Do Politicians' Relatives Get Better Jobs? Evidence from Municipal Elections in the Philippines*

Marcel Fafchamps† Julien Labonne‡

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Abstract

In this paper, we exploit naming conventions and a unique dataset to estimate the positive and negative impacts of being connected to local politicians on occupational choice. We use a large administrative dataset collected between 2008 and 2010 on 20 million individuals in 709 Philippine municipalities along with information on all 38,448 local candidates in the 2007 and 2010 elections in those municipalities. Unusually, the data include family names of all individuals surveyed and we rely on local naming conventions to assess blood and marriage links between households. We first estimate the value of political connections by applying a regression discontinuity design (RDD) to close elections in 2007 (*i.e.,* before the data were collected) but argue that those estimates likely combine both the benefits from connections to current office-holders and the cost associated with being related to a candidate that lost. To deal with this, we use individuals connected to successful candidates in the 2010 elections (*i.e.,* after the data were collected) as control group and find that connections to current office-holders increase the likelihood of being employed in better paying occupations. 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. This result is robust to the use of alternative control groups, specifications and estimation techniques. Comparing the RDD estimates with the ones obtained with our preferred control group suggests that relatives of candidates that were close to being elected in 2007 suffer cost from the connections and are less likely to be employed in better paying occupations.

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†Oxford University; email: marcel.fafchamps@economics.ox.ac.uk

‡Oxford University; email: julien.labonne@economics.ox.ac.uk

1 Introduction

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

The literature on the value of political connections for individuals has faced several difficulties and is not well-developed.[1] First, for lack of better data, researchers often rely on self-reported links to local politicians as a measure of political connections. Such data are subject to bias because the likelihood of reporting connections might be correlated with the benefits that are derived from them (Comola and Fafchamps forthcoming). Second, individuals connected to politicians may differ from the average citizen along unobservable characteristics that affect their welfare even if 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 has not accounted for the possibility that individuals connected to politicians who lost an election can suffer from their connections, especially in areas where elected officials have discretionary powers.[2]

[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). 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). This is related to the literature on the role of family links in labor markets. For example, Wang (2013) documents a significant reduction in earnings when a man's father-in-law dies.

[2]Hsieh, Miguel, Ortega, and Rodriguez (2011) offer a related perspective. They find that, in Venezuela, individuals who signed a petition against Chavez earn less and are less likely to be employed.

Using a large dataset from the Philippines, collected between the 2007 and 2010 elections, we test whether individuals who are connected to politicians in municipal elections are employed in better-paying occupations.[3] We contribute to the literature on the value of political connections in three ways. First, we distinguish between individuals connected to successful and unsuccessful candidates in different municipal elections. This allows us to estimate both the value of being connected to an elected local official and, for the first time, the cost of being connected to a losing candidate, both of which are present in our data. Second, we test the robustness of our findings to the use of different control groups. We find that this matters. Third, we rely on Filipino naming conventions introduced by Spanish colonial authorities to infer family ties to local politicians (see Angelucci, De Giorgi, Rangel, and Rasul (2010) for a similar approach). This bypasses the need to rely on self-reported links. Because Spanish family names were introduced in the Philippines recently (i.e, in the middle of the 19th century) and because local naming conventions are informative, they allow an unusually precise and objective identification of family ties.

To address concerns about specification search and publication bias (Leamer 1974, Leamer 1978, Leamer 1983, Lovell 1983, Glaeser 2006), we propose and implement a split sample approach. We asked a third party to split the data into two randomly generated, non-overlapping subsets, A and B, and to hand over sample A to us. This version of the paper uses sample A to narrow down the list of hypotheses we wish to test and to refine our methodology. Once the review process is completed, we will apply to sample B, to which we do not have access yet, the methodology that has been approved by the referees and editor, and this is what will be published.[4] We believe that our approach can improve the reliability of empirical work and could be adopted widely in a world of 'big data' (Einav and Levin 2013).

Our results can be summarized as follows. Individuals who share one or more family names with local elected officials are more likely to be employed in better paying occupations. This effect persists when we control for individual characteristics and when we compare relatives of politicians elected in 2007 and in 2010. The effect is particularly noticeable at the top of the occupational distribution: the probability of being employed in a managerial position

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

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

increases by 0.54 percentage-points, or more than 22 percent of the control group mean, for individuals related to current office holders compared to relatives of politicians elected after the occupational data were collected. This result is robust to the use of different control groups, alternative specifications, and estimation techniques.

We are also able to test for the impact of being related to local politicians who failed to be elected. While there is some existing anecdotal evidence that such individuals might suffer from their connections, we are the first to quantify the effects. Comparing regression discontinuity design estimates with results from our preferred control group, we find that relatives of candidates close to being elected as mayors or vice-mayors are less likely to be employed in better paying occupations than relatives of politicians that did not perform as well in the elections. We interpret this as suggesting that incumbents punish their serious opponents.

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

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' altruistic or redistributive motives towards their relatives.[5] Second, the fact that individuals connected to candidates who were almost elected are less likely to be employed in managerial positions suggest that family connections facilitate supervision and monitoring rather than screening. To illustrate, Cullinane (2009, p 190) reports that when asked about the employment of his relatives in the local government, Ramon Durano, a Cebuano politician, told a reporter that *'politics is not something you can entrust to non-*

[5] At least, this suggests that, based on observables, incompetent relatives are not the ones deriving the greatest benefits from their connections, thus reducing the potential efficiency costs.

relatives'. Sidel (1999) argue that municipal mayors use their control over tax collection and regulatory enforcement not only to enrich themselves but also to gain electoral rewards. In this case, we would expect politicians to value loyalty, before and after the elections. This interpretation is in line with a recent finding that politicians in India value both loyalty and expertise when assigning bureaucrats (Iyer and Mani 2012).

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

The paper is organized as follows. We describe the setting in Section 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 Local politics in the Philippines

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

In 1991 the Local Government Code (Republic Act 7160) devolved significant decision-making power and fiscal resources to mayors, vice-mayors and municipal councilors (Brillantes 1992). Local elections are organized, by law, at fixed intervals of three years. This rules out any possible endogeneity between the timing of local elections and the support politicians have in their constituency.

Diokno (2012) argues that, after two decades, the benefits of decentralization are far from clear. He blames the situation on poor governance practices at the local level rather than on inadequate fiscal arrangements. As some had originally feared,[6] available evidence indicates that municipal mayors tend to use their new resources and discretion to prolong their time in office (Capuno 2012). The primary drivers of resource allocations tend to be political considerations. For example, when the Department of Social Welfare and Development started implementing a large-scale conditional cash transfer program in 2008, it was deemed necessary to establish a centralized targeting system rather than to rely on local officials to identify beneficiaries.

There is evidence that local Filipino politicians act as employment brokers in both the public and private sectors (Sidel 1999). In the public sector, Hodder (2009) argues that they are able to use their hiring powers over a large number of staff that were transferred from national agencies to municipalities as part of the decentralization process.[7] 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.'*[8]

In the private sector, Sidel (1999) shows that local politicians can affect employment either directly, through their business holdings or, in a number of provinces, indirectly through their contacts with local businessmen. In addition, it is possible that local businessmen favor the relatives of local officials in their hiring decisions, in the hope of securing more favorable regulatory supervision. In the Philippines, a number of permits required to operate a business are delivered by the municipal bureaucracy (Llanto 2012).

There is some evidence that loyalty to local politicians is valued. Bureaucrats are often expected to engage in behavior favoring incumbents prior to the elections (Coronel 1995, Cullinane 2009). Cullinane (2009) reports that local politicians often staff the bureaucracy with loyal individuals they can trust to act in their best interest. In a case study of local politics in Cavite, a province outside of Metro Manila, Coronel (1995) points out *'public officials in the bureaucracy*

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

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

[8] 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.

