

CSAE Working Paper WPS/2012-09

The local electoral impacts of conditional cash transfers Evidence from a field experiment *

Julien Labonne[†]
Oxford University

July 2012

Abstract

I develop and test two competing models assessing the impacts of targeted government transfers on a local incumbent's electoral performance. I take advantage of the randomized roll-out of a large-scale Conditional Cash Transfer program in the Philippines, which offers an ideal setting to test the models. Although the program was usually implemented in all villages in a municipality, a subset of beneficiary municipalities were randomly selected to receive the program in a randomly selected subset of villages. I find that, in a competitive political environment, incumbent vote share is 26 percentage points higher in municipalities in which the program was implemented in all villages than in municipalities in which the program was implemented in half of the villages. The program had no impact in municipalities with low levels of political competition. Further, within municipalities, there is evidence consistent with the argument that incumbents compensated households in control villages by redistributing their own budget there. Results suggest that anti-poverty programs might have nefarious long-term consequences by preventing replacements of local incumbents.

JEL code: D72

Keywords: elections, conditional cash transfers, decentralization

*I am grateful to Marcel Fafchamps, Tarcisio Castaneda, Sam Clark, Luisa Fernandez, Milwida Guevara, Bert Hofman, Yoko Okamura, Junko Onishi, Rashiel Velarde and Kate Vyborny for fruitful discussions while working on this paper. The Department of Social Welfare and Development kindly allowed me to use data from the National Household Targeting System for Poverty Reduction. I thank Fermin Adriano, Hrithik Bansal, Paddy Barron, Cesi Cruz, Nancy Hite, Claire Labonne, Simon Quinn, Matthias Schündeln and participants in the CSAE Research Workshop and NEUDC Conference 2011 for comments. All remaining errors are mine.

[†]Department of Economics, University of Oxford, Manor Road, Oxford OX1 3UQ, United Kingdom, email: julien.labonne@economics.ox.ac.uk

1 Introduction

Can anti-poverty programs implemented by the central government at the local level increase local incumbents' re-election chances? Strategic concerns on the part of the central government have made it challenging to causally interpret correlations observed in the data. Recently, researchers have used data from quasi-experiments to provide evidence consistent with a causal impact of increased government spending on local incumbents' re-election chances (Brollo, Nannicini, Perotti and Tabellini (2010); Litschig and Morrison (2010)).¹ If that is the case, there is a risk that the loss associated with this indirect decrease in local electoral accountability outweighs the direct benefits from government spending.² This concern might be especially acute in the case of anti-poverty programs targeted to poor districts, that are often governed by poorly performing elected local officials. Even in situations where such programs have short-term positive impacts on household welfare, long-term benefits could be negative as they could prevent needed replacements of local incumbents.

Over the past decade, there has been a steady increase in the number of countries that have set up conditional cash transfer (CCT) programs. They were first implemented in Mexico, starting with the now well-known PROGRESA, before spreading to the rest of Latin America. Recently such programs have started in a number of countries from East Asia to Africa. While implementation arrangements vary by context, the basic premise is that once identified according to some pre-established criteria, poor households receive a grant as long as they meet certain conditions. Available evidence provides support for the view that such programs improve household welfare (Fiszbein and Schady 2009).

This paper assesses the impacts of conditional cash transfers on a local incumbent's electoral performance and tests whether the impacts vary with the district's political environment. Indeed, despite the current trend towards fiscal decentralization, there is no

¹This is in line with findings from the political budget cycle literature. However, in a number of empirical papers the source of variation in government spending is not identified.

²Brollo et al. (2010) show that in Brazil government transfers increased political corruption and decreased the quality of candidates for mayor.

direct evidence of CCT impacts in districts where CCT programs are implemented.³ This paper builds on the recent literature on the impacts of CCT programs on political support for, and the reelection prospects of, the government that implement them. For example, using data on villages involved in PROGRESA, De La O (forthcoming) finds that, compared with villages that had been treated by PROGRESA for only 6 months before the 2000 Presidential elections, incumbent vote share was five percentage points higher in villages in which PROGRESA had been implemented for 21 months. Further, using a regression discontinuity design, Manacorda, Miguel and Vigorito (2010) find that, in Uruguay, beneficiaries of a large-scale temporary CCT program are 11 to 13 percentage points more likely to report favoring the Government. Similarly, there is evidence on the positive political impacts of *Bolsa Familia* in Brazil and of *Familias en Accion* in Colombia (Zucco (2010); Nupia (2011)).

In this paper, I develop two competing models assessing the impacts of targeted transfers from the central government on a local incumbent's electoral performance. Each model highlights a different mechanism through which transfers could affect the incumbent. I then test the models using precinct-level results from the May 2010 municipal elections in municipalities participating in a large-scale CCT program in the Philippines. This program offers an ideal setting to test the models' predictions. Indeed, while the program is usually implemented in all villages in a municipality, from a subset of nineteen beneficiary municipalities, eight were randomly selected to receive the program in a randomly selected subset of the villages. In addition, the level of electoral competition varies greatly across municipalities in the Philippines. It is thus possible to test whether the effects vary depending on the municipality's political environment. In beneficiary municipalities, the program represents a large increase in resources received from the central government. In my sample of municipalities in which the program was implemented in all villages, the annual sum of household grants is about PHP 29.7 million which represents about 71 percent of regular

³The only exception is Bobba (2011) who finds that, in Mexico, candidates affiliated with the party that launched Progresa are less likely to be elected in municipal elections. There is a one term limit for local politicians and as such his sample does not include incumbents.

fiscal transfers from the central government.

The first model combines probabilistic voting, backward looking voters and strategic politicians. Citizens vote for the incumbent if their utility is higher than some pre-determined reservation utility level. That is, they reward politicians for past behavior rather than selecting the politician that will deliver the highest utility level next period. The model predicts that the incumbent will gain electorally from transfers set up by the central government. However, if the incumbent mayor can compensate non-recipients she may be able to equalize her voting score among recipients and non-recipients. The local incumbent can do so by targeting her own transfers towards non-recipients. In areas where the incumbent is unable to fully compensate non-recipients, the gap between recipients and non-recipients decreases in both the share of voters receiving the transfers and the incumbent's budget. As such, the model predicts that the incumbent will redistribute her own budget towards non beneficiaries.

The second model, building on Osborne and Slivinski (1996), introduces principled politicians and forward-looking voters with single-peaked preferences over the set of uni-dimensional policies. They select politicians who promise future policies aligned with their current preferences (*i.e.*, closest to their bliss-point). As a result, once voters have received transfers their policy preferences change and the incumbent might be challenged by a politician offering alternative policies. The model predicts that the local incumbent's probability of re-election is a decreasing function of the proportion of voters covered by the transfers. In addition, poor recipients will be less likely to support the incumbent than (ex-ante) equally poor non-recipients.

The main challenge to the empirical analysis stems from the fact that the set of eligible municipalities over which the randomization was carried out is small. Standard methods to correct for clustering still lead to over-rejection of the null hypothesis of no effect when the number of clusters is small (Cameron, Gelbach and Miller 2008). As a result, in addition to standard errors obtained with the cluster option in Stata, I report Moulton-type standard errors and, importantly, I use critical values from a t-distribution with degrees of freedom determined by the number of clusters and the number of covariates that do not vary

within clusters. I also use information about the randomization process to compute exact distributions for key estimates using randomization inference (Rosenbaum 2002).

Testing the competing models, I find empirical support for the first model and no support for the second one. First, in a competitive political environment incumbent vote share is 26 percentage points higher in municipalities where the program was fully implemented than in the municipalities where only half of the villages were treated. Second, within municipalities, there is no difference in incumbent vote share between villages that were treated and villages that were not. Third, consistent with the first model's predictions, there is evidence that, within municipalities, support for the incumbent is higher in treated villages in municipalities where the pre-transfer municipal budget is low. Those results are related to earlier findings that incumbents are rewarded or penalized for events beyond their control suggesting that, to some extent, voters are unable to distinguish competence and luck.⁴

The remainder of this paper is organized as follows. Section 2 provides an overview of local political dynamics in the Philippines and of the program being evaluated. Section 3 presents two models of voting behavior in a decentralized system. Section 4 introduces the data and the estimation strategy is presented in Section 5. The results are discussed in Section 6 and Section 7 concludes.

2 The Setting

In this section, I explain why both the political context and the program being evaluated offer an ideal setting to test the impacts of anti-poverty programs on local incumbents' electoral performance.

⁴See, for example, Wolfers (2007), Cole, Healy and Werker (2008) and Healy, Malhotra and Hyun-jung Mo (2010).

2.1 Local political dynamics in the Philippines

First, Philippine municipalities are characterized by strong mayors who often campaign on their ability to secure resources from higher levels of government (Lande (1965), Sidel (1999) and De Dios (2007)). As a result, they often attempt to claim credits for central government programs implemented in their municipalities. For example, a number of mayors attempted to claim credit for the program evaluated here, ahead of the 2010 elections (PCIJ 2011).⁵ Credit claiming continued after the elections and reached such high levels that the central government issued a directive preventing local officials from displaying their names and photos on materials bearing the project logo.⁶

The mayoral position has been strengthened by reforms that followed the fall of Marcos in 1986. The first Aquino government set out to re-establish the balance of power between national and local politicians that characterized the Philippines up until Marcos' presidency (Hutchcroft and Rocamora 2003). A new local government code, transferring significant responsibilities to provincial, municipal and barangay Local Government Units (LGU), was passed in 1991.⁷ It was widely acknowledged as a very progressive legislation that strengthened the power of local politicians (Brillantes 1992).

Second, the level of electoral competition varies greatly across municipalities. As a result, it is possible to test whether the effects of anti-poverty programs vary depending on the municipality's political environment. Indeed, despite being prohibited by the 1987 Constitution,⁸ political dynasties are a dominant feature of political life in a number of Philippine municipalities (De Dios 2007). They are characterized by persistence through time and space. Family members take turns holding certain offices and hold different

⁵In an extreme case, prior to the elections, a mayor was telling beneficiaries that transfers would be stopped if he was not re-elected. A project staff who tried to provide accurate information to beneficiaries received death threats and had to be transferred to another municipality.

⁶Memorandum Circular 2012-44 Department of the Interior and Local Government. Available at http://www.dilg.gov.ph/PDF_File/issuances/memo_circulars/DILG-Memo-Circular-201237-ce108cf224.pdf accessed on April 26, 2012

⁷The barangay is the lowest administrative unit in the Philippines and corresponds to a village.

⁸Article II, Section 26 of the 1987 Philippines constitution states *The State shall guarantee equal access to opportunities for public service, and prohibit political dynasties as may be defined by law*. Given the return of a large number of pre-martial law political families to Congress in the 1987 elections (Abinales and Amoroso 2005) no law defining political dynasties was ever passed.

elected offices at the same time. Indeed, while local officials can only hold their position for three consecutive terms, a common strategy is either for the incumbent's spouse or children to run for the position when the incumbent is up against his term limit (Coronel, Chua, Rimban and Cruz (2004); Querubin (2011)).

