Conditional Cash Transfers and Electoral Behavior: A Reassessment of a Randomized Experiment in Mexico

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Abstract

In this paper, we re-examine a recent finding that Progresa, Mexico's conditional cash transfer (CCT) program, mobilized voters in the 2000 presidential elections in favor of the incumbent party. First, after running several sensitivity analyses and balance checks, we find that the significance of these results does not hold when two categories of outlier observations are dropped from the dataset. Second, using the R package Amelia II, we impute a handful of missing observations in the dataset that could possibly bias results. When missing data is corrected for, we do confirm the effect of early assignment to Progresa on incumbent vote share in the IV model, a result consistent with the original findings. Third, we create OLS and IV models that account for heterogeneous variation in socioeconomic covariates. The results from the new models disprove the finding that CCTs had the effect of mobilizing voters for the incumbent. Instead, we find that although Progresa's favorable electoral effects for incumbents do hold, contrary to the existing finding, incumbent electoral gains are not a result of voter mobilization, but rather of vote choice switching away from opposition parties. Moreover, this vote-switching effect is only observed in high poverty precincts and precincts with high voter registration.

Keywords: Conditional cash transfer programs, voter mobilization, incumbent vote share, Mexico, Progresa, clientelism, programmatic parties.

1 Introduction

In her 2012 article in APSR, De La O[1] exploits the randomized component of Progresa, the anti-poverty conditional cash transfer (CCT) implemented in Mexico between 1997-2000, to examine the impact of conditional cash transfer programs on electoral behavior. She finds that early enrollment in the program led to a notable 7% increase in voter turnout and a 9% increase in incumbent vote share for the 2000 presidential election. She draws two key implications from her results: 1) exposure to Progresa allowed for voter mobilization in favor of the incumbent, yet had no effect on support for opposition parties, and 2) targeted programs, such as Progresa, affect voting behavior for programmatic as opposed to clientelistic reasons.

We expand on De La O's (2012) systematic analysis in several ways. We run several sensitivity and balance checks to determine whether her results hold under alternative specifications. Second, we test her theory that the favorable electoral effects for incumbents are not a result of vote choice switching away from the opposition party to the incumbent one, but rather of the mobilization of particular pro-incumbent groups. To do this, we use new models that account for heterogeneity across the socioeconomic covariates for the precincts in the sample. Third, we employ the R package Amelia II to impute missing observations in our dataset that could possibly bias results. Given the lack of additional data, the handful of missing values for the variables of interest could skew the analysis.

After conducting various sensitivities analyses where we identify and drop or rescale two categories of outlier observations in the data, we find that the original results demonstrating the electoral effects of Progresa are no longer statistically significant. Furthermore, our tests for heterogeneity effects in the original results using our new models show a different story: although Mexico's CCT program does result in gains for the incumbent, these gains cannot be explained by a mobilizing mechanism. Instead, we find that early enrollment in the program does not affect (i.e., has a small and statistically insignificant effect on) voter turnout or the incumbent vote share. It does, however, have a negative effect on the opposition's share of votes, and this effect is pronounced in the poorer precincts as well as the large precincts with a high number of registered voters.

2 Sensitivity Analyses of De La O's (2012) Results

We conduct several sensitivity diagnostics of the author's results. First, we test the validity of De La O's use of early assignment to Progresa as an instrument for the effects of Progresa on electoral behavior. We conduct an F-statistic test of the treatment variable in her first model (Model 1), and get a higher F-statistic than the one reported in her results.

[1] De La O, Ana, 2012, "Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico." *American Journal of Political Science* 57 (1): 1 - 14.

This finding supports De La O's choice of early enrollment as an instrument. Next, we test the validity of De La O's claim that assignment to treatment (exposure to Progresa) is randomized. We conduct a Hausman test to determine whether the coefficient estimates for her OLS model differs from those in her IV model. The results from the test suggest that random assignment in treatment is actually endogenous, undermining the claim of causality for De La O's findings in the OLS model. In this regard, it seems her inclusion of an instrumental variable model is appropriate.

Additionally, we run several diagnostics of the residuals and standard errors reported in the paper. We run the model with robust standard errors and find that the robust values are close to the author's reported values. We closely examine the residuals, making residual QQ plots to test for any evidence of non-normality. We also run CR plots for all the covariates, and find evidence of non-linearity in $poptot1994$, 1994 precinct total population variable. This finding led us to run an influence plot to help us identify the observations that drive the non-linearity in the population variable.

