CDDRL

WORKING PAPER

May 2015

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

Marcel Fafchamps
Stanford University

Julien Labonne
University of Oxford

Stanford | CDDRL Center on Democracy, Development, and the Rule of Law

FSI Freeman Spogli Institute for International Studies

CDDRL

ABOUT CDDRL

Since 2002, the Center on Democracy, Development and the Rule of Law (CDDRL) at Stanford University has collaborated widely with academics, policymakers and practitioners around the world to advance knowledge about the conditions for and interactions among democracy, broad-based economic development, human rights, and the rule of law.

The mission of CDDRL is to understand how countries can overcome poverty, instability, and abusive rule to become prosperous, just, democratic, and well-governed states. This concern for the overall trajectory of national development—and for the intricate links among the economic, political, legal, social, and health dimensions of development—sets CDDRL apart from other research centers.

Center on Democracy, Development and the Rule of Law
Freeman Spogli Institute for International Studies
Stanford University
Encina Hall
616 Serra St.
Stanford, CA 94305-6055

Voice: 650-723-4610
Fax: 650-724-2996

Website: http:/cddrl.fsi.stanford.edu

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

Marcel Fafchamps and Julien Labonne*

May 2015

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 apply a regression discontinuity design to close elections in 2007. We then use individuals connected to successful candidates in 2010 as control group to net out the possible cost associated with being related to a losing candidate. We find that relatives of current office-holders are more likely to be employed in better paying occupations.

*Fafchamps: Stanford University, Freeman Spogli Institute for International Studies, Encina Hall E105, Stanford CA 94305 USA (fafchamp@stanford.edu). Labonne: Oxford University, Blavatnik School of Government, 10 Merton Street, Oxford OX1 4JJ, United Kingdom (julien.labonne@bsg.ox.ac.uk). An earlier version of this paper was circulated under the title 'Nepotism and Punishment: the (Mis-)Performance of Elected Local Officials in the Philippines'. We are indebted to Lorenzo Ductor for agreeing to act as the third party who performed the random sample split. The Department of Social Welfare and Development kindly allowed us to use data from the National Household Targeting System for Poverty Reduction and Pablo Querubin kindly shared some electoral data. We thank Fermin Adriano, Farzana Afridi, Sam Asher, Jean-Marie Baland, Hrithik Bansal, Andrew Beath, Cesi Cruz, Lorenzo Ductor, Taryn Dinkelman, Motoky Hayakawa, Bert Hofman, Clement Imbert, Philip Keefer, Claire Labonne, Horacio Larreguy, Clare Leaver, Pablo Querubin, Simon Quinn, Ronald Rogowski, Matt Stephens and Kate Vyborny as well as seminar and conference participants in the CSAE 2012, South East Asia Symposium 2012, Gorman Workshop, UPSE, AIM Policy Center, DIAL 2013, CMPO Political Economy and Public Services workshop 2013, IFS/EDEPO, RECODE, CERDI, NEUDC 2013, Blavatnik School of Government, NYU Abu Dhabi, IIES, Monash, Yale-NUS, CGD, World Bank and Ateneo School of Government for comments. All remaining errors are ours.

1 Introduction

The focus of this paper is the value of political connections for individuals. The literature on this issue has faced several difficulties and is not well-developed.[1] First, 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 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.

In this paper we use a large dataset from the Philippines, collected between the 2007 and 2010 municipal elections, to test whether people who are related to a successful local politician are more likely to be employed in a higher-ranked, better paid occupation. This could arise for different reasons. One possibility is nepotism – politi-

[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.

cians 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). 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. It is also conceivable that employers recruit the elected officials' relatives in the hope of securing political support and protection. Given the data at our disposal, there is little we can do to distinguish between these different possibilities.

We contribute to the literature on the value of political connections in four ways. First, our dataset is unusually rich as it includes non-anonymized information on all individuals in a large number of municipalities combined with detailed electoral data across two elections.[2] Second, we use different estimation strategies, including regression discontinuity based on close elections, and various control groups. This allows us to estimate the value of being connected to an elected local official net of the potential cost of being connected to a losing candidate. 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 follow Spanish rules, they allow an unusually precise and objective identification of family ties. Fourth, we show that there is a reshuffling of rationed jobs among the local elites which leads to concerns regarding equality of opportunity. Data constraints prevent us from testing whether this reshuffling has any effect on

[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)).

efficiency.

To address concerns about specification search and publication bias (Leamer 1978, Leamer 1983, Glaeser 2006), we 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 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 is observed using a regression discontinuity design based on close elections, and it 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, specifications, and estimation techniques. The paper should be seen as documenting differences in equal opportunity. Given the data, we are unable to test whether it has any effect on efficiency. The point estimates at the top of the distribution correspond to 3.2 percent of the managerial jobs in the public sector in each municipality. As a result, we argue that this is not inconsistent with the view that the effect is driven by preferential treatment of relatives as managers in the public sector.

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: the impact decreases with the number of elected municipal councilors who did not run on the mayor's ticket. and the impact is larger in areas where the incumbent has been in office for longer. This latter finding is consistent with the view that strong family ties are correlated with patrimonialism and undermine democracy (Fukuyama 2011, Alesina and Giuliano 2013).

