Do Politicians' Relatives get Better Jobs? Evidence from Municipal Elections

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Abstract

This paper estimates the impacts of being connected to politicians on occupational choice. Using an administrative dataset collected in 2008–2010 on 20 million individuals in the Philippines, we rely on naming conventions to assess family links to candidates in elections held in 2007 and 2010. We combine a regression discontinuity design to close elections in 2007 with an alternative approach using individuals connected to successful candidates in 2010 as control group. This allows us to net out the possible cost associated with being related to a losing candidate. We find robust evidence that relatives of current office-holders are more likely to be employed in better paying occupations.

1 Introduction

The focus of this paper is the value of political connections for individuals. The literature on this issue has faced several difficulties and is not well developed.[1] First, due to a 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 the likelihood of reporting connections may be correlated with the benefits that are derived from them (Comola and Fafchamps, 2013). Second, individuals connected to politicians may differ from the average citizen in unobservable ways that affect their welfare even when their politician relatives are no longer in office (Besley, 2005; Dal Bo et al., 2016). It thus 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 on the value of political connections has not accounted for the possibility that an individual connected to a politician who lost an election can suffer from this connection, especially in areas where elected officials have discretionary powers

In this paper we use a large dataset from the Philippines, collected between the 2007 and 2010 municipal elections, to test whether people who are related to a successful local politician are more likely to be employed in a higher-ranked, better-paid occupation than if their relative was not in office. We find that individuals who share one or more family names with a local elected official are more likely to be employed in better-paying occupations. This effect is observed using a regression discontinuity design (RDD) based on close elections, and it persists both when we control for individual characteristics and when we compare relatives of politicians elected in 2007 and in 2010. The effect is present throughout the occupational distribution, but it is particularly noticeable at the top: the probability of being employed in a managerial position increases by 0.48 percentage points, or more than 19 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, specifications and estimation techniques.

The impact of family connections varies with individual and municipal characteristics. First, the impact is stronger for more educated individuals, and is mostly concentrated on individuals with some university education. Second, the effect of connections on the probability of being employed in a managerial position is 45 percent lower for women than it is for men. For all other occupations, the impact is similar for men and women. Finally, a family connection to a mayor has a stronger effect on occupation than a connection to a vice mayor or local councilor.

This paper documents a failure of equal opportunity. Given the data, we are unable to test whether this failure has any effect on efficiency. At the top of the occupational distribution, our point estimates correspond to 2.8 percent of the managerial jobs in the public sector in each municipality. The magnitude of the estimated effect is thus not inconsistent with the view that deviation from equal opportunity is due to the preferential treatment of relatives as managers in the public sector.

We contribute to the literature on the value of political connections in four ways. First, our dataset is unusually exhaustive as it includes non-anonymized information on all individuals in a large number of municipalities, combined with detailed electoral data across two elections.[2] Second, we bypass the need to rely on self-reported links. Instead, we rely on Filipino family names introduced by Spanish colonial authorities to infer ties to local politicians – see Angelucci et al. (2010) and Angelucci, De Giorgi and Rasul (2012) for a similar approach in Mexico.[3] Because Spanish naming conventions were only recently introduced in the Philippines (i.e., in the middle of the 19th century), they allow an unusually precise and objective identification of family ties. Third, by using different control groups, we are able to estimate the value of being connected to an elected local official net of the potential cost of being connected to a losing candidate. We show that this affects the estimation. Fourth, we provide evidence of unequal opportunity by showing that there is a reshuffling of rationed jobs among the local elite after a change of political personnel resulting from a local election.

While our data do not allow us to identify the precise channel of causation of our documented effects, we are able to offer some suggestive evidence of how the effect materializes. First, it is unlikely that our results are driven solely by politicians' redistributive motives among their relatives: the benefit of political connections is stronger for educated individuals, and educated individuals tend to be, on average, less poor.[4] Second, we find that individuals connected to candidates who were *almost* elected are less likely to be employed in managerial positions. This suggests that local politicians appoint relatives to positions of responsibility not so much because they can more easily identify good managers among them, but rather because they trust their relatives more, possibly because they can supervise and monitor them more easily. This interpretation is in line with recent findings that politicians value both loyalty and expertise when appointing bureaucrats (Iyer and Mani, 2012). It also agrees with qualitative evidence on the behavior of Filipino politicians (Sidel, 1999; Cullinane, 2009).[5] We nonetheless refrain from applying a value judgment to our interpretation of the findings, since we do not know whether incumbents favor relatives because they believe this is the best way to improve service delivery, or whether they do so merely for electoral gain.

The results presented in this paper have a number of implications for the empirical literature on the value of political connections. First, they indicate that, in the absence of an adequate control group, estimates of the value of political connections may be biased upward. Second, we show that estimates obtained by comparing individuals connected to the winner vs. the loser in close elections potentially include costs incurred by individuals related to the loser. It may be informative to try to separate the two empirically. Our results also provide a note of caution regarding political decentralization in areas of weak accountability, which characterizes most municipalities in the Philippines (De Dios, 2007). In such a setting, local officials might be able to favor their relatives in hiring decisions, as well as punish the relatives of their political opponents. This, in turn, may have a deleterious long-term influence on electoral competition, and thereby hinder growth (Besley, Persson and Sturm, 2010).

To address concerns about specification search and publication bias (Leamer, 1978, 1983; Glaeser, 2006), we implemented a split-sample approach (Fafchamps and Labonne, 2016). 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. We used sample A to narrow down the list of hypotheses we wished to test. We prepared a draft of the paper that took into account comments from the journal's co-editor and referees, which we registered with Evidence in Governance and Politics.[6] The journal's editorial board asked us to revise the paper using the full sample. The results obtained using sample B, together with results from an additional bootstrapping exercise, are discussed in the Appendix.

The paper is organized as follows. We describe the setting in Section 2 and the data in Section 3. The estimation strategy is presented in Section 4 and the balance tests in Section 5. In Section 6 we discuss the main results and a number of robustness checks. Section 7 concludes.

2 The setting

Guided by evidence on the history of clientelism in the United States and other Western democracies, we expect political connections to be especially valuable where politicians have access to significant resources and enjoy discretionary power.[7] Philippine municipalities are a particularly well-suited setting in which to estimate the value of political connections, because the 1991 Local Government Code (Republic Act 7160) devolved significant fiscal resources and decision-making power to local politicians. According to the Filipino constitution, mayors, vice mayors and eight municipal councilors are all individually elected in first-past-the-post elections. These elections are organized, by law, at fixed three-year intervals, which rules out any possible endogeneity between the timing of local elections and the level of politicians' support.

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 use their hiring power over the large number of staff that was transferred from national agencies to municipalities as part of the decentralization process. For example, he quotes a lawyer from the Civil Service Commission as explaining, "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 hiring, either directly through their personal business holdings or, in a number of provinces, indirectly through their contacts with local businessmen.[8] It is also possible that local businessmen favor local officials' relatives in their hiring decisions in the hope of securing favorable treatment, such as business-related permits.

There is some evidence that loyalty to local politicians is valued. Bureaucrats are often expected to engage in behavior that favors incumbents prior to elections. Cullinane (2009) reports that local politicians staff the bureaucracy with loyal individuals they can trust to act in their best interests. In a case study of local politics in Cavite, a province outside of Metro Manila, Coronel (1995) points out that "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 the relatives of known political challengers may suffer if incumbents are reluctant to staff the bureaucracy with individuals whose views and interests contradict their own. There is qualitative evidence that Filipino politicians can punish individuals connected to their opponents (McCoy, 2009).

3 Data

The primary dataset used in this paper was collected between 2008 and 2010 for the National Household Targeting System for Poverty Reduction (NHTS-PR). The survey was conducted by the Department of Social Welfare and Development

(DSWD) to select beneficiaries for the *Pantawid Pamilyia Pilipino Program*, a large-scale conditional cash transfer (CCT) program. DSWD uses the data to predict per capita income through a proxy means test, and to determine eligibility for the CCT program (Fernandez, 2012).

