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ABSTRACT

Politics in the Family: Nepotism and the Hiring Decisions of Italian Firms*

In this paper we investigate the effect of family connections to politicians on individuals' labor market outcomes. We combine data for Italy over almost three decades from longitudinal social security records on a random sample of around 1 million private sector employees with the universe of around 500,000 individuals ever holding political office, and we exploit information available in both datasets on a substring of each individual's last name and municipality of birth in order to identify family ties. Using a diff-in-diff analysis that follows individuals as their family members enter and leave office, and correcting for the measurement error induced by our fuzzy matching method, we estimate that the monetary return to having a politician in the family is around 3.5 percent worth of private sector earnings and that each politician is able to extract rents for his family worth between one fourth and one full private sector job per year. The effect of nepotism is long lasting, extending well beyond the period in office. Consistent with the view that this is a technology of rent appropriation on the part of politicians, the effect increases with politicians' clout and with the resources available in the administration where they serve.

JEL Classification: D72, D73, H72, J24, J30, M51

Keywords: Nepotism, family connections, politics, rent appropriation

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1. Introduction

There is plenty of anecdotal evidence that firms reserve a special treatment to politicians' family members.[1] However, credible evidence on this phenomenon, let aside its determinants, remains elusive. In this paper we combine micro data for Italy over almost thirty years on the universe of around 500,000 individuals holding political office with social security micro data on a random sample of around one million private sector employees in order to estimate the labor market returns to family connections to individuals in office and provide a wider measure of the returns to holding office.

Italy appears an ideal case study for our analysis. Since at least Banfield's (1958) *Moral Basis of a Backward Society* the roles of family ties, lack of trust and civic participation and the associated incidence of nepotism and lack of meritocracy in shaping the fabric of society and the economy have been long recognized, and they are often seen at the root of the country's inability to modernize (Pellegrino and Zingales 2014, Putnam et al 1993). Alongside, widespread red tape, a cumbersome bureaucracy, inefficient public and justice sectors, and a pervasive control of politics over the economy create wide opportunities for corruption and personal enrichment. The country ranks third from the bottom among OECD high-income countries in the Ease of Doing Business index (World Bank 2014) and the highest among all European countries in terms of the Corruption Perception Index (Transparency International 2014).

One major advantage of the data that we have assembled is that they provide information on individuals' tax code, which in Italy includes the first three consonants (in short F3C) of one's last name and an identifier for the municipality of birth. We define "families" as groups of individuals sharing the same F3C and born in the same municipality. Given the very high number of highly locally concentrated last names, the high number of geographical divisions and low geographical mobility this method has the potential to identify families with a high degree of precision.

In order to identify the causal effect of a family member holding office on individuals' labor market outcomes we exploit the longitudinal nature of the workers' data and the timing of "family" members' movements in and out of office, in the spirit of a differences-in-differences analysis. In particular, we examine how workers' earnings and employment change as individuals with the same F3C and born in the same municipality assume or leave office.

Despite the very fine-grained partition of the data, our matching method clearly identifies family connections with error, since not only does it fail to classify some connected individuals as family members, but also it erroneously classifies some unconnected individuals as family members. We show that misclassification induces a systematic downward bias in our estimates and that one can use information on the distribution of F3Cs in the population - which we

[1] Allegations of political nepotism against companies often surface in the press. One prominent recent case involves the SEC's allegations that "JPMorgan's [...] hired the children of high-ranking Chinese officials to help win business (Financial Times 2015).

derive from auxiliary data - together with plausible values of the number of truly connected individuals - which we derive based on simulations - to correct the estimates for measurement error.

Our estimates imply that family connections to an individual in office yield an average monetary return in terms of private sector earnings of around 700 euros per year (770 USD at today's exchange rate), approximately a 3.5 percent increase relative to a baseline level of private sector earnings of slightly less than 20,000 euros. This is not much below the return to one extra year of education in Italy (5.3, see Hanushek et al 2013). Using the benchmark estimate - that we derive from simulations - of an average of six consanguineal close family members i.e., those more likely to benefit from political connections, born in the same municipality, this yields an average return per politician from holding office of around 4,200 euros worth of private sector jobs per year.

These estimates are likely to be conservative as they exclude affinal relatives (as well as relatives born elsewhere, which we ignore throughout). If these individuals also benefit from connections, this number will need to be multiplied by a factor of four, implying a yearly return to holding office as high as 16,800 euros, i.e., earnings worth almost one extra private sector job per year per politician. Importantly, effects manifest precisely when an individual in the family assumes office but they persist even after this individual leaves office, suggesting not only sizeable but also long-lasting effects of holding political office on family members' private sector careers.

We bring ammunition to the argument that our estimates genuinely capture nepotistic practices by examining the gradient in the estimated effect as a function of politicians' clout. We find that the effect is larger the higher the level of political office, the higher the level of government, and the longer tenure in office. Similar to Brollo et al's (2013) and Dal Bó et al (2006) claim that corruption increases when resources increase, we also find that nepotistic practices appear to respond directly to the resources available to politicians, as measured by the budget available to the administration where they serve.