- the Comelec [Commission on Elections], teachers and the police - have not been neutral or objective. Since 1945, this machinery has been used, and it is embedded in the political structure.' It follows that relatives of known political challengers may suffer from their connections if incumbents are reluctant to staff the bureaucracy with individuals whose views and interests are antinomic to theirs. There is indeed qualitative evidence that Filipino politicians have the ability to punish individuals connected to their opponents.[9]

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

The NHTS data include information on the occupation of all individuals surveyed. The classification, developed by the National Statistics Office for its regular Labor Force Surveys (LFSs), include 11 occupations.[10] We rank them according to their average daily wage, computed

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

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

using wage data from eight nationally representative LFSs collected in 2008 and 2009.[11]

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 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 ask a third party to randomly split our data in two halves. The first half (*training set*) is used to narrow down the list of hypotheses we want to test. Once the list is finalized, they will be applied to the second half (*testing set*) to which we do not have access yet. These are the results that will be reported in the published version of this paper. To the best of our knowledge, this is the first time that this approach is used in economics and political science apart from forecasting purposes. The purpose is to provide credible estimates free of specification search and publication bias and to deliver adequately sized statistical tests.[12] This approach is best suited to studies that can take advantage of large samples which appears especially relevant given the ease with which large scale datasets are becoming available (Einav and Levin 2013).[13] 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).[14]

[11]The ranked categories are: 0-None; 1-Laborers and Unskilled Workers; 2-Farmers, Forestry Workers and Fishermen; 3-Service Workers and Shop and Market Sales Workers; 4-Trades and Related workers; 5-Plant and Machine Operators and Assemblers; 6-Clerks; 7-Technicians and Associate Professionals; 8-Special Occupations; 9-Professionals; 10-Officials, Managers, Supervisors. We only use data from the municipalities with full enumeration in the NHTS-PR.

[12]It is important to note that even with the unusually large sample to which we access, if in the regression $Y = \beta X + u$, the null hypothesis $H_0 : \beta = 0$ is true and H_0 is rejected in the training set, then the probability that it will be rejected at the 95% level in the testing test is 5%. As expected, this is confirmed through simulations.

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

[14]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,

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

3.2 Family ties

We take advantage of naming conventions in the Philippines to assess blood and marriage links between surveyed individuals and local politicians.[15] 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.[16]

Sanchez de la Sierra, and van der Windt 2013). The main advantage of our approach is that, since we are using real data, we are able to incorporate results from exploratory analyses in our analysis plans.

[15]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.

[16]Importantly, Article 376 of the Civil Code of the Philippines (Republic Act No. 386, 1949) states that *No person can change his name or surname without judicial authority.* This has been upheld in a number of court cases which have sometimes reached the Supreme Court. For example, in the majority decision in the case *Wang v. Cebu City Civil Registrar (G.R. No. 159966, 30 March 2005, 454 SCRA 155.)*, Justice Tinga wrote: *The Court has had occasion to express the view that the State has an interest in the names borne by individuals and entities for purposes of identification, and that a change of name is a privilege and not a right, so that before a*

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).[17] In other contexts, Angelucci, De Giorgi, Rangel, and Rasul (2010) used a similar strategy to measure family networks in Mexico and Allesina (2011) used shared last names to measure nepotism in Italian academia.

In our sample, sharing a last or a middle name is a good indicator of family ties. This could be challenged if names were too common. For example, if individuals from the same ethnic group all shared the same last name, results would capture ethnic ties rather than family connections. In our sample municipalities, there are an average of 5,998 names used (median 5,126). There is also a great diversity of names. We compute a Herfindhal index of name heterogeneity, computed as $1 - \sum s_i^2$ where s_i is the share of households in the municipality using name i. The index is higher than 96.4 percent in all municipalities, indicating a high level of heterogeneity.

The method described above generates a credible number of family ties. The average political candidate is found to be connected to 70 individuals aged 20-80 in his/her municipality.[18] While this may appear large at first, it is consistent with the way middle and last names are transmitted across generations. To illustrate, take the conservative estimate that the parents of each candidate had 3 children who in turn had two children each. With these assumptions, each candidate would be directly connected to 13 individuals. If in addition, each of his/her parent has two siblings, with three children each with two children of their own, a candidate would be indirectly connected to 56 individuals; for a total of 69 individuals.

There are two sources of measurement error in our measure of family ties. First, it is possible that non-related households share the same last name. As explained earlier, this potential source

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person can be authorized to change his name given him either in his certificate of birth or civil registry, he must show proper or reasonable cause, or any compelling reason which may justify such change. Otherwise, the request should be denied. This reduces concerns about strategic name changes.

[17] We have identified, and are in the process of securing access to, survey data that will allow us to provide further evidence that the proposed name matching method adequately captures family connections. The survey was carried out after the 2010 municipal elections with a sample size of 864 households in 24 villages in the province of Isabela in the Philippines. Survey respondents were asked to provide information on their names and to answer direct questions on family connections to various elected officials. We intend to compare the measures of links generated through the name matching method used in this paper and through the direct question. Once available, results will be reported in an Annex.

[18] Those statistics were computed using the full sample.

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 and in Figure 1. Simple comparisons reveal stark differences between individuals related to office holders and the rest of the population. For example, 3.2 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.

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, Schulz, and Vig 2013, Querubin and Snyder forthcoming) relies on the assumption that (relatives of) politicians who were narrowly defeated are most comparable to (relatives of) narrowly elected politicians. We use data on the breakdown of votes for the top two candidates in the 2007 mayoral and vice-mayoral elections.

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

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

where f is an unknown smooth function, V_{ij} is the vote margin of victory or defeat for successful or unsuccessful candidates, respectively, and ϵ_{ij} is an idiosyncratic error term. Equation (1) is estimated on three different samples defined as neighborhoods of the cutoff point, *i.e.,* using relatives of candidates with a 2007 vote margin of +/- 5 percent. 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).[19]

[19] This is implemented in Stata using the `rd` command developed by Nichols (2011).

Our objective is to assess the impact of family ties to local politicians on the probability of being employed in a better paying occupation. To this effect, we create a series of 10 dummy variables Y_{ij}^{p} equal to one if individual i in municipality j is employed in at least occupation p. We estimate equation (1) for all Y^{p}.

Results are consistent with strong positive impacts of political connections on the probability of being employed in better-paid occupations (Table 3). 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 7.36 percentage-points. Similarly, individuals connected to current office-holders appear to experience a 1.32 percentage-points increase in the probability of being employed in a managerial role. At the bottom of the distribution, family ties do no appear to affect the likelihood of being employed. The point estimates decrease as the bandwidth increases. For example with the bandwidth set at half the optimal bandwidth, connections appear to lead to a 1.67 percentage point increase in the probability of being employed in a managerial position. With twice the optimal bandwidth, the point estimates correspond to 1.19 percentage point increase. We get similar results when using relatives of candidates with a 2007 vote margin of either +/- 2.5 percent or +/- 10 percent (Tables A.1 and A.2).

The RDD results have potential weaknesses that we try to address below, however. First, the incentives of politicians who were narrowly elected might differ from those who were elected with wider margins (Vyborny and Haseeb 2013). Second, the estimates likely combine both the benefits from connections to current office-holders and the cost associated with being related to a candidate that lost.[20] Indeed, it is possible that that individuals related to candidates who were narrowly defeated tend to be penalized, perhaps because they are eliminated from local positions of power by the narrowly elected candidate. This could explain why the point estimates discussed above get smaller as the bandwidth increases. 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.[21] Data constraints prevent us from testing this directly.

