

EMPIRICAL ESSAYS ON POLITICAL ECONOMY

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A thesis submitted for the degree of
Doctor of Philosophy in Economics
Trinity 2013

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DPhil in Economics
Trinity 2013

Abstract

This thesis is about how elected politicians stay in power and about some of its economic and social consequences, fundamental political economy questions. It takes advantage of the decentralized political structure in the Philippines to test models of voter and politician behavior. In doing so, it contributes to the literature on clientelism and retrospective voting.

In Chapter One, I assess the impacts of targeted government transfers on a local incumbent's electoral performance. I use the randomized roll-out of a CCT program in the Philippines where a number of municipalities are tightly controlled by political dynasties. In a competitive political environment, incumbent vote share is 26 percentage-points higher in municipalities where the program was implemented in all villages than in municipalities where the program was implemented in half of them. The program had no impact in municipalities with low levels of political competition.

In Chapter Two, I test for the presence of political business cycles in Philippine municipalities over the period 2003-2009, a context where according to the literature such cycles are likely to be observed. I find robust evidence for the presence of political business. This effect is only present when I use quarterly data and vanishes when I aggregate the data at the yearly-level. The difference is not merely driven by a decline in statistical power due to aggregation: point estimates for the overall effects are 7 times larger when I use quarterly data than when I use yearly data. This discrepancy can be explained by a drop in employment post-election that dilutes the yearly effects.

In Chapter Three, we estimate the impacts of being connected to local politicians, either currently in office or in opposition, on occupational choice. We use a large administrative dataset collected between 2008 and 2010 on all individuals in 700 Philippine municipalities along with information on all candidates in the 2007 and 2010 municipal elections. We rely on local naming conventions to assess blood and marriage links between households. Using individuals connected to successful candidates in the 2010 elections that did not run in 2007 as a control group, we find that connections to current office-holders increase the likelihood of being employed in better paying occupations. Individuals connected to candidates that were close to being elected in 2007 are less likely to be employed in better paying occupations.

Word count

This thesis consists of approximately 66,585 words. This is calculated using the number of words on page 9 (345) multiplied by the number of pages (193).

Declaration of authorship

The third chapter of this thesis was prepared jointly with Marcel Fafchamps. The paper builds on a chapter of my single-authored MPhil dissertation and so my contributions were substantial. For the current version, Marcel Fafchamps provided further ideas and helped with the writing.

ACKNOWLEDGEMENTS

It goes without saying that this thesis would not have been written without the advice of my supervisor, Marcel Fafchamps. His frank and candid feedback helped me stay the course. Merci.

Prior to coming to Oxford, I had the chance of working on research projects with Jean-Louis Arcand, Robert Chase, Pierre-Alain Jayet and Nick Johnstone. They helped me be a little less clueless than otherwise when I started and for that I am grateful. Rob unknowingly introduced me to the Philippines and is indirectly responsible for this thesis.

I was fortunate to visit Manila a number of times while working on my thesis. During those visits, I have benefited from the kind advice and feedback of Dokoy Capuno, Ron Mendoza, Aniceto Orbeta, Sharon Piza, Lila Shalani and Shiel Velarde. A number of people helped me access data without which this thesis would look rather different. I am grateful to Secretary Soliman for positively responding to my request to access data collected by her department. I would also like to thank dedicated staff from the Commission on Elections, Department of Social Welfare and Development and National Statistics Office. Financial contributions from Mansfield College, the George Webb Medley Fund and the Centre for the Study of African Economies are gratefully acknowledged. I am indebted to Sean Bradley, Patricia Fernandes, Malu Padua and Andrew Parker for flexibly making those visits possible.

A number of friends, coursemates and housemates in Oxford provided encouragement and support. They made this seemingly lonely process more social.

Pour finir je tiens à remercier les membres de la Famille Labonne-Authier, y compris ceux qui, plus ou moins récemment, nous ont quittés ou rejoint, pour toute l'aide qu'ils m'ont apportée et leur soutien sans faille.

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INTRODUCTION

Background and Motivation

This thesis is about how elected politicians stay in power and about some of its economic and social consequences, fundamental political economy questions (Acemoglu and Robinson 2006, Besley 2006). It takes advantage of the decentralized political structure in the Philippines, where significant resources and responsibilities were transferred to elected officials at the provincial and municipal levels in 1991, to test models of voter and politician behavior. In doing so, it contributes to the literature on clientelism and retrospective voting (Hicken 2011, Healy and Malhotra 2013).

Main results and contributions

The first chapter of the thesis, forthcoming in the *Journal of Development Economics*, assesses the impacts of conditional cash transfers on a local incumbent's electoral performance and tests whether the impacts vary with the municipality's political environment. I constructed a dataset of precinct-level results in the 2007 and 2010 mayoral elections and take advantage of the randomized roll-out of a large-scale conditional cash transfer program, the *Pantawid Pamilyang Pilipino Program*, implemented by the central government. Results indicate that, in competitive political environments, local incumbents benefit electorally from cash transfer programs implemented by the central government. There is no effect in non competitive municipalities.

This Chapter makes three distinct contributions to the literature. First, it relies on a randomized experiment to test two competing models assessing the impacts of targeted transfers from the central government on a local incumbent's electoral performance. This is an improvement over the existing literature on the impacts of increased government spending on local incumbents' re-election chances as, to date, strategic concerns on the part of the central government have made it challenging to causally interpret correlations observed in the data. Each model highlights a different mechanism through which transfers could affect the incumbent. The first model, with strategic politicians, predicts that transfers would strengthen incumbency advantage. The second one, with prospecting voters, predicts that the incumbent's vote share decreases as she no longer represents the median voter's preferences.

Second, results highlight that the level at which randomized experiments are carried out can greatly affect estimated coefficients due to general equilibrium effects (Miguel and Kremer 2004). Indeed, within municipalities, there is no difference in incumbent vote share between villages that were treated and villages that were not. The model with strategic politicians provides an explanation for the differences as the marginal return to the incumbents' own transfers are higher when they target villages where the program is not implemented. Therefore she targets individuals living in those villages, leading to a violation of the Stable Unit Treatment Value Assumption (SUTVA) and, in this case, to downward biased estimates. This has clear implications for the design of randomized experiments. 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.

Third, results presented in Chapter One highlight risks associated with strict poverty targeting of government transfers. Such targeting appears to strengthen incumbency advantage. If poorer districts also have poorly performing local of-

ficials, then it might have nefarious long-term consequences in such areas. 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.

This chapter shows that increased spending before an election benefits local incumbents. This contributes to the literature on political budget cycles, the expected increase in government expenditures before elections (Rogoff 1990). In light of that, one would expect local politicians to have incentives to increase budget spending before elections. This question is taken up in Chapter Two.

The second chapter of the thesis tests for the presence of political business cycles in Philippine municipalities over the period 2003-2009. A series of nationally representative labor force surveys are used to build a unique balanced panel dataset of about 1,140 municipalities over 26 quarters. I find robust evidence for the presence of political business cycles. Specifically, I find that employment increases in the two pre-election quarters and decreases below trend in the two post-election quarters. However, once analyses are carried out with yearly data, the effects vanish.

This Chapter makes three distinct contributions to the literature. First, it provides a simple explanation for the challenges researchers face to identify political business and budget cycles (Drazen 2001). In a number of developing countries, local incumbents are *de facto* unable to borrow and so they face constraints to increase total yearly spending. However, they can still spend, in the pre-election period, resources they would have spent in the post-election period. Researchers have tended to use yearly data, which mask this within-year reallocation of resources. Future analyses of political budget and business cycles should, wherever possible, use monthly or quarterly data.

Second, Chapter Two provides one of the first political business cycle analyses at the subnational level. Because of data constraints, subnational analyses have

focused on budget cycles (Akhmedov and Zhuravskaya 2004). This is a return to the literature's origins (Nordhaus 1975). His model was framed in terms of the unemployment/inflation trade-off.

In contexts where local politicians have either significant business interests and/or have strong connections with local businessmen, incumbents might be able to influence employment levels above and beyond what one would expect from budget spending alone. As a result, moving from political business cycles analyses give a more precise description of the potential distortionary effects of elections on labor markets than political budget cycles analyses.

Third, this Chapter contributes to the literature on clientelism by presenting evidence that incumbents attempt to staff the bureaucracy with individuals whose incentives are aligned with their own electoral objectives (Robinson and Verdier 2013). Non-casual employment in the public sector increases ahead of elections and, in municipalities where the incumbent lost, employment in the public sector contracts in the post-election period. This is consistent with the view that jobs in the public sector that are provided before the elections are implicitly tied to the incumbent's electoral success.

In light of those results, one would expect connections to elected officials in the Philippines to be particularly valuable. This question is taken up in Chapter Three.

The third chapter of the thesis provides estimates of both the value of being connected to an elected local official, and the cost of being connected to a politician in opposition.¹ We secured access to an administrative dataset from the Philippines, collected between the 2007 and 2010 elections, which includes the occupation, middle and last names of all individuals residing in 700 municipalities (out of about 1,600 in the country). Results indicate that family connections to current office-holders increase the likelihood of being employed in better pay-

¹ This Chapter was prepared jointly with Marcel Fafchamps.

ing occupations. The effects are large. Further results suggest that relatives of candidates that were close to being elected are less likely to be employed in better paying occupations.

This Chapter makes four contributions to the literature. First, it takes advantage of naming conventions in the Philippines to infer family relationships. Spanish family names were introduced in the Philippines recently (*i.e.*, in the middle of the 19th century), with the specific objective of distinguishing families at the municipal level in order to facilitate census-taking and tax collection. As such they do not reflect other types of ties. This is a marked improvement over the existing literature which had to rely on self-reported data. Such data are subject to bias because those who derive benefits from their connections are more likely to report them.

Second, we provide some of the first estimates of the value of political connections for individuals. Our ability to infer family relationships allows us to construct a number of control groups, including one composed of relatives of successful candidates in elections that took place after the data were collected. We are able to circumvent the most common constraint that the literature has faced to-date: the lack of panel data. This is one of the reason why more progress has been made on establishing the value of political connections for firms (Fisman 2001, Faccio 2006).

Third, we identify a cost of being related to a politician that was close to being elected in the past election. The magnitude of these effects is large. Combined with results obtained in the second chapter, this suggests that incumbents value loyalty and might be reluctant to staff the bureaucracy with individuals whose views and interests are incompatible with theirs.

Fourth, related to the previous point, this Chapter makes a methodological contribution. It shows that, in some contexts, estimates of the value of political connections obtained with regression discontinuity designs potentially include

the cost of being connected to an unsuccessful candidate. Conditioning on close elections changes the nature of the parameter being estimated.

Finally, Chapter Three proposes a simple approach to improve the reliability of empirical work in economics. We asked a third party to split our dataset into two randomly generated, non-overlapping, subsets. We currently only have access to the first one which we use to select the exact set of regressions we would like to run to test our main hypotheses. Those specific regressions will then be estimated on the second subset and all results will be reported in the published version of the paper; thus dealing with concerns about specification search and reporting bias. This is related to the current push for design registration. By using real data, we are able to deal with the main concern associated with this approach: that it might "stifle innovation" (Casey, Glennerster, and Miguel 2012). We argue that, in a world of *big data* (Einav and Levin 2013), researchers could adopt this approach at little cost.

To summarize, this thesis uses original data to test models of voter and politician behavior. Results contribute to the literature on clientelism and retrospective voting. The thesis provides credible evidence that voters tend to reward local incumbents electorally for programs implemented by the central government. It also demonstrates that labor markets experience large fluctuations around elections and presents evidence that this is driven by within-year reallocation of spending by local incumbents. Third, there is suggestive evidence that incumbents are able to both reward their relatives and to punish the relatives of their opponents.

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THE LOCAL ELECTORAL IMPACTS OF CONDITIONAL CASH TRANSFERS: EVIDENCE FROM A FIELD EXPERIMENT

1.1 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 forthcoming, Litschig and Morrison 2010).¹ If that is the case, the loss associated with this indirect decrease in local electoral accountability might outweigh the direct benefits from government spending.²

This paper is forthcoming in the *Journal of Development Economics*. I am grateful to Marcel Fafchamps, Tarcisio Castaneda, Samuel 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 Dean Karlan, two anonymous referees, Fermin Adriano, Hrithik Bansal, Paddy Barron, Cesi Cruz, Nancy Hite, Claire Labonne, Pablo Querubin, Simon Quinn, Matthias Schündeln and participants in the CSAE Research Workshop, NEUDC Conference 2011 and PIDS Seminar for comments. All remaining errors are mine.

¹ This 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.

² Brollo, Nannicini, Perotti, and Tabellini (forthcoming) show that in Brazil government transfers increased political corruption and decreased the quality of candidates for mayor.

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 direct long-term benefits on household welfare, those impacts could be mitigated by negative impacts on accountability.

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 and the growing importance of local politicians, there is no 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 governments that implement them. There is evidence on the positive political impacts of CCT programs in Brazil, Colombia, Mexico and Uruguay (Zucco 2010, Manacorda, Miguel, and Vigorito 2011, Nupia 2011, De La O 2013).

In this paper, I develop two competing models assessing the impacts of targeted transfers from the central government on a local incumbent's electoral

³ The 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.

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 a good setting to test the models' predictions. 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 at the local level varies greatly in the Philippines, where a number of municipalities are characterized by the presence of family dynasties which have been in power for decades.⁴ Those dynasties use different electoral strategies which greatly limit voters' ability to cast their ballot as they wish (Sidel 2004). It is thus possible to test whether the effects are dependent upon the local 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 fiscal transfers from the central government.⁵

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

⁴ This is similar to patterns in the US Congress described by Dal Bo, Dal Bo, and Snyder (2009), who also provide evidence that political power is self-perpetuating. Querubin (2013) presents comparable evidence for the Philippine Congress.

⁵ This corresponds to USD 690,000 at the May 9th, 2011 exchange rate (USD 1 = PHP 43.1).

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

In competitive political environments incumbent vote share is 26 percentage points higher in municipalities where the program was fully implemented than in municipalities where only half of the villages were treated. In addition, within municipalities, there is no difference in incumbent vote share between villages that were treated and villages that were not. This surprising finding, when compared with the previous result, can be explained by incentives faced by incumbents in municipalities where only a subset of the villages are treated by the program. The marginal return of incumbents' own transfers are higher when they target villages where the program is not implemented. Consistent with this interpretation, 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, as incumbents are less able to target non-beneficiaries with their own resources. Those results are related to earlier findings that incumbents are rewarded or penalized for events beyond their control suggesting that, to some extent, voters do not distinguish competence and luck (Wolfers (2007); Cole, Healy, and Werker (2008) and Healy, Malhotra, and Hyun Jung Mo (2010)).

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.⁶ Recent studies have shown that, at the local level in developing countries, higher levels of government spending increase local incumbent's reelection rates (Jones, Meloni, and Tommasi 2012, Sakurai and Menezes-Filho 2010, Akhmedov and Zhuravskaya 2004). Jones, Meloni, and Tommasi (2012) suggest that the structure of fiscal de-

⁶ See, for example, Peltzman (1992), Brender (2003) and Brender and Drazen (2008).

centralization 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, Meloni, and Tommasi 2012). Evidence presented in this paper is consistent with this argument as increased spending is coming from the central government.

The remainder of this paper is organized as follows. Section 1.2 provides an overview of local political dynamics in the Philippines and of the program being evaluated. Section 1.3 presents two models of voting behavior in a decentralized system. Section 1.4 introduces the data and the estimation strategy is presented in Section 1.5. The results are discussed in Section 1.6 and Section 1.7 concludes.

1.2 SETTING

In this section, I explain why both the political context and the program being evaluated offer a good setting to test the impacts of programs implemented by the central government on local incumbents' electoral performance.

1.2.1 *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 credit for central government programs implemented in their municipalities. For example, a number of mayors claimed credit for the program evaluated here, ahead of the 2010 elections (PCIJ 2011).⁷ Credit claiming continued after

⁷ In 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.

the 2010 elections and reached such high levels that, in 2012, the central government issued a directive preventing local officials from displaying their names and photos on materials bearing the project logo.⁸ It is unclear whether the campaign was effective but it does not affect the analysis since the data used in the paper are from the 2007 and 2010 elections.

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 1991.⁹ 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 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,¹⁰ 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 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 described by Fegan (2009): *A family*

8 Memorandum 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.

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

10 Article 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 no law defining political dynasties was ever passed (Abinales and Amoroso 2005).

is a more effective political unit than an individual because it has a permanent identity as a named unit, making its reputation, loyalties, and alliances transferable from members who die or retire to its new standard bearer.

In addition, the nature of politics differs between dynastic and non-dynastic municipalities. Dynastic families are more likely not to derive their power from public office (Querubin 2013). A number of dynasties rely on threats of violence, both towards potential candidates and voters around election time. For example, Bentley (2009) carefully explains how Mohamad Ali Dimaporo, a politician in the Southern island of Mindanao, used his private army to instill fear and stay in office. Other dynasties control the local economy. For example, Sidel (2004) provides a detailed account of how large landowners in the province of Cebu managed to stay in power over generations. Dynasties that do not have access to those alternative sources of power tend to have better links with national politicians and are thus better able to secure funding from the central government. Overall, this suggest that dynastic incumbents are less dependent on central government's initiatives than non-dynastic incumbents.

As pointed out by Sidel (2004), in dynastic municipalities, incumbents are better able to use alternative electoral strategies which greatly limit voters' ability to cast their ballot as they wish. For 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). Similarly, Bentley (2009) indicates that Mohamad Ali Dimaporo was able to manipulate electoral processes to any end he desired.

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’. For example, Gloria Macapagal-Arroyo, daughter of former president Diosdado Macapagal, was president between 2000-2010. Her two sons, Diosdado Ignacio and Juan Miguel were, respectively, representatives for the 1st District of Camarines Sur and 2nd District of Pampanga in 2007-2010. Being constitutionally unable to run for a third term, President Arroyo decided to run for her son’s congressional district in Pampanga in 2010. She won the election with 84.2 percent of the votes. Similar patterns are observed at the provincial and municipal levels (McCoy 2009b).

Third, organized every three years, national and local elections are significant events.¹¹ 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.

Fourth, municipalities receive annual fiscal transfers, the so-called Internal Revenue Allotment (IRA), from the central government. According to Hutchcroft (2012), in practice budget allocations are controlled, with considerable personal discretion, by mayors. Hutchcroft (2012) further cites a study that refers to mayors as *budget dictators*; highlighting that they are able to change budget allocations with ease. In addition, it is important to emphasize that IRA allocations

¹¹ The only exceptions are the Presidential and Vice-Presidential elections which are organized every six years.

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.¹²

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.¹³ 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.

1.2.2 *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.¹⁴ 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

¹² This 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.

¹³ For example, out of the 17 mayors in my sample, 9 changed their party affiliations between the 2007 and 2010 elections: three joined the President's party, five left the President's party and one moved between two parties not affiliated with the President.

¹⁴ This corresponds to USD 11.6 at the May 9th, 2011 exchange rate (USD 1 = PHP 43.1).

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 a combined annual education and health 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.¹⁵ 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 for the program.

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 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 enrolled by 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

¹⁵ Camacho and Conover (2011), 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.

was implemented in their municipality and over the selection of beneficiaries.¹⁶ Importantly, the funds were transferred directly from the central government agency to the beneficiaries' bank accounts that were opened as part of project implementation.

1.2.3 *The evaluation*

The set-up and timing of the on-going 4Ps evaluation offers an opportunity to assess the impacts of well-targeted cash transfers on electoral performance.¹⁷ The evaluation aims at measuring project impacts on health, education and poverty outcomes. The randomization process, described below, was carried out by staff from the World Bank. It was not done through a public lottery.

When the evaluation was being planned in late 2008, project implementation was about to begin in a subset of municipalities in eight provinces.¹⁸ Four of them were chosen to span the three main island groups (Luzon, Visayas and Mindanao). Those four provinces included 19 municipalities where the program was about to begin. 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. There is no evidence of contamination of households in control villages. Program implementation proceeded as planned, and without any specific concerns, in the spring of 2009 in the municipalities not selected to be part of the evaluation.

16 This 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 2012).

17 The approach, a downstream experiment, is similar to the one taken by De La O (2013) and Bobba (2011).

18 In the Philippines, provinces are composed of municipalities and cities which are made up of villages (barangays).

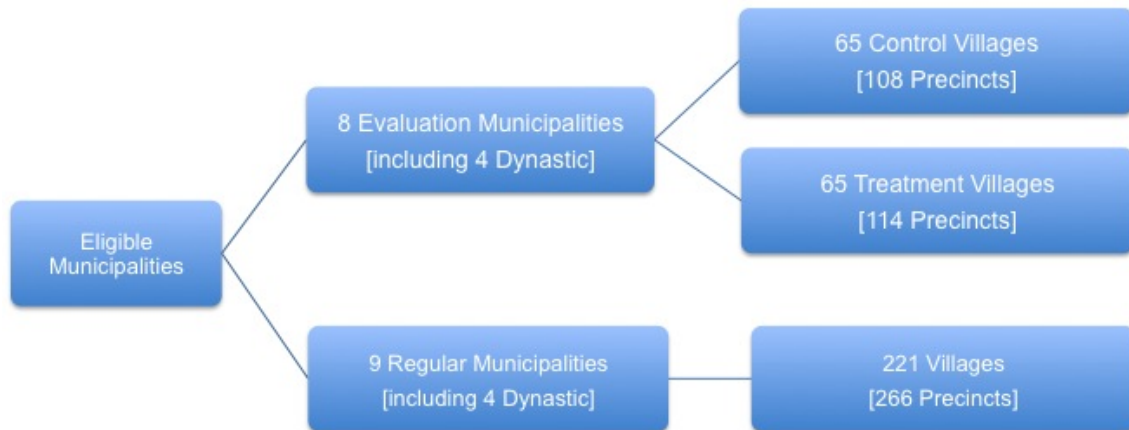


Figure 1.1: Randomization process and sample sizes

From now on, I refer to municipalities in which the program was implemented fully as regular municipalities and the other as evaluation municipalities.

Electoral results were not available for 2 municipalities and the final sample includes 8 evaluation municipalities and 9 regular municipalities. The regular municipalities include 221 villages and 266 precincts. The evaluation municipalities include a total of 65 treatment villages (114 precincts) and 65 control villages (108 precincts). A graphical representation of the randomization process, and associated sample sizes, is available in Figure 1.1. The list of municipalities is available in Table A1.1.

1.3 AN OVERVIEW OF THE MODELS

In this section, I summarize the main testable predictions stemming from the two competing models which are presented in greater details in Annex. 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 reward 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 combines probabilistic voting, backward looking voters and strategic politicians. Citizens vote for the incumbent if their utility is higher than some pre-determined reservation 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 local incumbents will benefit electorally from having a CCT program implemented in their electoral districts and that the benefits are lower in municipalities where the pre-program per capita budget was higher. 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 either 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.

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, which are wealth-dependent, change and the incumbent might be challenged by

a politician offering alternative policies. The 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.

Coming back to the earlier overview on political dynamics in the Philippines, I now briefly discuss how the models' results might be affected by the nature of politics. First, 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. Second, even if voters were able to vote freely, the values of some of the parameters, and some of the predictions, would be different in dynastic and non-dynastic municipalities. For example, dynasties tend to have better links with national-level politicians and, as a result, can secure more resources from higher levels of government. Given that the first model predicts that gains from the program are decreasing with per capita budget, one would expect lower impacts in dynastic municipalities.

1.4 DATA

I compiled data from a number of sources. Precinct-level electoral results for 2007 and 2010 were obtained from the Commission on Elections (COMELEC).¹⁹ The precinct-level 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.²⁰ This represents an improvement over previous research from Mexico where, while villages were

¹⁹ Results were not available for 2 of our regular CCT municipalities.

²⁰ As 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.

randomly selected for the project, precincts often covered more than one village with some overlap between treatment and control (De La O 2013).

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.²¹ In some cases, the mayors were against their 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, and create a dummy variable equal to one, if a family managed to keep control of the municipality in at least five of the six local elections between 1992 and 2007. Out of the 17 municipalities in the sample, 8 are classified as being dynastic, evenly split between regular and evaluation municipalities. Municipality-specific information is available in Table [A1.1](#).

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 (Fernandez 2012). 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 house-

²¹ In 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 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.

holds 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 1.1). 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 2,054 per capita.²² 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 regular 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.

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.

²² Data were collected from : http://www.dbm.gov.ph/lbm_2008-57new.htm and <http://blgf.gov.ph/#> both visited on Sept 21, 2010.

Table 1.1: 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

Notes: n=17

Interestingly, patterns of electoral competition are markedly different between dynastic and non-dynastic municipalities. During the 2007 elections the average number of candidates in dynastic municipalities was 2 while it was 3 in non-dynastic ones. A difference that is statistically different from zero (t-stat : 2.13). Further, the winner gathered 66 percent of the vote in the former set of municipalities and 50 percent of the vote in the latter. Again, the difference is statistically different from zero (t-stat: 2.07).

1.5 ESTIMATION STRATEGY

In this section, I present the empirical strategy. I start by testing whether the treatment and control groups were similar before the intervention. I then compare 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. Finally, I compare, within municipalities, an incumbent's electoral performance in villages that received the CCT program and in villages that did not. The unit of observation is the precinct.

1.5.1 *Testing for balance*

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 1.2). Results from t-tests indicate that the two groups are similar along electoral and fiscal dimensions. 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 1.2. 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 evaluation CCT group are classified as dynastic and 44.4 percent in the regular 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 sufficiently precise data on the candidate's background and I am unable to test whether the program had any impact on challengers quality. Indeed, the certificates of candidacy only collect basic information on the candidates (age, gender, civil status, etc.).

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 1.3. 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.

Table 1.2: Comparing the regular CCT and evaluation CCT municipalities at baseline

	Evaluation CCT	Regular CCT	T-test	OLS
	(1)	(2)	(3)	(4)
Population (2007)	24.157 (14.037)	26.324 (14.675)	0.311 [0.760]	-7.649 [0.536]
Poverty Incidence (2003)	72.127 (8.737)	62.301 (5.852)	-2.685 [0.017]	4.106 [0.397]
Share Poor (PMT)	83.729 (14.069)	67.993 (9.146)	-2.693 [0.017]	3.183 [0.548]
Share Eligible (PMT)	73.886 (10.939)	63.261 (5.947)	-2.440 [0.028]	2.243 [0.615]
Fiscal transfers (2008) (PHP million)	40.892 (11.961)	47.420 (16.579)	0.939 [0.362]	0.361 [0.969]
Fiscal transfers as % of income (2008)	95.288 (5.053)	93.141 (5.222)	-0.861 [0.403]	2.214 [0.647]
Per capita IRA	2,014.295 (820.141)	2,100.071 (952.362)	0.200 [0.844]	552.157 [0.140]
Registered Voters (2010)	15.760 (6.454)	17.162 (9.839)	0.351 [0.730]	-6.469 [0.366]
Nb Precincts (2010)	29.556 (9.554)	27.750 (13.530)	-0.321 [0.753]	-3.913 [0.629]
Nb Candidates (2010)	3.444 (1.130)	3.750 (2.053)	0.386 [0.705]	0.043 [0.934]
Winner's Vote Share (2007)	59.699 (14.312)	55.856 (20.634)	-0.451 [0.659]	-11.721 [0.318]
Nb Candidates (2007)	2.556 (1.014)	2.500 (1.195)	-0.104 [0.919]	0.870 [0.063]
Dynasty (chi-sq)	0.5 (0.535)	0.444 (0.527)	0.053 [0.819]	-0.152 [0.675]

Notes: The standard deviations are in (parentheses) (Column 1-2). In Column 3, the test statistics are reported along with the p-values [bracket]. Each cell in Column 4 is either 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 or the associated p-value in [bracket]. (n=17)

Table 1.3: Comparing treatment and control villages at baseline

	Treatment (1)	Comparison (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Population	1,626.169 (1902.585)	1,661.323 (1526.472)	0.116 [0.908]	0.169 [0.247]	65.017 [0.778]
Share poor	61.419 (19.154)	61.538 (18.720)	0.036 [0.971]	0.077 [0.985]	-0.485 [0.839]
Share eligible	52.545 (14.944)	53.433 (14.480)	0.344 [0.732]	0.123 [0.641]	-1.091 [0.614]
Nb Precincts	1.754 (1.415)	1.692 (1.158)	-0.271 [0.787]	0.031 [1.000]	0.147 [0.397]
Registered Voters	1,083.846 (1026.036)	1,029.738 (675.287)	-0.355 [0.723]	0.123 [0.641]	99.791 [0.476]
Winner's Vote Share (2007)	65.174 (21.327)	60.391 (23.418)	-1.188 [0.237]	0.180 [0.208]	2.327 [0.219]

Notes: The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the project was implemented in the village from a different OLS regression with municipal fixed-effects or the associated p-value in [bracket]. (n=130)

1.5.2 CCT impacts across municipalities

I start by using the full sample of regular and evaluation municipalities. The basic regression is of the form:

$$Y_{ikl} = \alpha \text{RegCCT}_{kl} + v_l + u_{ikl} \quad (1.1)$$

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, RegCCT_{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_{ikl} is either the incumbent vote share or the incumbent vote margin (in percent).

