2017-38

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Redistributive Programs' Implementation: Do Political Incentives Matter ?

Arthur Bauer*

March 2017

Please cite this paper as: BAUER, A. (2017), "Redistributive Programs' Implementation: Do Political Incentives Matter ?", *AFD Research Paper Series*, n° 2017-38, March.

Contact at AFD: Rohen d'Aiglepierre (daiglepierrer@afd.fr)

* CREST, E-mail: arthur_bauer@hks15.harvard.edu

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ISSN 2492 - 2846

Redistributive Programs' Implementation: Do Political incentives Matter ?

Arthur Bauer, CREST

Abstract

Despite innovative safeguards to reduce political interference in the nowadays famous Mexican conditional cash transfer, local politicians received important electoral rewards from its implementation. I ask whether program's implementation responded to political incentives. My empirical test leverages a natural-experiment: the interaction between the rotating structure of local elections and the timing of Progresa-Oportunidades' implementation. I show, with administrative data from the Secretariat of Social Development (SEDESOL), how exogenous political incentives determined the enrollment rate of beneficiaries. After the implementation of blindaje electoral, I use a close-election regression discontinuity design to show how local incumbent's alignment with the PAN causally impacted enrollment rates' responsiveness to political incentives, despite reduced political interference in average. Then, separating the different phases of program implementation I am able to identify which actors were more responsive to local political incentives: namely local governments' employees more than program's officials. Finally, I show that enrolling additional beneficiaries because of political incentives didn't foster program's efficiency in reducing inequality and poverty rate.

Key words: Redistributive Programs, political incentives, Mexico

JEL Classification: H23, D72.

Acknowledgements: I am indebted to Professors Rohini Pande and Michael Walton for their guidance as well as to Florencia Leyson for numerous conversations and her help accessing the data. I would like to thank Soledad Artiz Prillaman, Filipe Campante, Simon Georges-Kot, Marie-Pascale Grimon and Michael Kremer for helpful discussions and comments. Financial support from the Annette Kade Fellowship as a student of the MPA/ID program is gratefully acknowledged

Original version: English

Accepted: March 2017

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Abstract

Despite innovative safeguards to reduce political interference in the nowadays famous Mexican conditional cash transfer, local politicians received important electoral rewards from its implementation. I ask whether program's implementation responded to political incentives. My empirical test leverages a natural-experiment: the interaction between the rotating structure of local elections and the timing of Progresa-Oportunidades' implementation. I show, with administrative data from the Secretariat of Social Development (SEDESOL), how exogenous political incentives determined the enrollment rate of beneficiaries. After the implementation of *blindaje electoral*, I use a close-election regression discontinuity design to show how local incumbent's alignment with the PAN causally impacted enrollment rates' responsiveness to political incentives, despite reduced political interference in average. Then, separating the different phases of program implementation I am able to identify which actors were more responsive to local political incentives: namely local governments' employees more than program's officials. Finally, I show that enrolling additional beneficiaries because of political incentives didn't foster program's efficiency in reducing inequality and poverty rate.

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"The results of Alianza Cívica's research indicate that vote conditioning is done primarily by members of municipal governments who leverage federal state-funded programs. This implies that in close elections in areas with high degrees of socioeconomic marginality, bought or conditioned votes could produce a margin of victory in favor of those parties already in control of local governments" (Global-exchange, 2006).

1 Introduction

Understanding what incentives shape the implementation of re-distributive programs by politicians and possibly bureaucrats is a question at the core of the political economy. In particular in low-income countries where state redistribution of resources may be an important means of poverty reduction and economic mobility, failures of the state to implement effective re-distributive systems raise fairness and equity questions.

The Mexican Conditional Cash Transfer, Progresa-Oportunidades is an interesting case in point as it explicitly sought to reduce opportunities for political interference. [1] Despite those efforts, several studies have highlighted the continued existence of electoral rewards from Opportunidades (Rodríguez-Chamussy, 2015; Magaloni, 2006; Magaloni, Diaz-Cayeros, and Estevez, under review), leaving unanswered the mechanisms that generated those returns.

This paper asks first whether politicians or bureaucrats responded to political incentives in the implementation of the program. Second it analyses which actors of the implementation process responded the most to political incentives. Third it provides suggestive evidence for the impact of such incentives on program's effectiveness in reducing poverty and inequality.

The first empirical test leverages that created exogeneous variation of local political incentives: namely, the interaction between the timing of expansion and the rotating structure of municipal elections in Mexico -which are held every year in a third of the municipalities. I analyze how these incentives affected the delivery of the nowadays famous Mexican conditional cash transfer, in the spirit of Cole (2009). I focus on the rate of enrollemnt of beneficiaries as proxy for delivery.

I combine data on the number of beneficiary households per locality under Vicente Fox's and Ernesto Zedillo presidency (1998-2004), obtained from the Secretaría de Desarrollo Social – Ministry of human development (SEDESOL) with electoral results obtained from the CIDAC database, completed with data from *Institutos Federal Electoral*. I study separately rural and semi-urban areas, excluding urban areas where implementation was limited to 2 years 2002 and 2004 and which constitute a smaller sample.

[1] A federal agency, named the National Coordination Agency, was responsible for its implementations and operations at the local level. It had technical and operational autonomy (Bank, 2014) leaving little opportunity to politicians in local governments to abuse it. Furthermore, Rogelio Gomez Hermosillo, National Coordinator of Oportunidades from August 15, 2001 to November 30, 2006, explicitly sought to identify mechanisms to reduce political interference with the program. These mechanisms were *blindaje electoral* and applied during election years either at the national or local level: postponing payment and enrollment of new beneficiaries when elections happened before September and preponing it to March-April in states with elections in November. Since in Mexico, state and municipal elections are held at the same time, this legislation was also supposed to reduce political interference at the municipal level. In particular, this reorganization of the enrollment process is likely to affect the enrollment rate of beneficiaries in municipalities holding local elections.

I show that local elections boosted the implementation of Progresa under President Ernesto Zedillo in rural areas, in particular when they were more disputed. After the implementation of *blindaje electoral* under President Vicente Fox, local elections still drove the implementation of the program in semi-urban areas however their impact was limited to the first year of its implementation. I interpret those latter results as evidence that political interference in program operations were limited after *blindaje electoral*, and the corresponding institutional safeguards were efficient.

I further show that political determinants were particularly salient in municipalities governed by the national incumbent, under Presidente Vicente Fox. I alleviate concern of endogeneity and omitted variable biais by focusing on municipalities where PAN had lost shortly and those where it gained power through a disputed election.

To make sure my empirical work does not only capture statistical noise, I further identify two mechanisms through which political interference may have occured, within the de-jure rules of operations and consistently with qualitative studies. First local government may have driven the speed of implementation on the year the program started operating in their constituency, getting the different actors react faster. Second, I note that program officials and *Promotoras* in particular responded to political incentives (de la Jara, 2006a), only before *blindaje electoral* started.

As the impact of elections on program disbursment is actually persistent after *blindaje electoral*'s implementation only on the first year the program starts operating in a municipality but disappears the following ones I conclude that employees of local government, particularly involved at the early stages of program implementation were central in political interference with the program. On the contrary, program officials whose role was central in the later stages seem to have responded less to political incentives after the implementation of *blindaje electoral*.

Finally I compare the evolution of poverty in municipality where incentives were stronger during implementation period. I show that poverty and inequalities followed a similar evolution in those municipalities suggesting political incentives reduced per beneficiary's program effectiveness.

My paper builds on two different strands of the literature in economics and political sciences. First, my work on the impact of local elections on re-distributive program's disbursement contributes new evidence to development economic litterature focusing on political economy of social programs distribution. The pieces most related to mine are (Cole, 2009) and (Magaloni, Diaz-Cayeros, and Estevez, under review). On one hand (Cole, 2009) describes the importance of political incentives in the distribution of agricultural credit in India, leaving aside any analysis of this distribution on voters' votes. On the other hand, Magaloni, Diaz-Cayeros, and Estevez (under review)'s work tries to explain political rewards from re-distributive political programs in Mexico but is not identified.

Second it helps understand the well documented political rewards from conditional cash transfers (Rodríguez-Chamussy, 2015) [2]. It suggests that there is some equilibrium effects of the well-documented political rewards from this form of re-distributive policies. Politicians anticipate the electoral rewards from conditional cash transfers, adapting their implementa-

[2]See also (Rodríguez-Chamussy, 2015; Manacorda, Miguel, and Vigorito, 2011; Magaloni, 2006; Zuccho, 2013)

tion to better suit their own political interest.