As pointed out by Sidel (2004), in areas with entrenched political players, incumbents are better able to use alternative electoral strategies which greatly limit voters' ability to cast their ballot as they wish.⁹ Anecdotal evidence indicates that the presence of political dynasties is detrimental to electoral competition. Beckett (2004), in an essay on political families in a Mindanao province, points out that 'a few municipalities were so well controlled that elections went uncontested'.

Third, organized every three years, national and local elections are significant events.¹⁰ Participation rates measured by turnout rates and number of candidates tend to be very high. According to the Commission on Elections (COMELEC), turnout was about 70 percent and 85,000 candidates ran for 17,000 national and local positions in the May 2010 elections. Each citizen votes for the president, the vice-president, twelve senators, one congressman, one governor, one vice-governor, between four and six provincial councilors, one mayor, one vice-mayor and eight municipal councilors. Separate elections are organized for the positions of barangay captains and barangay councilors.

Finally, a striking feature of political dynamics in the Philippines is the general weakness and instability of political parties (Hutchcroft and Rocamora 2003). Ideological differences between the parties are thin at best and each election is usually followed by large shifts in party affiliations as newly elected officials attempt to gain better access to state resources. As a result, it is possible to focus on local incumbents regardless of their party affiliation as the effects are unlikely to be driven by party membership.

⁹For example, in an extreme case of entrenched political dynasty, the Ampatuan clan is known to control the electoral process in areas of Maguidanao by combining vote rigging and threats of violence. They control a well staffed and well equipped private army which they use to generate a culture of fear. As an indication of their ability to rig elections, thousands of voter identification cards were found in their houses in 2009 (Human Rights Watch 2010).

¹⁰The only exceptions are the Presidential and Vice-Presidential elections which are organized every six years.

2.2 The program

The *Pantawid Pamilyang Pilipino Program* (4Ps) is a CCT program implemented by the Department of Social Welfare and Development with financial support from the World Bank and the Asian Development Bank. Similar to other CCT programs, it seeks to reduce poverty by providing grants to poor households on the condition that they meet certain health and education requirements. The health and nutrition grant is given to households with children aged 0-14 years of age and/or pregnant women on the condition that children and pregnant women follow established health protocols. The health grant is currently set at PHP 500 per household per month (for a period of 12 months/year) regardless of the number of children 0-5 years old.¹¹ The education grant is given to households with school-aged children on condition that these children attend school classes on a regular basis (at least 85 percent of the time). The education transfer is of PHP 300 per month (for a period of 10 months/year), up to a maximum of three children. That is, a family with three eligible children receives an annual grant of PHP 15,000 as long as they meet the criteria.

Before the program is launched in a municipality, teams of enumerators carry out a census in all villages of the municipality. Data are collected on a simple set of indicators that are then used to estimate household per capita income through a Proxy Means Test (PMT). To reduce the risks of strategic misreporting, the exact formula is kept secret.¹² Households with children aged 0-14 years and an estimated per capita income below their regional poverty line are then eligible to enroll in the project. There is a small minority of eligible households that fails to register.

The first phase of program implementation, which started in 2008, covered the poorest provinces in the country and the poorest municipalities in each selected province. For practical purposes, implementation was divided into four batches. All participating mu-

¹¹This corresponds to USD 11.6 at the May 9th, 2011 exchange rate (USD 1 = PHP 43.1).

¹²Camacho and Conover (2009), using data from a similar project in Colombia, provide strong evidence of eligibility manipulation when the algorithm became public. They also report that eligibility manipulation was higher in municipalities with more competitive elections which highlights the importance of transfers from the central government for local officials.

municipalities in a given province were allocated to the same batch. The program was then quickly expanded to so-called Set 2 and 3 areas.

The project was implemented with the strong backing of the then president Gloria Macapagal-Arroyo. About 300,000 households had been reached in September 2009 and 700,000 at the end of her term in June 2010. Breaking away from a Filipino tradition whereby an incoming government launches new anti-poverty programs, the Aquino government announced in July 2010 its willingness to expand coverage to reach one million households by the end of 2010 and 2.3 millions by the end of 2011. Contrary to other CCT projects, 4Ps implementation was fairly centralized. Local mayors had no control over whether the program was implemented in their municipality and over the selection of beneficiaries.¹³ Importantly, the funds were transferred directly from the central government agency to the beneficiaries' bank accounts that were opened as part of project implementation.

The set-up and timing of the on-going 4Ps evaluation offers a perfect opportunity to assess the impacts of well-targeted cash transfers on electoral performance.¹⁴ The evaluation aims at measuring project impacts on health, education and poverty outcomes. When the evaluation was being planned in late 2008, project implementation had already started in Batch 1, 2 and 3 municipalities (of Set 1). The evaluation municipalities were selected from Batch 4 municipalities. They were identified as follows. From the eight provinces with Batch 4 municipalities, four were chosen to span the three main island groups (Luzon, Visayas and Mindanao). Those four provinces included 19 Batch 4 municipalities. In each of the four provinces, two municipalities were randomly selected to be part of the evaluation. In those eight municipalities, half of the villages were randomly selected to receive the program in the spring of 2009 while implementation was postponed in the remaining villages. In total, there are 65 treatment villages and 65 control villages. Program implementation proceeded as planned in the spring of 2009 in the 11 municipalities not selected

¹³This is the main difference with programs such as *Bolsa Familia* in Brazil where selection of beneficiaries was transferred to local mayors (de Janvry, Finan and Sadoulet forthcoming)

¹⁴The approach, a downstream experiment, is similar to the one taken by De La O (forthcoming) and Bobba (2011).

to be part of the evaluation. Implementation in the control villages started in early 2012. The randomization was carried out by staff from the World Bank and was fully complied with. From now on, I refer to municipalities in which the program was implemented fully as *full CCT* municipalities and the other as *half CCT* municipalities.

3 Models of Voting Behavior

I present two competing models of voting in local elections in a decentralized context which will help structure the econometric analysis. Starting with the pioneering work of Downs (1957), the literature on voting has been primarily concerned with understanding why and how citizens vote along with politicians' electoral strategies. In this paper, I abstract from the citizens' decision to vote and focus on which candidates they support. Any electorate is likely to be composed of both retrospective and forward-looking voters and politicians are likely to care about both rents and the policies they implement. I am presenting both extreme cases to highlight the general trade-offs faced by politicians and voters. The first model introduces strategic politicians who care about rents they can extract from office and retrospective voters. The second model emphasizes the role forward-looking voters can play in selecting politicians with strict preferences over policies. Both models assume that voters are able to express their preferences at the poll. I here present the main assumptions and key results. A more detailed write-up of the models is available in the Appendix.

3.1 A model of strategic incumbents and retrospective voters

In this model, I focus on the local politician's optimal response to transfers from the central government. It extends the model of Litschig and Morrison (2010) by allowing for transfers from different levels of government and by using more general utility functions. As Persson and Tabellini (2000) and Jones, Meloni and Tommasi (2009), the model combines probabilistic voting and retrospective, backward looking, voters.

The basic setup. The electorate consists of one type of voters with utility :

$$U = u(t) + \alpha$$

where α is a random shock, u is a strictly concave function and t are government transfers. The representative voter supports the incumbent if and only if his utility level is higher than some pre-determined reservation level $\bar{u}$.¹⁵ Otherwise, he votes for the challenger.

As Litschig and Morrison (2010), I assume that the incumbent is concerned about maximizing

$$\gamma v(r) + pR$$

where v is a strictly concave function, r are current rents, γ is a positive parameter, p is the probability of being re-elected and R are rents from winning the election. The incumbent trades off two competing goals; namely the benefits of extracting rents and the probability of re-election. The incumbent faces the budget constraint : $r + t = g$. One should think of g as the budget available to the incumbent to engage in clientelistic practices before the elections rather than as the funds devoted to vote buying. Indeed, Vicente (2010) provides evidence that incumbents tend to benefit more from campaigns to reduce vote-buying, suggesting that they rely more on targeting transfers than on vote-buying. I further assume that the local incumbent does not have the power to tax, which leads to $t \geq 0$. This assumption is meant to capture the fact that local governments' budgets in developing countries mostly consist of fiscal transfers from the central government.

Proposition 1 *Both the optimal level of transfers (t^*) and the probability of re-election (p^*) are increasing in g .*

Proof. In Appendix.

¹⁵While this assumption might appear too simplistic at first, Persson, Roland and Tabellini (2007) argue, in a similar setting, that '*it is much more important to model professional politicians as strategic and fully rational*'. Introducing strategic voters in this model would leave the main results basically unchanged as the incumbent would adjust her strategy accordingly.

As expected, the incumbent benefits electorally from a larger budget as they have more resources to provide transfers to their constituents.

With transfers from the central government. I now assume that the central government transfers $w > 0$ to a share b of voters. In practice, the transfers made to the households might be conditional on some criteria. I do not explicitly model these potential conditions and so one should think of w as the cash equivalent value of the actual transfer.¹⁶ The recipients' income is $w + t_r$ while it is t_n for non-recipients. I assume that the incumbent moves after the central government.

Proposition 2 *The incumbent is strictly better-off than she was without transfers from the central government as she is able to capture a higher level of rents and her probability of re-election has increased.*

Proof. In Appendix.

Since transfers from various sources are substitutes in the voters' utility function, transfers from the central government act as an increase in the local government's budget. The model's predictions would be qualitatively similar under the assumption that voters discount transfers from the central government when deciding who to vote for in the local elections. The incumbent would simply adjust her behavior accordingly.

Since I assume that u is strictly concave, the marginal returns of her own transfers are higher when she targets non-recipients than when she targets recipients of central government transfers. The incumbent re-allocates her own budget towards non-beneficiaries. More precisely, since I assume that transfers from the central government occur before the incumbent decides on the levels of transfers, she is able to target non-recipients of central government transfers.

Proposition 3 *Within an electoral district, recipients and non-recipients of transfers from the central government exhibit similar voting behavior.*

¹⁶See Moffitt (1989) for a similar discussion in the case of food stamps.

Proof. In Appendix.

If the incumbent has enough resources to do so, she is able to equalize her voting score among recipients and non-recipients. However, for this result to hold, one needs to assume interior solutions, which is more likely to be true for larger values of g and b .

Proposition 4 *In areas where the incumbent is unable to fully compensate non-recipients, recipients are more likely to support her than non-recipients. The gap in voting behavior between recipients and non-recipients is decreasing in the incumbent's budget g and in the share b of voters receiving transfers from the central government.*

Proof. In Appendix.

As before, the incumbent decides to target non-beneficiaries of central government transfers. If she is unable to do so, non-recipients' income (including the transfers) will be lower than recipients' income and as such they will be less likely to vote for the incumbent. To put it differently, redistribution towards non-beneficiaries within electoral districts is stronger when the incumbent has either more resources available or less voters to compensate.