Our paper relates and contributes to different streams of literature in both labor economics and political economy. An established body of literature in labor economics focuses on - and finds evidence in favor of considerable - intergenerational persistence in socio-economic status, income and human capital, occupations - including political occupations - jobs and even firm's control (Bertrand and Schoar 2006, Black and Devereux 2011, Dal Bó et al 2009, Durante et al 2011, Kramarz and Skans 2014, Solon 1999). A related body of literature in social sciences uses last names to identify family ties (e.g., Angelucci et al 2010, Fafchamps and Labonne 2013, Guell et al 2014) or to measure intergenerational mobility and the concentration of families in specific occupations (Clark 2012, Clark and Cummins 2014, Durante et al 2011).

While, through the provision of insurance, information or mechanisms of contract enforcement, family and other informal connections might provide a second best solution to market failures, assignment of jobs and the availability of opportunities based on one's name or contacts rather than one's talent might come to the detriment of others, i.e., those who do not boast such connections, potentially leading to a misallocation of resources in society and an overall efficiency loss, a point often made in relation to the management of family firms (Bertrand and Schoar 2006, Pérez-González 2006).

Low levels of mobility in socio-economic status across generations might also create incentives to divert resources away from productive investment, such as human capital, towards rent-seeking activities, such as the preservation of family ties, impede geographical mobility and risk taking, and overall reduce total output. Consistent with this view, there is compelling evidence that stronger family ties lead to lower levels of trust, political participation and social capital, lower economic development and poorer quality of institutions, including lower control of corruption (Alesina and Giuliano 2014).

A branch of literature in political economy focuses on the private returns to holding political office. Clearly, by the nature of their job, those in office have disproportionate control over public resources and authority over legislative and administrative acts that affect others, making it in principle possible to divert public resource for personal use or take decisions that are ultimately in the private as opposed to the public interest. The private returns to holding political office stem precisely from the specific rents associated to holding office. One direct measure of the returns to public office is politicians' pay. Borrowing from the literature on incentives in managerial and personnel economics, a number of authors emphasize that in addition to the systems of checks and balances that characterize most modern democracies, *in primis* elections, above-market pay can create a powerful discipline device, making politicians' misbehavior costly and improving effort (Besley 2004, Ferraz and Finan 2011, Gagliarducci and Nannicini 2013).

In addition to pay, there are other dimensions of the returns to political office. Not only do ego rents or utility gains presumably accrue even to benevolent politicians from serving, but holding political office might lead to powerful connections, put individuals in the "spotlight", hence revealing their quality (Mattozzi and Merlo 2008), or create opportunities for rent-seeking - and even illegal - activities when the incentives not to misbehave are not sufficiently powerful. Indeed, there is considerable evidence of substantial monetary and non-monetary returns to political careers both while in office and after that (Cingano and Pinotti 2013, Diermeier et al 2005, Fisman et al 2014, Merlo et al 2010), including through the establishment of political dynasties (Dal Bó et al 2009).[2] At the extreme, politicians can profit from their position in order to engage in corruption and grafting, i.e., illegal activities in connection to their office that yield a private utility (Banerjee et al 2012, Brollo et al 2013, Olken 2007, Shleifer and Vishny 1993).

[2] A related literature shows that, through pork mechanisms, clientelistic practices and vote buying, politicians can use public resources to generate political support, and hence reap political benefits from their office (Alesina et al 2001, Finan and Schechter 2012, Levitt and Snyder 1997, Manacorda et al 2011). A somewhat related stream of literature focuses on favoritism along ethnic lines (e.g., Burgess et al 2013) or city of birth and city of election lines (Carozzi and Repetto 2015).

Connecting the literature on the role of informal and family ties with the literature on political careers, others have documented that connections to politicians matter for the fortunes of individuals, groups and organizations. A number of papers document that companies linked to politicians or to ruling political parties - including through family ties - tend to perform better, have greater access to credit and are more likely to escape the burden of bureaucracy and regulation (see for example Acemoglu et al 2015, Amore and Bennedsen 2013, Cingano and Pinotti 2013, Faccio 2006, Ferguson and Voth 2008, Fisman 2001, Khwaja and Mian 2005). These links appear to be more frequent and more profitable in more corrupt environments, providing indirect evidence that they might directly benefit politicians. Consistent with this view, Bertrand et al (2007) show that firms connected to incumbent candidates engage in hiring around the time of election, something that they ascribe to the electoral returns accruing to the incumbent from such practices. These studies largely focus on connections to shareholders, CEOs and board members and typically refer to small samples of firms.