[20]There is also some debates in the literature as to whether close elections are indeed random (Caughey and Sekhon 2011, Snyder, Folke, and Hirano 2011, Eggers, Folke, Fowler, Hainmueller, Hall, and Snyder 2013).

[21]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.

5 Alternative estimation strategy

In this section we propose an alternative estimation strategy that seeks to address the possible weaknesses of the regression discontinuity approach applied to our data. Our aim is to obtain credible estimates of the causal effect of political connections on occupation in a way that distinguishes between the cost of losing an election and the benefit of winning one. We take a step-by-step approach, discussing how data constraints and unobserved heterogeneity combine to make the estimation of the value of political connections challenging. We also discuss how we test for heterogeneous effects.

5.1 The benefits of political connections

In order to deal with unobserved heterogeneity, researchers attempting to provide credible estimates of the value of political connections need to identify a valid control group. We present several approaches to identify a control group and discuss their relative advantages and drawbacks. Our objective is to measure the benefits of political connections in a way that nets out the possible cost of being connected to someone who just lost an election.

In most contexts, since data on connections to unsuccessful candidates tend not to be available, researchers are only able to compare politically connected individuals to individuals randomly drawn from the population. To allow comparison with this literature, we start by estimating the value of political connections by regressing the outcome variable on a dummy capturing links to elected local officials, plus individual controls. Specifically we estimate a linear probability model of the form:

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

where Y_{ijt} is a measure of occupational choice for individual i in municipality j at the time of the survey t, α is the parameter of interest, C_{ijt} is a dummy variable that equals one if individual i is related to an elected official in office in municipality j at time t, X_{ijt} is a vector of observable individual characteristics, v_{jt} is an unobservable affecting all individuals in municipality j at time t and u_{ijt} is an idiosyncratic error term. Occupational choice might be correlated within municipalities and provinces. Given that the municipalities are nested within provinces, we cluster standard errors at the provincial level.[22]

[22]The sample includes data from more than 60 provinces so we are not concerned about bias in our standard

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 effect on Y_{ijt} of being related to someone who ran at least once in a local election. There are good reasons to expect $E[\mu_{ij}|C_{ijt} = 1] > E[\mu_{ij}|C_{ijt} = 0]$. For instance, a higher social standing makes it more likely that an individual is related to the local political elite, but also that he or she has a better occupation. Similarly, let η_{ij} be the additional effect on Y_{ijt} of being connected to a candidate who has won a local election at least once. We expect $E[\eta_{ij}|C_{ijt} = 1] > E[\eta_{ij}|C_{ijt} = 0]$: on average, individuals with characteristics that make them more likely to be related to a successful politician also have, other things being equal, a social standing correlated with a better occupation. To the extent that $E[\mu_{ij}C_{ijt}] > 0$ and $E[\eta_{ij}C_{ijt}] > 0$, we expect an upward bias in estimates of α that are obtained by estimating equation (2) on the entire population. If we can control for μ_{ij} and η_{ij}, α then captures the effect of being related to an elected official currently in office, net of any correlation between 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

errors as a result of having too few clusters (Cameron, Gelbach, and Miller 2008).

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. But because it is the most restrictive, it also results in the smallest number of control observations – and thus to a possible loss of power. To the best of our knowledge, this is the first time this estimation strategy is being used to estimate the value of political connections for individuals.[23]

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

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

Control groups II and, especially, III are a marked improvement upon what the literature has been able to use until now. We are able to use these control groups for several reasons. First, we infer links from information about names, not from self-reported data. Control groups II and III could not be constructed from self-reported measures of political connections: how could respondents be asked about their connections to yet-to-be-revealed candidates? Second, using names to infer family connections could be problematic in many countries but, for reasons discussed in Section 4.2, 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

[23] 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.

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.

5.2 Heterogeneity

We investigate whether the value of political connections varies with the rank of the local politician to whom the individual is related. To this effect, we estimate equation (2) replacing C_{ij} with all possible interactions of three dummy variables capturing family ties to the mayor, the vice-mayor, and municipal councilors and the associated marginal effects. We also test whether the impact of political connections varies across individuals in a systematical way. More specifically, we test for heterogenous effects along three characteristics Z_{ij}^p – gender, education and age – estimating equations of the form:

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

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

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

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

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

6 Econometric results

6.1 Main results

We begin by reporting naive OLS estimates using the full sample. Results indicate that individuals connected to politicians in office are more likely to be employed in better paying occupations

(Table 4). For example, a randomly selected individual related to an elected local official is 1.45 percentage points more likely to be employed in a managerial role than the average citizen. This represents an increase of about 70 percent of the mean. As a a point of comparison, we plot the point estimates along with the cumulative distribution of occupations for the full sample and for individuals connected to successful candidates in the 2007 elections (Figure 3). The point estimates for each occupation level correspond to the difference between the two cumulative distributions.

As shown in Panels A and B of Table 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 the conceptual section, we now compare these results to those obtained using different control groups I, II and III. When we use control group I – *i.e.,* the relatives of unsuccessful 2007 candidates – 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 4. But point estimates are lower than the ones obtained on the full sample, a finding consistent with the argument that bias μ is positive: depending on the outcome of interest, point estimates fall by 29 to 40 percent (Panels B of Table 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. [24]

Next we use control group II, in which the relatives of 2010 candidates that did not run in 2007 are compared to those of successful 2007 candidates (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 4 and in Panel B of Table 5, continue to associate family ties to elected officials with better paid occupations.[25] As a point of comparison, we also estimate the regressions on the sample of individuals connected to unsuccessful candidates in 2010 that did not run in 2007, and find similar results (Table A.6).

[24] Additional results are available in Table A.3.

[25] Additional results are available in Table A.4.

Finally, we further restrict the control group only to those individuals connected to *successful* candidates in the 2010 elections but who did not run in 2007 to net out both μ_{ij} and η_{ij}. This is control group III. Results, presented in the bottom right corner of Figure 4 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.[26]

6.2 Discussion and interpretation

What do we learn from comparing the estimates we obtained using different control groups? First, as anticipated, an upward bias seems to arise when we estimate the impacts of political 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.3); similar results are obtained with control groups II and III, the latter also controls for η.

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

Third, control groups I and III provide point estimates of similar order of magnitude. At first glance, this suggests that η is close to zero and that the relatives of unsuccessful 2007 candidates do not suffer from their ties to an unlucky challenger. 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

[26] Additional results are available in Table A.5.

with any of the three control groups and why the RDD estimates increase as the bandwidth used decreases.

As point of further comparison with the RDD estimates provided above, we estimate equation (2) on the sample of individuals related to candidates for either mayor or vice-mayor in the 2007 elections (Table A.7). The RDD estimates are 1.7 times larger than the regression point estimates on that subsample.

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

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

In addition, we now estimate equation (2) for each Y^p ($p = 2, \ldots, 10$) restricting the sample to individuals for which $Y^{p-1} = 1$. Given that we can consider occupation choice as a sequential decision, this is equivalent to estimating the conditional impacts of connections. This gives us additional information about where in the distribution of occupations connections have an impact.[27] Results, available in Table A.9, suggest that, even the conditional estimates are consistent with a positive impact of family connections on occupational choice. It is important to note that those estimates need to be interpreted with caution as, for each value of p, the probability of being included in the sample is correlated with the level of connections.

[27] A sample example with two sequential decisions will highlight the differences between the conditional and unconditional estimates. Let's assume that connections affect the probability of going through the first step, but conditional of having gone through the first step connections. Thats is, assuming $P(Y^1 = 1|C = 1) > P(Y^1 = 1|C = 0)$ and $P(Y^2 = 1|Y^1 = 1, C = 0) = P(Y^2 = 1|Y^1 = 1, C = 0)$, will lead to $P(Y^2 = 1|C = 1) > P(Y^2 = 1|C = 0)$.

