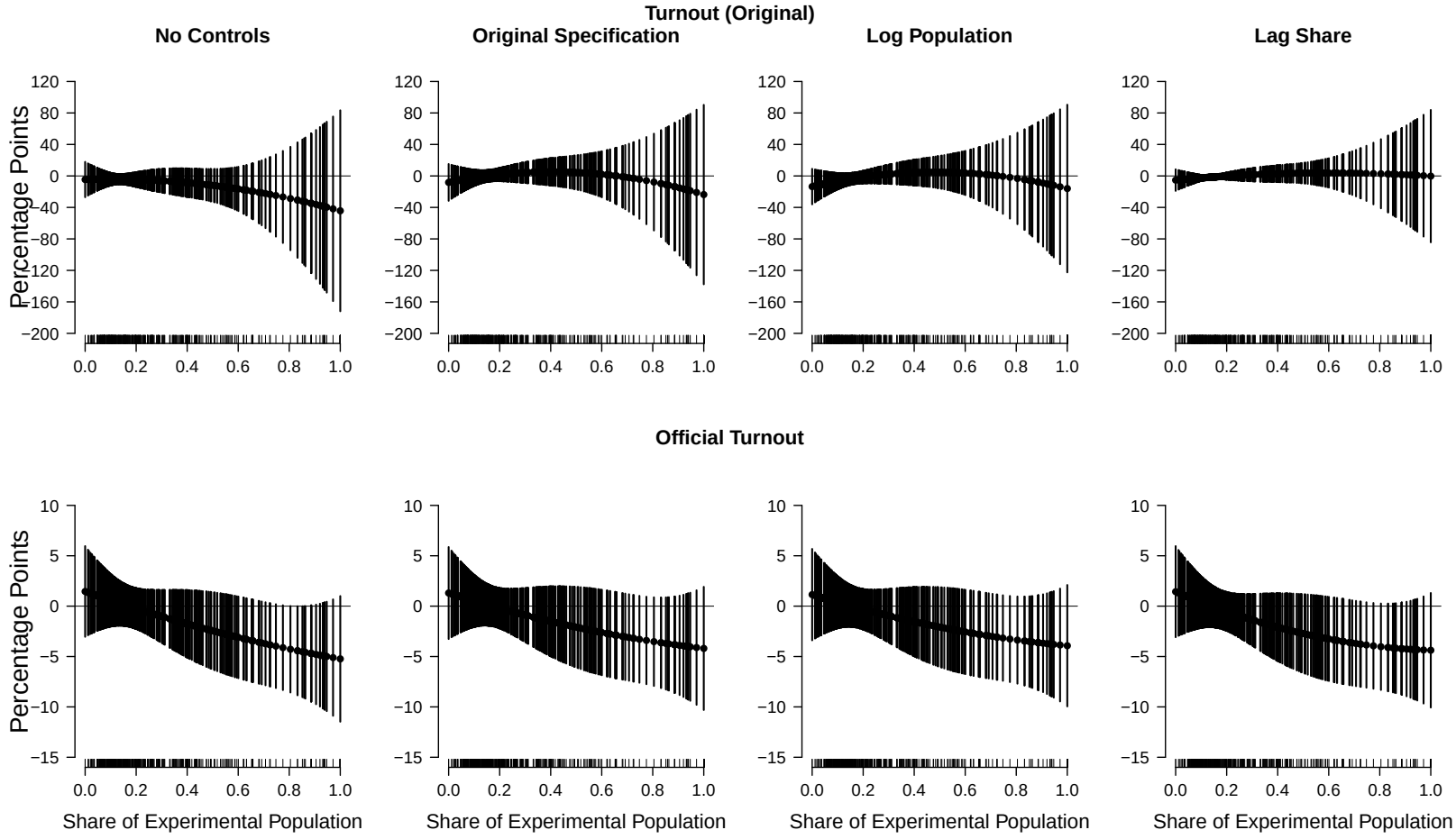

Figure 26: **Heterogenous Effects of *Progresa* on Turnout by Share of Population Participating in the Evaluation in GIS Sample.** The figure shows that *Progresa* did not have heterogeneous effects on turnout. Turnout is measured using the original outcome in De La O (2013, 2015) and official turnout. For each outcome the figure then reports the estimated treatment effect by observed value of experimental population from four different regression specifications, where each specification includes interactions between the treatment, experimental population, and experimental population squared.

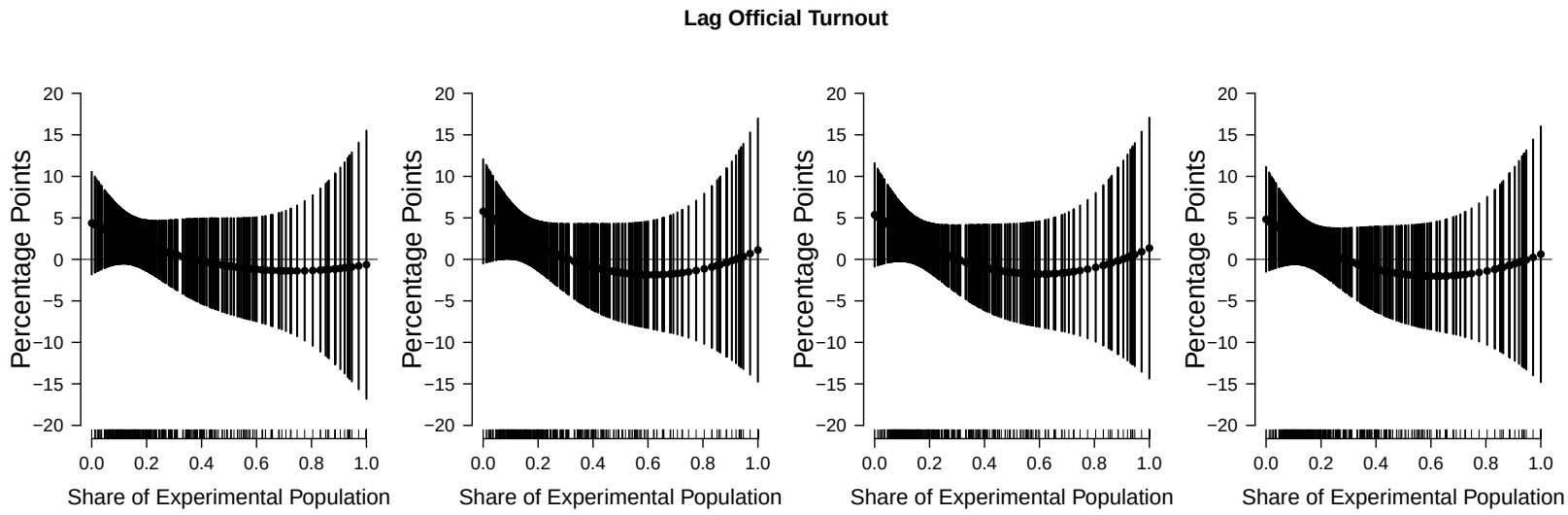

Figure 27: **Heterogenous Placebo Effects of *Progresa* on Turnout in Lag Congressional Election by Share of Population Participating in the Evaluation in GIS Sample.** The figure shows that *Progresa* did not have heterogeneous "effects" on lag turnout (although the estimates display a negative trend as in Figure 26). The outcome is turnout in the 1997 plurality congressional deputy races. The figure reports estimated treatment effect by observed value of experimental population from four different regression specifications, where each specification includes interactions between the treatment, experimental population, and experimental population squared.

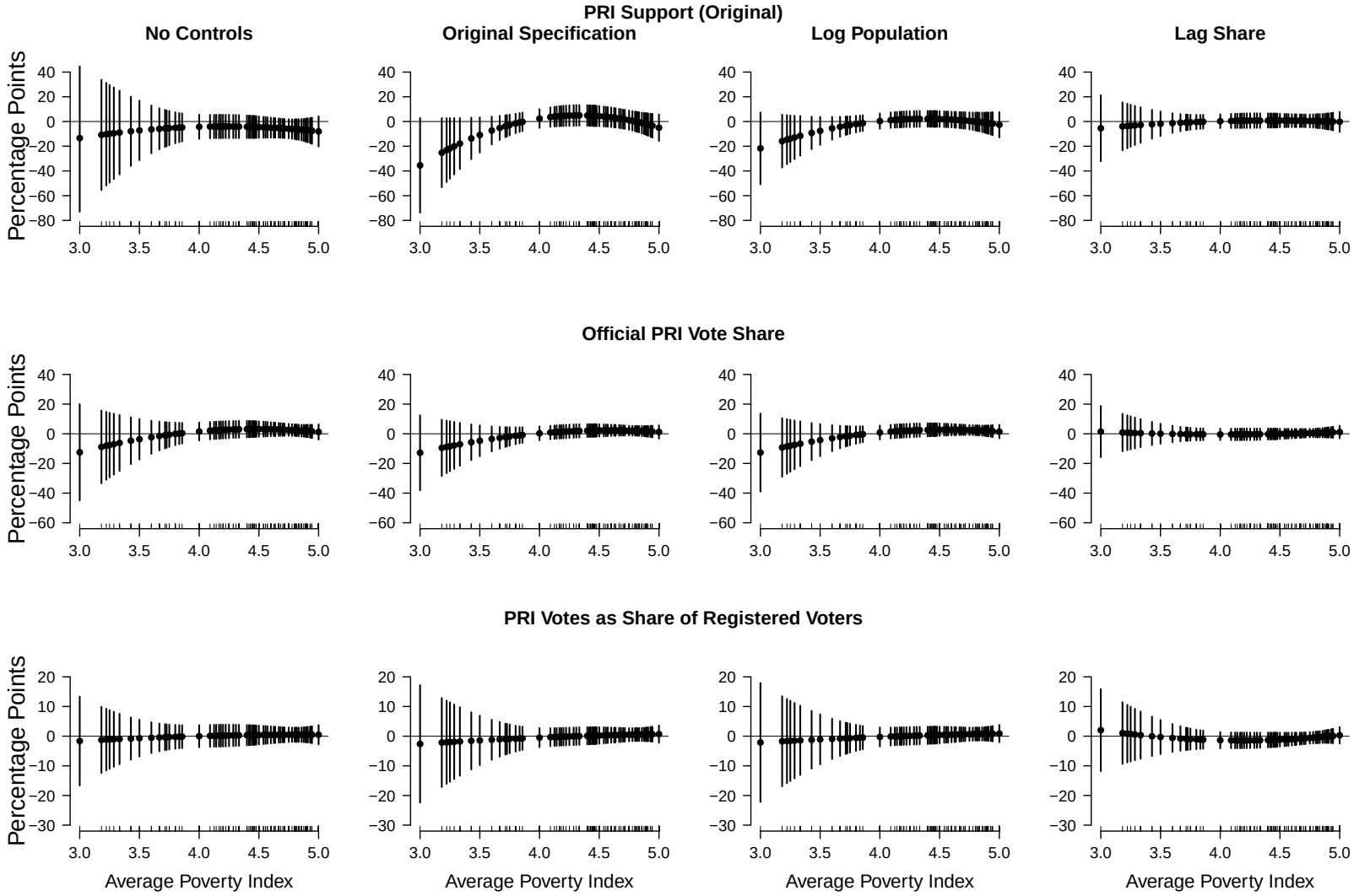

Figure 28: **Heterogenous Effects of *Progresa* on PRI Vote Share by Poverty Index in GIS Sample.** The figure shows that *Progresa* did not have heterogeneous effects on PRI vote share. PRI vote share is measured using the original outcome in De La O (2013, 2015), the official PRI vote share, and PRI votes as a share of registered voters. For each outcome the figure then reports the estimated treatment effect by the observed value of poverty from four different regression specifications, where each specification includes interactions between the treatment, poverty, and poverty squared.