We do not have sufficient data to ascertain the channel of the effect but our results offer some suggestive evidence as to how these effects materialize. First, since the benefits of political connections are stronger for educated individuals, it is unlikely that our results are solely driven by politicians' redistributive motives towards their relatives since educated individuals are on average less poor.[4] Second, we find that individuals connected to candidates who were almost elected are less likely to be employed in managerial positions. This suggests that 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). It also agrees with qualitative evidence on the behavior of Filipino politicians (Cullinane 2009, Sidel 1999).[5] We nonetheless refrain from proposing a benevolent interpretation of our findings since we do not know

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

[5] 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.

whether incumbents favor relatives to improve the way services are being delivered or merely for electoral gains.

The results presented in this paper have a number of implications for the empirical 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 costs incurred by individuals related to the loser. It may be informative to be able to separate the two empirically. The results also provide 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.

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.[6] 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

[6]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.

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 eight municipal councilors. They are all elected at-large in first-past the post elections 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.'*[7]

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.[8] In addition, it is possible that local businessmen favor local 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.

[7]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.

[8]Similarly, Hollnsteiner (1963) argues that Filipino voters expect incumbents to help them secure employment. Municipal politicians have incentives to develop relationships with those having influence positions in private business. This is especially true of owners of large companies as they need many unskilled workers such as messengers, labourer, guards and janitors, and many white collar workers such as clerks, accountants, collectors so that politicians might be able to help individuals secure employment across the distribution of jobs (Hollnsteiner 1963).

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 re-

covering 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.[9] We rank them according to their average daily wage, computed using wage data from eight nationally representative LFSs collected in 2008 and 2009.[10] 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

[9]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.

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

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 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).[11]

We want to emphasize three 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, among others, define the list of control variables and the list of heterogeneous treatment effects to carry out. 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. Third, the method reduces the risk of publication bias as editors decide whether to publish a paper before seeing the final results.

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

[11]These authors argue that researchers carrying out RCTs should write mock reports with fake data before the real data become available in order to distinguish between exploratory analyses and genuine tests (Humphreys 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.

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.

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.[12] Names used in the Philippines were imposed by Spanish colonial officials in the mid-19th century. One of the stated objective was to distinguish families at the municipal-level to facilitate census-taking and tax collection (Scott 1998, Gealogo 2010). Last names were selected from the *Catalogo alfabetico de apellidos*, a list of Spanish 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 one's middle or last name is long and the probability of success is low. This reduces concerns about strategic name changes. Article 376 of the Civil Code of the Philippines (Republic Act No. 386, 1949) states that *No person can change his name or surname without judicial authority.* This has been upheld in a number of court cases which have sometimes reached the Supreme Court.[13]

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

[13]For example, in the case Wang v. Cebu City Civil Registrar (G.R. No. 159966, 30 March 2005, 454 SCRA 155), Justice Tinga indicated that *the Court has had occasion to express the view that the State has an interest in the names borne by individuals and entities for purposes of identification, and that a change of name is a privilege and not a right, so that before a person can be authorized to*

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 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 individuals in the municipality using name i. The index is higher than 0.964 in all municipalities, indicating a high level of heterogeneity. The overall Herfindhal index for the municipalities on which we have data is greater than 0.999. In addition, the most common surname in our data, De La Cruz, is used by only .32 percent of individuals.[14]

The method described above generates a credible number of family ties. Importantly, as the data was collected to identify beneficiaries of a government program the names were recorded with great care. The database includes an entry for the middle name and an entry for last name and so we do not have to worry about how to split the full names. In addition, as data entry was done by staff specifically trained for that purpose, data entry errors are unlikely to be correlated with respondent characteristics.

change his name given him either in his certificate of birth or civil registry, he must show proper or reasonable cause, or any compelling reason which may justify such change. Otherwise, the request should be denied.

[14]This compares favorably with other countries. For example, in China, the most common surname is used by 7.25 percent population. In India it is 5.5 percent, in Taiwan it is 11 percent and in Vietnam it is 38 percent. For some of the countries we have enough data to compute the Herfinadal index as well and find that it is .971 (China), .953 (Taiwan) and .821 (Vietnam).

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.[15] Simple comparisons reveal stark differences between individuals related to office holders and the rest of the population. For example, 3.3 percent of individuals connected to successful candidates in the 2007 elections are employed in a managerial role, compared to 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

[15] Additional descriptive statistics are available in Table A.1.

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.[16] For each sample, we follow Imbens and Lemieux (2008) and estimate equation (1) non-parametrically. We use the optimal bandwidth recommended by Imbens and Kalyanaraman (2012).[17] 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).

Before running the regressions, we first plot local polynomial regressions of the probability of being employed in the best-paying occupation (manager) on their relatives' vote share in the 2007 elections (Figure 1). The graph indicates a clear jump in the probability of being employed in a managerial position around the cut-off. We also run some placebo tests and plot local polynomial regressions of the probability

[16]Using this cut-off to define close elections, we find that 17.1 percent of mayoral elections in our sample are close. Among the vice-mayoral elections, the proportion is 14.7 percent. Overall, there is at least one close election in 27.5 percent of the municipalities in our sample. Comparing municipalities with and without close elections we find 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).