In light of the differences between dynastic and non-dynastic municipalities discussed above, I also estimate:

$$Y_{ikl} = \alpha_d \text{RegCCT}_{kl} * D_{kl} + \alpha_{nd} \text{RegCCT}_{kl} * (1 - D_{kl}) + \beta D_{kl} + v_l + u_{ikl} \quad (1.2)$$

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 a 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, Gelbach, and Miller (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, Gelbach, and Miller (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, Gelbach, and Miller 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, Loewensohn, and Schwartz 2006). Specifically, one generates the full set of placebo treatments $\widetilde{CCT}_{kl}$ that could have

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

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

resulted from the randomization process. Then, for each such placebo treatment, one estimates the parameter of interest $\hat{\alpha}$ in equation (1.1). 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.

To test if the impacts are lower in municipalities with higher levels of per capita budget, I estimate the following regression:

$$Y_{ikl} = \alpha \text{RegCCT}_{kl} + \beta(Z_{kl} - \bar{Z}_k) * \text{RegCCT}_{kl} + \gamma Z_{kl} + v_l + u_{ikl} \quad (1.3)$$

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.

1.5.3 CCT impacts within municipalities

I now focus on the sample of evaluation municipalities. The basic regression is of the form:

$$Y_{ijk} = \alpha \text{CCT}_{jk} + v_k + u_{ijk} \quad (1.4)$$

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 (1.4) 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).

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.²⁵ 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:

$$Y_{ijk} = \alpha CCT_{jk} + \beta(Z_k - \bar{Z}_k) * CCT_{jk} + v_k + u_{ijk} \quad (1.5)$$

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.

1.6 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

²⁵ While 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.

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 evaluation municipalities, villages that received the program with villages that did not. As indicated above, the unit of observation is the precinct.

1.6.1 *CCT impacts across municipalities*

While I am unable to reject the null hypothesis of no effect when comparing incumbent vote share in regular CCT and in evaluation 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 1.4. 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.²⁶

The most conservative estimate indicates that the project increases incumbent vote share in non-dynastic municipalities by 26 percentage points.²⁷ This represents 1.08 standard deviations of incumbent vote share in the non-dynastic sample.²⁸

²⁶ Results obtained using the more conservative randomization inference approach, are consistent with the ones discussed above. I can still reject the null 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 5-7) and 95% (column 8) for the vote margin equation.

²⁷ This 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.

²⁸ I get similar results if I use the change in vote share between the 2007 and 2010 elections for the same candidates as the distribution of interest instead: the effect represents 1.06 standard deviation.

Table 1.4: Impacts of CCT on incumbent vote share (across)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Vote Share			Vote Margin				
Panel A								
Regular CCT	9.369 (16.776)	7.548 (16.033)	14.415 (10.732)	16.895 (11.479)	12.495 (28.443)	10.138 (27.619)	23.457 (17.160)	26.716 (18.939)
Observations	488	488	466	466	488	488	466	466
R-squared	0.401	0.410	0.606	0.617	0.262	0.270	0.534	0.541
Panel B								
A: Regular CCT *	-10.852 (13.352)	-10.564 (13.171)	-5.971 (8.091)	-0.293 (8.035)	-23.429 (24.822)	-22.439 (23.981)	-12.104 (13.468)	-4.672 (14.093)
Dynastic	[13.379]	[13.322]	[10.697]	[10.835]	[25.301]	[24.770]	[18.857]	[19.771]
B: Regular CCT *	26.348 (11.868)**	27.304 (11.597)**	30.999 (9.892)**	34.191 (10.147)***	42.737 (21.365)*	46.065 (21.033)*	52.285 (18.150)**	56.463 (19.958)**
Non-Dynastic	[11.848]**	[12.376]**	[10.784]***	[10.643]***	[21.349]*	[22.520]*	[16.764]**	[16.870]***
Dynastic	34.308 (11.436)**	35.329 (11.539)**	27.344 (9.082)**	31.829 (9.255)***	61.368 (17.530)***	64.905 (18.004)***	46.836 (12.686)***	52.706 (16.392)**
	[11.848] **	[12.376]**	[10.784]**	[10.643]**	[22.406]**	[23.011]**	[19.012]**	[19.422]**
Test $H_0 : A = B$	4.29	4.54	7.47	8.18	3.87	4.27	6.67	6.93
$H_a : A \neq B$ [p-value]	[.05]	[.05]	[.01]	[.01]	[.06]	[.06]	[.02]	[.02]
Observations	488	488	466	466	488	488	466	466
R-squared	0.552	0.555	0.690	0.710	0.427	0.438	0.619	0.631

Notes: Results from precinct-level OLS regressions. The dependent variable is the incumbent mayor's vote share in the May 2010 elections (Columns 1-4) and the incumbent mayor's vote margin (in percent) in the May 2010 elections (Columns 5-8). 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 and 6-8), for the 2007 winner's vote share at the precinct-level (Columns 3-4 and 7-8) and, for per capita fiscal transfers (Columns 4 and 8).

While the effect might appear large it is consistent with evidence from other contexts. First, other studies find large impacts of targeted government transfers on support for the government that implemented them. For example, Manacorda, Miguel, and Vigorito (2011) find that beneficiaries of a temporary CCT program in Uruguay were 11 to 13 percentage-points more likely to report favoring the government. This translates into 0.73 standard deviations of their outcome of interest. While the transfer in the Uruguyan program represented a larger share of pre-program per capita income (50 percent versus 24 percent in the Philippines) it only lasted a year. Similarly, Pop-Eleches and Pop-Eleches (2012), find that households in Romania that received a one-time subsidy, worth 25 percent of annual income, to purchase a computer were 15.5 percentage-points more likely to report planning to vote for the government. It is important to keep in mind that both studies use regression discontinuity designs around eligibility thresholds and, as a result, they are likely to report lower bound estimates. Indeed, their estimation sample consists of the richest households among the eligible; *i.e.* those for which the transfer represented the lowest share of pre-program income.

Second, the estimates are close to 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 regular CCT and evaluation CCT municipalities is about PHP 14.1 million, one would expect a 21 percentage point increase as a result of the project.

I now provide evidence that results are unlikely to be driven by pre-study imbalance between the two types of municipalities in the dynastic and non-dynastic samples. First, I plot the 2007 precinct-level distribution in winner vote share in regular CCT and evaluation CCT non-dynastic municipalities (Figure 1.2). I also

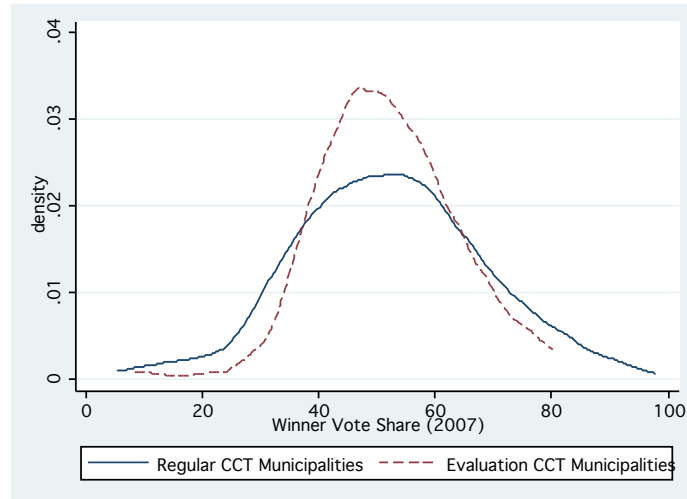


Figure 1.2: Precinct-level distribution of winner vote share in 2007 in non-dynastic municipalities

test whether there was any difference in the winner’s vote share between the two sets of municipalities in 2007 in both dynastic and non-dynastic municipalities. I carry out tests of equality of means at both the precinct- and municipal-level. Overall, results suggest that the sample is balanced in both sets of municipalities (cf. Table A1.2). For example, among the non-dynastic subsample, the 2007 winner’s vote share was 51.6 percent in half CCT municipalities and 52.1 in regular CCT municipalities. The differences are not different from zero at the usual levels of confidence ($t\text{-stat} = 0.27$, $p\text{-value} = 0.78$).

Second, I estimate equation (1.2) but use the 2007 winner’s vote share as the dependent variable. Results, available in Table A1.3, indicate that pre-study vote share was orthogonal to treatment in both the dynastic and non-dynastic sample.

In addition, I provide evidence that results are unlikely to be driven by outliers. First, I compare the distributions of changes in incumbent’s vote share between 2007 and 2010 in the regular and evaluation CCT municipalities in the sample of non-dynastic municipalities. Results displayed in Figure 1.3 do not seem to suggest that results are driven by outliers. The whole distribution appears to have shifted to the left.

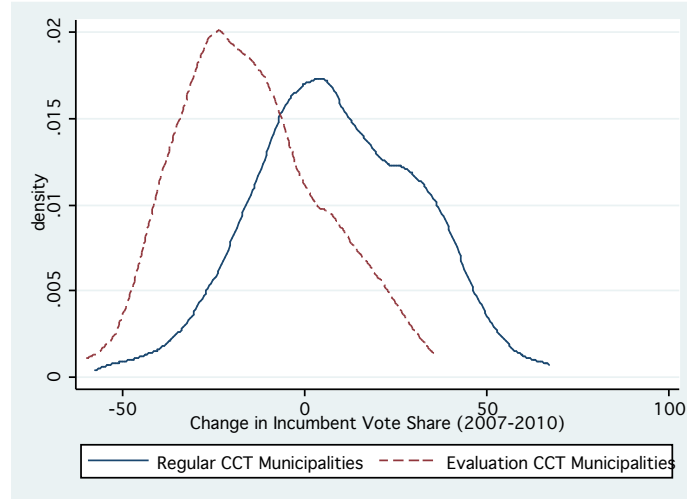


Figure 1.3: Precinct-level distribution of changes in incumbent's vote share between 2007 and 2010 in non-dynastic municipalities

Second, I re-estimate equation (1.2) 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.

Third, I re-estimate equation (1.2) on a number of sub-samples where I exclude the top and bottom one, five, ten and fifteen percent in the relevant distribution. Results for the four samples are available in Table A1.4. The size of the coefficients are stable and the standard errors are more precisely estimated which suggest that the size of the estimates are not driven by a few outliers.

As an additional robustness test, I take advantage of the availability of winner's vote share in the 2007 elections to estimate equation (1.2) through fixed-effects. I do so on the full sample and, as above, on the four samples where I exclude the top and bottom one, five, ten and fifteen percent in the relevant distribution. Results, available in Table A1.5, are robust to this alternative estimation strategy.²⁹ The point estimates are of a similar order of magnitude as the ones obtained previously and, again they are robust to the exclusion of outliers.

²⁹ In the pre-study period, vote counting used to take a few weeks and tended to be very controversial. This led to the introduction of electronic voting in 2010. As a result, I did not carry out the

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 (1.3) where Z_k is the winner's vote share in 2007. Results are available in Table 1.5. 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.

main analysis in a panel data set-up. This set of results should only be thought of as robustness checks.

Table 1.5: Is the impact decreasing with 2007 winner's share vote?

	(1)	(2)	(3)	(4)
Panel A- Vote Share				
Regular 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]*
Regular 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				
Regular 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]*
Regular 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 precinct-level 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 fiscal transfers (Column 4).

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 (1.3) and results are available in Panel A of Table 1.6. As would be expected given our previous results, I find no impact on the full sample and also report results on the sample of non-dynastic municipalities. Results are available in Panel B of Table 1.6. 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 (1.2) 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 1.7. As expected, results are robust to the inclusion of those additional control variables. Neither the size of the es-

Table 1.6: Is the impact on vote share decreasing with per capita budget?

	(1)	(2)	(3)
Panel A- Full Sample			
Regular 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]
Regular 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			
Regular CCT	0.380 (3.754) [12.955]	-1.732 (3.894) [11.825]	0.548 (4.083) [13.279]
pc Fiscal Transfers (IRA)	0.050 (0.010)** [0.019]*	0.055 (0.007)** [0.017]*	0.054 (0.008)** [0.019]
Regular 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 precinct-level 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).

estimated coefficients nor the standard errors change significantly which increase confidence in the results. The results should be interpreted with caution as out of the 17 incumbents in the sample, nine changed party affiliation between the 2007 and the 2010 elections : three joined the President's party, five left the President's party and one moved between two parties not affiliated with the President. As a result, I do not attempt to estimate coattail effects directly.

Table 1.7: Is the impact robust to controlling for party affiliation?

	(1)	(2)	(3)	(4)	(5)	(6)
	Congress			President		Governor
Panel A- Vote Share						
Regular CCT * Dynastic	-10.245 (12.751)	-2.950 (7.745)	-2.345 (16.888)	-7.001 (11.400)	-1.777 (14.439)	-2.789 (10.995)
	[13.974]	[10.759]	[18.503]	[15.215]	[15.630]	[12.286]
Regular CCT *Non-Dynastic	26.100 (12.277)*	35.581 (10.316)***	25.850 (11.850)*	34.989 (10.324)***	23.364 (13.712)	35.960 (11.474)**
	[11.411]**	[8.971]***	[11.161]**	[9.130]***	[11.280]*	[10.092] ***
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						
Regular CCT * Dynastic	-18.748 (23.947)	-6.547 (15.126)	1.732 (30.280)	-6.820 (22.393)	-14.858 (27.642)	-16.756 (17.517)
	[26.096]	[20.300]	[34.220]	[28.355]	[30.372]	[21.271]
Regular CCT * Non-Dynastic	40.824 (22.239)*	57.443 (20.950)**	41.266 (21.117)*	56.718 (20.775)**	39.919 (24.329)	65.023 (19.165)***
	[21.310]*	[16.925]***	[20.641]*	[17.014]***	[21.918]*	[17.473]***
Observations	488	466	488	466	488	466
R-squared	0.436	0.632	0.450	0.631	0.433	0.644

Notes: Results from precinct-level 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 and a dummy for whether the municipality is classified as dynastic. 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 fiscal transfers (columns 2, 4 and 6).

1.6.2 CCT impacts within municipalities

Within evaluation municipalities, incumbent vote share and vote margin are no higher in villages that received the program than in control villages (cf. Panel A of Table 1.8). 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. The point estimates are very small, about 0.6 percentage points in the vote share equation and with standard errors of 1.49, I am able to reject the null hypothesis that the effect is larger than 3.6 percent. This provides further evidence that the coefficients are estimated precisely. 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 and 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.³⁰

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 of Table 1.8). The estimates in the vote share equation are not significantly different from zero at the usual levels of confidence.³¹ 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.

³⁰ Results are not reported but are available upon request.

³¹ One 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.

In addition, I estimate equation (1.4) 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 least 5 of the 6 local elections between 1992 and 2007. Results are available in Table 1.8. 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.

Table 1.8: Impacts of CCT on incumbent vote share (within)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full Sample			Dynastic Municipalities			Non-Dynastic Municipalities		
Panel A - Share Vote									
CCT	0.654 (1.492)	0.667 (1.483)	0.632 (1.496)	-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 (village)		-0.069 (0.057)	-0.144* (0.080)		-0.041 (0.053)	-0.052 (0.064)		-0.074 (0.135)	-0.305* (0.172)
Registered voters		-0.008** (0.004)	-0.008* (0.004)		-0.010*** (0.004)	-0.010*** (0.004)		-0.004 (0.008)	-0.004 (0.007)
CCT *			0.141 (0.087)			0.021 (0.068)			0.470** (0.230)
Observations	222	222	222	117	117	117	105	105	105
R-squared	0.897	0.900	0.901	0.949	0.951	0.951	0.576	0.579	0.602
Panel B - Vote Margin									
CCT	0.031 (3.443)	0.085 (3.415)	-0.002 (3.419)	-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 (village)		-0.168 (0.122)	-0.353** (0.141)		-0.074 (0.106)	-0.121 (0.098)		-0.246 (0.268)	-0.766** (0.305)
Registered voters		-0.010 (0.008)	-0.010 (0.008)		-0.016* (0.009)	-0.016* (0.009)		-0.003 (0.015)	-0.001 (0.013)
CCT *			0.349** (0.173)			0.088 (0.137)			1.058** (0.448)
Observations	222	222	222	117	117	117	105	105	105
R-squared	0.826	0.829	0.831	0.901	0.904	0.904	0.259	0.268	0.318

Notes: Results from precinct-level 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.

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 (1.5) are available in Table 1.9. 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. In the sample, 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.

Table 1.9: 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 precinct-level 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).

1.7 CONCLUSION

Using precinct-level electoral results data from the May 2010 municipal elections in the Philippines and a downstream analysis of 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 suggest that, in competitive political environments, incumbent vote share is 26 percentage points higher in municipalities in which the CCT program was implemented in all villages than in municipalities in which the program was only implemented in half of the villages. This large effect could be a result of the weakness of political parties in the Philippines, with candidates campaigning on their ability to secure resources from higher levels of government. Pop-Eleches and Pop-Eleches (2012) mention that weak partisan ties of Romania's nascent party system might explain the large effect of a one-time subsidy on support for the government. In settings with more established parties, 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 (forthcoming) 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.

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. Expectations of reciprocity can fuel the use of patronage politics and vote buying (Schaffer (2007) and Finan and Schechter (2012)). 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, Chua, Rimban, and Cruz 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 strengthens incumbency advantage. If poorer districts also have poorly performing local officials then it might have nefarious long-term consequences in those areas. 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 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, both Angelucci and De Giorgi (2009) and Bobonis and Finan (2009) showed that PROGRESA beneficiaries are affecting 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. 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.

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APPENDIX: ADDITIONAL RESULTS

Table A1.1: List of sample municipalities

Name	Regular CCT	Dynastic
Ayungon	Yes	No
Basay	No	No
Jimalalud	No	Yes
Kapatagan	Yes	No
Lala	No	No
Magsaysay	Yes	Yes
Munai	Yes	No
Nunungan	Yes	Yes
Paluan	No	Yes
Paracelis	No	Yes
Poona Piagapo	Yes	Yes
Sadanga	No	No
Salvador	No	Yes
Sapad	Yes	Yes
Sta Cruz	No	No
Tangcal	Yes	No
Tayasan	Yes	No

Notes: 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.

Table A1.2: Comparing 2007 winner vote share in regular CCT and evaluation CCT municipalities

	Evaluation CCT (1)	Regular CCT (2)	T-test (3)
Panel A: Precinct-level analysis			
Dynastic	62.983 (29.601)	69.032 (22.772)	1.65 [0.099]
Non-dynastic	51.62 (11.957)	52.152 (16.263)	0.27 [0.780]
Panel B: Municipal-level analysis			
Dynastic	64.596 (25.141)	67.818 (15.636)	0.22 [0.834]
Non-dynastic	47.116 (12.556)	53.203 (10.373)	0.80 [0.451]

Notes: The standard deviations are in (parentheses) (Column 1-2). In Column 3, the test statistics are reported along with the p-values [bracket].

Table A1.3: Pre-study differences in voting behavior

	(1)	(2)	(3)
A: Regular CCT * Dynastic	-15.578 (11.350) [8.973]	-15.561 (11.338) [8.999]	-10.426 (11.317) [9.265]
B: Regular CCT * Non-Dynastic	-0.910 (4.782) [7.657]	-1.250 (4.944) [8.242]	1.375 (5.503) [7.998]
Dynastic	24.951 (7.867)*** [8.251]***	24.785 (8.114)*** [8.630]***	27.540 (8.362)*** [8.375]***
Observations	468	468	468
R-squared	0.363	0.363	0.385

Notes: Results from precinct-level OLS regressions. The dependent variable is the winner's vote share in the May 2007 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 10 degrees of freedom (Columns 1-2) and 9 degrees of freedom (Column 3). 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 per capita fiscal transfers (Column 3).

Table A1.4: Are results robust to exclusion of outliers?

	(1)	(2)	(3)	(4)
	1%	5%	10%	15%
Panel A - Vote Share				
A: Regular CCT * Dynastic	-9.367 (13.371) [13.293]	-6.124 (12.906) [12.145]	-6.617 (11.992) [11.130]	-3.199 (10.473) [10.374]
B: Regular CCT * Non-Dynastic	26.661 (11.611)** [11.110]**	25.510 (10.279)** [10.111]**	24.956 (9.461)** [9.118]**	21.431 (8.585)** [8.312]**
Dynastic	33.397 (11.063)** [11.703]**	31.158 (9.470)** [10.675]**	31.588 (8.765)** [9.675]**	28.120 (8.413)** [8.869]**
Observations	479	439	391	341
R-squared	0.534	0.459	0.382	0.346
Panel B - Vote Margin				
A: Regular CCT * Dynastic	-22.307 (24.498) [25.153]	-12.616 (20.993) [23.197]	-11.359 (18.251) [20.790]	-8.305 (15.681) [18.673]
B: Regular CCT * Non-Dynastic	41.962 (20.468)* [20.856]**	41.473 (18.300)** [18.148]**	40.013 (16.052)** [15.615]**	33.409 (13.791)** [14.139]**
Dynastic	58.405 (16.503)** [22.148]**	55.046 (14.593)** [19.861]**	52.706 (12.262)** [17.262]**	46.460 (10.555)** [15.342]**
Observations	479	439	391	341
R-squared	0.428	0.467	0.474	0.437

Notes: Results from precinct-level 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. All regressions include province dummies. I exclude the top and bottom precincts in the distribution of change in the vote share between the 2007 and 2010 elections: 1 percent (Column 2); 5 percent (Column 3); 10 percent (Column 4); 15 percent (Column 5).

Table A1.5: Are results robust to a panel data set-up?

	(1)	(2)	(3)	(4)	(5)
A: Regular CCT	9.415	9.917	9.571	9.866	10.071
* Dynastic	(8.608)	(8.294)	(7.540)	(6.286)	(5.497)*
	[8.925]	[8.665]	[7.703]	[6.405]	[5.432]*
B: Regular CCT	23.382	23.761	23.322	23.213	22.083
* Non-Dynastic	(10.472)**	(10.236)**	(9.059)**	(7.479)***	(6.378)***
	[7.595]***	[7.418]***	[6.645]***	[5.570]***	[4.736]***
# of Precincts	466	458	422	374	326
R-squared	0.297	0.311	0.359	0.402	0.435

Notes: Results from precinct-level fixed-effects regressions. The dependent variable is the candidate's vote share in the municipal 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 16 degrees of freedom. All regressions include Province*Year dummies. The full sample is used in Column 1. In the remaining columns, I exclude the top and bottom precincts in the distribution of change in the vote share between the 2007 and 2010 elections: 1 percent (Column 2); 5 percent (Column 3); 10 percent (Column 4); 15 percent (Column 5).

APPENDIX: 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.

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 (2012), 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 $\bar{u}$.³² 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 (forthcoming) 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 .*

³² While 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.

PROOF. 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 $\frac{\partial p^*}{\partial g} > 0$. As expected, incumbents benefit 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.³³ 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. The incumbent's objective function becomes:

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

³³ See Moffitt (1989) for a similar discussion in the case of food stamps.

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. 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.

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.

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. This could also explain why incumbents engage in credit claiming as it represents a low-cost way of increasing vote share.

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

PROOF. 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, $w + t_r^* = t_n^*$

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. 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.

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 than recipients. 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.

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.³⁴

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. 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.

³⁴ In 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.

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. 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 .

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.

LOCAL POLITICAL BUSINESS CYCLES: EVIDENCE FROM PHILIPPINE MUNICIPALITIES

2.1 INTRODUCTION

In this paper I examine whether employment levels are affected by election timing. Economists and political scientists have been interested in analyzing so-called political business cycles, the fluctuations of employment around elections, and political budget cycles, the expected increase in government expenditures before elections, as they provide insights into voter and politician behavior.¹ To date, the empirical evidence is not as strong as theory and anecdotal evidence would suggest.²

I am grateful to Marcel Fafchamps, Simon Franklin, Clement Imbert and Simon Quinn for useful discussions while working on this paper. APPC kindly shared their electoral data. Financial support from the CSAE and Oxford Economic Papers Fund is gratefully acknowledged. I thank Jacobus Cilliers, Paul Niehaus, Yasuhiko Matsuda and participants in the CSAE Research Workshop, CSAE Conference 2013 and UPSE Friday Seminar for comments. All remaining errors are mine.

- ¹ Recognizing that voters use retrospective information about the economy to decide whether to re-elect the incumbent, Nordhaus (1975) developed a model where politicians have incentives to decrease unemployment and increase inflation ahead of elections. While the model assumed myopic voters, it can be modified to incorporate rational agents. For example, shifting the focus from employment to budget spending, Rogoff (1990) proposed a model where it is optimal for voters to use information about public spending when deciding whether to re-elect the incumbent or not as it provides information about private incumbent's type. If voters observe an increase in public spending they rationally attribute some of the improvements to incumbent's actions.
- ² Political business and budget cycles have been identified in some countries but not in others (Peltzman (1992); Franzese (2002); Besley (2006); Shi and Svensson (2006)). More recently, the empirical literature has moved from cross-country to subnational analyses and has tested whether local governments spending follow a different pattern in election years. Evidence from countries as diverse as Argentina, Brazil, India, Indonesia, Israel, Portugal and Russia suggests that

A number of explanations have been proposed for the challenges in identifying political budget and business cycles in some contexts and for the weakness of the estimated effects when cycles are identified. Researchers have argued that cycles are a function of, among other, the structure of fiscal federalism (Jones, Meloni, and Tommasi 2012), institutional constraints on politicians (Shi and Svensson 2006) and, the degree of control politicians have over the economy (Duch and Stevenson 2008). To take one example, Duch and Stevenson (2008) argue that in an open economy politicians have less control over the state of the economy and as such it provides a weaker signal of incumbent quality. Voters are more reluctant to use the signal when deciding whether to re-elect the incumbent and, in turn, she has less incentives to attempt to improve the economy before elections. Similarly, Jones, Meloni, and Tommasi (2012) show that in a decentralized setting increased spending act as signal for the incumbent's ability to extract resources from the center.

In this paper, I test for political business cycles in Philippine municipalities over the period 2003-2009, a context where according to the literature strong cycles are likely to be observed. First, local politicians often campaign on their ability to secure resources from higher levels of government. Second, Philippine municipalities are often headed by strong mayors with significant discretionary powers over budget spending. Third, mayors sometimes act as employment brokers in both the public and private sectors.

I find robust evidence for the presence of political business. This effect is only present when I use quarterly data and vanishes when I aggregate the data at

local governments increase spending and/or reduce taxes before elections (Akhmedov and Zhuravskaya (2004); Khemani (2004); Brender (2003); Jones, Meloni, and Tommasi (2012); Sakurai and Menezes-Filho (2010); Aidt, Veiga, and Veiga (2011), Sjahrir, Kis-Katos, and Schulze (2013)). In addition, elections appear to affect spending composition, with local governments allocating more resources to visible investments (Drazen and Eslava 2010). This is consistent with Rogoff (1990)'s prediction that, ahead of elections, there is a re-allocation of government spending towards *easily observable expenditures*. More recently electoral cycles have also been identified in foreign aid (Faye and Niehaus 2012), in cement consumption in India (Kapur and Vaishnav 2011) and in prices paid to farmers for cane (Sukhtankar 2012).

the yearly-level. The difference is not merely driven by a decline in statistical power due to aggregation: point estimates for the overall effects are 7 times larger when I use quarterly data than when I use yearly data. This discrepancy can be explained by a drop in employment post-election that dilutes the yearly effects. A potential explanation for this decline is that, in situations where local governments are unable to borrow, local incumbents can increase spending ahead of elections by shifting some of their planned post-election spending in the pre-election period.³ This within-year effect is not captured by analyses using aggregated data.

The main findings can be summarized as follows. Using a unique balanced panel dataset of about 1,140 cities and municipalities, I show that the share of working-age population that is employed increases by 0.87 percentage-points in the two quarters before elections. This is equivalent to an additional 470,000 jobs in April 2004 compared with what employment would have been without the May 2004 elections. Employment as a share of the working-age population in the two post-election quarters is 0.48 percentage-point lower than it would have been without the elections. Results are robust to the inclusion of a number of control variables, time trends and to two-way clustering of the residuals along both time and geographic dimensions. In addition, results are similar when I control for lagged values of the dependent variables and estimate the model using the generalized methods of moment.

The paper provides a simple explanation for the challenges in identifying political business cycles when using yearly data and for the weakness of the estimated effects when cycles are identified. In addition, the paper expands the literature along three main dimensions. First, I return to the literature's origin and carry out one of the first political business cycle analyses at the subnational

³ Importantly, in the Philippines, the calendar and fiscal years coincide.

level.⁴ Indeed, to-date subnational analyses have focused on budget cycles, capturing only part of the potential distortions to labor markets. In contexts where local politicians have either significant business interests and/or have strong connections with local businessmen, incumbents might be able to influence employment levels above and beyond what one would expect from budget spending alone. For example, Bertrand, Kramarz, Schoar, and Thesmar (2007) provide convincing evidence that, in France, firms with political connections tend to delay firing employees until after the elections.