3.2 A model of forward-looking voters and 'principled' politicians

I now present an alternative model of voting behavior which is a simplified version of the citizen-candidates model proposed by Osborne and Slivinski (1996). In this model, voters support the candidate that will maximize their utility in the next period. There is a pool of potential candidates who are able to commit to a specific set of policies (unidimensional) captured by parameter ϕ . Once they have announced their policy preference, it can no longer be changed.¹⁷

¹⁷In this model, politicians are not concerned about being re-elected but simply about implementing their preferred policies. Indeed, if they were concerned about their re-election prospects they should not be seen doing anything while in office as it would negatively affect their probability of being re-elected.

Voters have single-peaked preferences over the set of policies and they support the candidate whose preferred policy is closest to their maximum. This maximum is a monotonic function of the voter's income (y_i). Voter i prefers candidate A over candidate B if and only if $u_i(\phi_A) > u_i(\phi_B)$. Potential candidates can decide to challenge the incumbent, incurring cost c . If they win the elections, they get benefits which are assumed to be greater than c . The elections are assumed to be decided by plurality rule. That is, the candidate with the most votes wins without run-off. In the first period, potential candidates decide whether or not to challenge the incumbent and announce their preferred policy. Citizens vote in the second period and the elected official implement her preferred policy in period 3. The solution concept is the pure strategy Nash equilibrium.

The electoral district is composed of two types of voters: poor (income y^L) and rich (income y^H), with the share of poor voters assumed to be greater than half. Given that the incumbent was elected in the last election, I further assume that his preferred policy is $\phi^*(y^L)$, that is he represents the preferences of the poor.

Proposition 5 *The unique pure strategy Nash equilibrium is that no challenger enters, only the incumbent runs for election and wins.*

Proof. In Appendix.

Intuitively, since voters' preferences have not changed since the last election, the incumbent still represents the median voter and, as such, no politician is willing to challenge the incumbent.

I now assume that the central government introduces transfers w to a share b of poor citizens. Recipients now have income $w + y^L$ which I assume to be strictly smaller than y^H . I further assume that recipients' utility is higher under policy $\phi^*(y^L)$ than under policy $\phi^*(y^H)$. This is meant to capture the idea that transfers are not so high as to generate a large shift in recipients's preferences.

Proposition 6 *For any non-zero government transfer, incumbent vote share is decreasing in the share of citizens that receive the transfers.*

Proof. In Appendix.

The introduction of transfers change recipients' preferences. This leaves some room for a challenger representing the preferences ($\phi^*(w + y^L)$) to enter the race. He captures the votes from both the rich and the poor that are receiving transfers from the central government.

It should be noted that this model is only one representation of a broader set of models in which voters support candidates who maximize their future welfare. While I interpreted y as income, nothing hinges on this interpretation. It could be any other welfare dimension that could be affected by the transfers. As long the transfers affect the recipients' preferences, they might change the median voter's preferences. As a result, the incumbent might be challenged at the poll.

3.3 Testable predictions

I now summarize the main testable predictions stemming from the two competing models. The models have polar assumptions about the strategic behavior of both voters or politicians. The first model assumes strategic politicians and retrospective voters, suggesting that voters rewards politicians for past behavior. Conversely, the second model assumes principled politicians and prospecting voters, suggesting that voters select politicians that will implement their preferred policies once in office. In light of those assumptions, one should expect incumbency advantage in the first model but not in the second one.

The first model predicts that local incumbents will benefit electorally from having a CCT program implemented in their electoral districts. The model then generates two sets of predictions for the respective voting behavior of beneficiaries and non-beneficiaries within electoral districts. Under general circumstances, beneficiaries and non-beneficiaries of central government transfers have the same probability of supporting the incumbent as she reallocates her own budget towards non-beneficiaries. She is better able to do so when either the budget at her disposal or the share of voters that receive transfers from the central government are high. Conversely, if the incumbent cares too much about current

rents or her budget is too low, she does not compensate non-beneficiaries, who are then less likely than beneficiaries to support her at the poll.

This second model generates two main testable predictions. First, assuming that a sufficient share of the population receives the transfers, local incumbent's vote share decreases as he no longer represents the median voter's preferences. Second, within an electoral district, non-recipients are more likely than recipients to support the incumbent.

The models' predictions rely on the implicit assumption that voters are able to vote freely. In situations where citizens are unable to vote according to their preferences, the model's predictions would unravel. Coming back to our earlier discussion on political dynamics in the Philippines, it is unclear whether any of the model's predictions applies in dynastic municipalities. As a result, in the empirical analysis, I compare estimates in dynastic and non-dynastic municipalities.

4 Data

I compiled data from a number of sources. Electoral results for 2007 and 2010 were obtained from the Commission on Elections (COMELEC).¹⁸ The data include the number of votes gathered by all candidates, the number of registered voters and participation rates in the municipal elections. In both 2007 and 2010 each village had at least one precinct.¹⁹ This represents an improvement over previous research from Mexico where, while villages were randomly selected for the project, precincts often covered more than one village with some overlap between treatment and control (De La O forthcoming).

I identified incumbents in the 2010 elections using results from the 2007 elections. I verified this information against the listing of Mayors and Vice-Mayors from the Department of the Interior and Local Government.²⁰ In some cases, the mayors were against their

¹⁸Results were not available for 2 of our full CCT municipalities.

¹⁹As part of the automatization process, precincts were aggregated. I matched the new and the old precincts using listings from the project of precinct. Source: http://www.comelec.gov.ph/statistics/2010natl_local/pop/pop_toc.html visited on June 28, 2010.

²⁰In Paracelis, one of our sample municipalities, the mayor elected in 2007 was shot dead on December 25, 2008. His vice-mayor took over and he is considered

term limit and so could not run for re-election. In those situations, a family member ran for the position and they are identified as the incumbent.

To get a proxy of municipalities' political environment, I gathered information on all mayors during the period 1992-2007 in the sample municipalities. The 1992 local elections were the first to be organized after the 1991 Local Government Code had been passed into law. I define a municipality as dynastic if a family managed to keep control of the municipality in at least five of the six local elections between 1992 and 2007.

I also gathered poverty data from various sources. First, data on municipal-level population and poverty incidence were gathered from the National Statistical Coordination Board (NSCB 2009). The latest population count available is from the 2007 Census of Population and poverty incidence was computed using 2003 data. Second, I supplemented this information with data from the National Household Targeting System, which are the data used by the project to select beneficiaries. I use these data sources to estimate poverty incidence at the village-level as well as the share of households that are eligible for the project. Given that, as indicated above, a small percentage of eligible households fail to register for the project, I will use the latter variable as a proxy for intensity of treatment at the village-level.

As would be expected given the targeting criteria used, poverty is prevalent in our sample municipalities (cf. Table 1). According to the 2003 official poverty statistics, poverty incidence was 68 percent, while results from the proxy means test indicate that in late 2008, poverty incidence was 76 percent. About 69 percent of households were eligible to receive the cash grants. In sample areas, eligible households' average annual per capita income was estimated to be PHP 8,958. The average annual per capita transfer represents about 24 percent of annual per capita income.

Data from the Department of Budget and Management (DBM) and from the Department of the Interior Government (DILG) indicate that, in the sample municipalities, average annual Internal Revenue Allotment (IRA) allocation is PHP 44 million or about PHP

the incumbent. Source: http://newsinfo.inquirer.net/breakingnews/regions/view/20080101-109845/Communist_rebels_deny_role_in_murder_of_Paracelis_mayor visited on Sept 26, 2010)

2,054 per capita.²¹ Consistent with the view that most rural municipalities rely on IRA allocations for their budget, IRA represented 94.3 % of total municipal income in 2008 in our sample (min: 86.2% ; max: 99.8%). However, municipal income data are self-reported so, to reduce measurement error, I use the more reliable municipal IRA as a measure of municipal budget. Further, to get a sense of the importance of the CCT program locally, I compare the IRA allocation with the annual sum of all CCT grants made to households in the municipality. In the full CCT municipalities it is about PHP 29.7 million, which represents about 70.9 percent of IRA allocation. In the half CCT municipalities, it is PHP 15.8 million, about 32 percent of IRA allocation. It is important to note that IRA allocations are computed using a fixed and well-known formula. As a result, they are not adjusted to account for the presence of the program in the municipality.²²

The number of candidates for the municipal elections in 2007 was 2.5 and the winner gathered 58 percent of the vote, with a maximum of 99 percent. The number of candidates increased to 3.6 in the 2010 elections. Finally, 47 percent of our municipalities are classified as dynastic.

5 Estimation Strategy

In this section, I present the empirical strategy. I start by comparing an incumbent's electoral performance in municipalities where the program was implemented in all villages and in municipalities where only half of the villages benefited from the transfers. I then compare, within municipalities, an incumbent's electoral performance in villages that received the CCT program and in villages that did not.

²¹Data were collected from : http://www.dbm.gov.ph/lbm_2008-57new.htm and <http://blgf.gov.ph/#> both visited on Sept 21, 2010

²²This is important as Bobba (2011) has shown that, in Mexico, municipalities that received Progreso experienced a decline in fiscal transfers. This could have limited local officials' ability to compensate non-beneficiaries.

5.1 CCT impacts across municipalities

The basic regression is of the form:

$$Y_{ikl} = \alpha FullCCT_{kl} + v_l + u_{ikl} \quad (1)$$

where α is the average treatment effect, Y_{ikl} is the outcome of interest in precinct i in municipality k in province l during the May 2010 elections, $FullCCT_{kl}$ is a dummy equal to one if the program was implemented in all villages in municipality k , v_l is an unobserved characteristics affecting all incumbents within the province equally and u_{ikl} is the usual idiosyncratic error term. I present results where Y_{ijk} is either the incumbent vote share or the incumbent vote margin (in percent).

In this set of regressions, the first model predicts that $\alpha > 0$ as we are comparing electoral results in municipalities receiving different levels of treatment. Conversely, the second model predicts that $\alpha < 0$. In light of the differences between dynastic and non-dynastic municipalities discussed above, I also estimate:

$$Y_{ikl} = \alpha_d FullCCT_{kl} * D_{kl} + \alpha_{nd} FullCCT_{kl} * (1 - D_{kl}) + \beta D_{kl} + v_l + u_{ikl} \quad (2)$$

where D_{kl} is a dummy equal to one if municipality k in province l is classified as dynastic. As indicated earlier, a municipality is classified as dynastic if a family managed to keep control of the municipality in at least five of the six local elections between 1992 and 2007.