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.

First, strict term limits were introduced in 1987, but political families in some municipalities circumvent them by having different members of the same family take turns in office (Querubin 2011). In these municipalities, relatives of candidates elected in 2010 might not be valid counterfactuals for current office holders.[28] To check whether this affected our results, we re-estimate equation (2) focusing on municipalities where the mayor's family has been in office for three terms or fewer (Panel A of Table A.10), two terms or fewer (Panel B of Table A.10) and one term (Panel C of Table A.10). 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 the subsample of municipalities where the mayor is in his first term, relatives of the current office holder are 0.31 percentage-points more likely to be employed in a managerial role, an increase that corresponds to 13 percent of the baseline probability in these municipalities. It is important to note that those estimates are likely to be downward biased as the benefits of connections might not materialize instantaneously and most of the data were collected between six months and two years after the 2007 elections.

Second, we introduced control variables flexibly by generating a different dummy for each value of those variables. Still, the model does not allow for possible interactions between control variables such as age, education, and gender. To verify whether this affected the results, we estimate an alternative model in which all explanatory variables are interacted with the gender dummy. We also estimate a fully saturated model for age, gender and education levels. We also implement versions of the model in which all interacted terms are themselves interacted with either province or municipal dummies. The most saturated specification is akin to a matching estimator: identification comes from comparing connected individuals of the same gender, age, and education living in the same municipality. Point estimates, reported in Table A.11, are smaller but still economically and statistically significant. For example, in the most restrictive regression, being connected to an elected official leads to a 0.36 percentage-points increase in the probability to be employed in a managerial role.

Third, some of the data were collected before the elections but after the date candidates

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

had to announce their candidacy (*i.e.,* November 2009). If incumbents were able to punish the relatives of now known challengers, our results would be downward biased. To check for this possibility, we re-estimate equation (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.12). 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.12).

Fourth, we worry that occupation may not depend on someone's absolute education level, but rather on their education relative to others in the municipality, a situation that would arise in the presence of localized labor markets. Because we have access to census data in each municipality, we are able to control for each individual's relative educational rank in their municipality of residence. Including those variables in equation (2) does not affect our results of interest (Table A.13).

Fifth, if connected individuals live in areas where returns to education are higher, this could lead us to overestimate the value of connections to elected officials. To deal with this concern, we estimate equation (2) allowing for either province, municipality, or village-specific returns to education. As shown in Table A.14, point estimates and significance levels are unaffected.

Sixth, 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.15, this does not affect the estimated value of α.

Seventh, we re-estimate equation (2) including enumerator $\times$ municipality fixed-effects to capture potential enumerator effects.[29] Results are robust to this change (Panel B of Table

[29]Enumerator quality might also have affected the way occupations and 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

A.15). 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.

Eight, we re-estimate equation (2) using probit instead of a linear probability model. The results are presented in Panel C of Table A.15. For most outcomes the point estimates are of similar order of magnitude, although they are smaller for professional and managerial occupations.

Ninth, we have so far used the full sample of individuals aged 20-80. It is however possible that older relatives of elected officials may retire earlier, which would bias our estimates downwards. By a similar reasoning, younger relatives of politicians may postpone entry on the job market. To check for this possibility, we re-estimate equation (2) excluding either younger or older cohorts. Estimates are reported in Table A.19. 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.

Tenth, we have assumed that errors are correlated within provinces. We cannot rule out the possibility that even after controlling for month $\times$ year dummies, there remains some correlation in errors for individuals interviewed at the same time. To investigate whether this affects our results, we report equation (2) results where we cluster standard errors along both month$\times$year and province, using the two-way clustering method developed Cameron, Gelbach, and Miller (2011). Our results, shown in Table A.20, are basically unchanged.

6.4 Heterogeneity

Having confirmed the robustness of our findings to a number possible confounding effects, we investigate whether the value of political ties varies with the type of elected official. To this effect, we estimate equation (3) with all possible interactions between three dummies capturing

bottom 5, 10 and 25 percent in the distribution of share of individuals that are connected. Results are robust to excluding them (Tables A.16 and A.17). Similarly, results are robust to excluding municipalities at the top 5, 10 and 25 percent in the distribution of population (Table A.18). In addition, 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.

links to a mayor, a vice-mayor or a municipal councilor. We then compute the marginal effects for each dummy. Results are shown in Table A.21. 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. Relatives of the mayor are 0.78 percentage-points more likely to be employed in a managerial position; the point estimate for relatives of municipal councilors is 0.42 percentage-points, a difference that is statistically different from zero at the 10 percent level.

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 6, 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 0.12 percentage-point increase in the likelihood of being in a better paid occupation. Second, the impact of family ties on the probability of being employed in a managerial position is 40 percent lower for women than it is for men. While male relatives of local politicians are less likely to be employed, no such effect is observed for female relatives. For all other occupations, we find no significant difference between men and women. Third, the impacts of connections appear to be increasing with age.

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 5 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 that 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 that 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 (7), we find that, in municipalities with higher per capita fiscal transfers, relatives of local politicians are less likely to be employed but also more likely to be employed in a managerial position.

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.22, indicate that the temporising effects of politically divided municipal councils on the benefits of political connections are concentrated on the relatives of the mayor.

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.[30] In addition to numerous control variables, we have dealt with unobserved heterogeneity by using a variety of control groups, including individuals related to candidates elected in subsequent elections. The effects of political connections on better paid occupation that we find are economically and statistically significant, and they are robust to controlling for a number of individual characteristics, and to using many alternative specifications. In addition, we are able to identify a cost of being related to an unsuccessful candidate who narrowly missed winning the 2007 local election.

Our results have a number of implications and suggest some ideas for further research. First, while we are unable to test this directly, we interpret our findings that relatives of close losers 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

[30] We recognize that we are presenting estimates of the value of political connections in the short run. In light of recent results that estimated effects can be either short-lived (Chen, Mu, and Ravallion 2009) or persistent (de Carvalho Filho and Litschig 2013), we will attempt to establish the dynamics of impacts in future research.

staff the bureaucracy with individuals whose views and interests are incompatible with theirs. They attempt to staff the bureaucracy with individuals whose incentives are aligned with their own electoral objectives. Second, this could explain why the returns to connections increase with education levels as politicians provide jobs to their most educated relatives as they are the most capable of steering the bureaucracy. Further, this set of result is not consistent with a view that the effects are purely driven by elected officials' altruistic motives towards their relatives. Third, in some contexts, estimates of the value of political connections obtained with regression discontinuity designs potentially include the cost of being connected to an unsuccessful candidate. Conditioning on close elections changes the nature of the parameter being estimated.

Finally, when deciding whether to run for local elections, candidates must factor in the negative impact that an unsuccessful bid will have on their relatives. The fear of such negative impact may explain the low level of electoral competition in some municipalities of the Philippines, with a large number of candidates running unopposed in the 2010 elections. Those candidates only need one vote to win, hereby muting electoral competition.

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 (Acemoglu, Reed, and Robinson 2013). 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). We use the difference in the probability of being employed as a manager between individuals related to a politician in office between 2007 and 2010 and individuals related to a politician that ran either in the 2007 or 2010 municipal elections as a measure of distortion and find that an increase in the level of distortions is associated with an increase in the percentage of kids under the age of 6 that are underweight (Panel A of Table 9). The correlation is robust to controlling for a number of municipal characteristics, including poverty incidence, average education levels, gini and incumbent vote share in the previous election. Interestingly, once we focus on education, a sector which has not been devolved to the municipal-level, we find no correlation between those outcomes and labor market distortions associated with political connections (Panels B-E of Table 9). While we are unable to make causal claims based on those results, they suggest that politicians's ability to help their relatives secure better-paying occupations affect the way services are delivered by the municipal bureaucracy.