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

of being employed in the best-paying occupation on their relatives' vote share in the 2010 elections; that is after the data were collected. There no evidence of discontinuity in the probability of being employed as a manager at the threshold (Figure 2).

We then estimate equation (1) for a number of outcome variables. 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 find 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 a 1.24 percentage point increase.

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 is driven by differences in the number of relatives candidates have.

The RD results discussed so far provides estimates of the effects of being connected to the winner among the set of individuals related to candidates. While inherently relevant, those estimates combine the effects of winning and the possible effects of losing the elections.[18] In the next section we implement an alternative strategy that allows us to parse out the two effects.

As pointed out by Medina and Stokes (2007), losing candidates' relatives might suffer from their connections. 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.[19]

[18]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.

[19]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 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).

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 nets out the potential cost of losing an election. We take a step-by-step approach, discussing how data constraints and unobserved heterogeneity combine to make the estimation of the value of political connections challenging. We also discuss how we test for heterogeneous effects. While the alternative estimation strategy presented below offers advantages, we nonetheless acknowledge that departing from a pure RD design raises the risk of spurious results, a risk that we minimize by resorting to a split sample approach.

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.[20]

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

[20]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.18) or along both month×year and province, using the two-way clustering method developed Cameron, Gelbach and Miller (2011) (Table A.19).

$E[\mu_{ij}C_{ijt}] > 0$ 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 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.[21] 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.[22]

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

[21]The maintained assumption here is that the pool of candidates is comparable across the two electoral cycles. We discuss evidence consistent with this assumption. First, relatives of candidates elected for the first time in 2007 have similar employment status than relatives of candidates elected for the second time in 2007. If trends in candidate quality explained our results we should observe differences between the two groups.

[22]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.

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, 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.

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.4.

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.5. 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.6).

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.7.

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.4); similar results are obtained with control groups II and III, the latter also controls for η.

Second, we argue that our results are consistent with the view that the effect is driven by preferential treatment of relatives as managers in the public sector. The point estimates at the top of the distribution correspond to 3.2 percent of the managerial jobs in the public sector in each municipality. 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.11).

Third, as point of 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.8). The RDD estimates are 60 percent larger than the regression point estimates on that subsample.

Fourth, we can compare the various point estimates discussed here with the RD point estimates to get a sense of the magnitude of η. Control groups I and III provide point estimates of similar order of magnitude. At first glance, this suggests that 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.

Fifth, we discuss some suggestive evidence consistent with the claim that there are cost associated with being related to a candidate who lost. 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. 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 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.[23] Data constraints prevent us from testing this directly.

In addition, 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.9). In municipalities where the incumbent lost, the RDD estimate obtained with the optimal bandwidth suggests that connections increase the likelihood of being

[23] 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.

employed as a manager increases by 2.45 percentage points (significant at the five percent level).[24] 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.

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.

Before turning to robustness checks, we explore the effects of connections to local politicians in office on two alternative measures of job quality. Second, 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.11). This corresponds to 1.84 percent of the mean in control group III.[25] Second, we get similar results if use median wage rather than mean wage in each occupation (Column 2 of Table A.11).

[24] 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.

[25] 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.

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.12.

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 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.13.

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 differences 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.[26] 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).[27] 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.

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 (2) with all possible interactions between three dummies capturing links to a mayor, a vice-mayor or a municipal councilor. We then compute the marginal effects for each dummy. Results are shown in Table

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

[27]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.14) and one term (Panel C of Table A.14).

A.15. 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.16, 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.17). 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.[28] In addition to a regression discontinuity design, we have also dealt with unobserved heterogeneity by using a variety of control groups, including individuals related to candidates elected in subsequent elections. Our findings are robust to alternative estimation techniques and to the inclusion of numerous control variables, and the effects of political connections on better paid occupation that we document are economically and statistically significant. 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 the idea directly with the data at hand, 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 if politicians provide jobs to their most educated relatives since they are the most capable of steering the bureaucracy. This result is hard to reconcile with the view that the effects are purely driven by elected officials' redistributive 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. In those cases, conditioning on close elections changes the nature of the parameter being

[28] 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.

estimated.

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).[29] We calculate 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. Using this difference as a measure of political distortion, we find that an increase in distortions is associated with an increase in the percentage of children 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 coefficient, and incumbent vote share in the previous election. In contrast, if we focus on education, a sector which has not been devolved to the municipal-level, we find no correlation between education 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 negatively affect the way services are delivered by the municipal bureaucracy.

[29]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.

References

Alesina, Alberto and Paola Giuliano, "Family Ties," in Philippine Aghion and Steven Durlauf, eds., *Handbook of Economic Growth*, North Holland, 2013.

Angelucci, Manuela, Giacomo De Giorgi, and Imran Rasul, "Resource Pooling Within Family Networks: Insurance and Investment," *University College London, mimeo*, 2012.