From the influence plots, we single out two outlier observations, two high population precincts (1994 total population values of 88,205 and 102,332 compared to a median of 1,164) that drive De La O's results. We examine the data by taking a closer look at these two identified outliers and their effect on the main result (i.e., the the effect of early assignment to Progresa on voter turnout). Excluding these two observations from the CR plots resolves issues of non-linearity for $poptot1994$. When we remove these two highly influential observations and re-run both the OLS and instrumental variables model, with early assignment to Progresa as the instrument, the effect of treatment more or less disappears (see Table 1). In this regard, dropping these observations is justified even if this is not a data entry or measurement error. If these precincts are actually extremely large compared to all other precincts in the sample, then results in these precincts are likely to be not generalizable to the vast majority of precincts in Mexico.

Another aspect of the data that we explored was the presence of thirteen districts with impossibly high turnout (over 100%). We treat these observations as missing data and employ list-wise deletion to handle them . When we re-run the IV regression model without the suspect observations, the effect on turnout disappears once again. We also run the same models after rescaling the 13 observations and find that there is still no effect of treatment on turnout (see Table 2). [2]

[2]The method of rescaling was as follows. Given that the ratios between the three parties' vote shares and the total turnout was very similar for these 13 observations compared to the rest of the sample, we simply set total turnout to 100% and rescaled the parties' vote shares' at corresponding levels. We ran the same ITT and IV models after this rescaling and found that the results presented in Table 2 hold. These results are not included in the paper but are available upon request

Table 1: Revised Models: Excluding High Population Outliers

	ITT Estimates of Assignment to Early versus Late Treatment:			
	Turnout	PRI	PAN	PRD
	(1)	(2)	(3)	(4)
Treatment	0.036	0.031*	0.004	−0.003
	(0.028)	(0.016)	(0.012)	(0.012)
Constant	0.652***	0.387***	0.129*	0.163**
	(0.159)	(0.090)	(0.071)	(0.067)
Village Count Fixed Effects	Yes	Yes	Yes	Yes
Observations	456	456	456	456
R^2	0.189	0.318	0.240	0.351
Adjusted R^2	0.152	0.286	0.205	0.321
Residual Std. Error (df = 435)	0.272	0.154	0.121	0.115
F Statistic (df = 20; 435)	5.067***	10.120***	6.871***	11.748***
	IV Estimates of Early Progresa Coverage:			
	Turnout	PRI	PAN	PRD
	(1)	(2)	(3)	(4)
Early Progresa	0.003	0.036	−0.017	−0.033
	(0.051)	(0.029)	(0.023)	(0.022)
Constant	0.685***	0.383***	0.150**	0.191***
	(0.165)	(0.093)	(0.073)	(0.070)
Village Count Fixed Effects	Yes	Yes	Yes	Yes
Observations	455	455	455	455
R^2	0.186	0.317	0.235	0.341
Adjusted R^2	0.149	0.286	0.200	0.311
Residual Std. Error (df = 434)	0.273	0.154	0.121	0.116

*p<0.1; **p<0.05; ***p<0.01

Table 2: Revised Models: Excluding Vote Share $> 100\%$ Outliers

	ITT Estimates of Assignment to Early versus Late Treatment:			
	Turnout	PRI	PAN	PRD
	(1)	(2)	(3)	(4)
Treatment	0.016	0.016	−0.003	−0.002
	(0.016)	(0.012)	(0.009)	(0.010)
Constant	0.762***	0.452***	0.145***	0.180***
	(0.094)	(0.071)	(0.052)	(0.056)
Village Count Fixed Effects	Yes	Yes	Yes	Yes
Observations	446	446	446	446
R^2	0.220	0.386	0.292	0.382
Adjusted R^2	0.183	0.357	0.259	0.352
Residual Std. Error (df = 425)	0.160	0.121	0.089	0.095
F Statistic (df = 20; 425)	5.999***	13.352***	8.784***	13.112***
	IV Estimates of Early Progresa Coverage:			
	Turnout	PRI	PAN	PRD
	(1)	(2)	(3)	(4)
Early Progresa	−0.015	0.015	−0.026	−0.022
	(0.030)	(0.023)	(0.017)	(0.018)
Constant	0.795***	0.454***	0.169***	0.199***
	(0.098)	(0.074)	(0.055)	(0.058)
Village Count Fixed Effects	Yes	Yes	Yes	Yes
Observations	445	445	445	445
R^2	0.213	0.385	0.284	0.376
Adjusted R^2	0.176	0.356	0.250	0.346
Residual Std. Error (df = 424)	0.160	0.121	0.089	0.095