We have access to the full NHTS-PR dataset, which contains information on the age, gender, education, occupation and complete family name for more than 50 million individuals. We limit our analysis to 20 million observations in the 709 municipalities in which full enumeration took place.[9] We further restrict the sample to data collected between 2008 and April 2010, that is, before the May 2010 elections.

The NHTS-PR data include information on the occupation of all surveyed individuals.The list of occupations was developed by the National Statistics Office for its regular Labor Force Surveys (LFSs) and includes 11 categories.[10] We rank occupations according to their average daily wage, computed using wage data from eight nationally representative LFSs collected in 2008 and 2009.[11] The ranking is unaffected if we focus instead on the median or the 75th percentile of the wage distribution in each occupation.

We obtained the names of all candidates (and winners) in elections for mayor, vice mayor and municipal councilor in 2007 and 2010. This yields a dataset of 38,448 candidates, 80 percent of whom ran for the position of municipal councilor. The rest are evenly split between candidates for mayor and vice mayor, for which we also have data on the total votes received by each candidate.

3.1 Family ties

We take advantage of naming conventions in the Philippines to assess blood and marriage links between the surveyed individuals and local politicians.[12] Surnames used in the Philippines were imposed by Spanish colonial authorities in the mid-19th century, in part to distinguish families at the municipal level in order 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 names, and thus do not reflect pre-existing family ties. In each municipality, a particular family name was given to only one family. As a result, there is considerable local heterogeneity in names, reducing concerns that names simply capture an individual's ethnic background or other group membership. Names are transmitted across generations according to well-established rules inspired by 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.

In the Philippines, the process of changing one's middle or last name is long, and the probability of success is low. This reduces concerns about strategic name changes. 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 court cases that have reached the Supreme Court.[13]

The dataset includes information on the middle and last names of all surveyed individuals. Using this information, an individual is classified as being related to a given politician if she or someone in her household has a middle or last name matching the politician's middle or last name. This strategy has been used to assess blood links between municipal- and provincial-level Filipino politicians over time (Querubin, 2011, 2016; Cruz and Schneider, forthcoming). Angelucci et al. (2010) use a similar strategy to measure family networks in Mexico.

In the context of our study, sharing a middle or last name is *a priori* a good indicator of family ties. This could be questioned, however, if some names are too common. For example, if individuals from the same ethnic group all shared the same last name, the results would capture ethnic ties rather than family connections. In our sample municipalities, there are, on average, 5,998 names used (median 5,126). There is also a great diversity of names. We compute a Herfindahl Index of name heterogeneity.[14] Its average across municipalities is 0.999 and its minimum is 0.964, indicating a high level of heterogeneity. The most common surname in our data, De La Cruz, is used by only 0.32 percent of individuals.[15]

Therefore, our method generates a credible number of family ties. Importantly, since the data were collected to identify beneficiaries of a government program, names were recorded with great care. In particular, middle and last names were entered separately, so we do not have to worry about how to split family names. Furthermore, data entry was performed by specially trained staff, so it is unlikely to be correlated with respondent characteristics.[16]

3.2 Descriptive statistics

Descriptive statistics on employment by occupation are displayed in Columns 1 and 2 of Table 1.[17] Simple comparisons reveal significant differences between individuals related to office holders and the rest of the population. For example, 3.3 percent of individuals connected to successful candidates in the 2007 elections are employed in a managerial role, compared to only 2 percent of the population as a whole. Conversely, 29.1 percent of individuals in the general population are employed as farmers or fishermen, compared to only 26.5 percent of those connected to successful candidates in the 2007 elections.

Our objective is to assess the impact of family ties to local politicians on the probability of being employed in a better-paying occupation (according to their average daily wage). To do so, we create 10 dummy variables Y_{ij}^p, each of which is equal to 1 if individual i in municipality j is employed in an occupation of rank p or above. To illustrate, if individual i is employed in occupation 5, $Y_{ij}^p = 0$ for $p = \{1, 2, 3, 4\}$ and 1 for $p = \{5, ..., 10\}$.[18] By examining how the entire set of Y_{ij}^p varies between groups of interest, we can trace, in a non-parametric way, how political connections affect the cumulative distribution of average earnings based on occupation.

«COMP: Place Table 1 about here»

4 Estimation strategy

In this section we discuss the relative advantages and drawbacks of several approaches used to identify a suitable control group. We aim to obtain a credible estimate of the causal effect of political connections on occupation in a way that nets out the potential cost of losing an election. We take a step-by-step approach, discussing how data constraints and unobserved heterogeneity combine to make it challenging to estimate the value of political connections.

4.1 Starting point: regression discontinuity design

We first estimate the value of political connections by applying a non-parametric RDD to close elections. This approach, which has been used to estimate the private returns of holding office (Eggers and Hainmueller, 2009; Querubin and Snyder, 2013; Fisman, Schulz and Vig, 2014), relies on the assumption that relatives of politicians who were narrowly defeated are most comparable to relatives of narrowly elected politicians.

We use data on votes for the top two candidates in the 2007 mayoral and vice-mayoral elections. Let Y_{ij}^{P} be an outcome of interest for individual i in municipality j. We estimate a model of the form:

$$Y_{ij}^{P} = \alpha C_{ij} + f(V_{ij}) + \epsilon_{ij}, \tag{1}$$

where α is the parameter of interest, C_{ij} is a dummy variable that equals 1 if individual i is related to an elected official in municipality j, f is an unknown smooth function, V_{ij} is the margin of electoral victory or defeat for i's relative, and ϵ_{ij} is an idiosyncratic error. Equation (1) is first estimated on a sample composed of relatives of candidates with a 2007 vote margin of +/- 5 percent.[19] For each sample, we follow Imbens and Lemieux (2008) and estimate Equation (1) non-parametrically with the optimal bandwidth recommended by Imbens and Kalyanaraman (2012).[20] We estimate Equation (1) for the outcome variables described in Section 3.2.

4.2 Main estimation strategy

Data on connections to unsuccessful candidates is seldom available. In such cases, researchers can only compare politically connected individuals to individuals randomly drawn from the population. To allow comparisons with this literature (Caeyers and Dercon, 2012), we estimate the value of political connections by regressing the outcome variable on being linked to an elected local official, plus individual controls. Specifically, we estimate a linear probability model of the form:

$$Y_{ijt}^{P} = \alpha C_{ijt} + \beta X_{ijt} + v_{jt} + u_{ijt}, \tag{2}$$

where Y_{ijt}^{P} 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 1 if individual i is related to an elected official 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. Here we introduce a time subscript since, in contrast to our RD estimate, we now use candidates across two electoral cycles. We cluster standard errors at the provincial level.[21]

We estimate Equation (2) 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 and history of displacement, and we include dummies for the month $\times$ year in which the interview took place. Since we have a large number of observations, we estimate a fully saturated model, i.e., we include a full set of dummies for each distinct value of each control variable.

Equation (2) 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 $E[e_{ijt}C_{ijt}] = 0$. Component μ_{ij} denotes the unobserved heterogeneity associated with being related to someone who ran at least once in a local election. There are good reasons to expect $E[\mu_{ij}|C_{ijt} = 1] > E[\mu_{ij}|C_{ijt} = 0]$. Component η_{ij} is the *additional* unobserved heterogeneity associated with being connected to a candidate who has won at least one local election. We expect that $E[\eta_{ij}|C_{ijt} = 1] > E[\eta_{ij}|C_{ijt} = 0]$. If we can control for μ_{ij} and η_{ij}, then α 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. We now discuss how different control groups help us reach this objective.

Control group I: relatives of unsuccessful 2007 candidates We first estimate Equation (2) on the restricted sample of all individuals related to local politicians who ran in the 2007 elections. In this approach, as in the RD design discussed above, individuals related to unsuccessful politicians serve as controls for those related to successful politicians. The purpose of this method is to net out unobserved heterogeneity μ_{ij}. It delivers an unbiased estimate of α provided that $E[\eta_{ij}C_{ijt}] = 0$.

