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Do Politicians' Relatives Get Better Jobs?

Evidence from Municipal Elections

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Abstract

We estimate the impacts of being connected to politicians on occupational choice. We use an administrative dataset collected in 2008-2010 on 20 million individuals and rely on naming conventions to assess family links to candidates in elections held in 2007 and 2010. We first estimate the value of political connections by applying a regression discontinuity design to close elections in 2007. Those estimates likely combine the benefits from connections to current office-holders and the cost associated with being related to a losing candidate. We use individuals connected to successful candidates in the 2010 elections as control group and find that relatives of current office-holders are more likely to employed in better paying occupations. Relatives of candidates who narrowly lost in 2007 have lower occupations. A third-party randomly split our dataset in two and gave us sample 1. Once the review is completed, we will apply the approved methodology to sample 2.

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1 Introduction

In this paper we examine whether people who are related to a successful politician get a better job. This could arise for different reasons. One possibility is nepotism – politicians could favor their relatives in public sector jobs, either because of redistributive norms/altruism, or as a reward for their political support. Another possibility is loyalty or screening – politicians may search among their relatives the able and reliable workers they need to implement their policies (Iyer and Mani 2012). It is also conceivable that employers recruit the elected officials' relatives in the hope of securing political support and protection. We test whether a successful politician's relatives are more likely to be employed in a higher-ranked, better paid occupation in the public or private sector. Evidence in support of this hypothesis would have implications for political economy models emphasizing the principal-agent relationships between politicians and either bureaucrats or firms. For example, if politicians are able to staff the bureaucracy with their relatives then the principal-agent problem in the relationship between politicians and bureaucrats might be overstated.

The literature on the value of political connections for individuals has faced several difficulties and is not well-developed.[1] 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 the likelihood of reporting connections might be correlated with the benefits that are derived from them (Comola and Fafchamps forthcoming). Second, individuals connected to politicians may differ from the average citizen along unobservable characteristics that affect

[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, either fixed ties (such as family ties) or more direct investment (such as campaign contributions) are valuable in the sense 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 being that connected firms are more likely to benefit from procurement contracts, tend to enjoy lower cost of capital and more favorable regulatory environment. A related literature has explored the private returns to holding office and has found that politicians' assets tend to grow faster either while in office or once they've left (Eggers and Hainmueller 2009, Fisman, Schulz and Vig forthcoming, Querubin and Snyder 2013). 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 forthcoming, Gagliarducci and Manacorda 2014). 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.

their welfare even when their politician relatives are not in office (Besley 2005). 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 on the value of political connections 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.

Using a large dataset from the Philippines, collected between the 2007 and 2010 municipal elections, we test whether individuals who are connected to politicians in municipal elections are employed in better-paying occupations.[2] We contribute to the literature on the value of political connections in three ways. First, 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, for the first time, the cost of being connected to a losing candidate, both of which are present in our data. Second, we test the robustness of our findings to the use of different control groups. We find that this matters. Third, 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) and Angelucci, De Giorgi and Rasul (2012) for a similar approach in Mexico).[3] 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 local naming conventions are informative, they allow an unusually precise and objective identification of family ties.

To address concerns about specification search and publication bias (Leamer 1978, Leamer 1983, Glaeser 2006), we propose and implement a split sample approach. 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, to which we do not have access yet, the detailed methodology (including the exact list of definitions of dependent and control variables, estimation strategy

[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 (2014)).

and sample) that has been approved by the referees and editor, and this is what will be published. We believe that our approach can improve the reliability of empirical work and could be adopted widely in a world of 'big data' (Einav and Levin 2013).

Our results can be summarized as follows. Individuals who share one or more family 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.

While we provide RD estimates based on close elections, we argue that they are problematic in contexts similar to ours.[4] Indeed, RD designs imply the use of losing candidates' relatives as control group but it might not be valid as they might suffer from their connections. As pointed out by Medina and Stokes (2007), this is likely to be a concern when elected officials have some information about how specific groups of individuals voted and have discretion over the distribution of some goods or benefits to punish or rewards individuals. There is ample evidence that politicians can discriminate against their opponents' supporters in settings as diverse as India (Wilkinson 2007), Russia (Hale 2007), Singapore (Tremewan 1994), Venezuela (Hsieh, Miguel, Ortega and Rodriguez 2011) and, to some extent, the United States (The Economist 2014). This is thought to be easier in our context as the family connections we are interested in are easily observable.[5]

[4]It is important to note that we are not arguing that RD estimates are never valid. The issues discussed in this paragraph 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 vote share for national politicians.

[5]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 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 lower priority for their building maintenance (Tremewan 1994). More recently, 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

We are also able to test for the impact of being related to local politicians who failed to be elected. While there is some existing anecdotal evidence that such individuals might suffer from their connections, we are the first to quantify the effect. Comparing regression discontinuity design (RDD) estimates based on close election in 2007 with results from our preferred control group, 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 who did not perform as well in the elections. This is confirmed by comparing the relatives of candidates who were narrowly defeated in 2007 to relatives of candidates who lost with similarly narrow margins in 2010 but did not run in 2007. We interpret this as suggesting that incumbents punish relatives of their serious opponents.

The impact of family connections varies with observable individual and municipal characteristics. First, the impact is stronger for more educated individuals, 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 in any occupation, 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. This is consistent with the view that strong family ties are correlated with patrimonialism and undermine democracy (Fukuyama 2011, Alesina and Giuliano 2013).

Our results offer some suggestive evidence as to how these effects materialize. First, since

that the local politics is organised 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 candidacy 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 fill his candidacy on his behalf to deter such an attack. 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).

the benefits of political connections are stronger for educated individuals, it is unlikely that our results are solely driven by politicians' altruistic or redistributive motives towards their relatives.[6] Second, the fact that individuals connected to candidates who were almost elected are less likely to be employed in managerial positions suggests that family connections facilitate supervision and monitoring rather than screening. This interpretation is in line with recent findings that politicians value both loyalty and expertise when assigning bureaucrats (Iyer and Mani 2012) and with qualitative evidence on the behavior of Filipino politicians (Cullinane 2009, Sidel 1999).[7]

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 not only be able to favor their relatives in hiring decisions but also to punish their political opponents' relatives. This in turn may have a deleterious long-term influence on electoral competition which might hinder growth (Besley, Persson and Sturm 2010).

The paper is organized as follows. We describe the setting in Section 2 and the data in Section 3. Results based on a regression discontinuity design are discussed in Section 4. An alternative estimation strategy is presented in Section 5. In Section 6 we discuss the main results and a number of robustness checks. Section 7 concludes.

[6] At least, this suggests that, based on observables, incompetent relatives are not the ones deriving the greatest benefits from their connections.

[7] To illustrate, Cullinane (2009, p 190) reports that when asked about his relatives' employment in the local government, Ramon Durano, a Filipino politician, 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 not only to enrich themselves but also to gain electoral rewards.

2 The setting

Guided by evidence on the history of clientelism in the US and other Western democracies, we expect political connections to be especially valuable in contexts where politicians have access to significant resources and enjoy discretionary powers.[8] In light of that, 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. 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.

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 who were transferred from national agencies to municipalities as part of the decentralization process. 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.'*[9]

In the private sector, Sidel (1999) shows that local politicians can affect employment either directly, through their business holdings or, in a number of provinces, indirectly through their contacts with local businessmen. In addition, it is possible that local businessmen favor local

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

[9] Consistent with this, Labonne (2013) tests for the presence of local political business cycles in the Philippines over the period 2003-2009 and, among other things, finds that non-casual employment in the public sector drops in the two post-election quarters in municipalities where the incumbent failed to be re-elected.

officials' relatives 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.

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. 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, 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 known political challengers' relatives 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 (McCoy 2009).

3 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 Pamilyia 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 million individuals. For each individual we have data on age, gender, education, occupation, and family names. In 709 municipalities full enumeration of all residents took place. The data cover about 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 709 municipalities where full enumeration took place. The main concern here is that we

do not have information on how the pockets of poverty were selected which prevents us from recovering survey weights. In addition, there is a risk that since it is correlated with poverty it might also be correlated with political connections which would affect our main estimates. 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 individuals surveyed. The classification, developed by the National Statistics Office for its regular Labor Force Surveys (LFSs), include 11 occupations.[10] We rank them 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 on either the median or the 75th percentile in the distribution of daily wage in each occupation instead.

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 709 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 709 municipalities, so that we know who was elected and who was not. For the 2007 and 2010 mayoral and vice-mayoral elections we have the number of votes for all candidates.

3.1 A split sample approach

As indicated in the introduction, to deal with concerns about specification search and publication bias, we asked a third party to 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*) to which we do not have access yet. These are the results that will be reported in the published version of this paper. To the best of our

[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 collected 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.

knowledge, apart from forecasting purposes, this is the first time that this approach is used in economics and political science. The purpose is to provide credible estimates free of specification search and publication bias and to deliver adequately sized statistical tests. 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 2012). It is related to the strategy advocated by Humphreys, Sanchez de la Sierra and van der Windt (2013).[12]

We want to emphasize two important features of the proposed method. First, the method would be valuable even if we had strong priors regarding the most appropriate way to estimate the parameters of interest. Indeed, in such cases, researchers still have to make a number of micro-decisions regarding the precise way to define the dependent variables, the list of controls and the exact estimation sample. The method ensures that researchers do not, consciously or unconsciously, focus on regressions where the null hypothesis is rejected. Second, the information available to the referees and editor at the time of submission is similar to the information contained in regular submissions.

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.

[12]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 et al. 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.

3.2 Family ties

We take advantage of naming conventions in the Philippines to assess blood and marriage links between surveyed individuals and local politicians.[13] 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 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.

In the Philippines the process to change ones 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 a number of court cases which have sometimes reached the Supreme Court.[14]

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 she or someone in her household has a middle or last name matching the politician's middle or last name. The strategy has been used to assess blood links between municipal and provincial-level Filipino politicians through time (Cruz and Schneider 2013, Querubin 2011, Querubin 2013). In

[13]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.

[14]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.*

other contexts, Angelucci et al. (2010) used a similar strategy to measure family networks in Mexico.

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 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 0.964 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.[15] 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 each candidate's parents had three married children and six grandchildren. With these assumptions, each candidate would be directly connected to 13 individuals. If in addition, her parents had two siblings each, with three married children and six grandchildren each, a candidate would be indirectly connected to 56 individuals; for a total of 69 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 Reyez). Those sources of measurement errors generate an attenuation bias that works *against* rejecting the null of no effect.