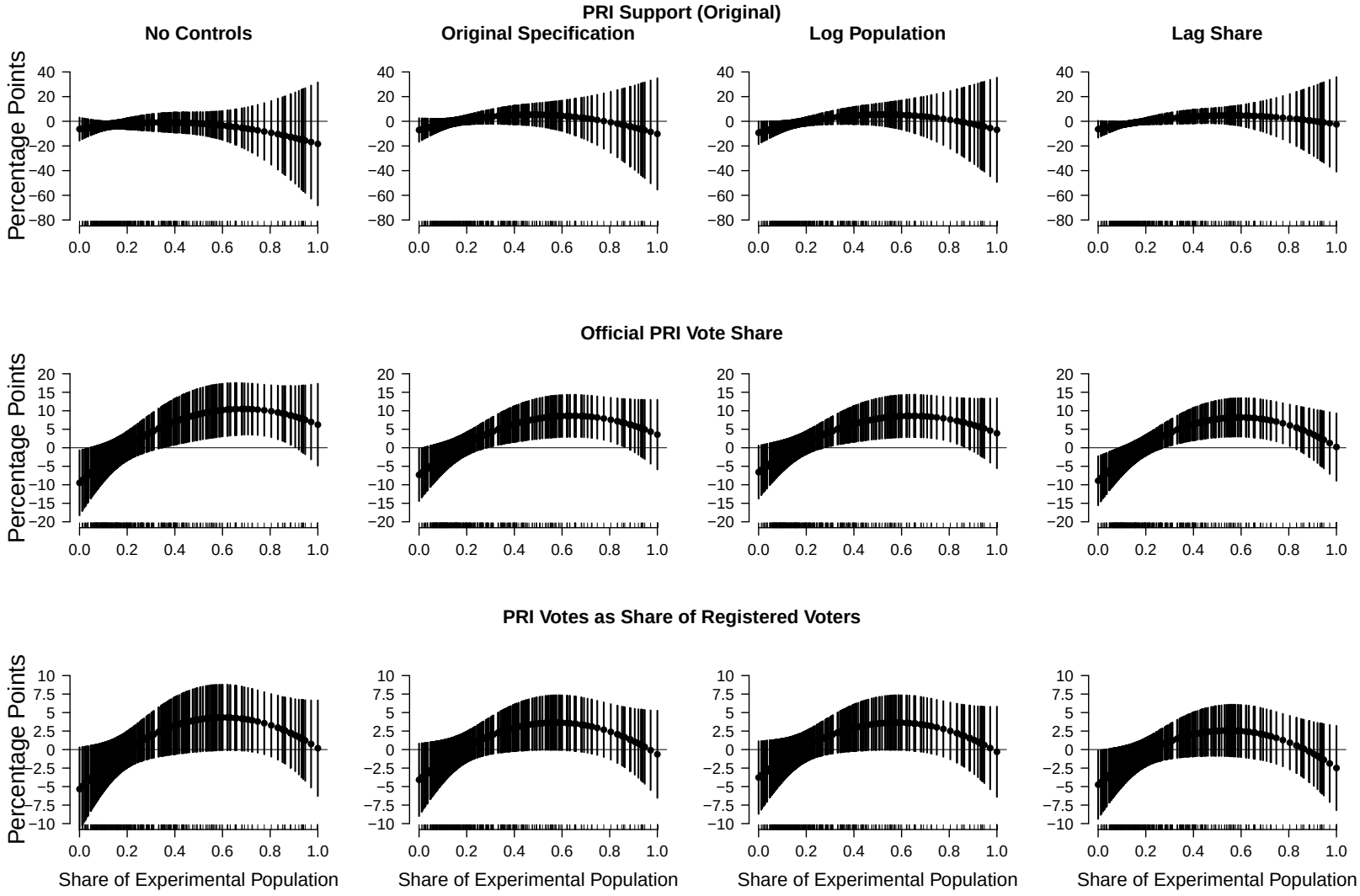

Figure 29: **Heterogenous Effects of *Progresa* on PRI Vote Share by Share of Population Participating in the Evaluation in GIS Sample.** The figure shows that in precincts with a higher share of experimental population *Progresa* seem to have experienced a positive effect on PRI vote share. PRI vote share is measured using the original outcome in De La O (2013, 2015), the official PRI vote share, and PRI votes as a share of registered voters. For each outcome the figure then reports the estimated treatment effect by the observed value of share of experimental population from four different regression specifications, where each specification includes interactions between the treatment, share of experimental population and share of experimental population squared.

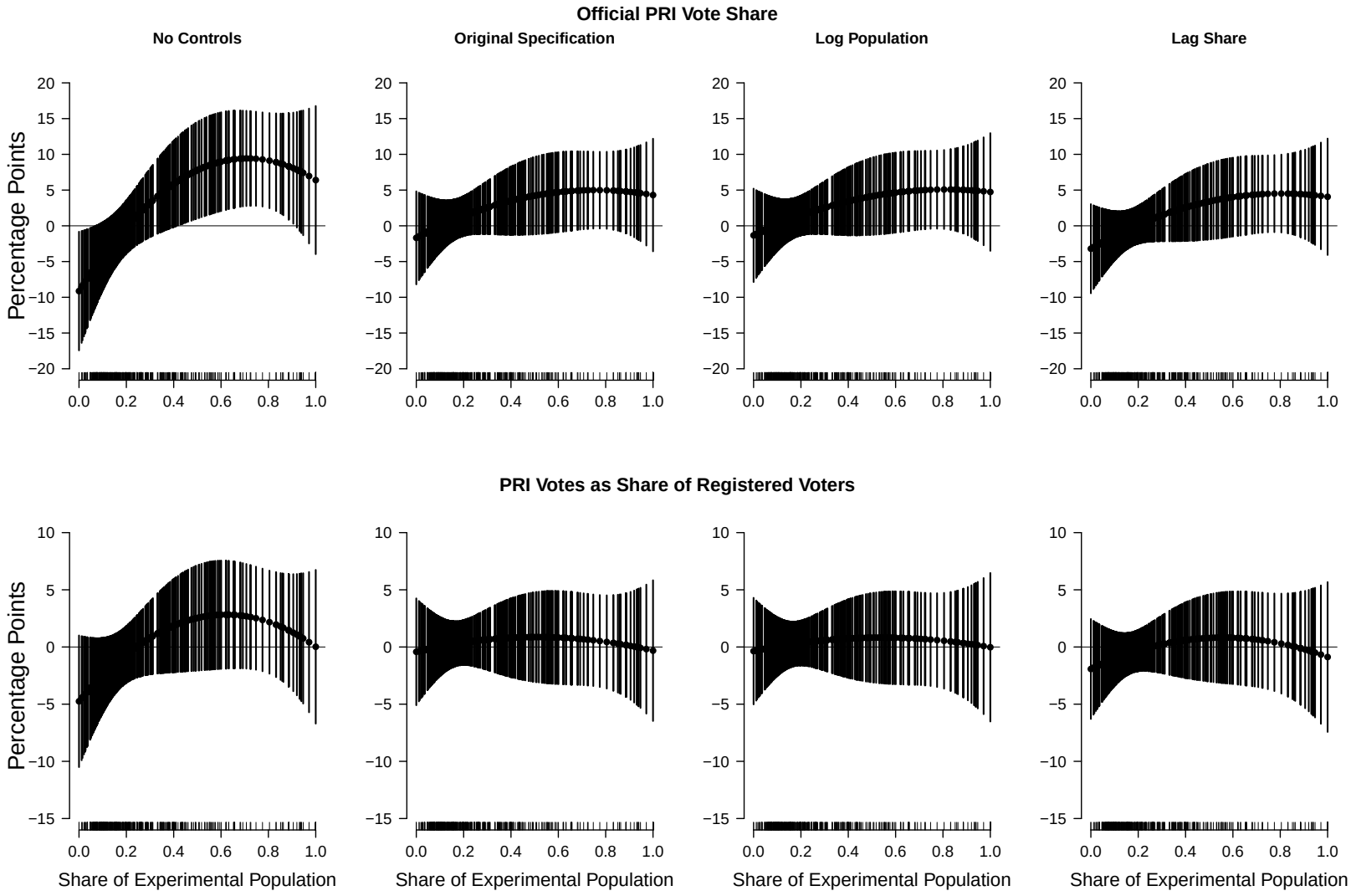

Figure 30: **Heterogenous Effects of *Progresa* on PRI Vote Share in Congressional Elections by Share of Population Participating in the Evaluation in GIS Sample.** The figure reproduces the analysis reported in Figure 29 but using PRI support in the year 2000 plurality congressional deputy races as the outcome of interest. The figure shows similar patterns as those reported in Figure 29 (i.e, precincts with higher share of experimental population experienced a larger effect of *Progresa* on PRI support).

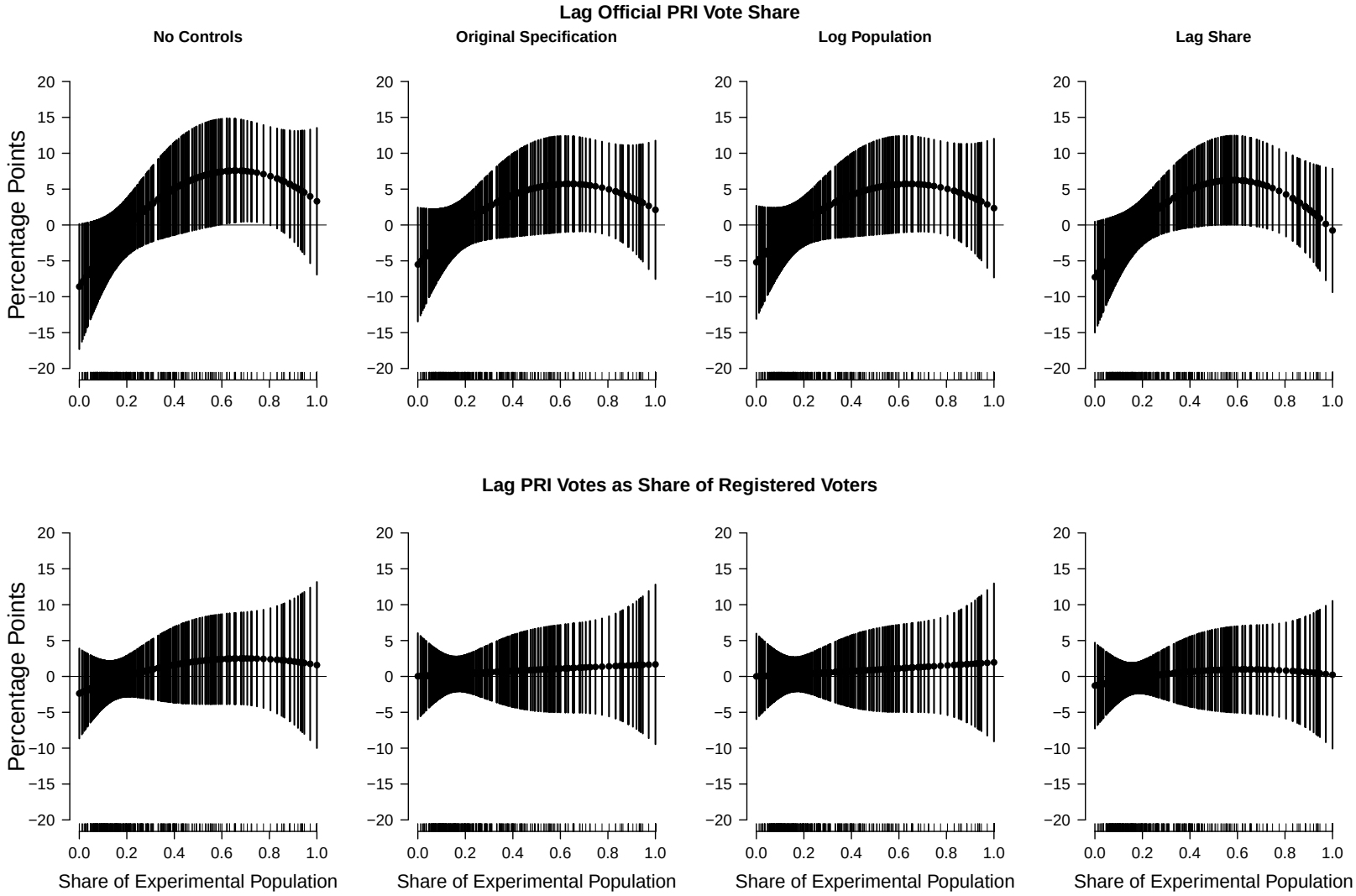

Figure 31: **Heterogenous Placebo Effects of *Progresa* on PRI Vote Share in Congressional Elections by Share of Population Participating in the Evaluation in GIS Sample.** The figure reproduces the analysis reported in Figure 30 but using PRI support as in the 1997 plurality congressional deputy races as the outcome of interest. The figure shows similar patterns as those reported in Figure 29 and 30 (i.e, precincts with higher share of experimental population experienced a larger effect of *Progresa* on PRI support). This evidence suggest that the findings reported in the previous figure are the result of imbalance in pre-treatment PRI support.