Control group II: relatives of 2010 candidates Even when $E[\eta_{ij}C_{ijt}] = 0$, control group I remains vulnerable to the following concern. As pointed out by Medina and Stokes (2007), the relatives of losing candidates may suffer from their connections.[22] Such situations are most likely to arise when, as in our study, elected officials have information about how specific groups of individuals voted, and use their control over the distribution of certain goods or benefits to punish or reward individuals.

To illustrate, imagine that an elected politician punishes the relatives of an unsuccessful opponent in a way that results in a lower-paying occupation. In this case, the difference in occupation level Y^P_{ijt} between relatives of successful and unsuccessful candidates overestimates α since it also includes the punishment imposed on relatives of the loser.

One possible solution is to use as the control group the relatives of politicians who ran in an election that took place *after* survey time t, but who did not run in elections before time t. By construction, the relatives of these politicians cannot be punished at t for opposing the currently elected official. Based on this idea, we estimate Equation (2) based 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 potential punishment of unsuccessful opponents.

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 (2) using a third control group that only includes relatives of successful 2010 candidates who did not run for election in 2007. This group minimizes several possible sources of bias and should yield the most accurate estimate of α.[23]

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.[24]

We report estimates using all three control groups. Control groups II and, especially, III represent a marked improvement on the literature to date. We are able to use these control groups for three 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, since respondents could not be asked about their connections to future candidates. Second, using names to infer family connections could be problematic in many countries but, for reasons discussed in Section 3.2, Filipino 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, the control and treatment groups would have been too small to estimate α.

5 Balance

Before reporting the econometric results, Table 2 displays the results of a battery of balancedness tests for the different estimation strategies used in the analysis.

5.1 RDD approach

To address standard concerns about the possible manipulation of RD estimates around the threshold, we carry out standard balance tests for RD analysis. We proceed in two steps. First, we test whether incumbents are more likely to win close elections, which is in line with the recommendation by Eggers et al. (2015) to validate RDD assumptions by showing that the sample of close elections is balanced. Second, we compare the relatives of narrow losers and narrow winners along a number of observable characteristics.

We test whether incumbents (or someone from their family) are more or less likely to win close races. In the sample of races that we use in the main analysis, the point estimate is 0.057 with a standard error of 0.203. We are thus unable to reject the null hypothesis that incumbents and challengers are equally likely to win close races. This provides reassurance that the sample is indeed balanced.

The results from the analyses of relatives' characteristics are presented in Column 1 of Table 2. We see that individuals on the right side of the threshold have 0.68 more years of education and 38 percent more relatives than those on the left side of the threshold. We revisit this issue when we discuss the control group approach.[25]

«COMP: Place Table 2 about here»

5.2 Control group approach

We now turn to the control group approach and examine the data for evidence of unbalancedness. As suggested by the figures reported in Columns 3–5 of Table 1, a non-negligible share of the difference between individuals who are related to office holders and the rest of the population 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, 2.5 percent are employed in a managerial role, which is 24 percent more than in the general population. In other words, relatives of future political office holders are more likely to occupy a managerial position than the general population.

To investigate this further, we report balance tests for our three control groups; the results are presented in Columns 2–4 of Table 2. Overall, it appears that for the years of education and network size, balance is improved when using a control group approach compared to an RDD method: the gap between the relatives of elected officials and the different control groups is about half of what it is when using the RDD approach. This reinforces our confidence that using relatives of future winners as a control group is a valid strategy.

To deal with the residual lack of balance, we use two different strategies. We start by adding individual controls, and include a full set of dummies for each distinct value of each control variable. We then conduct robustness checks in which all key variables are fully saturated at the municipal level. This reduces the risk that our results are driven by a lack of balance on observables.

6 Econometric analysis

We now turn to the estimation of our model of interest.

6.1 Starting point: RDD approach

Before running the RDD regressions, we first show the regression discontinuity graphically by plotting a local polynomial regression of the probability that someone will be employed in the best-paying occupation (manager) on the vote share of their politician relative in the 2007 elections. The result, presented in Figure 1, shows a clear jump in the probability of being employed as a manager around the election victory cut-off.[26] As a placebo test, we report a local polynomial regression of the probability of being employed as a manager on vote share in the 2010 elections (i.e., after the occupation data were collected). We find no meaningful evidence of discontinuity in the probability of being employed as a manager at the cut-off (Figure 2).[27]

«COMP: Place Figure 1 about here»

«COMP: Place Figure 2 about here»

Table 3 presents the RDD results. We find that political connections have a strong positive impact on the probability of being employed in a better-paid occupation.[28] RDD estimates obtained using the optimal bandwidth suggest that connections increase the likelihood of being employed by 3.9 percentage points. We find a similar result if we combine the two top occupations: political connections increase the likelihood of being employed in either a professional or a managerial role (occupations 9 and 10) by 2 percentage points.

«COMP: Place Table 3 about here»

Since the optimal bandwidth is relatively narrow (around 1.1 percentage points), there is a risk that our results are driven by a limited number of observations around the cut-off (Gelman and Imbens, 2014). To show that our results are robust, we re-estimate the model using twice the optimal bandwidth (Panel C of Table 3). In addition, we estimate non-parametric regressions with bandwidths of 3, 4 and 5 (Table A4). As shown in Figure A5, larger bandwidths yield results that are equivalent to assuming that the function f in Equation (1) is linear on both sides of the cut-off, and thus observations just around the cut-off are less likely to influence the results. This set of results confirms our previous findings and reduces concerns that our RDD findings are driven by a limited number of observations just around the cut-off.[29]

The RD results discussed thus far estimate the effect of being connected to a closely elected politician within the set of individuals related to close winners and losers. While inherently relevant, those estimates nonetheless combine two possible effects: the benefits for relatives of the winner and the punishment of relatives of the loser.[30] In the next section, we seek to parse out the two effects.

6.2 Main results: control group approach

To fix ideas, we begin by reporting naive ordinary least squares estimates using the entire population as the control group. The results indicate that individuals related to politicians in office are more likely to be employed in better-paying occupations (Table 4). For example, a randomly selected individual related to an elected local official is 4.1 percentage points more likely to be employed as at least a service worker than an average citizen. The effect of a connection to a local politician is 1.5 percentage points for managerial positions. This represents an increase of about 70 percent in the mean probability of being a manager.

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The results reported in Panels A and B of Table 4 show that a share of the difference in occupations 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. With the full set of controls, the impact of political connections on the probability of being employed as a manager drops to 0.75 percentage points. However, adding controls other than those reported in the table no longer affects the point estimates.

As explained in Section 4, we now compare the naive results reported in Table 4 to those obtained with control groups I, II and III. The results using control group I are reported in the right corner of Figure 3 and Panel A of Table 5. When we use control group I – i.e., the relatives of unsuccessful 2007 candidates – to net out unobserved heterogeneity μ_{ij}, our results are qualitatively similar to those obtained using the full sample. This is clearest when comparing the top left and right corners of Figure 3. However, if we compare Panel B of Table 4 to Panel A of Table 5, the point estimates are lower than those obtained using the full sample:

depending on the outcome of interest, they fall by 29 to 40 percent. In particular, political family ties are now associated only with a 0.55-percentage-point increase in the probability of being employed in a managerial role.[31] These findings are consistent with the idea that the bias caused by μ_{ij} is positive: individuals related to politicians who run in local elections come from more privileged social backgrounds and, on average, tend to have slightly better occupations.

Next we use control group II, in which the relatives of 2010 candidates who did not run in 2007 are compared to those of successful 2007 candidates. The purpose is, as with control group I, to net out μ_{ij}, and to avoid including in the estimate of political connections the potential costs incurred by individuals connected to unsuccessful candidates. The results are shown in the bottom-left corner of Figure 3 and Panel B of Table 5. They show that family ties to elected officials remain associated with better-paid occupations. We also estimate the same regressions on individuals connected to *unsuccessful* candidates in 2010 who did not run in 2007, and find similar results (see Table A8).[32]

There is still a possibility that relatives of successful local politicians differ from those of unsuccessful ones. To account for this possibility, we further restrict the control group to individuals connected to candidates who were *successful* in the 2010 election but did not run in 2007. This nets out both μ_{ij} and η_{ij}. This is control group III. The results are presented in the bottom-right corner of Figure 3 and in Panel C of Table 5. They confirm that individuals connected to currently serving local officials are more likely to be employed in better-paid occupations.[33]

Although small in magnitude, the effect of political connections is economically significant: using our most conservative estimates, we find that individuals connected to current office holders are 14.8 percent more likely than individuals in control group III to be employed in either a professional or managerial position, and 19.2 percent more likely to be employed in a managerial position.