Second, I focus on a setting where election timing is exogenous to the outcome of interest. Strategic election timing is a common concern in the literature but, in the Philippines, it was decided as part of the 1987 Constitution and since then all elections have been organized as planned. Put differently, politicians are *de facto* unable to organize elections in good years.

Third, results discussed in this paper contribute to the literature on clientelism. The increase in employment in the public sector before the elections is concentrated on long-term contracts (*i.e.* non-casual). This is in agreement with the model of clientelism developed by Robinson and Verdier (2013) as it allows politicians to align bureaucrats' incentives with their own electoral objectives as their job tenure is implicitly tied to the incumbent's electoral success. The size of the effect suggests that incumbents are not merely providing jobs to secure additional votes. The observed effects do not appear large enough to affect election results. Rather, results are consistent with the argument that incumbents attempt to provide their constituents with benefits that will last until after the elections. The increase in employment in the construction sector is expected to lead to an increase in either maintenance of existing public goods or the construction of new infrastructure.

⁴ To the best of my knowledge, the only exceptions are Coelho, Veiga, and Veiga (2006) who test for political business cycles in Portuguese municipalities, using yearly data, over the period 1985-2000 and Dahlberg and Mork (2011) who test for electoral cycles in public employment in Finnish and Swedish municipalities, using yearly data, over the period 1985-2002.

Results presented in this paper suggest that, to the extent possible, future analyses of political business cycles should use monthly or quarterly data. A similar point is implicit in results presented by Akhmedov and Zhuravskaya (2004) for the analysis of political budget cycles, but it appears that data constraints have prevented researchers from doing so.

The remainder of the paper is organized as follows. Section 2.2 provides some background on local elections in the Philippines. Section 2.3 introduces the data. The estimation strategy is presented in Section 2.4 and results are discussed in Section 2.5. Section 2.6 concludes.

2.2 SETTING

In this section, I briefly present the local political context, highlighting factors that make Philippine municipalities particularly well-suited to study political business cycles, especially over the period 2003-2009. I start with a brief description of the institutional context, followed by a discussion of local elected officials' behavior and ability to affect local labor markets.

First, carrying subnational analyses allows me to control for the specificities of the institutional context. As pointed out by Drazen and Eslava (2010) in their study of the effects of elections on local government spending in Colombia, variations in institutions make the interpretation of cross-country regressions challenging. This is especially relevant over the period 2003-2009 in the Philippines as the same president, Gloria Macapagal-Arroyo, was in office throughout. Unobserved links with central government officials which could affect local politicians' ability to access state resources will be stable throughout the period and can thus be controlled for by using municipal fixed-effects.

Second, since the fall of Marcos elections have followed a pre-established calendar set out in the 1987 Constitution. This rules out concerns that election timing

was endogenous to the outcome of interest as local governments were unable to control election timing. The literature has recognized that most governments have some control over election timing and that observed correlations between elections and the outcome of interest could be driven by incumbents ability to time elections as they please (Khemani 2004, Shi and Svensson 2006). To deal with those concerns, researchers often provide evidence that results are robust to restricting the sample to elections that were implemented according to the constitutionally mandated schedule. The idea being that in such cases the timing of elections is exogenous to the outcome of interest. However, if incumbents have the power to either call for elections early or postpone them and have used it in the past, then the decision not to do so might be correlated with the outcome of interest as well. As a result, the assumption that election timing is exogenous is more likely to be valid when local politicians are *de facto* unable to affect when elections take place; as is the case in the Philippines.

Third, Capuno (2012) and Sidel (1999) provide credible evidence that municipalities are headed by strong mayors, often referred to as local bosses and perceived as such by the population. One would expect economic voting to be especially strong in such a setting. Duch and Stevenson (2008) argue that economic voting is stronger when voters perceive incumbents to be in control of the local economy. When voters expect elected officials to control the local economy, they are more likely to use the state of the economy as a signal of incumbent's quality. Incumbents therefore have more incentives to distort the economy ahead of the elections.

As shown by Hutchcroft (2012), most decisions regarding municipal budgets are made by mayors who use available funds with very little oversight. This is despite the fact that the 1991 Local Government Code established municipal councils and gave them decision-making powers. Mayors control both how the budgets are spent and public sector employment (Hodder 2009, Hutchcroft 2012).

In addition, there is evidence consistent with the argument that they are able to staff the bureaucracy with their relatives (Fafchamps and Labonne 2013); thus ensuring that bureaucrats have incentives closely aligned with their electoral objectives (Robinson and Verdier 2013). In clientelistic systems where local elected officials are often assessed on their ability to respond to citizens' requests, one would expect incumbents to use their discretionary powers more frequently ahead of elections. For example, in a case study of Naga City in the Philippines, Kawanaka (2002) notes that *resident requests greatly increase as elections near. The city government intensifies its services to reflect favorably on Robredo's [the mayor] leadership.*

Fourth, a number of municipalities are characterized by the presence of family dynasties which have been in power for decades. It is common for one of the incumbent's family member to run for the position when the incumbent has reached the 3-term limit introduced in the 1987 Constitution (Coronel, Chua, Rimban, and Cruz (2004) and Querubin (2011)). The relevant unit of analysis in Philippine politics is the family rather than the individual politician or the political party (McCoy 2009a). As described by Fegan (2009): *A family is a more effective political unit than an individual because it has a permanent identity as a named unit, making its reputation, loyalties, and alliances transferable from members who die or retire to its new standard bearer.* Assuming that citizens learn about politician's quality during their time in office, the set-up allows me to test whether political business cycles are more pronounced in areas where incumbent's quality is more uncertain.

Finally, available qualitative evidence suggests that strong political business cycles are likely to be identified around municipal elections in the Philippines. Incumbents attempt to increase public spending, especially on visible projects, in the pre-election period. This is similar to findings from Colombia where incumbents have been shown to allocate a greater share of their budget to con-

struction projects in election years (Drazen and Eslava 2010). Thus one would expect an increase in employment in the construction sector ahead of the elections. Given that most municipalities face strict budget constraints due to their reliance on fixed fiscal transfers from the central government and their *de facto* inability to borrow, the pre-election increase in spending might be followed by a post-election decline as fiscal resources for the fiscal year have been depleted.

In addition, local incumbents' electoral strategies might affect private sector employment directly. Local politicians often use the power of their office to increase their business holdings (Sidel 1999). They are then able to provide their constituents with jobs. Further, in a number of Philippine municipalities, mayors act as employment brokers, helping their constituents find jobs. For example, in a province surrounding Manila, job applicants in local factories were required to provide letters of recommendation from local officials (Sidel 1999, pp 76-77). There is qualitative evidence that this role intensifies before elections as voters have more bargaining power (Kawanaka 2002).

2.3 DATA

2.3.1 *Employment*

I use data from Labor Force Surveys (LFS) collected by the National Statistics Office of the Philippines. The surveys are implemented four times a year (January, April, July and October) and I have access to all 26 surveys in the period July 2003 - October 2009. Each survey has a sample size of approximately 200,000 individuals in 50,000 households.⁵ Data from the surveys are used to compute official employment statistics. A person is considered employed if he reported

⁵ More information on the survey design is available at: http://www.census.gov.ph/data/technotes/notelfs_new.html visited on March 26, 2012.

at work for at least an hour during the week prior to the survey. In addition, information is collected on the total number of hours worked during the past week, the sector of employment and the daily wage. I use the available data to build a balanced panel of about 1,140 cities and municipalities, out of 1,634 in the country.

For each municipality/survey wave, I compute the share of the working age population (above 15 year old) that is employed. It is not possible to compute employment rate as a share of the economically active population consistently across survey waves as the definition of the economically active population changed in April 2005. The information required to adjust past series is not available. However, the definition of employment has not changed and I compute the employment ratio as a share of the working age population rather as a share of the economically active population. As a result, estimates presented in this paper combine the effects of elections on the decision to enter/exit the labor force and of getting a job for those in the labor force.

2.3.2 *Political environment*

In accordance with the 1987 Constitution, elections have been organized every three years. The two elections of interest in our sample period are the May 2004 and May 2007 elections. I distinguish between pre-election months (January and April waves) and post-elections months (July and October waves).

A number of Philippine municipalities are controlled by so-called political dynasties which have often been in power for decades and I expect political business cycles to be different in those municipalities. To control for that, I use lists of elected officials at the municipal and provincial levels for the period 1987-2007 and compute for the 2004 and 2007 elections, the number of terms

the incumbent's family has been in office in the same municipality since 1988.⁶ I consider that an incumbent is related to an earlier official if they share the same last name.⁷ In addition, for each municipality, I compute the number of family links between the mayor and either the provincial governor, vice-governor or congressmen using the same matching procedure

I also use yearly data on municipal budgets from the Department of Budget and Management.⁸ The data are all expressed in 2000 Pesos using regional Consumer Price Indices.

2.3.3 *Descriptive statistics*

The average municipal employment rate in January and April in election years is 59.6 percent while it is only 58.7 percent in non-elections years (Figure 2.1). Simple tests of equality of means suggest that the observed differences are statistically significant ($t=3.5$, $p\text{-value}=0.0004$). In addition, it appears that not only are the means of the two distributions different but the two distributions are also different. This is confirmed by results from Kolmogorov-Smirnov test of equality of distributions ($p\text{-value}=0.003$).

Overall patterns of employment rates in July and October in election and non-election years are also consistent with the argument that the pre-election increase in employment levels is followed by a post-electoral decline (Figure 2.2). The average employment rate in the July and October waves is 59.3 percent in non-elections years while it is 58.9 percent in elections years. As above, the differences are statistically significant ($t=2.93$, $p\text{-value}=0.0034$) and this is confirmed

6 Municipal elections were organized in 1988. In accordance with transitory provisions of the 1987 Constitution (Article XVIII) the next municipal elections were organized in 1992. Elections have been organized every 3 years ever since.

7 It is possible to do so due to naming conventions imposed by Spanish colonial officials in the 19th century. Querubin (2013) and Fafchamps and Labonne (2013) use a similar strategy with data from the Philippines. I am only able to match on last names and not on middle names and, as such, it is likely that I underestimate links.

8 The data are available from: <http://www.blgf.gov.ph/#> visited on March 26, 2012.

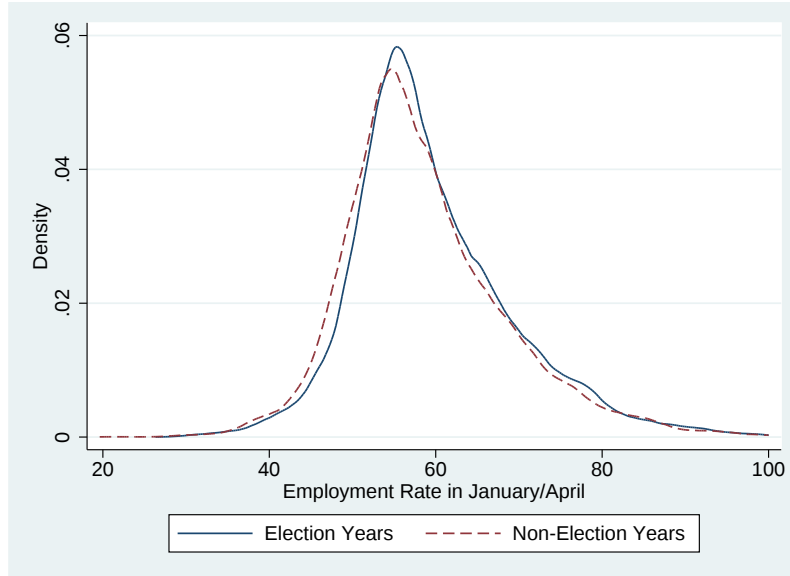


Figure 2.1: Municipal-level employment rate in elections and non-elections years by results from Kolmogorov-Smirnov test of equality of distributions (p-value= 0.013).

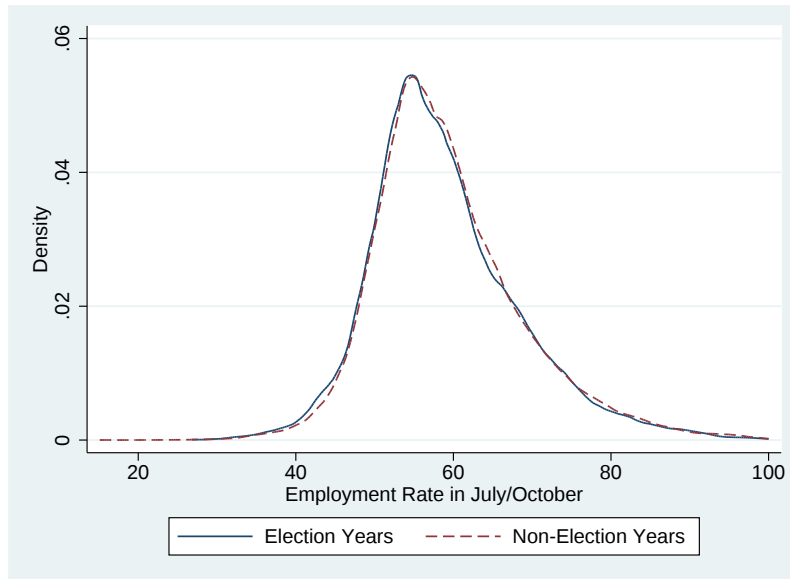


Figure 2.2: Municipal-level employment rate in elections and non-elections years

Despite the prohibition on political dynasties and the three term limit that were introduced in the 1987 Constitution, a number of mayors come from families that have been in power more than three times since 1987.⁹ About 9.1 percent of mayors elected during the 1998 elections were from families that had been elected at least 3 times since 1987. The numbers then increased to 22.9 percent in 2001, 30.0 percent in 2004 and to 36.2 percent 2007. Such families are better able to stay in power. For example, in the 2007 elections, a new family came to power in 44.2 percent of municipalities where the incumbent had been in power three times or less. The rate of turnover was only 26.7 percent in municipalities where the incumbent had been in power four times or more. The difference between the two groups is statistically different from zero at the usual levels of significance ($\chi^2 = 30.4$). While such descriptive statistics are interesting, they do not provide credible estimate of incumbency advantage as they could merely reflect a selection effect with more competent political families more likely to be elected to begin with.

⁹ This is consistent with findings at the provincial and congressional levels reported by Querubin (2011).

Table 2.1: Descriptive statistics

	(1)	(2)
	Mean	Std. Dev.
Share working-age population with a job in:		
Overall	59.14	(9.57)
Public Sector	4.61	(3.15)
Private Sector	54.53	(10.14)
Public Sector (short-term)	0.55	(0.91)
Public Sector (long-term)	4.06	(2.88)
Private Sector (short-term)	44.34	(11.25)
Private Sector (long-term)	10.19	(7.96)
Construction (short-term)	5.05	(3.32)
Other variables:		
No Education	2.16	(4.60)
Some Primary	13.94	(10.86)
Primary Graduate	14.50	(7.73)
Some Secondary	16.93	(5.53)
Secondary Graduate	24.27	(9.34)
Some College +	28.19	(13.68)
Population	82,347	(207,659)
Share female	0.50	(0.04)
Age	35.82	(2.23)
Observations	29,715	

As expected, incumbent political families that have links with provincial politicians are more likely to be re-elected to municipal office. For example during the 2004 elections, a new family came to power in only 17.3 percent of the municipalities where the incumbent family had links with provincial politicians. In municipalities with incumbent without such links, the proportion was 28.0 percent. The difference was even stronger in 2007, with a new family coming to power in 24.2 percent of the municipalities with provincial links and in 39.8 percent of municipalities with politicians without family links. In both cases, the differences are significantly statistically different from zero at the usual levels of significance (for 2007: $\chi^2 = 6.4$). Again, the statistics discussed above do not provide any evidence on the value of connections as competent political families might be better able to be elected to provincial office.

2.4 ESTIMATION STRATEGY

In this section, I present the empirical strategy. First, I describe how I test for the presence of political business cycles, the fluctuations of employment around elections, including various robustness checks. Second, I discuss channels that could explain the strength of political business cycles. Finally, I test for the presence of political budget cycles, the expected increase in government expenditures before elections.

2.4.1 *Local political business cycles*

I start by estimating equations of the form:

$$Y_{ijt} = \alpha E_t + \beta X_{ijt} + u_{ij} + w_{ijt} \quad (2.1)$$

Where Y_{ijt} is the outcome of interest in municipality i in province j at time t , E_t is a vector of electoral variables, X_{it} is a vector of municipal characteristics that vary across time, u_{ij} is a municipality-specific unobservable and, w_{ijt} is the usual idiosyncratic term. Each observation is weighted using the sum of individual survey weights in the municipality at that time period. I also present results from unweighted regressions (Solon, Haider, and Wooldridge 2013).

I estimate equation (2.1) where Y_{ijt} is either the share of the working age population that is employed, the average number of hours worked over the past week (for those with a job) or the average log daily wage (for those with a job).¹⁰ I estimate equation (2.1) separately for the public and private sector. The differences, if any, will provide information on the channels through which the effects operate.

As discussed in Section 2.2, I argue that election timing in this context is exogenous. Indeed, election timing was decided when the constitution was drafted in 1987 and all elections since then, including the two elections of interest (May 2004 and May 2007), were implemented as planned. To check that the results are not driven by one particular election, I also estimate the model with separate dummies for each election and test whether the coefficients are equal.

In line with the literature, I start by estimating equation (2.1) with annual data. In addition, to test my argument that granularity in data explains variations in electoral cycles, I also estimate equation (2.1) with quarterly data and introduce two dummy variables, one capturing the two pre-election quarters and one capturing the two post-election quarters.

Given the data structure, error terms are not independent and are likely to be correlated both within municipalities, provinces and time periods. Standard errors need to be corrected to account for the specific structure of the error term.

¹⁰ As the surveys were designed to provide representative estimates at the regional level, the municipal-level estimates used in the paper are likely measured with error. As a result, the estimates have larger variances and the null hypothesis of no effect will tend to be under-rejected.

Failure to do so would lead to downward biased standard errors and to over-rejection of the null hypothesis of no effect. For example, in Monte Carlo simulations reported by Cameron, Gelbach, and Miller (2011), the null hypothesis of no effect is rejected twice as often when clustering is done along one dimension as when it is done along two dimensions. When clustering levels are nested, as is the case here with municipalities and provinces, clustering needs to be done at the most aggregated level (Cameron, Gelbach, and Miller 2008). As a result, I use a method developed by Cameron, Gelbach, and Miller (2011) and cluster standard errors across both time and provinces.

The vector X_{ijt} includes controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women in the sample, all computed using the LFS data. I also control for population levels and per capita fiscal transfers. I include either region-specific or province-specific quadratic time trends. Due to the seasonal nature of employment in the Philippines, I include quarter-specific dummies in all regressions. I also estimate equation (2.1) with quarter/municipality fixed effects. This allows me to control for potential variations in the degree of seasonality across municipalities.

I check whether results are robust to alternative specifications and estimation strategies. I assume a dynamic model of the form:

$$Y_{ijt} = \sum_{p=1}^P \alpha_p Y_{ijt-p} + \beta X_{ijt} + u_{ij} + w_{ijt} \quad (2.2)$$

I estimate equation (2.2) for various values of P using the fixed effects estimator. As an additional robustness check, I eliminate u_{ij} by taking the first difference and use lagged values of Y_{ijt} as instruments for ΔY_{ijt-p} . This leads to:

$$\Delta Y_{ijt} = \sum_{p=1}^P \alpha_p \Delta Y_{ijt-p} + \gamma \Delta X_{ijt} + \Delta w_{ijt} \quad (2.3)$$

When using yearly data, I estimate equation (2.2) using the Generalized Methods of Moment with all the available moment conditions (Arellano and Bond (1991) and Blundell and Bond (1998)). However, when using quarterly data, given the length of the panel, using the full set of moment conditions might lead to over-fitting and to possible small sample bias (Roodman 2009). As a result, I only use Y_{ijt-2} and Y_{ijt-3} as instruments for ΔY_{ijt-1} when estimating equation (2.3) with $P = 1$ and Y_{ijt-3} and Y_{ijt-4} as instruments for ΔY_{ijt-1} and ΔY_{ijt-2} when estimating equation (2.3) with $P = 2$.

I then provide evidence on which politicians attempt to increase employment ahead of the elections and which sectors of the economy are the most affected.

First, I expect political business cycles to be stronger in municipalities with relatively inexperienced incumbents as they represent a way of signaling quality to voters. Since voters face difficulties when trying to distinguish between incumbent's ability and the overall economic environment, incumbents try to improve economic conditions ahead of the elections which voters will then partly attribute to the incumbent. Assuming that voters learn from politician's ability during their times in office, one would expect uncertainty about ability to decrease with the number of terms the incumbent, or one of his family member, has been in office. To test whether uncertainty about the incumbent's ability explain the presence of political business cycles, I estimate:

$$Y_{ijt} = \alpha E_t + \beta X_{ijt} + \gamma Z_{ijt} + \delta E_t * (Z_{ijt} - \bar{Z}_{ijt}) + u_{ij} + w_{ijt} \quad (2.4)$$

where Z_{ijt} is a variable capturing the number of times a member of the incumbent's family has been elected mayor since 1987. Since there is no particular reason to expect that the relationship between length in office and the strength of the political cycle is linear, and given the wealth of data available, I also estimate the effect separately for each level of political experience. I am unable to interpret the estimates of δ causally as municipalities that are controlled by dynasties might be different from municipalities with inexperienced incumbents along dimensions that could affect labor markets. However, they provide useful information on the relationship between political experience and political business cycles and they might allow me to rule out alternative explanations as well.

2.4.2 *Local political budget cycles*

In addition, I test for political budget cycles using annual data.¹¹ Specifically, I estimate equations of the form:

$$B_{ijt} = \sum_{p=1}^P \alpha_p B_{ijt-p} + \beta E_t + \gamma X_{ijt} + u_{ij} + w_{ijt} \quad (2.5)$$

Where B_{ijt} is the outcome of interest in municipality i , in province j in year t , E_t is an indicator equal to one if elections took place in year t , X_{ijt} is a vector of municipal characteristics that vary across time, u_{ij} is a municipality-specific unobservable and, w_{ijt} is the usual idiosyncratic term.

The budget data are available for all municipalities and cities in the country over the period 2001-2009. As a result the fixed effects estimator might be biased and I also report results of GMM estimation for $P = 1, 2$. The specification is consistent with previous contributions to the political budget cycle literature (Drazen and Eslava 2010).

¹¹ As such, I am unable to test whether elections affect within-year allocations of resources.

2.5 RESULTS

In this section, I first present results on the presence of local political business cycles in Philippine municipalities. I then highlight potential channels which might affect the strength of the cycles. Finally, I discuss results on whether municipal budgets are affected by the timing of elections.

2.5.1 *Local political business cycles*

Main results

Analyses carried out with yearly data fail to identify political business cycles in Philippine municipalities over the sample period. The proportion of the working-age population that is employed is neither higher nor lower in election years (Panel A of Table 2.2).¹² The conclusions are unchanged when employment in the public and private sectors are analyzed separately (Panels B and C of Table 2.2). To account for the fact that I only have data for the July and October waves in 2003, I re-estimate the model on the 2004-2009 sample. This does not affect the results (Table A2.1)

Once I use quarterly data, I find robust evidence of the presence of political business cycles. The differences with the yearly results are not merely driven by an increase in statistical power but rather by a post-election decline in employment which dilutes the overall effect when using yearly data. Point estimates for the overall effects are 7 times larger when I use quarterly data than when I use yearly data.

¹² When I estimate the model with a separate dummy for each election, I am unable to reject the null that the coefficients are equal ($\chi^2 = 0.56$, p-value=0.454).

Table 2.2: Yearly political business cycles: Employment levels

	(1)	(2)	(3)	(4)
Panel A - All sectors				
Election Year	0.1608 (0.128)	0.1241 (0.135)	0.1209 (0.160)	0.1209 (0.140)
Additional Controls	No	Yes	Yes	Yes
Time Trend	No	Yes	Region	Province
Observations	8,004	7,896	7,896	7,896
R-squared	0.821	0.827	0.842	0.842
Panel B - Public sector				
Election Year	-0.0338 (0.139)	0.0561 (0.052)	0.0591 (0.061)	0.0591 (0.060)
Additional Controls	No	Yes	Yes	Yes
Time Trend	No	Yes	Region	Province
Observations	8,004	7,896	7,896	7,896
R-squared	0.807	0.839	0.847	0.847
Panel C - Private sector				
Election Year	0.1947 (0.179)	0.0680 (0.170)	0.0618 (0.186)	0.0618 (0.190)
Additional Controls	No	Yes	Yes	Yes
Time Trend	No	Yes	Region	Province
Observations	8,004	7,896	7,896	7,896
R-squared	0.831	0.843	0.857	0.857

Notes: Results from fixed-effects regressions. The dependent variable is the yearly average of the share of the working age population with a job in the week before the survey (Panel A), with a job in the public sector in the week before the survey (Panel B) and, with a job in the private sector in the week before the survey (Panel C). Regressions in Columns 2-4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and, per capita fiscal transfers. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

First, results, available in Panel A of Table 2.3, indicate that there is an increase in employment in the two quarters preceding the elections.¹³ Results are robust to the inclusion of a number of control variables and region-specific or province-specific quadratic time trends. The point estimates suggest that the share of the working-age population that is employed increases by 0.87 percentage-points in the two quarters before elections. This is a large effect as it translates into an additional 470,000 individuals employed in April 2004 than would have been the case without the elections. The effects do not appear to be driven by one specific election. When I estimate the model with separate dummies for each election, the 2004 coefficient is 0.77 and the 2007 coefficient is 0.96. The differences are not statistically significant at the usual levels of confidence (p-value=0.75).

Second, results indicate that employment in the two post-elections quarters is 0.48 percentage-points lower than it would have been without the elections. Again, the effects do not appear to be driven by one specific election. When I estimate the model with separate dummies for each election, the 2004 coefficient is -.41 and the 2007 coefficient is -.55. The differences are not statistically significant at the usual levels of confidence (p-value=0.73).

The observed effects are consistent with the argument that they are driven by within-year reallocation of resources. I am unable to reject the null hypothesis that the sum of the coefficients on the pre-election and the post-election dummies is equal to zero ($\chi^2 = 0.97$, p-value=0.325). More specifically, while data constraints prevent me from testing this directly, results are consistent with the view that incumbents shift some of their planned end-of-year budget spending before the elections which would explain the drop in employment in the two

¹³ In some cases, standard errors are 200 percent larger than standard errors obtained without clustering and 100 percent larger than standard errors obtained with one way clustering. This confirms the importance of computing standard errors with two-way clustering. An added complication arises from the fact that I only have 26 time periods and, as a result, standard errors are downward biased and lead to over-rejection of the null hypothesis of no effect (Cameron, Gelbach, and Miller 2008). As I have 6 time invariant regressors, a solution is to use critical values from a T distribution with 20 degrees of freedom. Results are robust to using such critical values.

Table 2.3: Quarterly political business cycles: Employment levels

	(1)	(2)	(3)	(4)
Panel A - All sectors				
Pre-election quarters	0.8949*** (0.268)	0.8725*** (0.281)	0.8740*** (0.276)	0.8760*** (0.300)
Post-election quarters	-0.4619** (0.199)	-0.4843** (0.191)	-0.4859** (0.231)	-0.4864** (0.201)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	29,715	29,283	29,283	29,283
R-squared	0.626	0.636	0.641	0.649
Panel B - Public sector				
Pre-election quarters	-0.0068 (0.077)	0.1483* (0.085)	0.1488* (0.087)	0.1487* (0.086)
Post-election quarters	-0.0909 (0.099)	-0.0011 (0.034)	-0.0017 (0.043)	-0.0018 (0.036)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	29,715	29,283	29,283	29,283
R-squared	0.548	0.610	0.611	0.618
Panel C - Private sector				
Pre-election quarters	0.9017*** (0.270)	0.7242*** (0.275)	0.7252*** (0.277)	0.7273*** (0.281)
Post-election quarters	-0.3711 (0.243)	-0.4832** (0.201)	-0.4843** (0.224)	-0.4846** (0.205)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	29,715	29,283	29,283	29,283
R-squared	0.641	0.661	0.666	0.673

Notes: Results from fixed-effects regressions. The dependent variable is the yearly average of the share of the working age population with a job in the week before the survey (Panel A), with a job in the public sector in the week before the survey (Panel B) and, with a job in the private sector in the week before the survey (Panel C). All regressions include controls for survey quarter. Regressions in Columns 2-4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and, per capita fiscal transfers. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

post-elections quarters. This suggests that the failure to identify political cycles in a number of countries could be partly driven by aggregation issues as the yearly effect captures both the pre-election increase and the post-election decline.