Third, they help understand the contrasted evidence on the impact of non-partisan programmatic re-distributive policies on support to the incumbents. On one side Imai, King, and Rivera (Working Paper) find that participating to non-partisan programmatic re-distributive policies implemented in an experimental setting have no impact on support to the incumbents. They highlight minors coding errors De La O (2013), and reach results in line with the theory: voters do not rewards political parties for programs which they have no control over. On the other side, leveraging the expansion criteria of the program, Rodríguez-Chamussy (2015) finds a positive impact on electoral rewards to local incumbents. We argue that Imai, King, and Rivera (Working Paper)'s results are especially valid in the context of the program's experimental implementation with widespread international attention for limited political interference, while Rodríguez-Chamussy (2015) results concern the effect of the program implemented in a non-experimental setting which was partially subject to political interference (as mentioned qualitatively by de la Jara (2006a); Escobar Latapí and de la Rocha (2005); Takahashi (2007) and as we show more formally here.)

Those results have two important political implications. First, aligning incentives of local politicians with the implementation of re-distributive programs, may help better implement those programs. To the extent that re-distributive programs and conditional cash transfers in particular increase equality of opportunity they might limit the existence of rents in the economy. This might have consequences both on social welfare as well as on aggregate efficiency. Second, the adequate replacement of *Promotora* after the implementation of *blindaje electoral* seems to have been particularly effective in reducing political interference in program's implementation. This might be of particular interest for policy makers given the lack of knowledge on successful interventions for political interference reduction (Pande and Olken, 2012).

2 Social programs and politics in Mexico

2.1 History of Patronage

Porfirio Diaz's dictatorship, already patronage-based, ended with the Mexican Revolution. In its aftermath, PRI took power and instituted a very pyramidal patronage system which lasted until the 1990s. When political competition increased and new parties came in power, patronage system survived as PRI's practices served as example to newly elected officials, shaping traditions and habits. PRI's patronage system was legitimized by the public sector labor law of 1938 which granted the right to unionize only to the base of the pyramid, while acquaintance was the main channel for job stability in higher levels. The system was highly centralized: the presidential office nominated candidates to state or major municipal elections and selected PRI's leaders. At the municipal and state levels, the same discretionary selection of administrative workers and political candidates was reproduced, which guaranteed the effective implementation of the policy agenda decided within the presidential office.

The absence of political competition shaped both politicians' and mid or high level bureaucrats' accountability. They were not accountable to the State nor to the civil society. Jobs' allocation within the dominant party motivated their performance. It was indeed com-

mon knowledge that "jobs and spoils" would be distributed to those who show higher effort and had "attracted attention of political bosses" (Grindle, 2012)[3]. Incentives were particularly stronger during election times, as they augured a new distribution of administrative jobs.

PRI's patronage system did not only guarantee the implementation of the policy agenda but also strengthened PRI's hegemony from the 1920s to the 1990s. According to Grindle (2012)[4] it allowed clientelism and abuse of "public sector investments, land, electoral opportunities, and public sector jobs". Similarly, Magaloni (2006) reports "a wide consensus among experts on Mexico about the key role played by patronage politics in maintaining the PRI regime (Ames (1970); Cornelius (1975, 2000); Corenlius, Craig, and Fox (1994); Dresser (1994); Fox (1994), among others)."

Despite decentralization and strengthening of the opposition, patronage practices remained in the 1990s. Decentralization brought more electoral competition in the local constituencies, giving an opportunity for civil society to hold politicians accountable for good governance and for politicians to introduce better practices. Hence, bureaucrats and politicians' incentives for their personal career within the administration or within their party-machine became aligned with good governance (Grindle, 2012) [5] However, their personal objectives did not change, characterizing the patronage system: climbing the vertical structure of the party-machine. In particular, jobs instability, which we saw was an important component of higher level of government's ability to shape lower levels' incentives, remained despite attempts to implement a professional career service. A case in point is the 2004 reform's failure due to "traditions of discretion in appointments".

2.2 Redistributive programs as a component of patronage politics in Mexico

Abuses also occurred through social program distribution, which constituted an important component of patronage politics during the last two decades of the 20th century (Magaloni, Diaz-Cayeros, and Estevez, under review). Several empirical studies have shown how the PRI used Pronasol money to buy votes. While Hiskey (2003) argued that the funds were allocated to maintain leaderships in municipalities where the PRI had the largest electoral basis; Molinar and Weldon (1994) and Magaloni (2006) argue the contrary: namely, the program had been used to "buy back opposition voters" in particular in municipalities where last election's results had been close. The three confirm that social programs' abuse in Mexico was common practice in the 1980s.

Social programs were part of the patronage system because they brought electoral rewards. A case in point is the Conditional Cash Transfer Progresa-Oportunidades. First, Magaloni, Diaz-Cayeros, and Estevez (under review) show how beneficiaries favored the PAN in the 2006 elections. Second, De La O (2013) [6] uses the randomized implementation of the program to show how longer exposure to the program increased vote shares to the incumbent.

[3] p.154

[4] p.154

[5] p.138

[6] Recent work by (Imai, King, and Rivera, Working Paper) nonetheless suggests that her conclusions might have been driven by minor coding errors.

Alberto Diaz-Cayeros, Frederico Estevez, and Beatriz Magaloni, (Welfare Benefits, Canvassing, and Campaign Handouts, 2009) stress the important and lasting support of Progresa's beneficiaries for the PRI in rural areas both in the 2000 and 2006 presidential elections. This ex-post analysis points out the electoral gains of the party in charge of the implementation of the social program: the PRI in rural areas and the PAN in urban areas. More recently, Rodríguez-Chamussy (2015) reveals electoral rewards from Progresa-Oportunidades for local incumbent in municipal elections. Assuming politicians internalized or at least expected rewards from re-distributive policies before rigorous evaluations took place and demonstrated it, they might have taken them into account while determining the timing of Progresa-Oportunidades' expansion. (Magaloni, Diaz-Cayeros, and Estevez, under review).

2.3 Political interference in Progresa-Oportunidades

Despite attempts to reduce political interference with Progresa at the local and state level, Rocha-Menocal (2001) and Takahashi (2007) revealed "electoral geographic targeting before the 2000 elections" and new Progresa-Oportunidades' director, Mr. Hermosillo, "recognized electoral abuses of the program" before its mandate. Municipal government and local officials may have played a role in this abuse, as "most complaints about politicization involved abuses by leaders of local program committees and municipal officials".(Fox, 2008)[7]

Their role in the implementation and administration of the conditional cash transfer was certainly not negligible. Municipal governments provided a central logistical support, through a "Municipal Liaison officer" (*enlace*): guaranteeing security to cash distribution, venues for cash transfers delivery, and communications[8]. *Enlaces* were municipal bureaucrats (de la Jara, 2009) and given the long tradition of patronage in Mexico, arguably responsive to political incentives and or pressures. *Enlaces* were also responsible for identifying geographical limits, "helping with registering and re-certifying" beneficiaries, "collecting information on attendance from schools and health clinics". They delegated power to *Promotoras*, which were supposed to be elected but were often selected by higher level of the administration (Fox, 2008) [9]. *Promotoras* were in charge of "ensuring beneficiary compliance with program requirement" and sometimes built clientelistic relations with the community (Escobar Latapí and de la Rocha, 2005)[10]. de la Jara (2005) de la Jara (2006b) de la Jara (2006a) (quoted) further reports how they "imposed unpaid work tasks, asked for money and used the program for political campaigning". In order to reduce political interference Vicente Fox mandated Rogelio Gomez Hermosillo former founder and director of Civic Alliance. Under his term, Oportunidades strengthened institutional mechanisms to limit political interference. He implemented *blindaje electoral* and created *vocales* to replace *Promotoras*. [11].

[7] p.263

[8] From personal communication with Rogelio Hermosillo

[9] p.274 and Table 9.5 p.276

[10] quoted by Jonatahn Fox

[11] Even though, the effectiveness of the latter has however been questioned by Fox (2008) (p.274)

2.4 Channels for political interference and their evolution with *blindaje electoral*

As suggested in the previous paragraph, we expect political determinants for program implementation to be channeled through two actors: program officials, when there is evidence for their political involvement, and local governments when they get involved in the implementation.