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6.3 Discussion and interpretation

What do we learn from comparing estimates obtained using different approaches and control groups? First, by comparing the results from the full sample to those using control group I, we found that, as anticipated, an upward bias arises when estimating the impact of political connections without controlling for unobserved heterogeneity μ_{ij}: point estimates obtained using the full sample are 40 to 57 percent higher than those obtained using control group I (Panel B of Tables 4 and A6). Similar conclusions are obtained with control groups II and III, which also control for η_{ij}.

Second, we can assess the magnitude of the bias generated by η_{ij} by comparing the point estimates obtained using control groups II and III: those obtained with control group III are smaller than those calculated using control group II, which suggests that η_{ij} is positive but much smaller than μ_{ij}.

Third, we have presented evidence consistent with the idea that there are costs associated with being related to a candidate who lost.[34] Comparing the point estimates obtained with control groups I and II suggests that the relatives of unsuccessful 2007 candidates do not suffer from their ties to an unlucky challenger. However, in a context where the bureaucracy is politicized, only a small number of individuals may suffer. Relatives of close losers, i.e., relatives of candidates who *almost* won the 2007 elections, represent a bigger threat than relatives of non-close losers, and therefore might be the ones suffering such costs. This could explain why the point estimates obtained through RDD are higher than those obtained with any of the three control groups, and why RDD estimates increase as the bandwidth decreases.[35]

Fourth, to more directly test the costs associated with being related to a candidate who lost, we combine individuals connected to unsuccessful candidates in the 2007 elections to those connected to candidates who were unsuccessful in the 2010 elections but who did not run in 2007. We plot local polynomial regressions of the probability of being employed in the best-paying occupations on their relatives' vote share (Figure A7). For individuals connected to candidates who lost by a margin of less than 5 percentage points, the probability of being employed as a manager is noticeably lower for individuals connected to 2007 candidates than for those connected to 2010 candidates. We also regress on that sample the probability of being employed in the best-paying occupations on a dummy equal to 1 if the individual is connected to a losing candidate in 2007. We are able to reject the null of no effect (at the 10 percent level) for individuals connected to candidates who lost by less than 5 percentage points, but not for those connected to candidates who lost by larger margins (Figure A8). This provides additional evidence consistent with a theory of political control of the bureaucracy whereby incumbents staff the local bureaucracy with individuals whose incentives are aligned with their own electoral objectives.[36] Data constraints prevent us from testing this directly, however.

Based on the above evidence, we conclude that control group III provides the most credible estimates of the benefits of family ties to elected local officials net of potential punishment. The robustness checks discussed below and in the online appendix therefore focus on that control group.

In the online appendix, we discuss a large number of falsification and robustness checks. We highlight the main ones below. The results are robust to controlling flexibly for family size (Panel A of Table A14) and to estimating a restrictive matching estimator in which we compare connected individuals of the same gender, age and education living in the same municipality (Panel B of Table A14). In addition, we find no difference between relatives of candidates elected for the first or the second time in 2007. This reduces concerns that our results are driven by trends in the type of candidates running for office. Finally, the results are also robust to excluding municipalities where the incumbent mayor's family has been in office for three terms or more (Panel D of Table A14). These results and a number of additional robustness checks are discussed in detail in the online appendix.

6.4 Heterogeneity

Having confirmed the robustness of our findings to a number of possible confounding effects, we investigate whether the occupational benefit of family connections varies with observable individual characteristics. To this effect, we interact the family ties dummy with gender, age and education.[37] As is clear from Table 6, we find evidence of significant heterogeneity. First, the benefits derived from political connections are stronger for more-educated individuals: each additional year of education is associated with a 0.09-percentage-point increase in the likelihood of managerial employment among connected individuals. Second, the impact of family ties on managerial employment is 45 percent lower for women than it is for men. The impact of political connections also appears to be increasing with age.

«COMP: Place Table 6 about here»

To investigate this further, we relax the assumption that the relationship between education and the value of political connections is linear. To analyze this possibility, we estimate the value of political connections separately for each education level. In Figure 4 we plot each point estimate and its associated 95 percent confidence interval. The results show a convex increasing relationship between education and the value of political connections.

«COMP: Place Figure 4 about here»

These findings are hard to reconcile with a simple model of patronage in which jobs are provided to unqualified individuals who are connected to politicians. In such a setting, it is the less-educated and inexperienced individuals who would benefit the most from political connections. This is not what we find. Furthermore, additional analysis suggests that connected individuals tend to be better educated than non-connected individuals employed in the same occupations. For example, among individuals who are employed in the best-paying occupations, 57.6 percent of individuals connected to office holders are college graduates, compared to 53.8

percent of unconnected individuals. The corresponding figure for individuals in control group III is 55.5 percent. This suggests that, if anything, it is the more-educated individuals who get managerial jobs thanks to their political connections.

Finally, we investigate whether the value of political ties varies according to the type of elected official. To this effect, we estimate Equation (2) with all the possible interactions between three dummies capturing links to a mayor, vice-mayor or municipal councilor. We then compute the marginal effects for each dummy. The results are shown in Table A18. As could be anticipated, the estimated impact of a family tie tends to be greater for mayors than for vice mayors and municipal councilors. The relatives of the mayor are 0.85 percentage points more likely to be employed in a managerial position. The point estimate for municipal councilors is a smaller 0.43 percentage points, a difference that is statistically different from zero at the 1 percent level.

7 Conclusion

In this paper, we have provided evidence that family ties to locally elected politicians are associated with better-paid occupations. The evidence we have brought to bear indicates that this association is probably causal.[38] In addition to an RDD, we have dealt with unobserved heterogeneity using a variety of control groups, including individuals related to candidates who are elected in subsequent elections. Our findings are robust to these alternative estimation techniques and to the inclusion of numerous control variables: the effect of political connections on better-paid occupations is consistently shown to be economically and statistically significant. In addition, we find that being related to an unsuccessful candidate who lost by a small margin is associated with a less favorable occupation.

Although the data at our disposal are quite comprehensive, they do not allow us to establish unambiguously whether political favoritism in employment affects the local delivery of public services. To explore this issue, we construct a municipal-level measure of the extent to which political connections affect the types of occupations people are engaged in, and correlate it with the quality of health service delivery, which has been devolved to the municipality level (Capuno, 2009; Khemani, 2015). Our measure of distortion is calculated as the difference in the probability of being employed as a manager between individuals related to a politician in office between 2007 and 2010 and those related to a politician who ran in either the 2007 or 2010 municipal election. Using this difference as a measure of political distortion, we find that more distortion is associated with an increase in the percentage of children under the age of 6 that is underweight (Panel A of Table 7). The correlation is robust to controlling for a number of municipal characteristics, including poverty incidence, average education levels, Gini coefficient and incumbent vote share in the previous election. In contrast, if we focus on education, a sector that has *not* been devolved to the municipal level, we find no correlation between education outcomes and labor market distortions due to political connections (Panels B–E of Table 7). While we are unable to make causal claims based on those results, they suggest that politicians' ability to help their relatives secure better-paying occupations negatively affects the delivery of services by the municipal bureaucracy.

«COMP: Place Table 7 about here»

Appendix: background on the split-sample approach

To deal with concerns about specification search and publication bias, we initially planned to implement a method described by Fafchamps and Labonne (2016), which proceeds as follows. A third party is asked to randomly split the data into two halves. The first half (the *training set*) is used to narrow down the list of hypotheses researchers want to test. Once the list is finalized and agreed with referees and editors, they are applied to the second half (the *testing set*). The purpose of this approach is to provide credible estimates that are free of specification search and publication bias, and to deliver adequately sized statistical tests. By allowing the researcher to learn from the training sample, this approach reduces concerns that pre-analysis plans might 'stifle innovation' (Casey, Glennerster and Miguel, 2012; Deaton, 2012). It is related to the strategy advocated by Humphreys, Sanchez de la Sierra and van der Windt (2013), who argue that researchers carrying out Randomized Controlled Trials (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 the results from the exploratory analyses into our analysis plans.