If the argument is correct, the strength of political business cycles detected with yearly data will depend on the timing of elections and the overlap between the fiscal and calendar years. If elections are early in the fiscal year, incumbents might not have enough time to increase spending before elections. Similarly if elections are late in the fiscal year, incumbents do not have enough resources to bring forward before elections. As a result, we would expect the effects to be stronger for elections organized in the middle of the fiscal year. In addition, the larger the overlap between the fiscal and calendar years the more likely is the pre-election increase attenuated with the post-election decrease. Findings from the literature tend to support this argument. For example, Coelho, Veiga, and Veiga (2006) who test for political business cycles in Portuguese municipalities, using yearly data, over the period 1985-2000. In Portugal, elections take place at the end of the fiscal year which coincides with the calendar year and, as such, effect sizes are expected to be small. The estimated effects indicate that total municipal employment increases by 2.5 jobs in election years (Coelho, Veiga, and Veiga 2006). In a similar context, Dahlberg and Mork (2011) find that, in Sweden and Finland there are 0.6 more full-time public employees per 1000 capita in election years. For the average municipality in Sweden, it translates into 15.6 additional public-sector jobs.

I further test whether the cycles are present in both public sector and private sector employment. Results, available in Panels B and C of Table 2.3, indicate that most of the variations are concentrated in the private sector. Elections lead to a 0.15 percentage-point increase in public sector employment and to a 0.73 percentage-point increase in private sector employment in the two pre-election

quarters. That is, 83 percent of the increase in employment levels occurs in the private sector. Given that only 7.8 percent of those employed work in the public sector, the relative increase is larger in the public sector than it is in the private sector. Specifically, the public sector effect represents about 3.3 percent of the mean value of public sector employment while the private sector effect represents only about 1.3 percent of the mean value of private sector employment. There is no evidence of a decline in public sector employment in the two post-elections quarters (point estimate: -0.002) but private sector employment is 0.48 percentage-points lower than it would be if elections did not take place.

Interestingly, there is no evidence that elections affect either the number of working hours or wage for employed individuals (Tables [A2.2](#) and [A2.3](#)). This suggests that, along those two dimensions, jobs created during the political business cycles are neither superior nor inferior to average jobs in the municipalities. They provide similar working hours and similar pay.

Channels and robustness checks

A potential concern with the above results is that they might simply be capturing the fact that elections require labor which would automatically lead to a pre-election increase; not related to political business cycles. Evidence discussed above seems to suggest that this is not case. First, the increase is larger for private sector employment than it is for public sector employment.

Second, as a further check, I use data on the type of employment contracts to compare the effects of elections on public sector employment in long-term (*i.e.* non-casual) and short-term (*i.e.* casual) contracts. If the results were driven by hiring of election workers, we would expect an increase in employment on short-term contracts but no increase in employment on long-term contracts. Available results suggest that there is an increase in public sector employment on long-term contracts but no effect on employment on short-term contracts in the two

Table 2.4: Quarterly political business cycles: Channels

	(1)	(2)	(3)	(4)	(5)
	Public		Private		Construction
	ST	LT	ST	LT	ST
Pre-election quarters	0.0276 (0.048)	0.1211** (0.053)	1.5826* (0.847)	-0.8553 (0.803)	0.2035* (0.119)
Post-election quarters	0.0334 (0.048)	-0.0352 (0.044)	-0.4466 (0.651)	-0.0380 (0.767)	-0.1039 (0.092)
Observations	29,283	29,283	29,283	29,283	29,283
R-squared	0.270	0.595	0.412	0.550	0.337

Notes: Results from fixed-effects regressions. The dependent variable is the share of the working age population with a short-term job in the public sector in the week before the survey (Column 1) with a long-term job in the public sector in the week before the survey (Column 2), with a short-term job in the private sector in the week before the survey (Column 3), with a long-term job in the private sector in the week before the survey (Column 4) and with a short-term job in the construction sector in the week before the survey (Column 5). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population per capita fiscal transfers, a dummy for whether or not the previous municipal election led to a change in local leadership and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

pre-elections quarters (Columns 1 and 2 of Table 2.4). Again, these results are not in agreement with the view that effects are driven by hiring of election workers. In addition, this increases the likelihood that local bureaucrats' incentives are aligned with the incumbent's electoral objectives as their job tenure is implicitly linked with the incumbent's electoral success (Robinson and Verdier 2013, Iyer and Mani 2012).¹⁴

Further, the scale of the increase is difficult to reconcile with the number of jobs that are actually needed to organize the elections. Taking 2004 as an example, the

¹⁴ As cited by Fafchamps and Labonne (2013), Hodder (2009) quotes a lawyer for the Civil Service Commission: *We can even go so far as saying that you cannot be appointed in local government if you do not know the appointing authority or, at least, if you do not have any [political] recommendation....And even once in place, the civil servant's position is not secure: when the new mayor [comes], he just tells them 'resign or I'll file a case against you.'*

point estimates suggest that pre-election increase in employment represents 1.1 percent of the population that was registered to vote. It seems unlikely that so many individuals would have to be hired given that a number of the required activities are carried out by existing civil servants, usually public school teachers, and do not require additional hiring. In addition, the last pre-election surveys are carried out about a month prior to the elections, that is before the final few weeks of campaigning and election day which are likely to be the most labor-intensive.

In light of evidence from other countries (Drazen and Eslava 2010), incumbents might attempt to increase both the levels of government spending and to target spending on visible projects ahead of elections. Given that in the Philippines most government-financed construction work is done through private contractors, one would expect the increase in private sector employment to be concentrated on short-term contracts rather on long-term contracts. Firms might be reluctant to hire employees on long-term contracts in response to short-term increase in government spending. To test whether those channels explain the strength of the political business cycles, I estimate equation (2.1) where Y_{ijt} is either the share of the working age population that is employed on short-term contracts in the private sector, on long-term contracts in the private sector. Results available in Columns 3 and 4 of Table 2.4 suggest that elections affect employment in the private sector on short-term contracts but not on long-term contracts. In addition, consistent with findings from the literature on elections and firm investment decisions, while the coefficient on the pre-elections quarters dummy is not significant in the long term contract regression, the point estimate is negative (-0.855). Firms are reluctant to invest before the uncertainty surrounding the elections is resolved (Julio and Yook 2012).

In addition, one would expect private sector employment to be more responsive to elections in sectors where local governments can invest in visible projects.

To this end, I test whether short-term employment in the construction sector is affected by elections. There is evidence that employment in the construction sector increases by 0.20 percentage-points in the two pre-election quarters (Column 5 of Table 2.4). This represents about 4 percent of the mean value of employment in the construction sector. It is expected to lead to an increase in either maintenance of existing public goods or the construction of new infrastructure. As above, this seems to suggest that incumbents attempt to provide lasting benefits to their constituents ahead of the elections.

I now present results from a number of robustness checks. First, further results suggest that the main conclusions are robust to the inclusion of lagged values of the dependent variables (Tables A2.4-A2.6). As expected point estimates differ slightly as one needs to account for the additional effect through the lagged values but the results are of the same sign and still statistically significant. This reinforces confidence that results discussed above are capturing the presence of political business cycles in Philippine municipalities.

Second, I assess whether the results are robust to the exclusion of outliers. I estimate equation (2.1) on a number of sub-samples where I exclude the top and bottom one, two, three and four percent in the distribution of employment levels. Results, available in Table A2.7, are consistent with the ones obtained previously.

Third, as indicated in Section 2.4, I also estimate equation (2.1) with quarter/-municipality fixed effects. Results are available in Panel A of Table A2.8. Overall, results are consistent with the ones obtained above. The point estimates are similar but with slightly larger standard errors and I can still reject the null hypothesis of no effect for overall employment, employment in the private sector and long-term employment in the public sector at, at least, the 5% significance level. The only exceptions are for employment levels in the public sector and in the construction sector. The point estimate is as before (.15) but the standard error increases from .086 to .109, with the results going from being marginally signifi-

cant at the 10% level to being marginally insignificant at the 10% level. When it comes to the construction sector, the point estimates are stable but the standard errors increase by about 20 percent and I am no longer able to reject the null of no effect.

Fourth, I also present results from unweighted regressions (Solon, Haider, and Wooldridge 2013). Results, available in Panel B of Table A2.8, are consistent with those obtained previously. There are two exceptions. Employment on long-term contracts in the private sector is now 1.1 percentage-point lower in the two pre-election quarters. This is in line with previous findings on firm investment ahead of elections (Julio and Yook 2012)., In addition, while the coefficient is still positive, I am unable to reject the null hypothesis that employment in short-term contracts in the construction sector is not affected by elections. This seems to indicate that the increase in the construction sector is concentrated in more populous municipalities.

Heterogeneity

As explained in Section 2.4, I test whether political business cycles are dependent upon the municipality's political environment. More specifically, I compare electoral cycles in municipalities with established incumbents and in municipalities with relatively new incumbents. If electoral cycles serve to signal incumbent's quality to voters, they are likely to be concentrated in municipalities where incumbents are from politically inexperienced families.

Results available in Panel A of Table 2.5 provide mixed support for the argument that political business cycles arise as incumbents attempt to signal their types to voters. Once I interact the electoral dummies with the number of times the incumbent's family has been elected mayor in the same municipality between 1988 and the current election, the increase in public sector employment ahead

of elections is lower in municipalities where the incumbent or one of his family member has been elected more often.¹⁵

An alternative explanation for the results that employment rates in the private sector are lower in the post-election months is that incoming officials are learning how to use the bureaucracy which might delay government spending. To test if that is the case, I compare employment rates in municipalities where the incumbent was re-elected and in municipalities where she lost. If learning about bureaucratic procedures explains the negative impact on employment in the post-election period it should be concentrated in municipalities where a challenger won. Results, available Panel B of Table 2.5, indicate that the effect of the post-electoral period on employment in the public and private sectors are similar regardless of whether the incumbent won or lost. The only exception is on employment on long-term contracts in the public sector. It is 0.19 percentage-points lower in the two post-elections quarters in municipalities where a challenger won than in municipalities where the incumbent won. A potential explanation is that, consistent with Fafchamps and Labonne (2013), some employees affiliated with the previous incumbent leave their positions just after the elections and, through time, are replaced by employees affiliated with the new mayor. In addition, this finding is in line with the model of clientelism developed by Robinson and Verdier (2013) who argue that jobs are a credible way of redistributing resources from politicians to bureaucrats as they have incentives to ensure that the incumbent is re-elected.

¹⁵ This set of results provides further evidence that results are not merely capturing the fact that organizing elections require labor as there would be no reason to expect different effects by number of terms. Estimating political business cycles separately for each level of political experience does not yield additional insights (Table A2.9).

Table 2.5: Quarterly political business cycles: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full		Public	Private	ST	LT	ST	LT	Construction
Panel A - Incumbent's experience								
Pre-election quarters	0.9058*** (0.303)	0.1437* (0.087)	0.7621*** (0.285)	0.0255 (0.049)	0.1182** (0.053)	1.5685* (0.849)	-0.8064 (0.795)	0.1998* (0.119)
Post-election quarters	-0.5059** (0.201)	-0.0021 (0.037)	-0.5038** (0.204)	0.0350 (0.047)	-0.0371 (0.044)	-0.4319 (0.647)	-0.0718 (0.755)	-0.1017 (0.093)
Pre-election quarters X Nb terms	0.0088 (0.098)	-0.0707** (0.035)	0.0795 (0.087)	0.0000 (0.012)	-0.0708** (0.033)	0.1440 (0.162)	-0.0645 (0.177)	-0.0008 (0.034)
Post-election quarters X Nb terms	0.0447 (0.094)	-0.0097 (0.024)	0.0544 (0.094)	-0.0012 (0.015)	-0.0085 (0.031)	-0.0478 (0.093)	0.1022 (0.088)	-0.0100 (0.025)
R-squared	0.649	0.618	0.673	0.270	0.595	0.412	0.550	0.337
Panel B - Incumbent Lost								
Pre-election quarters	0.8636*** (0.303)	0.1602* (0.087)	0.7033** (0.284)	0.0323 (0.048)	0.1279** (0.053)	1.5904* (0.850)	-0.8870 (0.803)	0.2094* (0.099)
Post-election quarters	-0.4996** (0.198)	0.0513 (0.052)	-0.5509*** (0.198)	0.0287 (0.052)	0.0226 (0.058)	-0.5108 (0.701)	-0.0401 (0.776)	0.0207 (0.087)
Post-election quarters X Change	0.0703 (0.277)	-0.1939 (0.127)	0.2641 (0.335)	0.0039 (0.050)	-0.1978* (0.103)	0.1841 (0.475)	0.0800 (0.569)	0.0630 (0.063))
R-squared	0.649	0.618	0.673	0.270	0.595	0.412	0.550	0.337

Notes: Results from fixed-effects regressions. The dependent variable is the share of the working age population with a job in the week before the survey (Column 1), with a job in the public sector in the week before the survey (Column 2), with a job in the private sector in the week before the survey (Column 3), with a short-term job in the public sector in the week before the survey (Column 4) with a long-term job in the public sector in the week before the survey (Column 5), with a short-term job in the private sector in the week before the survey (Column 6), with a long-term job in the private sector in the week before the survey (Column 7) and with a short-term job in the construction sector in the week before the survey (Column 8). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population per capita fiscal transfers, a dummy for whether or not the previous municipal election led to a change in local leadership and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Observations: 29,283.

2.5.2 Local political budget cycles

I now test for the presence of budget cycles in Philippine municipalities. Results are available in Table 2.6. Overall, spending does not seem to increase but tax collection effort is lower in election years.

Depending on the number of lags included in the regression and the estimation method, per capita revenues is between 2 and 5 percent lower in election years than in non-election years. Local governments appear to collect less taxes during election years.¹⁶ In addition, fiscal transfers from the central government are lower in election years. Given that fiscal transfers from the central government are computed using tax collection three years before, this suggests that overall tax collection effort by the central government are lower in election years as well.

However, incumbents do not increase spending in election years, which is consistent with the argument that local governments are *de facto* unable to borrow and, as such, are unable to increase spending in election years. It is possible that incumbents alter their within-year spending allocation but I am unable to directly test for it as the data are aggregated yearly.

As above, a potential explanation for the drop in local tax collection is that, in municipalities where the incumbent lost, the new administration might need time to adjust to its new role which could decrease tax collection effort. To test for that, I estimate equation (2.5) and interact the election year dummy with a dummy equal to one if the incumbent lost the election. There is no evidence of an additional drop in tax collection in municipalities where a challenger won the election. The coefficients on the election year dummy are of similar order of magnitude as above. However, there is limited evidence that spending is lower in election years in municipalities where the incumbent lost. As quarterly or

¹⁶ This is consistent with findings from India (Khemani 2004).

monthly data are unavailable, this could either be due to the fact the incumbents who did not spend enough money in the pre-election months tend to lose elections or to the fact that spending slows down after the recently elected mayor takes office. Previous results suggest that the latter is unlikely (Panel B of Table 2.5).

Table 2.6: Political budget cycles

	(1)	(2)	(3)	(4)	(5)
Number of lags	0	1	1	2	2
	FE	FE	GMM	FE	GMM
Panel A - log per capita revenues					
Election Year	-0.0593* (0.031)	-0.0546** (0.025)	-0.0313*** (0.003)	-0.0462* (0.024)	-0.0207*** (0.003)
Observations	14,107	12,578	12,578	10,981	10,981
R-squared	0.911	0.923		0.928	
Panel B - log per capita local tax collection					
Election Year	-0.0266** (0.011)	-0.0235** (0.011)	-0.0170*** (0.004)	-0.0236** (0.011)	-0.0166*** (0.004)
Observations	14,092	12,554	12,554	10,953	10,953
R-squared	0.938	0.948		0.952	
Panel C - log per capita transfers from National gvt					
Election Year	-0.0666 (0.043)	-0.0601* (0.032)	-0.0385*** (0.002)	-0.0439* (0.025)	-0.0120*** (0.003)
Observations	14,103	12,574	12,574	10,977	10,977
R-squared	0.939	0.953		0.962	
Panel D - log per capita spending					
Election Year	-0.0131 (0.021)	-0.0156 (0.024)	-0.0075** (0.004)	-0.0181 (0.023)	-0.0021 (0.004)
Observations	14,105	12,575	12,575	10,977	10,977
R-squared	0.873	0.890		0.897	
Panel E - Share municipal budget spent					
Election Year	4.0713** (1.713)	2.8342*** (0.966)	2.4552*** (0.259)	2.1065** (0.973)	2.6537*** (0.266)
Observations	14,107	12,578	12,578	10,981	10,981
R-squared	0.169	0.178		0.196	

Notes: Results from fixed-effects and GMM regressions. All regressions include in Columns 2-5 controls for population, the number of times the incumbent's family has been elected in the municipality since 1987, a dummy capturing family links between the mayor and provincial officials, a dummy capturing change in mayor and, a simple time trend (Columns 1 and 2), region-specific time trends (Column 3), province-specific time trends (Columns 4) and, municipality-specific time trends (Column 5). The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 2.7: Political budget cycles: heterogeneity

	(1)	(2)	(3)	(4)	(5)
Nb Lags	0	1	1	2	2
	FE	FE	GMM	FE	GMM
Panel A - log per capita revenues from local sources					
Election Year	-0.0289** (0.012)	-0.0188 (0.012)	-0.0127** (0.006)	-0.0201 (0.013)	-0.0123** (0.006)
Election Year X Change	0.0082 (0.024)	-0.0140 (0.017)	-0.0134 (0.012)	-0.0106 (0.022)	-0.0132 (0.012)
Observations	14,092	12,554	12,554	10,953	10,953
R-squared	0.938	0.948		0.952	
Panel B - log per capita spending					
Election Year	-0.0111 (0.021)	-0.0102 (0.023)	0.0001 (0.005)	-0.0124 (0.021)	0.0027 (0.005)
Election Year X Change	-0.0075 (0.013)	-0.0164 (0.013)	-0.0231*** (0.009)	-0.0173 (0.017)	-0.0145* (0.009)
Observations	14,105	12,575	12,575	10,977	10,977
R-squared	0.873	0.890		0.897	

Notes: Results from fixed-effects and GMM regressions. All regressions include in Columns 2-5 controls for population, the number of times the incumbent's family has been elected in the municipality since 1987, a dummy capturing family links between the mayor and provincial officials, a dummy capturing change in mayor and, a simple time trend (Columns 1 and 2), region-specific time trends (Column 3), province-specific time trends (Columns 4) and, municipality-specific time trends (Column 5). The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

2.6 CONCLUSION

In this paper, using a balanced panel of about 1,140 municipalities over 26 quarters, I have identified political business cycles in the Philippines. In election years, the share of the working-age population that is employed is higher in the two pre-election quarters and lower in the two post-election quarters than what it would have been without elections.

The results have methodological implications for the literature on political business cycles. Once analyses are carried out with yearly data, I am unable to identify political business cycles. The difference is not merely driven by a decline in statistical power due to aggregation: point estimates for the overall effects are 7 times larger when I use quarterly data than when I use yearly data. This discrepancy can be explained by a drop in employment post-election that dilutes the yearly effects. Researchers interested in estimating political business cycles should use either monthly or quarterly data.

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Table A2.1: Yearly political business cycles: Employment Levels (excluding 2003)

	(1)	(2)	(3)	(4)
Panel A - All sectors				
Election Year	0.1957 (0.154)	0.1417 (0.151)	0.1411 (0.166)	0.1393 (0.159)
Additional Controls	No	Yes	Yes	Yes
Time Trend	No	Yes	Region	Province
Observations	6,860	6,752	6,752	6,752
R-squared	0.857	0.863	0.868	0.874
Panel B - Public sector				
Election Year	-0.0698 (0.108)	0.0656 (0.056)	0.0657 (0.065)	0.0655 (0.082)
Additional Controls	No	Yes	Yes	Yes
Time Trend	No	Yes	Region	Province
Observations	6,860	6,752	6,752	6,752
R-squared	0.837	0.867	0.868	0.872
Panel C - Private sector				
Election Year	0.2655 (0.197)	0.0761 (0.196)	0.0754 (0.227)	0.0738 (0.208)
Additional Controls	No	Yes	Yes	Yes
Time Trend	No	Yes	Region	Province
Observations	6,860	6,752	6,752	6,752
R-squared	0.864	0.875	0.879	0.885

Notes: Results from fixed-effects regressions. The dependent variable is the yearly average of the share of the working age population with a job in the week before the survey (Panel A), with a job in the public sector in the week before the survey (Panel B) and, with a job in the private sector in the week before the survey (Panel C). Regressions in Columns 2-4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and, per capita fiscal transfers. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.2: Quarterly political business cycles: Average hours worked

	(1)	(2)	(3)	(4)
Panel A - Employment				
Pre-election quarters	-0.5615 (0.523)	-0.5702 (0.501)	-0.5711 (0.510)	-0.5755 (0.585)
Post-election quarters	0.1341 (0.281)	0.1526 (0.325)	0.1551 (0.325)	0.1544 (0.308)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	29,715	29,283	29,283	29,283
R-squared	0.706	0.709	0.712	0.718
Panel B - Public sector employment				
Pre-election quarters	-0.8382 (0.691)	-1.1972 (0.771)	-1.1923 (0.772)	-1.1968* (0.695)
Post-election quarters	0.2837 (0.280)	0.0387 (0.378)	0.0408 (0.390)	0.0466 (0.369)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	25,442	25,083	25,083	25,083
R-squared	0.305	0.309	0.311	0.318
Panel C - Private sector employment				
Pre-election quarters	-0.5433 (0.521)	-0.5132 (0.493)	-0.5142 (0.502)	-0.5188 (0.569)
Post-election quarters	0.1152 (0.289)	0.1651 (0.324)	0.1676 (0.325)	0.1666 (0.307)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	29,715	29,283	29,283	29,283
R-squared	0.700	0.703	0.706	0.712

Notes: Results from fixed-effects regressions. The dependent variable is the average number hours worked in the week before the survey for those who have a job in the week before the survey (Panel A), with a job in the public sector in the week before the survey (Panel B) and with a job in the private sector in the week before the survey (Panel C). All regressions include controls for survey quarter. Regressions in Columns 2-4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and, per capita fiscal transfers. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.3: Quarterly political business cycles: Log wage

	(1)	(2)	(3)	(4)
Panel A - Employment				
Pre-election quarters	-0.0746** (0.035)	-0.0147* (0.009)	-0.0147 (0.009)	-0.0147 (0.009)
Post-election quarters	-0.0307 (0.040)	0.0047 (0.008)	0.0047 (0.008)	0.0048 (0.009)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	28,818	28,403	28,403	28,403
R-squared	0.700	0.758	0.761	0.765
Panel B - Public sector employment				
Pre-election quarters	-0.0705** (0.035)	-0.0223 (0.014)	-0.0224 (0.015)	-0.0226 (0.015)
Post-election quarters	-0.0400 (0.031)	-0.0167 (0.020)	-0.0167 (0.021)	-0.0167 (0.020)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	23,324	23,002	23,002	23,002
R-squared	0.260	0.289	0.292	0.302
Panel C - Private sector employment				
Pre-election quarters	-0.0684* (0.035)	-0.0098 (0.010)	-0.0100 (0.010)	-0.0101 (0.011)
Post-election quarters	-0.0263 (0.041)	0.0120 (0.008)	0.0122 (0.008)	0.0121 (0.008)
Additional Controls	No	Yes	Yes	Yes
Quadratic Time Trend	No	Yes	Region	Province
Observations	28,104	27,699	27,699	27,699
R-squared	0.768	0.805	0.809	0.812

Notes: Results from fixed-effects regressions. The dependent variable is the average log wage for those who have a job in the week before the survey (Panel A), with a job in the public sector in the week before the survey (Panel B) and with a job in the private sector in the week before the survey (Panel C). All regressions include controls for survey quarter. Regressions in Columns 2-4 include controls for average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population and, per capita fiscal transfers. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.4: Yearly political business cycles: Alternative specification

	(1)	(2)	(3)	(4)	(5)	(6)
	Full		Public		Private	
Number of lags	1	2	1	2	1	2
Panel A: Fixed effects						
Election Year	0.1606 (0.188)	0.1990 (0.206)	0.0684 (0.058)	0.0190 (0.054)	0.0909 (0.209)	0.1758 (0.253)
Observations	6,751	5,607	6,751	5,607	6,751	5,607
R-squared	0.873	0.887	0.872	0.880	0.884	0.896
Panel B: GMM						
Election Year	0.1745 (0.128)	0.2684* (0.153)	0.0550* (0.031)	0.0450 (0.039)	0.0925 (0.127)	0.2179 (0.161)
Observations	6,751	5,607	6,751	5,607	6,751	5,607

Notes: Results from Fixed-Effects and GMM regressions. The dependent variable is the yearly average of the share of the working age population with a job in the week before the survey (Columns 1-2), with a job in the public sector in the week before the survey (Columns 3-4) and, with a job in the private sector in the week before the survey (Columns 5-6). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and region-specific time trend. The dependent variable is lagged once in Columns 1, 3 and 5 and twice in Columns 2, 4 and 6. The standard errors (in parentheses) account for potential correlation within time period and province (Panel A), and within province (Panel B). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.5: Yearly political business cycles: Alternative specification (Excluding 2003)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full		Public		Private	
Number of lags	1	2	1	2	1	2
Panel A: Fixed effects						
Election Year	0.1957 (0.205)	0.2512 (0.172)	0.0276 (0.057)	0.0037 (0.045)	0.1687 (0.256)	0.2420 (0.185)
Observations	5,607	4,464	5,607	4,464	5,607	4,464
R-squared	0.886	0.901	0.875	0.894	0.895	0.908
Panel B: GMM						
Election Year	0.1406 (0.129)	0.2711* (0.152)	0.0529 (0.036)	0.0314 (0.038)	0.1758 (0.132)	0.2238 (0.159)
Observations	5,607	4,464	5,607	4,464	5,607	4,464

Notes: Results from Fixed-Effects and GMM regressions. The dependent variable is the yearly average of the share of the working age population with a job in the week before the survey (Columns 1-2), with a job in the public sector in the week before the survey (Columns 3-4) and, with a job in the private sector in the week before the survey (Columns 5-6). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and region-specific time trend. The dependent variable is lagged once in Columns 1, 3 and 5 and twice in Columns 2, 4 and 6. The standard errors (in parentheses) account for potential correlation within time period and province (Panel A), and within province (Panel B). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.6: Quarterly political business cycles: Alternative specification

	(1)	(2)	(3)	(4)	(5)	(6)
	Full		Public		Private	
Number of lags	1	2	1	2	1	2
Panel A: Fixed effects						
Pre-election quarters	0.7388** (0.294)	0.7991*** (0.271)	0.1452 (0.098)	0.1386 (0.093)	0.6013 (0.446)	0.6648* (0.361)
Post-election quarters	-0.5921*** (0.192)	-0.5750*** (0.217)	-0.0071 (0.042)	-0.0210 (0.051)	-0.5838*** (0.218)	-0.5506** (0.264)
Observations	28,132	26,984	28,132	26,984	28,132	26,984
R-squared	0.655	0.657	0.615	0.613	0.678	0.679
Panel B: IV						
Pre-election quarters	1.2551*** (0.230)	1.3011*** (0.201)	0.1939*** (0.065)	0.1641** (0.079)	1.0612*** (0.240)	1.1176*** (0.166)
Post-election quarters	-0.1882 (0.206)	-1.2071** (0.480)	0.0581 (0.041)	-0.1130 (0.142)	-0.2526 (0.219)	-1.1795** (0.509)
Observations	25,837	24,690	25,837	24,690	25,837	24,690

Notes: Results from Fixed-Effects and IV regressions. The dependent variable is the share of the working age population with a job in the week before the survey (Columns 1-2), with a job in the public sector in the week before the survey (Columns 3-4) and, with a job in the private sector in the week before the survey (Columns 5-6). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population, per capita fiscal transfers and province-specific quadratic time trend. The dependent variable is lagged once in Columns 1, 3 and 5 and twice in Columns 2, 4 and 6. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.7: Quarterly political business cycles: Excluding outliers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full	Public	Private	Public (ST)	Public (LT)	Private (ST)	Private (LT)	Construction (ST)	
Panel A - Exclude top and bottom 1% (n=28,710)								
Pre-election quarters	0.8661*** (0.269)	0.1452* (0.087)	0.7208*** (0.257)	0.0285 (0.048)	0.1168** (0.055)	1.5623** (0.769)	-0.8415 (0.680)	0.2098 (0.160)
Post-election quarters	-0.4489** (0.198)	-0.0020 (0.037)	-0.4469** (0.205)	0.0342 (0.050)	-0.0362 (0.045)	-0.4556 (0.649)	0.0086 (0.761)	-0.1042 (0.091)
Panel B - Exclude top and bottom 2% (n=28,123)								
Pre-election quarters	0.8041*** (0.266)	0.1386 (0.085)	0.6656*** (0.251)	0.0268 (0.047)	0.1117** (0.057)	1.5656* (0.808)	-0.9001 (0.751)	0.2142 (0.158)
Post-election quarters	-0.4228** (0.197)	0.0020 (0.038)	-0.4248** (0.204)	0.0334 (0.051)	-0.0315 (0.044)	-0.4653 (0.652)	0.0405 (0.754)	-0.1025 (0.091)
Panel C - Exclude top and bottom 3% (n=27,539)								
Pre-election quarters	0.8100*** (0.264)	0.1359 (0.083)	0.6741*** (0.243)	0.0251 (0.045)	0.1108** (0.053)	1.5703* (0.834)	-0.8962 (0.642)	0.2157* (0.125)
Post-election quarters	-0.3909** (0.199)	0.0043 (0.038)	-0.3952 (0.292)	0.0321 (0.049)	-0.0278 (0.070)	-0.4629 (0.680)	0.0678 (0.781)	-0.1005 (0.088)
Panel D - Exclude top and bottom 4% (n=26,952)								
Pre-election quarters	0.8154*** (0.259)	0.1358 (0.083)	0.6797*** (0.246)	0.0249 (0.046)	0.1109** (0.053)	1.5462** (0.761)	-0.8665 (0.634)	0.2177* (0.120)
Post-election quarters	-0.3477 (0.214)	0.0000 (0.039)	-0.3477 (0.217)	0.0289 (0.050)	-0.0289 (0.049)	-0.4435 (0.649)	0.0958 (0.759)	-0.1006 (0.089)