Those two actors can impact the efficiency of program's delivery at different moment of the implementation process. Their respective responsiveness to political determinants will determine at which phases of program's implementation local elections impact program's disbursement. In particular, local governments would channel local elections' impact on program's operations on the first year of operations while program officials would channel the impact of local elections on program's distribution when the program has already started operating.

Blindaje electoral impacted the room of maneuver of program officials and their ability to respond to political incentives. It changed the selection process of key program's officials. A small group of elected spokeswomen (*vocales*) replaced each *promotora*, who *de-facto* were designated by higher level of the administration. As a result, this reform affected one of the two channels selectively: it reduced the room of maneuver of local officials while local governments could still affect the effectiveness of program distribution on the first year of operations.

3 A rational for and a description of political incentives

In this section, I formalize how political incentives may have shaped Progresa-Oportunidades implementation. I therefore focus on rewards from Progresa-Oportunidades, which if anticipated, may have been of great appeal for politicians. The literature suggests two types of rewards. On one hand Magaloni, Diaz-Cayeros, and Estevez (under review) identifies political rewards at the national level during presidential elections. On the other hand (Rodríguez-Chamussy, 2015) reports political rewards for incumbent in local elections.

I first consider the general gratitude channel, suggested by Manacorda, Miguel, and Vigorito (2011) in the Uruguayan context. This explanation could apply to both types of rewards observed both at the municipal and presidential levels. This channel as long as politicians internalize the rewards creates a strong incentive for implementation especially around elections if voters can be framed on a short period.

3.1 Voters express gratitude towards politicians who implemented favorable policies

If electoral rewards were only due to pure gratitude in voters' rewards for politicians who implemented Progresa-Oportunidades, we should not observe any influence of the political cycle in program's implementation. Past studies have highlighted this mechanism. A case in point is Manacorda, Miguel, and Vigorito (2011) which interprets electoral rewards from

PANES in Uruguay as evidence for rational but poorly informed decisions. Interestingly, their empirical work cannot rule out an alternative interpretation: namely, behavioral explanation based on models of reciprocity. This second mechanism relies on the importance of reciprocity, fairness and gratitude in voters' political decision making. Frederico Finan (2012) provide first evidence of it. They show how politicians in Paraguay internalized this psychological mechanism and targeted reciprocal individuals.

3.2 Two kinds of incentives

Assuming voters reward politicians who implement policies favorable to them and that politicians know they will be rewarded if they implement re-distributive policies, political incentives such as incoming disputed elections, may shape politicians' willingness to implement re-distributive policies. Here we analyze how two types of incentives may have shaped the implementation of the program. First election years and political competition may have accelerated the registration process. Second, political support for the program at the national level may motivate local mayors aligned with the national incumbent to facilitate the implementation process.

Elections and political competition as incentives for the implementation

A clear incentive for politicians to try to please their constituents is the occurrence of elections and in particular disputed ones. For politicians who know that beneficiaries would reward them, the occurrence of elections acts as clear incentives for the implementation of the program.[12]

Alignment with the federal incumbent as incentives for the implementation

Another incentive[13] for local incumbents to implement a policy is its support at the federal level by their party.[14] Such behaviors could frame voters' willingness to reward both the local and national incumbent, and help explain why previous studies have identified such rewards. Indeed, studies finding an impact of Progresa-Oportunidades' participation on beneficiaries' votes in presidential elections focus on marginal beneficiaries: they compare their votes to those of other individuals comparable in terms of observable characteristics. If alignment with the federal incumbent fosters enrollment of beneficiaries such individuals will be more likely to live in municipalities aligned with the federal incumbent. As a result their willingness to reward the local incumbent for implementing policies that benefit them will translate into voting for the federal incumbent during presidential elections.

[12] In case they are not directly involved in it's implementation they may well use their whole network to reach the relevant actors. The pyramidal structure of those political networks might even facilitate the flow of orders to reach really local actors.

[13] Both incentives might actually interact.

[14] Dell (2015) provides evidence for such behavior in the context of war against drugs.

4 Data and empirical strategy

4.1 Data

Electoral data on municipal elections between 1992 and 2009 are obtained from the Electoral database CIDAC, which I complete with data downloaded from three Federal Electoral Institutes' websites: Jalisco, Quintana Roo and Queretaro. From this data-set I am able to compute electoral margin of elections that happened between 1998 and 2005 and to determine electoral calendar between 1998 and 2005.

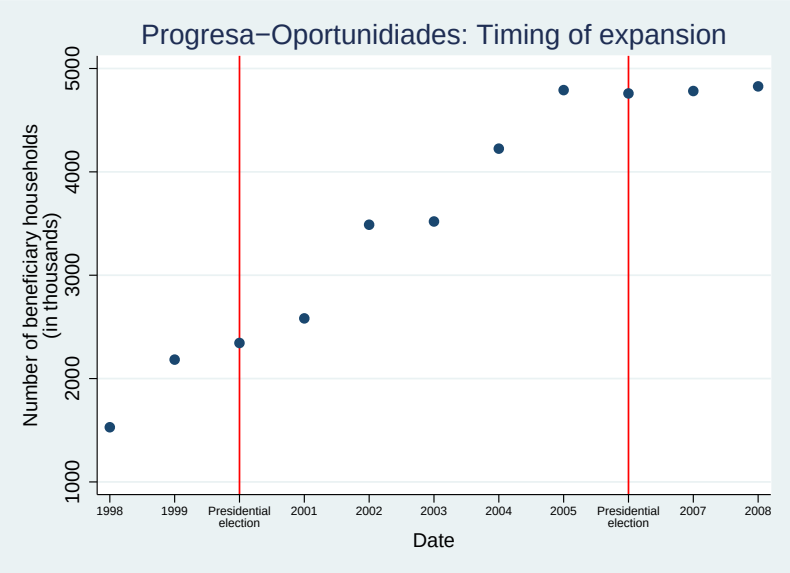
Upon request to SEDESOL I obtained data on families' enrollment. Those data constitute a panel spanning from 1998 to 2012, and contain locality level aggregates of the number of households participating to the program.

1995, 2000, 2005 Census provide socio-demographic information. I linearly extrapolate to dates between censuses, assuming that population grew at a constant rate between those dates.

Index of marginality at the municipal level for years 1995, 2000 and 2005 are collected from Conapo's website. I do not extrapolate these indexes, as to the extent of my knowledge SEDESOL did not update its targeting indicator between two census dates.

4.2 Descriptive statistics

Figure 1: Progresa-Oportunidades expansion 1998-2008



As can be seen in Graph 1, the expansion was mostly during the 1998-2004 period. I therefore focus the analysis to this period. The final sample is constituted of municipalities for which electoral and demographic data as well as data from Oportunidades were available. While aggregating census or SEDESOL data at the municipal level, I only consider localities with both demographic and Oportunidades data. (As, around 7% of the localities present in

SEDESOL database are absent from the census, taking into account the whole municipal population would underestimate the take-up, potentially selectively). My choice is tantamount to focusing on take-up among localities identifiable in the census data and where information on the number of Progresa-Oportunidades beneficiaries between 1998 and 2004 is available.

There also are 4 municipalities with an irregular cycle that overlaps with the pre-*blindaje* period and 211 (about 10% of the 2036 municipalities in the final sample) with an irregular cycle that overlaps with the post-*blindaje* period.

Since the electoral cycle is used to assess political incentives, I restrict the sample to municipalities with no irregularities in any electoral cycle between 1998 and 2000, for the analysis of the pre-*blindaje* period and to municipalities with no irregularities between 2001 and 2004 for the post-*blindaje* analysis. As a result all municipalities in the data-set held elections every 3 years, and I observe all of them during elections, one year before elections and two years before elections.

4.3 Specification

4.3.1 Litterature on political cycles in Mexico

This paper aims at understanding the role of political incentives in the implementation of Oportunidades.

Several studies on political cycles in Mexico have shown how incoming elections have affected monetary and fiscal policies. They identify movements in the exchange rate, the budget or the rate of inflation that follow the presidential electoral cycle. Macroeconomic variables, such as the exchange rate have an impact on people's budget constraint and therefore affect their living conditions. However in contrast to participation in Progresa-Oportunidades, changes in macro variables are transient and simultaneously affect the whole country. Besides, one might be concern that those studies cannot disentangle year-specific shocks and the effect of elections.