I test if results are robust to the inclusion of pre-treatment values of some covariates. Doing so can improve the precision of the estimates by reducing the standard errors (Duflo, Glennerster and Kremer 2007). Further, as pointed out by Leamer (2010), in finite samples, the correlation between control and treated units is not zero as randomization only ensures lack of correlation asymptotically. One needs to check if results are sensitive to the inclusion of baseline covariates. I control for the number of registered voters in the precinct, the candidate's vote share in the 2007 local elections and the percentage of households eligible

for the program in the village.²³

I account for how the randomization was carried out by computing standard errors robust to arbitrary variance structure within municipality. With a limited number of clusters, as is the case here (17), these standard errors are downward biased and lead to over-rejection of the null hypothesis of no effect. Cameron et al. (2008) propose various methods to adjust standard errors to improve inference. In addition to the standard cluster option in Stata, I report standard errors obtained with the Moulton-type estimator (using the Moulton command developed by Steve Pischke). As recommended by Cameron et al. (2008), I use critical values from a T distribution with $G - c$ degrees of freedom, where G is the number of clusters and c is the number of regressors that do not vary within clusters. In settings similar to those discussed in this paper, once the adequate t-distribution is used, results obtained with the cluster option in Stata lead to a slight over-rejection of the null hypothesis and results using the Moulton standard errors have rejection rates that are not statistically different from 0.05 (Cameron et al. 2008).

An alternative approach that does not rely on large sample approximations is randomization inference (Rosenbaum 2002).²⁴ The method relies on knowledge of the randomization procedure to get the exact distribution of the test statistics over the population. As such, it does not require any assumption about the data generating process and is usually thought to be conservative (Bloom, Bhushan, Clingingsmith, Hong, King, Kremer, Loevinsohn and Schwartz 2006). Specifically, one generates the full set of placebo treatments $\widetilde{CCT}_{kl}$ that could have resulted from the randomization process. Then, for each such placebo treatment, one estimates the parameter of interest $\hat{\alpha}$ in equation (1). By construction, the distribution of $\hat{\alpha}$ is such that $E(\hat{\alpha}) = 0$. The null hypothesis is rejected if the estimated parameter lies outside the relevant percentile in the distribution of placebo effects.

In light of the first model's predictions, the gain from the transfers for the local in-

²³The candidate's vote share in the 2007 elections is likely endogenous and thus, those results should be only thought of as robustness checks.

²⁴See Iyer (2010) and Small, Ten Have and Rosenbaum (2008) for recent applications.

cumbent are decreasing in per capita budget. As a result, I also estimate the following regression:

$$Y_{ikl} = \alpha FullCCT_{kl} + \beta(Z_{kl} - \bar{Z}_k) * FullCCT_{kl} + \gamma Z_{kl} + v_l + u_{ikl} \quad (3)$$

where Z_{kl} is the municipal budget. I use $(Z_k - \bar{Z}_k)$ as it facilitates interpretation of the estimated coefficients. In such a set-up α can be interpreted as the average treatment effect and β as the effect of the interaction. It is expected that $\beta < 0$ and, while it is not possible to give a causal interpretation to the parameter γ , it is expected to be positive (Litschig and Morrison 2010). I perform the same type of standard errors adjustment as above to account for the limited number of clusters.

I test whether the municipalities in which the program was implemented fully and the municipalities in which the program was only implemented in half of the villages are comparable along observables measured before treatment by the program (Table 2). Results from t-tests indicate that the two groups are similar along electoral and fiscal dimension. However, there is evidence that the municipalities in which the program was implemented fully are poorer. This seems to be driven by the fact that the number of eligible municipalities was not constant in the four provinces from which the evaluation sample is drawn. Given that only two municipalities were selected for the evaluation in each province and that poorer provinces tended to have more eligible municipalities, the sample of municipalities in which the program was implemented fully includes a larger number of poor municipalities. However, once I account for the fact the randomization was carried out at the provincial level by regressing each variable on a dummy indicating whether the program was implemented fully in the municipality and a set of province dummies, I am unable to reject the null that the two groups are similar. Results are available in column 4 of Table 2. None of the estimated coefficients are significantly different from zero at the usual levels of confidence.

Importantly, the two groups are also balanced along political dimensions. For example, 50 percent of municipalities in the half CCT group are classified as dynastic and 44.4

percent in the full CCT group. Of particular interest, the number of candidates is similar in the two groups of municipalities indicating that the program had little impact on pre-electoral competition. Monteiro and Ferraz (2010) obtain similar findings in their analysis of the impacts of resource abundance in Brazilian municipalities. This indicates that results discussed below are more likely to reflect direct changes in incumbent’s behavior than indirect changes in political competition. I do not have data on the candidate’s background and I am unable to test whether the program had any impact on challengers’ quality, however.

5.2 CCT impacts within municipalities

The basic regression is of the form:

$$Y_{ijk} = \alpha CCT_{jk} + v_k + u_{ijk} \quad (4)$$

where Y_{ijk} is the outcome of interest in precinct i in village j in municipality k during the May 2010 elections, CCT_{jk} is a dummy equal to one if the program was implemented in village j , v_k is a municipality-specific unobservable and u_{ijk} is the usual idiosyncratic error term. I account for the way the randomization was carried out by computing standard errors robust to arbitrary variance structure within villages. To estimate whether the intensity of treatment matters, I also estimate equation (4) interacting the treatment dummy with the share of households that are eligible for the program in the treated villages. As above, I present results where Y_{ijk} is either the incumbent vote share or the incumbent vote margin (in percent).

As I am comparing incumbent vote share in treatment and control villages within the same municipality, the first model predicts that $\alpha = 0$ unless the incumbent’s selfishness parameter is large enough in which case, it is expected that $\alpha > 0$.²⁵ Conversely, the

²⁵It is important to note that the first model’s predictions rest on the the implicit assumption that the incumbent knew that the CCT project was implemented in their municipality and that they were able to identify which households received the transfers. This assumption is very likely to hold in practice. Further, in our empirical test the only necessary assumption is weaker as we only require that incumbents

second model predicts it is expected that $\alpha < 0$.

Before proceeding further, I test whether the control and treatment villages were comparable along observables measured before treatment by the program. I do so using t-test of equality of means and Kolmogorov-Smirnov tests of equality of distribution. Results are available in Table 3. For none of the variables tested are the differences between the treatment and the control group significantly different from zero at the usual levels of confidence. As an additional test, I regress each of the variable on a dummy equal to one if the village received the program and on a set of municipal dummies. I include municipal dummies to account for the fact that randomization was carried out at the municipal level. Again, none of the estimated coefficients are significantly different from zero at the usual levels of confidence. This provides strong support for the view that control and treatment villages were statistically comparable prior to project implementation.

Further, in the context of the first model, I test the presence of redistribution by interacting the treatment dummy with the per capita IRA allocation for the municipality and the municipal-level share of eligible households.²⁶ This is consistent with Leamer (2010) and Deaton (2010), who argue that one should control for interactive confounders to capture heterogeneous impacts. I estimate equations of the form:

$$Y_{ijk} = \alpha CCT_{jk} + \beta(Z_k - \bar{Z}_k) * CCT_{jk} + v_k + u_{ijk} \quad (5)$$

where Z_k is an interactive confounder that varies across but not within municipalities. I do not need to include Z_k directly in the regression as it is captured in v_k , the municipality-specific unobservable characteristics.

knew which villages received the project and which did not.

²⁶While in the model g is the flexible part of municipal budget (*i.e.*, the part of the budget that the incumbent can use to engage in clientelistic practices), I only have data on the overall municipal budget. Incumbents might not be able to use all of those resources for clientelistic practices but I assume that it is proportional to the overall budget.

6 Results

I discuss estimates of the program’s impacts on incumbent vote share and vote margin in the May 2010 elections. I start with results comparing municipalities in which the program was implemented fully and municipalities in which the program was only implemented in half of the villages. I then present results comparing, within municipalities, villages that received the program with villages that did not.

6.1 CCT impacts across municipalities

While I am unable to reject the hypothesis of no effect when comparing the incumbent vote share in full CCT and in half CCT municipalities, once I run the regressions interacting the treatment dummy with dummies capturing whether a municipality is dynastic, I find a large positive impact in non-dynastic municipalities. The program had no impacts on incumbent vote share and vote margin in dynastic municipalities. Results are available in Tables 4 and 5.

The most conservative estimate indicates that the project increases incumbent vote share in non-dynastic municipalities by 26 percentage points.²⁷ While it might appear large at first, this is not too far from what one would expect by looking at the relationship between IRA allocations and incumbent vote share in the 2007 elections. In the sample municipalities, the incumbents gathered 64 percent of the vote and had access to about PHP 42.3 million in IRA in 2007. Given that the difference in the annual sum of household grants from the project between full CCT and half CCT municipalities is about PHP 14.1 million, one would expect a 21 percentage point increase as a result of the project.

The estimated coefficients are statistically different from zero at the 5% level even when I use critical values from a T-distribution with 10 degrees of freedom to account for the limited number of clusters. Results obtained using the more conservative randomization inference approach, are consistent with the ones discussed above. I can still reject the null

²⁷This might seem large given that the program was due to be rolled out in control areas a few months after the elections. It might indicate that, as would be expected, voters value current benefits significantly more than promises about what they will get in the future.

hypothesis of no effect at the 90% (column 1), 95% (columns 2 and 3) and 99% (column 4) for the vote share equations and at the 90% (columns 1-3) and 95% (column 4) for the vote margin equation.

In addition, to test whether results are driven by one outlier, I re-estimate the equation dropping one municipality at a time. All 17 point estimates on the interaction term between the treatment dummy and the non-dynastic dummy are positive and I can reject the null hypothesis that they are equal to zero at, at least, the 95% confidence level, except in one case where I can reject at the 90% confidence level.

I also compare the distributions of changes in incumbent's vote share between 2007 and 2010 in the full and half CCT municipalities in the sample of non-dynastic municipalities. Results displayed in Figure 1 do not seem to suggest that results are driven by one outlier. The whole distribution appears to have shifted to the left. Further, I test whether there was any difference in the winner's vote share between the two sets of municipalities in 2007. Among the non-dynastic subsample, the 2007 winner's vote share was 51.6 percent in half CCT municipalities and 52.1 in full CCT municipalities. The differences are not different from zero at the usual levels of confidence (t-stat = 0.27, p-value = 0.78). This is confirmed by result of a Kolmogorov-Smirnov test of equality of distribution (p-value=0.180).

An explanation for the lack of positive impact of the program on incumbent vote share in dynastic municipalities is that winner's vote share is higher in dynastic municipalities. The incumbent already benefits from strong support (through legal or illegal means) and, it is possible that there is simply no room for the program to have a positive impact. To test if that is the case, I estimate equation (3) where Z_k is the winner's vote share in 2007. Results are available in Table 6. While the interaction term is not significantly different from zero at the usual levels of confidence, results suggest that the program has a positive impact on incumbent vote share in municipalities where winner's vote share was low in 2007. I can reject $H_0 : \beta_{CCT} + (Z - \bar{Z})\beta_{Z*CCT} = 0$ for values of Z below 72 percent at the 90% level (or 67 percent at the 95% level). That is, assuming that the relationship is effectively linear, the program has a positive impact on incumbent vote share in municipalities where the winner gathered less than 72 percent of the votes in 2007. In only 3 of the sample

municipalities did the winner manage to get more than 72 percent of the votes in the 2007 and they are all classified as dynastic. This provides some evidence for the argument that, in dynastic municipalities, incumbents do not gain electorally from transfers because they face less competition.