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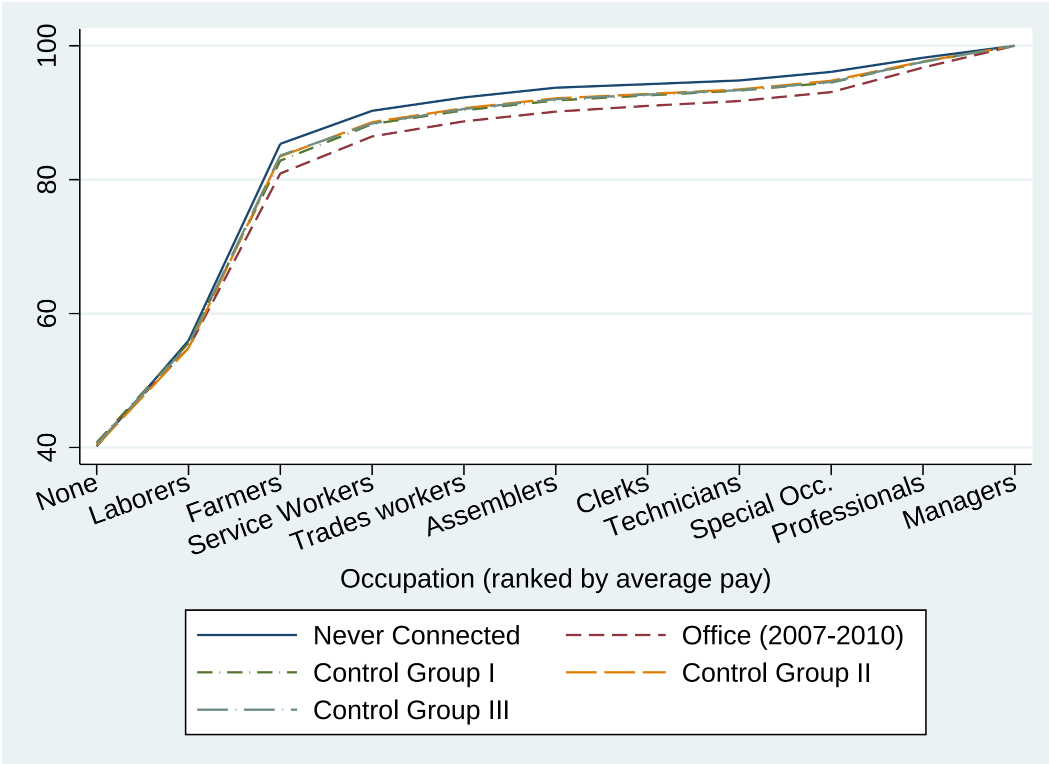

Figure 1: Distribution of occupations

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

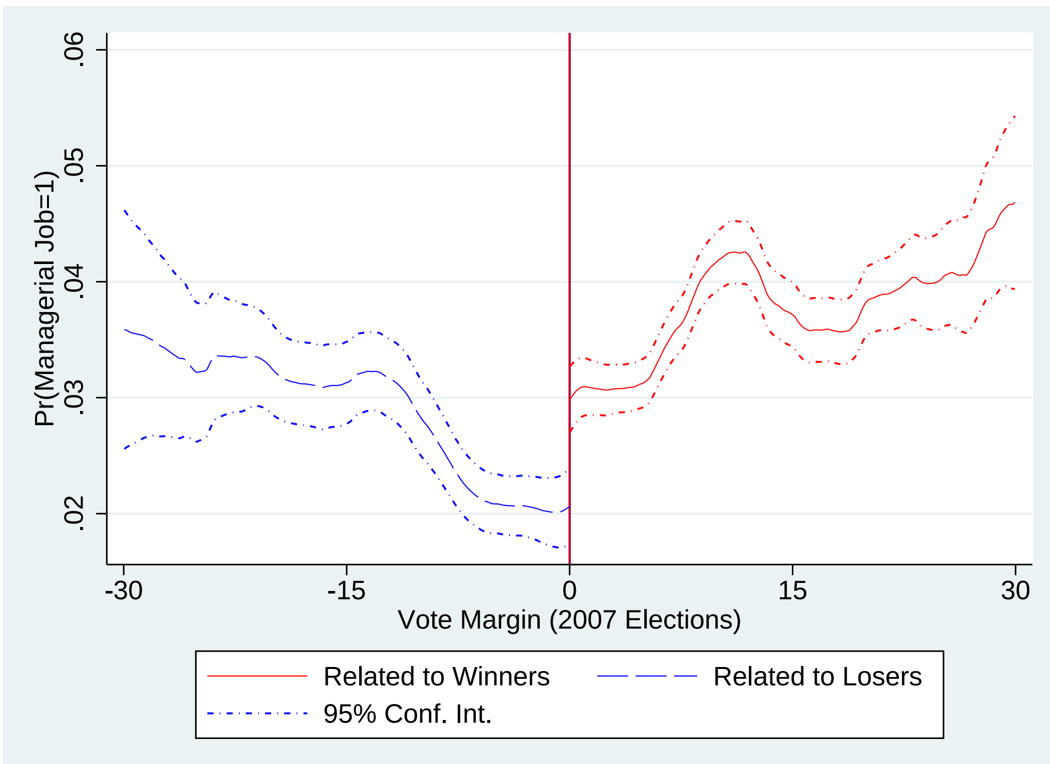

Figure 2: Non-parametric estimates of the probability of being employed in a managerial position

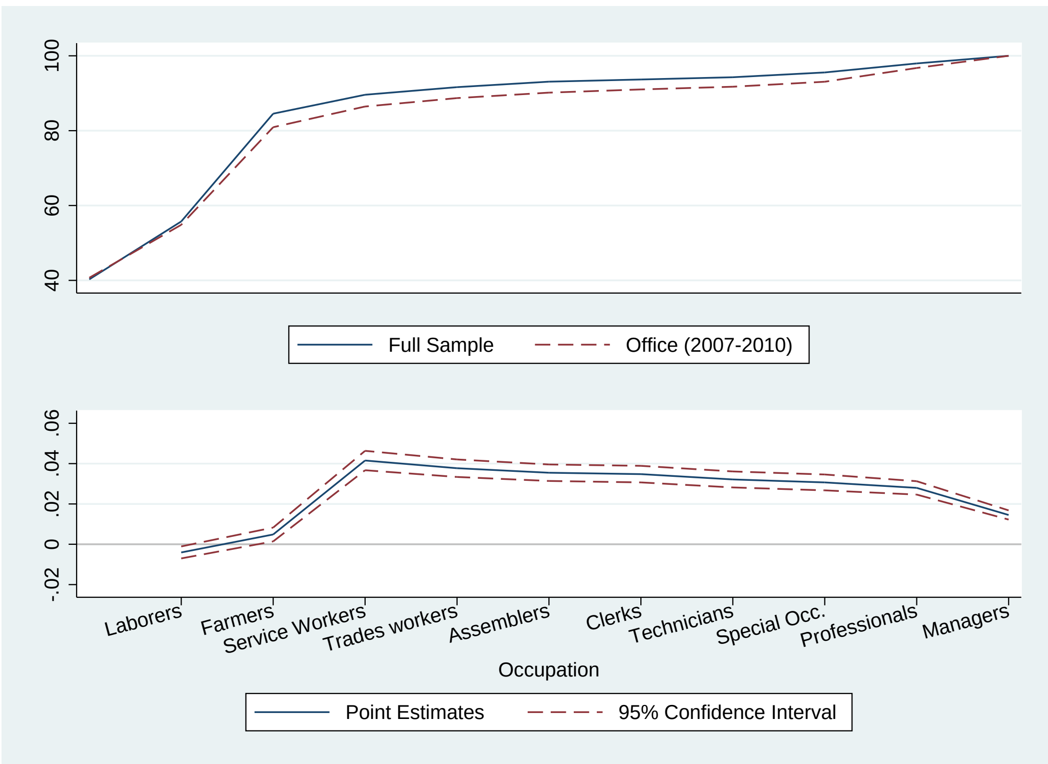

Figure 3: Estimated effects of connections (full sample)

Notes: Cumulative distribution of occupation for the full sample and for relatives of politicians elected in 2007 (upper panel). Results from municipal fixed-effects regressions (lower panel). The standard errors used to generate the 95% confidence intervals account for potential correlation within province. Associated results are reported in Panel A of Table 4.