The exact procedure we used is as follows. After putting 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 reduce the chance that the two halves may be too correlated, we sampled villages, rather than individuals or households. We sent the program and the dataset to a third party who generated the two random samples. The third party sent us the training sample and kept the testing sample. Importantly, the program used to generate the samples generates new provincial, municipal, village, household and individual IDs. As a result, at no point were we able to reconstruct the testing sample from the training and full samples.

Using the training sample, we wrote a draft that we presented at a number of conferences and seminars. The feedback we received was incorporated into the paper that we submitted to *The Journal of Law, Economics, & Organization*. The referees provided further comments, and the co-editor invited us to revise and resubmit the paper. As part of the revision process, we prepared a full draft of the paper – taking into account the referees' comments – using the training data. We then registered that version of the paper with Evidence in Governance and Politics. We then generated the full set of results using both the full dataset and the testing sample.

The Editorial board asked us to resubmit the paper using the full dataset, which we did. Below we briefly discuss the results obtained using the testing sample (these results are available in the online appendix in Figures T1–T4 and Tables T1 – T8).

The results obtained on the testing sample with control groups I, II and III are consistent with those obtained using the full sample. The main difference is that the RDD results and some of the heterogeneous treatment effects are no longer significant. In particular, on the testing sample, there is no evidence that more-educated individuals benefit more from political connections. Similarly, there is no evidence that the effects are stronger in municipalities where the incumbent's family has been in power longer or has facilitated larger fiscal transfers. For these reasons, we downplay these findings in the body of the paper.

As an additional robustness check, we carry out a bootstrap exercise for the estimates obtained with control group III, our preferred/most conservative control group. In each iteration we sample, in each municipality, 50 percent of the individuals are either connected to a politician in office between 2007 and 2010 or are part of control group III. For each sample, we estimate Equation (2) using the full set of controls. We run 1,000 iterations of this exercise. The point estimates on the effects of connections on the likelihood of being employed in a managerial role range from 0.32–0.65 percentage points (average 0.48). Similarly, the point estimates of the effects of connections on the likelihood of being employed in a professional or managerial position range from 0.25–0.67 percentage points (average 0.45). In both cases, the relevant point estimates are significant in all regressions.

Notes

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[1]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). Studies have established that political connections, in the form of either fixed ties (such as family ties) or more direct investment (such as campaign contributions), are valuable in the sense that the connected firms tend to have higher stock market valuations. This is a feature of both developed and developing economies, and those effects tend to be higher in countries with higher levels of corruption (Faccio, 2006). A number of channels have been identified, the most common of which are that connected firms are more likely to benefit from procurement contracts, and tend to enjoy lower capital costs and a more favorable regulatory environment. A related literature that explores the private returns on holding office has found that politicians' assets tend to grow faster than if they had not been elected either while in they are office or once they have left (Eggers and Hainmueller, 2009; Querubin and Snyder, 2013; Fisman, Schulz and Vig, 2014). Less progress has been

made in identifying the value of political connections for individuals (Besley, Pande and Rao, 2012; Blanes i Vidal, Draca and Fons-Rosen, 2012; Caeyers and Dercon, 2012; Markussen and Tarp, 2014; Gagliarducci and Manacorda, 2016; Folke, Persson and Rickne, forthcoming). This is related to the literature on the role of family links in labor markets. For example, Wang (2013) documents a significant reduction in earnings when a man's father-in-law dies.

[2]The dataset does not include information on the sector of employment.

[3]Others have used information on rare surnames to study intergenerational mobility (*e.g,* Clark (2014) and Guell, Rodriguez Mora and Telmer (2015)).

[4]This suggests at the very least that, based on observables, incompetent relatives are not the ones deriving the greatest benefits from their connections.

[5]For example, Cullinane (2009, p 190) reports that when asked about his relatives' employment in the local government, Filipino politician Ramon Durano told a reporter that "politics is not something you can entrust to non-relatives." Sidel (1999) argues that municipal mayors in the Philippines use their control over tax collection and regulatory enforcement both to enrich themselves and to gain electoral rewards.

[6]All relevant documents are available at http://egap.org/registration/1891.

[7]See, for example, Wallis (2006) for a detailed account of systematic corruption in the 19th and early 20th centuries in the United States and Wallis, Fishback and Kantor (2006) on efforts to reduce political manipulation at the local level during the New Deal.

[8]Similarly, Hollnsteiner (1963) argues that Filipino voters expect incumbents to help them secure employment. Municipal politicians have incentives to develop relationships with those who have influential positions in private business - for example, owners of large companies need many unskilled workers such as messengers, laborer, guards and janitors, as well as many white collar workers such as clerks, accountants and collectors. Therefore ties to them may enable politicians to help individuals secure employment in many types of jobs (Hollnsteiner, 1963).

[9]In the remaining municipalities, information was only collected on residents in so-called *pockets of poverty.* The main concern here is that we do not have information on how these pockets of poverty were selected, which prevents us from recovering survey weights. In addition, there is a risk that since these selection criteria are correlated with poverty, they might also be correlated with political connections, which would affect our main estimates.

[10]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 col-

lected with the Labor Force Surveys classification. This leaves us with data on 562 municipalities.

[11]The sample is restricted to municipalities in the NHTS-PR dataset.

[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 our intent-to-treat population. The mean effects are probably stronger.

[13]For example, in the case *Wang v. Cebu City Civil Registrar* (G.R. No. 159966, 30 March 2005, 454 SCRA 155), Justice Tinga indicated that "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."

[14]It is defined as $1 - \sum s_i^2$, where s_i is the share of individuals in the municipality using name i.

[15]The distribution of names in our study population is much more dispersed than in neighboring East Asian countries. For example, in China, the most common surname is used by 7.25 percent of the population and the Herfindahl Index at the national level is 0.971; in Taiwan the comparable figures are 11 percent and 0.953, respectively; and in Vietnam they are 38 percent and 0.821. Thus in all three countries, the diversity of names at the *national* level is lower than that in the Philippines at the *municipal* level.

[16]There are two sources of potential 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 misspelled, e.g., De Los Reyes spelled De Los Reyez). These sources of measurement error generate an attenuation bias that works *against* rejecting the null of no effect.

[17]It is important to note that the 'none' category (which contains about 40.2 percent of individuals) includes both individuals not in the labor force and unemployed individuals. This is consistent with official employment statistics: between 2007 and 2010, labor force participation in the Philippines was around 65 percent and, among the working-age population, the unemployment rate was 7 percent. Additional descriptive statistics are available in Table A1.

[18]Y_{ij}^0 drops out since, by construction, it is always equal to 1.

[19]Using this cut-off to define close elections, we find that 17.1 percent of mayoral elections in

our sample are close. Among the vice-mayoral elections, the proportion is 14.7 percent. Overall, there is at least one close election in 27.5 percent of the municipalities in our sample. Comparing municipalities with and without close elections, we find no statistically significant difference in poverty incidence, the number of times the incumbent's family has been in office, or per capita fiscal transfers from the central government. There is some evidence that municipalities with close elections are slightly less populous (significant at the 5 percent level), but once we regress a dummy equal to 1 if either the mayoral or vice-mayoral election was close in 2007 on the set of four control variables, we are unable to reject the null of no effect for any of the coefficients. The F-stat for the regression is 1.95 (p-value 0.114).

[20]This is implemented in Stata using the `rd` command developed by Nichols (2011). It estimates a local linear regression with a triangular kernel.

[21]The results are similar if we cluster standard errors at either the municipal level (Table A22) or along both month$\times$year and province, using the two-way clustering method developed by Cameron, Gelbach and Miller (2011) (Table A23).