___ , ___ , **Marcos Rangel, and Imran Rasul**, "Family Networks and School Enrolment: Evidence from a Randomized Social Experiment," *Journal of Public Economics*, 2010, *94* (3-4), 197 – 221.

Besley, Timothy, "Political Selection," *The Journal of Economic Perspectives*, 2005, *19* (3), 43–60.

___ , **Rohini Pande, and Vijayendra Rao**, "Just Rewards? Local Politics and Public Resource Allocation in South India," *World Bank Economic Review*, 2012, *26* (2), 191–216.

___ , **Torsten Persson, and David Sturm**, "Political Competition, Policy and Growth: Theory and Evidence from the US," *Review of Economic Studies*, 2010, *77*, 1329–1352.

Blanes i Vidal, Jordi, Mirko Draca, and Christian Fons-Rosen, "Revolving Door Lobbyists," *American Economic Review*, 2012, *102* (7), 3731–48.

Caeyers, Bet and Stefan Dercon, "Political Connections and Social Networks in Targeted Transfer Programmes: Evidence from Rural Ethiopia," *Economic Development and Cultural Change*, 2012, *60* (4), 639–675.

Cameron, Colin, Jonah Gelbach, and Douglas Miller, "Bootstrap-based improvements for inference with clustered errors," *Review of Economics and Statistics*, 2008, *90* (3), 414–427.

___ , ___ , **and** ___ , "Robust Inference with Multiway Clustering," *Journal of Business and Economic Statistics*, 2011, *29* (2), 238–249.

Capuno, Joseph, "A Case Study of the Decentralization of Health and Education Services in the Philippines," *HDN Discussion Paper Series No. 3*, 2009.

Casey, Katherine, Rachel Glennerster, and Edward Miguel, "Reshaping Institutions: Evidence on Aid Impacts Using a Pre-Analysis Plan," *Quarterly Journal of Economics*, 2012, *127* (4), 1755–1812.

Cingano, Federico and Paolo Pinotti, "Politicians at Work: The Private Returns and Social Costs of Political Connections," *Journal of the European Economic Association*, 2013, *11* (2), 433–465.

Clark, Gregory, *The Son Also Rises: Surnames and the History of Social Mobility*, New Jersey, USA: Princeton University Press, 2014.

Comola, Margherita and Marcel Fafchamps, "Testing Unilateral and Bilateral Link Formation," *Economic Journal*, forthcoming.

Coronel, Sheila, "Cavite - The Killing Fields of Commerce," in "Boss - Five Case Studes of Local Politics in the Philippines," Philippine Center for Investigative Journalism, 1995.

Cruz, Cesi and Christina Schneider, "The (Unintended) Electoral Effects of Multilateral Aid Projects," *University of California - San Diego, mimeo*, 2013.

Cullinane, Michael, "Patron as Client: Warlord Politics and the Duranos of Danao," in Alfred McCoy, ed., *An Anarchy of Families: State & Family in the Philippines*, Madison, WI: University of Wisconsin Press, 2009, pp. 163–241.

Deaton, Angus, "Your Wolf is Interfering with my T-value!," *Royal Economic Society Newsletter*, 2012, *159* (4).

Dios, Emmanuel De, "Local Politics and Local Economy," in Arsenio M. Balisacan and Hall Hill, eds., *The Dynamics of Regional Development*, Quezon City: Ateneo de Manila University Press, 2007.

Eggers, Andrew and Jens Hainmueller, "MPs for Sale? Returns to Office in Postwar British Politics," *American Political Science Review*, 2009, *103* (4), 513–533.

Einav, Liran and Jonathan Levin, "The Data Revolution and Economic Analysis," *NBER Working Paper 19035*, 2013.

Faccio, Mara, "Politically Connected Firms," *American Economic Review*, 2006, *96* (1), 369–386.

Fernandez, Luisa, "Design and Implementation Features of the National Household Targeting System in the Philippines," *World Bank - Philippines Social Protection Note No 5*, 2012.

Ferraz, Claudio and Frederico Finan, "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments," *American Economic Review*, 2011, *101* (4), 1274–1311.

Fisman, Ray, Florian Schulz, and Vikrant Vig, "The Private Returns to Public Office," *Journal of Political Economy*, forthcoming.

Fisman, Raymond, "Estimating the Value of Political Connections," *American Economic Review*, 2001, *91* (4), 1095–1102.

Fukuyama, Francis, *The Origins of political order: from prehuman times to the French revolution*, New York, NY: Farrar, Straus and Giroux, 2011.

Gagliarducci, Stefano and Marco Manacorda, "Politics in the Family: Nepotism and the Hiring Decisions of Italian Firms," *mimeo*, 2014.

Gealogo, Francis Alvarez, "Looking for Claveria's Children: Church, State, Power, and the Individual in Philippine Naming Systems during the Late Nineteenth Century," in Zheng Yangwen and Charles J-H MacDonald, eds., *Personal Names in Asia. History, Culture and History*, Singapore: NUS Press, 2010, pp. 37–51.