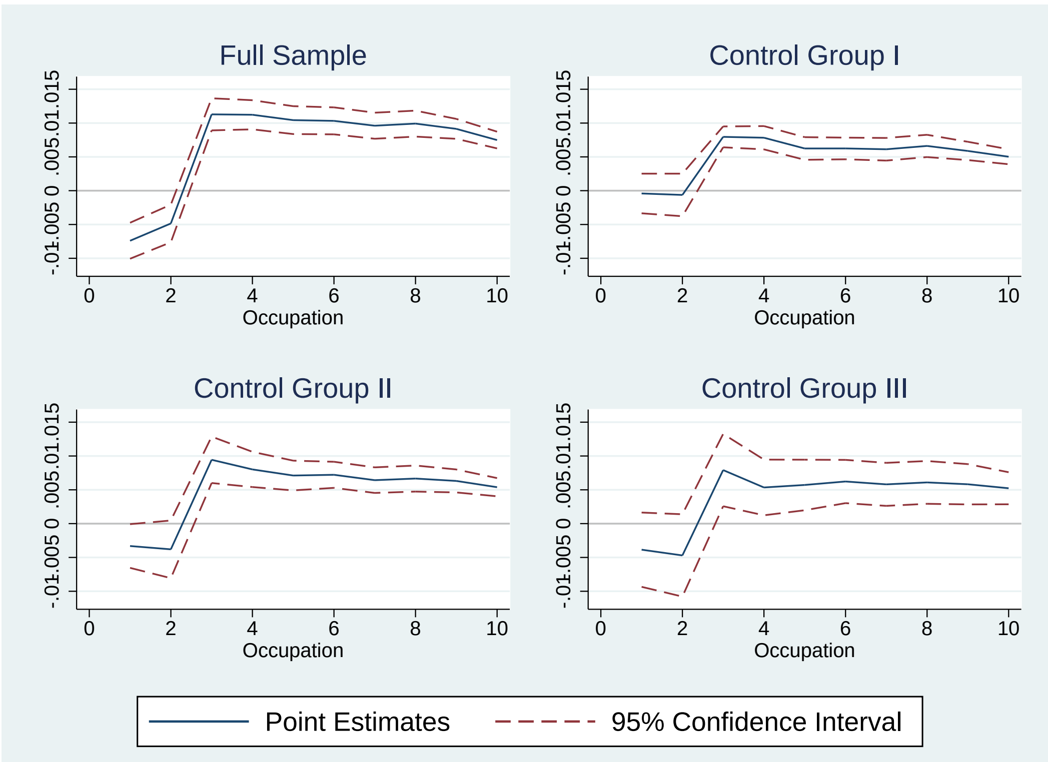

Figure 4: Estimated effects of connections with various control groups

Notes: Results from municipal fixed-effects regressions. Control group I includes relatives of unsuccessful candidates in the 2007 elections, Control group II includes relatives of candidates in the 2010 elections that did not run in 2007 and Control group III includes relatives of successful candidates in the 2010 elections that did not run in 2007. All regressions include a full set of dummies for age, education level and gender. The standard errors used to generate the 95% confidence intervals account for potential correlation within province. Associated results are reported in Panel B of Tables 4 and A.3-A.5.

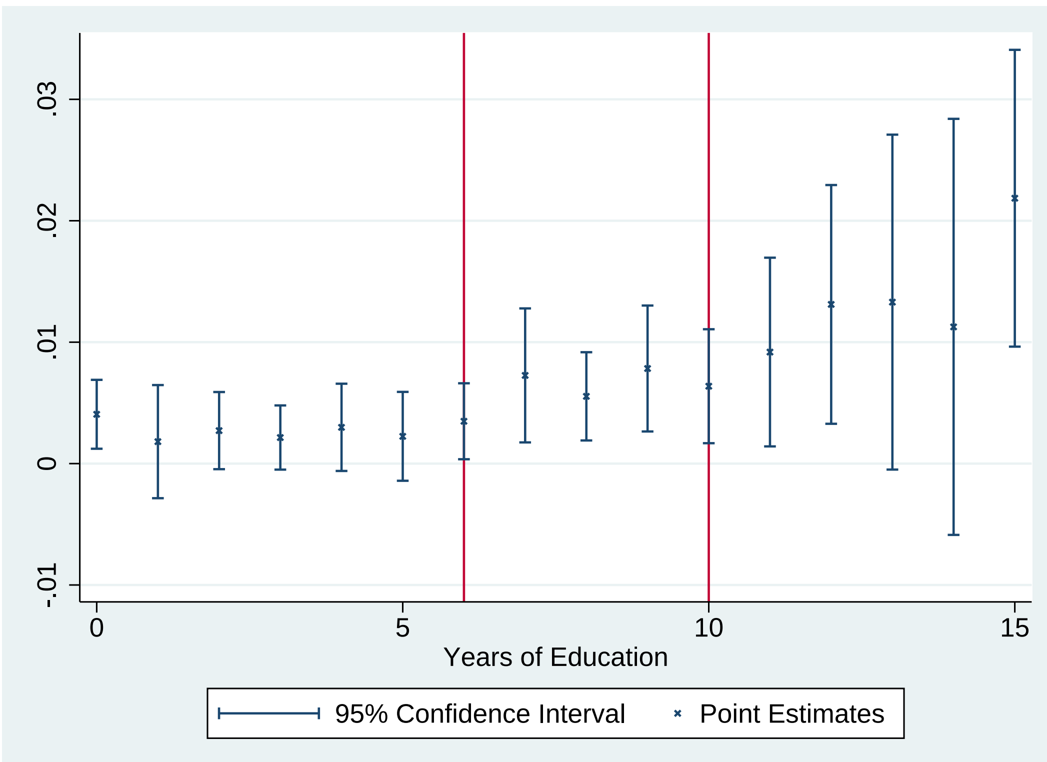

Figure 5: Estimated effects of connections by education levels

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	394,490	390,294	239,926	59,195

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

Table 2: Descriptive statistics: Municipal-level

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

Table 3: The effects of connections on the probability of being in any occupation with regression discontinuity designs - Nonparametric

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Optimal Bandwidth										
Connected Office (2007)	0.0146	0.0509**	0.0944***	0.0822***	0.0792***	0.0847***	0.0739***	0.0655***	0.0736***	0.0132*
	(0.025)	(0.025)	(0.018)	(0.015)	(0.015)	(0.016)	(0.014)	(0.014)	(0.014)	(0.007)
Observations	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0314	0.1174***	0.1290***	0.1187***	0.1069***	0.0944***	0.0791***	0.0704***	0.0570***	0.0167**
	(0.036)	(0.035)	(0.023)	(0.020)	(0.019)	(0.018)	(0.016)	(0.017)	(0.019)	(0.008)
Observations	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	-0.0023	0.0191	0.0589***	0.0517***	0.0417***	0.0458***	0.0419***	0.0447***	0.0400***	0.0119**
	(0.017)	(0.018)	(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.006)
Observations	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734	30,734