Notes: Results from fixed-effects regressions. The dependent variable is the share of the working age population with a job in the week before the survey (Column 1), with a job in the public sector in the week before the survey (Column 2), with a job in the private sector in the week before the survey (Column 3), with a short-term job in the public sector in the week before the survey (Column 4) with a long-term job in the public sector in the week before the survey (Column 5), with a short-term job in the private sector in the week before the survey (Column 6), with a long-term job in the private sector in the week before the survey (Column 7) and with a short-term job in the construction sector in the week before the survey (Column 8). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population per capita fiscal transfers, a dummy for whether or not the previous municipal election led to a change in local leadership and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.8: Quarterly political business cycles: Additional robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full	Public	Private	ST	LT	Public	Private	Construction
				ST	LT	ST	LT	ST
Panel A - Municipality*quarter fixed effects								
Pre-election quarters	0.8723*** (0.314)	0.1469 (0.109)	0.7254** (0.290)	0.0276 (0.060)	0.1193** (0.060)	1.5746 (0.977)	-0.8492 (0.731)	0.2028 (0.146)
Post-election quarters	-0.4848** (0.224)	-0.0011 (0.048)	-0.4837** (0.229)	0.0338 (0.052)	-0.0349 (0.051)	-0.4472 (0.683)	-0.0364 (0.805)	-0.1056 (0.102)
Observations	29,283	29,283	29,283	29,283	29,283	29,283	29,283	29,283
R-squared	0.702	0.687	0.723	0.376	0.668	0.484	0.610	0.438
Panel B - No weights								
Pre-election quarters	0.7965*** (0.307)	0.1842* (0.106)	0.6122* (0.327)	0.0490 (0.060)	0.1353** (0.058)	1.7107** (0.738)	-1.0985* (0.644)	0.1568 (0.100)
Post-election quarters	-0.7115*** (0.268)	0.0187 (0.055)	-0.7302*** (0.273)	0.0151 (0.042)	0.0036 (0.045)	-0.5142 (0.678)	-0.2160 (0.828)	-0.0891 (0.091)
Observations	29,283	29,283	29,283	29,283	29,283	29,283	29,283	29,283
R-squared	0.603	0.585	0.642	0.215	0.561	0.366	0.528	0.299

Notes: Results from fixed-effects regressions. The dependent variable is the share of the working age population with a job in the week before the survey (Column 1), with a job in the public sector in the week before the survey (Column 2), with a job in the private sector in the week before the survey (Column 3), with a short-term job in the public sector in the week before the survey (Column 4) with a long-term job in the public sector in the week before the survey (Column 5), with a short-term job in the private sector in the week before the survey (Column 6), with a long-term job in the private sector in the week before the survey (Column 7) and with a short-term job in the construction sector in the week before the survey (Column 8). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population per capita fiscal transfers, a dummy for whether or not the previous municipal election led to a change in local leadership and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A2.9: Quarterly political business cycles: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full	Public	Private	Public (ST)	Public (LT)	Private (ST)	Private (LT)	Construction (ST)
Pre-election quarters X								
First term	0.9595** (0.413)	0.2755** (0.108)	0.6840* (0.363)	0.0037 (0.050)	0.2718*** (0.095)	1.1990 (1.067)	-0.5150 (1.152)	0.1866 (0.141)
Second term	0.6908* (0.358)	0.2173** (0.104)	0.4735 (0.356)	0.0648 (0.062)	0.1524** (0.076)	1.5678* (0.837)	-1.0943 (0.786)	0.2346* (0.120)
Third term	1.0389*** (0.314)	0.0857 (0.141)	0.9532*** (0.320)	0.0265 (0.056)	0.0592 (0.117)	1.4673* (0.826)	-0.5141 (0.797)	0.1655 (0.126)
Fourth term	1.2127* (0.626)	-0.0663 (0.171)	1.2790* (0.678)	-0.0277 (0.055)	-0.0386 (0.148)	1.8920 (1.152)	-0.6130 (1.148)	0.2027 (0.221)
Fifth term	0.5005 (0.466)	0.0004 (0.130)	0.5001 (0.448)	0.0246 (0.109)	-0.0242 (0.095)	1.5103* (0.897)	-1.0102 (0.921)	0.1858 (0.188)
Post-election quarters X								
First term	-0.7884*** (0.302)	-0.0171 (0.074)	-0.7713** (0.306)	0.0099 (0.061)	-0.0270 (0.077)	-0.9749 (0.686)	0.2036 (0.758)	-0.1523 (0.116)
Second term	-0.2804 (0.268)	0.0142 (0.069)	-0.2946 (0.255)	0.0592 (0.054)	-0.0450 (0.091)	-0.1483 (0.919)	-0.1463 (1.053)	-0.0282 (0.125)
Third term	-0.4559 (0.348)	0.0592 (0.102)	-0.5151 (0.385)	0.0413 (0.074)	0.0179 (0.072)	-0.0043 (0.554)	-0.5108 (0.754)	-0.1142 (0.119)
Fourth term	-0.3573 (0.459)	-0.0215 (0.119)	-0.3358 (0.469)	-0.0022 (0.049)	-0.0193 (0.112)	-0.4867 (0.812)	0.1509 (0.855)	-0.1520 (0.187)
Fifth term	-0.4892 (0.454)	-0.0520 (0.093)	-0.4372 (0.447)	0.0520 (0.079)	-0.1041 (0.111)	-0.2294 (0.694)	-0.2078 (0.744)	-0.0426 (0.122)
R-squared	0.649	0.618	0.673	0.271	0.596	0.413	0.551	0.338

Notes: Results from fixed-effects regressions. The dependent variable is the share of the working age population with a job in the week before the survey (Column 1), with a job in the public sector in the week before the survey (Column 2), with a job in the private sector in the week before the survey (Column 3), with a short-term job in the public sector in the week before the survey (Column 4) with a long-term job in the public sector in the week before the survey (Column 5), with a short-term job in the private sector in the week before the survey (Column 6), with a long-term job in the private sector in the week before the survey (Column 7) and with a short-term job in the construction sector in the week before the survey (Column 8). All regressions include controls for survey quarters, average age (and its square) in the municipality (for those older than 15), education levels (for those older than 15), the share of women, population per capita fiscal transfers, a dummy for whether or not the previous municipal election led to a change in local leadership and province-specific quadratic time trends. The standard errors (in parentheses) account for potential correlation within time period and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level. Observations: 29,283.

DO POLITICIANS' RELATIVES GET BETTER JOBS? EVIDENCE FROM MUNICIPAL ELECTIONS IN THE PHILIPPINES

3.1 INTRODUCTION

In this paper we examine whether people who are related to a successful politician get a better job. Thanks to panel data, progress has been made on establishing the value of political connections for firms (Fisman 2001, Khwaja and Mian 2005, Faccio 2006, Cingano and Pinotti 2013). Less progress has been made in identifying the causal effect or value of political connections for individuals (Besley, Pande, and Rao 2012, Caeyers and Dercon 2012).¹

The literature on the value of political connections has faced several difficulties and is not well-developed. First, for lack of better data, researchers often rely on self-reported links to local politicians as a measure of political connections. Such data are subject to bias because those who derive benefits from their connections are more likely to report them (Comola and Fafchamps forthcoming).

This is joint work with Pr. Marcel Fafchamps. We are indebted to Lorenzo Ductor for agreeing to act as the third party that performed the sample split. We are grateful to Bert Hofman and Motoki Hayakawa for fruitful discussions while working on this paper. The Department of Social Welfare and Development kindly allowed us to use data from the National Household Targeting System for Poverty Reduction. We thank Fermin Adriano, Hrithik Bansal, Cesi Cruz, Clement Imbert, Claire Labonne, Pablo Querubin, Matt Stephens and Kate Vyborny and participants in the CSAE Conference 2012, South East Asia Symposium 2012, Gorman Workshop, UPSE Friday Seminar, AIM Policy Center Seminar, DIAL Conference 2013 and 2013 CMPO Political Economy and Public Services workshop for comments. All remaining errors are ours.

¹ This is related to the literature on the role of family links in labor markets. For example, Wang (forthcoming) documents a significant reduction in earnings when a man's father-in-law dies.

Second, individuals connected to politicians may differ from the average citizen along unobservable characteristics that affect their welfare even if their politician relatives are not in office. It follows that when researchers observe a correlation between individual welfare and political connections, it is unclear how much of this correlation is due to unobserved heterogeneity. Third, the literature has not accounted for the possibility that individuals connected to politicians who lost an election can suffer from their connections, especially in areas where elected officials have discretionary powers.²

Using a large dataset from the Philippines, collected between the 2007 and 2010 elections, we test whether individuals who are connected to politicians in municipal elections have a better-paying occupation.³ We contribute to the literature in two ways. First, we rely on Filipino naming conventions introduced by Spanish colonial authorities to infer family ties to local politicians (see Angelucci, De Giorgi, Rangel, and Rasul (2010) for a similar approach). This bypasses the need to rely on self-reported links. Because Spanish family names were introduced in the Philippines recently (i.e, in the middle of the 19th century) and because Spanish naming conventions are more informative, they allow an unusually precise and objective identification of family ties.

Second, we test the robustness of our findings to the use of different control groups. We find that this matters. In particular, we distinguish between individuals connected to successful and unsuccessful candidates in different municipal elections. This allows us to estimate both the value of being connected to an elected local official, and the cost of being connected to a politician in opposition, both of which are present in our data.

Since we are using observational data, concerns arise regarding specification search and publication bias (Leamer 1974, Leamer 1978, Leamer 1983, Lovell

² Hsieh, Miguel, Ortega, and Rodriguez (2011) for example find that, in Venezuela, individuals who signed a petition against Chavez earn less and are less likely to be employed.

³ The dataset does not include information on the sector of employment.

1983, Glaeser 2006). To address these concerns, we use a split sample approach that, we believe, could be adopted widely in a world of *big data* (Einav and Levin 2013). We asked a third party to split the data into two randomly generated, non-overlapping subsets, A and B, and to hand over sample A to us. This version of the paper uses sample A to narrow down the list of hypotheses we wish to test and to refine our methodology. Once the review process is completed, we will apply to sample B the methodology that has been approved by the referees and editor, and this is what will be published.⁴

Our results can be summarized as follows. Individuals who share one or more middle or last names with local elected officials are more likely to be employed in better paying occupations. This effect persists when we control for individual characteristics and when we compare relatives of politicians elected in 2007 and in 2010. The effect is particularly noticeable at the top of the occupational distribution: the probability of being employed in a managerial position increases by 0.54 percentage-points, or more than 22 percent of the control group mean, for individuals related to current office holders compared to relatives of politicians elected after the occupational data were collected. This result is robust to the use of different control groups, alternative specifications, and estimation techniques.

We are also able to test for the impact of being related to local politicians who failed to be elected. We find that relatives of candidates close to being elected as mayors or vice-mayors are less likely to be employed in better paying occupations than relatives of politicians that did not perform as well in the elections. We interpret this as suggesting that incumbents punish their serious opponents.

The impact of family connections varies with observable individual and municipal characteristics. First, the impact is stronger for more educated individu-

⁴ The current version of the paper is akin to the '*mock report*' that Humphreys, Sanchez de la Sierra, and van der Windt (2013) recommend writing using fake data before analyzing data from RCTs. They argue that simple registration requirements still leave room for data mining. The main concern associated with this approach, however, is that it might "stifle innovation"(Casey, Glennerster, and Miguel 2012, Deaton 2012). By using real data we are able to deal with this problem.

als, and most of the impact is concentrated on individuals with some university education. Second the impact of connections on the probability of being employed in a managerial position is 40 percent lower for women than it is for men. For all other occupations, the impacts are similar for men and women, except that connected men are less likely to be employed, while no such effect is observed for women. Finally, a family connection to a mayor has a stronger effect on occupation than a connection to a local councilor. We also find evidence consistent with the idea that the benefit from political connections is lower in more politically contested areas. First, the impact decreases with the number of elected municipal councilors who did not run on the mayor's ticket. Second, the impact is larger in areas where the incumbent has been in office for longer.

While further research is needed to understand how these effects materialize, the results offer some suggestive evidence. First, since the benefits of political connections is stronger for educated individuals, it is unlikely that our results are solely driven by politicians' altruistic or redistributive motives towards their relatives. Second, the fact that individuals connected to candidates that were almost elected are less likely to be employed in managerial positions suggest that family connections facilitate supervision and monitoring rather than screening. To illustrate, Cullinane (2009, p 190) reports that when asked about the employment of his relatives in the local government, Ramon Durano, a Cebuano politician, told a reporter that *'politics is not something you can entrust to non-relatives'*. Sidel (1999) argue that municipal mayors use their control over tax collection and regulatory enforcement not only to enrich themselves but also to gain electoral rewards. In this case, we would expect politicians to value loyalty, before and after the elections. This interpretation is line with Iyer and Mani (2012)'s finding that politicians in India value both loyalty and expertise when assigning bureaucrats.

The results presented in this paper have a number of implications for the literature on the value of political connections. First, they suggest that, in the absence of an adequate control group, estimates of the value of political connections tend to be biased upward. Second, estimates obtained by comparing individuals connected to the winner and loser in close elections potentially include the cost suffered by individuals related to the loser. Conditioning on close elections changes the nature of the parameter being estimated. It also provides a note of caution regarding political decentralization in areas of weak accountability, a description that fits most municipalities in the Philippines (De Dios 2007). In such settings, local officials might be able not only to favor their relatives in hiring decisions but also punish the relatives of their political opponents. This in turn may have a deleterious long-term influence on electoral competition and thus on the quality of local political leadership.

The paper is organized as follows. We describe the setting in Section 3.2 and the estimation strategy in Section 3.3. The data and sample are presented in Section 3.4. In Section 3.5 we discuss the main results and a number of robustness checks. Section 3.6 concludes.

3.2 LOCAL POLITICS IN THE PHILIPPINES

Philippine municipalities represent a particularly well-suited setting to estimate the value of political connections at the local level. To support this point, we summarize here some of what is known about the institutional and political context in the country.

In 1991 the Local Government Code (Republic Act 7160) devolved significant decision-making power and fiscal resources to mayors, vice-mayors and municipal councilors (Brillantes 1992). Local elections are organized, by law, at fixed

intervals of three years. This rules out any possible endogeneity between the timing of local elections and the support politicians have in their constituency.

Diokno (2012) argues that, after two decades, the benefits of decentralization are far from clear. He blames the situation on poor governance practices at the local level rather than on inadequate fiscal arrangements. As some had originally feared,⁵ available evidence indicates that municipal mayors tend to use their new resources and discretion to prolong their time in office (Capuno 2012). The primary drivers of resource allocations tend to be political considerations. For example, when the Department of Social Welfare and Development started implementing a large-scale conditional cash transfer program in 2008, it was deemed necessary to establish a centralized targeting system rather than to rely on local officials to identify beneficiaries.

There is evidence that local Filipino politicians act as employment brokers in both the public and private sectors (Sidel 1999). In the public sector, Hodder (2009) argues that they are able to use their hiring powers over a large number of staff that were transferred from national agencies to municipalities as part of the decentralization process.⁶ For example, Hodder (2009) quotes a lawyer for the Civil Service Commission: *We can even go so far as saying that you cannot be appointed in local government if you do not know the appointing authority or, at least, if you do not have any [political] recommendation....And even once in place, the civil servant's position is not secure: when the new mayor [comes], he just tells them 'resign or I'll file a case against you.'*

In the private sector, Sidel (1999) shows that local politicians can affect employment either directly, through their business holdings or, in a number of

5 For a prescient analysis of the potential issues associated with decentralization, see Brillantes (1992).

6 This was a common occurrence even before decentralization. For example, in 1963, the Durano clan was accused of having relatives run 10 public offices in Danao City: the mayor, the vice-mayor, the president of the city council, the chief of police, the city assessor, the Bureau of Internal Revenue collecting agent, the city health officer, the city medical officer, the supervising nurse and the school division's head nurse (Cullinane 2009, p 210).

provinces, indirectly through their contacts with local businessmen. In addition, it is possible that local businessmen favor the relatives of local officials in their hiring decisions, in the hope of securing more favorable regulatory supervision. In the Philippines, a number of permits required to operate a business are delivered by the municipal bureaucracy (Llanto 2012).

There is some evidence that loyalty to local politicians is valued. Bureaucrats are often expected to engage in behavior favoring incumbents prior to the elections (Coronel 1995, Cullinane 2009). Cullinane (2009) reports that local politicians often staff the bureaucracy with loyal individuals they can trust to act in their best interest. In a case study of local politics in Cavite, Coronel (1995) points out *'public officials in the bureaucracy - the Comelec [Commission on Elections], teachers and the police - have not been neutral or objective. Since 1945, this machinery has been used, and it is embedded in the political structure.'* It follows that relatives of known political challengers may suffer from their connections if incumbents are reluctant to staff the bureaucracy with individuals whose views and interests are antinomic to theirs. There is indeed qualitative evidence that Filipino politicians have the ability to punish individuals connected to their opponents.⁷

3.3 ESTIMATION STRATEGY

In this section we describe how we estimate the value of political connections. First, we present the estimation strategy we follow to test if being related to an elected local official has a causal impact on occupational choice. We take a

⁷ For example, according to McCoy (2009a, p 17) President Marcos *"used his martial-law powers to punish enemies among the old oligarchy, stripping them of assets"*. One of the best known examples involves the relationship between Ferdinand Marcos and Fernando Lopez (McCoy 2009b). While Lopez was Marcos' running mate in the 1965 and 1969 elections, a rift occurred in 1971. Marcos imprisoned one of Lopez's nephews on dubious charges and forced Lopez's brother Eugenio to sell his shares in the country's leading utility. He was also stripped of his media empire and suffered total losses amounting to millions of US dollars (McCoy 2009b). Highlighting the benefits of being related to an elected politician, some of the assets were transferred to Marcos' brother-in-law, Benjamin Romualdez.

step-by-step approach, discussing how data constraints and unobserved heterogeneity combine to make the estimation of the value of political connections challenging. We also discuss how we test for heterogeneous effects.

3.3.1 *The value of political connections*

In order to deal with unobserved heterogeneity, researchers attempting to provide credible estimates of the value of political connections need to identify a valid control group. In the absence of exogenous variation in political connections, identifying a valid control group is difficult. We present several approaches in turn and discuss their relative advantages and drawbacks.

In most contexts, researchers are only able to compare politically connected individuals to individuals randomly drawn from the population. To allow comparison with this literature, we start by estimating the value of political connections by regressing the outcome variable on a dummy capturing links to elected local officials, plus individual controls. Specifically we estimate a linear probability model of the form:

$$Y_{ijt} = \alpha C_{ijt} + \beta X_{ijt} + v_{jt} + u_{ijt} \quad (3.1)$$

where Y_{ijt} is a measure of occupational choice for individual i in municipality j at the time of the survey t , α is the parameter of interest, C_{ijt} is a dummy variable that equals one if individual i is related to an elected official in office in municipality j at time t , X_{ijt} is a vector of observable individual characteristics, v_{jt} is an unobservable affecting all individuals in municipality j at time t and

u_{ijt} is an idiosyncratic error term. Since occupational choice might be correlated within provinces, we cluster standard errors at the provincial level.⁸

We estimate equation (3.1) in three different ways. We begin by including only municipal fixed-effects. Then, we add individual controls X_{ijt} for age, gender and, educational achievements. In the third regression, we also control for i 's marital status, relationship to the household head, history of displacement, and we include dummies for the month*year in which the interview took place. Since we have a large number of observations, we include a full set of dummies for each distinct value of each control variable.

While this approach has been used in the literature (*e.g.*, Caeyers and Dercon (2012)), it remains vulnerable to the presence of unobserved heterogeneity correlated with political connections C_{ijt} . To make this explicit, let us decompose u_{ijt} into three components:

$$u_{ijt} = \mu_{ij} + \eta_{ij} + e_{ijt}$$

where e_{ijt} is a pure random term with $E[e_{ijt}C_{ijt}] = 0$. Let μ_{ij} be the effect on Y_{ijt} of being related to someone who ran at least once in a local election. There are good reasons to expect $E[\mu_{ij}|C_{ijt} = 1] > E[\mu_{ij}|C_{ijt} = 0]$. For instance, a higher social standing makes it more likely that an individual is related to the local political elite, but also that he or she has a better occupation. Similarly, let η_{ij} be the additional effect on Y_{ijt} of being connected to a candidate who has even won a local election. We expect $E[\eta_{ij}|C_{ijt} = 1] > E[\eta_{ij}|C_{ijt} = 0]$: on average, individuals with characteristics that make them more likely to be related to a successful politician also have, other things being equal, a social standing correlated with a better occupation. To the extent that $E[\mu_{ij}C_{ijt}] > 0$ and $E[\eta_{ij}C_{ijt}] > 0$, we expect an upward bias in estimates of α that are obtained by estimating equation

⁸ The sample includes data from more than 60 provinces so we are not concerned about bias in our standard errors as a result of having too few clusters (Cameron, Gelbach, and Miller 2008).

(3.1) on the entire population. If we can control for μ_{ij} and η_{ij} , α then captures the effect of being related to an elected official currently in office, net of any correlation between social status and local politicians, successful or otherwise.

CONTROL GROUP I: RELATIVES OF UNSUCCESSFUL 2007 CANDIDATES As a first step in controlling for unobserved heterogeneity, we estimate equation (3.1) on the restricted sample of all individuals related to local politicians who ran in the 2007 elections. In this approach, individuals related to unsuccessful politicians serve as controls for individuals related to successful politicians. The purpose of this approach is to net out unobserved heterogeneity μ_{ij} . It delivers an unbiased estimate of α provided that $E[\eta_{ij}C_{ijt}] = 0$. Comparing to the $\hat{\alpha}$ obtained from (3.1) using the total population as control group yields an estimate of the bias:

$$\mu \equiv E[\mu_{ij}|C_{ijt} = 1] - E[\mu_{ij}|C_{ijt} = 0]$$

CONTROL GROUP II: RELATIVES OF 2010 CANDIDATES Even in situations where $E[\eta_{ij}C_{ijt}] = 0$, using control group I is vulnerable to one possible source of bias. Imagine that relatives of an unsuccessful opponent in the last election are punished by the successfully elected politician, and further imagine that this punishment translates in a lower occupation. In this case, the difference in occupation level Y_{ijt} between relatives of successful and unsuccessful candidates in the last election overestimates α since it includes the value of the punishment.

One possible solution is to use as controls the relatives of politicians who ran in an election taking place *after* survey time t , but who did not run in elections that took place before time t . By construction, these politicians – and their relatives – cannot be punished at t for opposing the currently elected official after t . To the best of our knowledge this is the first time this approach is being used.

Based on this idea, we estimate equation (3.1) on the sample of individuals connected to either successful candidates in the 2007 elections, or to candidates in the 2010 elections who did not run in 2007. This provides an estimate of α that nets out both μ_{ij} and the punishment meted out on unsuccessful opponents. Comparing it to the $\hat{\alpha}$ obtained using control group I yields an estimate of the punishment effect. But this method does not control for η_{ij} .

CONTROL GROUP III: RELATIVES OF SUCCESSFUL 2010 CANDIDATES To control for both μ_{ij} and η_{ij} while netting out possible punishments, we estimate equation (3.1) using a third control group that only includes relatives of successful 2010 candidates who did not run for election in 2007. This control group minimizes sources of bias and should arguably yield the most accurate estimate of α . But because it is the most restrictive, it also results in the smallest number of control observations – and thus to a possible loss of power. To the best of our knowledge, this is the first time this estimation strategy is being used to estimate the value of political connections for individuals.⁹

We report estimates using all three control groups. Comparison of $\hat{\alpha}$ estimates obtained with control groups I and II provides an estimate of the punishment bias that can arise when using a control group I approach, as is commonly done in the literature. Comparison between estimates of α obtained with control groups II and III provides an estimate of the bias:

$$\eta \equiv E[\eta_{ij}|C_{ijt} = 1] - E[\eta_{ij}|C_{ijt} = 0]$$

Control groups II and, especially, III are a marked improvement upon what the literature has been able to use until now. We are able to use these control

⁹ Implicitly, this method is used in the literature on the impacts of political connections for firms. Researchers often have access to panel data and can thus compare, within the set of firms that are politically connected at some point in their sample years, firms that are connected at time t and those are not.

groups for several reasons. First, we infer links from information about names, not from self-reported data. Control groups II and III could not be constructed from self-reported measures of political connections: how could respondents be asked about their connections to yet-to-be-revealed candidates. Second, using names to infer family connections could be problematic in many countries but, for reasons that will soon be made clear, in the Philippines names are particularly informative about family ties. Finally, we have a very large sample and there is ample turnover of local politicians from one election to the next. Had the sample been smaller and turnover less frequent, control and treatment groups would have been too small to estimate α .

3.3.2 *Heterogeneity*

We investigate whether the value of political connections varies with the rank of the local politician to whom the individual is related. To this effect, we estimate equation (3.1) replacing C_{ij} with all possible interactions of three dummy variables capturing family ties to the mayor, the vice-mayor, and municipal councilors and the associated marginal effects. We also test whether the impact of political connections varies across individuals in a systematical way. More specifically, we test for heterogenous effects along three characteristics Z_{ij}^p – gender, education and age – estimating equations of the form:

$$Y_{ijt} = \alpha C_{ijt} + \beta X_{ijt} + \sum_{p=1}^3 (\delta_p C_{ijt} (Z_{ij}^p - \bar{Z}^p) + \gamma_p Z_{ij}^p) + v_{jt} + u_{ijt} \quad (3.2)$$

In the interaction term the Z_{ij}^p variables are demeaned so that α still measures the average treatment effect.

We also investigate heterogeneity at the municipal level. We expect that the economic and political environment in the municipality influences the incentives and constraints that politicians face to reward their relatives (Weitz-Shapiro 2012). To implement this idea, we estimate a model of the form:

$$Y_{ij} = \alpha C_{ij} + \beta X_{ij} + \sum_{p=1}^P \delta_p C_{ij} * (Z_j^p - \bar{Z}^p) + v_j + u_{ij} \quad (3.3)$$

where Z_j^p is a relevant characteristic of the municipality. We do not control for Z_j^p directly since all regressions include municipal fixed-effects.

3.4 DATA

The primary dataset used in this paper comes from data collected between 2008 and 2010 for the National Household Targeting System for Poverty Reduction (NHTS-PR). The data were collected by the Department of Social Welfare and Development (DSWD) to select beneficiaries for the *Pantawid Pamilya Pilipino Program*, a large-scale conditional cash transfer (CCT) program. The data are used by DSWD to predict per capita income through a Proxy Means Test and to determine eligibility in the CCT program (Fernandez 2012).

We have access to the full dataset which covers more than 50 millions individuals. For each individual we have data on, among other things, age, gender, education, occupation, and the middle and last names. In approximately 700 municipalities full enumeration of all residents took place. The data cover approximately 20 million individuals in those municipalities. In the remaining municipalities, information was only collected on residents in so-called *pockets of poverty*. To avoid sample selection issues, we limit our analysis to those 700 mu-

municipalities in which full enumeration took place. We restrict the sample to data collected between 2008 and April 2010, that is, before the May 2010 elections.

We obtained from the Commission on Elections the names of all the candidates for the positions of mayors, vice-mayors, and municipal councilors in the 2007 and 2010 local elections for the 700 municipalities where full enumeration took place. There are a total of 38,448 candidates, 80 percent of whom ran for the position of municipal councilor. The rest are evenly split between candidates for the mayoral and vice-mayoral positions. We also have information on the outcome of the elections in each of the 700 municipalities, so that we know who was elected and who was not.