3.3 Descriptive statistics

Descriptive statistics on employment by occupation are displayed in columns 1 and 2 of Table 1.[16] Simple comparisons reveal stark differences between individuals related to office holders

[15] Those statistics were computed using the full sample.
[16] Additional descriptive statistics are available in Table A.1.

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 2 percent in the population as a whole.

It is important to note that the category labelled as 'None' includes both individuals not in the labor force and unemployed individuals. About 40 percent of individuals are included in this category in our data which is consistent with official employment statistics. Indeed, between 2007 and 2010, labor force participation in the Philippines was around 65 percent and, among them, the unemployment rate was about seven percent.

4 Regression discontinuity design

We first 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 et al. forthcoming, Querubin and Snyder 2013), 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 the breakdown of votes for the top two candidates in the 2007 mayoral and vice-mayoral elections.

Let Y_{ij} be the outcome of interest for individual i in municipality j. We estimate a model of the form:

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

where α is the parameter of interest, C_{ij} is a dummy variable that equals one if individual i is related to an elected official in office in municipality j, f is an unknown smooth function, V_{ij} is i's relative vote margin of victory or defeat, and ϵ_{ij} is an idiosyncratic error term. Equation (1) is first estimated on a sample composed of relatives of candidates with a 2007 vote margin of +/- 5 percent.[17] For each sample, we follow Imbens and Lemieux (2008) and estimate

[17]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

equation (1) non-parametrically. We use the optimal bandwidth recommended by Imbens and Kalyanaraman (2012).[18] To check robustness of our findings we also estimate equation (1) with half the optimal bandwidth and twice the optimal bandwidth. In addition, we get similar results when using relatives of candidates with a 2007 vote margin of either +/- 2.5 percent or +/- 10 percent (Tables A.2 and A.3).

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 ranked occupation p. We estimate equation (1) for all Y^{p}.

Results are consistent with strong positive impacts of political connections on the probability of being employed in better-paid occupations (Table 2). The RDD estimate obtained with the optimal bandwidth suggests that connections increase the likelihood of being employed in either a professional or a managerial role by 7.42 percentage-points. Similarly, individuals connected to current office-holders appear to experience a 1.25 percentage-points increase in the probability of being employed in a managerial role. The point estimate represents 51 percent of the control group mean. At the bottom of the distribution, family ties do no appear to affect the likelihood of being employed. The point estimates decrease as the bandwidth increases. For example with the bandwidth set at half the optimal bandwidth, connections appear to lead to a 1.67 percentage point increase in the probability of being employed in a managerial position. With twice the optimal bandwidth, the point estimates correspond to 1.24 percentage point increase.

As discussed in the Introduction, we argue that in this particular context the RDD point estimates combine both the benefits from connections to current office-holders and the cost associated with being related to a candidate who lost which would explain the large point

in 27.5 percent of the municipalities in our sample. Comparing municipalities with and without close elections we find that there is no statistically significant difference in terms of poverty incidence, of the number of times the incumbent's family has been in office and of per capita fiscal transfers from the central government. There is some evidence that municipalities with close elections are slightly less populous (significant at the five percent level) but once we regress a dummy equal to one if either the mayoral or the 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 coefficient. The F-stat for the regression is 1.95 (p-value .114).

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

estimates obtained.[19] Indeed, individuals connected to candidates who were narrowly defeated in the 2007 elections might suffer from their connections. We present four distinct pieces of evidence consistent with this interpretation. First, we plot local polynomial regressions of the probability of being employed in the best-paying occupation on their relatives' vote share in the 2007 elections (Figure 1). While the estimated probability is more or less stable at three percent for individuals connected to losing candidates, starting at around 10 percentage-points, the probability drops drastically and reaches about two percent for individuals connected to politicians who were very narrowly defeated.

Second, to check that is not a general pattern of close elections, we also run some placebo tests and plot local polynomial regressions of the probability of being employed in the best-paying occupation on their relatives' vote share in the 2010 elections; that is after the data were collected. Not only there is no evidence of a drop for individuals connected to close losers, but there is also no evidence of discontinuity in the probability of being employed as a manager at the threshold (Figure 2).

Third, we compare individuals connected to unsuccessful candidates in the 2007 elections and in the 2010 elections (but who did not run in 2007) and plot local polynomial regressions of the probability of being employed in the best-paying occupation on their relatives' vote share (Figure A.1). For individuals connected to candidates who lost by a margin of less than five percentage-points, the probability of being employed as a manager is noticeably lower for individuals connected to 2007 candidates than for individuals connected to 2010 candidates. To test this more formally, we restrict the sample to individuals connected to losing candidates in either the 2007 or in the 2010 elections (but who did not run in 2007) and regress the probability of being employed in the best-paying occupation on a dummy equal to one if the individual is connected to a losing candidate in 2007. We are able to reject the null of no effect for the sample of individuals connected to candidates who lost by less than five percentage-points but not for individuals connected to candidates who lost by larger margins (Figure A.2). As

[19] Interpreting the RDD results could face additional challenges. First, the incentives of politicians who were narrowly elected might differ from those who were elected with wider margins (Vyborny and Haseeb 2013). Second, there is also some debates in the literature as to whether close elections are indeed random (Caughey and Sekhon 2011, Eggers, Folke, Fowler, Hainmueller, Hall and Snyder 2013).

discussed above, this is consistent with a theory of political control of the bureaucracy whereby incumbents attempt to staff the bureaucracy with individuals whose incentives are aligned with their own electoral objectives.[20] Data constraints prevent us from testing this directly.

Fourth, if potential punishment explains the size of the RDD estimates, we would expect them to be larger in municipalities where the incumbent lost than in municipalities where the incumbent managed to get re-elected. This is what we find (Table A.4). In municipalities where the incumbent lost, the RDD estimate obtained with the optimal bandwidth suggests that connections increase the likelihood of being employed as a manager increases by 2.45 percentage points (significant at the five percent level).[21] In municipalities where the incumbent won, the point-estimate drops to 0.44 percentage-points and we are unable to reject the null that it is different from zero at the usual levels of statistical significance.

Finally, in light of common concerns of manipulation around the threshold, we carry out standard balance tests, results of which are available in Column 1 of Table 3. Individuals on the right side of the threshold have .68 more years of education and 38 percent more relatives than individuals on the left side of the threshold. This has implications for the way the test developed by McCrary (2008) needs to be interpreted in our context. Indeed, we use data from the top two candidates in the mayoral and vice-mayoral elections and, by construction, for each candidate who managed to get elected with vote margin x there is another candidate that lost with vote margin $-x$; implying that the underlying density is smooth. However, in the case at hand, the test implicitly weights each candidate by the number of relatives. The McCrary statistics is .655 with a standard error of .044 which lead us to reject the null. We argue that this driven by differences in the number of relatives candidates have.

[20] 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. This is left for future research.

[21] It is important to note that this is not merely a short-run effect as most of the data were collected between 18 and 30 months after the mayors elected in 2007 assumed office.

5 Alternative estimation strategy

In this section we propose an alternative estimation strategy that seeks to address the possible interpretation challenges of the regression discontinuity approach applied to our data. Our aim is to obtain credible estimates of the causal effect of political connections on occupation in a way that distinguishes between the cost of losing an election and the benefit of winning one. We take a 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.

5.1 The benefits 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. We present several approaches to identify a control group and discuss their relative advantages and drawbacks. Our objective is to measure the benefits of political connections in a way that nets out the possible cost of being connected to someone who just lost an election.

In most contexts, since data on connections to unsuccessful candidates tend not to be available, 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:

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

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. We introduce the time subscript since, as opposed

to the RD discussion, we use candidates across two electoral cycles. Occupational choice might be correlated within provinces and we cluster standard errors at the provincial level.[22]

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, 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 unobserved heterogeneity associated with being related to someone who ran at least once in a local election. There are good reasons to expect $E[\mu_{ij}|C_{ijt} = 1] > E[\mu_{ij}|C_{ijt} = 0]$. 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* unobserved heterogeneity associated with being connected to a candidate who has won a local election at least once. 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 (2) 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

[22]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). We also show that our results are similar if we cluster standard errors either at the municipal-level (Table A.19) or along both month×year and province, using the two-way clustering method developed Cameron, Gelbach and Miller (2011) (Table A.20).

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 (2) 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. This is similar to the RDD set-up presented above but all individuals are weighted equally, irrespective of their relatives' vote share in the past election. 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 $\widehat{\alpha}$ obtained from (2) 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 to estimate the benefits of connections is vulnerable to one possible weakness identified above. 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. This is only a source of bias if we think of the counterfactual as the situation where none of individual i's relatives had ran for office. Alternatively, if we think of the counterfactual as the situation where the relative had ran but lost then the punishment is part of what we are trying to estimate. We argue that being able to separately identify the costs and benefits leads to a more precise interpretation of our findings.

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 (2) 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 $\widehat{\alpha}$ obtained using control group I yields an estimate of the punishment effect. Next, we discuss the method that allows us to also 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 (2) 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.[23] 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 being connected to an elected official for an individual.[24]

We report estimates using all three control groups. Comparison of $\widehat{\alpha}$ 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. 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 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,

[23] Of course, the maintained assumption here is that the pool of candidates is comparable across the two electoral cycles. We are unable to completely rule out slight differences between the groups.

[24] 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.

using names to infer family connections could be problematic in many countries but, for reasons discussed in Section 3.2, 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.

To better explain how the control groups are generated we now provide an example from the municipality of Aguilar in the province of Pangasinan. In the 2007 mayoral election, candidate Evangelista defeated candidate Zamuco for the position of mayor. In our set-up, individuals related to Evangelista are classified as being connected to the current office-holder and all individuals related to candidate Zamuco belong to control group I. In the 2010 election, candidate Evangelista ran against three candidates: De Los Santos, Sagles and Ballesteros. The latter won the election. Control group II consists of individuals related to one of the three opponents (Ballesteros, De Los Santos and Sagles). Control group III is made of individuals related to Ballesteros.

Coming back to the descriptive statistics presented above, Columns 3-5 of Table 1 suggests that a non-negligible share of the difference between individuals 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, 2.4 percent are employed in a managerial role, which is 20 percent more than in the general population.