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

Table 4: The effects of connections on the probability of being in any occupation - Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects										
Connected Office (2007)	-0.0040***	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)	-0.0074***	-0.0048***	0.0113***	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	0.0125***	0.0121***	0.0110***	0.0107***	0.0100***	0.0103***	0.0094***	0.0077***
	(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***	0.0051***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784	784,784
R-squared	0.330	0.269	0.223	0.238	0.259	0.287	0.267	0.254	0.255	0.080
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	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.332	0.271	0.226	0.240	0.261	0.290	0.270	0.257	0.259	0.083
Panel C: Control Group III										
Connected Office (2007)	-0.002	-0.003	0.0086***	0.0060***	0.0062***	0.0066***	0.0062***	0.0064***	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	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

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

Table 6: Individual heterogeneity: Age, education and gender

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

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

Table 7: Municipal heterogeneity: Economic variables

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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 8: Municipal heterogeneity: Political variables

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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	41.03**	28.99*	29.24*	40.63**	28.84**	29.26**
	(17.34)	(14.99)	(14.88)	(16.66)	(13.81)	(13.61)
Observations	404	404	403	404	404	403
R-squared	0.599	0.635	0.636	0.563	0.623	0.623
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 that are underweight (Panel A), the share of 4-5 year olds that are enrolled in kindergarden (Panel B), the share of 6 year olds that 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 that 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.

Table A.1: 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)	0.0149	0.1134***	0.1269***	0.1151***	0.0750***	0.0632***	0.0691***	0.0648***	0.0616***	0.0132*
	(0.028)	(0.035)	(0.022)	(0.019)	(0.015)	(0.013)	(0.014)	(0.014)	(0.013)	(0.007)
Observations	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0378	0.0994**	0.0956***	0.0886***	0.1073***	0.0971***	0.0809***	0.0716***	0.0697***	0.0170**
	(0.037)	(0.047)	(0.029)	(0.024)	(0.018)	(0.017)	(0.016)	(0.017)	(0.016)	(0.008)
Observations	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	0.0057	0.0463*	0.0903***	0.0786***	0.0435***	0.0348***	0.0380***	0.0438***	0.0275***	0.0118**
	(0.019)	(0.024)	(0.016)	(0.014)	(0.012)	(0.011)	(0.010)	(0.010)	(0.009)	(0.006)
Observations	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700	14,700

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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.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)	0.0153	0.0468*	0.0911***	0.0812***	0.0744***	0.0632***	0.0581***	0.0502***	0.0615***	0.0131*
	(0.025)	(0.025)	(0.017)	(0.015)	(0.014)	(0.013)	(0.013)	(0.012)	(0.013)	(0.007)
Observations	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706
Panel B: Half Optimal Bandwidth										
Connected Office (2007)	0.0305	0.1140***	0.1288***	0.1184***	0.1072***	0.0971***	0.0831***	0.0745***	0.0697***	0.0171**
	(0.036)	(0.035)	(0.023)	(0.020)	(0.018)	(0.017)	(0.016)	(0.015)	(0.016)	(0.008)
Observations	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706
Panel C: Twice Optimal Bandwidth										
Connected Office (2007)	-0.0030	0.0203	0.0576***	0.0494***	0.0398***	0.0324***	0.0323***	0.0316***	0.0273***	0.0119**
	(0.017)	(0.017)	(0.013)	(0.011)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)	(0.006)
Observations	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706	55,706

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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.4: 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	0.0286***	0.0253***	0.0236***	0.0235***	0.0215***	0.0207***	0.0193***	0.0104***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.024	0.048	0.039	0.028	0.027	0.028	0.027	0.028	0.018	0.016
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0033**	-0.0038*	0.0094***	0.0080***	0.0071***	0.0072***	0.0064***	0.0067***	0.0063***	0.0054***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.271	0.229	0.223	0.237	0.259	0.288	0.269	0.256	0.257	0.081
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.001	-0.001	0.0100***	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	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416	634,416
R-squared	0.332	0.271	0.226	0.240	0.261	0.290	0.270	0.257	0.259	0.083

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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 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	0.0231***	0.0192***	0.0190***	0.0194***	0.0181***	0.0176***	0.0165***	0.0094***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.024	0.048	0.038	0.028	0.028	0.029	0.028	0.028	0.020	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.004	-0.005	0.0079***	0.0054**	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	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.271	0.229	0.229	0.243	0.266	0.295	0.275	0.263	0.263	0.086
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.002	-0.003	0.0086***	0.0060***	0.0062***	0.0066***	0.0062***	0.0064***	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	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.331	0.271	0.232	0.246	0.268	0.297	0.277	0.264	0.264	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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***	0.0203***	0.0107***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221
R-squared	0.024	0.047	0.039	0.028	0.027	0.028	0.028	0.028	0.019	0.016
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	-0.0030*	-0.003	0.0097***	0.0088***	0.0076***	0.0077***	0.0068***	0.0070***	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	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221
R-squared	0.270	0.228	0.224	0.239	0.261	0.289	0.269	0.256	0.257	0.082
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	-0.000	-0.001	0.0101***	0.0093***	0.0078***	0.0079***	0.0070***	0.0072***	0.0068***	0.0055***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221	575,221
R-squared	0.331	0.270	0.227	0.241	0.263	0.290	0.271	0.258	0.259	0.084

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

Table A.7: The effects of connections on the probability of being in any occupation using unsuccessful 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	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229
R-squared	0.027	0.053	0.044	0.034	0.033	0.035	0.034	0.033	0.027	0.023
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	0.003	0.0063*	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	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229
R-squared	0.269	0.231	0.239	0.257	0.278	0.306	0.286	0.275	0.276	0.093
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	0.004	0.0076**	0.0100***	0.0100***	0.0083***	0.0090***	0.0084***	0.0090***	0.0079***	0.0078***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229	207,229
R-squared	0.334	0.276	0.243	0.261	0.281	0.308	0.288	0.277	0.278	0.096

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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 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	-0.0216***	0.0039**	0.000	-0.000	0.0014***	0.001	0.001	0.0071***	0.0094***
	(0.003)	(0.004)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.058	0.080	0.033	0.016	0.010	0.006	0.006	0.036	0.013	0.018
Panel B: Municipal Fixed Effects and Individual Controls (1)										
Connected Office (2007)	0.001	-0.0126***	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	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.089	0.294	0.044	0.020	0.025	0.024	0.021	0.043	0.202	0.086
Panel C: Municipal Fixed Effects and Individual Controls (2)										
Connected Office (2007)	0.001	-0.0111***	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	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.097	0.330	0.045	0.021	0.026	0.026	0.022	0.043	0.202	0.088

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed in 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.9: The conditional effects of connections on the probability of being in each occupation using successful 2010 candidates as a control group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2-10	3-10	4-10	5-10	6-10	7-10	8-10	9-10	10-
Panel A: Municipal Fixed Effects									
Connected Office (2007)	0.002	0.0473***	0.012	0.0216***	0.0251***	0.001	0.0119**	0.0223**	0.0236**
	(0.004)	(0.008)	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)	(0.011)	(0.010)
Observations	269,149	204,550	84,952	60,309	50,135	43,537	39,748	36,493	30,519
R-squared	0.107	0.132	0.100	0.102	0.095	0.049	0.070	0.181	0.134
Panel B: Municipal Fixed Effects and Individual Controls (1)									
Connected Office (2007)	-0.004	0.0179***	-0.003	0.009	0.0112**	-0.002	0.0095**	0.0161*	0.0233**
	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.009)	(0.010)
Observations	269,149	204,550	84,952	60,309	50,135	43,537	39,748	36,493	30,519
R-squared	0.152	0.471	0.206	0.215	0.305	0.077	0.109	0.281	0.278
Panel C: Municipal Fixed Effects and Individual Controls (2)									
Connected Office (2007)	-0.004	0.0180***	-0.002	0.009	0.0113**	-0.001	0.0098**	0.0158*	0.0245**
	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.008)	(0.010)
Observations	269,149	204,550	84,952	60,309	50,135	43,537	39,748	36,493	30,519
R-squared	0.158	0.475	0.209	0.218	0.310	0.082	0.112	0.283	0.284