3.4.1 *A split sample approach*

As indicated in the introduction, to deal with concerns about specification search and reporting bias, we have randomly split our data in two halves. The first half (*training set*) is used to narrow down the list of hypotheses we want to test. Once the list is finalized, they will be applied to the second half (*testing set*) and these are the results that will be reported in the published version of this paper. The purpose of this approach is to provide credible estimates free of specification search and reporting bias. It is best suited to studies that can take advantage of large samples.¹⁰ This appears especially relevant given the ease with which large scale datasets are becoming available (Einav and Levin 2013). By allowing us to learn from the first sample, our approach reduces concerns that pre-analysis plans might ‘stifle innovation’ (Casey, Glennerster, and Miguel 2012, Deaton

¹⁰ In an RCT-based study, while it might be too costly to increase the sample size to allow for our approach to be used, it might be possible to set aside some of the data to run some preliminary analyses (Heller, Rosenbaum, and Small 2009, Rosenbaum 2012, Zhang, Small, Lorch, Srinivas, and Rosenbaum 2011).

2012). It is related to the strategy advocated by Humphreys, Sanchez de la Sierra, and van der Windt (2013).¹¹

The exact procedure followed is as follows. After having put the data together, we wrote a program to split the sample into two randomly generated halves. For a number of variables, intra-cluster correlations within households and villages is relatively high. Hence, to minimize the chance that the two halves may be too correlated, we sample villages, rather than individuals or households. We sent the program along with the dataset to a third party who generated the two random samples. He sent us the first sample and kept the second one. Importantly, the program used to generate the samples generates new provincial, municipal, village, household, and individual IDs. As a result, at no point are we able to reconstruct the second sample from the data we have access to. We use the first sample to narrow down the set of hypotheses we want to test and to fully specify the associated regressions. The published version of the paper will report results from regressions estimated on the second sample.

3.4.2 *Family ties*

We take advantage of naming conventions in the Philippines to assess blood and marriage links between surveyed individuals and local politicians.¹² Names used in the Philippines were imposed by Spanish colonial officials in the mid-19th century. One of the stated objective was to distinguish families at the municipal-level to facilitate census-taking and tax collection (Scott 1998, Gealogo 2010). Last names were selected from the *Catalogo alfabetico de apellidos*, a list of Spanish

¹¹ These authors argue that researchers carrying out RCTs should write mock reports with fake data before the real data become available in order to distinguish between *exploratory analyses* and *genuine tests* (Humphreys, Sanchez de la Sierra, and van der Windt 2013). The main advantage of our approach is that, since we are using real data, we are able to incorporate results from exploratory analyses in our analysis plans.

¹² To be clear, we realize that not all people who are related by blood or by marriage have strong social links. The interested reader should think of our results as ITT. Mean effects are probably stronger.

names and thus do not reflect pre-existing family ties. In each municipality a name was only given to one family. As a result, there is a lot of heterogeneity in names used at the local level, reducing concerns that names capture similar ethnic background or other group membership. Names are transmitted across generations according to well-established rules inspired from Spanish naming conventions. Specifically, a man's last name is his father's last name and his middle name is his mother's last name. Similar conventions apply to unmarried women. A married woman has her husband's last name and her middle name is her maiden name, *i.e.*, her father's last name.¹³

The dataset includes information on the middle and last names of all individuals surveyed. Using this information, an individual is classified as being related to a given politician if someone in her household has a middle or last name matching the politician's middle or last name. Angelucci, De Giorgi, Rangel, and Rasul (2010) use a similar strategy to measure family networks in Mexico. They find evidence consistent with the argument that PROGRESA beneficiaries shared some of their resources with family members who did not participate in the program (Angelucci, De Giorgi, Rangel, and Rasul 2010). The strategy has also been used to assess blood links between provincial-level Filipino politicians through time (Querubin 2011, Querubin 2013). Allesina (2011) used shared last names to measure nepotism in Italian academia.

In our sample, sharing a last or a middle name is a good indicator of family ties. This could be challenged if names were too common. For example, if individuals from the same ethnic group all shared the same last name, results would

¹³ Importantly, Article 376 of the Civil Code of the Philippines (Republic Act No. 386, 1949) states that *No person can change his name or surname without judicial authority*. This has been upheld in a number of court cases which have sometimes reached the Supreme Court. For example, in the majority decision in the case *Wang v. Cebu City Civil Registrar* (G.R. No. 159966, 30 March 2005, 454 SCRA 155.), Justice Tinga wrote: *The Court has had occasion to express the view that the State has an interest in the names borne by individuals and entities for purposes of identification, and that a change of name is a privilege and not a right, so that before a person can be authorized to change his name given him either in his certificate of birth or civil registry, he must show proper or reasonable cause, or any compelling reason which may justify such change. Otherwise, the request should be denied*. This reduces concerns about strategic name changes.

capture ethnic ties rather than family connections. In our sample municipalities, there are an average of 5,998 names used (median 5,126). There is also a great diversity of names. We compute a Herfindhal index of name heterogeneity, computed as $1 - \sum s_i^2$ where s_i is the share of households in the municipality using name i . The index is higher than 96.4 percent in all municipalities, indicating a high level of heterogeneity.

The method described above generates a credible number of family ties. The average political candidate is found to be connected to 70 individuals aged 20-80 in his/her municipality.¹⁴ While this may appear large at first, it is consistent with the way middle and last names are transmitted across generations. To illustrate, take the conservative estimate that the parents of each candidate had 3 children who in turn had two children each. With these assumptions, each candidate would be directly connected to 14 individuals. If in addition, each of his/her parent has two siblings, with three children each, a candidate would be indirectly connected to 56 individuals; for a total of 70 individuals.

There are two sources of measurement error in our measure of family ties. First, it is possible that non-related households share the same last name. As explained earlier, this potential source of error is reduced in our data due to the mid-19th century renaming of all citizens. Second, data entry errors might have led to some names being mis-spelled (*e.g.*, De Los Reyes spelled De Los Reytez). Those sources of measurement errors generate an attenuation bias that works *against* rejecting the null of no effect.

3.4.3 Descriptive statistics

The NHTS data include information on the occupation of all individuals surveyed. The classification, developed by the National Statistics Office for its reg-

¹⁴ Those statistics were computed using the full sample.

ular Labor Force Surveys, include 11 occupations.¹⁵ We rank them according to their average daily wage. To compute average wage by occupation, we use wage data from 8 nationally representative Labor Force Surveys collected in 2008 and 2009, and we use only those 700 municipalities with full enumeration in the NHTS-PR.¹⁶

Descriptive statistics on employment by occupation are displayed in Table (3.1) and in Figure 3.1. Simple comparisons reveal stark differences between individuals related to office holders and the rest of the population. For example, 3.2 percent of the individuals connected to successful candidates in the 2007 elections are employed in a managerial role, compared to 2 percent in the population as a whole. The Table already suggests that a non-negligible share of this difference may be due to unobserved heterogeneity correlated with political connections: among individuals related to successful candidates in the 2010 elections who did not run in 2007, 2.4 percent are employed in a managerial role, which is 20 percent more than in the general population.

¹⁵ During the first few months of NHTS-PR survey collection, a different list of occupation was used. Given that the two classifications cannot be reconciled, we restrict our sample to the data collected with the Labor Force Surveys classification. This leaves us with data on 562 municipalities.

¹⁶ The ranked categories are: 0-None; 1-Laborers and Unskilled Workers; 2-Farmers, Forestry Workers and Fishermen; 3-Service Workers and Shop and Market Sales Workers; 4-Trades and Related workers; 5-Plant and Machine Operators and Assemblers; 6-Clerks; 7-Technicians and Associate Professionals; 8-Special Occupations; 9-Professionals; 10-Officials, Managers, Supervisors.

Table 3.1: Descriptive statistics: Individual-level

	Full Sample			Control Group		
	Sample	Connected		I	II	III
Occupation						
None	40.28	40.70		40.74	40.27	40.49
1 Laborers, Unskilled Workers	15.46	14.14		14.86	14.56	14.87
2 Farmers, Forestry Workers, Fishermen	28.80	26.08		27.25	28.64	28.25
3 Service, Shop, Market Sales Workers	5.06	5.53		5.45	5.12	4.77
4 Trades, Related workers	2.04	2.25		2.06	2.09	2.17
5 Plant, Machine Operators, Assemblers	1.46	1.45		1.48	1.47	1.46
6 Clerks	0.59	0.86		0.75	0.65	0.69
7 Technicians, Associate Professionals	0.60	0.73		0.72	0.68	0.66
8 Special Occupations	1.27	1.33		1.20	1.29	1.22
9 Professionals	2.41	3.67		3.08	2.89	3.01
10 Officials, Managers, Supervisors	2.04	3.26		2.42	2.34	2.40
Controls						
Age	39.26	40.16		39.77	39.46	39.66
Education (years)	8.17	9.04		8.68	8.48	8.52
Female	0.49	0.50		0.50	0.49	0.49
Observations	3,917,712	394,490		390,294	239,926	59,195

Notes: Control group I includes relatives of unsuccessful candidates in the 2007 elections, Control group II includes relatives of candidates in the 2010 elections that did not run in 2007 and Control group III includes relatives of successful candidates in the 2010 elections that did not run in 2007.

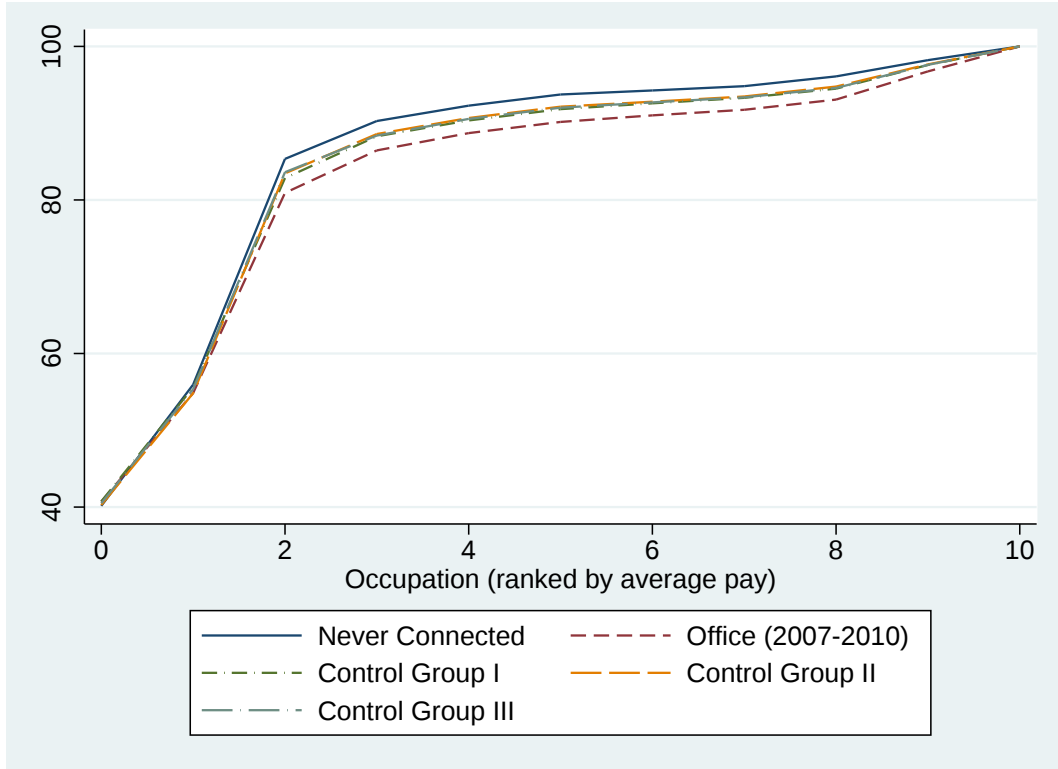


Figure 3.1: Distribution of occupations

Notes: Control group I includes relatives of unsuccessful candidates in the 2007 elections, Control group II includes relatives of candidates in the 2010 elections that did not run in 2007 and Control group III includes relatives of successful candidates in the 2010 elections that did not run in 2007.

3.5 ECONOMETRIC RESULTS

Descriptive statistics may be misleading because they do not control for covariates. We thus turn to econometric analysis. Our objective is to assess the impact of family ties to local politicians on the probability of being employed in a better paying occupation. To this effect, we create a series of 10 dummy variables Y_{ij}^p equal to one if individual i in municipality j is employed in at least occupation p . We estimate equation (3.1) for all Y^p .

Table 3.2: Descriptive statistics: Municipal-level

	Mean	Std Dev.	Min	Max
Population	32,782	28,400	1,240	322,821
Poverty incidence	41.47	11.54	5.140	72.32
p.c. Fiscal transfers	2.33	1.60	0	14.46
Gini	0.29	0.04	0.17	0.37
2007 Mayoral Election				
Nb Candidates	2.56	1.16	1	9
Vote margin	32.14	33.42	0.05	100
Winner's previous experience	1.99	1.83	0	6
Incumbent lost	0.37	0.48	0	1

3.5.1 *Main results*

We begin by reporting naive OLS estimates using the full sample. Results indicate that individuals connected to politicians in office are more likely to be employed in better paying occupations (Table 3.3). For example, a randomly selected individual related to an elected local official is 1.45 percentage-points more likely to be employed in a managerial role than the average citizen. This represents an increase of about 70 percent of the baseline mean. As a point of comparison, we plot the point estimates along with the cumulative distribution of occupations for the full sample and for individuals connected to successful candidates in the 2007 elections (Figure 3.2). The point estimates for each occupation level correspond to the difference between the two cumulative distributions.

As shown in Panels A and B of Table (3.3), a large share of this difference can be attributed to observable characteristics. Depending on the outcome of interest, the inclusion of additional controls reduces point estimates by 0.5-0.75 percentage points. For example, when we control for age, gender, and education levels, the point estimates on the impact of connections on the probability of being employed in a managerial role drops to 0.75 percentage-points. Adding further controls does not affect the point estimates. To make this point clearer, we plot the point estimates obtained with and without individual controls (Figure 3.3).

Table 3.3: Effects of connections using the full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.0040*** (0.002)	0.0049*** (0.002)	0.0415*** (0.002)	0.0377*** (0.002)	0.0355*** (0.002)	0.0348*** (0.002)	0.0322*** (0.002)	0.0307*** (0.002)	0.0280*** (0.002)	0.0145*** (0.001)
Observations	3,917,712	3,917,712	3,917,712	3,917,712	3,917,712	3,917,712	3,917,712	3,917,712	3,917,712	3,917,712
R-squared	0.022	0.052	0.037	0.026	0.023	0.023	0.022	0.022	0.014	0.010
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0074*** (0.001)	-0.0048*** (0.001)	0.0113*** (0.001)	0.0112*** (0.001)	0.0104*** (0.001)	0.0103*** (0.001)	0.0096*** (0.001)	0.0099*** (0.001)	0.0091*** (0.001)	0.0075*** (0.001)
Observations	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706
R-squared	0.283	0.233	0.197	0.208	0.229	0.260	0.242	0.229	0.239	0.068
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.002 (0.001)	-0.001 (0.001)	0.0125*** (0.001)	0.0121*** (0.001)	0.0110*** (0.001)	0.0107*** (0.001)	0.0100*** (0.001)	0.0103*** (0.001)	0.0094*** (0.001)	0.0077*** (0.001)
Observations	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706	3,917,706
R-squared	0.347	0.275	0.200	0.211	0.231	0.261	0.243	0.230	0.240	0.069

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

As explained in the conceptual section, we now compare these results to those obtained using different control groups I, II and III. When we use control group I – *i.e.*, the relatives of unsuccessful 2007 candidates – we obtain qualitatively similar results to Table (3.3). A point made clearer by the comparison of the top left and top right corners of Figure 3.4. But point estimates are lower than the ones obtained on the full sample, a finding consistent with the argument that bias μ is positive: depending on the outcome of interest, point estimates fall by 29 to 40 percent (Panels B of Table 3.3 and A3.1). For example, political family ties are now associated with a 0.5 percentage-point increase in the probability of being employed in a managerial role.

Next we use control group II, in which the relatives of 2010 candidates that did not run in 2007 are compared to those of successful 2007 candidates. The purpose is to avoid including in the estimate of political connections the potential cost suffered by individuals connected to unsuccessful candidates. Results, shown in the bottom left corner Figure 3.4 and in Table (A3.2), continue to associate family ties to elected officials with better paid occupations. As a point of comparison, we also estimate the regressions on the sample of individuals connected to unsuccessful candidates in 2010 that did not run in 2007, and find similar results (Table A3.4).

Finally we further restrict the control group only to those individuals connected to *successful* candidates in the 2010 elections but who did not run in 2007. This is control group III. Results, presented in the bottom right corner of Figure 3.4 and in Table (A3.3), confirm that individuals connected to currently elected local officials are more likely to be employed in better paid occupations. Although apparently small in magnitude, the effect is economically significant: individuals connected to current office holders are 11 percent more likely than individuals in the control group to be employed in either a professional or managerial position and 22 percent more likely to be employed in a managerial position.

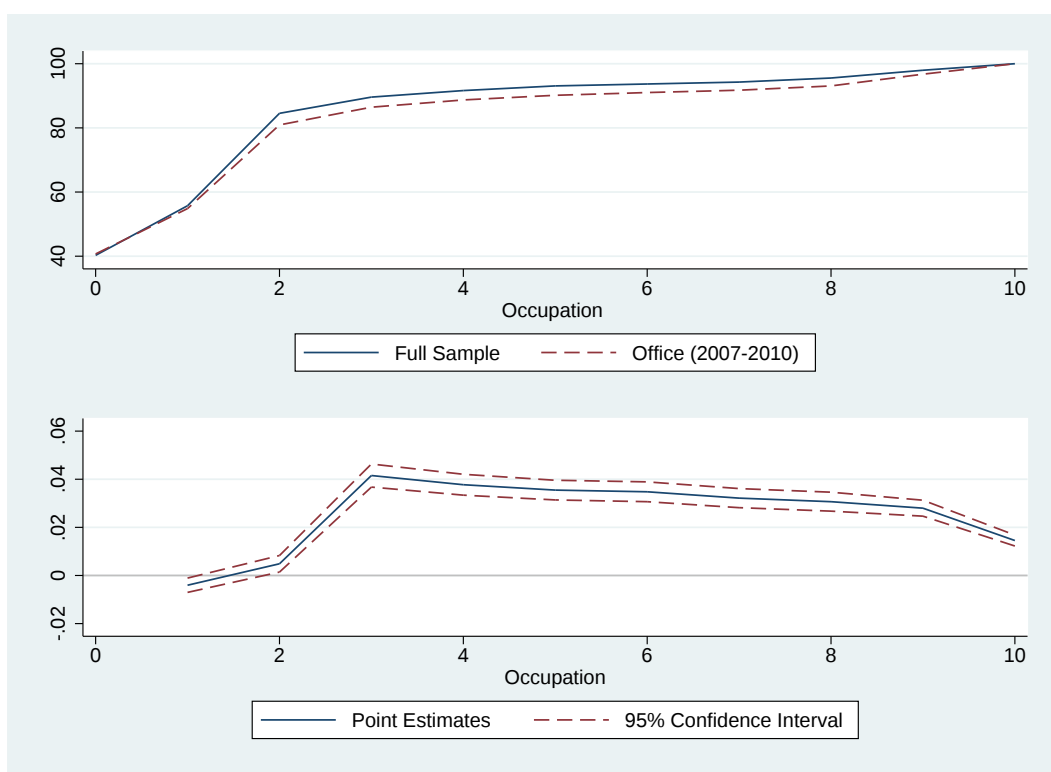


Figure 3.2: Estimated effects of connections (full sample)

Notes: Cumulative distribution of occupation for the full sample and for relatives of politicians elected in 2007 (upper panel). Results from municipal fixed-effects regressions (lower panel). The standard errors used to generate the 95% confidence intervals account for potential correlation within province. Associated results are reported in Panel A of Table 3.3.

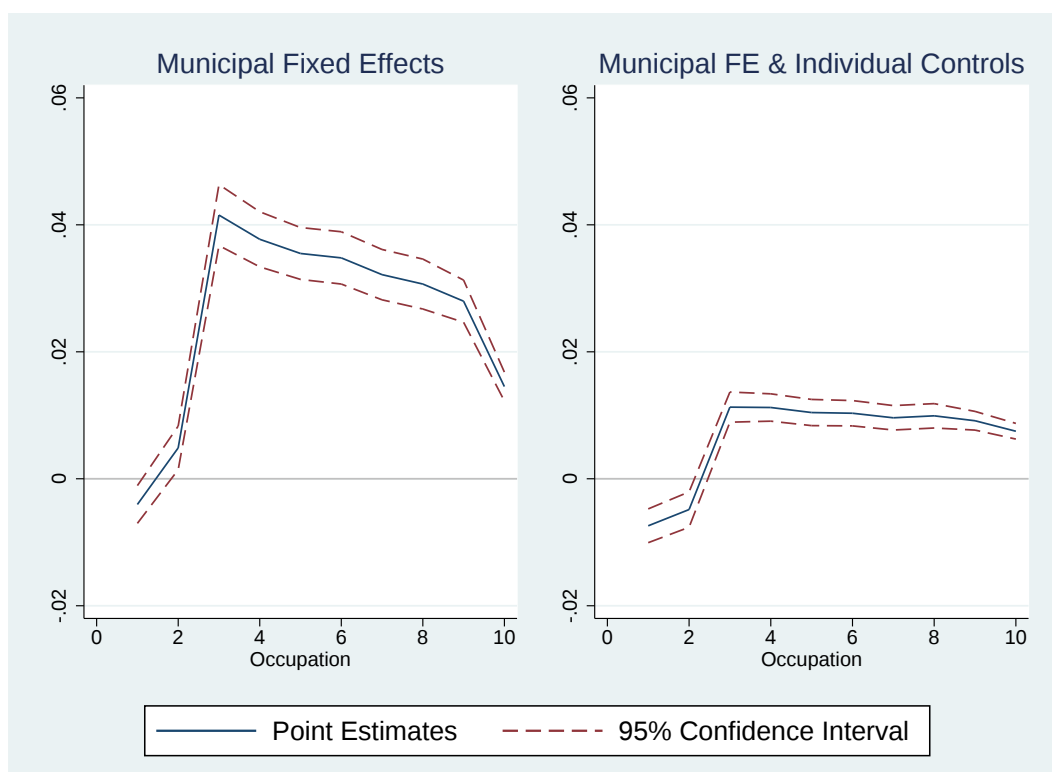


Figure 3.3: Estimated effects of connections with and without individual controls (full sample)

Notes: Results from municipal fixed-effects regressions. Regressions in the right-panel include a full set of dummies for age, education level and gender. The standard errors used to generate the 95% confidence intervals account for potential correlation within province. Associated results are reported in Panels A and B of Table 3.3.

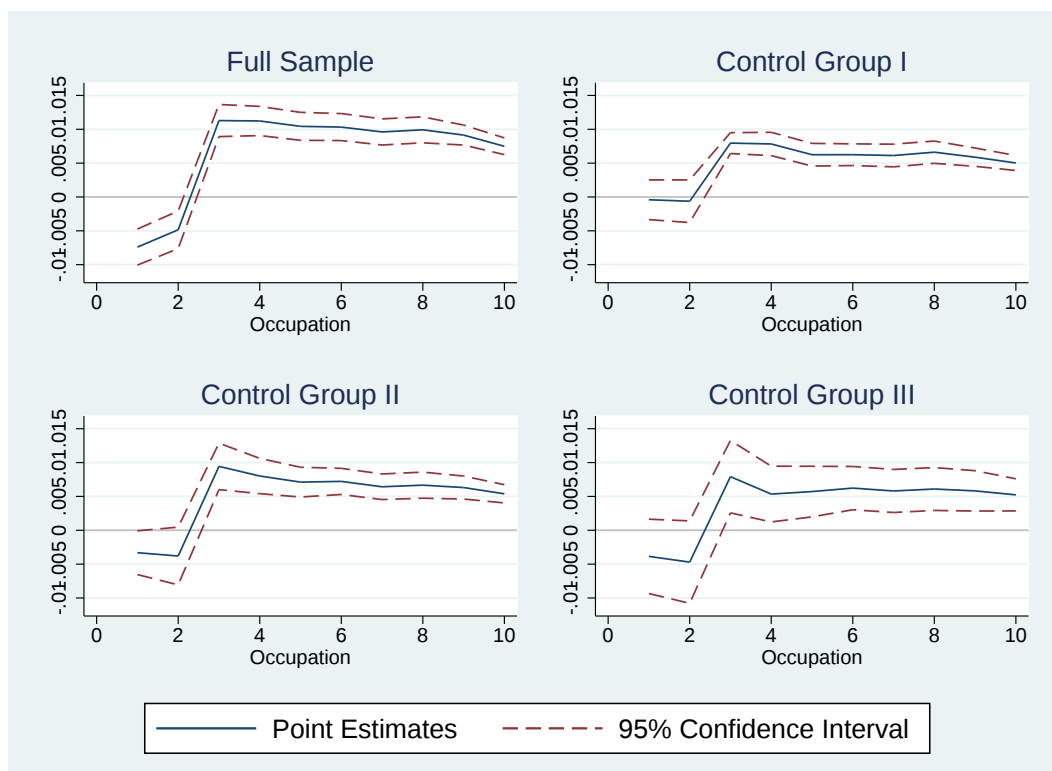


Figure 3.4: Estimated effects of connections with various control groups
Notes: Results from municipal fixed-effects regressions. Control group I includes relatives of unsuccessful candidates in the 2007 elections, Control group II includes relatives of candidates in the 2010 elections that did not run in 2007 and Control group III includes relatives of successful candidates in the 2010 elections that did not run in 2007. All regressions include a full set of dummies for age, education level and gender. The standard errors used to generate the 95% confidence intervals account for potential correlation within province. Associated results are reported in Panel B of Tables 3.3 and A3.1-A3.3.

3.5.2 Discussion and interpretation

What do we learn from comparing the estimates we obtained using different control groups? First, as anticipated, an upward bias seems to arise when we estimate the impacts of political connection without adequately controlling for unobserved heterogeneity μ : point estimates obtained with naive OLS are 50 to 70 percent higher than those obtained using control group I (Panels B of Table 3.3 and A3.1); similar results are obtained with control groups II and III, which also control for μ .

Second, estimated impacts are larger with control group II than with control group I. This is contrary to expectations: if relatives of unsuccessful 2007 candidates are punished by successful 2007 candidates, we would expect the opposite. One possible explanation is that control group II only includes non-repeat candidates: repeat candidates may enjoy higher social standing and their relatives might have a better occupation, and their omission from control group II results in a larger estimated impact of political connections.

Third, control groups I and III provide point estimates of similar order of magnitude. At first glance, this suggests that η is close to zero and that the relatives of unsuccessful 2007 candidates do not suffer from their ties to an unlucky challenger. It remains possible, however, that such costs are only suffered by a small number of individuals, *i.e.*, relatives of candidates who *almost* won the 2007 elections. To shed light on this, we estimate the value of political connections by applying a non-parametric regression discontinuity design (RDD) to close elections. This approach, which has been used to estimate the private returns to holding office (Eggers and Hainmueller 2009, Fisman, Schulz, and Vig 2013) relies on the assumption that (relatives of) politicians who were narrowly defeated are most comparable to (relatives of) narrowly elected politicians.

To investigate this approach, we use data on the breakdown of votes by candidate for the 2007 mayoral and vice-mayoral elections. Specifically, let Y_{ij} be the outcome of interest for household i in village j . We estimate a model of the form:

$$Y_{ij} = \alpha C_{ij} + f(V_{ij}) + u_{ij} \quad (3.4)$$

where f is an unknown smooth function, V_{ij} is the vote margin of victory or defeat for successful or unsuccessful candidates, respectively, and u_{ij} is an error term. We follow Imbens and Lemieux (2008) and estimate equation (3.4) non-parametrically, using the optimal bandwidth recommended by Imbens and Kalyanaraman (2012). This is implemented in Stata using the `rd` command developed by Nichols (2011). Equation (3.4) is estimated with three different neighborhoods of the cutoff point, *i.e.*, using relatives of candidates with a 2007 vote margin of ± 10 percent, ± 5 percent, and ± 2.5 percent.

The point estimates of the value of political connections obtained through RDD are much higher than the ones obtained previously (Table A3.5).¹⁷ The most conservative RDD estimate suggests that connections increase the likelihood of being employed in a managerial role by 2.46 percentage-points. As point of further comparison, we estimate equation (3.1) on the sample of individuals related to candidates for either mayor or vice-mayor in the 2007 elections (Table A3.6). The RDD estimates are 3.15 times larger than the regression point estimates on that subsample. To highlight the differences between the two estimation strategies we plot the point estimates obtained through RDD and through fixed-effect regressions (Figure 3.5).

We interpret these findings as indicating that individuals related to candidates who were narrowly defeated tend to be penalized, perhaps because they are eliminated from local positions of power by the narrowly elected candidate. A graphical representation of the results brings this point home. Figure 3.6 indicates that, starting with candidates

¹⁷ We get similar results with different samples (Table A3.7).