Glaeser, E., "Researcher Incentives and Empirical Methods," *Harvard Institute of Economic Research, Discussion Paper Number 2122*, 2006.

Guell, Maia, Jos V. Rodriguez Mora, and Christopher I. Telmer, "The Informational Content of Surnames, the Evolution of Intergenerational Mobility and Assortative Mating," *The Review of Economic Studies*, 2014.

Hale, Henry, "Correlates of Clientelism: Political Economy, Politicized Ethnicity, and Post-Communist Transition," in Herbert Kitschelt and Steven Wilkinson, eds., *Patrons, Clients, and Policies. Patterns of Democratic Accountability and Political Competition*, Cambridge University Press, 2007.

Hodder, Rupert, "Political Interference in the Philippine Civil Service," *Environment and Planning C: Government and Policy*, 2009, *27* (5), 766–782.

Hollnsteiner, Mary, *The Dynamics of Power in a Philippine Municipality*, University of the Philippines, 1963.

Hsieh, Chang-Tai, Edward Miguel, Daniel Ortega, and Francisco Rodriguez, "The Price of Political Opposition: Evidence from Venezuela's Maisanta," *American Economic Journal: Applied Economics*, 2011, *3* (2), 196–214.

Human Rights Watch, *They Own the People. The Ampatuans, State-Backed Militias, and Killings in the Southern Philippines*, New York, NY: Human Rights Watch, 2010.

Humphreys, Macartan, Raul Sanchez de la Sierra, and Peter van der Windt, "Fishing, Commitment, and Communication: A Proposal for Comprehensive Nonbinding Research Registration," *Political Analysis*, 2013, *21* (1), 1–20.

Imbens, Guido and Karthik Kalyanaraman, "Optimal Bandwidth Choice for the Regression Discontinuity Design," *Review of Economic Studies*, 2012, *79* (3), 933–959.

Imbens, Guido W. and Thomas Lemieux, "Regression Discontinuity Designs: A Guide to Practice," *Journal of Econometrics*, 2008, *142* (2), 615–635.

Iyer, Lakshmi and Anandi Mani, "Traveling Agents: Political Change and Bureaucratic Turnover in India," *Review of Economics and Statistics*, 2012, *94* (3), 723–739.

Khemani, Stuti, "Buying Votes vs. Supplying Public Services: Political Incentives to Under-Invest in Pro-Poor Policies," *World Bank Policy Research Working Paper 6339*, 2013.

Khwaja, Asim Ijaz and Atif Mian, "Do Lenders Favor Politically Connected Firms? Rent Provision in an Emerging Financial Market," *Quarterly Journal of Economics*, 2005, *120* (4), 1371–1411.

Labonne, Julien, "Local Political Business Cycles. Evidence from Philippine Municipalities," *University of Oxford, mimeo*, 2013.

Lande, Carl, *Leaders, Factions and Parties: The Structure of Philippines Politics*, Yale University Southeast Asia Studies, New Haven CO, 1965.

Leamer, Edward, *Specification Searches. Ad Hoc Inference with Nonexperimental Data*, New York, NY: Wiley, 1978.

___ , "Let's Take the Con out of Econometrics," *American Economic Review*, 1983, *73* (1), 31–43.

Markussen, Thomas and Finn Tarp, "Political Connections and Land-Related Investment in Rural Vietnam," *Journal of Development Economics*, forthcoming.

McCoy, Alfred, "An Anarchy of Families: The Historiography of State and Family in the Philippines," in Alfred McCoy, ed., *An Anarchy of Families: State & Family in the Philippines*, Madison, WI: University of Wisconsin Press, 2009, pp. 1–32.

McCrary, Justin, "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 2008, *142*.

Medina, Luis Fernando and Susan Stokes, "Monopoly and Monitoring: An Approach to Political Clientelism," in Herbert Kitschelt and Steven Wilkinson, eds., *Patrons, Clients, and Policies. Patterns of Democratic Accountability and Political Competition*, Cambridge University Press, 2007.

Nichols, Austin, "rd 2.0: Revised Stata module or regression discontinuity design," 2011.

Querubin, Pablo, "Political Reform and Elite Persistence: Term Limits and Political Dynasties in the Philippines," *mimeo, MIT*, 2011.

___ , "Family and Politics: Dynastic Incumbency Advantage in the Philippines," *mimeo, NYU*, 2013.

___ **and James Snyder**, "The Control of Politicians in Normal Times and Times of Crisis: Wealth Accumulation by U.S. Congressmen, 1850-1880," *Quarterly Journal of Political Science*, 2013, *8*, 409–450.

Scott, James, *Seeing like a State: How Certain Schemes to Improve the Human Condition Have Failed*, New Haven, Conn.: Yale University Press, 1998.

Sidel, John, *Capital, Coercion, and Crime: Bossism in the Philippines* Contemporary Issues in Asia and Pacific, Stanford, CA: Stanford University Press, 1999.

The Economist, *A bridge too far?* January 2014.

Tremewan, Christopher, *The Political Economy of Social Control in Singapore*, New York, USA: St Martin's, 1994.