Before proceeding further, we now report balance tests for our three control groups. Results are available in Columns 2-4 of Table 3. Overall, it appears that for the two key variables, years of education and network size, the gap between elected officials' relatives and the control groups is about half for control group than what it is when using the RDD approach. This reinforces our confidence that using relative of future winners as a control group is a valid strategy. In the analysis below, we use two strategies to deal with this lack of balance. We start by flexibly controlling for all variables and include a full set of dummies for each distinct value of each control variable. We also present robustness checks where all the key variables are fully saturated at the municipal-level. This will greatly reduce the risk that our results are driven by

lack of balance along observables.

5.2 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 (2) 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:

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

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:

$$Y_{ijt} = \alpha C_{ijt} + \beta X_{ijt} + \sum_{p=1}^{P} \delta_p C_{ijt} * (Z_j^p - \bar{Z}^p) + v_{jt} + u_{ijt} \tag{4}$$

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.

6 Econometric results

6.1 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 4). 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 mean.

As shown in Panels A and B of Table 4, 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.

As explained in Section 5, 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 – to net out unobserved heterogeneity μ_{ij}, we obtain qualitatively similar results to Table 4. A point made clearer by the comparison of the top left and top right corners of Figure 3. 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 4 and Panel A of Table 5). 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. Additional results are available in Table A.5.

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 (Panel B of Table 5). The purpose is to net out μ_{ij} and 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 and in Panel B of Table 5, continue to associate family ties to elected officials with better paid occupations. Additional results are available in Table A.6. As a point of comparison, we also estimate the regressions on the sample of individuals connected to unsuccessful candidates in 2010 who did not run in 2007, and find similar results (Table A.7).

Finally, we further restrict the control group to those individuals connected to *successful* candidates in the 2010 elections but who did not run in 2007 to net out both μ_{ij} and η_{ij}. This is control group III. Results, presented in the bottom right corner of Figure 3 and in Panel C

of Table 5, 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. Additional results are available in Table A.8.

6.2 Discussion and interpretation

What do we learn from comparing estimates obtained using different control groups? First, as anticipated, an upward bias seems to arise when we estimate the impacts of political connections 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 4 and A.5); similar results are obtained with control groups II and III, the latter also controls for η.

Second, 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. However, in a context where the bureaucracy is politicized, such costs might only be suffered by a small number of individuals. Indeed, incumbents might value loyalty, especially around election time, and might be reluctant to staff the bureaucracy with individuals whose views and interests are antinomic to theirs. Relative 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 might be the ones suffering such costs. This could explain why the point estimates obtained through RDD are higher than the ones obtained with any of the three control groups and why the RDD estimates increase as the bandwidth used decreases.

As point of further comparison with the RDD estimates provided above, we estimate equation (2) on the sample of individuals related to candidates for either mayor or vice-mayor in the 2007 elections (Table A.9). The RDD estimates are 60 percent larger than the regression point estimates on that subsample.

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. Consequently, the robustness checks presented in the next sub-Section focus on that control group.

We test for the impact of family ties on each occupation separately (Table A.10). We only find significant effects for two occupations: local politicians' relatives 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.

In addition, we estimate equation (2) for each Y^p $(p = 2, \ldots, 10)$ restricting the sample to individuals for which $Y^{p-1} = 1$. Given that we can consider occupation choice as a sequential decision, this is equivalent to estimating the conditional impacts of connections. This gives us additional information about where in the distribution of occupations connections have an impact.[25] Results, available in Table A.11, suggest that, even the conditional estimates are consistent with a positive impact of family connections on occupational choice. It is important to note that those estimates need to be interpreted with caution as, for each value of p, the probability of being included in the sample is correlated with the level of connections.

Before turning to robustness checks, we explore the effects of connections to local politicians in office on three alternative measures of job quality. First, assuming that everyone employed in occupation i earns the average daily wage in that occupation, we can put a monetary value on the effect of political connections. Using control group III, we find that being connected to a local politician in office leads to an increase of 2.27 Pesos per day (Column 1 of Table A.12).

[25] A simple example with two sequential decisions will highlight the differences between the conditional and unconditional estimates. Let's assume that connections affect the probability of going through the first step, but conditional of having gone through the first step connections do not affect the probability of going through the second step. That is, assuming $P(Y^1 = 1|C = 1) > P(Y^1 = 1|C = 0)$ and $P(Y^2 = 1|Y^1 = 1, C = 0) = P(Y^2 = 1|Y^1 = 1, C = 0)$, will lead to $P(Y^2 = 1|C = 1) > P(Y^2 = 1|C = 0)$.

This corresponds to 1.84 percent of the mean control group III.[26] Second, we get similar results if use median wage rather than mean wage in each occupation (Column 2 of Table A.12). Third, while we are unable to distinguish between the public and private sector in our data, we can compute the percentage of individuals employed in occupation p who work in the public sector from the nationally representative labor force surveys. We can then estimate the effect of being connected to a politician in office on the probability of being employed in the public sector. The point estimate is 0.204, which corresponds to 3.3 percent of the mean in control group III (Column 3 of Table A.12).

6.3 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. We ran a number of additional robustness checks that are not discussed here but are included in the online appendix.

First, so far we have not allowed for the possibility that the size of the individual's family network could affect occupational choice. Results presented in Table 3 indicate that this might be a concern and we take advantage of the data available to estimate equation (2) with a full set of dummies for the number of individuals who share the individual's last name in the municipality and for the number of individuals who share the individual's middle name in the municipality. Results are robust to this change (Panel A of Table 6) which deals with concerns that we are merely capture differences in network size. The full set of results are available in Table A.13.

Second, to reduce concerns about lack of balance, we introduced control variables flexibly by generating a different dummy for each value of our control 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 the age, gender and education variables and municipal dummies are all interacted with each other. This leads us to estimating equation (2) with about 250,000 fixed-effects. This is akin to a very

[26] Gagliarducci and Manacorda (2014) estimates that, in Italy, having one more relative in office is associated with a 1.6 percent increase in private sector earnings. They identify connected individuals through shared last name but only have data on the first three consonants of everyone last name and can only track family connections on the father's side. This generates both inclusion and exclusion errors in their connection measures.

restrictive matching estimator: identification comes from comparing connected individuals of the same gender, age, and education living in the same municipality. Point estimates, reported in Panel B of Table 6 are smaller but still economically and statistically significant. For example, being connected to an elected official leads to a 0.36 percentage-points increase in the probability of being employed in a managerial role. Further results are available in Table A.14.

Third, as indicated above, the main maintained assumption is that the pool of candidates is comparable across the two electoral cycles. Violation of that assumption would imply that our results might simply be capturing between candidates elected for the first time in 2007 and candidates elected for the first time in 2010. While we are unable to test this directly, we we estimate equation (2) on the sample of officials' relatives in municipalities where the incumbent mayor's family was elected for either the first or second time in 2007. If our results were driven by trends in the type of candidates running for office, we would expect officials' relatives in municipalities where the incumbent was elected for the second time in 2007 to be employed in better-paying occupations than officials' relatives in municipalities where the incumbent was elected for the first time in 2007. This is not what we find. Results are available in Panel C of Table 6.

Fourth, politicians are only able to stay in office for three consecutive terms, but political families in some municipalities circumvent those term limits by having different members of the same family take turns in office (Querubin 2011). In these municipalities, relatives of candidates elected in 2010 might not be valid counterfactuals for current office holders.[27] We re-estimate equation (2) focusing on municipalities where the mayor's family has been in office for three terms or fewer (Panel D of Table 6).[28] As expected the point estimates tend to be smaller, but they remain economically and statistically significant and they tend to be located at the top of the distribution of occupations. For example, in this subsample of municipalities, current office holder's relatives are 0.46 percentage-points more likely to be employed in a managerial role and we are unable to reject the null hypothesis that the point estimates are equal to the ones obtained on the full sample.

[27]This issue is discussed in detail in Ferraz and Finan (2011).

[28]We re-estimate equation (2) focusing on municipalities where the mayor's family has been in office fo two terms or fewer (Panel B of Table A.15) and one term (Panel C of Table A.15).

6.4 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) 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 A.16. The estimated impacts of a family tie to the mayor tend to be larger than for vice-mayors and municipal councilors. Furthermore, they are concentrated in the top of the occupational distribution. Mayor's relatives are 0.79 percentage-points more likely to be employed in a managerial position; the point estimate for municipal councilors' relatives is 0.42 percentage-points, a difference that is statistically different from zero at the 10 percent level.

Next we investigate whether the occupational benefit from family connections varies with observable individual characteristics. To this effect, we interact the family ties dummy with gender, age, and education. As is clear from Table 7, we find evidence of significant heterogeneity. First, the benefits from political connections are stronger for more educated individuals: each additional year of education is associated with a 0.11 percentage-point increase in the impacts of connections on 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 50 percent lower for women than it is for men. While local politicians' male relatives are less likely to be employed, no such effect is observed for female relatives. For other occupations, we find no significant difference between men and women. Third, the impacts of connections appear to be increasing with age.

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 4 we plot each point estimate and their associated 95 percent confidence interval, which shows a convex relationship between education level and the value of political connections.

This set of results is not consistent with simple models of patronage where unqualified individuals who are connected to politicians are provided with jobs. In such a setting, one would

expect less educated and inexperienced individuals related to politicians to benefit from connections the most. This is not what we find. While we do not have information about job requirements, further analyses suggest that connected individuals tend to be better educated than non-connected individuals employed in the same occupation. For example, among individuals who are employed in the best-paying occupation, 58.4 percent of individuals connected to office-holders are college graduate while 53.3 percent of unconnected individuals are. The corresponding figure for individuals in control group III is 54.8 percent.

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 8, we find that, in municipalities with higher per capita fiscal transfers, local politicians' relatives are less likely to be employed but also more likely to be employed in a managerial position.

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 8. 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 A.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.

Finally, in some municipalities members of the same family compete against each other in mayoral elections. In those municipalities our measure of connections likely does not capture the relevant ties to elected officials and as such, we would expect our estimated coefficients to be smaller there. As a result, we estimate equation (2) and interact our measure of connections to elected officials with a dummy indicating whether two members of the same family ran against each other during the 2007 mayoral elections. The effect is concentrated in municipalities

without within-family feuds in the 2007 mayoral elections (Table A.18). This does not imply that connections are not important in those municipalities but, more likely, that our measure of connections is not capturing the relevant ties in this specific context.