[22]There is evidence that politicians discriminate against their opponents' supporters in settings as diverse as India (Wilkinson, 2007), Russia (Hale, 2007), Singapore (Tremewan, 1994), Venezuela (Hsieh et al., 2011) and, to some extent, the United States (The Economist, 2014). Hsieh et al. (2011) find that Chavez's opponents were less likely to be employed once their names became public. Similarly, in the 1980s the People's Action Party (PAP), the ruling party in Singapore, changed its vote-counting system. In a country where a large share of the population lives in public housing, the PAP has access to electoral outcomes down to the apartment block level and voters know that supporting the opposition translates into a lower priority for their building maintenance (Tremewan, 1994). More recently, New Jersey Governor Chris Christie became involved in an imbroglio when it surfaced that two traffic lanes on a bridge between New Jersey and Manhattan were closed to punish supporters of a political opponent (The Economist, 2014). In the Philippines, Lande (1965) argued that local politics is organized around factions and that politicians often avenge themselves by attacking their opponents' relatives or followers. In an extreme example in the Philippines, in November 2009, Esmael Mangudadatu wanted to file his candidate registration form in order to run for provincial governor of Maguindanao in the May 2010 elections against the powerful Ampatuan clan. Aware of threats against his life, he asked some of his relatives and a number of journalists to file the paperwork on his behalf to deter such an attack. On the way to do so, their convoy was stopped. Fifty-eight people were brutally massacred and members of the Ampatuan

clan have been charged with their murder (Human Rights Watch, 2010).

[23]We maintain the assumption that the pool of candidates is comparable across the two electoral cycles. We discuss evidence consistent with this assumption. For example, relatives of candidates elected for the first time in 2007 have a similar employment status as relatives of candidates elected for the second time in 2007. If trends in candidate quality explained our results, we should observe differences between the two groups.

[24]This method has been implicitly used in the literature on the impact 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 that are not.

[25]We also use the approach developed by McCrary (2008) to test for potential manipulation of the running variable (vote margin in our case). The null hypothesis is that the vote margin variable is continuous at the threshold. The McCrary statistics is 0.655 with a standard error of 0.044, which leads us to reject the null. It is important to note that, for each candidate elected with vote margin x, there is another candidate who lost with vote margin $-x$. This implies that, across the sample of all elections, the vote margin variable is continuous around the threshold by construction. It follows that the test is not particularly useful in our case. Continuity appears to be rejected because, in the individual-specific results presented in Column 1 of Table 2, elected individuals have more relatives, and hence there are more observations above the threshold than below.

[26]To check that our results are not driven by non-linearities, we reproduce the graph for samples composed of relatives of candidates with a 2007 vote margin of +/- 5 percent, +/- 7.5 percent, +/- 12.5 percent and +/- 15 percent. See Figures A1 to A4.

[27]The estimation sample includes relatives of winners and runners-up in elections that were determined by vote margins of 10 percentage points or less. To avoid capturing the effects of the 2007 elections, the estimation sample excludes all individuals who are connected to either the winner or the runner-up in the 2007 elections. Note that we use a slightly larger bandwidth to generate this graph than to generate Figure 1 in order to avoid a statistical artifact (given that we drop all 2010 observations that involve winners and runners-up in the 2007 election, there are very few observations just below the threshold).

[28]We get similar results when using the relatives of candidates with a 2007 vote margin of either +/- 2.5 percent or +/- 10 percent – see Tables A2 and A3.

[29]Importantly, when we use the same estimation strategy we are unable to reject the null of no

effect for the 2010 elections - those that took place after the data were collected (Table A5). We use the same sample as the one used to generate Figure 2 (cf. footnote 27). As with the results obtained in the 2007 sample, we also display the placebo results with the larger bandwidth in Figure A6.

[30]It is important to note that we are not arguing that RD estimates are never valid. The issues discussed here would not affect papers interested in using RD designs to estimate, for example, the effects of partisan alignment between different levels of government on fiscal transfers or on the vote share of national politicians.

[31]Additional results are available in Table A6. They confirm the conclusions drawn here.

[32]Additional results are available in Table A7. They do not affect our conclusions.

[33]Our robustness analysis presented in Table A9 does not change this conclusion.

[34]Additional results are available in the Appendix.

[35]Recall that the RDD estimates were obtained on the sample of individuals related to candidates for either mayor or vice mayor in the 2007 elections. The three control groups include relatives of candidates for municipal councilor. To reduce concerns that the differences in the estimates are driven by the inclusion of individuals related to candidates for municipal councilor, we estimate Equation (2) on the sample of individuals related to candidates for either mayor or vice mayor in the 2007 elections (Table A10). The RDD estimates are much larger than the regression point estimates on that subsample.

[36]An alternative view is that incumbents are sending a signal to potential challengers: an unsuccessful bid for office will involve costs for the candidate's relatives. If this second interpretation is correct, then we would expect individuals connected to opposition politicians to suffer from their connections across a broad range of outcomes, not simply in terms of occupation. This is left for future research.

[37]We also explore heterogeneity along family size. We are unable to reject the null that the coefficient on the interaction term is 0 (results available upon request).

[38]While our current data only allow us to estimate the short-term value of political connections, we will attempt to establish the longer-term impacts in future research.

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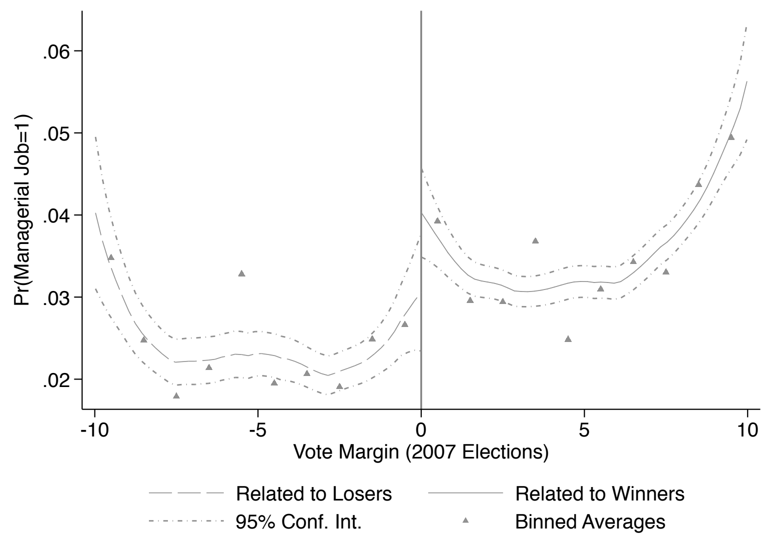

Figure 1: Non-parametric estimates of the probability of being employed in a managerial position

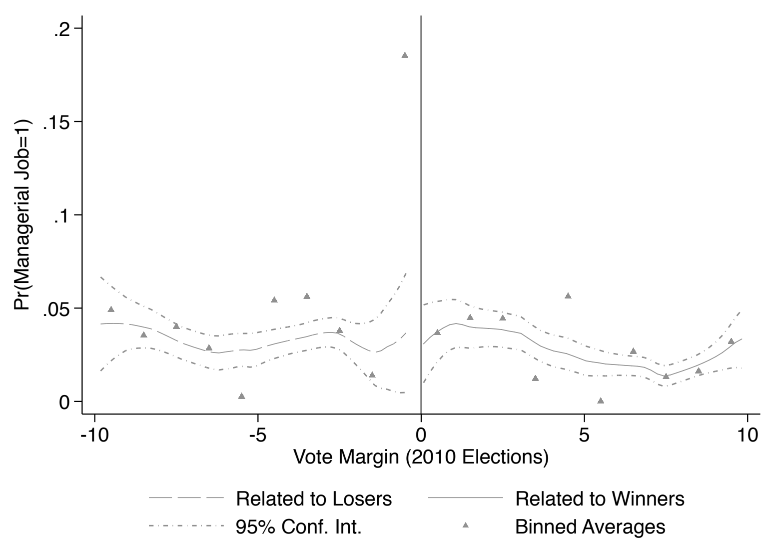

Figure 2: Non-parametric estimates of the probability of being employed in a managerial position [Placebo - 2010 elections]