Progresa-Oportunidades' implementation has not followed the presidential cycle as can been seen in Graph 1. Under President Fox, *blindaje electoral* strategy prevented expansion the year before presidential elections. However, decentralization of implementation gave a role to municipal and local officials of the program, who were most of the time perceived as politically connected (Fox, 2008). Besides, patronage systems implemented by both incumbent parties – from 1997 to 2000 the PRI and the PAN from 2001 to 2006, made abuse particularly appealing to local officials. Hence, I focus on municipal elections.

The difficulty of identifying an impact of municipal elections on the implementation of Progresa arises from time varying policies regulating its distribution. As of 2001, *blindaje electoral* indeed froze expansions in municipalities holding municipal elections. This therefore limits the period over which the impact of municipal elections is most likely to be observed to 4 years (1997-2000). I therefore conduct two separate analysis before and after 2001.

In most countries municipal elections happen at the same time in the whole country. In such a case, comparing numbers of beneficiaries before and number of beneficiaries after elections would fail to identify the impact of elections over a small period of time. Over the 1997-2000 period of Progresa's implementation in rural areas, municipalities all over the country would hold elections on the same year. Progresa's re-certification process and

exclusion of participants who do not meet the requirements happen every 3 years, which also limit annual variations in the number of participants.

4.3.2 Leveraging the rotating structure of municipal elections: baseline regression

Fortunately for the identification, each year in Mexico one third of the municipalities holds elections. Hence for one third of them the 1997 large-scale implementation happened before municipal elections, for one third it happened during and for one third it happened the year after. This rotating structure of municipal elections created an exogenous variation in the political incentives for the implementation of the program.

I therefore run the following regression:

$$Y_{mt} = \beta X_{mt} + \gamma^0 Election_{mt} + \delta^1{}_m + \delta^2{}_t + \epsilon_{mt} \quad (1)$$

Where Y_{mt} is the logarithm of one plus the relative ratio of number of beneficiaries over years t and $t-1$ as this ratio is simply the growth of beneficiaries plus 1 in municipality m [15]. $X_{m,t}$ is a vector including the constant and a control for time varying characteristics of municipality m, here the population of municipality m in year t. [16]. $Election_{mt}$ is an indicator (*resp.* a vector of indicators) indicating whether year t was an election year (*resp.* preceding or following an election) in municipality m.

The rotating structure of elections allows us to analyze the dynamics as of 1998 [17]. For each year of the 1998-2000 period, I can compare municipalities with elections and municipalities without election, disentangling the political impact of elections from potential year-specific shocks. Since the number of beneficiaries at one point in time in a specific municipality is the result of a cumulative process of past enrollments, I must rely on longitudinal data-set to analyze this period.

Another concern I can alleviate with longitudinal data is the potential differences in municipalities where initial implementation happened during election years and municipalities where initial implementation happened after or before elections. The former group of municipalities might simply be poorer inducing a spurious correlation between the political incentives and the enrollment level. Following municipalities across time enables us to see how the rate of Progresa's distribution varies within a municipality when it holds local elections, while controlling for all municipalities invariant characteristics by including municipality-fixed effects.

4.3.3 Measuring enrollment rates

Given that the number of beneficiaries observed in a specific year is the result of a cumulative process-namely the inclusion of new beneficiairies in all past years, I adopt a first difference

[15] As we cannot divide by zero, I add one to the denominator, so that this ratio in the first year the program was implemented in the municipality simply collapses to the number of beneficiaries plus one.

[16] Since we do not have access to yearly population level we cannot control for population growth rate which would be constant and captured by the fixed effect with the extrapolation method we use

[17] In 1997 nationally representative data for Progresa participation aggregated at the municipal level is not available. I therefore cannot analyze separately the initial 1997 large scale implementation.

approach. In particular, I am interested in capturing the relative increase in the number of beneficiaries. What matters is therefore more the relative drop in the growth rate than the actual number of percentage points it drops by. The easiest way to deal with this non linearity is to focus on the log of the growth rate as the dependent variable.

To see why even with a constant rate of enrollment and an infinite number of eligible households[18] willing to participate, the growth rate of beneficiaries would not be linear, consider a smooth and regular enrollment process only constrained by local capacity. The first year after implementation, the number of beneficiaries should increase by 100%, the year after by 50% more, then by 33% and so on. If anything, the actual sequence of percentages I observe should decrease slightly faster than this theoretical one because of some saturation effects. After some years, it becomes more difficult to enroll new beneficiaries as the eligible households that remain simply may be those for which the cost of participating is too high.

4.3.4 Heterogeneous effects

The analysis of heterogeneous effects allows us to assess the impact of the different types of political determinants.

By level of competition: Main specification

First of all I estimate equation (1) on various sub-samples of the municipalities with different levels of political competition. I compare municipalities where past elections were close and those where margins of the past elections were large. I conduct the analysis focusing first on municipal elections and then on state elections.

Second I allow more flexibility and include the interaction term between the $Election_{mt}$ indicator or vector of indicators and the margin of elections in past elections. I estimate the following regression:

$$Y_{mt} = \boldsymbol{\beta X_{m,t}} + \gamma^0 Election_{m,t} + \gamma^1 Election_{m,t} \cdot Margin_{m\ or\ s,\ t} + \delta^1{}_m + \delta^2{}_t + \epsilon_{m,t} \qquad (2)$$

Where $\boldsymbol{X_{m,t}}$ is a vector including the constant and time-varying characteristics of municipality m, as the logarithm of population and the electoral margin ($Margin_{m\ or\ s,\ t}$) in past elections in municipality m or state s in year t. I estimate two models, one with margin in municipal elections ($Margin_{mt}$) and one with margin in state elections ($Margin_{s,t}$).

By level of competition: Robustness check

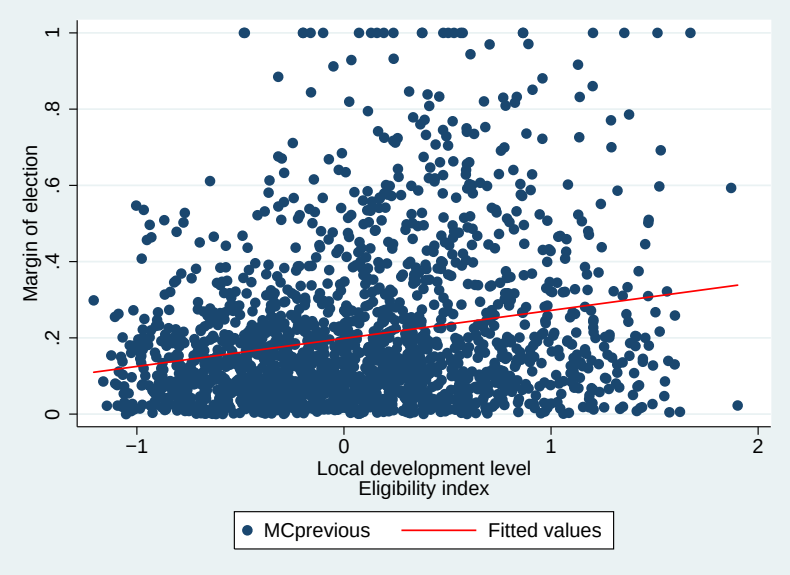
The analysis of the differential effects for different levels of competition is only informative if levels of electoral competition are not correlated with other important determinants of implementation's effectiveness that would also amplify the impact of local elections.

We know that, independently of differential impacts for different levels of competition, we should observe differential impacts by level of local development. The variation of the growth rate of beneficiaries in poorer municipalities being larger, we indeed need less power to estimate the impact of elections in those municipalities.

[18] We consider that given the eligibility requirement for the program, namely wealth and number of school-aged children the overall stock of eligible household is fixed, over the period we consider.

Fortunately, theory predicts that the poorest municipalities are also less disputed (Dixit and Londregan, 1996), suggesting that this correlation would only bias downwards the estimated impact of political determinants in the implementation process. I empirically confirm this positive correlation in the sample (Figure 2).

Figure 2: Positive relationship between poverty level and margin of election in 1998 (t-statistics=9.6)



This positive correlation suggests that the two effects will balance each other. As a result if I observe that the impact of local elections is amplified in poorer constituencies, the effect of economic development may dominate and I might not be able to observe the impact of political competition. On the contrary, if I observe that the impact of local elections is larger in more disputed constituencies, I might not be able to observe a stronger impact in poorer constituencies.