7 Conclusion

In this paper, we have provided evidence that family ties to a locally elected politician are associated with a better paid occupation. We argue that this association is causal.[29] 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.

Our results have a number of implications and suggest some ideas for further research. First, while we are unable to test this directly, we interpret our findings that close losers' relatives suffer from their connections as consistent with models of political control of the bureaucracy. This is in line with the argument that incumbents value loyalty and might be reluctant to staff the bureaucracy with individuals whose views and interests are incompatible with theirs. They attempt to staff the bureaucracy with individuals whose incentives are aligned with their own electoral objectives. Second, this could explain why the returns to connections increase with education levels as politicians provide jobs to their most educated relatives as they are the most capable of steering the bureaucracy. Further, this set of result is not consistent with a view that the effects are purely driven by elected officials' altruistic motives towards their relatives. Third, 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.

[29] We recognize that we are presenting estimates of the value of political connections in the short run. We will attempt to establish the dynamics of impacts in future research.

A question that remains unaddressed is whether or not the tendency for individuals related to office-holders to be employed in better-paying occupations affects the way services are delivered at the local level. Due to data constraints, we attempt to shed some light on this by correlating municipal-level measures of the extent to which political connections distort local labor markets with the quality of health service delivery, which have been devolved to the municipal-level (Capuno 2009, Khemani 2013).[30] We use 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 that ran either in the 2007 or 2010 municipal elections as a measure of distortion and find that an increase in the level of distortions is associated with an increase in the percentage of kids under the age of 6 that are underweight (Panel A of Table 9). The correlation is robust to controlling for a number of municipal characteristics, including poverty incidence, average education levels, gini and incumbent vote share in the previous election. Interestingly, once we focus on education, a sector which has not been devolved to the municipal-level, we find no correlation between those outcomes and labor market distortions associated with political connections (Panels B-E of Table 9). 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 affect the way services are delivered by the municipal bureaucracy.

[30]Given that the analysis is carried out at the municipal-level, we do not use the sample split approach for this set of regressions to avoid a strong loss in statistical power.

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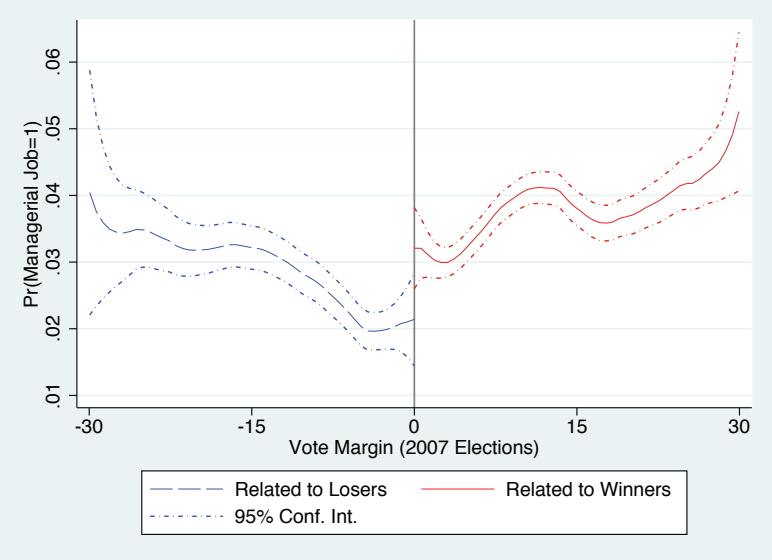

Figure 1: Non-parametric estimates of the probability of being employed in a managerial position

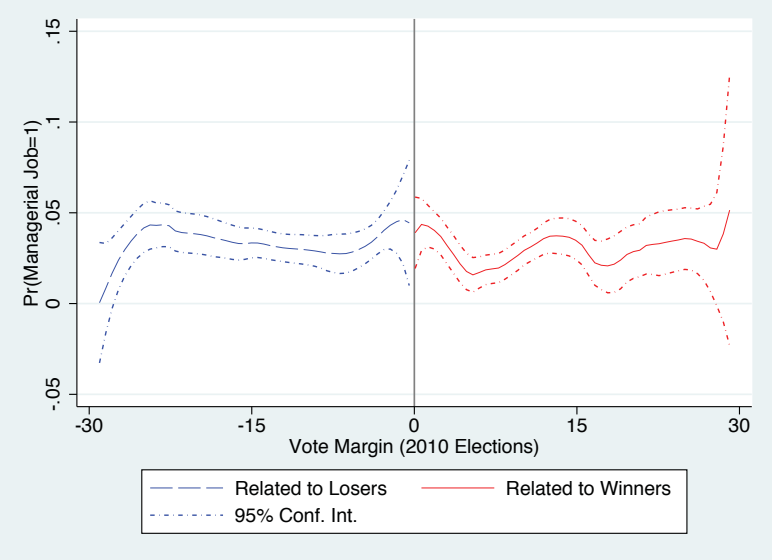

Figure 2: Non-parametric estimates of the probability of being employed in a managerial position

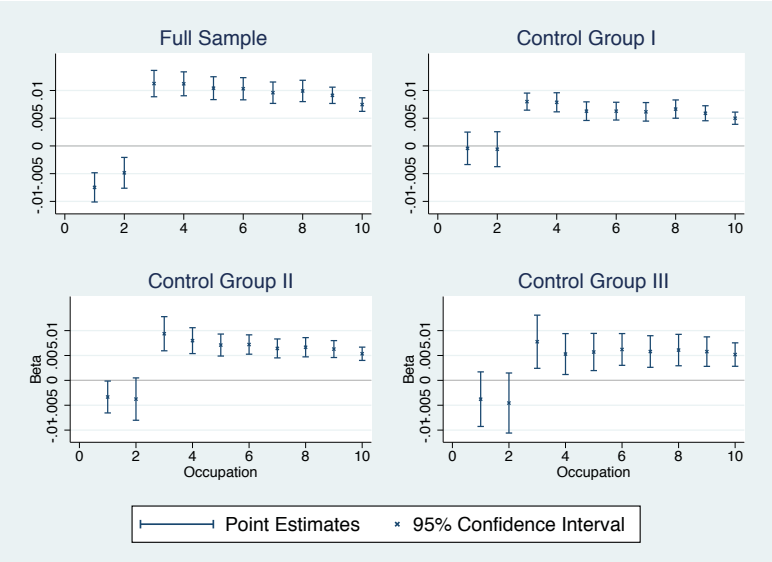

Figure 3: Estimated effects of connections with various control groups

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 A.5-A.8.

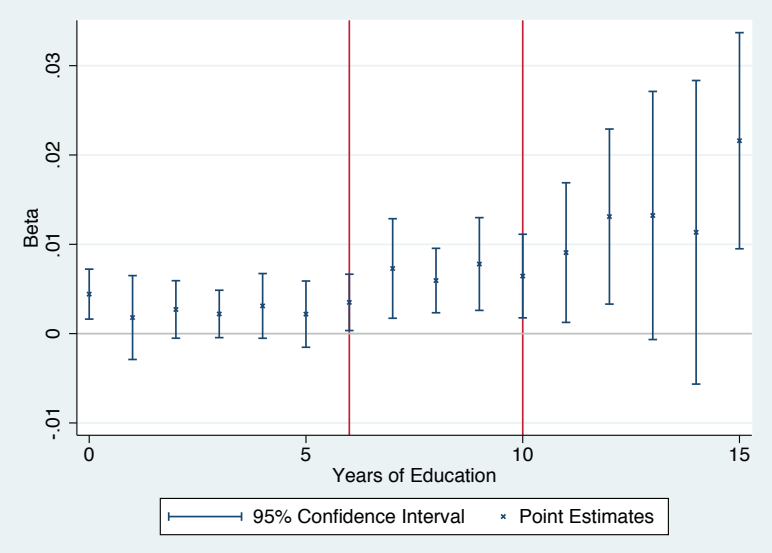

Figure 4: Estimated effects of connections by education levels

Table 1: Descriptive statistics: Individual-level

		Full Sample	Connected	Control Group		
				I	II	III
		(1)	(2)	(3)	(4)	(5)
Occupation						
0	None	40.28	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	395,392	390,300	239,431	58,952

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: 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)	0.0106	0.0740***	0.0961***	0.0830***	0.0778***	0.0830***	0.0723***	0.0669***	0.0742***	0.0125*
	(0.025)	(0.028)	(0.018)	(0.015)	(0.015)	(0.016)	(0.014)	(0.015)	(0.015)	(0.007)
Observations	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0446	0.1299***	0.1317***	0.1193***	0.1072***	0.0946***	0.0796***	0.0696***	0.0573***	0.0167**
	(0.037)	(0.037)	(0.023)	(0.020)	(0.019)	(0.018)	(0.016)	(0.018)	(0.019)	(0.008)
Observations	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	-0.0129	0.0068	0.0569***	0.0513***	0.0406***	0.0435***	0.0403***	0.0442***	0.0399***	0.0124**
	(0.018)	(0.019)	(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.006)
Observations	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427	30,427

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 3: 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	0.030	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 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)	-0.0041***	0.0049***	0.0415***	0.0377***	0.0355***	0.0348***	0.0322***	0.0307***	0.0280***	0.0145***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(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)	-0.0075***	-0.0048***	0.0112***	0.0112***	0.0104***	0.0103***	0.0096***	0.0099***	0.0091***	0.0075***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(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	0.0124***	0.0121***	0.0109***	0.0107***	0.0100***	0.0103***	0.0094***	0.0076***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(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 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 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 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)	0.001	0.001	0.0082***	0.0080***	0.0063***	0.0063***	0.0062***	0.0067***	0.0060***	0.0050***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692
R-squared	0.330	0.270	0.223	0.238	0.259	0.287	0.267	0.254	0.255	0.080
Panel B: Control Group II										
Connected Office (2007)	-0.001	-0.001	0.0100***	0.0085***	0.0075***	0.0075***	0.0067***	0.0070***	0.0066***	0.0056***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823
R-squared	0.332	0.271	0.226	0.240	0.261	0.290	0.270	0.258	0.259	0.083
Panel C: Control Group III										
Connected Office (2007)	-0.002	-0.002	0.0085***	0.0060***	0.0062***	0.0066***	0.0062***	0.0065***	0.0061***	0.0054***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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 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: Robustness Checks