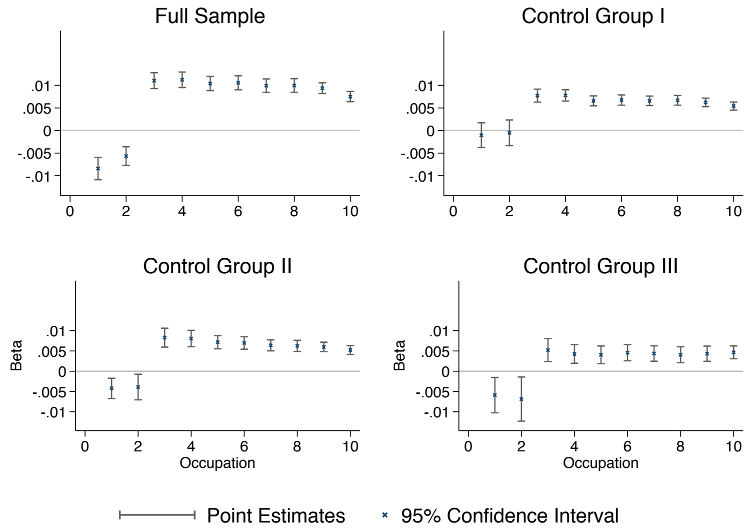

Figure 3: Estimated effects of connections with various control groups

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 who did not run in 2007 and Control group III includes relatives of successful candidates in the 2010 elections who 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 4 and A6-A9.

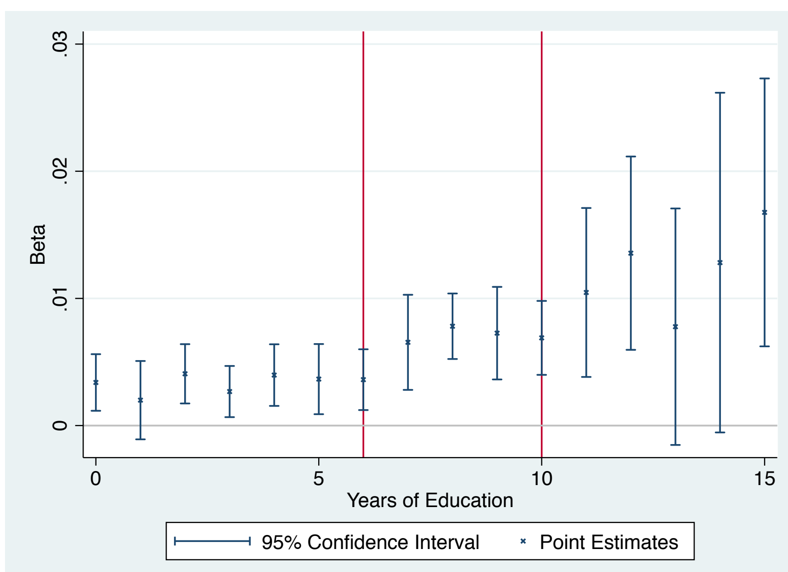

Figure 4: Estimated effects of connections by education levels

Table 1: Descriptive statistics: Individual-level

	Full Sample	Connected	Control Group		
			I	II	III
	(1)	(2)	(3)	(4)	(5)
Occupation					
0. None	40.18	40.72	40.65	40.1	39.97
1. Laborers, Unskilled Workers	15.38	13.93	14.84	14.58	14.50
2. Farmers, Forestry Workers, Fishermen	29.12	26.49	27.22	28.71	28.76
3. Service, Shop, Market Sales Workers	5.01	5.41	5.44	5.19	5.05
4. Trades, Related workers	2.02	2.22	2.11	2.10	2.13
5. Plant, Machine Operators, Assemblers	1.45	1.43	1.50	1.45	1.45
6. Clerks	0.58	0.82	0.75	0.66	0.70
7. Technicians, Associate Professionals	0.59	0.74	0.71	0.66	0.61
8. Special Occupations	1.30	1.35	1.25	1.31	1.30
9. Professionals	2.36	3.64	3.11	2.85	3.03
10. Officials, Managers, Supervisors	2.02	3.25	2.43	2.38	2.50
Controls					
Age	39.25	40.17	39.82	39.48	39.75
Education (years)	8.15	9.02	8.70	8.48	8.52
Female	0.49	0.50	0.50	0.49	0.49
Observations	7,839,602	786,126	779,823	480,700	116,931

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

Table 2: Balance Tests

	RDD	Control Group		
		I	II	III
	(1)	(2)	(3)	(4)
Panel A: Age				
Connected Office (2007)	0.0014	0.0021***	0.0026***	0.0013
	(0.025)	(0.001)	(0.001)	(0.002)
Observations	30,427	786,436	635,432	454,889
R-squared		0.001	0.001	0.001
Panel B: Education (years)				
Connected Office (2007)	0.6498***	0.3245***	0.4794***	0.3600***
	(0.174)	(0.041)	(0.043)	(0.064)
Observations	30,427	786,436	635,432	454,889
R-squared		0.137	0.144	0.134
Panel C: Female				
Connected Office (2007)	-0.2358	0.0718	0.2804***	0.2856***
	(0.700)	(0.048)	(0.052)	(0.073)
Observations	30,427	786,436	635,432	454,889
R-squared		0.029	0.03	0.030
Panel D: Number of Relatives (log)				
Connected Office (2007)	0.3875***	0.1627***	0.2814***	0.2099***
	(0.059)	(0.021)	(0.029)	(0.049)
Observations	30,427	786,436	635,432	454,889
R-squared		0.219	0.224	0.236

Notes: This table reports various balance tests estimated on different samples either through RDD or OLS. In Column 1, the sample includes relatives of one of the top two candidates in the 2007 mayoral and vice-mayoral elections (vote margin +/- 5 percent) and the effects are estimated through RDD. In Columns 2-4, the dependent variable is regressed on a dummy equal to one if the respondent is related to a politician that was elected to office in 2007 and a full set of municipal dummies. In Column 2, officials' relatives are compared to relatives of unsuccessful candidates in the 2007 elections (Control Group I). In Column 3, officials' relatives are compared to relatives of candidates in the 2010 elections who did not run in 2007 (Control Group II). In Column 4, officials' relatives are compared to relatives of successful candidates in the 2010 elections who did not run in 2007. In Panel A, the dependent variable is age. In Panel B, the dependent variables is the number of years of educations. In Panel C, the dependent variable is a dummy equal to one if the respondent is female. In Panel D, the dependent variable is the log of the sum of the number of individuals who share the individual's middle name in the municipality and of the number of individuals who share the individual's middle name in the municipality. In Columns 2-4, 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 3: The effects of connections on the probability of being in any occupation with regression discontinuity designs - Nonparametric

	(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: Optimal Bandwidth										
Connected Office (2007)	0.0392*	0.0564***	0.0394**	0.0349***	0.0294**	0.0240**	0.0288**	0.0268**	0.0205**	0.0018
	(0.022)	(0.021)	(0.016)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.010)	(0.007)
Observations	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0497*	0.0797***	0.0595***	0.0612***	0.0497***	0.0459***	0.0303**	0.0227	0.0299**	0.0078
	(0.028)	(0.026)	(0.019)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.012)	(0.008)
Observations	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	0.0254*	0.0191	0.0178*	0.0241***	0.0196**	0.0153*	0.0316***	0.0287***	0.0180**	0.0093*
	(0.014)	(0.013)	(0.011)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.007)	(0.005)
Observations	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716	59,716