Wallis, J.J., "The Concept of Systematic Corruption in American History," in Edward Glaeser and Claudia Goldin, eds., *Corruption and Reform: Lessons from America's Economic History*, University of Chicago Press, 2006.

___ **, Price Fishback, and Shawn Kantor**, "Politics, Relief, and Reform. Roosevelt's Efforts to Control Corruption and Political Manipulation during the New Deal," in Edward Glaeser and Claudia Goldin, eds., *Corruption and Reform: Lessons from America's Economic History*, University of Chicago Press, 2006.

Wang, Shing-Yi, "Marriage Networks, Nepotism and Labor Market Outcomes in China," *American Economic Journal: Applied Economics*, 2013, *5* (3), 91–112.

Wilkinson, Steven, "Explaining Changing Patterns of Party-voter Linkages in India," in Herbert Kitschelt and Steven Wilkinson, eds., *Patrons, Clients, and Policies. Patterns of Democratic Accountability and Political Competition*, Cambridge University Press, 2007.

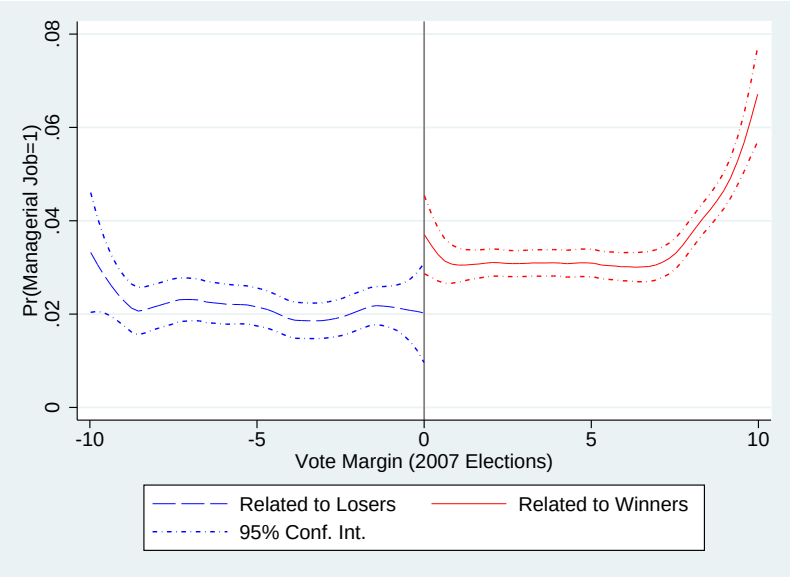

Figure 1: Non-parametric estimates of the probability of being employed in a managerial position

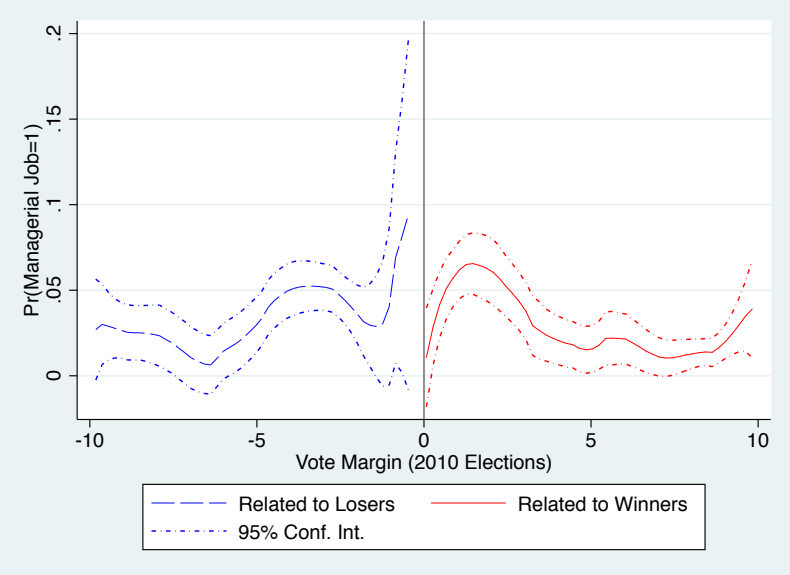

Figure 2: Non-parametric estimates of the probability of being employed in a managerial position

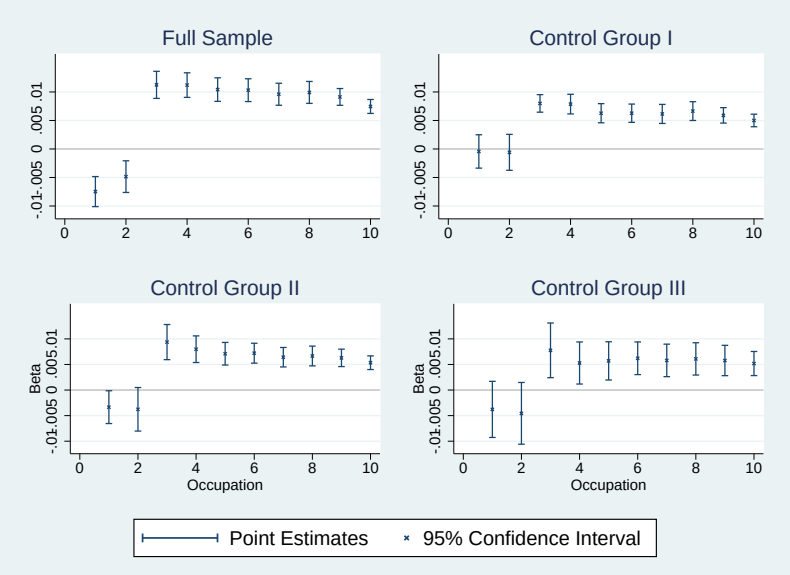

Figure 3: Estimated effects of connections with various control groups

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.4-A.7.