	(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: Controlling for Network Size										
Connected Office (2007)	-0.001	-0.002	0.0097***	0.0067***	0.0070***	0.0071***	0.0067***	0.0068***	0.0062***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
R-squared	0.332	0.273	0.233	0.247	0.269	0.298	0.278	0.265	0.265	0.089
Panel B: Estimate a Saturated Model										
Connected Office (2007)	-0.003	-0.000	0.0106***	0.0071***	0.0066***	0.0064***	0.0045**	0.0047**	0.0050***	0.0036**
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R-squared	0.073	0.042	0.008	0.006	0.005	0.004	0.004	0.003	0.003	0.003
Panel C: First term vs. Second term										
Second term	0.019	0.019	-0.004	-0.003	-0.001	-0.006	-0.005	-0.006	-0.002	0.002
	(0.012)	(0.017)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
R-squared	0.322	0.251	0.220	0.235	0.257	0.285	0.263	0.249	0.252	0.082
Panel D: Municipalities where mayor's family has been in office three times or less										
Connected Office (2007)	-0.003	-0.002	0.0071**	0.003	0.0044**	0.0050***	0.0044**	0.0053***	0.0055***	0.0046***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
R-squared	0.330	0.268	0.231	0.243	0.264	0.293	0.272	0.258	0.259	0.087

Notes: Results from fixed-effects regressions. $n = 454,344$ (Panels A and B), $n = 170,030$ (Panel C) and $n = 277,285$ (Panel D). 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, 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, regressions include a full of set of dummies for the number of individuals who share the individual's middle name in the municipality and for the number of individuals who share the individual's middle name in the municipality. In Panel B, regressions include dummies for each interaction of the age, education, gender and municipal dummies. In Panel C, the sample is restricted to individuals connected to elected officials in 2007 in municipalities where the incumbent mayor was elected for the first or second time in 2007. In Panel D the sample is restricted to municipalities where the incumbent mayor's family has been in office less than 4 times. 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: 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	-0.0135***	-0.0123**	0.0086**	0.0082***	0.0074***	0.0079***	0.0077***	0.0076***	0.0064***	0.0074***
	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Connected*Female	0.0194**	0.016	-0.001	-0.0052*	-0.003	-0.003	-0.003	-0.002	-0.001	-0.0037***
	(0.008)	(0.010)	(0.006)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Connected*Edu	-0.000	0.001	0.001	0.0010*	0.0013**	0.0012**	0.0011**	0.0011**	0.0010**	0.0011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Connected*Age	-0.000	-0.0003*	0.0003**	0.0002*	0.0003***	0.0003***	0.0003***	0.0003***	0.0003***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.271	0.229	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	-0.0082**	-0.008	0.0096***	0.0091***	0.0081***	0.0084***	0.0081***	0.0079***	0.0067***	0.0076***
	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Connected*Female	0.0133*	0.011	-0.002	-0.0056*	-0.003	-0.003	-0.003	-0.002	-0.001	-0.0038***
	(0.008)	(0.009)	(0.006)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Connected*Edu	-0.000	0.000	0.001	0.0010*	0.0012**	0.0012**	0.0011**	0.0011**	0.0010**	0.0011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Connected*Age	-0.0003*	-0.0003*	0.0003**	0.0002*	0.0002***	0.0003***	0.0003***	0.0003***	0.0003***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.265	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 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 8: Municipal heterogeneity

	(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: Fiscal Transfers										
Connected	-0.004	-0.004	0.0083***	0.0057***	0.0062***	0.0064***	0.0060***	0.0062***	0.0062***	0.0056***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Interaction	-0.0089***	-0.0079**	-0.002	-0.001	0.000	-0.000	-0.001	-0.001	0.000	0.0024*
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	411,580	411,580	411,580	411,580	411,580	411,580	411,580	411,580	411,580	411,580
R-squared	0.330	0.267	0.231	0.246	0.268	0.296	0.276	0.263	0.263	0.088
Panel B: Nb of terms										
Connected	-0.002	-0.002	0.0086***	0.0060***	0.0061***	0.0066***	0.0061***	0.0064***	0.0062***	0.0053***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Interaction	0.002	0.001	0.001	0.0020**	0.0017**	0.0017**	0.0018***	0.0015**	0.0013**	0.0011**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Observations	436,509	436,509	436,509	436,509	436,509	436,509	436,509	436,509	436,509	436,509
R-squared	0.328	0.268	0.231	0.247	0.269	0.297	0.278	0.265	0.264	0.088
Panel C: Municipal Council										
Connected	-0.002	-0.003	0.0085***	0.0059***	0.0061***	0.0065***	0.0061***	0.0064***	0.0060***	0.0053***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Interaction	-0.003	-0.011	-0.011	-0.0136**	-0.0152**	-0.0130**	-0.0108*	-0.009	-0.006	-0.0081*
	(0.007)	(0.009)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
Observations	451,715	451,715	451,715	451,715	451,715	451,715	451,715	451,715	451,715	451,715
R-squared	0.330	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 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 9: 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	0.600	0.640	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	-0.10	-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	-1.90	-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	0.820	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	0.900	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.

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Additional Tables and Robustness Checks

Here we discuss additional robustness checks. First, some of the 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 now-known challengers' relatives, our results would be upward biased. To check for this possibility, we re-estimate equation (2) on the sample of individuals who were interviewed before November 2009. Again, results are robust to using this restricted sample (Panel A of Table A.21). 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 A.21).

Second, 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 (2) including village fixed-effects. As shown in Panel A of Table A.22, this does not affect the estimated value of α.

Third, we re-estimate equation (2) including enumerator $\times$ municipality fixed-effects to capture potential enumerator effects. Results are robust to this change (Panel B of Table A.22). 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.

Fourth, we re-estimate equation (2) using probit instead of a linear probability model. The results are presented in Panel C of Table A.22. For most outcomes the point estimates are of similar order of magnitude, although they are smaller for professional and managerial occupations.

Fifth, we estimate equation (2) including measures of name complexity (middle and last name length, middle and last name first letter) and name origin to capture potential name effects. We also estimate equation (2) on a sample excluding the small proportion of individuals with either an autochthonous middle or last name or a middle or last name of Chinese origin. Results are robust to both changes (Tables A.23 and A.24).

Sixth, enumerator quality might also have affected the way names were recorded. To check that our results are not driven by this, we estimate equation (2) on samples excluding municipalities at the top or bottom 5, 10 and 25 percent in the distribution of the share of individuals who are connected. Results are robust to excluding them (Tables A.25 and A.26). Similarly, results are robust to excluding municipalities at the top 5, 10 and 25 percent in the distribution of population (Table A.27). All of the estimates are of similar orders of magnitude as on the full sample which reduces concerns about measurement error in our indicator of family connections. In addition, some might be worried about strategic migration by officials' family members after the elections and we estimate equation (2) on samples excluding individuals at the bottom 5, 10 and 25 percent in the distribution of length of stay in their village of residence. Results are robust to excluding them (Table A.28).

Seventh, we have so far used the full sample of individuals aged 20-80. It is however possible that elected officials' older relatives may retire earlier, which would bias our estimates downwards. By a similar reasoning, politicians' younger relatives may postpone entry on the job market. To check for this possibility, we re-estimate equation (2) excluding either younger or older cohorts. Estimates are reported in Table A.29. 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 strengthen our results: coefficient estimates go up from 0.54 percentage-points to 0.60 percentage-points.

Table A.1: 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

Table A.2: 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)	0.0179	0.1289***	0.1322***	0.1160***	0.0738***	0.0633***	0.0764***	0.0648***	0.0597***	0.0126*
	(0.028)	(0.037)	(0.023)	(0.019)	(0.014)	(0.014)	(0.015)	(0.014)	(0.013)	(0.007)
Observations	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0442	0.1184**	0.0961***	0.0887***	0.1075***	0.0974***	0.0731***	0.0730***	0.0703***	0.0168**
	(0.038)	(0.051)	(0.029)	(0.024)	(0.019)	(0.018)	(0.018)	(0.017)	(0.015)	(0.008)
Observations	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	-0.0046	0.0772***	0.0899***	0.0783***	0.0428***	0.0346***	0.0508***	0.0416***	0.0263***	0.0123**
	(0.019)	(0.028)	(0.017)	(0.014)	(0.012)	(0.011)	(0.011)	(0.010)	(0.009)	(0.006)
Observations	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408	14,408

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 +/- 2.5 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 A.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)	0.0106	0.0374	0.0912***	0.0812***	0.0729***	0.0634***	0.0600***	0.0497***	0.0457***	0.0126*
	(0.026)	(0.024)	(0.017)	(0.015)	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.007)
Observations	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0446	0.1282***	0.1327***	0.1196***	0.1075***	0.0974***	0.0828***	0.0757***	0.0731***	0.0168**
	(0.037)	(0.035)	(0.023)	(0.020)	(0.019)	(0.018)	(0.016)	(0.016)	(0.015)	(0.008)
Observations	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	-0.0120	0.0090	0.0546***	0.0483***	0.0386***	0.0316***	0.0327***	0.0301***	0.0194**	0.0124**
	(0.018)	(0.017)	(0.013)	(0.012)	(0.011)	(0.011)	(0.010)	(0.010)	(0.009)	(0.006)
Observations	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368	55,368

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 +/- 10 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 A.4: 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 - Incumbent lost										
Connected Office (2007)	-0.0042	0.0601	0.1492***	0.1276***	0.1328***	0.1187***	0.1181***	0.1114***	0.1054***	0.0245**
	(0.036)	(0.038)	(0.026)	(0.022)	(0.023)	(0.022)	(0.021)	(0.020)	(0.019)	(0.012)
Observations	16,837	16,837	16,837	16,837	16,837	16,837	16,837	16,837	16,837	16,837
Panel B: Optimal Bandwidth - Incumbent won										
Connected Office (2007)	0.0358	0.0622	0.0557**	0.0440**	0.0362*	0.0296	0.0279	0.0115	0.0068	0.0044
	(0.036)	(0.048)	(0.026)	(0.022)	(0.020)	(0.020)	(0.022)	(0.020)	(0.021)	(0.008)
Observations	12,735	12,735	12,735	12,735	12,735	12,735	12,735	12,735	12,735	12,735