By party affiliation

In addition, one of the working hypothesis is that alignment with the federal incumbent may have facilitated political interference, making political incentives more salient. To empirical test this hypothesis, I first compare municipalities where the local incumbent is aligned with the federal incumbent with other municipalities. This is tantamount to include an interaction term in the baseline equation 1:

$$Y_{mt} = \boldsymbol{\beta X_{mt}} + Alignment_{mt} + \gamma^0 Election_{mt} + \gamma^1 Election_{mt} \cdot Alignment_{mt} + \delta^1{}_m + \delta^2{}_t + \epsilon_{mt} \quad (3)$$

Where $Alignment_{mt}$ indicates whether the mayor in municipality m in year t is affiliated with the same party as the President's. This specification allows a comparison on the strength of the political determinants in municipalities run by members of the presidential party and

others. [19]

However those municipalities also differ along other characteristics, potentially biasing my results. As a result I am interested in comparing municipalities where the candidate of the federal incumbent won with a very short margin and those where it lost with a very short margin. This close election regression discontinuity design has been widely adopted in the literature, a case in point in the Mexican context being Dell (2015).

One way of applying it is to estimate the following equation:

$$Y_{mt} = \boldsymbol{\beta X_{mt}} + \gamma^0 Election_{mt} + \gamma^1 Election_{mt} \cdot Close(\alpha)_{mt} + \gamma^2 Election_{mt} \cdot Incumbent_{mt} + \gamma^3 Close(\alpha)_{mt} \cdot Incumbent_{mt} + \gamma^4 Close(\alpha)_{mt} \cdot Incumbent_{mt} \cdot Election_{mt} + \delta^1{}_m + \delta^2{}_t + \epsilon_{mt} \quad (4)$$

Where $Incumbent_{mt}$ is a dummy indicating whether the mayor is aligned with the federal incumbent, i.e. belongs to the PRI before 2000 or belongs to the PAN after 2000. Given that Vicente Fox actually took power only as of December 1st 2000, I consider that PRI mayors in 2000 were still aligned. $Close(\alpha)_{mt}$ is a dummy indicating that the previous election was close in municipality m and that one of the two main candidates was affiliated with the same party as the federal incumbent. As a time-varying municipal characteristics, it is included in the vector X_{mt} in addition to the constant and the logarithm of population in municipality m and year t.

4.4 Identifying actors who respond to political incentives

The identification of local political determinants does not inform the policy debate on the efficiency of *blindaje electoral* and the lever decision-makers should activate to reduce such political interference in future programs. Hence I here turn to the analysis of the channels through which this political determinants operate. As mentioned in 2.3., political determinants of the expansion may have been channeled either by local governments or by local officials.

Comparing the impact of political determinants the year the program starts operating in a municipality and the following years, help disentangle between those two channels. To make sure I am identifying the correct mechanisms I compare before and after *blindaje electoral*. This policy implemented in 2001 left less room for political incentives to impact local officials' behavior, as a result-and assuming it met its objectives- I should not observe any impact of political determinants the years following implementations after *blindaje electoral*.

4.5 Discussion

One might be concerned that the enrollment rate would be dependent of the date at which the program started. Indeed, in a given year, the municipalities where the program has been implemented for a longer time, even if the enrollment process was very regular and

[19] To complement this analysis I have run a similar regressions where $Alignment_{mt}$ equals one when the mayor or his strongest opponent in the previous election was affiliated with the party of the President. which did not any more interesting result.

the amount of eligible household unlimited, would have a lower growth rate of beneficiaries. However the date at which the program starts being implemented in a given municipality is an invariant characteristic of this municipality and is therefore captured by the fixed effects.

One might also be concerned that the dependent variable is much larger the year of the program's implementation. This should not however bias the results since I treat all municipalities the same way and only investigate the variation of this variable, within a municipality, during election years. Even for the identification of the actors who respond more to political incentives, which compares the impact of incentives the years the program starts operating in a municipality and the years that follow, this construction of the variable should only affect the conclusion if there is a very specific kind of noise in the measure of the number of beneficiaries. Such noise should be small enough to allow us to detect an impact on the first year the program starts operating in a municipality but large enough so that when we build the growth rate it adds up and becomes so large that we cannot detect an impact the years following the start of a program in a municipality.

5 Results

5.1 Before and after *blindaje electoral*

I first estimate equation 1 both at early stage of Progresa's *expansion* in rural areas (1998-2000) and at early stages of Oportunidades' *implementation* (2001-2004) in semi-urban areas.

Table 1: Before and after *blindaje electoral*

	Before *blindaje* electoral	After *blindaje* electoral		Before *blindaje* electoral		
	(1) rural areas	(2) rural areas	(3) semi-urban areas	(4) rural areas	(5) rural areas	(6) rural areas
Log. pop.	1.015***	−0.262**	−3.901	2.020*	2.020*	2.020*
	(0.158)	(0.120)	(19.153)	(1.188)	(1.188)	(1.188)
Election year	0.596***	−0.048	5.939	0.375***		
	(0.188)	(0.077)	(4.181)	(0.121)		
One year before election					−0.750***	
					(0.241)	
One year after election						−0.750***
						(0.241)
Constante	−8.698***	3.648***	148.236	−19.084*	−18.709*	−18.709*
	(1.683)	(1.236)	(175.270)	(10.853)	(10.939)	(10.939)
R-squared	0.560	0.291	0.271	0.518	0.518	0.518
Observations	3899	6903	4547	1966	1966	1966

Note: Dependent variable is the logarithm of the ratio over two consecutive years of the number of beneficiaries per municipality. There is one observation per municipalities for each of the 1999 and 2000 years in columns (1) and (4) to (6) and for each of the 2002,2003 and 2004 years in columns (2) and(3). Columns (4) to (6) exclude municipalities that did not hold elections neither in 1999 nor in 2000. Source: Administrative data obtained from SEDESOL dataset merged with "Base de datos electoral CIDAC", and census 1995, 2000 and 2005. Municipalities and year fixed-effects. Baselines are year 1999 for columns (1) and (4) to (6) and year 2002 for columns (2) and (3). Standard errors clustered at the state and year levels in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

Table 1 shows how the growth rate of enrollment was systematically larger on elections years in rural areas before *blindaje electoral*. On the contrary in both rural and semi-urban

areas, after *blindaje electoral* was implemented, election years were not systematically associated with higher enrollment rate.

As a robustness check, I exclude municipalities that did not hold any elections neither in 1999 nor in 2000 (which are the two year for which I have observations on the growth rate of beneficiaires, since I have no data on enrollment level before 1998). Results, reported in columns (4) to (6) confirm the positive impact of elections on enrollment years. The point estimate on this sub-sample also remain non-statistically different from the estimate on the whole population reported in column (1).

5.2 Heterogeneous impact of the elections

By level of political competition

Panel estimates reported in the previous sections may however be driven by year-group-of-municipalities specific shocks. To alleviate this possibility, I investigate heterogeneous impact of elections. In particular, if results in previous section identify a political effect, they should be amplified in municipalities where these incentives are larger, namely those where elections are more disputed.

As mentioned in the identification strategy, I also expect this impact to be magnified in municipalities with more eligible households. However the effects are opposite given the positive correlation between levels of development and level of political competition reported in Graph 2.

Table 2 reports results from estimating equation 1 on different sub-samples of municipalities that vary along their levels of electoral competition.

Table 2: Differential effects for different levels of political competition

	Margin state elections			Margin municipal elections					
	(1) <5%	(2) >20%	(3) >25%	(4) <10%	(5) <5%	(6) <4%	(7) <6%	(8) >20%	(9) >15%
Log. rural pop.	4.354***	0.864***	0.894***	0.792***	3.315***	3.332***	3.322***	3.611***	2.475*
	(0.174)	(0.047)	(0.056)	(0.052)	(0.430)	(0.441)	(0.418)	(1.356)	(1.265)
Election year	0.352***	0.604***	−0.199	0.624***	0.564***	0.480***	0.567***	−0.085	0.122
	(0.054)	(0.192)	(0.296)	(0.184)	(0.155)	(0.161)	(0.169)	(0.128)	(0.155)
R-squared	0.587	0.521	0.597	0.593	0.639	0.619	0.632	0.628	0.619
Observations	496	1329	700	1504	790	653	928	1459	1893

Note: Please see Table 1 "Margin state elections" refers to the state-wide margin in past elections of state governors. "Margin municipal elections" refers to the municipal-wide margin in past municipal elections.