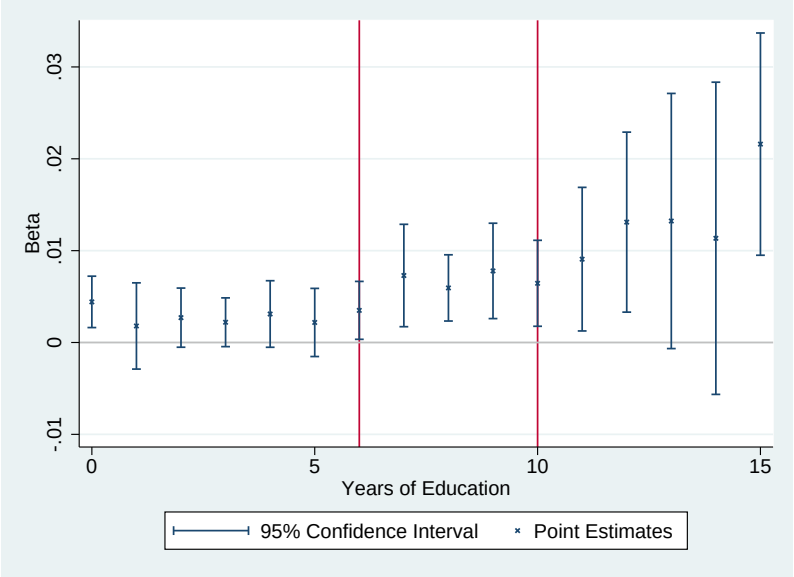

Figure 4: Estimated effects of connections by education levels

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.

For Online Publication:

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.20). 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.20).

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.21, 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.21). 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.21. 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.22 and A.23).

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.24 and A.25). Similarly, results are robust to excluding municipalities at the top 5, 10 and 25 percent in the distribution of population (Table A.26). 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.27).

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.28. 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.

Eighth, given the size of the estimated effects, one might be worried that they are driven by a few outliers. To reduce those concerns, for each outcome of interest we compute the mean of the variable for individuals connected to elected officials in each municipality. We then exclude either the top 1%, 5% or 10%of municipalities in the relevant distributions and estimate our main equations on those subsamples. The results of this conservative exercise are qualitatively similar (Table A.29).

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 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.5: 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.6: 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.7: 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.8: 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.9: 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.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 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.12: 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.13: 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.14: 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.15: 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.16: 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.17: 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.18: 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.19: 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.20: 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.21: 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.22: 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.23: 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.24: 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.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 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.26: 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.27: 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.28: 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.

Table A.29: Robustness checks: Exclude outlying municipalities in the occupation distribution (for connected individuals)

	(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 1%										
Connected Office (2007)	-0.0014	-0.0028	0.0080***	0.0055***	0.0056***	0.0060***	0.0055***	0.0058***	0.0059***	0.0052***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	452,110	450,850	452,456	451,072	451,627	451,401	451,773	451,541	453,155	453,627
R-squared	0.331	0.269	0.231	0.244	0.267	0.295	0.276	0.263	0.263	0.086
Panel A: Exclude top 5%										
Connected Office (2007)	-0.0017	-0.0031	0.0075***	0.0056***	0.0054***	0.0057***	0.0056***	0.0059***	0.0048***	0.0043***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	440,173	438,627	437,853	441,422	441,772	439,682	439,690	439,661	442,947	443,050
R-squared	0.333	0.267	0.228	0.241	0.264	0.293	0.273	0.260	0.259	0.079
Panel A: Exclude top 10%										
Connected Office (2007)	-0.0023	-0.0021	0.0074***	0.0051***	0.0055***	0.0051***	0.0047***	0.0053***	0.0040***	0.0038***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	421,847	423,068	417,948	421,328	420,794	421,547	424,469	424,264	428,928	429,036
R-squared	0.337	0.264	0.225	0.239	0.261	0.289	0.271	0.256	0.257	0.075

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.