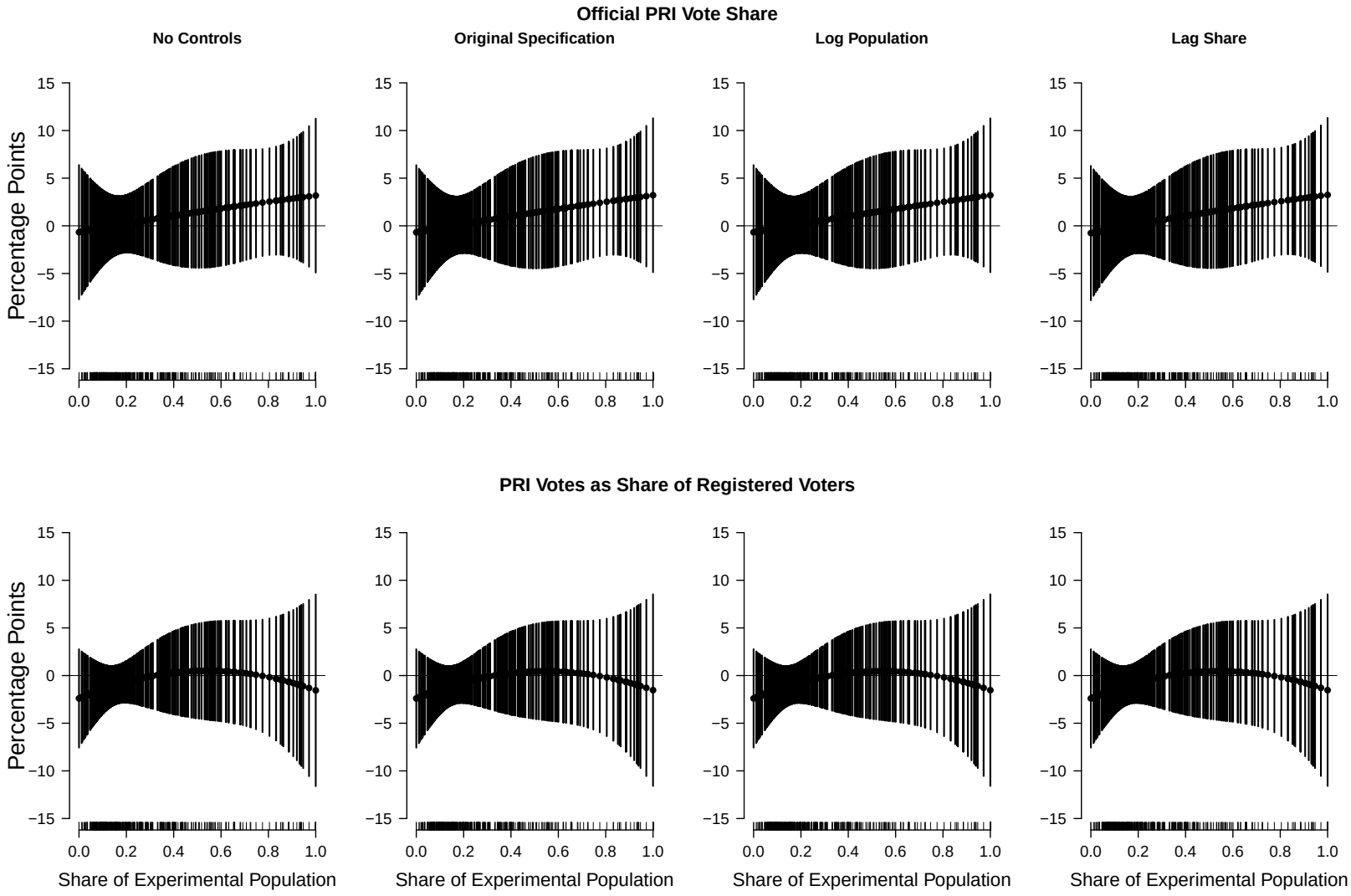

Figure 32: **Heterogenous Differences-in-Differences Effects of *Progresa* on PRI Vote Share by Share of Population Participating in the Evaluation in GIS Sample.** The figure reports differences-in-differences effect of *Progresa* on PRI support in congressional elections (plurality deputy races). The figure shows that *Progresa* does not have a statistically or substantively significant impact on PRI support.

Comparison of name-matching sample to GIS sample	Share of Precincts
Population discrepancy in relation to GIS sample	71.30%
Reasons for the discrepancies:	
Does not exclude census villages returned by GIS but includes additional census villages as a result of name-match	11.80%
Excludes at least one census village returned by GIS and includes additional census villages as a result of name-match	32.70%
Excludes at least one census village returned by GIS and does not include additional census villages as a result of name-match	32.4%
Arbitrary choices in name-matching sample	Share of Precincts
At least on electoral village with census village match not in sample	11.40%
At least one electoral village without census village match	26.50%
At least one electoral village matched to multiple census villages	2.20%
Wrong number of electoral villages	5.10%

Table 28: **Characteristics of the De La O (2013, 2015) Name-matching Sample**. The top panel reports the proportion of precincts in the name-matching sample with a population discrepancy from the correct GIS sample, as well as the types of mistakes that led to this problem. The bottom panel reports the proportion of precincts with choices made in generating the name-matching sample that would be inappropriate even if the electoral and census offices had coordinated and names meant the same thing.

3.1 Tests for Spillover Effects

We implement the approach introduced in Athey, Eckles, and Imbens (Forthcoming) to assess whether the presence of treatment spillovers accounts for the null impact of SPS and *Progresa* on turnout and incumbent support. The procedure involves the the following three steps. First, determining the fraction of treated neighbors across all observations in the sample. Second, selecting a set of "focal" units to compute as test statistic the covariance between the residuals of a regression of turnout (and incumbent support) on a unit's treatment status, and a unit's fraction of treated neighbors. And finally, calculating empirical *p*-values by comparing the absolute value of the observed covariance described in the previous step and the ones resulting from artificially manipulating the treatment status of neighbors in the sample of "focal" units.

The test statistic of interest seeks to determine whether there is any relationship between the explanatory power of a regression ignoring spillovers and the fraction of a unit's treated neighbors. Intuitively, in the presence of spillovers, units with a higher proportion of treated neighbors should report larger residuals. The approach proposed in Athey, Eckles, and Imbens (ibid.) then leverages the artificial experiments to determine whether this is the case. By construction, there is no systematic relationship between a unit's residual and the fraction of its artificially treated neighbors. Thus, we only expect small *p*-values, or a small proportion of artificial covariances to be larger than the one associated with the actual experiment, when there are spillovers.

The procedure to determine the number of neighbors for units in the SPS and *Progresa* evaluations is as follows. For SPS, recall that precinct-clusters are a collection of precincts defined by a cluster's villages (*localidades*). Since village centroids provide higher granularity, we use these and define the distance between cluster i and j as the minimum distance between a village centroid in i and j.[4] We then define as neighbors for each cluster i all clusters j found within a 20 km radius. In the *Progresa* sample, we use the same 20 km distance threshold but rely on a precinct centroids to compute distance

[4]Urban precinct-clusters are a collection of precincts defined by a cluster's census tracts. In this case, precincts provide a lower level of aggregation, and therefore we use their centroids to measure the distance urban precinct-clusters and their neighbors.

between units in the sample. Finally, to select focal observations we follow the algorithm introduced in Section 5.4.3 of Athey, Eckles, and Imbens (Forthcoming).[5]

	Turnout (Eligible)	Turnout (Official)	Incumbent (Eligible)	Incumbent (Official)	Incumbent (Registered)
SPS	0.9516	0.8446	0.9605	0.9697	0.9772
Progresa (Original)	0.1776	0.6316	0.1662	0.1863	0.1733
Progresa (GIS)	0.4200	0.9228	0.8814	0.0381	0.0498

Table 29: **p-values for Spillover Effects Test.** The rows report p-values for the Athey, Eckles, and Imbens (Forthcoming) spillover effects test for each evaluation (and samples) across the different outcomes. We fail to reject the null hypothesis of no spillovers in all evaluations and outcomes (When testing 5 hypotheses, one rejects the null of no spillovers at the 5% significance level when $p < \frac{0.05}{5} = 0.01$).

Table 29 shows there is no evidence of spillovers in the SPS and *Progresa* evaluations. The rows in the table report p-values for the specific evaluations (and samples) across the different regression outcomes. In the case of SPS we find that all p-values are above 0.84, indicating that the covariance associated with the intervention is not significantly different from the ones associated with the artificial experiments. Similarly, the second row shows no evidence of treatment spillovers in the sample analyzed in De La O (2013). Finally, we find p-values less than 0.05 only in the GIS sample of the *Progresa* evaluation when the regression outcomes are the incumbent's official vote share and incumbent votes as a share of registered voters. However, we fail to reject the null of no spillovers when we use Bonferroni corrections to account for multiple testing.

[5]The algorithm gives priority to units bringing the largest number of net ties to the sample of focal units. Neighbors of focal candidate units are classified into auxiliary and focal observations. Then, in a given iteration the algorithm adds to the focal sample the unit with the largest difference between auxiliary and focal units, continuing in this fashion until no unit reports a positive difference between auxiliary and focal ties.

3.2 Tests for Heterogenous Effects

In this sub-section we implement the test introduced in Crump et al. (2008) to show that there is no evidence of treatment heterogeneity in the SPS and *Progresa* evaluations. To implement the test one first runs a regression separately in treatment groups $t \in \{0, 1\}$ of outcome Y_{it} on pre-treatment covariates X_{it} (along which there could be treatment heterogeneity). One then uses the estimated coefficients from both regressions to compute a test statistic to assess whether there is evidence of zero treatment effects across subgroups.

In particular, define n_t as the number of observations in treatment group t, and Ω_t as n_t times the heteroskedasticity consistent variance-covariance matrix of regression Y_{it} on X_{it}. The statistic to test for average zero treatment effects across sub-groups is then given by:

$$T = (\hat{\beta}_1 - \hat{\beta}_0)^\top \times \left(\frac{\hat{\Omega}_1}{n_1} + \frac{\hat{\Omega}_0}{n_0} \right) \times (\hat{\beta}_1 - \hat{\beta}_0). \quad (1)$$

where $\hat{\beta}_0$ represents a vector with the regression coefficients of regression Y_{it} on X_{it}. As discussed in Crump et al. (ibid.), under the null of zero treatment effects across subgroups, T is chi-squared distributed with K degrees of freedom (where K represents the number of pre-treatment covariates).

For each evaluation (SPS and *Progresa*) and sample (in the case of *Progresa* we analyze the original and GIS sample), we fit two regressions. The first regression includes as predictors in X_{it} poverty, the log of pre-treatment population, a Herfindahl-Hirschman index of political competition, and its interaction with poverty. In the second regression, we add to these covariates the partisan identity of municipal and state incumbents, and the interaction between the two, for the set of states that observed local elections before treatment assignment.[6]

Our choice of covariates is informed by existing work that provides predictions related to the conditions under which one may expect either of the policies we analyze to have an impact of electoral outcomes. For instance, Weitz-Shapiro (2012) finds that clientelism

[6]For the SPS evaluation we can only include the partisan identity of state incumbents because only two municipalities in the sample held elections after treatment assignment.

is more prevalent in environments with high poverty and low political competition. In such contexts local bureaucrats are able to condition the distribution of policy benefits on the political behavior of voters, which may translate into higher turnout and incumbent support at the polls. Similarly, Duch, Przepiorka, and Stevenson (2015) find in a lab experiment that individuals hold actors with proposal power responsible for collective decisions. Therefore, in the Mexican context perhaps local governments sharing partisan identity with the national incumbent are more effective in conveying how the national incumbent was instrumental in bringing about a given programmatic policy, thereby making voters more likely to reward the incumbent's party at the polls.

	SPS		Progresa (Original)		Progresa (GIS)	
	n = 90	n = 83	n = 417	n = 178	n = 410	n = 169
Turnout (Original)	0.218	0.190	0.718	0.561	0.377	0.887
Turnout (Official)	0.019	0.079	0.341	0.247	0.176	0.587
Incumbent (Original)	0.127	0.142	0.398	0.220	0.305	0.955
Incumbent (Official)	0.365	0.390	0.625	0.170	0.079	0.049
Incumbent (Registered)	0.141	0.352	0.348	0.334	0.618	0.428

Table 30: **p-values for Zero Treatement Effects Across Sub-Groups Test.** Each row reports p-values for the test of zero treatment effects introduced in Crump et al. (2008) for a given outcome across the different evaluations and samples. Columns (1), (3), and (5) report p-values associated with regressions omitting the identity of local incumbents; columns (2), (4), (6) report p-values when we include this information in the regressions to compute the statistic required for the test. The table reports no evidence of treatment effects different from zero across the evaluations and outcomes (Rejecting the null of zero average treatment effects when testing 10 hypothesis at the 5% level requires $p < \frac{0.05}{10} = 0.005$.

Table 30 reports the *p*-values associated with the tests of zero average treatment effects across subgroups introduced in (Crump et al., 2008). Each row reports *p*-values for a given outcome across the different evaluations and samples. The first column for each evaluation (or sample) reports *p*-values when the test omits the identity of local incumbents; the second column for each evaluation reports p-values when this information is included in the regressions required for the heterogeneity test. Of the 30 *p*-values reported in the table, 3 are below a critical value of 5%. However, once we make the appropriate Bonferroni corrections to account for multiple testing, we fall to reject the null of zero effects across all sub-groups and outcomes (Rejecting the null at a 5 percent level when testing 10 hypothesis requires $p < \frac{0.05}{10} = 0.005$).

3.3 Identification of Progresa Effects under a RD Design

This section reports the results from the identification strategy discussed in Section 3.3 of the main paper. Following Green (2006) we leverage the discontinuities generated in the enrollment of localities to *Progresa* to estimate the impact of this conditional cash transfer program on turnout and PRI support in the 2000 presidential, Proportional Representation (PR) senate, and simple plurality deputies elections (the latter two being the original electoral races that Green (ibid.) examined).