* p <0.1, ** p <0.05, *** p <0.01.

I can see that the level of political competition in state elections aggregated at the state level does not seem to be a determinant of implementation's effectiveness. I do not see any different impact of local elections in both more disputed elections (with margin in past elections lower than 5%) and in less disputed elections (with margin in past elections larger than 20%).

On the contrary the differential impact of local elections on growth rate is more important when I consider margins in municipal elections at the municipal level. More specifically, local elections have significantly more impact in more disputed elections than in less disputed

elections. Columns (4) to (9) shows that this result does not really depend on the threshold I consider to define disputed elections.

To confirm the latter and to allow for more flexibility in the heterogeneous effect of local elections by level of electoral elections I estimate equation 2. Results reported in Table 3 confirm the previous ones. In particular more disputed elections are relevant and magnify the impact of local elections on the speed of the implementation, only when I consider local electoral results (at the municipality level).

Table 3: Differential effects by levels of political competition: more flexible specification

	(1) Municipal elections	(2) State elections
Election year	0.877***	0.745***
	(0.179)	(0.229)
Margin in previous elections	0.230	10.795***
	(0.259)	(1.728)
Election year * Margin in previous elections	−0.980***	−0.050
	(0.363)	(1.778)
R-squared	0.562	0.572
Observations	3614	3866

Note: Please see Table 3

* p <0.1, ** p <0.05, *** p <0.01.

By level of local development

As mentioned in section 4.3.5, the heterogeneous impact by level of economic development of political implementation on program disbursement may reflect two dimensions.

First, the poorest municipalities are also those with the largest concentration of beneficiaries. As a result we might expect the administrative capability in those municipalities to constrain enrollment of additional beneficiaries driven by political incentives. On the other side of the development spectrum, the richest municipalities have fewer eligible households. The stock of eligible households is rapidly enrolled, and elections can only marginally impact enrollment of additional households. As a result, the heterogeneous impact by economic development to the extent economic development captures the concentration of eligible households is expected to be non linear.

Second, economic development at the local level is negatively correlated with political competition (Graph 1) and political competition is an additional incentive for the implementation of the re-distributive program (Tables 3 and 2), suggesting that economic development might also capture political competition. To alleviate this latter concern, I restrict the analysis to two sub-samples of the population: the richest and the poorest municipalities. I analyze the impact of economic development for those two sub-samples of municipalities within which the level of political competition is more homogenous.

Table 4: Differential impact by level of local development

		Margin municipal elections	
	(1) All municipalities	(2) Richest municipalities	(3) Poorest municipalities
Log. rural pop.	1.016***	0.895***	3.759***
	(0.157)	(0.149)	(0.286)
Election year	0.596***	1.082***	0.722***
	(0.189)	(0.238)	(0.226)
Election year * Backwardness index	−0.013	0.466***	−0.391*
	(0.227)	(0.170)	(0.207)
Constante	−8.704***	−7.626***	−36.144***
	(1.666)	(1.581)	(2.701)
R-squared	0.528	0.543	0.532
Observations	3877	2329	2342

Note: For dependent variable and source please see Table 1. Eligibility is assessed with the index of marginality at the municipality level. Municipalities and year fixed effects, standard errors clustered at the state and year level in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

Consistently with the expected non-linear heterogeneous effects by level of electoral competition, I cannot identify any linear heterogeneous impact by level of local development. In Table 4 the coefficient on the interaction term between the dummy for election year and local economic development in column (1) is not significant. On the contrary the same coefficient is statistically significant in the two other columns (2) and (3). It has an opposite sign in those two columns, confirming that capacity constraints may refrain very marginal municipalities from enrolling additional beneficiaries, and that on the contrary in richer municipalities there is not enough eligible households for political incentives to impact their enrollment.

By party affiliation

In this paragraph I report differential impact by alignment of the mayors with the national incumbent. I first simply compare political incentives as proxied by local elections in municipalities in which the municipal incumbent was aligned with the national incumbent with municipalities in which it was not. Table 5 presents the results distinguishing the pre-*blindaje* and the post-*blindaje* period. I also use this specification as a robustness check for the main result on the impact of political incentives before *blindaje electoral*.

Table 5: Differential impact by party affiliation

	Before *blindaje*				After *blindaje*
	(1) Rural areas	(2) <10%	(3) <5%	(4) >15%	(5) Semi-urban areas
Log. rural pop.	1.008***	0.793***	3.300***	2.472*	
	(0.155)	(0.052)	(0.440)	(1.274)	
Election year	0.777***	0.680***	0.488**	0.102	0.094
	(0.237)	(0.203)	(0.201)	(0.321)	(0.123)
Alignment	0.333**	−0.062	0.055	0.171	−0.019
	(0.135)	(0.211)	(0.283)	(0.233)	(0.077)
Election year * alignment	−0.268**	−0.096	0.133	0.056	−0.062
	(0.124)	(0.276)	(0.376)	(0.268)	(0.190)
Log. semi-urban pop.					−0.226
					(0.470)
Constante	−8.628***	−6.352***	−26.916***	−24.174**	3.208
	(1.652)	(0.512)	(3.493)	(12.060)	(5.239)
R-squared	0.561	0.593	0.639	0.620	0.334
Observations	3890	1504	790	1884	3286

Note: For dependent variable and source please see Table 1. Alignment is defined as a dummy variable indicating whether the mayor is aligned with the federal incumbent, i.e. belongs to the PRI before 2000 or belongs to the PAN after 2000. Municipalities and year fixed effects, standard errors clustered at the state and year level in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

Table 5 suggests that the alignment channel does not increase the impact of political incentives before *blindaje electoral* was implemented. On the contrary the impact of local elections on the rate of enrollment of beneficiaries seem significantly smaller in municipalities where the municipal incumbent was aligned with the national incumbent.

However, as mentioned in section 4.3.4, which presents the identification strategy, municipalities governed by the national incumbent and those that were not may differ along other characteristics that may well affect the impact of political incentives.

To alleviate this concern I restrict the comparison to municipalities where the national incumbent's party gained power after a disputed election, to municipalities where his party lost narrowly. As often in the applied economic literature, I consider alignment of the mayor with the national incumbent in those municipalities as randomly determined. Table 6 presents the results varying the threshold used to classify an election as disputed.

Table 6: Differential impact by party affiliation: identified

	(1) PRI	(2) PAN
Election year * alignment * margin	1.564	−19.264***
	(2.278)	(5.521)
Election year * alignment * margin squared	9.772	54.066***
	(7.188)	(17.342)
R-squared	0.564	0.332
Observations	3528	2990

Note: Please see Table 5. Close is a dummy variable equal to 1 when past elections were disputed. I define an election as disputed when the electoral margin falls below a threshold, indicated in the column labels.

* p <0.1, ** p <0.05, *** p <0.01.

Overall the results add to our understanding of the impact of political determinants on the efficiency of Progresa-Oportunidades' implementation. First they confirm that before

blindaje electoral alignment of the mayor and the national incumbent, namely the PRI, did not drive the impact of political determinant in the program's expansion. This is in line with recent studies (Imai, King, and Rivera, Working Paper) which finds no impact of participation to Progresa in 2000 presidential elections.

Second, in the post-*blindaje* period, the triple-interaction term between the three dummies election year, alignment with the president's party and disputed election, is positive and significant. This means, that among municipalities where elections were disputed, those where the PAN won narrowly, political incentives were more important for Oportunidades' implementation than those where it lost.

The latter suggests that alignment of the mayor with the national incumbent did matter for Oportunidadess' implementation during the post-*blindaje* period. This also suggests that the *blindaje electoral* strategy left some room for political interference in program's operation, without any judgment call on the need to further reduce this kind of political interference. I now turn to the analysis of the actors who matter for this political interference.

6 Disentangling the actors

6.1 Identification strategy

While the previous analysis focused on the aggregated impact of elections on program operations, it is informative to dissociate the effect of elections on program operations, after the first year it starts operating in a particular municipality and the effect of elections on actual take-up on the first year it starts operating. This distinction is important as it allows us to disentangle the potential channels through which political incentives have operated. In particular, if I observe that the impact of political incentives persist after the first year the program operates, I can rule out mechanisms that do not involve programs officials.