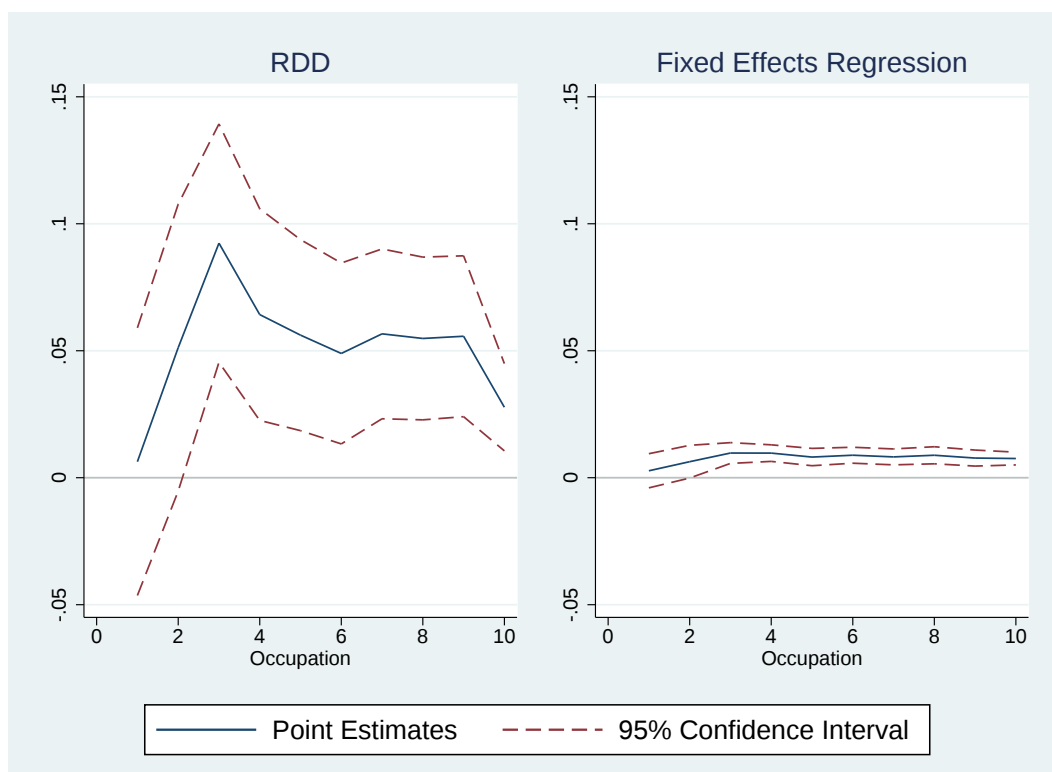


Figure 3.5: Estimated effects of connections with RDD and with fixed-effects regressions. Notes: Results from nonparametric regressions (left panel) and fixed effects regressions (right panel). Associated results are reported in Panel A of Table A3.5 and Panel B of Table A3.6

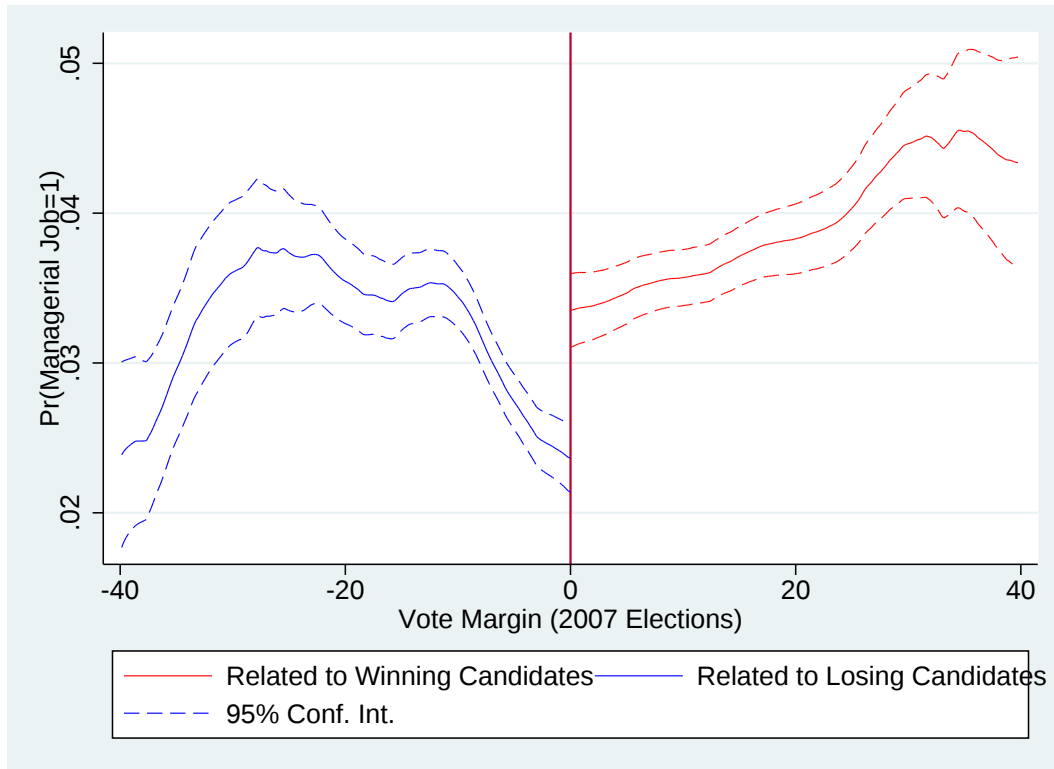


Figure 3.6: Non-parametric estimates of the probability of being employed in a managerial position

who lost the elections by about 10 percentage points, there is a significant drop in the probability of being employed in a managerial role.

Based on the above evidence, we conclude that control group III provides the most credible estimates of the impacts of family ties to elected local officials. Consequently, the robustness checks presented in the next sub-Section focus on that control group.

Before turning to robustness checks, we test for the impact of family ties on each occupation separately (Table A3.8). We only find significant effects for two occupations: relatives of local politicians are less likely to be employed as farmers (the second lowest paid occupation) and more likely to be employed in a managerial position (the highest paid occupation). Since it is unlikely that farmers get assigned to managerial posts, what our results suggest is that there is a shift of connected individuals from lower to higher occupations across the whole spectrum, so that flows in and out of each intermediate occupation cancel each other. This confirms that connected individuals benefit from their ties to local politicians across the whole range of occupations.

3.5.3 *Robustness checks*

In this sub-section we verify the robustness of our results to various potential threats to our identification strategy and interpretation of the results.

First, strict term limits were introduced in 1987, but political families in some municipalities circumvent them by having different members of the same family take turns in office. In these municipalities, relatives of candidates elected in 2010 might not be valid counterfactuals for current office holders.¹⁸ To check whether this affected our results, we re-estimate equation (3.1) focusing on municipalities where the mayor's family has been in office for three terms or less (Panel A of Table A3.9), two terms or less (Panel B of Table A3.9) and one term (Panel C of Table A3.9). As expected the point estimates tend to be smaller, but they remain economically and statistically significant. They tend to be located at the top of the distribution of occupations. For example, in the subsample of municipalities where the mayor is in his first term, relatives of the current office holder are 0.31 percentage-points more likely to be employed in a managerial role, an increase that corresponds to 13 percent of the baseline probability in these municipalities. It is important to note that those estimates are likely to be downward biased as the benefits of connections might not materialize instantaneously and most of the data were collected between six months and two years after the 2007 elections.

Second, we introduced control variables flexibly by generating a different dummy for each value of those variables. Still, the model does not allow for possible interactions between control variables such as age, education, and gender. To verify whether this affected the results, we estimate an alternative model in which all explanatory variables are interacted with the gender dummy. We also estimate a fully saturated model for age, gender and education levels. We also implement versions of the model in which all interacted terms are themselves interacted with either province or municipal dummies. The most saturated specification is akin to a matching estimator: identification comes

¹⁸ This issue is discussed in detail in Ferraz and Finan (2011).

from comparing connected individuals of the same gender, age, and education living in the same municipality. Point estimates, reported in Table (A3.10), are smaller but still economically and statistically significant. For example, in the most restrictive regression, being connected to an elected official leads to a 0.36 percentage-points increase in the probability to be employed in a managerial role.

Third, we worry that occupation may not depend on someone's absolute education level, but rather on their education relative to others in the municipality, a situation that would arise in the presence of localized labor markets. Because we have access to census data in each municipality, we are able to control for each individual's relative educational rank in their municipality of residence. Including those variables in equation (3.1) does not affect our results of interest (Table A3.11).

Fourth, if connected individuals live in areas where returns to education are higher, this could lead us to overestimate the value of connections to elected officials. To deal with this concern, we estimate equation (3.1) allowing for either province, municipality, or village-specific returns to education. As shown in Table (A3.12), point estimates and significance levels are unaffected.

Fifth, connected individuals may live disproportionately in villages where the incumbent vote share was high in past elections. This would introduce a possible confound because α would capture the value of political ties as well as the possible advantage of living in a village that supports the incumbent. To investigate this possibility, we re-estimate equation (3.1) including village fixed-effects. As shown in Panel A of Table (A3.13), this does not affect the estimated value of α .

Sixth, we re-estimate equation (3.1) including enumerator $\times$ municipality fixed-effects to capture potential enumerator effects. Results are robust to this change (Panel B of Table A3.13). Another concern is that local officials might have been able to influence data collection to favor their relatives. Given that the NHTS-PR data were collected for enrollment in an antipoverty program, this bias would work against rejecting the null of

no effect: connected individuals would have incentives not to report working in a better paying occupation to appear poorer than they are. This is not what we find.

Seventh, we re-estimate equation (3.1) using probit instead of a linear probability model. The results are presented in Panel C of Table (A3.13). For most outcomes the point estimates are of similar order of magnitude, although they are smaller for professional and managerial occupations.

Eighth, we have so far used the full sample of individuals aged 20-80. It is however possible that older relatives of elected officials may retire earlier, which would bias our estimates downwards. By a similar reasoning, younger relatives of politicians may postpone entry on the job market. To check for this possibility, we re-estimate equation (3.1) excluding either younger or older cohorts. Estimates are reported in Table (A3.14). When we drop the top 10 percent of the age distribution, results are similar to the ones obtained previously. When we drop the bottom 10 percent of the age distribution, this strengthens our results: coefficient estimates go up from 0.54 percentage-points to 0.60 percentage-points.

Ninth, we have assumed that errors are correlated within provinces. We cannot rule out the possibility that even after controlling for month $\times$ year dummies, there remains some correlation in errors for individuals interviewed at the same time. To investigate whether this affects our results, we report equation (3.1) results where we cluster standard errors along both month $\times$ year and province, using a method developed Cameron, Gelbach, and Miller (2011). Our results, shown in Table (A3.15), are basically unchanged.

Finally, some of the NHTS-PR data were collected before the elections but after the date candidates had to announce their candidacy (*i.e.*, November 2009). If incumbents were able to punish the relatives of now known challengers, our results would be downward biased. To check for this possibility, we re-estimate equation (3.1) on the sample of individuals who were interviewed before November 2009. Again, results are robust to using this restricted sample (Panel A of Table A3.16). Following the same logic, incumbents might be able to find out the identity of individuals likely to challenge them

before they officially announce their candidacy. If that was the case, one would expect the estimated effects of connections to be higher the closer to November 2009 the data were collected as it would now include the potential punishment of being connected to a known challenger. To test for that, we interact the connection dummy with the length of time (in months) between the day the data were collected and the elections. We are unable to reject the null hypothesis that the interaction term is zero (Panel B of Table [A3.16](#)).

3.5.4 *Heterogeneity*

Having confirmed the robustness of our findings to a number possible confounding effects, we investigate whether the value of political ties varies with the type of elected official. To this effect, we estimate equation (3.2) with all possible interactions between three dummies capturing links to a mayor, a vice-mayor or a municipal councilor. We then compute the marginal effects for each dummy. Results are shown in Table (3.4). The estimated impacts of a family tie to the mayor tend to larger than for vice-mayors and municipal councilors. Furthermore, they are concentrated in the top of the occupational distribution. Relatives of the mayor are 0.78 percentage-points more likely to be employed in a managerial position; the point estimate for relatives of municipal councilors is 0.42 percentage-points, a difference that is statistically different from zero at the 10 percent level.

Table 3.4: Marginal effects of connections to each type of elected official with all possible interactions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects and Individual Controls (2)										
A: Connected Mayor (2007)	0.002 (0.003)	-0.001 (0.004)	0.0154*** (0.002)	0.0141*** (0.002)	0.0131*** (0.002)	0.0124*** (0.002)	0.0107*** (0.002)	0.0109*** (0.002)	0.0093*** (0.002)	0.0078*** (0.002)
B: Connected Vice-Mayor (2007)	0.002 (0.004)	0.004 (0.004)	0.0099*** (0.003)	0.0083*** (0.003)	0.0061** (0.002)	0.0068*** (0.002)	0.0059*** (0.002)	0.0064*** (0.002)	0.0058** (0.003)	0.0055*** (0.002)
C: Connected Councilor (2007)	0.000 (0.003)	-0.002 (0.003)	0.0086*** (0.003)	0.0062*** (0.002)	0.0055*** (0.002)	0.0056*** (0.001)	0.0050*** (0.001)	0.0052*** (0.001)	0.0045*** (0.001)	0.0042*** (0.001)
Test Ho: A = B	0.020	0.887	1.853	2.349	5.379	3.599	2.675	2.110	1.182	0.876
Ha: A $\neq$ B [p-value]	[0.888]	[0.346]	[0.173]	[0.125]	[0.020]	[0.058]	[0.102]	[0.146]	[0.277]	[0.349]
Test Ho: A = C	0.274	0.014	4.265	6.121	8.776	8.084	4.898	5.262	3.815	3.529
Ha: A $\neq$ C [p-value]	[0.600]	[0.904]	[0.039]	[0.013]	[0.003]	[0.004]	[0.027]	[0.022]	[0.051]	[0.060]
Test Ho: B = C	0.061	1.244	0.123	0.549	0.044	0.265	0.135	0.257	0.194	0.304
Ha: B $\neq$ C [p-value]	[0.805]	[0.265]	[0.726]	[0.459]	[0.833]	[0.607]	[0.713]	[0.613]	[0.659]	[0.582]
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685

Notes: Mean marginal effects from fixed-effects regressions. The regressions include all possible interactions of the three dummies. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panel A, all regressions include a full set of dummies for age, education level, gender, relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Next we investigate whether the occupational benefit from family connections varies with observable individual characteristics. To this effect, we interact the family tie dummy with gender, age, and education. As is clear from Table (3.5), we find evidence of significant heterogeneity. First, the benefit from political connections is stronger for more educated individuals: each additional year of education is associated with a 0.12 percentage-point increase in the likelihood of being in a better paid occupation. Second, the impact of family ties on the probability of being employed in a managerial position is 40 percent lower for women than it is for men. While male relatives of local politicians are less likely to be employed, no such effect is observed for female relatives. For all other occupations, we find no significant difference between men and women. Third, the impacts of connections appear to be increasing with age.

Table 3.5: Individual heterogeneity: Age, education and gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects and Individual Controls (1)										
Connected	-0.0137*** (0.004)	-0.0129** (0.006)	0.0086** (0.004)	0.0082*** (0.003)	0.0074*** (0.003)	0.0078*** (0.002)	0.0076*** (0.002)	0.0075*** (0.002)	0.0063*** (0.002)	0.0074*** (0.002)
Connected*Female	0.0198** (0.008)	0.017 (0.010)	-0.001 (0.006)	-0.0051* (0.003)	-0.003 (0.003)	-0.002 (0.002)	-0.003 (0.002)	-0.002 (0.002)	-0.000 (0.002)	-0.0037** (0.001)
Connected*Edu	0.000 (0.001)	0.001 (0.001)	0.001 (0.001)	0.0011* (0.001)	0.0013** (0.001)	0.0013** (0.000)	0.0011** (0.000)	0.0011** (0.000)	0.0010** (0.000)	0.0012*** (0.000)
Connected*Age	-0.000 (0.000)	-0.0003* (0.000)	0.0003** (0.000)	0.0002* (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.271	0.230	0.229	0.243	0.266	0.295	0.275	0.263	0.263	0.086
Panel B: Municipal Fixed Effects and Individual Controls (2)										
Connected	-0.0084** (0.004)	-0.009 (0.006)	0.0096*** (0.004)	0.0090*** (0.003)	0.0080*** (0.003)	0.0083*** (0.002)	0.0080*** (0.002)	0.0078*** (0.002)	0.0066*** (0.002)	0.0076*** (0.002)
Connected*Female	0.0137* (0.008)	0.012 (0.010)	-0.001 (0.006)	-0.0056* (0.003)	-0.003 (0.003)	-0.003 (0.002)	-0.003 (0.002)	-0.002 (0.002)	-0.000 (0.002)	-0.0037** (0.001)
Connected*Edu	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)	0.0010* (0.001)	0.0013** (0.001)	0.0012** (0.000)	0.0011** (0.000)	0.0011** (0.000)	0.0010** (0.000)	0.0012*** (0.000)
Connected*Age	-0.0003* (0.000)	-0.0003* (0.000)	0.0003** (0.000)	0.0002* (0.000)	0.0002*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)	0.0003*** (0.000)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels A and B, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel B, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

We then relax the assumption that the relationship between education levels and the value of political connections is linear and we estimate the value of political connections separately for each education level. In Figure (3.7) we plot each point estimate and their associated 95 percent confidence interval, which shows a concave relationship between education level and the value of political connections.

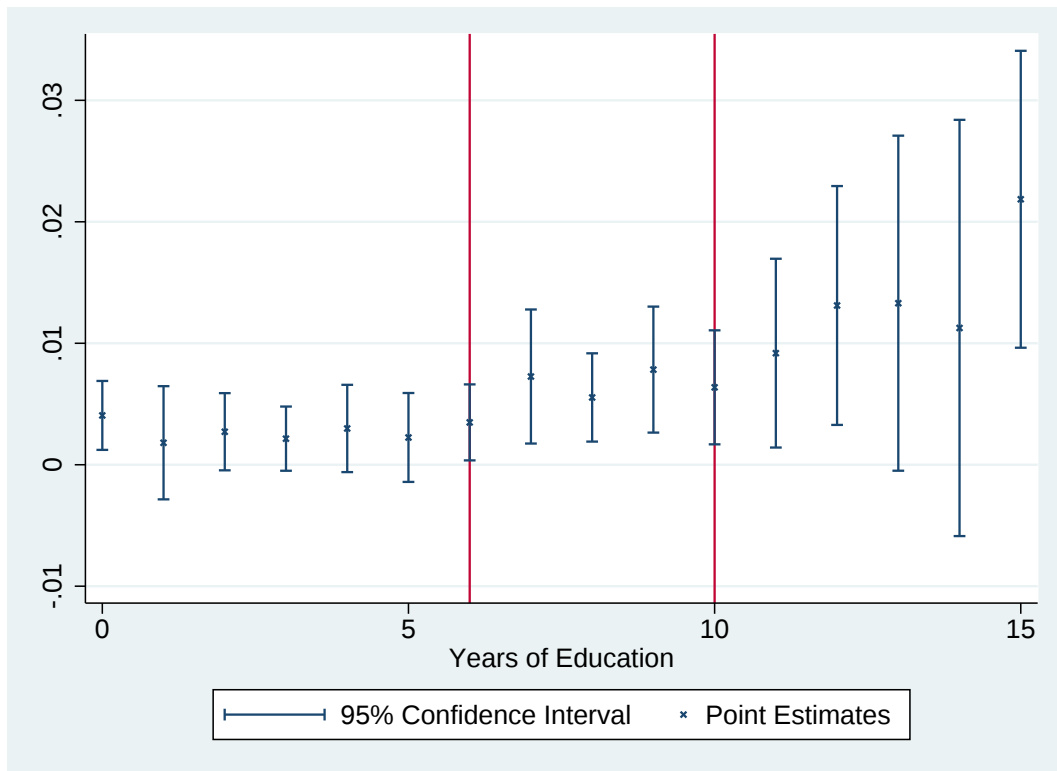


Figure 3.7: Impacts of political connections by education levels

Having examined individual-level heterogeneity, we turn to municipal-level heterogeneity and investigate whether the value of political connections varies systematically with the municipal environment. We first examine the role of per capita fiscal transfers to municipalities. We expect that elected local politicians are better able to favor their relatives in municipalities that receive larger transfers. As shown in Table (3.6), we find that, in municipalities with higher per capita fiscal transfers, relatives of local politicians are less likely to be employed but also more likely to be employed in a managerial position.

Table 3.6: Municipal heterogeneity: Economic variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Poverty Levels										
Connected	-0.002 (0.003)	-0.003 (0.003)	0.0086*** (0.003)	0.0060*** (0.002)	0.0062*** (0.002)	0.0067*** (0.002)	0.0063*** (0.002)	0.0065*** (0.002)	0.0062*** (0.002)	0.0054*** (0.001)
Interaction	0.0004** (0.000)	0.000 (0.000)	0.0004** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Observations	451,692	451,692	451,692	451,692	451,692	451,692	451,692	451,692	451,692	451,692
R-squared	0.330	0.270	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088
Panel B: Fiscal Transfers										
Connected	-0.003 (0.003)	-0.003 (0.003)	0.0084*** (0.003)	0.0058*** (0.002)	0.0062*** (0.002)	0.0064*** (0.002)	0.0060*** (0.002)	0.0062*** (0.002)	0.0063*** (0.002)	0.0057*** (0.001)
Interaction	-0.0086*** (0.003)	-0.0073** (0.003)	-0.001 (0.002)	-0.000 (0.002)	0.000 (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.000 (0.001)	0.0026* (0.001)
Observations	412,730	412,730	412,730	412,730	412,730	412,730	412,730	412,730	412,730	412,730
R-squared	0.330	0.267	0.231	0.246	0.268	0.296	0.276	0.263	0.263	0.088
Panel C: Gini										
Connected	-0.002 (0.003)	-0.003 (0.003)	0.0087*** (0.003)	0.0060*** (0.002)	0.0062*** (0.002)	0.0066*** (0.002)	0.0062*** (0.002)	0.0064*** (0.002)	0.0061*** (0.002)	0.0054*** (0.001)
Interaction	-0.029 (0.089)	0.004 (0.091)	0.100 (0.063)	0.039 (0.055)	0.017 (0.055)	0.021 (0.052)	0.002 (0.050)	-0.001 (0.047)	0.011 (0.033)	0.023 (0.029)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level, gender, relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

We also investigate whether the value of political ties is stronger in municipalities where the mayor's family has been in office longer. Presumably, more entrenched incumbents are in a better position to favor their relatives. This is indeed what we find – see Table (3.7). We also find that the value of connections is lower in municipalities where a larger number of municipal councilors did not run on the mayor's ticket. This could indicate that municipal councils exert a modicum of accountability check. To shed further light on this, we look separately at the effects on individual connected to the mayor, vice-mayor, and municipal councilors. Results, shown in Table (A3.17), indicate that the temporising effects of politically divided municipal councils on the benefits of political connections are concentrated on the relatives of the mayor.

Table 3.7: Municipal heterogeneity: Political variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Nlb of terms										
Connected	-0.002 (0.003)	-0.002 (0.003)	0.0087*** (0.003)	0.0060*** (0.002)	0.0061*** (0.002)	0.0065*** (0.002)	0.0061*** (0.002)	0.0064*** (0.002)	0.0062*** (0.002)	0.0054*** (0.001)
Interaction	0.002 (0.001)	0.001 (0.001)	0.001 (0.001)	0.0020** (0.001)	0.0017* (0.001)	0.0017** (0.001)	0.0018*** (0.001)	0.0015** (0.001)	0.0013** (0.001)	0.0011** (0.000)
Observations	437,659	437,659	437,659	437,659	437,659	437,659	437,659	437,659	437,659	437,659
R-squared	0.328	0.268	0.231	0.247	0.269	0.297	0.277	0.265	0.264	0.088
Panel B: Municipal Council										
Connected	-0.002 (0.003)	-0.003 (0.003)	0.0086*** (0.003)	0.0059*** (0.002)	0.0061*** (0.002)	0.0065*** (0.002)	0.0061*** (0.002)	0.0064*** (0.002)	0.0060*** (0.002)	0.0054*** (0.001)
Interaction	-0.003 (0.007)	-0.011 (0.009)	-0.011 (0.008)	-0.0139** (0.007)	-0.0155** (0.006)	-0.0133** (0.006)	-0.0111* (0.006)	-0.009 (0.006)	-0.006 (0.005)	-0.0083* (0.004)
Observations	452,865	452,865	452,865	452,865	452,865	452,865	452,865	452,865	452,865	452,865
R-squared	0.330	0.270	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088
Panel C: Vote Margin (mayor 2007)										
Connected	-0.001 (0.003)	-0.002 (0.003)	0.0082*** (0.003)	0.0053** (0.002)	0.0055*** (0.002)	0.0060*** (0.002)	0.0055*** (0.002)	0.0058*** (0.002)	0.0060*** (0.002)	0.0053*** (0.001)
Interaction	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Observations	430,845	430,845	430,845	430,845	430,845	430,845	430,845	430,845	430,845	430,845
R-squared	0.331	0.268	0.233	0.248	0.269	0.297	0.277	0.264	0.262	0.086

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level, gender, relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

3.6 CONCLUSION

In this paper, we have provided evidence that family ties to a locally elected politician are associated with a better paid occupation. There are good reasons to believe that this association is causal. In addition to numerous control variables, we have dealt with unobserved heterogeneity by using a variety of control groups, including individuals related to candidates elected in subsequent elections. The effects of political connections on better paid occupation that we find are economically and statistically significant, and they are robust to controlling for a number of individual characteristics, and to using many alternative specifications.

In addition, we are able to identify a cost of being related to an unsuccessful candidate who narrowly missed winning the 2007 local election. This has a number of implications. First, standard RDD estimates obtained by combining winners and losers of an election combine the positive effects of connections and the cost suffered by individuals related to unsuccessful candidates.

Second, when deciding whether to run for local elections, candidates must factor in the negative impact that an unsuccessful bid will have on their relatives. The fear of such negative impact may explain the low level of electoral competition in some municipalities of the Philippines, with a large number of candidates running unopposed in the 2010 elections. In the more than 1,600 cities and municipalities that compose the country, 8% of mayoral candidates and 9% of vice-mayoral candidates ran unopposed.¹⁹ Those candidates only need one vote to win, hereby muting electoral competition.

¹⁹ <http://www.abs-cbnnews.com/-depth/04/30/10/only-one-vote-win> visited on March 18, 2011.

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Table A3.1: Effects of connections using unsuccessful 2007 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	0.001 (0.002)	0.0036** (0.002)	0.0209*** (0.002)	0.0195*** (0.002)	0.0174*** (0.001)	0.0172*** (0.001)	0.0162*** (0.001)	0.0160*** (0.001)	0.0145*** (0.001)	0.0082*** (0.001)
Observations	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784
R-squared	0.330	0.050	0.036	0.026	0.025	0.026	0.025	0.025	0.018	0.015
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.000 (0.001)	-0.001 (0.002)	0.0080*** (0.001)	0.0078*** (0.001)	0.0062*** (0.001)	0.0062*** (0.001)	0.0061*** (0.001)	0.0066*** (0.001)	0.0059*** (0.001)	0.0050*** (0.001)
Observations	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784
R-squared	0.270	0.228	0.220	0.235	0.257	0.285	0.265	0.252	0.254	0.079
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	0.001 (0.002)	0.001 (0.002)	0.0082*** (0.001)	0.0080*** (0.001)	0.0063*** (0.001)	0.0063*** (0.001)	0.0062*** (0.001)	0.0067*** (0.001)	0.0060*** (0.001)	0.0051*** (0.001)
Observations	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784
R-squared	0.330	0.269	0.223	0.238	0.259	0.287	0.267	0.254	0.255	0.080

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.2: Effects of connections using 2010 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.001 (0.002)	0.003 (0.002)	0.0286*** (0.003)	0.0253*** (0.003)	0.0236*** (0.002)	0.0235*** (0.002)	0.0215*** (0.002)	0.0207*** (0.002)	0.0193*** (0.002)	0.0104*** (0.001)
Observations	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.024	0.048	0.039	0.028	0.027	0.028	0.027	0.028	0.018	0.016
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0033** (0.002)	-0.0038* (0.002)	0.0094*** (0.002)	0.0080*** (0.001)	0.0071*** (0.001)	0.0072*** (0.001)	0.0064*** (0.001)	0.0067*** (0.001)	0.0063*** (0.001)	0.0054*** (0.001)
Observations	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.271	0.229	0.223	0.237	0.259	0.288	0.269	0.256	0.257	0.081
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.001 (0.001)	-0.001 (0.002)	0.0100*** (0.002)	0.0085*** (0.001)	0.0075*** (0.001)	0.0075*** (0.001)	0.0067*** (0.001)	0.0070*** (0.001)	0.0066*** (0.001)	0.0056*** (0.001)
Observations	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.332	0.271	0.226	0.240	0.261	0.290	0.270	0.257	0.259	0.083

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.3: Effects of connections using successful 2010 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.001 (0.003)	0.002 (0.003)	0.0231*** (0.004)	0.0192*** (0.004)	0.0190*** (0.003)	0.0194*** (0.003)	0.0181*** (0.003)	0.0176*** (0.003)	0.0165*** (0.003)	0.0094*** (0.002)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.024	0.048	0.038	0.028	0.028	0.029	0.028	0.028	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004 (0.003)	-0.005 (0.003)	0.0079*** (0.003)	0.0054** (0.002)	0.0057*** (0.002)	0.0062*** (0.002)	0.0058*** (0.002)	0.0061*** (0.002)	0.0058*** (0.002)	0.0052*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.271	0.229	0.229	0.243	0.266	0.295	0.275	0.263	0.263	0.086
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.002 (0.003)	-0.003 (0.003)	0.0086*** (0.003)	0.0060*** (0.002)	0.0062*** (0.002)	0.0066*** (0.002)	0.0062*** (0.002)	0.0064*** (0.002)	0.0061*** (0.002)	0.0054*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5%, *** at the 1% level.