I test whether, as predicted by the first model, the electoral benefits that the incumbent gets from central government transfers being implemented in his municipality are decreasing with the pre-transfer per capita budget. I estimate equation (3) and results are available in Panel A of Table 7. As would be expected given our previous results, I find no impact on the full sample. I also report results on the sample of non-dynastic municipalities. Results are available in Panel B of Table 7. On this sub-sample, the model predictions are supported by the data. The incumbent benefits from the program and the benefits decrease with municipal budget. There is a strong correlation between per capita IRA and incumbent vote share. While I cannot establish causality, this is consistent with previous findings on the role of local government resources in determining incumbent's re-election chances (Litschig and Morrison (2010) and Monteiro and Ferraz (2010)). Further, the estimated coefficient is about twice as large as on the full sample, suggesting that incumbents in dynastic municipalities are less reliant on fiscal transfers. This is consistent with the paper's main result that the program being evaluated had no effect in dynastic municipalities but a large effect in non-dynastic municipalities.

Finally, while I have previously argued that political parties are not very important in the Philippines, I test whether results are robust to controlling for party membership. Specifically, I look at whether the incumbent belonged to the same party as key politicians in higher levels of government (the congressman, the president and the provincial governor) as this might facilitate access to resources. I create three dummy variables capturing whether the mayor is of the same party as the congressman, as the president or as the provincial governor. I re-estimate equation (2) with those variables. Due to the cost of including additional variables that do not vary within municipalities, I include these variables in turn. Results are available in Table 8. As expected, results are robust to the inclusion of those additional control variables. Neither the size of the estimated coefficients

nor the standard errors change significantly which increase confidence in the results.

6.2 CCT impacts within municipalities

Within participating municipalities, incumbent vote share and vote margin are no higher in villages that received the program than in control villages (cf. Table 9). This provides empirical support for the first model's prediction that, within municipalities, CCT beneficiaries and non-beneficiaries will exhibit similar voting behavior. Failure to reject the null hypothesis does not appear to be driven by large standard errors as the point estimates are very small, about 0.6 percentage points in the vote share equation. Consistent with this interpretation, I can reject the hypothesis that the effect size is larger than x at the 95 percent confidence level for values of x as low as 3.6 percentage-points. As expected given the random assignment of treatment at the village-level, this initial result is robust to the inclusion of a variety of baseline covariates. Controlling for the number of registered voters, the share of households that are eligible for the program do not change the size of the estimates. Further, results are robust to the inclusion of the 2007 vote share.²⁸

As indicated above, I now test whether controlling for intensity of treatment, measured as the percentage of households in the village that are eligible for the project instead of simply controlling for the treatment dummy affects our results (cf. Column 3 in Table 9). The estimates in the vote share equation are not significantly different from zero at the usual levels of confidence.²⁹ However, there is a small positive impact in the vote margin equation. Specifically, a one standard deviation increase in the share of households that are treated by the project increase incumbent vote margin by 4.7 percentage points, or 10 percent of a standard deviation in incumbent vote margin.

In addition, I estimate equation (4) on the sub-sample of dynastic municipalities and on the sub-sample of non-dynastic municipalities. As indicated above, a municipality is classified as dynastic if a family managed to keep control of the municipality in at

²⁸Results are not reported but are available upon request.

²⁹One should note that this has no implication for our theoretical results on redistribution. Indeed, the relevant variable is the municipal-level share of households that are eligible for the transfers.

least 5 of the 6 local elections between 1992 and 2007. Results are available in Table 10. Taking into account the municipality's political environment does not alter our main result. In both types of municipalities, voters in villages in which the CCT program was implemented do not support the incumbent in greater proportion than voters in villages in which it was not implemented. Again, this finding is robust to the inclusion of baseline covariates. Interestingly, in the sub-sample of non-dynastic municipalities, project impacts with respect to incumbent's vote share and vote margin increase with the intensity of treatment. Specifically, a one standard deviation increase in the share of households that are treated by the project increases incumbent vote share by 5 percentage points, or 14 percent of the mean incumbent vote share in this sub-sample.

I now turn my attention to testing the first model's predictions that redistribution will be higher in municipalities with larger budgets or share of voters that are eligible for the program. Results from estimations of equation (5) are available in Table 11. As indicated above, I do not control for municipal budget and for the share of voters that are eligible for the program as regressions include municipal fixed effects. In both cases the estimated coefficients for the interaction term is significantly different from zero at (at least) the 5% level. In line with the first model's predictions, the differences between incumbent vote share in treated and control villages are smaller in municipalities with larger budget but, surprisingly, they are larger in municipalities with a large share of households receiving the program. The correlation between the two variables is -0.68, which could explain the conflicting results. As a result, I run the regressions with both variables at the same time. While the signs of the estimated coefficients do not change, none of them are statistically different from zero at the usual levels of confidence in the vote share regression. I can reject the hypothesis that the two coefficients are jointly equal to zero (F -statistics= 4.54, p -value=0.0124). Given the limited variation in the share of citizens that benefit from the transfer in those municipalities, this set of findings can be interpreted as being consistent with the argument that the lack of observed differences in voting behavior in treatment and control villages is driven by the incumbent's ability to shift his resources towards potential beneficiaries in control villages.

7 Conclusion

Using precinct-level electoral results data from the May 2010 municipal elections in the Philippines and a field experiment, I have shown that local incumbents benefit electorally from CCT programs implemented in their municipality. This is striking once one considers that the incumbent was neither able to influence whether her municipality was selected to participate in the program nor able to influence who received the transfers.

Results presented in this paper are consistent with the view that voters tend to select politicians based on past behavior rather than on the policies they would implement once in office.³⁰ Expectations of reciprocity can fuel the use of patronage politics and vote buying (Schaffer (2007) and Finan and Schechter (2011)). The above results offer new insights on the structure of the Philippines political system. Challengers have less opportunities to provide benefits to voters prior to the elections, providing an explanation for the low turnover observed in elections in the Philippines and the persistence of political dynasties (Coronel et al. 2004) .

The paper highlights additional risks associated with strict poverty targeting of development programs. While such targeting decreases the central government's ability to favor politically important constituencies, it also favors local incumbents, in decentralized settings. This might have nefarious long-term consequences in areas with poorly performing local officials. Even if at first poverty targeting can be justified on equity grounds, it might be necessary to consider additional criteria reflecting the quality of local leadership after an initial round of program implementation.

Results discussed in this paper have implications for the design of, and the interpretation of results from, randomized experiments. Indeed, they highlight that the level at which randomized experiments are carried out can greatly affect estimated coefficients and that individuals in the control group can indirectly benefit from treatment even when it

³⁰In some contexts, voters have shifted their electoral preferences away from the central government as a result of policy shift. For example, in Mexico, de Janvry, Gonzalez-Navarro and Sadoulet (2011) find that, as a result of being granted more secure property rights by a left-wing party, communities were more likely to vote for the more conservative and pro-market party.

is allocated at a fairly aggregated level. Miguel and Kremer (2004) convincingly showed that externalities of health interventions could occur both across children within schools and across schools. Similarly, Angelucci and De Giorgi (2009) showed that PROGRESA beneficiaries shared some of the transfers with non-beneficiaries living in the same communities suggesting that randomization should be carried out at the village rather than at the household-level. Results presented in this paper suggest that, for some outcomes of interest, local officials have incentives to re-allocate their budget towards non-beneficiaries. As such, it might be preferable, though more expensive, to carry out the randomization at the level of the electoral district. As Deaton (2010) argued, to justify that random assignment is truly exogenous to the outcome of interest, one needs to understand the incentives of local actors in responding to the intervention being assessed.

Finally, the results also contribute to the literature on political budget cycles by showing that increased spending before an election benefit local incumbents. In developed countries, available evidence suggests that incumbents tend to be penalized for increasing spending ahead of an election.³¹ Recent studies have shown that, at the local level in developing countries, higher levels of government spending increase local incumbent's reelection rates, however (Jones et al. 2009 Sakurai and Menezes-Filho 2010 Akhmedov and Zhuravskaya 2004). Jones et al. (2009) suggest that the structure of fiscal decentralization can explain those conflicting findings. In a decentralized setting with soft budget constraints, increased spending signals local officials' ability to extract resources from the center; conversely, in areas where local governments face stricter budget constraints, deficits are punished (Jones et al. 2009). Evidence presented in this paper is consistent with this explanation as increased spending is coming from the central government.

³¹See, for example, Peltzman (1992), Brender (2003) and Brender and Drazen (2008).

Appendix

A model of strategic incumbents and retrospective voters

Proposition 1 *Both the optimal level of transfers (t^*) and the probability of re-election (p^*) are increasing in g .*

The first-order condition for the maximization problem is :

$$\gamma v'(g - t) = Ru'(t)$$

Having assumed that u and v are strictly concave functions, it follows that the function $\frac{v'(g-t)}{u'(t)}$ is strictly increasing in t . As a result, the problem has a unique solution t^* which is increasing in g according to the envelope theorem. Depending on the relative slopes of u and v this optimum can be zero. Given that the incumbent's probability of re-election is increasing in t^* it follows directly that $\frac{\partial p^*}{\partial g} > 0$.

Proposition 2 *The incumbent is strictly better-off than she was without transfers from the central government as she is able to capture a higher level of rents and her probability of re-election has increased.*

The incumbent's objective function becomes:

$$\gamma v(g - bt_r - (1 - b)t_n) + 1 - \bar{u} + bu(w + t_r) + (1 - b)u(t_n)$$

For an interior solution, the optimal solutions are given by the following first-order conditions:

$$\begin{aligned}\gamma v'(g - bt_r - (1 - b)t_n) &= Ru'(w + t_r) \\ \gamma v'(g - bt_r - (1 - b)t_n) &= Ru'(t_n)\end{aligned}$$

It follows directly that $w + t_r^* = t_n^*$ and that

$$\begin{aligned}\gamma v'(g + bw - t_n^*) &= Ru'(t_n^*) \\ \gamma v'(g - (1 - b)w - t_r^*) &= Ru'(w + t_r^*)\end{aligned}$$

As above, having assumed that u and v are strictly concave, this set of equations has a unique solution (t_r^*, t_n^*) . They satisfy $t_r^* < t^* < t_n^*$. The incumbent reallocates her own transfers away from recipients of central government transfers. More precisely, since transfers from the central government occur before the incumbent decides on the levels of transfers, she is able to target non-recipients of central government transfers since the marginal return of her own transfer is higher than if she was targeting recipients of central government transfers.

The incumbent is strictly better-off than she was without transfers from the central government as she is able to capture a higher level of rent $(g - bt_r^* - (1 - b)t_n^* > g - t^*)$ and her probability of re-election has increased.