On the contrary if political incentives faded away after the first year the program starts operating, I could rule out an impact of political incentives on program operations - either through political pressures on or through political motivation of program officials.

In that perspective I start by running the following regression:

$$Y_{mt} = \boldsymbol{\beta X_{mt}} + \gamma^0 Election_{mt} + \gamma^1 Election_{mt} \cdot ImplementationYear_{mt} + \delta^1{}_m + \delta^2{}_t + \epsilon_{mt} \quad (5)$$

Where $\boldsymbol{X_{m,t}}$ is still a vector including the constant and the logarithm of population in municipality m in year t but now also includes an indicator that indicates whether year t is the first year that the program operates in municipality m.

As there is no data at the dis-aggregated level on Progresa's enrollment in 1997, I cannot dissociate municipalities that started the program in 1997 and those that started it in 1998. As a result, the pre-*blindaje* analysis is restricted to municipalities that implemented in 1999. For the post-*blindaje* analysis the analysis includes all municipalities with semi-urban localities that started to implement the program after 2001 (the official date of expansion to those localities).

The post-*blindaje* analysis is pursued by separating municipalities which implemented the program in 2001 and those that implemented it in 2002, to confirm the existence of a

political effect on election year. On these samples I still take the municipal electoral cycle as exogenous and can additionally compare the heterogeneous effect of concomitant municipal elections by margin of previous elections.

In that perspective I estimate the following equation:

$$Y_m = \beta Margin_m + \gamma^0 Election_m + \gamma^1 Election_m \cdot Margin_m + \epsilon_m \quad (6)$$

where Y_m is the logarithm of the percentage of beneficiary in municipality m. Here take-up levels identify with take-up rates since there was no beneficiary the year preceding the start of program implementation. I therefore decide to consider the concentration of beneficiary in municipality m as the variable of interest. It also provides an important robustness check that consists in running equation 6 with the index of backwardness as dependent variable. Corresponding results are presented in the two last columns of table **??**. If I am actually identifying political determinants of the program's take-up that are not justified by variation in concentration of eligible households, those determinants should not predict the index of backwardness. I actually assume that the index of backwardness is a good proxy for eligible households' concentration.

Another concern with this identification was mentioned in section 4.3.5. As Graph 2 demonstrated, economic development at the local level is correlated with municipal elections when I consider the whole population. Table 7 columns (1) and (2) confirms that this is still true when I restrict the sample to semi-urban municipalities which implemented the program in 2001 but not when I restrict the sample to semi-urban municipalities which implemented the program in 2002. To pin-point political effects, I therefore further restrict the sample to a group of municipalities where political competition is not correlated to economic development, and where, at the same time, the effect of political competition can be observed, namely in municipalities of the three richest quintiles. In the other municipalities, there are indeed too few observations with close elections.

Table 7: Relationship between local level of development and electoral margin

	All municipalities		Richest municipalities	
	2001	2002	2001	2002
Margin in previous municipal elections	0.749***	0.679	0.145	0.771
	(0.290)	(0.630)	(0.211)	(0.743)
Constante	−0.564***	−0.882***	−0.754***	−1.184***
	(0.106)	(0.189)	(0.074)	(0.109)
R-squared	0.018	0.007	0.001	0.021
Observations	389	110	302	91

Note: Dependent variable is the local level of development, assessed at the municipality level using the backwardness index. Source: "Base de datos electoral CIDAC". Standard errors clustered at the state and year levels in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

As can be seen in column (3) the relationship between local economic development and political competition disappears after I restrict the sample to the richest semi-urban municipalities. In the analysis I therefore report robustness checks on this sub-sample of municipalities.

6.2 Results

This section presents the results that allow to disentangle when in the implementation process political determinants are more important and consequently which actors, among those able to affect program's operations, respond to political incentives.

6.2.1 Before *blindaje electoral*

I first start by comparing the actual impact of political determinants on Progresa's implementation on the year the program's operations start in a municipality and on the following years. Column (1) of Table 8 below presents the result on municipalities with rural localities where the program started operating in 1999.

Table 8: Differential impact by level of local development

	(1)	(2) Close margin <5%	(3) Close margin <6%	(4) Close margin <7%	(5) Close margin <10%	(6) Large margin >20%	(7) More flexible Margin of elections
Log. rural pop.	5.598***	5.445***	5.626***	5.754***	5.520***	5.852***	6.278***
	(1.950)	(1.941)	(1.927)	(1.962)	(1.984)	(1.966)	(2.317)
Election year	1.068***	1.236***	1.223***	1.159***	1.116***	0.425	−0.070
	(0.302)	(0.257)	(0.247)	(0.247)	(0.271)	(0.511)	(0.637)
Implementation year	5.208***	5.153***	5.147***	5.156***	5.177***	5.180***	5.241***
	(0.237)	(0.242)	(0.232)	(0.230)	(0.244)	(0.243)	(0.256)
Implementation year * Election year	−0.672	−0.941	−0.930	−0.776	−0.749	0.226	0.796
	(0.669)	(0.664)	(0.658)	(0.664)	(0.679)	(0.744)	(0.897)
Margin		1.451***	1.134***	0.581	0.745*	−1.374***	−3.741**
		(0.406)	(0.387)	(0.620)	(0.449)	(0.427)	(1.523)
Implementation year * margin		0.247**	0.226*	0.172	0.072	0.096	−0.156
		(0.117)	(0.120)	(0.150)	(0.156)	(0.130)	(0.290)
Election year * margin		−2.214***	−1.857***	−0.821	−0.311	0.979**	4.738**
		(0.512)	(0.596)	(0.859)	(0.652)	(0.464)	(2.011)
Election year * Implementation year * margin		2.466***	2.126***	0.826	0.370	−1.930***	−6.803***
		(0.562)	(0.644)	(0.910)	(0.716)	(0.643)	(2.120)
constante	−59.432***	−57.772***	−60.945***	−61.730***	−59.368***	−62.111***	−66.460***
	(20.757)	(20.670)	(20.462)	(20.896)	(21.125)	(20.924)	(24.661)
R-squared	0.920	0.922	0.922	0.920	0.920	0.922	0.924
Observations	779	779	779	779	779	779	683

Note: For dependent variable and source please see Table 1. Sample is restricted to rural municipalities that started operating Progresa in 1999. Municipalities and year fixed effects, standard errors clustered at the state and year level in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

It confirms the impact of political determinants on Progresa's implementation before *blindaje electoral*. However I cannot identify any differential effect on the year program starts operating. This suggests that the importance of both actors-local governments and program's officials - is comparable.

It is however difficult from this simple specification to say whether program's officials simply did not influence at all the beginning of operations and program's official already had an impact on the first year or on the contrary whether the impact of local government on the first year to facilitate the launch of program's operations was comparable to the impact of program's official on the following years.

Columns (2) to (5) in Table 8 help addressing this concern. They allow us to compare more precisely the impact of political incentives on the implementation year and the following

one. In particular, if the political incentives had the same impact through out the different steps of program's implementation, I should not observe any differential effect on the first year of Progresa's implementation and the following ones.

Analyzing separately municipalities with different levels of competition does reveal that differential effects of political incentives on the first year of Progresa's implementation and the following ones exist. Besides, the different channels, involving different actors, seem to express themselves in different kind of municipalities.

On one hand, column (6) reveals that the impact of elections in municipalities where the incumbent won margin in previous elections were very large (larger than 20%) on the year after the program started operating (the sum of the coefficients on variable "Election year" and "Election year * large margin") is positive and significant. It is well known that the poorest municipalities where patronage systems are better established are also those where elections are less disputed. It is therefore not completely surprising that I observe an impact of elections on beneficiaries' enrollment even after the first year the program starts operating in those municipalities, despite large effort to limit political interference in program operations.

On the other hand, the analysis of political incentives in municipalities with high level of political competition (disputed election) suffer from a loss of statistical power. As mentioned in the identification strategy the impact of political competition and the impact of local development oppose each other. Splitting further the sample by distinguishing for each municipality the year they started implementing Progresa and the following year limit the statistical power of the tests, making it therefore more difficult to estimate the impact of political determinants in disputed municipalities. In column (2) to (5) we see that political interference was larger on the implementation year than in the following one. This makes the channel involving responsiveness of local government to political incentives more relevant in those municipalities.