We first implement a sharp RD design to estimate the Intention-to-Treat (ITT) of *Progresa* on voter turnout and PRI support across all the electoral races of interest. To do this, we simply fit a non-parametric regression of the outcomes of interest on the poverty index around a neighborhood close to the cutoff for treatment assignment. The difference in the average value of the outcome between observations above and below the cutoff for treatment assignment gives the ITT estimate of *Progresa*. We report estimates separately for observations in the neighborhood of the first and second thresholds, and for the sample pooling observations from both cutoffs. As for the choice of estimation bandwidth, we rely precincts located within 0.1185 points from each of the cutoffs.[7] We report standard RD estimates as well as estimates incorporating the bias-correcting procedure proposed in Calonico, Cattaneo, and Titiunik (2014), and assess whether the estimates are sensitive to the use of uniform and triangular kernels.

We then implement a fuzzy RD design to estimate the Local Average Treatment Effects (LATEs) of *Progresa* on voter turnout and incumbent support. LATEs under a fuzzy RD design are estimates via two-stage least squares regression. In our specific application we first fit a non-parametric regression of each the two versions of the treatment (enrollment on the locality to *Progresa* or the share of household in a locality enrolled in the program) on an indicator for whether a locality is above a cutoffs to receive priority for program enrollment. In the second stage we then regress the outcome (PRI official vote share, PRI vote as a share of registered voters, and official turnout across each of the three elections of interest) on the predicted value of the treatment obtained from the

[7] The magnitude of the bandwidth represents the distance from the midpoint between Thresholds 1 and 2 to either cutoff.

first stage regression. As for the sharp RD analyses, we report estimates across different samples, accounting for bias-correction procedures, and relying on different kernels for the estimation of local treatment effects.

The key assumption for the identification of ITT effects under a sharp RD design is continuity of potential outcomes around the treatment encouragement threshold. To check the validity of the first assumption, in Figure 33 we graphically examine the reduced effect of Progresa enrollment on the following pre-treatment covariates: Log average number of households per locality (top left), and percent of localities with electricity (top right), with access to water (bottom left), and with access to drainage (bottom right). Neither of the panels in the figure suggest appreciable discontinuities in these covariates around the government cutoffs for *Progresa* enrollment. Figure 34-36 repeat the graphical analysis for the lag outcomes of interest across the presidential, PR senate, and simple plurality elections.[8] Again, we find that *Progresa* did not have any "effect" on lag PRI official vote share, lag PRI vote as a share of registered voters, and official turnout across any of the electoral races of interest. Identification of LATEs under a fuzzy RD design requires an monotonicity (i.e., no defiers) assumption. Although we cannot directly test the presence of defiers, it is unlikely that any localities of this type exist in the sample we analyze. Localities and households did not have the power to enroll in *Progresa* unless the government offered access to it; and there are no documented instances of a locality (or household) refusing *Progresa* once it was selected for enrollment into the program.

The panels in Figure 6 in Section 3.3 of the paper and Figures 37-38 below display reduced form effects of *Progresa* enrollment on PRI official vote share (left), PRI vote as a share of registered voters (center), and official turnout (right) in the 2000 presidential, PR senate, and simple plurality deputy elections. The figures display no distinguishable discontinuity in either of these outcomes around the government cutoffs (Threshold 1 and 2) for enrollment of localities to *Progresa*, suggesting the program did not affect electoral behavior in a meaningful way. The panels in Figure 39 report point estimates (and 95% confidence interval) of *Progresa*'s ITT on the electoral outcomes of interest

[8]For the presidential race we use the results from the 1994 election to measure lag outcomes. For the senate and deputy elections we rely on the returns from the 1997 midterm elections.

(dots for presidential, triangles for the PR senate, and squares for the simple plurality elections point estimates). The panels reports sharp RD estimates across all outcomes for the Threshold 1, Threshold 2, and Pooled samples when using a uniform (solid lines) and triangular kernel (dashed lines), relying on standard RD estimation (left column) and implementing the bias-correcting procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column), and based on the sample of all precincts (top row) and only those with a population of less than $2,500$ inhabitants (bottom row).[9] The panels display estimates not significantly different from zero across the different samples and specifications we consider. Figure 40 reports ITT estimates when we repeat the analysis to account for pre-treatment covariates in the estimation. Paralleling the figures reporting ITT estimates, Figures 41 and 42 report LATE estimates of *Progresa* locality enrollment on voter turnout and incumbent support. Following the same structure, Figures 43 and 44 report LATE estimates of the proportion of households enrolled in *Progresa* per locality on the outcomes of interest. Across all we find no evidence of *Progresa* having a positive and significant effect on voter turnout or incumbent support.

The lack of effects identified via the sharp and fuzzy RDD strategies is remarkable for two reasons. First, the sample we focus for the estimation avoids potential issues of attenuation bias by focusing on precincts that contain only one locality. And second, as the right panel in Figure 5 shows, the average proportion of households benefiting from *Progresa* experiences a significant increase around the government cutoffs for program enrollment.

[9] We have also considered including in the sample for the estimation of the ITTs only localities reporting 1000, 1500, and 3000 inhabitants and we obtaining substantively similar results as the one we report in this sub-section.

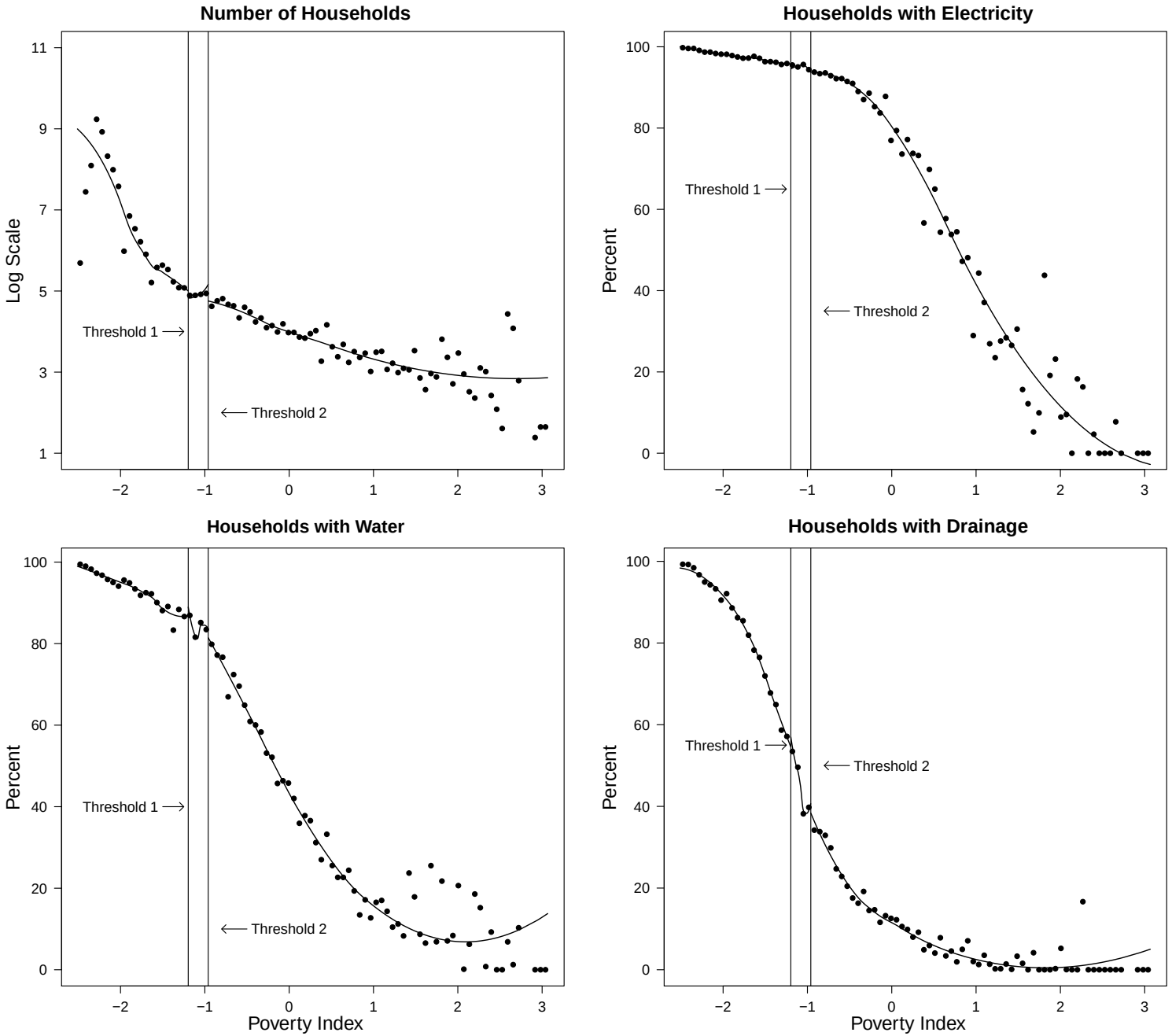

Figure 33: **Reduced Form RD Effects of Progresa on Pre-Treatment Covariates.** The panels in the figure display the average log number of households (top left), the percent of households with electricity (top right), water (bottom left), and drainage (bottom right) per locality as a function of the census poverty index. The panels display no discernable discontinuity in any of the pre-treatment covariates at the poverty index cutoffs that the government used to phase-in localities to *Progresa*.

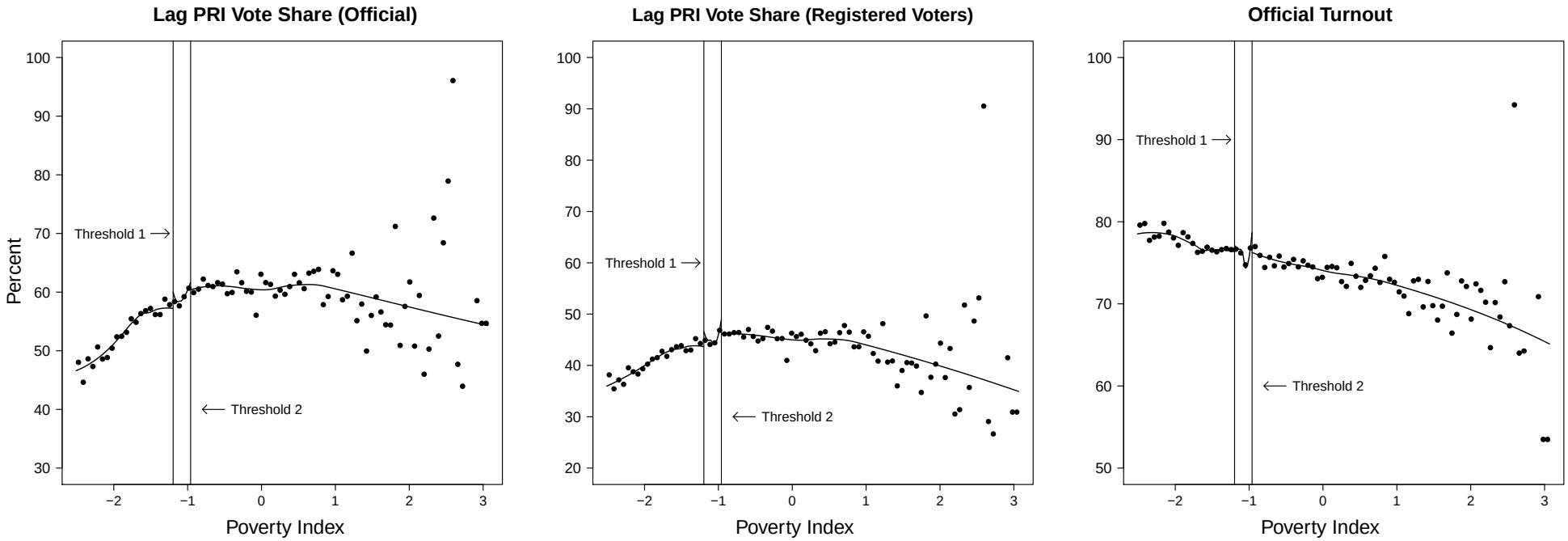

Figure 34: **Reduced Form RD Placebo Effects of Progresa on PRI Vote Share and Turnout in the 1994 Presidential Election (Lag Outcomes).** The panels in the figure display the average lag PRI official vote share (left), lag PRI vote as a share of registered voters (center), and lag official turnout (right) in the 1994 presidential election as a function of the census poverty index. The panels display no discernible discontinuity in any of the pre-treatment covariates at the poverty index cutoffs that the government used to phase-in localities to *Progresa*.