Table A3.4: Effects of connections using unsuccessful 2010 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.000 (0.002)	0.004 (0.002)	0.0300*** (0.003)	0.0272*** (0.003)	0.0251*** (0.002)	0.0250*** (0.002)	0.0228*** (0.002)	0.0219*** (0.002)	0.0203*** (0.002)	0.0107*** (0.001)
Observations	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221
R-squared	0.024	0.047	0.039	0.028	0.027	0.028	0.028	0.028	0.019	0.016
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0030* (0.002)	-0.003 (0.002)	0.0097*** (0.002)	0.0088*** (0.001)	0.0076*** (0.001)	0.0077*** (0.001)	0.0068*** (0.001)	0.0070*** (0.001)	0.0065*** (0.001)	0.0053*** (0.001)
Observations	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221
R-squared	0.270	0.228	0.224	0.239	0.261	0.289	0.269	0.256	0.257	0.082
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.000 (0.002)	-0.001 (0.002)	0.0101*** (0.002)	0.0093*** (0.001)	0.0078*** (0.001)	0.0079*** (0.001)	0.0070*** (0.001)	0.0072*** (0.001)	0.0068*** (0.001)	0.0055*** (0.001)
Observations	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221
R-squared	0.331	0.270	0.227	0.241	0.263	0.290	0.271	0.258	0.259	0.084

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.5: Effects of connections using unsuccessful 2007 candidates as a control group - RDD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Sample: + / - 10										
Connected Office (2007)	0.0107 (0.026)	0.0614** (0.027)	0.0965*** (0.022)	0.0651*** (0.020)	0.0541*** (0.018)	0.0468*** (0.017)	0.0605*** (0.017)	0.0554*** (0.016)	0.0631*** (0.015)	0.0246*** (0.009)
Observations	68,825	68,825	68,825	68,825	68,825	68,825	68,825	68,825	68,825	68,825
Sample: + / - 5										
Connected Office (2007)	0.0209 (0.022)	0.0600** (0.027)	0.0937*** (0.024)	0.0652*** (0.021)	0.0567*** (0.019)	0.0511*** (0.018)	0.0590*** (0.017)	0.0552*** (0.016)	0.0607*** (0.016)	0.0268*** (0.009)
Observations	38,989	38,989	38,989	38,989	38,989	38,989	38,989	38,989	38,989	38,989
Sample: + / - 2.5										
Connected Office (2007)	0.0153 (0.024)	0.0466 (0.030)	0.0903*** (0.024)	0.0612*** (0.022)	0.0565*** (0.019)	0.0513*** (0.018)	0.0553*** (0.017)	0.0551*** (0.016)	0.0592*** (0.016)	0.0270*** (0.009)
Observations	18,576	18,576	18,576	18,576	18,576	18,576	18,576	18,576	18,576	18,576

Notes: Results from nonparametric regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.6: Effects of connections using unsuccessful 2007 candidates as a control group (mayors/vice-mayors only)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	0.006 (0.003)	0.0131*** (0.003)	0.0278*** (0.004)	0.0262*** (0.003)	0.0240*** (0.003)	0.0246*** (0.003)	0.0228*** (0.003)	0.0225*** (0.003)	0.0204*** (0.002)	0.0124*** (0.002)
Observations	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229
R-squared	0.027	0.053	0.044	0.034	0.033	0.035	0.034	0.033	0.027	0.023
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	0.003 (0.003)	0.0063* (0.003)	0.0097*** (0.002)	0.0097*** (0.002)	0.0081*** (0.002)	0.0089*** (0.002)	0.0082*** (0.002)	0.0089*** (0.002)	0.0077*** (0.002)	0.0076*** (0.001)
Observations	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229
R-squared	0.269	0.231	0.239	0.257	0.278	0.306	0.286	0.275	0.276	0.093
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	0.004 (0.003)	0.0076** (0.003)	0.0100*** (0.002)	0.0100*** (0.002)	0.0083*** (0.002)	0.0090*** (0.002)	0.0084*** (0.002)	0.0090*** (0.002)	0.0079*** (0.002)	0.0078*** (0.001)
Observations	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229
R-squared	0.334	0.276	0.243	0.261	0.281	0.308	0.288	0.277	0.278	0.096

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.7: Effects of connections using unsuccessful 2007 candidates as a control group - RDD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Full Sample										
Connected Office (2007)	0.0063 (0.027)	0.0512* (0.029)	0.0924*** (0.024)	0.0642*** (0.021)	0.0562*** (0.019)	0.0489*** (0.018)	0.0567*** (0.017)	0.0548*** (0.016)	0.0557*** (0.016)	0.0278*** (0.009)
Observations	177,874	177,874	177,874	177,874	177,874	177,874	177,874	177,874	177,874	177,874
Sample: +/- 50										
Connected Office (2007)	0.0210 (0.022)	0.0596** (0.027)	0.0932*** (0.024)	0.0650*** (0.021)	0.0564*** (0.019)	0.0499*** (0.018)	0.0582*** (0.017)	0.0549*** (0.016)	0.0599*** (0.016)	0.0270*** (0.009)
Observations	160,879	160,879	160,879	160,879	160,879	160,879	160,879	160,879	160,879	160,879
Sample: +/- 20										
Connected Office (2007)	0.0111 (0.026)	0.0614** (0.027)	0.0974*** (0.022)	0.0652*** (0.021)	0.0555*** (0.018)	0.0512*** (0.018)	0.0597*** (0.017)	0.0554*** (0.016)	0.0627*** (0.015)	0.0249*** (0.009)
Observations	119,420	119,420	119,420	119,420	119,420	119,420	119,420	119,420	119,420	119,420

Notes: Results from nonparametric regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.8: Effects of connections using successful 2010 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-	2-	3-	4-	5-	6-	7-	8-	9-	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.002 (0.003)	-0.0216*** (0.004)	0.0039** (0.002)	0.000 (0.001)	-0.000 (0.001)	0.0014*** (0.000)	0.001 (0.000)	0.001 (0.001)	0.0071*** (0.001)	0.0094*** (0.002)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.058	0.080	0.033	0.016	0.010	0.006	0.006	0.036	0.013	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	0.001 (0.003)	-0.0126*** (0.003)	0.003 (0.002)	-0.000 (0.001)	-0.001 (0.001)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.001)	0.001 (0.001)	0.0052*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.089	0.294	0.044	0.020	0.025	0.024	0.021	0.043	0.202	0.086
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	0.001 (0.003)	-0.0111*** (0.003)	0.003 (0.002)	-0.000 (0.001)	-0.000 (0.001)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.001)	0.001 (0.001)	0.0054*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.097	0.330	0.045	0.021	0.026	0.026	0.022	0.043	0.202	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed in sector 1 (Column 1), is employed in sector 2 (Column 2), is employed in sector 3 (Column 3), is employed in sector 4 (Column 4), is employed in sector 5 (Column 5), is employed in sector 6 (Column 6), is employed in sector 7 (Column 7), is employed in sector 8 (Column 8), is employed in sector 9 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.9: Exclude municipalities where the mayor's family has been in office at least 4 times

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipalities where mayor's family has been in office three times or less										
Connected Office (2007)	-0.0028 (0.004)	-0.0019 (0.004)	0.0073** (0.003)	0.0033 (0.002)	0.0044** (0.002)	0.0051*** (0.002)	0.0044** (0.002)	0.0052*** (0.002)	0.0055*** (0.002)	0.0048*** (0.002)
Observations	278,435	278,435	278,435	278,435	278,435	278,435	278,435	278,435	278,435	278,435
R-squared	0.330	0.268	0.230	0.243	0.264	0.293	0.271	0.258	0.259	0.087
Panel B: Municipalities where mayor's family has been in office twice or less										
Connected Office (2007)	-0.0054 (0.004)	-0.0030 (0.004)	0.0074** (0.003)	0.0031 (0.003)	0.0025 (0.003)	0.0027 (0.002)	0.0022 (0.002)	0.0030* (0.002)	0.0028* (0.002)	0.0037** (0.001)
Observations	197,401	197,401	197,401	197,401	197,401	197,401	197,401	197,401	197,401	197,401
R-squared	0.334	0.273	0.230	0.240	0.263	0.290	0.268	0.255	0.255	0.087
Panel C: Municipalities where mayor's family has been in office once										
Connected Office (2007)	-0.0057 (0.004)	-0.0024 (0.005)	0.0072* (0.004)	0.0035 (0.003)	0.0031 (0.004)	0.0029 (0.003)	0.0034 (0.003)	0.0046* (0.002)	0.0039* (0.002)	0.0031** (0.002)
Observations	127,342	127,342	127,342	127,342	127,342	127,342	127,342	127,342	127,342	127,342
R-squared	0.336	0.270	0.227	0.237	0.258	0.283	0.262	0.248	0.249	0.077

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level gender, relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.10: Robustness checks: Towards a fully saturated model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Interact all variables with gender										
Connected Office (2007)	-0.002 (0.003)	-0.003 (0.003)	0.0086*** (0.003)	0.0060*** (0.002)	0.0062*** (0.002)	0.0066*** (0.002)	0.0062*** (0.002)	0.0064*** (0.002)	0.0061*** (0.002)	0.0054*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.351	0.282	0.236	0.249	0.271	0.298	0.278	0.265	0.266	0.089
Panel B: Age/Edu/Gender specific dummies										
Connected Office (2007)	-0.002 (0.003)	-0.003 (0.003)	0.0083*** (0.003)	0.0056** (0.002)	0.0058*** (0.002)	0.0063*** (0.002)	0.0058*** (0.002)	0.0061*** (0.002)	0.0058*** (0.002)	0.0052*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.354	0.287	0.248	0.263	0.286	0.315	0.297	0.285	0.287	0.104
Panel C: Age/Edu/Gender/Province specific dummies										
Connected Office (2007)	-0.001 (0.003)	-0.002 (0.004)	0.0086** (0.003)	0.0053** (0.002)	0.0056*** (0.002)	0.0058*** (0.002)	0.0051*** (0.002)	0.0054*** (0.002)	0.0049*** (0.002)	0.0043*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.461	0.405	0.366	0.379	0.399	0.425	0.408	0.399	0.395	0.263
Panel D: Age/Edu/Gender/Muni specific dummies										
Connected Office (2007)	-0.002 (0.003)	-0.000 (0.003)	0.0109*** (0.003)	0.0073*** (0.003)	0.0067*** (0.002)	0.0064*** (0.002)	0.0045** (0.002)	0.0046** (0.002)	0.0050*** (0.002)	0.0036** (0.002)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.065	0.038	0.005	0.003	0.002	0.002	0.002	0.002	0.002	0.001

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level, gender, for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. In Panel A, all variables are interacted with the gender dummy. In Panel B, regressions are fully saturated for age, education and gender. In Panel C, education dummies are interacted with province dummies. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.11: Robustness checks: Relative educational achievements

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004 (0.003)	-0.005 (0.003)	0.0078*** (0.003)	0.0052** (0.002)	0.0056*** (0.002)	0.0061*** (0.002)	0.0057*** (0.002)	0.0060*** (0.002)	0.0057*** (0.001)	0.0052*** (0.001)
Observations	453,532	453,532	453,532	453,532	453,532	453,532	453,532	453,532	453,532	453,532
R-squared	0.271	0.230	0.229	0.244	0.267	0.296	0.276	0.264	0.264	0.087
Panel B: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.002 (0.003)	-0.003 (0.003)	0.0085*** (0.003)	0.0058*** (0.002)	0.0060*** (0.002)	0.0065*** (0.002)	0.0060*** (0.002)	0.0063*** (0.002)	0.0059*** (0.002)	0.0054*** (0.001)
Observations	453,532	453,532	453,532	453,532	453,532	453,532	453,532	453,532	453,532	453,532
R-squared	0.331	0.271	0.232	0.247	0.269	0.297	0.278	0.266	0.265	0.089

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels A and B, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel B, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.12: Robustness checks: Area-specific returns to education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Province-specific returns to education										
Connected Office (2007)	-0.001 (0.003)	-0.002 (0.003)	0.0087*** (0.003)	0.0059*** (0.002)	0.0061*** (0.002)	0.0065*** (0.001)	0.0059*** (0.002)	0.0061*** (0.001)	0.0059*** (0.001)	0.0051*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.334	0.277	0.236	0.250	0.273	0.302	0.282	0.270	0.269	0.098
Panel B: Municipality-specific returns to education										
Connected Office (2007)	-0.001 (0.003)	-0.002 (0.003)	0.0090*** (0.003)	0.0060*** (0.002)	0.0060*** (0.002)	0.0064*** (0.002)	0.0057*** (0.002)	0.0060*** (0.001)	0.0059*** (0.001)	0.0051*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.348	0.294	0.253	0.268	0.290	0.319	0.300	0.289	0.287	0.127
Panel C: Village-specific returns to education										
Connected Office (2007)	-0.001 (0.004)	-0.001 (0.004)	0.0085*** (0.003)	0.0053* (0.003)	0.0059** (0.002)	0.0059*** (0.002)	0.0048** (0.002)	0.0048** (0.002)	0.0048*** (0.002)	0.0050*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.446	0.411	0.362	0.366	0.382	0.407	0.389	0.379	0.371	0.241

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels A and B, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel B, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5%, *** at the 1% level.

Table A3.13: Robustness checks: Alternative fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Village Fixed Effects and Individual Controls										
Connected Office (2007)	-0.002 (0.003)	-0.002 (0.003)	0.0068*** (0.003)	0.0043* (0.002)	0.0055*** (0.002)	0.0057*** (0.002)	0.0049*** (0.002)	0.0051*** (0.002)	0.0054*** (0.002)	0.0053*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.354	0.308	0.259	0.266	0.285	0.313	0.293	0.282	0.279	0.112
Panel B: Enumerator* Municipal Fixed Effects and Individual Controls										
Connected Office (2007)	-0.0048* (0.003)	-0.004 (0.003)	0.0068*** (0.003)	0.0040* (0.002)	0.0051** (0.002)	0.0052*** (0.002)	0.0048*** (0.002)	0.0051*** (0.002)	0.0052*** (0.002)	0.0054*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.376	0.326	0.283	0.295	0.314	0.342	0.324	0.314	0.309	0.152
Panel C: Municipal Fixed Effects and Individual Controls - PROBIT										
Connected Office (2007)	-0.002 (0.004)	-0.003 (0.004)	0.0092*** (0.003)	0.0053*** (0.002)	0.0048*** (0.002)	0.0045*** (0.001)	0.0039*** (0.001)	0.0039*** (0.001)	0.0029*** (0.001)	0.0021*** (0.000)
Observations	453,671	453,656	453,617	453,577	453,562	453,565	453,565	453,565	453,184	451,249

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level, gender, relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.14: Robustness checks: Exclude some age groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Age < 63										
Connected Office (2007)	-0.000 (0.003)	-0.001 (0.003)	0.0085*** (0.003)	0.0062*** (0.002)	0.0061*** (0.002)	0.0063*** (0.002)	0.0058*** (0.002)	0.0062*** (0.002)	0.0061*** (0.002)	0.0054*** (0.001)
Observations	407,771	407,771	407,771	407,771	407,771	407,771	407,771	407,771	407,771	407,771
R-squared	0.338	0.274	0.236	0.250	0.271	0.302	0.281	0.267	0.267	0.090
Panel B: Age > 22										
Connected Office (2007)	-0.002 (0.003)	-0.004 (0.003)	0.0093*** (0.003)	0.0063*** (0.002)	0.0068*** (0.002)	0.0071*** (0.002)	0.0066*** (0.002)	0.0069*** (0.002)	0.0068*** (0.002)	0.0060*** (0.001)
Observations	405,741	405,741	405,741	405,741	405,741	405,741	405,741	405,741	405,741	405,741
R-squared	0.329	0.265	0.243	0.255	0.279	0.308	0.288	0.275	0.274	0.091
Panel C: Age > 22 and Age < 63										
Connected Office (2007)	-0.000 (0.003)	-0.001 (0.003)	0.0093*** (0.003)	0.0066*** (0.002)	0.0067*** (0.002)	0.0068*** (0.002)	0.0062*** (0.002)	0.0067*** (0.002)	0.0069*** (0.002)	0.0060*** (0.001)
Observations	359,827	359,827	359,827	359,827	359,827	359,827	359,827	359,827	359,827	359,827
R-squared	0.335	0.267	0.248	0.260	0.283	0.315	0.293	0.279	0.278	0.094

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels A and B, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel B, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.15: Robustness checks: Two-way clustering

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.001 (0.003)	0.002 (0.003)	0.0231*** (0.004)	0.0192*** (0.004)	0.0190*** (0.003)	0.0194*** (0.003)	0.0181*** (0.003)	0.0176*** (0.003)	0.0165*** (0.003)	0.0094*** (0.002)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.024	0.048	0.038	0.028	0.028	0.029	0.028	0.028	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004 (0.003)	-0.005 (0.003)	0.0079*** (0.003)	0.0054** (0.003)	0.0057*** (0.002)	0.0062*** (0.002)	0.0058*** (0.002)	0.0061*** (0.002)	0.0058*** (0.002)	0.0052*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.271	0.229	0.229	0.243	0.266	0.295	0.275	0.263	0.263	0.086
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.002 (0.003)	-0.003 (0.004)	0.0086*** (0.003)	0.0060** (0.002)	0.0062*** (0.002)	0.0066*** (0.002)	0.0062*** (0.002)	0.0064*** (0.002)	0.0061*** (0.002)	0.0054*** (0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). In Panels B and C, all regressions include a full set of dummies for age, education level and gender. In addition, in Panel C, regressions include a full set of dummies for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5%, *** at the 1% level.

Table A3.16: Robustness checks: Exclude data collected after November 2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects and Individual Controls										
Connected Office (2007)	-0.001 (0.003)	0.000 (0.003)	0.0116*** (0.003)	0.0079*** (0.003)	0.0076*** (0.002)	0.0080*** (0.002)	0.0074*** (0.002)	0.0075*** (0.002)	0.0075*** (0.002)	0.0065*** (0.002)
Observations	326,218	326,218	326,218	326,218	326,218	326,218	326,218	326,218	326,218	326,218
R-squared	0.319	0.261	0.228	0.242	0.265	0.295	0.274	0.262	0.261	0.087
Panel B: Add Interaction Term										
Connected Office (2007)	-0.0010 (0.003)	0.0000 (0.003)	0.0117*** (0.003)	0.0079*** (0.003)	0.0077*** (0.002)	0.0080*** (0.002)	0.0074*** (0.002)	0.0075*** (0.002)	0.0076*** (0.002)	0.0065*** (0.002)
Connected Office (2007) X Months before 11/2009	-0.0000 (0.000)	0.0000 (0.001)	-0.0003 (0.000)	-0.0004 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)	-0.0004 (0.000)	-0.0004 (0.000)	-0.0001 (0.000)
Observations	326,218	326,218	326,218	326,218	326,218	326,218	326,218	326,218	326,218	326,218
R-squared	0.319	0.261	0.228	0.242	0.265	0.295	0.274	0.262	0.261	0.087

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level, gender, for relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A3.17: Municipal heterogeneity: Municipal council

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects and Individual Controls (1)										
Connected Mayor (2007)	-0.000 (0.003)	-0.003 (0.003)	0.0125*** (0.002)	0.0127*** (0.002)	0.0118*** (0.002)	0.0112*** (0.002)	0.0101*** (0.002)	0.0103*** (0.002)	0.0088*** (0.001)	0.0074*** (0.001)
Connected Vice-Mayor (2007)	-0.002 (0.003)	0.003 (0.003)	0.0116*** (0.003)	0.0089*** (0.003)	0.0079*** (0.003)	0.0089*** (0.002)	0.0079*** (0.002)	0.0087*** (0.002)	0.0076*** (0.002)	0.0068*** (0.002)
Connected Councilor (2007)	0.001 (0.003)	-0.002 (0.003)	0.0092*** (0.002)	0.0069*** (0.002)	0.0057*** (0.002)	0.0056*** (0.001)	0.0048*** (0.001)	0.0050*** (0.001)	0.0040*** (0.001)	0.0040*** (0.001)
Interaction Mayor (2007)	-0.009 (0.008)	-0.013 (0.014)	-0.013 (0.009)	-0.0157* (0.008)	-0.0139* (0.008)	-0.012 (0.008)	-0.0129* (0.008)	-0.0138* (0.008)	-0.0121* (0.006)	-0.0117* (0.006)
Interaction Vice-Mayor (2007)	-0.0210* (0.012)	-0.014 (0.015)	-0.0162* (0.008)	-0.013 (0.009)	-0.0189** (0.008)	-0.0140* (0.007)	-0.011 (0.007)	-0.009 (0.007)	-0.007 (0.006)	-0.005 (0.005)
Interaction Councilor (2007)	0.003 (0.006)	0.000 (0.009)	-0.001 (0.008)	-0.003 (0.007)	-0.006 (0.007)	-0.006 (0.007)	-0.005 (0.007)	-0.003 (0.006)	0.000 (0.005)	-0.005 (0.004)
Observations	452,865	452,865	452,865	452,865	452,865	452,865	452,865	452,865	452,865	452,865
R-squared	0.330	0.270	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in sectors 2-10 (Column 2), is employed in sectors 3-10 (Column 3), is employed in sectors 4-10 (Column 4), is employed in sectors 5-10 (Column 5), is employed in sectors 6-10 (Column 6), is employed in sectors 7-10 (Column 7), is employed in sectors 8-10 (Column 8), is employed in sectors 9-10 (Column 9) and is employed in sector 10 (Column 10). All regressions include a full set of dummies for age, education level, gender, relationship to the household head, marital status, month/year of the interview, history of displacement and length of stay in the village. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

CONCLUSION

This thesis was concerned about how, in a decentralized system, local politicians stay in power and about some of its economic and social consequences. Chapter One showed that, in competitive political environments, local incumbents benefit electorally from cash transfer programs implemented by the central government. Chapter Two identified local political business cycles, with employment increasing in the two pre-election quarters and decreasing below trend in the two post-election quarters. Chapter Three provided some of the first individual-level estimates of both the value of being connected to an elected local official, and the cost of being connected to a politician in opposition.

DIRECTIONS FOR FURTHER RESEARCH

I now present potential areas for future research prompted by evidence discussed in this thesis.

Voting behavior

The model of retrospective voting presented in Chapter One could be expanded to incorporate two common features of electoral competition in developing countries, namely credit claiming and vote-buying. Voters would not decide whether to vote for the incumbent simply based on their utility levels on election day but rather they would assess candidates based on the relative benefits they directly provided to them. They would vote for the candidate responsible for the largest marginal increase in their utility level.

One could test the model through a mix of laboratory and field experiments. In the lab, a simple voting game could be designed with voters having to decide between an incumbent whose past performance is known and an unknown challenger. A subset of voters would enter a lottery in the pre-election period which will be either unsponsored or sponsored by one of the candidates. In this sub-treatment, the utility increase for voters would be directly attributed to one of the candidates. By comparing vote share in the various treatments we would be able to assess whether voters are using a naive version of retrospective voting or a more refined version where they are able to attribute their utility increases to specific candidates.

In the field, information about a program implemented by the central government – explaining carefully the role (or lack thereof) of the local incumbent – would be provided to voters in randomly selected districts. If credit claiming is indeed salient, we would expect support for the incumbent to drop; at least initially. The timing of information provision could make a difference as it would affect the incumbent's ability to react. As a result, in a subset of districts the information would be provided in the weeks leading up to the elections (T_1). In the other, the information would be provided regularly throughout the incumbent's term (T_2). In T_1 , the incumbents' ability to react would be constrained and we would expect lower incumbent vote share. In T_2 , incumbents would be able to adjust their electoral strategies. For example they might increase effort during their term in office and we might expect more services to be provided by the district bureaucracy. The impact on incumbent vote share in this treatment would be unclear as it would depend on the extent to which incumbents respond to the treatment.

Evidence presented in this thesis, especially in Chapters One and Two, suggests that voters tend to select politicians based on past behavior rather than on the policies they would implement if elected. This places constraints on potential challengers and could explain why they are more likely than incumbents to attempt to buy votes and why there is an incumbency advantage.

A potential explanation for the presence of retrospective voting is that candidates are unable to credibly commit to the policies they would implement if elected. I am engaged in two projects trying to address those issues. First, I am currently working on an information campaign that was implemented in conjunction with a reputable NGO prior to the May 13, 2013 elections in the Philippines.¹ Mayoral candidates were asked to provide information about how they intend to allocate the municipal budget across 10 sectors if elected. In addition, they also gave a list of programs that they would implement to support the proposed allocations. Municipality-specific flyers comparing responses from all candidates were prepared and distributed to voters in randomly selected villages in the days leading up to the elections. Crucially, voters were also informed that the promises would be monitored by our partner NGO. We expect marginal voters to shift from voting based on retrospective information to voting for the candidate whose promises are closer to their own preferences which we measure as well.

Second, in light of the importance of family networks identified in Chapter Three, candidates might be able to make more credible promises in villages where their relatives live.² Using the strategy presented in this thesis we will be able to identify, for each candidate, the number of relatives that live in each village. We would then be able to test, controlling for both candidate and village fixed effects, whether one additional relative in a village translates into more than one vote for the candidate. We will attempt to rule out alternative explanations by distinguishing between voting age and non-voting age population.

Politicians and the bureaucracy

Results presented in Chapters Two and Three are in agreement with the argument that the local bureaucracy is politicized. Not only do incumbents appear to provide individual with non-casual jobs in the public sector before the elections, they also seem to staff

¹ This is joint work with Cesi Cruz (UC San Diego) and Philip Keefer (World Bank).

² This is joint work with Cesi Cruz (UC San Diego) and Pablo Querubin (NYU).

the bureaucracy with loyal individuals they can trust to act in their best interest. This could also explain why politicians appear to be able to motivate bureaucrats ahead of elections while often blaming them for weak performance during their term in office. How are they able to do so? Why is effort not higher throughout their terms?

A potentially fruitful area of research might be to test whether there is a *political effort cycle* for bureaucrats and if it is due to an increase in supervision by incumbents ahead of elections. If so, what explains the variations? Building on the literature on teacher absence, data on bureaucrat effort would be collected through random visits in selected public offices (schools, health centers, land registry, etc.) at various points in the electoral cycles to record whether staff are present, working and the time it takes them to respond to citizens request. This would be complemented with a survey of bureaucrats and local politicians together with qualitative data collection.

Political Connections

Evidence on the cost of connections to politicians in opposition presented in Chapter Three is consistent with two, not necessarily mutually exclusive, explanations. First, as described in the Chapter, incumbents value loyalty and they might be reluctant to staff the bureaucracy with individuals whose views and interests are antinomic to theirs. An alternative view is that incumbents are sending a signal to potential challengers: an unsuccessful bid for office will induce cost on the candidate's relatives. If this second interpretation is correct then we would expect individuals connected to politicians in opposition to suffer from their connections across a broad range of outcomes; not simply in terms of occupation. We could use the same estimation strategy to test whether relative of politicians in opposition are less likely to benefit from a number of services that are provided by the municipality. The information is available in our dataset.

To better quantify the benefits and cost of political connections identified in Chapter Three one would need measures of wages, hours worked and effort on the job at

different points in the electoral cycle. Using the data as a sampling frame,³ one could implement a survey of connected individuals and collect working life histories.

Political Business Cycles

My preferred explanation for results presented in Chapter Two is that incumbents shift some of their planned post-election spending in the pre-election period. A natural extension would be to obtain monthly or quarterly data on budget spending at the local level and test this model more directly.

As discussed in Chapter Two, if the argument is correct, the strength of political business cycles detected with yearly data will depend on the timing of elections and the overlap between the fiscal and calendar years. If elections are early in the fiscal year, incumbents might not have enough time to increase spending before elections. Similarly if elections are late in the fiscal year, incumbents do not have enough resources to bring forward before elections. As a result, we would expect the effects to be stronger for elections organized in the middle of the fiscal year. In addition, the larger the overlap between the fiscal and calendar years the more likely is the pre-election increase attenuated with the post-election decrease. This could be tested in a country where local elections are not synchronized.

³ This would require obtaining the adequate permissions from the government department that collected the data.