Further, both t_n^* and t_r^* are increasing in g and b . It follows directly that the incumbent's probability of being re-elected $p^* = 1 - \bar{u} + u(t_n^*)$ is increasing in b . The local incumbent benefits electorally from transfers set up by the central government.

Proposition 3 *Within an electoral district, recipients and non-recipients of transfers from the central government exhibit similar voting behavior.*

Within an electoral district, recipients and non-recipients of transfers from the central government exhibit similar voting behavior as $p_r = p_n$, where p_r is the probability that recipients vote for the incumbent and p_n is the probability that non-recipients do. Indeed, $w + t_r^* = t_n^*$

Proposition 4 *In areas where the incumbent is unable to fully compensate non-recipients, recipients are more likely to support her than non-recipients. The gap in voting behavior between recipients and non-recipients is decreasing in the incumbent's budget g and in the share b of voters receiving transfers from the central government.*

I consider a corner solution with $t_n^* > 0$. Indeed, the incumbent will set t_r^* to zero before setting t_n^* to zero as the marginal return of additional transfers to non-beneficiaries is higher than it is for beneficiaries. The optimal solution t_n^* is given by the following first-order condition:

$$\gamma v'(g - (1 - b)t_n^*) = Ru'(t_n^*)$$

Given the assumptions on u and v , the optimal solution is unique and the incumbent's probability of re-election is given by:

$$p^* = 1 - \bar{u} + bu(w) + (1 - b)u(t_n^*)$$

It follows that, since $w > t_n^*$, $\frac{\partial p^*}{\partial b} > 0$. In addition, since $w + t_r^* > t_n^*$ recipients are more likely to support the incumbent than non-recipients. I now look at what explains the gap between the two groups. It follows that $\Delta p = p_r - p_n = u(w) - u(t_n^*)$. Thus $\frac{\partial \Delta p}{\partial g} < 0$ and $\frac{\partial \Delta p}{\partial b} < 0$. The gap in voting behavior between recipient and non-recipient is decreasing in the incumbent's budget g and in the share b of voters receiving transfers from the central government.

A model of forward-looking voters and 'principled' politicians

Proposition 5 *The unique pure strategy Nash equilibrium is that no challenger enters, only the incumbent is running and wins.*

The median voter's preferences have not changed since last elections. The incumbent still represents them and, as such, no politicians is willing to incur cost c to challenge the incumbent.

Proposition 6 *For any non-zero government transfer, incumbent vote share is decreasing in the share of citizens that receive the transfers.*

If θ is the share of poor voters in the population and b is the share of poor voters covered by the transfers.

Case 1: $\theta(1 - b) > \frac{1}{2}$ The unique pure strategy Nash equilibrium is that no challenger enters, only the incumbent is running and wins. In this case, incumbent vote share is one.

Case 2: $\theta(1 - b) < \frac{1}{2}$ The unique pure strategy Nash equilibrium is that a candidate with preferred policy $\phi^*(w + y^L)$ enters and wins. In this case, poor non-recipients vote for the incumbent while poor recipients and rich individuals vote for the challenger. As a result, incumbent vote share is $\theta(1 - b)$ which is decreasing in b .

References

- Abinales, Patricio and Donna Amoroso**, *State and Society in the Philippines*, Anvil Publishing, 2005.
- Akhmedov, Akhmed and Ekaterina Zhuravskaya**, “Opportunistic Political Cycles: Test in A Young Democracy Setting,” *Quarterly Journal of Economics*, 2004, 119, 1301–1338.
- Angelucci, Manuela and Giacomo De Giorgi**, “Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles’ Consumption?,” *American Economic Review*, March 2009, 99 (1), 486–508.
- Beckett, Jeremy**, “Political Families and Family Politics Among Muslim Maguindanaon of Cotabato,” in Alfred McCoy, ed., *An Anarchy of Families: State & Family in the Philippines*, University of Wisconsin Press, 2004, pp. 285–310.
- Bloom, Erik, Indu Bhushan, David Clingingsmith, Rathavuth Hong, Elizabeth King, Michael Kremer, Benjamin Loevinsohn, and Brad Schwartz**, “Contracting for Health: Evidence from Cambodia,” *Harvard University*, *mimeo*, 2006.
- Bobba, Matteo**, “Redistributive Politics and Cash Transfer Programs,” *mimeo*, *Paris School of Economics*, 2011.
- Brender, Adi**, “The effect of Fiscal Performance on local government election results in Israel: 1989-1998,” *Journal of Public Economics*, 2003, 87, 2187–2205.
- and **Allan Drazen**, “How Do Budget Deficits and Economic Growth Affect Re-election Prospects? Evidence from a Large Panel of Countries,” *American Economic Review*, 2008, 98 (5), 2203–2220.
- Brillantes, Alex**, “The Philippines in 1991: Disasters and Decisions,” *Asian Survey*, 1992, 32 (2), 140–145.

- Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti, and Guido Tabellini,** “The Political Resource Curse,” Working Paper 15705, National Bureau of Economic Research January 2010.
- Camacho, Adriana and Emily Conover,** “Manipulation of Social Program Eligibility: Detection, Explanations, and Consequences for Empirical Research,” *Universidad de los Andes - CEDES*, 2009.
- Cameron, Colin, Jonah Gelbach, and Douglas Miller,** “Bootstrap-based improvements for inference with clustered errors,” *Review of Economics and Statistics*, 2008, 90 (3), 414–427.
- Cole, Shawn, Andrew Healy, and Eric Werker,** “Do Voters Appreciate Responsive Governments? Evidence from Indian Disaster Relief,” *Harvard Business School Working Paper No. 09-050*, 2008.
- Coronel, Sheila, Yvonne Chua, Luz Rimban, and Booma Cruz,** *The rulemakers : how the wealthy and well-born dominate Congress*, Quezon City: Philippine Center for Investigative Journalism, 2004.
- de Janvry, Alain, Frederico Finan, and Elisabeth Sadoulet,** “Local Electoral Accountability and Decentralized Program Performance,” *Review of Economics and Statistics*, forthcoming.
- , **Marco Gonzalez-Navarro, and Elisabeth Sadoulet,** “Why Are Land Reforms Granting Complete Property Rights Politically Risky? Electoral Outcomes of Mexico’s Second Land Reform,” *mimeo, University of California - Berkeley*, 2011.
- Deaton, Angus,** “Instruments, Randomization, and Learning about Development,” *Journal of Economic Literature*, 2010, 48, 424–455.
- Dios, Emmanuel De,** “Local Politics and Local Economy,” in Arsenio M. Balisacan and Hall Hill, eds., *The Dynamics of Regional Development*, Ateneo de Manila University Press, 2007.

- Downs, A.**, *An Economic Theory of Democracy*, Harper Row, 1957.
- Duflo, Esther, Rachel Glennerster, and Michael Kremer**, “Using Randomization in Development Economics Research: A Toolkit,” in T. Paul Schultz and John A. Strauss, eds., *Handbook of Development Economics*, Vol. 4, Elsevier, 2007, pp. 3895 – 3962.
- Finan, Frederico and Laura Schechter**, “Vote-Buying and Reciprocity,” *Econometrica*, forthcoming, 2011.
- Fiszbein, Ariel and Norbert Schady**, *Conditional cash transfers: reducing present and future poverty*, The World Bank, Washington, DC., 2009.
- Healy, Andrew, Neil Malhotra, and Cecilia Hyunjung Mo**, “Irrelevant events affect voters’ evaluations of government performance,” *Proceedings of the National Academy of Sciences*, 2010, 107 (29), 12804–12809.
- Human Rights Watch**, “They Own the People. The Ampatuans, State-Backed Militias, and Killings in the Southern Philippines,” 2010.
- Hutchcroft, Paul and Joel Rocamora**, “Strong Demands and Weak Institutions: The Origins and Evolution of the Democratic Deficit in the Philippines,” *Journal of East Asian Studies*, 2003, 3.
- Iyer, Lakshmi**, “Direct Versus Indirect Colonial Rule in India: Long-term Consequences,” *Review of Economics and Statistics*, 2010, 92 (4), 693–713.
- Jones, Mark, Osvaldo Meloni, and Mariano Tommasi**, “Voters as Fiscal Liberals: Incentives and Accountability in Federal Systems,” *mimeo, Rice University*, 2009.
- Lande, Carl**, *Leaders, Factions and Parties: The Structure of Philippines Politics*, Yale University Southeast Asia Studies, New Haven CO, 1965.
- Leamer, Edward**, “Tantalus on the Road to Asymptopia,” *Journal of Economic Perspectives*, 2010, 24 (2), 31–46.

- Litschig, Stefan and Kevin Morrison**, “Government Spending and Re-election: Quasi-Experimental Evidence from Brazilian Municipalities,” *mimeo*, 2010.
- Manacorda, Marco, Edward Miguel, and Andrea Vigorito**, “Government Transfers and Political Support,” 2010. University of California - Berkeley, *mimeo*.
- Miguel, Edward and Michael Kremer**, “Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities,” *Econometrica*, 2004, 72 (1), 159–217.
- Moffitt, Robert**, “Estimating the Value of an In-Kind Transfer: The Case of Food Stamps,” *Econometrica*, 1989, 57 (2), 385–409.
- Monteiro, Joana and Claudio Ferraz**, “Does Oil Make Leaders Unaccountable? Evidence from Brazil’s offshore boom,” *PUC-Rio mimeo*, 2010.
- NSCB**, “2003 City and Municipal Level Poverty Estimates,” *National Statistical Coordination Board*, 2009.
- Nupia, O**, “Anti-poverty programs and presidential election outcomes: Familias en accion in colombia,” *Working Paper, Universidad de los Andes*, 2011.
- O, Ana Lorena De La**, “Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico,” *American Political Science Review*, forthcoming.
- Osborne, Martin and Al Slivinski**, “A Model of Political Competition with Citizen-Candidates,” *The Quarterly Journal of Economics*, 1996, 111 (1), pp. 65–96.
- PCIJ**, “The political perils of the 4Ps,” *Philippines Center for Investigative Journalism*, 2011, <http://www.pcij.org/blog/2011/06/02/the-political-perils-of-the-4ps>.
- Peltzman, Sam**, “Voters as Fiscal Conservatives,” *Quarterly Journal of Economics*, 1992, 107, 325–345.