*p<0.1; **p<0.05; ***p<0.01

3 Amelia II for Missing Data

As discussed in the previous section, there are a handful of outlier observations that bias the results. When these influential outliers are dropped from her models, the original findings in the paper are no longer significant. As we learned in class, the imputation of values for outlier observations or missing data is an alternative method for handling our missing data that is superior to simply dropping these observations. Since De La O constructed her small sample data set by overlaying only 506 experimental villages to the level of the precinct in Mexico, her data set is uniquely constructed. The lack of precinct identifiers in her data prevents us from pulling in new data from INE and INEGI, and we were also not able to procure additional data from the author herself.

To overcome the missing data problems, we use the R package Amelia II to impute values for the handful of observations with vote share greater than 100% as well as for a slew of other missing observations. The results from the revised OLS and IV models using Amelia II for missing observations are provided below (see Table 3). As the table shows, even with imputations for missing data, the treatment effect on turnout and PRI is not statistically significant. We do find, however, that after correcting for missing data using Amelia II, there is a 7.5 percentage point increase for PRI using the instrumental variable model that is statistically significant at the 10% level. This result is consistent with De La O's original findings.

4 New Models: Heterogeneous Effects

One of Da La O's (2012) main conclusions is that Progresa generated electoral gains for the incumbent party in the 2000 presidential election, while having no impact on support for opposition parties. In other words, Progresa mobilized more voters in favor of the incumbent to come to the polls, instead of swaying votes away from the opposition parties in support of the incumbent. Existing research examining the effects of CCTs on voter behavior has provided contradictory results in regard to this claim.

Various authors in the literature have argued against De La O's (2012) claim, contending that it is more a matter of vote-switching than mobilizing voters. For instance, such targeted cash transfer programs can effectively persuade voters to switch their vote choice to reward politicians for programmatic policies (Díaz-Cayeros, Esteves, and Magaloni 2009). Similarly, another camp of authors –namely, Cornelius (2004) and Schedler (2000)– have cited the threat of program discontinuation as a possible mechanism behind persuading voters to vote against their party of preference. This would categorize conditional cash transfer programs as being clientelistic in nature. Given these different mechanisms, it is worth reconsidering the question of whether Progresa is bringing more voters to the polls, as opposed to prompting a switch in party preference among supporters of the opposition. In this regard, our hypothesis is as

Table 3: Revised Models: Complete Data using Amelia II for Missing Observations

	ITT Estimates of Assignment to Early versus Late Treatment:			
	Turnout	PRI	PAN	PRD
	(1)	(2)	(3)	(4)
Treatment	0.042	0.028	0.009	0.001
	(0.033)	(0.017)	(0.013)	(0.012)
Constant	0.315888	0.261	0.006	0.082
	(0.182)	(0.100)	(0.078)	(0.074)
Village Count Fixed Effects	Yes	Yes	Yes	Yes
Observations	446	446	446	446
R^2	0.220	0.386	0.292	0.382
Adjusted R^2	0.183	0.357	0.259	0.352
Residual Std. Error (df = 425)	0.160	0.121	0.089	0.095
F Statistic (df = 20; 425)	5.999***	13.352***	8.784***	13.112***
	IV Estimates of Early Progresa Coverage:			
	Turnout	PRI	PAN	PRD
	(1)	(2)	(3)	(4)
Early Progresa	0.114	0.075*	0.023	0.023
	(0.062)	(0.033)	(0.026)	(0.026)
Constant	0.243	0.033*	-0.007	-0.007
	(0.190)	(0.105)	(0.085)	(0.085)
Village Count Fixed Effects	Yes	Yes	Yes	Yes
Observations	445	445	445	445
R^2	0.213	0.385	0.284	0.376
Adjusted R^2	0.176	0.356	0.250	0.346
Residual Std. Error (df = 424)	0.160	0.121	0.089	0.095

*p<0.1; **p<0.05; ***p<0.01

follows:

Hypothesis 1. *Progresa's positive electoral effects in favor of the incumbent is a result of vote-switching away from the opposition party as opposed to voter mobilization.*

De La O does not directly test her claim by examining change in the distribution of vote share across political parties post-Progresa. Furthermore, if her argument does hold, her analysis does not extend to whether particular groups belonging to a specific demographic are mobilized. We test this hypothesis using a heterogeneous effects instrumental variable model. We slice her data, using other socioeconomic covariates on the precincts in her sample. De La O only reports the average treatment effect. Since Progresa is a need-based program, we posit that we might find evidence that the effects of treatment (i.e., early enrollment in Progresa) on voting behavior vary across income groups. Furthermore, since her results are no longer significant once outlier observations are dropped, we hope the new heterogeneous models can capture statistically significant effects within sub-samples. Using a poverty index covariate available in her dataset, we establish the median of the index (4.75) as a threshold cutoff above which observations fall into the "high poverty" category.[3]

Excluding the outlier observations discussed in the data discussed in the previous section, we run both an OLS and De La O's instrumental variable regression (i.e., with early enrollment in Progresa as an instrument) on each of these two subsets of the data (see Table 4). As the results below show, within the high poverty group, there is, once again, no effect of treatment on turnout, disproving De La O's claim that voters were mobilized. Within this subgroup, we also do not observe any effect of treatment on the incumbent's vote share (i.e., the coefficient is lower in magnitude and loses significance).

We construct an "opposition vote share" variable that is an aggregation of PAN and PRD's shares. Contrary to De La O's conclusion, the effect of treatment does negatively affect opposition vote share. We find that within the high poverty group, early recipients of Progresa are less likely to vote for the opposition parties. More specifically, in the IV model, early assignment to treatment results in an approximately 8 percentage points reduction in votes cast by high poverty voters for opposition parties (PAN or PRD), and this result is statistically significant at the 5% level. It is clear that this result is mostly driven by Progresa's negative effect on PRD vote share. We observe this when we run both the OLS and IV regression on PAN and PRD's vote share separately. Early assignment to Progresa decreased PRD vote share by around 6.3 percentage points among high poverty recipients of assistance, and this result is significant at the 5% level. Although negative effects on opposition vote share are also observed in the low poverty group, the results are not statistically significant.

[3]These results are robust to having the cutoff at points 0.05 below, 0.05 above and 0.10 above the median. The results are presented at the median in an attempt to divide the sample into roughly equal sized 'poor' and 'non-poor' subsamples with a cut-off that is the least arbitrary.

Table 4: Revised OLS and IV Models with Heterogeneous Effects: High Poverty Precincts Only

	ITT Estimates of Assignment to Early versus Late Treatment:				
	Turnout	PRI	PAN	PRD	Opposition
	(1)	(2)	(3)	(4)	(5)
Treatment	0.001	0.026	−0.012	−0.030**	−0.042**
	(0.018)	(0.017)	(0.013)	(0.015)	(0.016)
Constant	0.878	−0.369	1.568***	−0.172	1.396**
	(0.750)	(0.717)	(0.539)	(0.610)	(0.683)
Village Count Fixed Effects	Yes	Yes	Yes	Yes	
Observations	211	211	211	211	211
Adjusted R^2	0.369	0.396	0.266	0.246	0.317
	IV Estimates of Early Progresa Coverage:				
	Turnout	PRI	PAN	PRD	Opposition
	(1)	(2)	(3)	(4)	(5)
Early Progresa	−0.043	0.010	−0.018	−0.063***	−0.081***
	(0.028)	(0.026)	(0.020)	(0.022)	(0.025)
Constant	0.773***	0.564***	0.060**	0.142***	0.202***
	(0.037)	(0.036)	(0.027)	(0.031)	(0.035)
Village Count Fixed Effects	Yes	Yes	Yes	Yes	
Observations	210	210	210	210	210
Adjusted R^2	0.355	0.390	0.242	0.227	0.287

*p<0.1; **p<0.05; ***p<0.01

This is an important result because Progresa is Mexico's main poverty alleviation program, and it currently consumes nearly half of the nation's anti-poverty budget.[4].By helping poor rural families –and has since been expanded to include urban– with healthcare, education and nutrition, Progresa aims to alleviate poverty at the national level. Our results, then, show that the program is particularly effective in generating electoral gains for the incumbent among those precincts that need Progresa the most. Additionally, contrary to De La O's conclusion, these electoral gains do not work via a mobilizing mechanism (i.e. increasing turnout), but rather by diverting votes away from opposition parties.