6.2.2 After *blindaje electoral*

Table 9 reveals that on implementation years, local elections actually are associated with a faster enrollment rate of eligible households, even during the post-*blindaje* period (2001-2004). Column (1) presents a fixed effect estimation. The dependent variable is the logarithm of the rate of enrollment of beneficiaries at the municipality level. We include as independent variable a dummy for the first year of program operation and another one for election year as well as the interaction term between those two dummies. We see that the coefficient on the interaction term is both positive and significant, suggesting that even after *blindaje electoral*, elections had an impact on program's disbursement on the first year it started operating. Columns (2) and (3) present simple OLS estimations for municipalities that started operating the program in 2001. Columns (4) and (5) present similar estimations for municipalities that started operating the program in 2002. Those OLS estimations confirm the panel estimate. Within municipalities that started the program in a given year after *blindaje electoral*'s implementation those that were holding elections enrolled beneficiaries at a higher rate.

Table 9: Specific impact during years of implementation

		Implementation in 2001		Implementation in 2002	
	(1) 2001-2004	(2) 2001	(3) 2001	(4) 2002	(5) 2002
Log. semi-urban pop.	−0.188**				
	(0.078)				
Election Year	−0.008	0.591***	0.590***	0.365	0.470***
	(0.020)	(0.137)	(0.177)	(0.240)	(0.169)
Implementation Year	4.638***				
	(0.176)				
Implementation Year * Election year	0.613***				
	(0.176)				
Backwardness index		1.001***	1.001***	0.782***	0.756***
		(0.083)	(0.083)	(0.165)	(0.201)
One year after election			−0.001		0.200
			(0.200)		(0.396)
Constante	2.256***	−3.607***	−3.607***	−3.290***	−3.421***
	(0.675)	(0.111)	(0.158)	(0.200)	(0.218)
R-squared	0.930	0.469	0.469	0.272	0.277
Observations	3376	462	462	116	116

Note: Dependent variable in column (1) is the logarithm of the ratio over two consecutive years of the number of beneficiaries per municipality, in columns (2) to (5) it is the logarithm of the percentage of beneficiary in municipality m. In column (1) the sample contains all semi-urban municipalities which started the program after 2001. In columns (2) and (3) it contains all semi-urban municipalities which started the program in 2001, and in columns (4) and (5) it contains all semi-urban municipalities which started the program in 2002. Source: "Base de datos electoral CIDAC", administrative data obtained from SEDESOL, census 2000 and 2005. Standard errors clustered at the state and year levels in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

Table 10 confirms that this actually identifies political determinants of the rate of enrollment. The impact of elections is indeed higher in more disputed constituencies. Columns (3) and (4) confirm that results still hold when I restrict the sample to municipalities where there is no correlation between the levels of political competition and of economic development. The placebo test reported in columns (5) and (6) shows that there is no spurious correlation between the determinants I identify and the determinants of the concentration of eligible households (as assessed by the index of backwardness)

Table 10: Heterogenous effect and robustness checks

	All municipalities		Richest municipalities		Placebo tests on richest municipalities	
	2001	2002	2001	2002	2001	2002
Backwardness index	1.004***	0.782***	1.315***	0.635***		
	(0.104)	(0.201)	(0.129)	(0.240)		
Election Year	0.696***	1.034***	0.714**	1.293***	0.301**	0.030
	(0.229)	(0.273)	(0.278)	(0.264)	(0.120)	(0.266)
One year after election	0.293	0.877**	0.498	0.824**	0.160	−0.073
	(0.286)	(0.381)	(0.312)	(0.406)	(0.148)	(0.236)
Margin in previous municipal elections	0.234	4.076***	0.443	5.610***	0.007	−0.032
	(0.423)	(0.905)	(0.454)	(1.018)	(0.300)	(0.746)
Election year * Margin	−0.855	−5.335***	−1.301*	−7.516***	0.575	0.190
	(0.597)	(1.501)	(0.703)	(1.450)	(0.476)	(1.285)
One year after election * Margin	−1.390**	−6.178***	−2.057***	−7.280***	0.114	1.347
	(0.620)	(1.226)	(0.756)	(1.613)	(0.429)	(1.366)
_cons	−3.636***	−3.802***	−3.423***	−4.076***	−0.989***	−1.143***
	(0.212)	(0.285)	(0.277)	(0.326)	(0.068)	(0.083)
R-squared	0.470	0.336	0.426	0.172	0.096	0.039
Observations	389	110	302	91	302	91

Note: Dependent variable in columns (1) to (4) is the logarithm of the percentage of beneficiary per municipality. Dependent variable in columns (5) and (6) is the backwardness index. In columns (1) (resp. (2)) the sample contains all semi-urban municipalities which started the program in 2001 (resp. 2002). In columns (3) (5) and (4) (6) it is further restrained to semi-urban municipalities in the three richest quintiles. In column and in columns (4) and (5) it contains all semi-urban municipalities which started the program in 2002. Source: "Base de datos electoral CIDAC", administrative data obtained from SEDESOL, census 2000 and 2005. Standard errors clustered at the state and year levels in parenthesis.

* p <0.1, ** p <0.05, *** p <0.01.

An interesting results that emerges from Table 10 is the persistence of elections' impact the following year. This result is particularly consistent with the channels that considers municipal elections as a channel of information.

7 Consequences for program's effectiveness

In this section we ask whether politically induced implementation affects program's effectiveness. We focus on the redistributive component of Progresa-Oportunidades and it's expected impact on poverty and inequalities. We compare how the evolution of the latter differ in municipalities where initial implementation was an election year vs. municipalities where initial implementation wasn't an election year.

Given available data on poverty representative at the municipality or state level, we focus on implementation that happened in 1999 and on three poverty indexes computed by CONEVAL: Alimentaria Capacidades and Patrimonio. We compare the relative evolution of these three indexes between 1990 and 2000 in municipalities.

Table 11: Specific impact during years of implementation

Relative change in poverty rate or inequality level based on	
Alimentaria	0.106
Capacides	0.086
Patrimonio	0.053
Gini	0.023
Observations	420

* p <0.1, ** p <0.05, *** p <0.01.

Results are reported in Table 11 We see that the additional enrollment of beneficiaries due to elections is not associated with significantly higher poverty reduction or drop in inequalities. This gives suggestive evidence for less efficient targeting during election years, leading to a lower per-beneficiary's program effectiveness. However this result should be taken with a pinch of salt. First the power of this statistical test is low given the small sample size. Second additional test should be carried out to make sure municipality only differ by the timing of the election. Third to the best of my knowledge no impact evaluation has been carried out with the same outcome we consider here.

To attenuate those caveat one should first consider that the sample size is constrained here by the choice of focusing on municipalities that started implementing the program in 1999 which is itself constrained by availability of administrative data for progresa only as of 1998. Second the timing of election is here exogeneously determined by the rotating structure of municipal elections in Mexico. Third an extensive litterature has highlighted the efficiency of Progresa-Oportunidades to alleviate poverty, and improve well-being along many components as health, nutrition, education... This suggests an impact evaluation of the program would lead to a decrease of the gini and as well as a likely decrease of poverty based on the three indices used here.

8 conclusion

This paper adopts a fresh look on political incentives for Progresa-Oportunidades' implementation. Acknowledging that innovative safeguards have limited political interference it carefully identifies actors whose behavior may have respond to political incentives and at the same time affect the implementation of the program.

Leveraging the rotating structure of municipal elections it relies on a quasi-experiment that created exogenous political incentives for local actors. I show that enrollment of new beneficiaries was significantly higher during elections before the *blindaje electoral* reform in 2001 that increased barriers to political interference. Consistently with qualitative studies and reported observations this pattern was larger in more disputed municipalities, which also confirms that I am identifying political determinants of the expansion.

After 2001, elections still mattered for a more efficient enrollment of beneficiaries but only on the year when the program started operating. This suggests that replacing *Promotoras* by a group of elected *vocales* was successful in limiting political interference once the program started operating. Indeed my analysis of the actors for whom political incentives matter the most point toward local government's employee rather than program officials after blindage electoral was implemented. Beyond election, alignment with the national incumbent was another kind of political incentives affecting enrollment rate.

Finally my analysis suggests that additional enrollment due to political incentives worsened targeting and program's efficiency. Per-beneficiary program's effectiveness in reducing poverty and inequality seems to have dropped.

More research should however be conducted to analyze the impact of political incentives during program implementation on program's effectiveness.

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