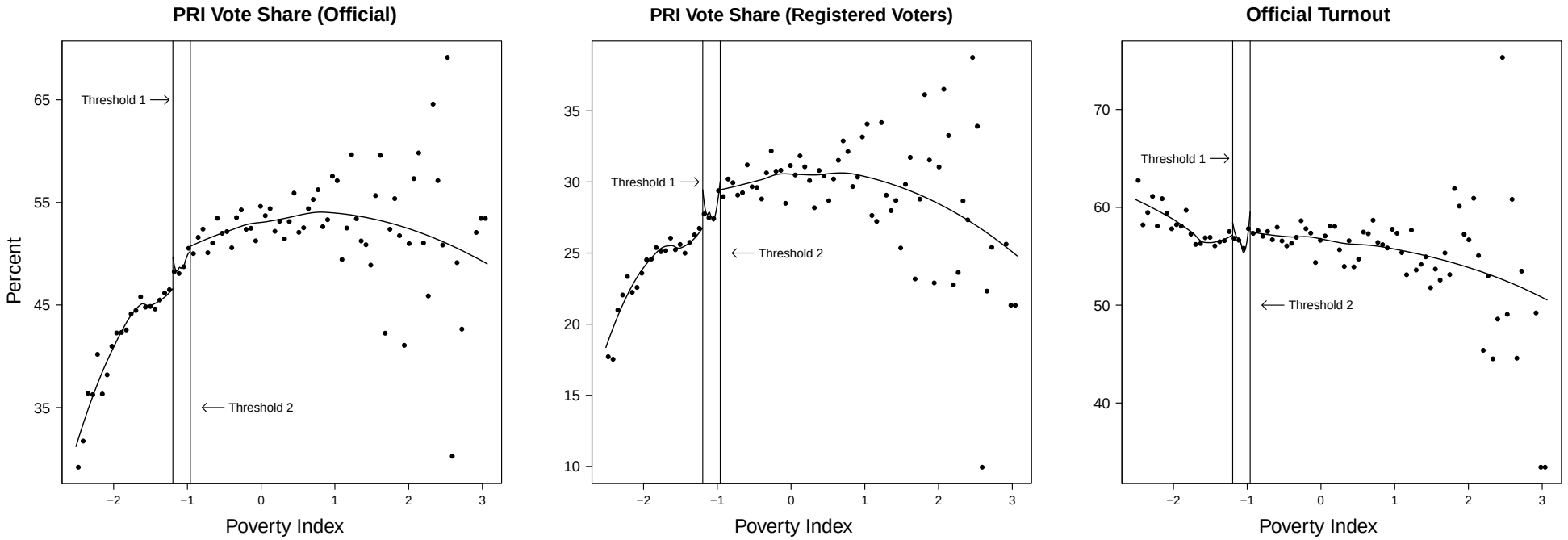

Figure 35: **Reduced Form RD Placebo Effects of Progresa on PRI Vote Share and Turnout in the 1997 PR Senate Election (Lag Outcomes).** The panels in the figure display the average lag PRI official vote share (left), lag PRI vote as a share of registered voters (center), and lag official turnout (right) in the 1997 senate election as a function of the census poverty index. The panels display no discernible discontinuity in any of the pre-treatment covariates at the poverty index cutoffs that the government used to phase-in localities to *Progresa*.

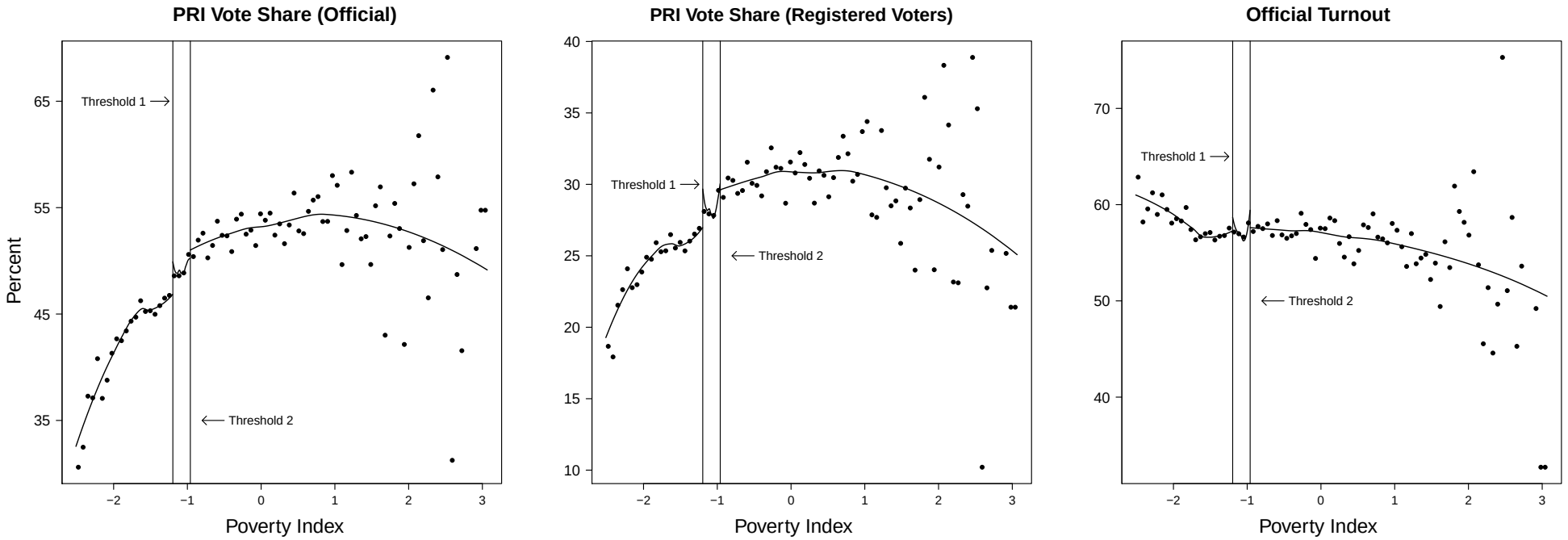

Figure 36: **Reduced Form RD Placebo Effects of Progresa on PRI Vote Share and Turnout in the 1997 Plurality Deputies Election (Lag Outcomes).** The panels in the figure display the average lag PRI official vote share (left), lag PRI vote as a share of registered voters (center), and lag official turnout (right) in the 1997 plurality deputies election as a function of the census poverty index. The panels display no discernible discontinuity in any of the pre-treatment covariates at the poverty index cutoffs that the government used to phase-in localities to *Progresa*.

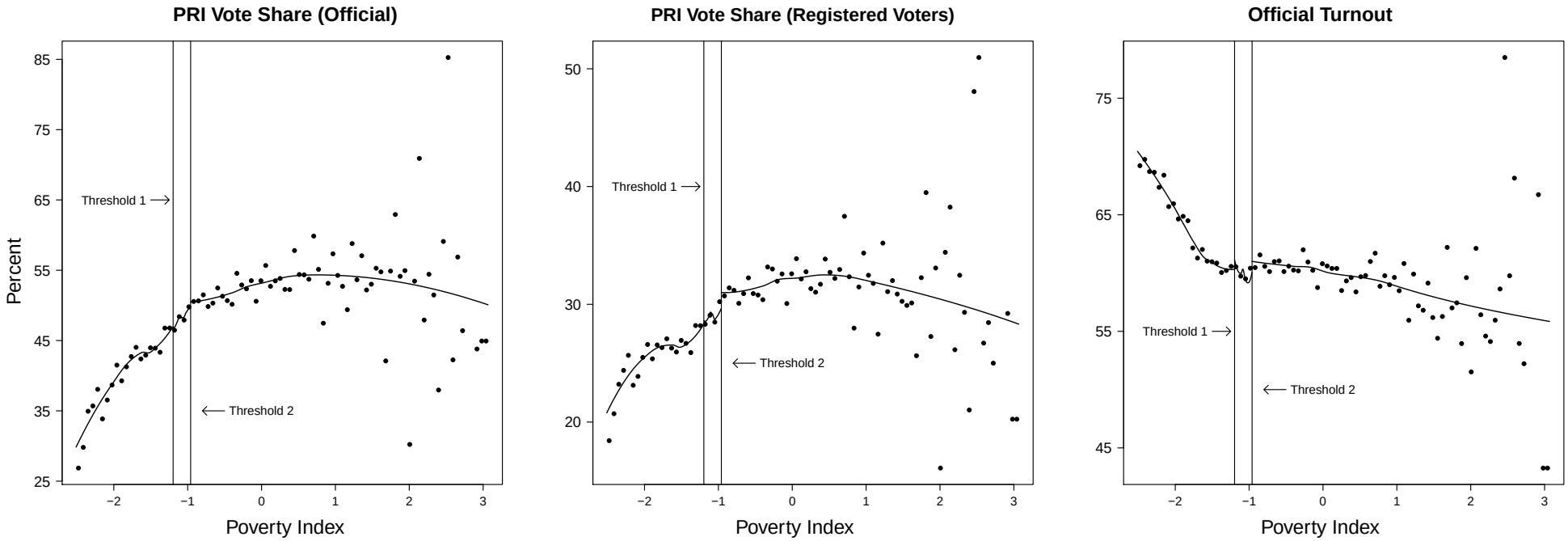

Figure 37: **Reduced Form RD Effects of Progresa on PRI Vote Share and Turnout in the 2000 PR Senate Election.** The panels display average official PRI vote share (left), PRI vote as a share of registered voters (center), and official turnout (right) in the 2000 senate election as a function of the poverty index. There is no discernible discontinuity in the outcomes at either of the government cutoffs used to phase-in localities to *Progresa*, indicating that the anti-poverty program did not have an effect on these electoral outcomes.

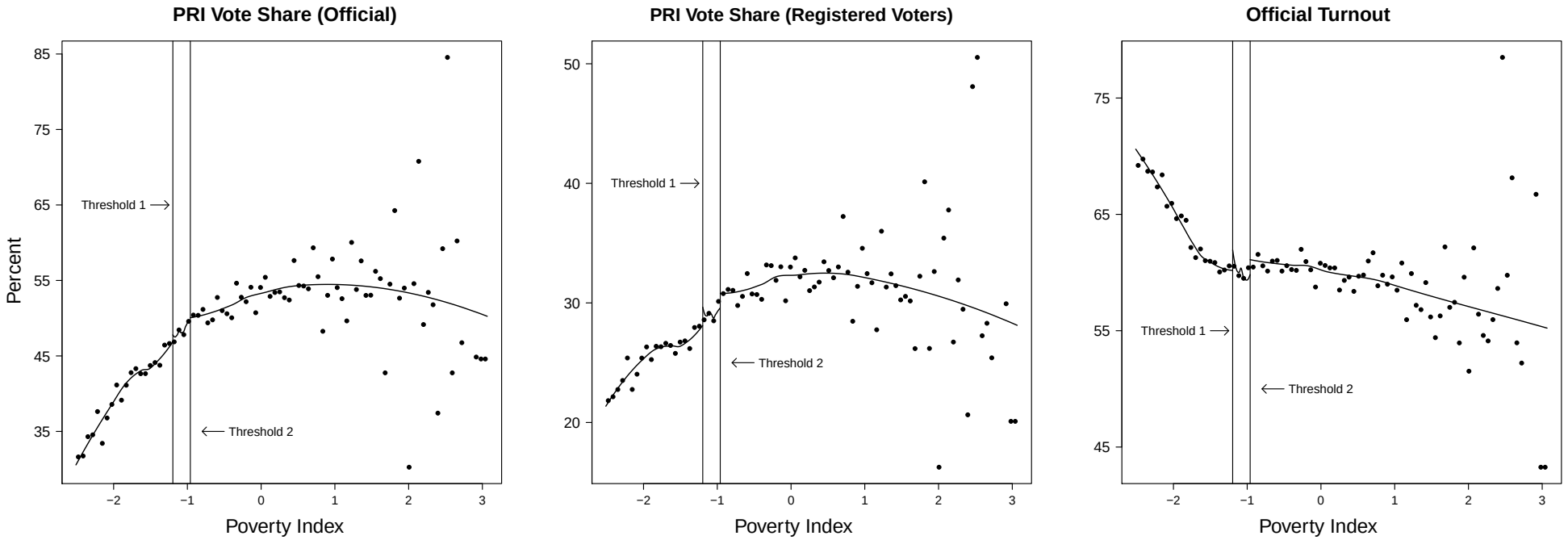

Figure 38: **Reduced Form RD Effects of Progresa on PRI Vote Share and Turnout in the 2000 Plurality Deputies Election.** The panels display average official PRI vote share (left), PRI vote as a share of registered voters (center), and official turnout (right) in the 2000 deputies election as a function of the poverty index. There is no discernible discontinuity in the outcomes at either of the government cutoffs used to phase-in localities to *Progresa*, indicating that the anti-poverty program did not have an effect on these electoral outcomes.