We also test for heterogeneous effects across precinct size determined by total number of registered voters. Using the median of 370 voters as a cutoff point, we find that, similarly, precincts with lower numbers of registered voters were less likely to vote for opposition parties in response to early assignment to Progresa (see Table 5). Specifically, in precincts where the number of registered voters is lower than the median, early enrollment in Progresa reduced the total vote share of opposition parties by about 8.1%. Looking at the effects for PAN and PRD separately, the results mirror those for the heterogeneous effects across the poverty divide: the treatment reduces PRD vote share by 4.9 percentage points in the 2000 presidential elections. This result is consistent with the one above, as high poverty districts tend to be less politically mobilized (i.e., have fewer registered voters).

5 Conclusion

In this paper, we re-visit the relationship between targeted assistance programs and voting behavior. We examine a prior claim that early enrollment in Progresa, the Mexican conditional cash transfer program, resulted in increased voter turnout in the 2000 presidential election, a higher vote share for the incumbent, and no changes in vote shares of the opposition parties. After conducting a series of sensitivity analyses on the residuals as well as balance and linearity checks on the data, we identify two outlier precincts with high population that violate the assumption of linearity in the population density covariate. We find that the original results are driven by these outlier precincts and another set of outlier observations (i.e., vote share greater than 100%), and the significance of the results no longer hold when they are excluded from the model.

To correct for potential bias associated with missing data from dropping outlier observations, we employ the R package Amelia II. After correcting for missing data using imputations of the data set courtesy of Amelia II, we find a 7.5 percentage point increase for PRI in the instrumental variable model that is statistically significant at the 10% level, a result consistent with the original findings. Furthermore, we incorporate heterogeneous effects in socioeconomic

[4] $http://web.worldbank.org/archive/website00819C/WEB/PDF/CASE_62.PDF$

Table 5: Revised OLS and IV Models with Heterogeneous Effects: Low Voter Registration Precincts Only

	ITT Estimates of Assignment to Early versus Late Treatment:				
	Turnout	PRI	PAN	PRD	Opposition
	(1)	(2)	(3)	(4)	(5)
treatment	0.002	0.033*	−0.016	−0.019	−0.035**
	(0.019)	(0.018)	(0.012)	(0.014)	(0.016)
Constant	0.681***	0.308***	0.211***	0.159*	0.369***
	(0.109)	(0.100)	(0.070)	(0.082)	(0.091)
Village Count Fixed Effects	Yes	Yes	Yes	Yes	
Observations	222	222	222	222	222
Adjusted R^2	0.373	0.439	0.457	0.424	0.444
	IV Estimates of Early Progresa Coverage:				
	Turnout	PRI	PAN	PRD	Opposition
	(1)	(2)	(3)	(4)	(5)
Treatment1	−0.021	0.039	−0.029	−0.049**	−0.081***
	(0.029)	(0.026)	(0.018)	(0.022)	(0.025)
Constant	0.671***	0.300***	0.224***	0.159*	0.202***
	(0.112)	(0.101)	(0.071)	(0.085)	(0.035)
Village Count Fixed Effects	Yes	Yes	Yes	Yes	
Observations	221	221	221	221	210
R^2	0.410	0.488	0.508	0.447	0.352

*p<0.1; **p<0.05; ***p<0.01

covariates, such as poverty levels and extent of voter registration–which we interpret as a proxy for extent of political mobilization of the precinct, in both the instrumental variable and OLS models. Contrary to the original findings, our results from the new models show that the pro-incumbent effects generated by the CCT are not explained by a mobilizing mechanism, but by a diversion of votes away from opposition parties.

This last finding can be explored further. In our analysis, we aggregate left and right wing opposition parties into a single category. However, when we disaggregate them, we find that the result is mainly driven by a diversion of votes away from PRD, a left-wing social democratic party. Further work may address why it is PRD, and not PAN, a right-wing, conservative party that is losing votes among poorer voters. This avenue of future research could shed light on whether De La O's (2012) characterization of Progresa as a programmatic as opposed to clientelistic program is accurate.

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