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 A.5: The effects of connections on the probability of being in any occupation 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	0.0037**	0.0210***	0.0196***	0.0175***	0.0173***	0.0163***	0.0160***	0.0146***	0.0082***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692
R-squared	0.330	0.050	0.037	0.026	0.025	0.026	0.025	0.026	0.018	0.015
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.000	-0.001	0.0080***	0.0079***	0.0063***	0.0063***	0.0062***	0.0066***	0.0059***	0.0050***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692
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	0.001	0.0082***	0.0080***	0.0063***	0.0063***	0.0062***	0.0067***	0.0060***	0.0050***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692	785,692
R-squared	0.330	0.270	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 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 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 A.6: The effects of connections on the probability of being in any occupation 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	0.003	0.0285***	0.0253***	0.0236***	0.0235***	0.0215***	0.0207***	0.0192***	0.0104***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823
R-squared	0.024	0.048	0.039	0.028	0.027	0.028	0.027	0.028	0.018	0.015
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0033**	-0.0038*	0.0094***	0.0080***	0.0071***	0.0072***	0.0064***	0.0067***	0.0063***	0.0053***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823
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	0.0100***	0.0085***	0.0075***	0.0075***	0.0067***	0.0070***	0.0066***	0.0056***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823	634,823
R-squared	0.332	0.271	0.226	0.240	0.261	0.290	0.270	0.258	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 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 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 A.7: The effects of connections on the probability of being in any occupation 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	0.004	0.0300***	0.0272***	0.0251***	0.0250***	0.0228***	0.0219***	0.0202***	0.0106***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871
R-squared	0.025	0.047	0.039	0.028	0.027	0.029	0.028	0.028	0.019	0.016
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0030*	-0.003	0.0096***	0.0088***	0.0075***	0.0076***	0.0067***	0.0069***	0.0065***	0.0053***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871
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	-0.001	0.0101***	0.0092***	0.0078***	0.0079***	0.0070***	0.0072***	0.0067***	0.0055***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871	575,871
R-squared	0.331	0.271	0.227	0.242	0.263	0.291	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 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 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 A.8: The effects of connections on the probability of being in any occupation 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	0.002	0.0230***	0.0192***	0.0190***	0.0194***	0.0181***	0.0176***	0.0164***	0.0093***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.025	0.048	0.038	0.028	0.028	0.029	0.028	0.029	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0078***	0.0053**	0.0057***	0.0062***	0.0058***	0.0061***	0.0058***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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	-0.002	0.0085***	0.0060***	0.0062***	0.0066***	0.0062***	0.0065***	0.0061***	0.0054***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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 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 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 A.9: The effects of connections on the probability of being in any occupation 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	0.0131***	0.0278***	0.0262***	0.0240***	0.0246***	0.0228***	0.0225***	0.0204***	0.0124***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314
R-squared	0.027	0.053	0.044	0.034	0.034	0.035	0.034	0.033	0.027	0.023
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	0.003	0.0063*	0.0097***	0.0097***	0.0081***	0.0089***	0.0082***	0.0089***	0.0077***	0.0076***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314
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	0.0076**	0.0100***	0.0100***	0.0083***	0.0091***	0.0084***	0.0090***	0.0080***	0.0078***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314	207,314
R-squared	0.334	0.276	0.244	0.262	0.282	0.308	0.289	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 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 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 A.10: The effects of connections on the probability of being in each occupation 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	-0.0214***	0.0038**	0.000	-0.000	0.0014***	0.001	0.001	0.0071***	0.0093***
	(0.003)	(0.004)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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	-0.0123***	0.003	-0.000	-0.001	0.000	-0.000	0.000	0.001	0.0052***
	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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	-0.0109***	0.003	-0.000	-0.000	0.000	-0.000	0.000	0.001	0.0054***
	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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 occupation 1 (Column 1), is employed in occupation 2 (Column 2), is employed in occupation 3 (Column 3), is employed in occupation 4 (Column 4), is employed in occupation 5 (Column 5), is employed in occupation 6 (Column 6), is employed in occupation 7 (Column 7), is employed in occupation 8 (Column 8), is employed in occupation 9 (Column 9) and is employed in occupation 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 A.11: The conditional effects of connections on the probability of being in each occupation using successful 2010 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	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)	0.002	0.0176***	-0.002	0.010	0.0117**	-0.001	0.0102**	0.0151*	0.0239**
	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.008)	(0.010)
Observations	269,482	204,852	85,093	60,409	50,224	43,617	39,822	36,563	30,579
R-squared	0.107	0.475	0.210	0.219	0.312	0.084	0.115	0.286	0.287
Panel B: Municipal Fixed Effects and Individual Controls (1)									
Connected Office (2007)	-0.004	0.0174***	-0.003	0.009	0.0114**	-0.002	0.0097**	0.0157*	0.0231**
	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.009)	(0.010)
Observations	269,482	204,852	85,093	60,409	50,224	43,617	39,822	36,563	30,579
R-squared	0.152	0.471	0.206	0.215	0.305	0.077	0.109	0.280	0.278
Panel C: Municipal Fixed Effects and Individual Controls (2)									
Connected Office (2007)	-0.003	0.0176***	-0.002	0.010	0.0117**	-0.001	0.0102**	0.0151*	0.0239**
	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.008)	(0.010)
Observations	269,482	204,852	85,093	60,409	50,224	43,617	39,822	36,563	30,579
R-squared	0.159	0.475	0.210	0.219	0.312	0.084	0.115	0.286	0.287

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed in occupations 2-10 (Column 1), is employed in occupations 3-10 (Column 2), is employed in occupations 4-10 (Column 3), is employed in occupations 5-10 (Column 4), is employed in occupations 6-10 (Column 5), is employed in occupations 7-10 (Column 6), is employed in occupations 8-10 (Column 7), is employed in occupations 9-10 (Column 8) and is employed in occupation 10 (Column 9). 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 A.12: The effects of connections on alternative outcomes

	(1)	(2)	(1)
Panel A: Municipal Fixed Effects			
Connected Office (2007)	7.0985***	6.9889***	0.9773***
	(1.248)	(1.220)	(0.153)
Observations	454,889	454,889	454,889
R-squared	0.024	0.024	0.029
Panel B: Municipal Fixed Effects and Individual Controls (1)			
Connected Office (2007)	1.8226**	1.7197**	0.1967**
	(0.816)	(0.773)	(0.093)
Observations	454,889	454,889	454,889
R-squared	0.283	0.285	0.254
Panel C: Municipal Fixed Effects and Individual Controls (2)			
Connected Office (2007)	2.2738***	2.1457***	0.2043**
	(0.773)	(0.736)	(0.093)
Observations	454,889	454,889	454,889
R-squared	0.306	0.306	0.255

Notes: Results from fixed-effects regressions. The dependent variable is equal to average wage of individuals employed in the same occupation as the individual (Column 1). The dependent variable is equal to median wage of individuals employed in the same occupation as the individual (Column 2). The dependent variable is equal to the share of individuals employed in the same occupation as the individual who are employed in the public sector (Column 3). 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 A.13: Robustness checks : Controlling for Network Size

	(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	0.0261***	0.0216***	0.0215***	0.0214***	0.0199***	0.0191***	0.0176***	0.0096***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.029	0.053	0.042	0.032	0.031	0.032	0.031	0.032	0.023	0.019
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0092***	0.0063***	0.0068***	0.0069***	0.0064***	0.0065***	0.0059***	0.0050***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.273	0.232	0.230	0.245	0.267	0.296	0.276	0.264	0.264	0.087
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.001	-0.002	0.0097***	0.0067***	0.0070***	0.0071***	0.0067***	0.0068***	0.0062***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.332	0.273	0.233	0.247	0.269	0.298	0.278	0.265	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 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 of set of dummies for the number of individuals who share the individual's middle name in the municipality and for the number of individuals who share the individual's middle name in the municipality. 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 A.14: 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	-0.002	0.0085***	0.0059***	0.0062***	0.0066***	0.0062***	0.0065***	0.0061***	0.0054***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.351	0.282	0.236	0.250	0.271	0.298	0.278	0.266	0.266	0.089
Panel B: Age/Edu/Gender specific dummies										
Connected Office (2007)	-0.002	-0.002	0.0082***	0.0056***	0.0059***	0.0063***	0.0059***	0.0061***	0.0058***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.355	0.287	0.248	0.264	0.287	0.315	0.297	0.286	0.288	0.104
Panel C: Age/Edu/Gender/Province specific dummies										
Connected Office (2007)	-0.001	-0.002	0.0084**	0.0053**	0.0056***	0.0059***	0.0052***	0.0054***	0.0049***	0.0043***
	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.461	0.405	0.366	0.379	0.399	0.425	0.408	0.399	0.395	0.263
Panel C: Age/Edu/Gender/Muni specific dummies										
Connected Office (2007)	-0.003	-0.000	0.0106***	0.0071***	0.0066***	0.0064***	0.0045**	0.0047**	0.0050***	0.0036**
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.073	0.042	0.008	0.006	0.005	0.004	0.004	0.003	0.003	0.003

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, 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, the age*education*gender dummies are interacted with province dummies. In Panel D, the age*education*gender dummies are interacted with municipal 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 A.15: Robustness checks: 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)	-0.003	-0.002	0.0071**	0.003	0.0044**	0.0050***	0.0044**	0.0053***	0.0055***	0.0046***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	277,285	277,285	277,285	277,285	277,285	277,285	277,285	277,285	277,285	277,285
R-squared	0.330	0.268	0.231	0.243	0.264	0.293	0.272	0.258	0.259	0.087
Panel B: Municipalities where mayor's family has been in office twice or less										
Connected Office (2007)	-0.006	-0.004	0.0070**	0.003	0.002	0.003	0.002	0.0030*	0.0028*	0.0036**
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	195,862	195,862	195,862	195,862	195,862	195,862	195,862	195,862	195,862	195,862
R-squared	0.335	0.274	0.231	0.240	0.262	0.290	0.269	0.255	0.256	0.088
Panel C: Municipalities where mayor's family has been in office once										
Connected Office (2007)	-0.006	-0.003	0.006	0.003	0.003	0.003	0.003	0.0044*	0.0038*	0.0028*
	(0.004)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	125,803	125,803	125,803	125,803	125,803	125,803	125,803	125,803	125,803	125,803
R-squared	0.337	0.271	0.227	0.237	0.258	0.283	0.262	0.248	0.249	0.078