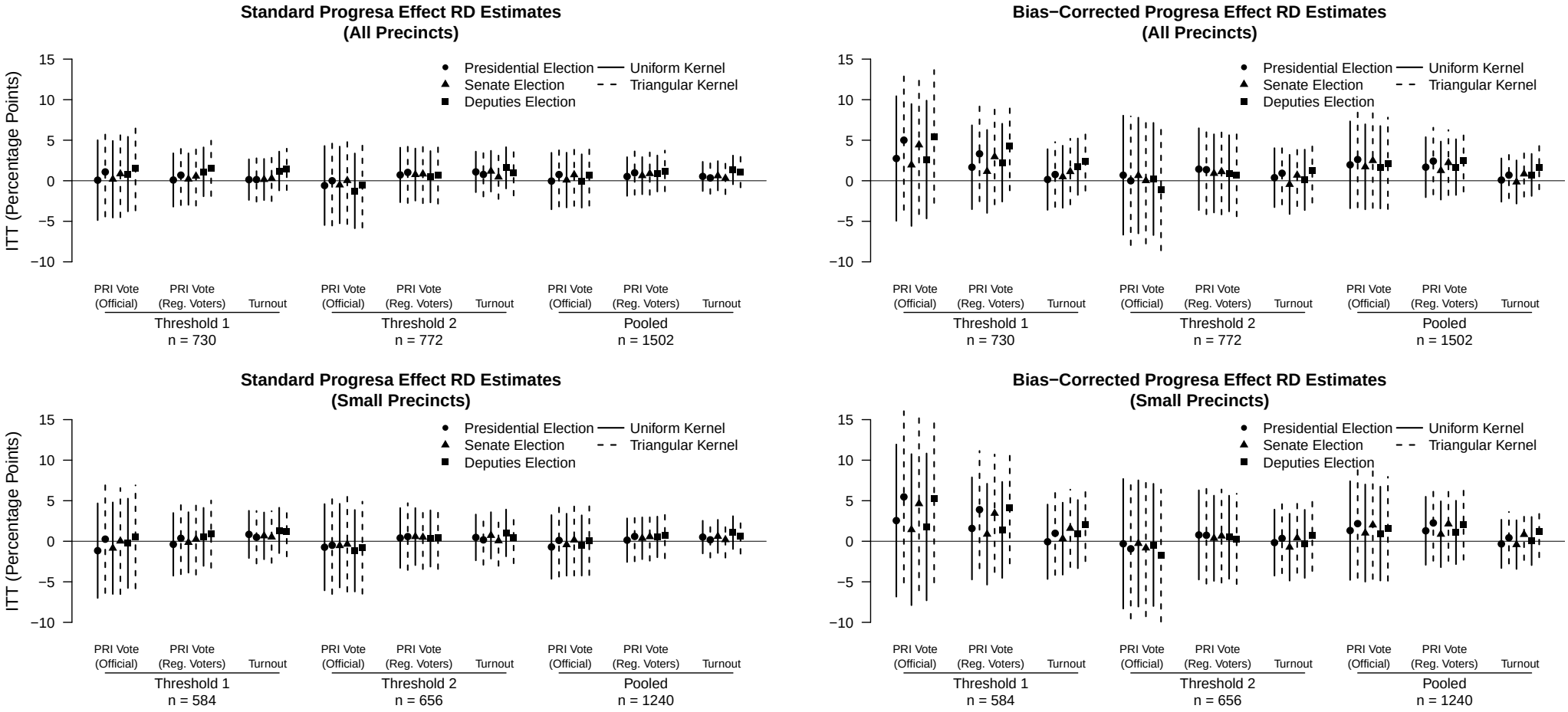

Figure 39: **RD ITT Estimates of Progresa on Electoral Outcomes (No Pre-treatment Covariates).** The figure reports RD point estimates (and 95% confidence intervals) of the Intention-to-Treat Effect (ITT) of Progresa on PRI official vote share, PRI vote as a share of registered voters, and official turnout in the 2000 presidential (dots), PR senate (triangles), and simple plurality deputies (squares) elections relying on a uniform (solid lines) and triangular (dashed lines) kernel across the threshold 1, threshold 2, and pooled samples. The panels group estimates according to whether the estimation relies on all precincts (top row) or only those with a population less than 2500 inhabitants (bottom row), and according to whether the estimates are standard (left column) or incorporate the bias-correction procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column). The figures shows that across all samples and specifications there is no evidence Progresa had any significant effect on incumbent support or turnout.

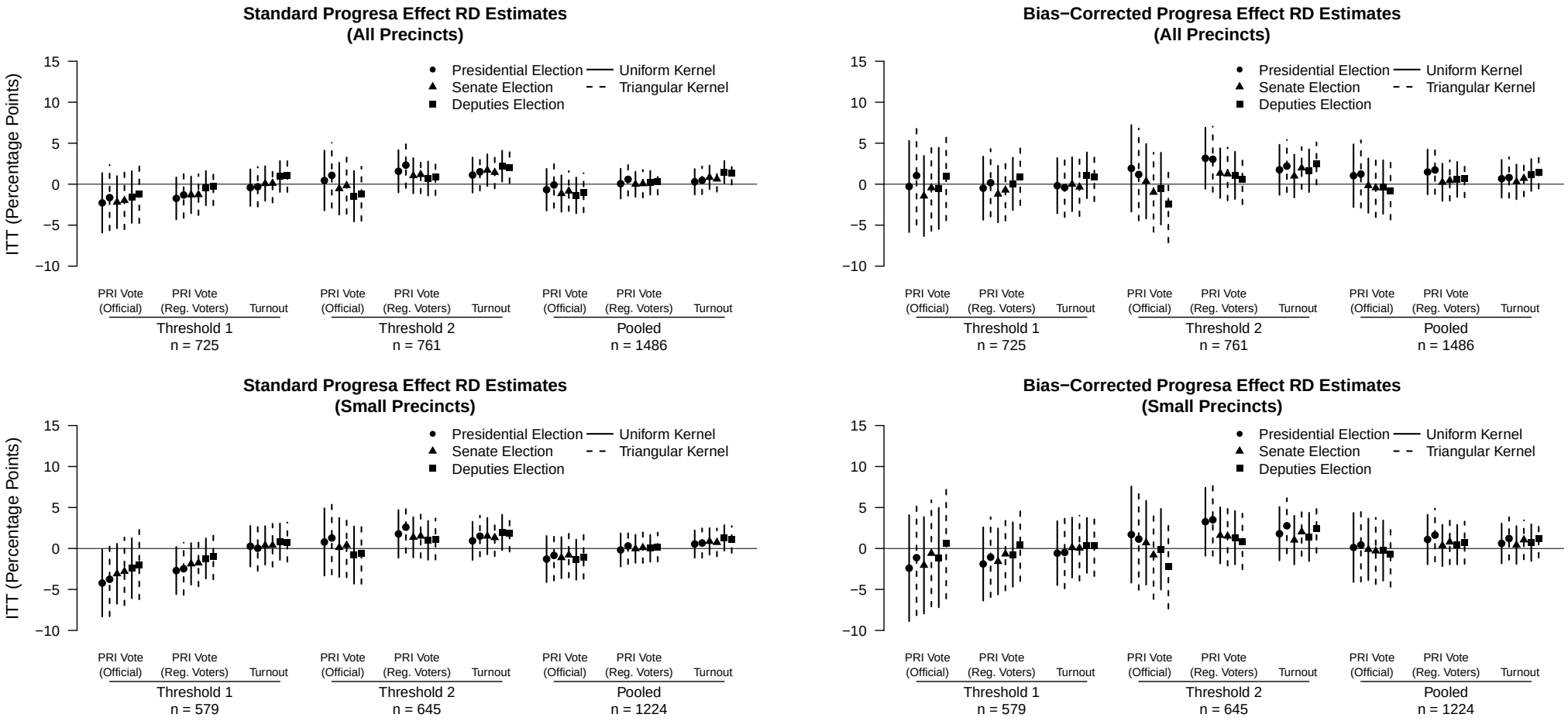

Figure 40: **RD ITT Estimates of Progresa on Electoral Outcomes (Controlling for Pre-treatment Covariates).** The figure reports RD point estimates (and 95% confidence intervals) of the Intention-to-Treat Effect (ITT) of Progresa on PRI official vote share, PRI vote as a share of registered voters, and official turnout in the 2000 presidential (dots), PR senate (triangles), and simple plurality deputies (squares) elections relying on a uniform (solid lines) and triangular (dashed lines) kernel across the threshold 1, threshold 2, and pooled samples. The panels group estimates according to whether the estimation relies on all precincts (top row) or only those with a population less than 2500 inhabitants (bottom row), and according to whether the estimates are standard (left column) or incorporate the bias-correction procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column). All estimates control for pre-treatment electoral outcomes and log population. The figures shows that across all samples and specifications there is no evidence Progresa had any significant effect on incumbent support or turnout.

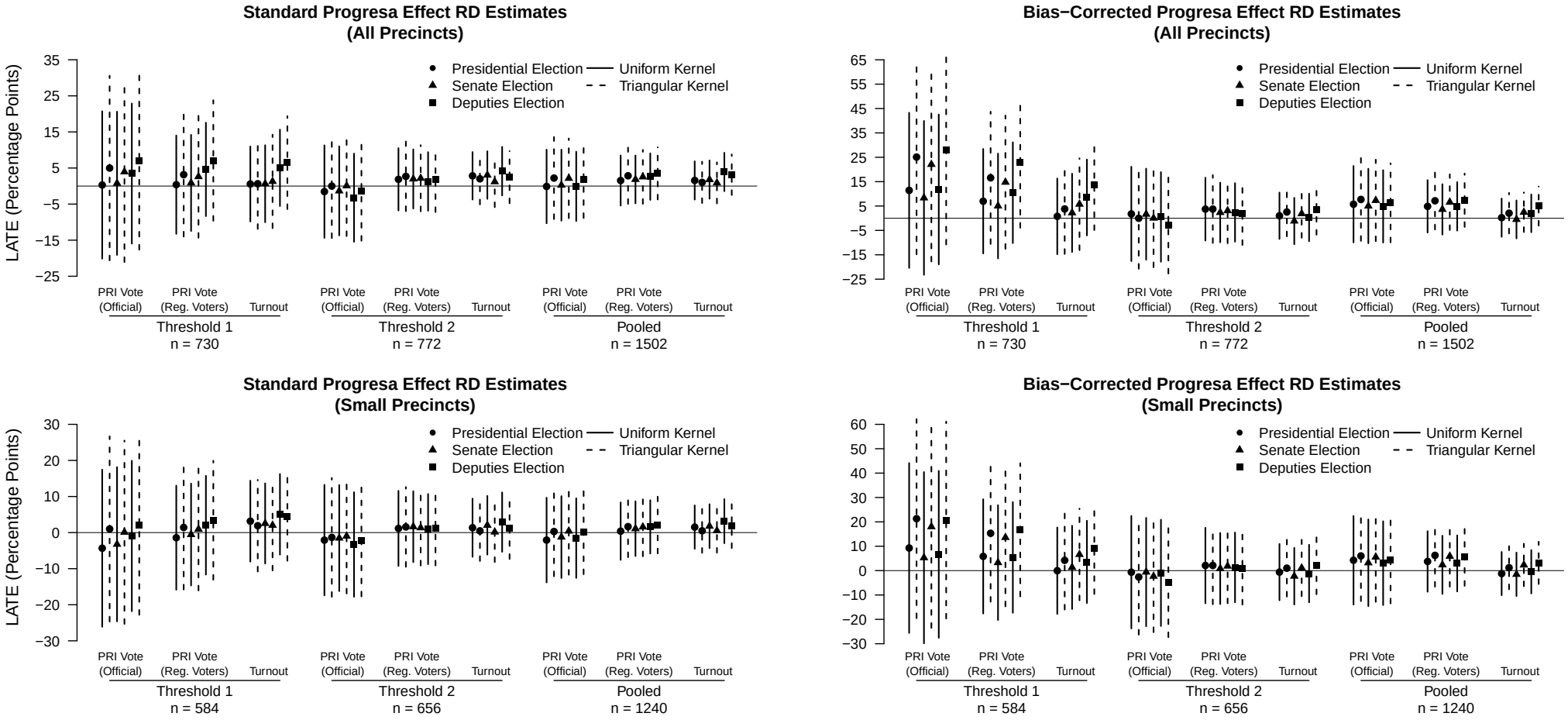

Figure 41: **LATE Estimates of Progresa on Electoral Outcomes (Treatment is whether locality is enrolled in Progresa; No Pre-treatment Covariates).** The figure reports point estimates (and 95% confidence intervals) of the Local Average Treatment Effect (LATE) of a locality enrolling in Progresa on PRI official vote share, PRI vote as a share of registered voters, and official turnout in the 2000 presidential (dots), PR senate (triangles), and simple plurality deputies (squares) elections relying on a uniform (solid lines) and triangular (dashed lines) kernel across the threshold 1, threshold 2, and pooled samples. The panels group estimates according to whether the estimation relies on all precincts (top row) or only those with a population less than 2500 inhabitants (bottom row), and according to whether the estimates are standard (left column) or incorporate the bias-correction procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column). The figures shows that across all samples and specifications there is no evidence Progresa had any significant effect on incumbent support or turnout.