- Persson, Torsten and Guido Tabellini**, *Political Economics: Explaining Economic Policy*, Cambridge, MA: MIT Press, 2000.
- , **Gerard Roland, and Guido Tabellini**, “Electoral Rules and Government Spending in Parliamentary Democracy,” *Quarterly Journal of Political Science*, 2007, *2*, 155–188.
- Querubin, Pablo**, “Political Reform and Elite Persistence: Term Limits and Political Dynasties in the Philippines,” *mimeo*, MIT, 2011.
- Rosenbaum, Paul**, *Observational Studies*, New York, NY: Springer, 2002.
- Sakurai, Sergio and Naercio Menezes-Filho**, “Opportunistic and partisan election cycles in Brazil: new evidence at the municipal-level,” *Public Choice*, 2010.
- Schaffer, Frederic Charles**, *Elections For Sale. The Causes and Consequences of Vote Buying*, Ateneo de Manila University Press, 2007.
- Sidel, John**, *Capital, coercion, and crime: bossism in the Philippines* Contemporary Issues in Asia and Pacific, Stanford University Press, 1999.
- , “Bossism and democracy in the Philippines, Thailand and Indonesia: Towards an alternative framework for the study of ‘local strongmen’,” in J. Harris, K. Stokke, and O. Tornquist, eds., *Politicizing democracy: The new local politics of democratization.*, Palgrave, 2004.
- Small, Dylan, Thomas Ten Have, and Paul Rosenbaum**, “Randomization Inference in a Group-Randomized Trial of Treatments for Depression: Covariate Adjustment, Noncompliance, and Quantile Effects,” *Journal of the American Statistical Association*, 2008, *103* (481), 271–279.
- Vicente, Pedro**, “Is Vote Buying Effective? Evidence from a Field Experiment in West Africa,” *University of Oxford*, *mimeo*, 2010.

Wolfers, Justin, “Are Voters Rational? Evidence from Gubernatorial Elections,” *The Wharton School, mimeo*, 2007.

Zucco, Cesar, “Cash-Transfers and Voting Behavior: An assessment of the political impacts of the Bolsa Familia Program,” *mimeo, Princeton University*, 2010.

Table 1: Descriptive statistics

	Mean	St. Dev.	Min	Max
Population (2007) (thousands)	25.2	(13.9)	9.4	58.4
Poverty Incidence (2003)	67.5	(8.9)	54.0	86.7
IRA (2008) (PHP million)	44.0	(14.2)	25.2	71.7
Per capita IRA (2008) (PHP)	2054.7	(857.4)	1015.3	3946.1
IRA as % of income (2008)	94.3	(5.1)	82.6	99.8
Share Poor (PMT)	76.3	(14.2)	59.2	97.4
Share Eligible (PMT)	68.9	(10.3)	55.8	88.3
Registered Voters (2010) (thousands)	16.4	(8.0)	5.3	38.4
Nb Precincts (2010)	28.7	(11.3)	9	51
Nb Candidates (2010)	3.6	(1.6)	1	8
Incumbent's Vote Share (2010)	54.5	(25.0)	7.4	94.4
Winner's Vote Share (2007)	57.9	(17.1)	29.6	98.9
Nb Candidates (2007)	2.5	(1.1)	1	5
Dynasty	47.1	(51.4)	0	100

Table 2: Comparing the full CCT and half CCT municipalities

	Half CCT	Full CCT	T-test	OLS
	(1)	(2)	(3)	(4)
Population (2007)	26.324 (14.675)	24.157 (14.037)	-0.311 [0.760]	7.649 (12.016)
Poverty Incidence (2003)	62.301 (5.852)	71.754 (9.191)	2.490 [0.025]	-3.252 (4.340)
Share Poor (PMT)	67.993 (9.146)	83.311 (14.816)	2.523 [0.023]	-2.595 (4.735)
Share Eligible (PMT)	63.261 (5.947)	73.512 (11.562)	2.251 [0.040]	-1.434 (4.110)
IRA (2008) (PHP million)	47.420 (16.579)	40.892 (11.961)	-0.939 [0.362]	-0.361 (9.006)
IRA as % of income (2008)	93.141 (5.222)	95.288 (5.053)	0.861 [0.403]	-2.214 (4.718)
Per capita IRA	2100.071 (952.362)	2014.295 (820.141)	-0.200 [0.844]	-552.157 (349.047)
Registered Voters (2010)	17.162 (9.839)	15.760 (6.454)	-0.351 [0.730]	6.469 (6.888)
Nb Precincts (2010)	27.750 (13.530)	29.556 (9.554)	0.321 [0.753]	3.913 (7.883)
Nb Candidates (2010)	3.750 (2.053)	3.444 (1.130)	-0.386 [0.705]	-0.043 (0.516)
Winner's Vote Share (2007)	55.856 (20.634)	59.699 (14.312)	0.451 [0.659]	11.721 (11.254)
Nb Candidates (2007)	2.500 (1.195)	2.556 (1.014)	0.104 [0.919]	-0.870 (0.424)
Dynasty (chi-sq)	0.444 (0.527)	0.5 (0.535)	0.053 [0.819]	-.152 (0.357)

Notes: The standard deviations are in (parentheses) (Column 1-2) and the p-values are in [bracket] (Column 3). Each cell in Column 4 is the coefficient on the dummy variable indicating whether the project was implemented in all villages in the municipality from a different OLS regression with province fixed-effects. In Column 4, the standard errors (in parentheses) are Huber-corrected. (n=17)

Table 3: Comparing treatment and control villages

	Treatment (1)	Comparison (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Population	1626.169 (1902.585)	1661.323 (1526.472)	0.116 [0.908]	0.169 [0.247]	65.017 (230.591)
Share poor	61.419 (19.154)	61.538 (18.720)	0.036 [0.971]	0.077 [0.985]	-0.485 (2.376)
Share eligible	52.545 (14.944)	53.433 (14.480)	0.344 [0.732]	0.123 [0.641]	-1.091 (2.157)
Nb Precincts	1.754 (1.415)	1.692 (1.158)	-0.271 [0.787]	0.031 [1.000]	0.147 (0.173)
Registered Voters	1083.846 (1026.036)	1029.738 (675.287)	-0.355 [0.723]	0.123 [0.641]	99.791 (139.654)
Winner's Vote Share (2007)	58.68 (2.199)	56.95 (2.415)	-0.529 [0.597]	0.1528 [0.126]	0.719 (1.200)

Notes: The standard deviations are in (parentheses) (Column 1-2) and the p-values are in [bracket] (Column 3-4). Each cell in Column 5 is the coefficient on the dummy variable indicating whether the project was implemented in the village from a different OLS regression with municipal fixed-effects. In Column 5, the standard errors (in parentheses) are Huber-corrected. (n=130)

Table 4: Impacts of CCT on incumbent vote share (across)

	(1)	(2)	(3)	(4)
Panel A - Share Vote				
Full CCT	9.369 (16.776)	7.548 (16.033)	14.415 (10.732)	16.895 (11.479)
Share eligible (village)		-0.046 (0.155)	-0.061 (0.119)	-0.056 (0.118)
Registered voter		-0.014 (0.011)	-0.008 (0.006)	-0.012** (0.005)
Winner's Vote Share (2007)			0.669*** (0.143)	0.672*** (0.145)
pc IRA				-0.005 (0.005)
Observations	488	488	466	466
R-squared	0.401	0.410	0.606	0.617
Panel B - Vote Margin				
Full CCT	12.495 (28.443)	10.138 (27.619)	23.457 (17.160)	26.716 (18.939)
Share eligible (village)		-0.228 (0.298)	-0.275 (0.235)	-0.268 (0.232)
Registered voter		-0.021 (0.020)	-0.010 (0.010)	-0.014 (0.009)
Winner's Vote Share (2007)			1.303*** (0.242)	1.308*** (0.245)
pc IRA				-0.007 (0.010)
Observations	488	488	466	466
R-squared	0.262	0.270	0.534	0.541

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). The standard errors (in parentheses) are Huber-corrected and account for intra-municipality correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Significance levels are computed using critical values from a T distribution with 12 degrees of freedom (Columns 1-3) or 11 degrees of freedom (Column 4). All regressions include province dummies.

Table 5: Impacts of CCT on incumbent vote share by type of municipalities (across)

	(1)	(2)	(3)	(4)
Panel A - Vote Share				
A: Full CCT * Dynastic	-10.852 (13.352) [13.379]	-10.564 (13.171) [13.322]	-5.971 (8.091) [10.697]	-0.293 (8.035) [10.835]
B: Full CCT * Non-Dynastic	26.348 (11.868)** [11.848]**	27.304 (11.597)** [12.376]**	30.999 (9.892)** [10.784]***	34.191 (10.147)*** [10.643]***
Dynastic	34.308 (11.436)** [11.848]**	35.329 (11.539)** [12.376]**	27.344 (9.082)** [10.784]**	31.829 (9.255)*** [10.643]**
Test $H_0 : A = B$	4.29	4.54	7.47	8.18
$H_a : A \neq B$ [p-value]	[.05]	[.05]	[.01]	[.01]
Observations	488	488	466	466
R-squared	0.552	0.555	0.690	0.710
Panel B - Vote Margin				
A: Full CCT * Dynastic	-23.429 (24.822) [25.301]	-22.439 (23.981) [24.770]	-12.104 (13.468) [18.857]	-4.672 (14.093) [19.771]
B: Full CCT * Non-Dynastic	42.737 (21.365)* [21.349]*	46.065 (21.033)* [22.520]*	52.285 (18.150)** [16.764]**	56.463 (19.958)** [16.870]***
Dynastic	61.368 (17.530)*** [22.406]**	64.905 (18.004)*** [23.011]**	46.836 (12.686)*** [19.012]**	52.706 (16.392)** [19.422]**
Test $H_0 : A = B$	3.87	4.27	6.67	6.93
$H_a : A \neq B$ [p-value]	[.06]	[.06]	[.02]	[.02]
Observations	488	488	466	466
R-squared	0.427	0.438	0.619	0.631

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). Standard errors obtained with the cluster option in Stata are in (parentheses) and Moulton standard errors are in [brackets]. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Significance levels are computed using critical values from a T distribution with 10 degrees of freedom (Columns 1-3) and 9 degrees of freedom (Column 4). All regressions include province dummies. In addition to the coefficients reported, I control for the share of household eligible for the program and the number of registered voters (Columns 2-4), for the 2007 winner's vote share at the precinct-level (Columns 3-4) and, for per capita IRA (Column 4).

Table 6: Is the impact decreasing with 2007 winner's share vote?

	(1)	(2)	(3)	(4)
Panel A- Vote Share				
Full CCT	21.158 (10.318)* [8.608]**	20.025 (10.429)* [9.046]*	19.977 (10.157)* [8.837]**	21.930 (11.673)* [8.935]**
2007 Winner's Vote Share (WVS)	1.193 (0.242)*** [0.356]***	1.172 (0.243)*** [0.358]***	0.782 (0.227)*** [0.414]*	0.762 (0.248)** [0.403]*
Full CCT*($WVS - \bar{WVS}$)	-0.204 (0.240) [0.488]	-0.178 (0.255) [0.490]	-0.173 (0.244) [0.482]	-0.176 (0.245) [0.469]
Observations	488	488	466	466
R-squared	0.612	0.615	0.661	0.669
Panel B - Vote Margin				
Full CCT	33.972 (15.179)* [15.023]**	33.002 (15.822)* [15.682]*	32.766 (15.422)* [15.226]*	35.056 (18.534)* [15.636]*
2007 Winner's Vote Share (WVS)	2.185 (0.343)*** [0.622]***	2.167 (0.359)*** [0.621]***	1.402 (0.338)*** [0.713]*	1.378 (0.384)*** [0.706]*
Full CCT*($WVS - \bar{WVS}$)	-0.053 (0.344) [0.852]	-0.025 (0.376) [0.850]	-0.012 (0.358) [0.831]	-0.016 (0.359) [0.820]
Observations	488	488	466	466
R-squared	0.537	0.542	0.605	0.609

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). Standard errors obtained with the cluster option in Stata are in (parentheses) and Moulton standard errors are in [brackets]. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Significance levels are computed using critical values from a T distribution with 9 degrees of freedom (Columns 1-3) and 8 degrees of freedom (Column 4). All regressions include province dummies. In addition to the coefficients reported, I control for the share of household eligible for the program and the number of registered voters (columns 2-4), for the 2007 winner's vote share at the precinct-level (Columns 3-4) and, for per capita IRA (Column 4).