Notes: Results from nonparametric regressions. The sample includes relatives of one of the top two candidates in the 2007 mayoral and vice-mayoral elections (vote margin +/- 5 percent). The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in occupations 2-10 (Column 2), is employed in occupations 3-10 (Column 3), is employed in occupations 4-10 (Column 4), is employed in occupations 5-10 (Column 5), is employed in occupations 6-10 (Column 6), is employed in occupations 7-10 (Column 7), is employed in occupations 8-10 (Column 8), is employed in occupations 9-10 (Column 9) and is employed in occupation 10 (Column 10). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 4: The effects of connections on the probability of being in any occupation - 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)	-0.0050***	0.0037**	0.0410***	0.0374***	0.0351***	0.0346***	0.0321***	0.0304***	0.0278***	0.0145***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	7,821,772	7,821,772	7,821,772	7,821,772	7,821,772	7,821,772	7,821,772	7,821,772	7,821,772	7,821,772
R-squared	0.021	0.051	0.035	0.024	0.023	0.022	0.022	0.021	0.012	0.009
Panel B: Municipal Fixed Effects and Individual Controls										
Connected Office (2007)	-0.0026*	-0.002	0.0122***	0.0121***	0.0109***	0.0110***	0.0103***	0.0104***	0.0097***	0.0077***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	7,821,756	7,821,756	7,821,756	7,821,756	7,821,756	7,821,756	7,821,756	7,821,756	7,821,756	7,821,756
R-squared	0.347	0.274	0.198	0.209	0.230	0.260	0.241	0.228	0.238	0.068

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 occupations 2-10 (Column 2), is employed in occupations 3-10 (Column 3), is employed in occupations 4-10 (Column 4), is employed in occupations 5-10 (Column 5), is employed in occupations 6-10 (Column 6), is employed in occupations 7-10 (Column 7), is employed in occupations 8-10 (Column 8), is employed in occupations 9-10 (Column 9) and is employed in occupation 10 (Column 10). In Panel B, 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 5: The effects of connections on the probability of being in any occupation - Three control 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: Control Group I										
Connected Office (2007)	0.00	0.001	0.0079***	0.0079***	0.0066***	0.0068***	0.0067***	0.0068***	0.0063***	0.0055***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Observations	1,564,515	1,564,515	1,564,515	1,564,515	1,564,515	1,564,515	1,564,515	1,564,515	1,564,515	1,564,515
R-squared	0.33	0.268	0.221	0.237	0.258	0.285	0.265	0.252	0.254	0.078
Panel B: Control Group II										
Connected Office (2007)	-0.002	-0.002	0.0087***	0.0084***	0.0074***	0.0072***	0.0066***	0.0065***	0.0062***	0.0054***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	1,265,506	1,265,506	1,265,506	1,265,506	1,265,506	1,265,506	1,265,506	1,265,506	1,265,506	1,265,506
R-squared	0.332	0.271	0.226	0.24	0.261	0.288	0.269	0.255	0.257	0.081
Panel C: Control Group III										
Connected Office (2007)	-0.0044**	-0.0056**	0.0055***	0.0046***	0.0043***	0.0048***	0.0046***	0.0043***	0.0045***	0.0048***
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	901,91	901,91	901,91	901,91	901,91	901,91	901,91	901,91	901,91	901,910
R-squared	0.331	0.271	0.232	0.246	0.268	0.295	0.276	0.262	0.263	0.085

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 occupations 2-10 (Column 2), is employed in occupations 3-10 (Column 3), is employed in occupations 4-10 (Column 4), is employed in occupations 5-10 (Column 5), is employed in occupations 6-10 (Column 6), is employed in occupations 7-10 (Column 7), is employed in occupations 8-10 (Column 8), is employed in occupations 9-10 (Column 9) and is employed in occupation 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 6: 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	-0.0109***	-0.0123**	0.0065***	0.0067***	0.0055***	0.0064***	0.0066***	0.0057***	0.0054***	0.0071***
	(0.004)	(0.006)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Connected*Female	0.010	0.011	-0.002	-0.0044*	-0.003	-0.0031*	-0.0039**	-0.0028*	-0.002	-0.0044***
	(0.007)	(0.009)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Connected*Edu	0.000	0.001	0.000	0.0007*	0.0008**	0.0009**	0.0008**	0.0007*	0.0008**	0.0009***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Connected*Age	-0.000	-0.000	0.0002*	0.0002**	0.0002***	0.0002***	0.0002***	0.0002***	0.0003***	0.0002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	901,910	901,910	901,910	901,910	901,910	901,910	901,910	901,910	901,910	901,910
R-squared	0.271	0.228	0.229	0.244	0.266	0.294	0.274	0.260	0.261	0.083
Panel B: Municipal Fixed Effects and Individual Controls (2)										
Connected	-0.0075**	-0.0096*	0.0070***	0.0071***	0.0058***	0.0066***	0.0067***	0.0058***	0.0055***	0.0072***
	(0.003)	(0.006)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Connected*Female	0.006	0.009	-0.003	-0.0047*	-0.003	-0.0032*	-0.0038**	-0.0027*	-0.002	-0.0044***
	(0.007)	(0.008)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Connected*Edu	0.000	0.001	0.000	0.001	0.0008**	0.0009**	0.0008**	0.0007*	0.0008**	0.0009***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Connected*Age	-0.000	-0.000	0.0002*	0.0002**	0.0002***	0.0002***	0.0002***	0.0002***	0.0002***	0.0002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	901,910	901,910	901,910	901,910	901,910	901,910	901,910	901,910	901,910	901,910
R-squared	0.331	0.271	0.232	0.246	0.268	0.295	0.276	0.262	0.263	0.085

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 occupations 2-10 (Column 2), is employed in occupations 3-10 (Column 3), is employed in occupations 4-10 (Column 4), is employed in occupations 5-10 (Column 5), is employed in occupations 6-10 (Column 6), is employed in occupations 7-10 (Column 7), is employed in occupations 8-10 (Column 8), is employed in occupations 9-10 (Column 9) and is employed in occupation 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 7: Political Distortions and Service Delivery

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Share of 0-71 months old that are underweight						
Distortion	44.53***	31.96**	32.26**	44.29***	32.12**	32.50**
	(16.37)	(14.06)	(13.89)	(15.63)	(13.21)	(12.93)
Observations	404	404	403	404	404	403
R-squared	0.60	0.64	0.641	0.565	0.631	0.631
Panel B: Share of 4-5 year old that are enrolled in kindergarden						
Distortion	0.09	0.07	0.03	-0.09	-0.09	-0.11
	(0.37)	(0.36)	(0.37)	(0.37)	(0.33)	(0.34)
Observations	557	555	519	555	555	519
R-squared	0.636	0.669	0.639	0.611	0.637	0.613
Panel C: Share of 6 year old that are enrolled in primary school						
Distortion	-0.07	-0.05	-0.09	-0.02	-0.1	-0.10
	(0.28)	(0.30)	(0.32)	(0.31)	(0.32)	(0.34)
Observations	557	555	519	555	555	519
R-squared	0.628	0.649	0.648	0.609	0.623	0.623
Panel D: Total years of schooling for 11 year old						
Distortion	-1.93	-1.15	-1.19	-1.9	-0.94	-0.85
	(1.40)	(0.96)	(1.00)	(1.56)	(1.03)	(1.02)
Observations	557	555	519	555	555	519
R-squared	0.685	0.842	0.82	0.671	0.831	0.814
Panel E: Total years of schooling for 15 year old						
Distortion	-2.83	-1.47	-1.38	-3.22	-1.32	-1.13
	(2.44)	(1.12)	(1.12)	(2.54)	(1.18)	(1.18)
Observations	557	555	519	555	555	519
R-squared	0.698	0.912	0.90	0.671	0.903	0.893

Notes: Results from fixed-effects regressions. In Columns 1-3, regressions are unweighted. In Columns 4-6, regressions are weighted by the 2010 municipal population. The dependent variable is the share of 0-71 months old who are underweight (Panel A), the share of 4-5 year olds who are enrolled in kindergarden (Panel B), the share of 6 year olds who are enrolled in primary school (Panel C), the total number of years of school for 11 year olds (Panel D) and, the total number of years of schooling for 15 years old (Panel E). The measure of distortion is the difference in the probability of being employed as a manager between individuals related to a politician in office between 2007 and 2010 and individuals related to a politician who ran either in 2007 or 2010. In Columns 2-3 and 5-6. all regressions control for population, poverty incidence, gini and average years of education for individual age 20-80. In addition, in Columns 3 and 6, regressions control for winner vote share in the 2007 elections and the number of terms her family has been in office. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.