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 A.16: The marginal effects of connections to each type of elected official on the probability of being in any occupation - 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: Mayor (2007)	0.003	-0.001	0.0154***	0.0141***	0.0132***	0.0125***	0.0108***	0.0109***	0.0093***	0.0079***
	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
B: Vice-Mayor (2007)	0.001	0.004	0.0100***	0.0085***	0.0063**	0.0070***	0.0061***	0.0066***	0.0060**	0.0057***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
C: Councilor (2007)	0.000	-0.002	0.0084***	0.0062***	0.0055***	0.0056***	0.0051***	0.0053***	0.0045***	0.0042***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Test H0: A =B	0.034	0.856	1.854	2.248	5.232	3.482	2.564	1.977	1.096	0.776
Ha: A $\neq$ B [p-value]	[0.854]	[0.355]	[0.173]	[0.134]	[0.022]	[0.062]	[0.109]	[0.160]	[0.295]	[0.378]
Test H0: A =C	0.363	0.022	4.518	6.224	9.054	8.333	5.106	5.406	3.966	3.622
Ha: A $\neq C$ [p-value]	[0.547]	[0.882]	[0.034]	[0.013]	[0.003]	[0.004]	[0.024]	[0.020]	[0.046]	[0.057]
Test H0: B =C	0.068	1.292	0.157	0.651	0.083	0.350	0.203	0.328	0.259	0.414
Ha: B $\neq$ C [p-value]	[0.794]	[0.256]	[0.692]	[0.420]	[0.773]	[0.554]	[0.652]	[0.567]	[0.611]	[0.520]
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344

Notes: Mean marginal effects from fixed-effects regressions. The regressions include all possible interactions of the three dummies (A, B and C). 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 A.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 (2)										
Mayor (2007)	-0.000	-0.003	0.0125***	0.0125***	0.0117***	0.0113***	0.0102***	0.0103***	0.0088***	0.0073***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Vice-Mayor (2007)	-0.002	0.003	0.0116***	0.0091***	0.0080***	0.0090***	0.0080***	0.0087***	0.0078***	0.0069***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Councilor (2007)	0.001	-0.002	0.0091***	0.0069***	0.0057***	0.0056***	0.0049***	0.0050***	0.0040***	0.0040***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Int. Mayor (2007)	-0.009	-0.014	-0.013	-0.0158*	-0.0139*	-0.012	-0.0130*	-0.0140*	-0.0122*	-0.0115*
	(0.008)	(0.014)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)
Int. Vice-Mayor (2007)	-0.0208*	-0.013	-0.0162*	-0.013	-0.0188**	-0.0138*	-0.011	-0.009	-0.007	-0.005
	(0.012)	(0.015)	(0.008)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)
Int. Councilor (2007)	0.004	0.000	-0.001	-0.003	-0.006	-0.006	-0.005	-0.003	0.000	-0.005
	(0.006)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)	(0.004)
Observations	451,715	451,715	451,715	451,715	451,715	451,715	451,715	451,715	451,715	451,715
R-squared	0.330	0.271	0.232	0.246	0.268	0.297	0.277	0.265	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 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 A.18: Municipal heterogeneity: Within-Family Feud

	(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	0.003	0.0254***	0.0213***	0.0214***	0.0215***	0.0200***	0.0196***	0.0180***	0.0104***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)
Interaction	-0.005	-0.009	-0.0168*	-0.0148**	-0.0166**	-0.0144**	-0.0129*	-0.0139**	-0.0109*	-0.0077*
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.004)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.025	0.048	0.038	0.028	0.028	0.029	0.028	0.029	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.003	-0.004	0.0092***	0.0065***	0.0072***	0.0074***	0.0068***	0.0073***	0.0066***	0.0060***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Interaction	-0.003	-0.005	-0.010	-0.0084**	-0.0103**	-0.0081**	-0.0071*	-0.0085**	-0.0059*	-0.0056*
	(0.007)	(0.008)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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)	-0.001	-0.002	0.0096***	0.0068***	0.0074***	0.0076***	0.0070***	0.0075***	0.0068***	0.0061***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Interaction	-0.004	-0.005	-0.0102*	-0.0083**	-0.0101**	-0.0079**	-0.0069*	-0.0083**	-0.0057*	-0.0055*
	(0.007)	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.331	0.271	0.231	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The interaction variable is a dummy equal to one if the individual is connected to a politician in office in a municipality where members of the same family competed against each other in mayoral elections. 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 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 A.19: Robustness checks: Municipal 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	0.002	0.0230***	0.0192***	0.0190***	0.0194***	0.0181***	0.0176***	0.0164***	0.0093***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.025	0.048	0.038	0.028	0.028	0.029	0.028	0.029	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0078***	0.0053***	0.0057***	0.0062***	0.0058***	0.0061***	0.0058***	0.0052***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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	-0.002	0.0085***	0.0060***	0.0062***	0.0066***	0.0062***	0.0065***	0.0061***	0.0054***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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 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 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 municipality. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.20: 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	0.002	0.0230***	0.0192***	0.0190***	0.0194***	0.0181***	0.0176***	0.0164***	0.0093***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.025	0.048	0.038	0.028	0.028	0.029	0.028	0.029	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0078***	0.0053**	0.0057***	0.0062***	0.0058***	0.0061***	0.0058***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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	-0.002	0.0085***	0.0060**	0.0062***	0.0066***	0.0062***	0.0065***	0.0061***	0.0054***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
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 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 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 month/year of the interview and province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.21: 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 (2)										
Connected Office (2007)	-0.001	0.000	0.0116***	0.0079***	0.0077***	0.0081***	0.0075***	0.0076***	0.0076***	0.0065***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	326,656	326,656	326,656	326,656	326,656	326,656	326,656	326,656	326,656	326,656
R-squared	0.319	0.262	0.229	0.242	0.266	0.296	0.275	0.262	0.262	0.088
Panel B: Add Interaction Term										
Connected Office (2007)	-0.0012	-0.0001	0.0116***	0.0080***	0.0078***	0.0081***	0.0076***	0.0077***	0.0076***	0.0065***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Connected Office (2007) X	0.0000	0.0001	-0.0003	-0.0004	-0.0003	-0.0002	-0.0003	-0.0004	-0.0003	-0.0001
Months before 11/2009	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	326,656	326,656	326,656	326,656	326,656	326,656	326,656	326,656	326,656	326,656
R-squared	0.319	0.262	0.229	0.242	0.266	0.296	0.275	0.262	0.262	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 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, 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 A.22: 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	-0.002	0.0068***	0.0044*	0.0056***	0.0058***	0.0050***	0.0051***	0.0055***	0.0053***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.354	0.308	0.260	0.266	0.285	0.313	0.294	0.282	0.280	0.112
Panel B: Enumerator*Municipal Fixed Effects and Individual Controls										
Connected Office (2007)	-0.0046*	-0.003	0.0068***	0.0040*	0.0051***	0.0052***	0.0049***	0.0052***	0.0053***	0.0054***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344	454,344
R-squared	0.376	0.326	0.284	0.296	0.314	0.342	0.325	0.314	0.309	0.152
Panel C: Municipal Fixed Effects and Individual Controls - PROBIT										
Connected Office (2007)	-0.002	-0.003	0.0091***	0.0053***	0.0049***	0.0045***	0.0039***	0.0039***	0.0029***	0.0021***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Observations	454,320	454,307	454,261	454,215	454,200	454,203	454,203	454,203	453,822	451,748

Notes: Results from fixed-effects regressions. In Panel C, marginal effects computed at the mean are reported. 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 A.23: Robustness checks: Control for Measures of Name Complexity

	(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	0.001	0.0227***	0.0189***	0.0186***	0.0191***	0.0177***	0.0172***	0.0162***	0.0092***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949
R-squared	0.025	0.048	0.040	0.030	0.029	0.031	0.030	0.030	0.021	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0077***	0.0052**	0.0055***	0.0061***	0.0056***	0.0059***	0.0057***	0.0051***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949
R-squared	0.271	0.230	0.229	0.244	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	0.0081***	0.0056***	0.0057***	0.0063***	0.0059***	0.0062***	0.0059***	0.0053***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949	452,949
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 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 middle name length, last name length, middle name's first letter, last name's first letter and whether the middle or last name is autochthonous or of Chinese origin. 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 A.24: Robustness checks: Exclude individuals with either Autochthonous or Chinese Family Names

	(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	0.001	0.0222***	0.0188***	0.0184***	0.0189***	0.0175***	0.0172***	0.0160***	0.0087***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376
R-squared	0.025	0.048	0.038	0.027	0.027	0.028	0.028	0.028	0.019	0.017
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0074**	0.0053**	0.0055***	0.0061***	0.0056***	0.0061***	0.0057***	0.0047***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376
R-squared	0.272	0.230	0.227	0.241	0.264	0.294	0.274	0.261	0.262	0.085
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.002	-0.003	0.0077***	0.0057**	0.0057***	0.0063***	0.0058***	0.0063***	0.0059***	0.0048***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376	440,376
R-squared	0.332	0.272	0.230	0.244	0.266	0.295	0.275	0.263	0.263	0.087

Notes: Results from fixed-effects regressions. The sample excludes individuals with either an autochthonous middle or last name or a middle or last name of Chinese origin. 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 middle name length, last name length, middle name's first letter, last name's first letter, 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 A.25: Robustness checks: Exclude outlying municipalities (share connected)

	(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: Exclude top 5%										
Connected Office (2007)	-0.001	-0.003	0.0083***	0.0060***	0.0063***	0.0069***	0.0065***	0.0068***	0.0062***	0.0056***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	416,293	416,293	416,293	416,293	416,293	416,293	416,293	416,293	416,293	416,293
R-squared	0.331	0.269	0.231	0.246	0.267	0.296	0.277	0.265	0.264	0.089
Panel B: Exclude top 10%										
Connected Office (2007)	-0.001	-0.003	0.0086***	0.0057***	0.0060***	0.0069***	0.0065***	0.0068***	0.0062***	0.0057***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	389,551	389,551	389,551	389,551	389,551	389,551	389,551	389,551	389,551	389,551
R-squared	0.331	0.266	0.232	0.247	0.269	0.299	0.279	0.267	0.267	0.091
Panel C: Exclude top 25%										
Connected Office (2007)	-0.001	-0.002	0.0075**	0.0051**	0.0057***	0.0066***	0.0065***	0.0069***	0.0062***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	283,887	283,887	283,887	283,887	283,887	283,887	283,887	283,887	283,887	283,887
R-squared	0.330	0.271	0.235	0.246	0.265	0.294	0.275	0.261	0.261	0.091