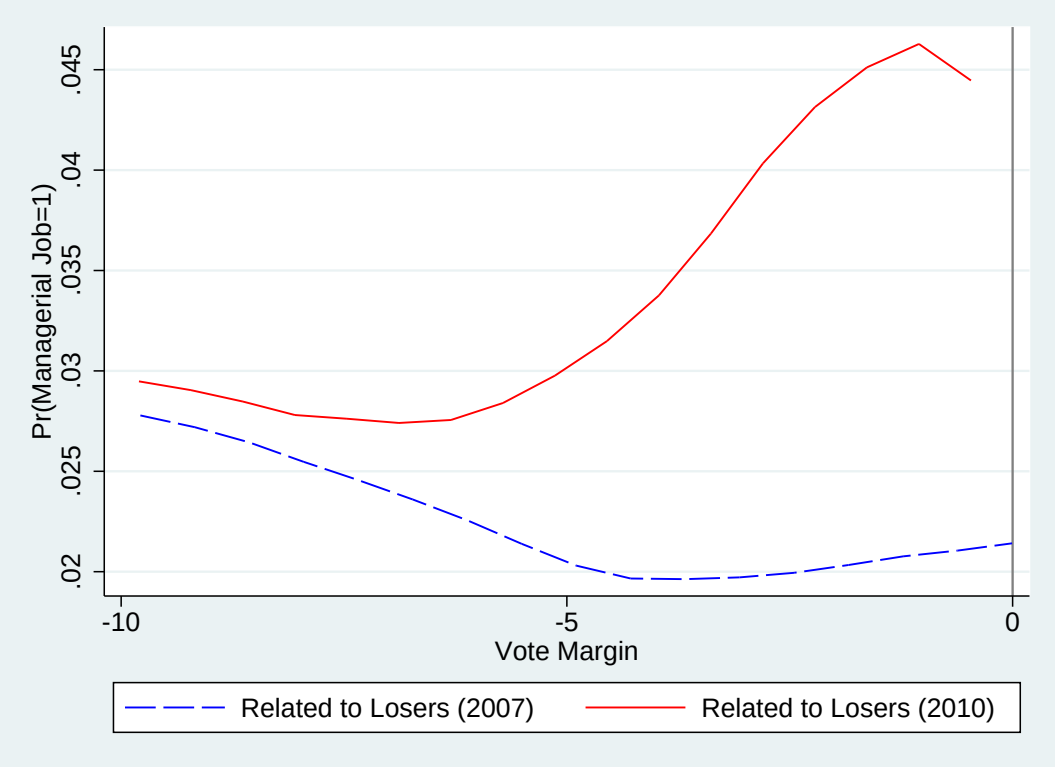

Figure A.1: Non-parametric estimates of the probability of being employed in a managerial position

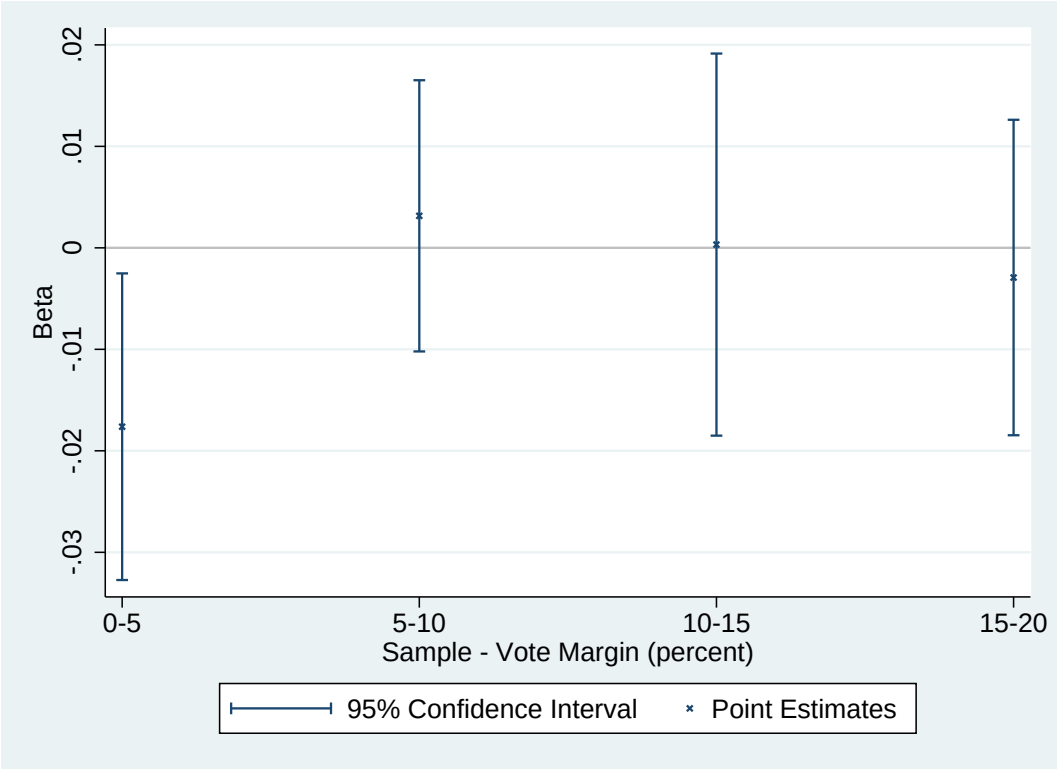

Figure A.2: Estimated Effects by Loss Margin