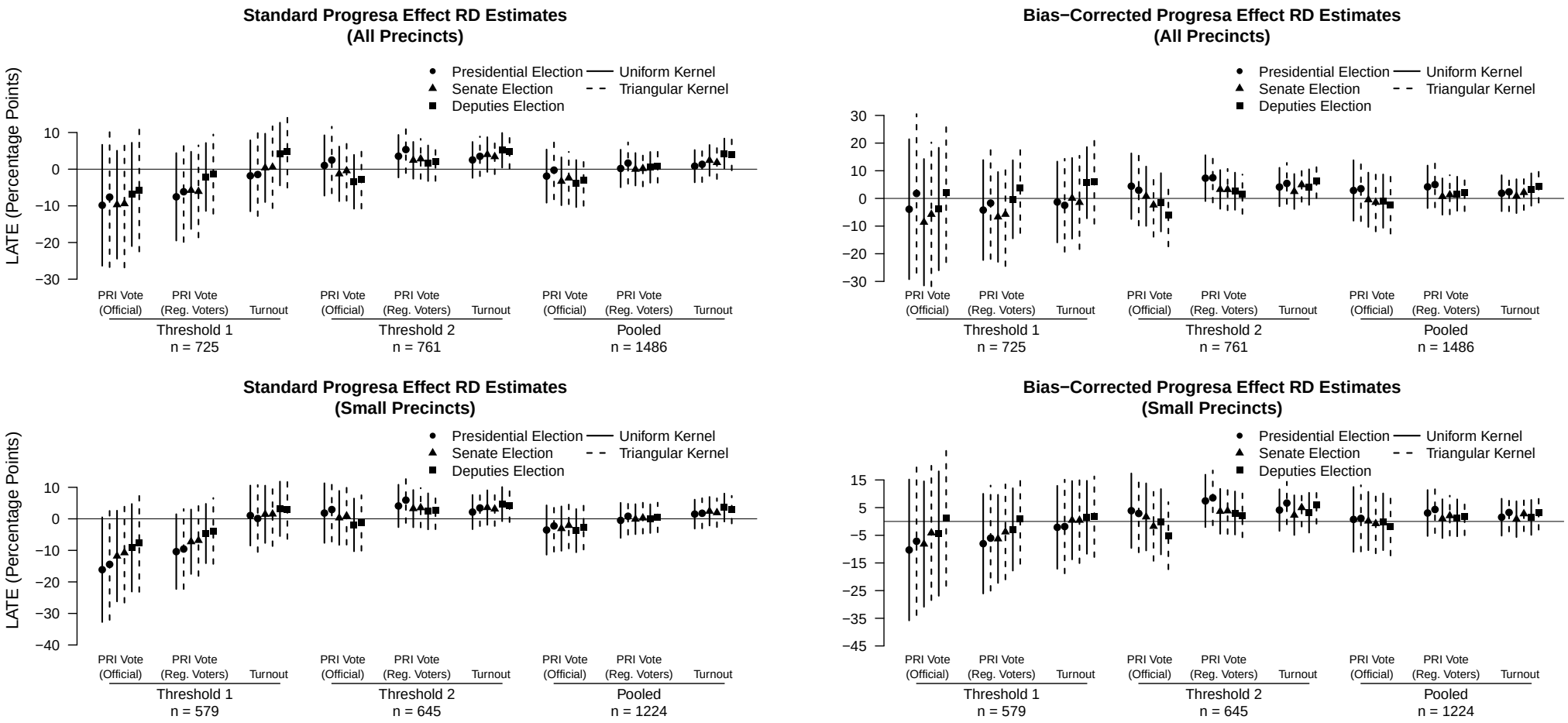

Figure 42: **LATE Estimates of Progresa on Electoral Outcomes (Treatment is whether locality is enrolled in Progresa; Controlling for Pre-treatment Covariates).** The figure reports point estimates (and 95% confidence intervals) of the Local Average Treatment Effect (LATE) of a locality enrolling in Progresa on PRI official vote share, PRI vote as a share of registered voters, and official turnout in the 2000 presidential (dots), PR senate (triangles), and simple plurality deputies (squares) elections relying on a uniform (solid lines) and triangular (dashed lines) kernel across the threshold 1, threshold 2, and pooled samples. The panels group estimates according to whether the estimation relies on all precincts (top row) or only those with a population less than 2500 inhabitants (bottom row), and according to whether the estimates are standard (left column) or incorporate the bias-correction procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column). All estimates control for pre-treatment electoral outcomes and log population. The figures shows that across all samples and specifications there is no evidence Progresa had any significant effect on incumbent support or turnout.

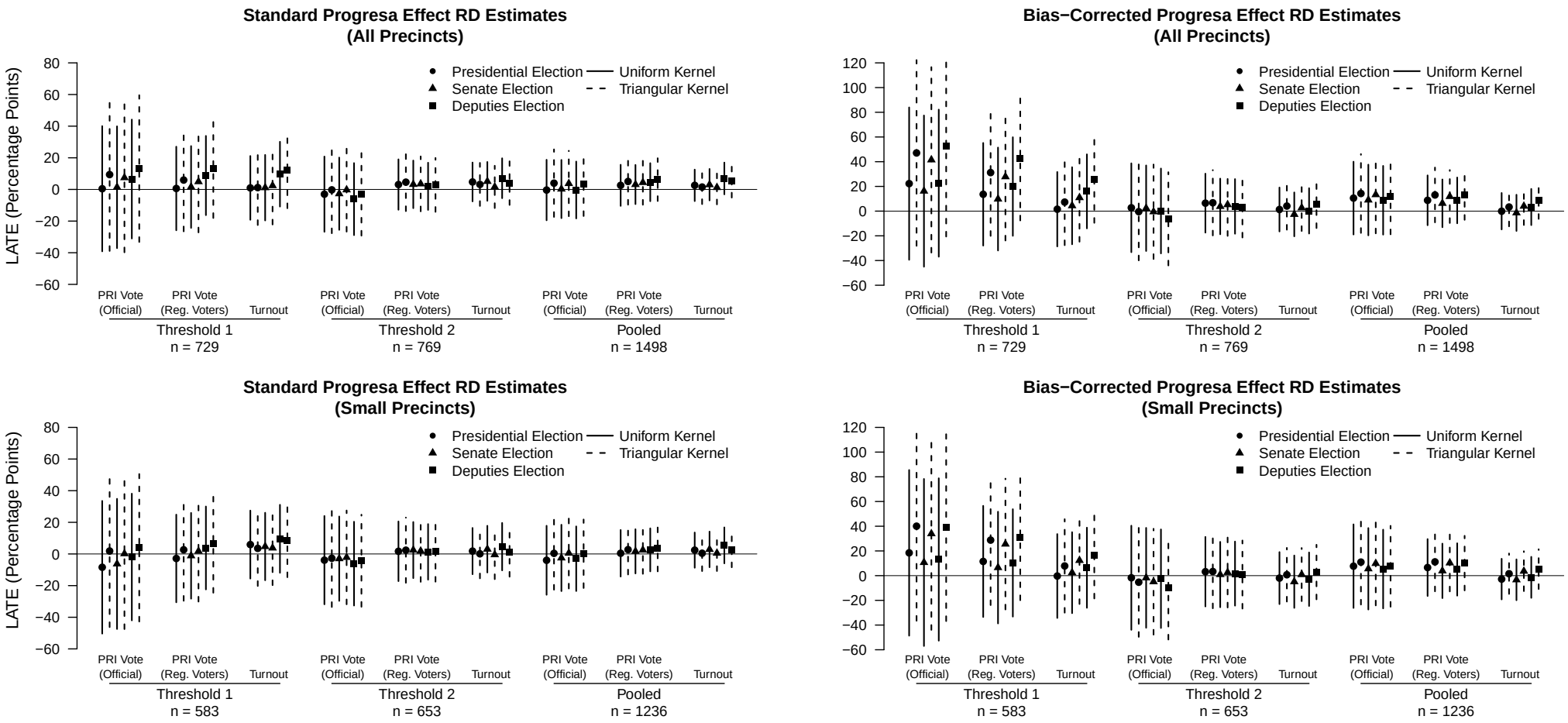

Figure 43: **LATE Estimates of Progresa on Electoral Outcomes (Treatment is Share of Families in Progresa; No Pre-treatment Covariates).** The figure reports point estimates (and 95% confidence intervals) of the Local Average Treatment Effect (LATE) of an increase in the proportion of families receiving Progresa on PRI official vote share, PRI vote as a share of registered voters, and official turnout in the 2000 presidential (dots), PR senate (triangles), and simple plurality deputies (squares) elections relying on a uniform (solid lines) and triangular (dashed lines) kernel across the threshold 1, threshold 2, and pooled samples. The panels group estimates according to whether the estimation relies on all precincts (top row) or only those with a population less than 2500 inhabitants (bottom row), and according to whether the estimates are standard (left column) or incorporate the bias-correction procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column). The figures shows that across all samples and specifications there is no evidence Progresa had any significant effect on incumbent support or turnout.

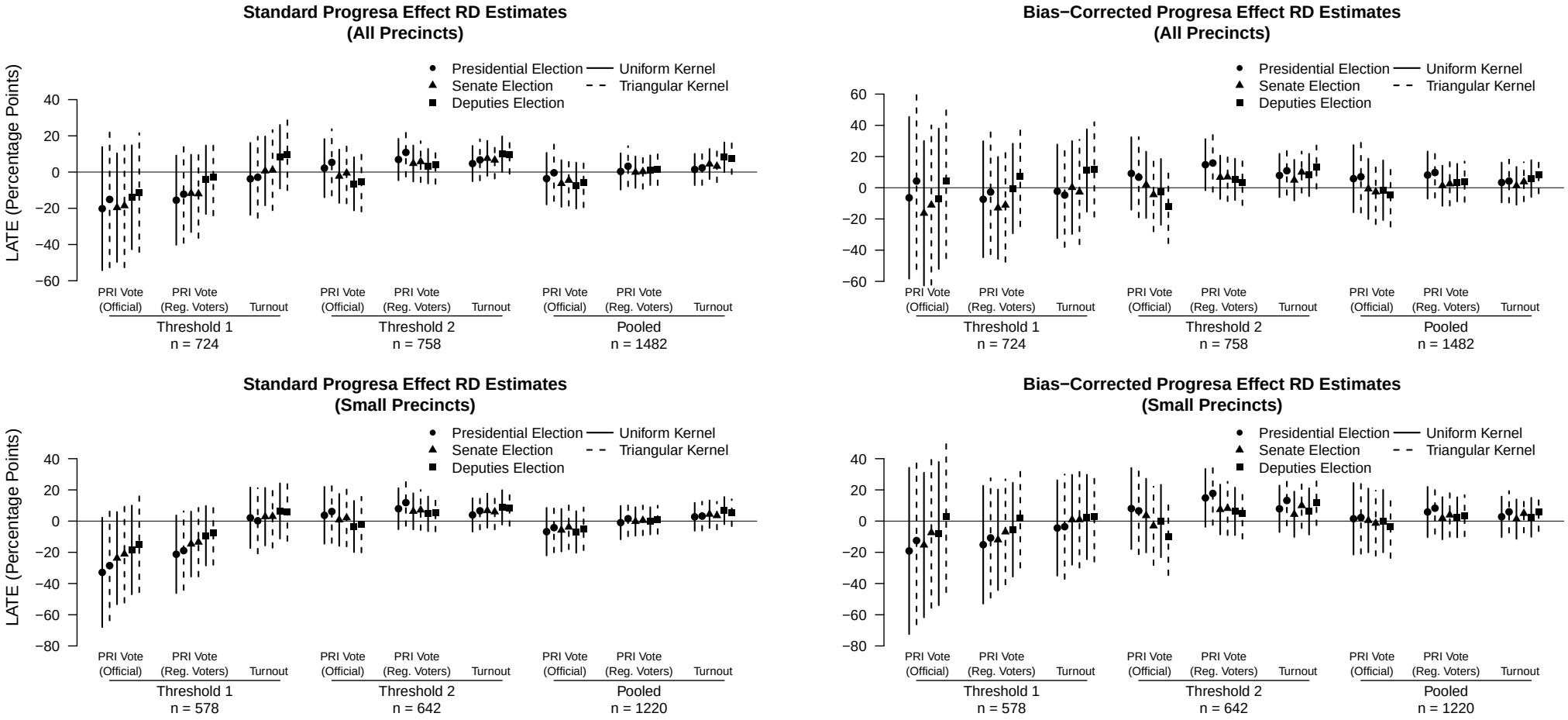

Figure 44: **LATE Estimates of Progresa on Electoral Outcomes (Treatment is Share of Families in Progresa; Controlling for Pre-treatment Covariates).** The figure reports point estimates (and 95% confidence intervals) of the Local Average Treatment Effect (LATE) of an increase in the proportion of families Progresa on PRI official vote share, PRI vote as a share of registered voters, and official turnout in the 2000 presidential (dots), PR senate (triangles), and simple plurality deputies (squares) elections relying on a uniform (solid lines) and triangular (dashed lines) kernel across the threshold 1, threshold 2, and pooled samples. The panels group estimates according to whether the estimation relies on all precincts (top row) or only those with a population less than 2500 inhabitants (bottom row), and according to whether the estimates are standard (left column) or incorporate the bias-correction procedure introduced in Calonico, Cattaneo, and Titiunik (2014) (right column). All estimates control for pre-treatment electoral outcomes and log population. The figures shows that across all samples and specifications there is no evidence Progresa had any significant effect on incumbent support or turnout.