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, 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 A.26: Robustness checks: Exclude outlying municipalities (share connected)

	(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: Exclude bottom 5%										
Connected Office (2007)	-0.001	-0.003	0.0089***	0.0064***	0.0064***	0.0067***	0.0062***	0.0064***	0.0063***	0.0055***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	447,572	447,572	447,572	447,572	447,572	447,572	447,572	447,572	447,572	447,572
R-squared	0.331	0.271	0.232	0.246	0.269	0.297	0.277	0.265	0.264	0.088
Panel B: Exclude bottom 10%										
Connected Office (2007)	-0.001	-0.003	0.0087***	0.0060***	0.0058***	0.0061***	0.0055***	0.0059***	0.0061***	0.0055***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	438,037	438,037	438,037	438,037	438,037	438,037	438,037	438,037	438,037	438,037
R-squared	0.331	0.270	0.231	0.246	0.268	0.297	0.277	0.265	0.264	0.087
Panel C: Exclude bottom 25%										
Connected Office (2007)	-0.001	-0.003	0.0095***	0.0059***	0.0060***	0.0063***	0.0056***	0.0061***	0.0065***	0.0064***
	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	392,007	392,007	392,007	392,007	392,007	392,007	392,007	392,007	392,007	392,007
R-squared	0.331	0.269	0.228	0.243	0.266	0.294	0.274	0.262	0.261	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 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, 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 A.27: Robustness checks: Exclude outlying municipalities (population)

	(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: Exclude top 5%										
Connected Office (2007)	-0.002	-0.004	0.0096***	0.0065***	0.0061***	0.0066***	0.0061***	0.0064***	0.0060***	0.0050***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	405,176	405,176	405,176	405,176	405,176	405,176	405,176	405,176	405,176	405,176
R-squared	0.335	0.278	0.233	0.248	0.271	0.298	0.279	0.267	0.267	0.090
Panel B: Exclude top 10%										
Connected Office (2007)	-0.001	-0.003	0.0103***	0.0080***	0.0075***	0.0075***	0.0069***	0.0071***	0.0068***	0.0050***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	372,528	372,528	372,528	372,528	372,528	372,528	372,528	372,528	372,528	372,528
R-squared	0.337	0.279	0.234	0.249	0.271	0.298	0.279	0.267	0.268	0.088
Panel C: Exclude top 25%										
Connected Office (2007)	0.000	-0.004	0.0134***	0.0104***	0.0100***	0.0090***	0.0082***	0.0087***	0.0082***	0.0062***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	291,874	291,874	291,874	291,874	291,874	291,874	291,874	291,874	291,874	291,874
R-squared	0.337	0.287	0.237	0.251	0.271	0.295	0.276	0.265	0.267	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 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, 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 A.28: Robustness checks: Exclude outlying individuals (length of stay in the village)

	(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: Exclude bottom 5%										
Connected Office (2007)	-0.002	-0.004	0.0085***	0.0062***	0.0063***	0.0068***	0.0063***	0.0068***	0.0064***	0.0056***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	419,083	419,083	419,083	419,083	419,083	419,083	419,083	419,083	419,083	419,083
R-squared	0.324	0.270	0.234	0.249	0.271	0.298	0.279	0.266	0.266	0.089
Panel B: Exclude bottom 10%										
Connected Office (2007)	-0.002	-0.004	0.0086***	0.0061***	0.0063***	0.0066***	0.0063***	0.0067***	0.0064***	0.0058***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	405,341	405,341	405,341	405,341	405,341	405,341	405,341	405,341	405,341	405,341
R-squared	0.321	0.269	0.234	0.249	0.272	0.299	0.279	0.267	0.266	0.090
Panel C: Exclude bottom 25%										
Connected Office (2007)	-0.003	-0.005	0.0089***	0.0058**	0.0060***	0.0063***	0.0062***	0.0063***	0.0062***	0.0053***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	320,459	320,459	320,459	320,459	320,459	320,459	320,459	320,459	320,459	320,459
R-squared	0.302	0.261	0.235	0.252	0.275	0.300	0.281	0.268	0.267	0.090

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, 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 A.29: 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	-0.000	0.0084***	0.0062***	0.0061***	0.0063***	0.0058***	0.0062***	0.0061***	0.0054***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	408,386	408,386	408,386	408,386	408,386	408,386	408,386	408,386	408,386	408,386
R-squared	0.338	0.275	0.236	0.251	0.272	0.302	0.281	0.268	0.267	0.090
Panel B: Age > 22										
Connected Office (2007)	-0.002	-0.003	0.0092***	0.0063***	0.0068***	0.0071***	0.0067***	0.0070***	0.0068***	0.0060***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	406,311	406,311	406,311	406,311	406,311	406,311	406,311	406,311	406,311	406,311
R-squared	0.329	0.265	0.243	0.256	0.279	0.309	0.288	0.275	0.274	0.092
Panel C: Age > 22 and Age < 63										
Connected Office (2007)	-0.000	-0.001	0.0091***	0.0066***	0.0068***	0.0068***	0.0063***	0.0067***	0.0069***	0.0060***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	360,353	360,353	360,353	360,353	360,353	360,353	360,353	360,353	360,353	360,353
R-squared	0.335	0.267	0.248	0.261	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 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, 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.

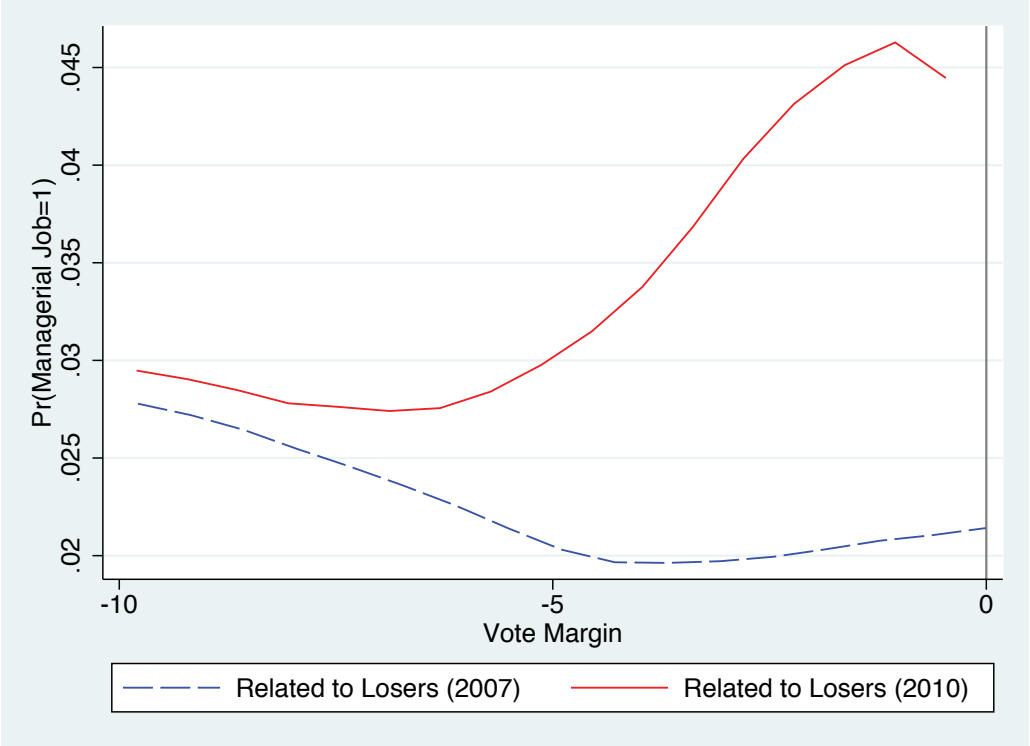

Figure A.1: Non-parametric estimates of the probability of being employed in a managerial position

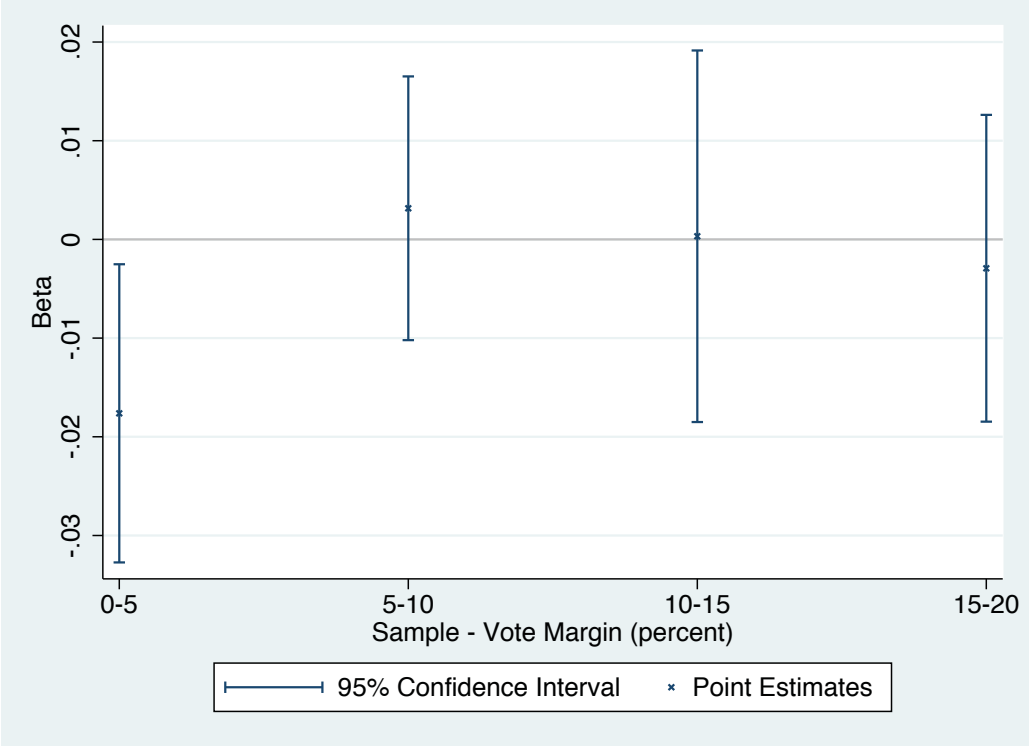

Figure A.2: Estimated Effects by Loss Margin