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

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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.11: Robustness checks: Towards a fully saturated model

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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, education dummies are interacted with province dummies. The standard errors (in parentheses) account for potential correlation within province. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.12: Robustness checks: Exclude data collected after November 2009

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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.13: Robustness checks: Relative educational achievements

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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 A.14: Robustness checks: Area-specific returns to education

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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 A.15: 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***	0.0043*	0.0055***	0.0057***	0.0049***	0.0051***	0.0054***	0.0053***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.354	0.308	0.259	0.266	0.285	0.313	0.293	0.282	0.279	0.112
Panel B: Enumerator*Municipal Fixed Effects and Individual Controls										
Connected Office (2007)	-0.0048*	-0.004	0.0068***	0.0040*	0.0051**	0.0052***	0.0048***	0.0051***	0.0052***	0.0054***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685
R-squared	0.376	0.326	0.283	0.295	0.314	0.342	0.324	0.314	0.309	0.152
Panel C: Municipal Fixed Effects and Individual Controls - PROBIT										
Connected Office (2007)	-0.002	-0.003	0.0092***	0.0053***	0.0048***	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	453,671	453,656	453,617	453,577	453,562	453,565	453,565	453,565	453,184	451,249

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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: 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	-0.002	0.0087***	0.0061***	0.0063***	0.0067***	0.0063***	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	448,144	448,144	448,144	448,144	448,144	448,144	448,144	448,144	448,144	448,144
R-squared	0.330	0.270	0.231	0.246	0.268	0.297	0.277	0.264	0.264	0.087
Panel B: Exclude top 10%										
Connected Office (2007)	-0.001	-0.002	0.0087***	0.0062***	0.0063***	0.0068***	0.0064***	0.0067***	0.0063***	0.0056***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	435,023	435,023	435,023	435,023	435,023	435,023	435,023	435,023	435,023	435,023
R-squared	0.330	0.270	0.231	0.246	0.268	0.296	0.277	0.264	0.264	0.088
Panel C: Exclude top 25%										
Connected Office (2007)	-0.001	-0.003	0.0088***	0.0058***	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	387,622	387,622	387,622	387,622	387,622	387,622	387,622	387,622	387,622	387,622
R-squared	0.331	0.266	0.231	0.247	0.269	0.298	0.279	0.267	0.267	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.17: 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	0.0091***	0.0064***	0.0061***	0.0064***	0.0058***	0.0061***	0.0062***	0.0058***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	434,194	434,194	434,194	434,194	434,194	434,194	434,194	434,194	434,194	434,194
R-squared	0.330	0.269	0.230	0.245	0.268	0.297	0.277	0.265	0.264	0.087
Panel B: Exclude bottom 10%										
Connected Office (2007)	-0.002	-0.003	0.0095***	0.0062***	0.0058***	0.0062***	0.0056***	0.0061***	0.0064***	0.0066***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	395,418	395,418	395,418	395,418	395,418	395,418	395,418	395,418	395,418	395,418
R-squared	0.331	0.269	0.228	0.244	0.266	0.294	0.275	0.263	0.262	0.084
Panel C: Exclude bottom 25%										
Connected Office (2007)	-0.002	-0.004	0.0131***	0.0086***	0.0084***	0.0069***	0.0063**	0.0064**	0.0070***	0.0063***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	287,665	287,665	287,665	287,665	287,665	287,665	287,665	287,665	287,665	287,665
R-squared	0.332	0.270	0.225	0.243	0.267	0.296	0.276	0.264	0.264	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, 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: 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	0.0097***	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	406,715	406,715	406,715	406,715	406,715	406,715	406,715	406,715	406,715	406,715
R-squared	0.335	0.277	0.233	0.248	0.271	0.298	0.279	0.267	0.267	0.089
Panel B: Exclude top 10%										
Connected Office (2007)	-0.001	-0.003	0.0104***	0.0080***	0.0075***	0.0075***	0.0069***	0.0072***	0.0068***	0.0051***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	374,067	374,067	374,067	374,067	374,067	374,067	374,067	374,067	374,067	374,067
R-squared	0.336	0.279	0.233	0.249	0.271	0.298	0.279	0.267	0.267	0.088
Panel C: Exclude top 25%										
Connected Office (2007)	0.000	-0.003	0.0135***	0.0103***	0.0099***	0.0090***	0.0082***	0.0087***	0.0082***	0.0063***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	292,796	292,796	292,796	292,796	292,796	292,796	292,796	292,796	292,796	292,796
R-squared	0.336	0.286	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.19: 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	-0.001	0.0085***	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	407,771	407,771	407,771	407,771	407,771	407,771	407,771	407,771	407,771	407,771
R-squared	0.338	0.274	0.236	0.250	0.271	0.302	0.281	0.267	0.267	0.090
Panel B: Age > 22										
Connected Office (2007)	-0.002	-0.004	0.0093***	0.0063***	0.0068***	0.0071***	0.0066***	0.0069***	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	405,741	405,741	405,741	405,741	405,741	405,741	405,741	405,741	405,741	405,741
R-squared	0.329	0.265	0.243	0.255	0.279	0.308	0.288	0.275	0.274	0.091
Panel C: Age > 22 and Age <63										
Connected Office (2007)	-0.000	-0.001	0.0093***	0.0066***	0.0067***	0.0068***	0.0062***	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	359,827	359,827	359,827	359,827	359,827	359,827	359,827	359,827	359,827	359,827
R-squared	0.335	0.267	0.248	0.260	0.283	0.315	0.293	0.279	0.278	0.094

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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 A.20: Robustness checks: Two-way clustering

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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.21: 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: Connected Mayor (2007)	0.002	-0.001	0.0154***	0.0141***	0.0131***	0.0124***	0.0107***	0.0109***	0.0093***	0.0078***
	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
B: Connected Vice-Mayor (2007)	0.002	0.004	0.0099***	0.0083***	0.0061**	0.0068***	0.0059***	0.0064***	0.0058**	0.0055***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
C: Connected Councilor (2007)	0.000	-0.002	0.0086***	0.0062***	0.0055***	0.0056***	0.0050***	0.0052***	0.0045***	0.0042***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Test H0: A =B	0.020	0.887	1.853	2.349	5.379	3.599	2.675	2.110	1.182	0.876
Ha: A $\neq$ B [p-value]	[0.888]	[0.346]	[0.173]	[0.125]	[0.020]	[0.058]	[0.102]	[0.146]	[0.277]	[0.349]
Test H0: A =C	0.274	0.014	4.265	6.121	8.776	8.084	4.898	5.262	3.815	3.529
Ha: A $\neq C$ [p-value]	[0.600]	[0.904]	[0.039]	[0.013]	[0.003]	[0.004]	[0.027]	[0.022]	[0.051]	[0.060]
Test H0: B =C	0.061	1.244	0.123	0.549	0.044	0.265	0.135	0.257	0.194	0.304
Ha: B $\neq$ C [p-value]	[0.805]	[0.265]	[0.726]	[0.459]	[0.833]	[0.607]	[0.713]	[0.613]	[0.659]	[0.582]
Observations	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685	453,685

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

Table A.22: Municipal heterogeneity: Municipal council

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

Notes: Results from fixed-effects regressions. The dependent variable is a dummy equal to one if the individual is employed (Column 1), is employed in 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.