Table 7: Is the impact on vote share decreasing with per capita budget?

	(1)	(2)	(3)
Panel A- Full Sample			
Full CCT	-8.270 (16.717) [17.904]	-9.152 (16.312) [17.652]	5.993 (10.717) [13.771]
pc IRA	0.019 (0.014) [0.018]	0.018 (0.014) [0.018]	0.007 (0.010) [0.013]
Full CCT*($pcIRA - pc\bar{I}RA$)	-0.029 (0.015)* [0.020]	-0.028 (0.015)* [0.020]	-0.016 (0.012) [0.015]
Observations	488	488	466
R-squared	0.454	0.464	0.632
Panel B - Non-Dynastic Municipalities Only			
Full CCT	0.380 (3.754) [12.955]	-1.732 (3.894) [11.825]	0.548 (4.083) [13.279]
pc IRA	0.050 (0.010)** [0.019]*	0.055 (0.007)** [0.017]*	0.054 (0.008)** [0.019]
Full CCT*($pcIRA - pc\bar{I}RA$)	-0.071 (0.013)** [0.021]*	-0.076 (0.010)** [0.019]*	-0.071 (0.010)** [0.021]*
Observations	271	271	252
R-squared	0.687	0.699	0.722

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections. Standard errors obtained with the cluster option in Stata are in (parentheses) and Moulton standard errors are in [brackets]. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Significance levels are computed using critical values from a T distribution with 9 degrees of freedom (Panel A) and 2 degrees of freedom (Panel B). All regressions include province dummies. In addition to the coefficients reported, I control for the share of household eligible for the program and the number of registered voters (columns 2-3) and for the 2007 winner's vote share at the precinct-level (Column 3).

Table 8: Is the impact robust to controlling for party affiliation?

	(1)	(2)	(3)	(4)	(5)	(6)
	Congress			President		
Panel A- Vote Share						Governor
Full CCT * Dynastic	-10.245 (12.751) [13.974]	-2.950 (7.745) [10.759]	-2.345 (16.888) [18.503]	-7.001 (11.400) [15.215]	-1.777 (14.439) [15.630]	-2.789 (10.995) [12.286]
Full CCT * Non-Dynastic	26.100 (12.277)* [11.411]**	35.581 (10.316)*** [8.971]***	25.850 (11.850)* [11.161]**	34.989 (10.324)*** [9.130]***	23.364 (13.712) [11.280]*	35.960 (11.474)** [10.092] ***
Dynastic	33.788 (10.810)** [12.343]**	33.298 (9.159)*** [10.304]**	26.576 (14.203)* [16.600]	37.567 (11.833)** [13.984]**	24.952 (12.992) [14.600]	35.658 (14.361)** [13.964]**
Observations	488	466	488	466	488	466
R-squared	0.552	0.717	0.560	0.713	0.570	0.712
Panel B - Vote Margin						
Full CCT * Dynastic	-18.748 (23.947) [26.096]	-6.547 (15.126) [20.300]	1.732 (30.280) [34.220]	-6.820 (22.393) [28.355]	-14.858 (27.642) [30.372]	-16.756 (17.517) [21.271]
Full CCT * Non-Dynastic	40.824 (22.239)* [21.310]*	57.443 (20.950)** [16.925]***	41.266 (21.117)* [20.641]*	56.718 (20.775)** [17.014]***	39.919 (24.329) [21.918]*	65.023 (19.165)*** [17.473]***
Dynastic	57.357 (16.386)*** [23.049] **	53.742 (17.158)** [19.441]**	38.498 (21.945) [30.700]	54.543 (22.693)** [26.060]*	52.531 (20.946)** [28.370]*	71.238 (21.798)*** [24.176] **
Observations	488	466	488	466	488	466
R-squared	0.436	0.632	0.450	0.631	0.433	0.644

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). Standard errors obtained with the cluster option in Stata are in parentheses and Moulton standard errors are in brackets]. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Significance levels are computed using critical values from a T distribution with 9 degrees of freedom (Columns 1, 3 and 5) and 8 degrees of freedom (Columns 2, 4 and 6). All regressions include province dummies. In addition to the coefficients reported, I control for shared party affiliation with the congressman (Columns 1-2), with the president (Columns 3-4) and with the Governor (Columns 5-6). I further control for the share of household eligible for the program and the number of registered voters, for the 2007 winner's vote share at the precinct-level and, for per capita IRA (columns 2, 4 and 6).

Table 9: Impacts of CCT on incumbent vote share (within)

	(1)	(2)	(3)
Panel A - Share Vote			
CCT	0.654 (1.492)	0.667 (1.483)	0.632 (1.496)
Share eligible (se, village)		-0.069 (0.057)	-0.144* (0.080)
Registered voters		-0.008** (0.004)	-0.008* (0.004)
$CCT * (se - \bar{se})$			0.141 (0.087)
Observations	222	222	222
R-squared	0.897	0.900	0.901
Panel B - Vote Margin			
CCT	0.031 (3.443)	0.085 (3.415)	-0.002 (3.419)
Share eligible (se, village)		-0.168 (0.122)	-0.353** (0.141)
Registered voters		-0.010 (0.008)	-0.010 (0.008)
$CCT * (se - \bar{se})$			0.349** (0.173)
Observations	222	222	222
R-squared	0.826	0.829	0.831

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. All regressions include municipal dummies.

Table 10: Impacts of CCT on incumbent vote share (within)

	(1)	(2)	(3)	(4)	(5)	(6)
	Dynastic Municipalities			Non-Dynastic Municipalities		
Panel A - Share Vote						
CCT	-1.833 (1.599)	-1.764 (1.584)	-1.792 (1.586)	3.551 (2.587)	3.361 (2.685)	3.887 (2.713)
Share eligible (se, village)		-0.041 (0.053)	-0.052 (0.064)		-0.074 (0.135)	-0.305* (0.172)
Registered voters		-0.010*** (0.004)	-0.010*** (0.004)		-0.004 (0.008)	-0.004 (0.007)
$CCT * (se - \bar{se})$			0.021 (0.068)			0.470** (0.230)
Observations	117	117	117	105	105	105
R-squared	0.949	0.951	0.951	0.576	0.579	0.602
Panel B - Vote Margin						
CCT	-5.593 (3.938)	-5.453 (3.824)	-5.568 (3.783)	6.583 (5.674)	5.934 (5.497)	7.119 (5.511)
Share eligible (se, village)		-0.074 (0.106)	-0.121 (0.098)		-0.246 (0.268)	-0.766** (0.305)
Registered voters		-0.016* (0.009)	-0.016* (0.009)		-0.003 (0.015)	-0.001 (0.013)
$CCT * (se - \bar{se})$			0.088 (0.137)			1.058** (0.448)
Observations	117	117	117	105	105	105
R-squared	0.901	0.904	0.904	0.259	0.268	0.318

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. All regressions include municipal dummies.

Table 11: Testing for redistribution

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Share Vote						
CCT	-0.395 (1.330)	-0.394 (1.308)	-0.795 (1.454)	-0.801 (1.430)	-0.793 (1.439)	-0.795 (1.417)
A: CCT*($pcIRA - pc\bar{I}RA$)	-0.005*** (0.002)	-0.005*** (0.002)			-0.002 (0.003)	-0.002 (0.003)
B: CCT*($Eligible - Eligible$)			0.720** (0.280)	0.724*** (0.272)	0.489 (0.480)	0.484 (0.464)
Test $H_0 : A = B$					4.54	4.95
$H_a : A \neq B$ [p-value]					[0.01]	[0.01]
Observations	222	222	222	222	222	222
R-squared	0.902	0.904	0.902	0.905	0.903	0.905
Panel B - Vote Margin						
CCT	-2.116 (3.136)	-2.058 (3.121)	-3.365 (3.202)	-3.292 (3.174)	-3.364 (3.200)	-3.287 (3.172)
A: CCT*($pcIRA - pc\bar{I}RA$)	-0.010*** (0.004)	-0.010*** (0.003)			-0.001 (0.005)	-0.002 (0.005)
B: CCT*($Eligible - Eligible$)			1.687*** (0.623)	1.666*** (0.599)	1.534* (0.903)	1.484* (0.879)
Test $H_0 : A = B$					4.18	4.42
$H_a : A \neq B$ [p-value]					[0.02]	[0.01]
Observations	222	222	222	222	222	222
R-squared	0.833	0.836	0.837	0.839	0.837	0.839

Notes: Results from OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Panel A) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Panel B). The standard errors (in parentheses) are Huber-corrected and account for intra-village correlation. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. All regressions include municipal dummies. In addition to the coefficients reported, I control for the share of household eligible for the program and the number of registered voters (Columns 2, 4 and 6).

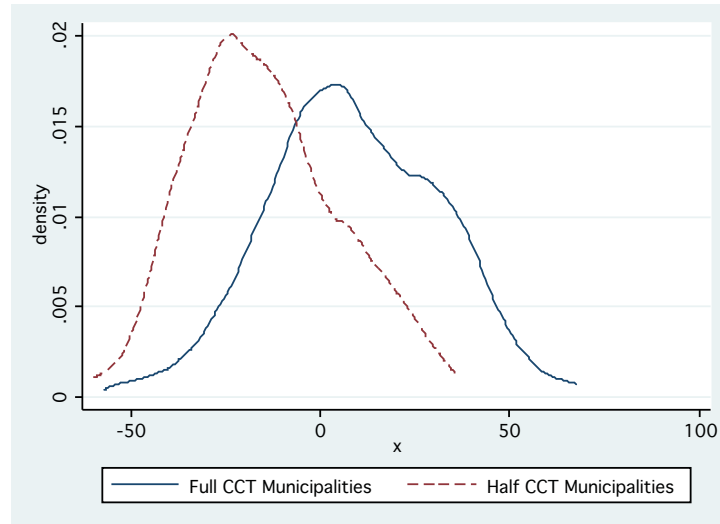


Figure 1: Precinct-level distribution of changes in incumbent's vote share between 2007 and 2010 in non-dynastic municipalities