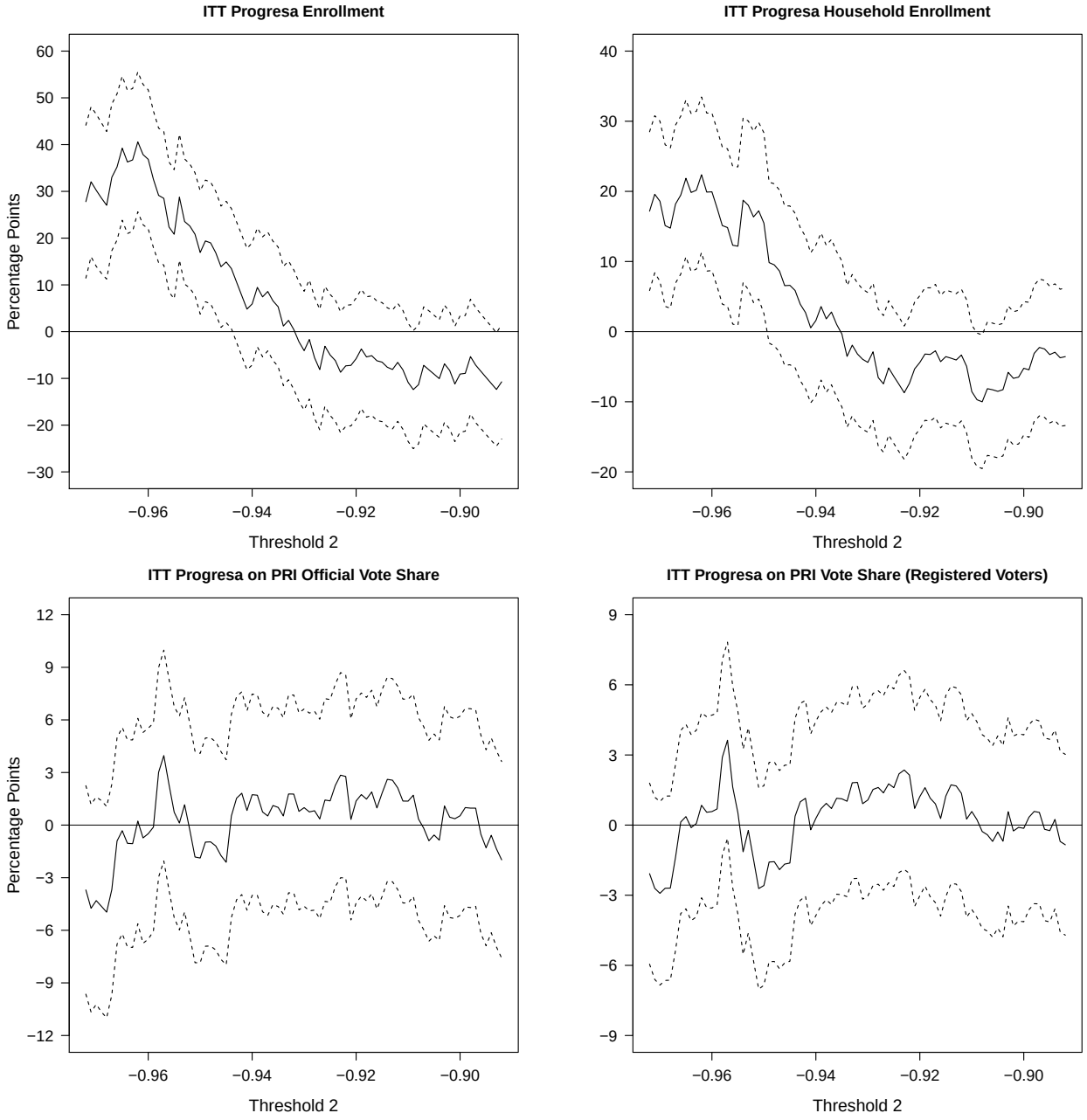

Figure 45: **ITT of Encouragement on Progresa Enrollment and ITT of Progresa on PRI Vote Share by Different Values of Threshold 2.** The top panel shows point estimates (and 95% confidence interval) of the impact of different values of the Threshold 2 encouragement on *Progresa* locality (left) and household (right) enrollment. Only Threshold 2 encouragement values lower than -0.944 and -0.95 have a positive and significant effect on locality and household *Progresa* enrollment, respectively. When Threshold 2 is -0.932 (i.e., the value separating the poorest three quintiles from the two richest quintiles among localities with moderate poverty) the effect of the encouragement on enrollment is virtually zero. The bottom panel shows the ITT of *Progresa* enrollment on PRI vote share is not statistically different from zero regardless of the value of Threshold 2. The estimates reported in Section 3 rely on Threshold 2 set equal to -0.96.

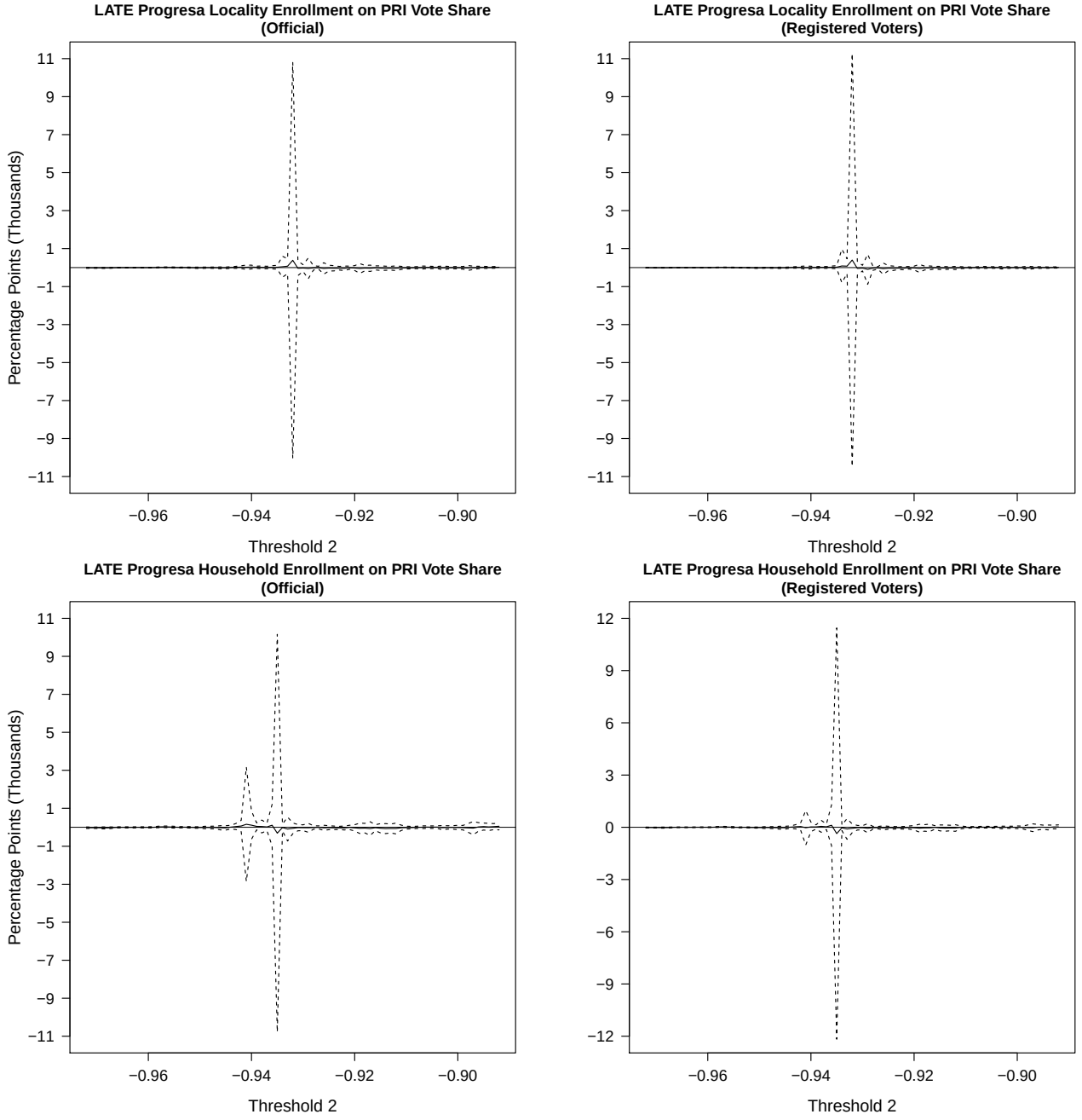

Figure 46: **LATE of Progresa on PRI Vote Share by Different Values of Threshold 2.** The top panels report point estimates (and 95% confidence interval) of the *Progresa* LATE on PRI Vote Share for different values of the Threshold 2 treatment assignment cutoff. The top panel report estimates when the treatment is *Progresa* locality enrollment and the bottom panel when the treatment is *Progresa* household enrollment. The figure shows that regardless of Threshold 2 value, the *Progresa* LATE estimate is not statistically different from zero. The estimates of the *Progresa* LATE become unrealistically large for for certain values close or equal to -0.932 (i.e., the value separating the poorest three quintiles from the two richest quintiles among localities with moderate poverty). This is mainly the result of the small effect the encouragement has on *Progresa* enrollment for these Threshold 2 values.

4 Additional Theoretical Analysis

In the version of the formal theory proposed in De La O (2015) closest to the contexts in which *Progresa* and SPS were passed and implemented, the incumbent president's party (P) proposes a CCT that is partisan (PCCT, i.e., over which incumbents have discretion) or nonpartisan (NCCT, i.e., a programmatic policy), and the median opposition party legislator (L_O) then decides whether to pass the program. Under the status quo, P receives payoff p^{sq}. Denote the value of clientelism, which is realized only if PCCT is passed, as v_1 for P and v_2 for L_O, where $v_1 > v_2$. Then, if PCCT is passed, the total payoff for P is $p + v_1 - v_2$, where p is the probability P being reelected if NCCT is passed. The payoff for L_O passing PCCT mirrors that of the incumbent: $1 - p - (v_1 - v_2)$. If L_O rejects PCCT, the payoffs for P and L_O are $p^{\text{sq}} - e$ and $1 - p^{\text{sq}} + e$, respectively, where $e > 0$ "represents the payoff from supporting a nonclientlist poverty relief program, or rejecting a clientelist program, when the other player does not" De La O (ibid., p. 50). The book also assumes $p > p^{\text{sq}}$ "because the president can claim credit for the policy innovation." Thus, L_O will only pass PCCT if $1 - p - (v_1 - v_2) > p^{\text{sq}} - e$, a condition which never holds.

Alternatively, if P proposes an NCCT, and the opposition passes it, P and L_O obtain payoffs of p and $1-p$, respectively. If the opposition does not pass the NCCT, the payoffs are $p^{\text{sq}} + e$ and $1 - p^{\text{sq}} - e$. Thus, De La O (ibid.) shows if P proposes an NCCT, the opposition will pass it if the cost of passing the legislation is less than the cost of blocking it or, in other words:

$$p - p^{\text{sq}} < e. \tag{2}$$

Then, the incumbent, knowing that the opposition party will never pass a PCCT, is better off with the status quo than PCCT (since $p^{\text{sq}} > p^{\text{sq}} - e$) and so never proposes a PCCT in the first place. Then, if an NCCT that P proposes doesn't pass, P gets payoff $p^{\text{sq}} + e$; if it passes, P gets p. Thus, because P is better off proposing an NCCT, regardless of what L_O does, the equilibrium result is for P to propose and L_0 to pass the NCCT.

The result in equation 2 from De La O (ibid.) shows that there exist conditions under which the opposition may pass policies that could hurt them. From this result, we now

derive two new implications that may be worthy of further study.

First, we show that the theory is also consistent with the opposite result, that the programmatic incumbent support hypothesis is false. Suppose that incumbents receive little or no benefits from passing an NCCT, i.e., $p \approx p^{sq}$. In this situation, Equation 2 still holds if e is sufficiently large. As such, the theory is consistent with the programmatic incumbent support hypothesis being false and also with it being true; as a result, even if the theory itself is true, it provides no information about the veracity of the programmatic incumbent support hypothesis.

Second, although the theory does not imply the programmatic incumbent support hypothesis, we analyze here whether it is possible to estimate the parameters of the theory (e.g., p, p^{sq}, and e) to test this hypothesis from the experiment, as claimed by De La O (2015). As it turns out, this is not possible. The reason is that estimation would require observing the counterfactual case when an NCCT (or a programmatic policy more generally), was proposed but not passed. However, in the treated and control conditions of both experiments, the policy was proposed and passed for *every* observation, making it impossible to identify most parameters of the theory, including the causal effect of the opposition rejecting vs accepting the proposal, from either the SPS or *Progresa* experiment. Instead, each of the two experiments compares areas where the proposed-and-passed policy was implemented vs not implemented. This is an important quantity, relevant to the programmatic incumbent support hypothesis, but it cannot be used to test the theory in De La O (ibid.).

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