Indeed, very few studies examine the individual labor market returns to political connections. A few existing studies focus on political occupations (Blanes et al 2012 investigate the career of US lobbyists, and Dal Bó et al 2009 focus on political dynasties). A paper that is very closely related to ours is Fafcahamps and Labonne (2013), which investigates the effect of family connections to local politicians - identified similarly to us through homonymy - in the Philippines. Although they find no evidence of such connections having an effect on the probability of employment, they also find a positive effect on the probability of holding managerial positions and an associated negative effect on the probability of holding unskilled manual positions, implying that such connections might lead to higher wages. One drawback of this study is that it does not distinguish between private and public employment, leaving open the possibility that most of the effects found are ascribable to nepotistic hiring or promotions in the public sector, where these decisions are under the - direct or indirect - control of politicians. In contrast, we focus on the returns to connections in the private sector, where politicians have, at least in principle, no direct control over hiring, firing and promotion decisions.

The rest of the paper is organized as follows. In Section 2, we describe the data. In Section 3 we discuss the identification of the model, while in Sections 4 we present the regression results. Section 5 finally concludes.

2. Data

2.1. Workers' data

For the purpose of the empirical exercise, we use a workers' micro data from the Italian National Institute of Social Security (*Istituto Nazionale della Previdenza Sociale*, in short INPS). These are matched employer-employee data that, for each year, record all employment spells and the associated earnings for the universe of dependent workers in the private sector, hence excluding self-employment and public sector employment.[3] Different versions of the same data have been used extensively (see for example Card et al 2014, Cingano and Pinotti 2003, Guiso et al 2005). The version of the data we use refers to a random sample of around 3 percent (12/365, i.e., those born on the first day of each month in each year) of the population of individuals with at least one social security spell between 1985 and 2011.

In addition to the number of months of work during the year and gross labor income (including bonuses and premia) in each job in each year, the data provide basic job characteristics, including occupation (in three broad categories: blue collar, white collar and manager) and sector of activity (at one digit level). Unfortunately, other than for an anonymous firm identifier, no additional information is available on the firm.

Importantly, INPS data contain workers' tax code (*codice fiscale*), which, in addition to gender and date of birth, is a function of each worker's first three consonants of the last name (F3C) and municipality of birth.

Administratively, Italy is divided into 8,110 municipalities, 103 provinces (similar to US counties), and 20 regions (roughly corresponding to US states). The data also provide province (but not municipality) of work.[4]

The original data provide information on all employment spells during each year, where an employment spell is defined by the interaction of employer X level of occupation. This means that individuals can have more than one observation per year if they work for more than one employer or if they change occupation. For computational purposes, we transform the data so to have one observation for individual per year. We assign to each individual in each year the total number of calendar months worked and total earnings in all jobs while we assign the characteristics (i.e., occupation and industry) of the most highly paying job in that year.

In order not to confound the effect of family connections with the effect of one's political career on own earnings and employment, we also exclude from the sample workers who ever appear in the politicians' data set (see next section).

Overall, over twenty-seven years, the data provide information on around 925,000 individuals, with an average number of years in the data just over 10, i.e., a total of around 9.5 million individual X year observations.

Table 1 provides summary statistics for the workers' data. Average real (at 2005 prices)

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[3] Since the mid 1990's, a series of reforms have extended the mandate of INPS to include some categories of self-employed workers and public sector workers. Our data only refer to those originally included in the INPS fund.

[4] The first three digits of a worker's tax code correspond to the first three consonants of one's last name (e.g., GGL for Gagliarducci and MNC for Manacorda). For women, there are derived from their maiden name. For last names that contain less than three consonants, any missing digit is replaced with the first available vowel. For last names that contain less than three letters, which is very uncommon, the missing digit is replaced with the letter X. The remaining digits of the tax code are functions of the individual's first name (three digits), the day (two digits), month (two digits), year (four digits), and municipality (or country, for those born abroad) of birth (3 digits), plus a control digit. The algorithm to compute one's personal tax code is public (http://goo.gl/MEFDlo). In the very rare case of two or more individuals with the same first 15 digits of the tax code, the last digit is recomputed by the Italian Revenue Agency to guarantee a unique tax code.

yearly earnings among those with a least one day of social security contributions during the year are about 19,500 euros (around US$ 21,000), with workers working on average 10 calendar months and holding 1.2 jobs in the year, either simultaneously or in different months. Unsurprisingly, there are more men than women in the data, only a small fraction of workers are in managerial positions, and a comparison between place of birth, place of residence and place of work illustrates a steady migration flow from the poorer regions in the South to the richer and more industrialized regions in the North.

2.2. Politicians' data

We combine the INPS data with yearly data from the Ministry of Interior Affairs on the universe of individuals holding political office between 1985 and 2011. The data refer to the universe of individuals holding political office, at any level of government - local, subnational and national - whether elected or appointed and whether in the legislative or executive branch.

In addition to the central government (composed by the two houses of parliament, the central government and the prime minister), each geographical entity (municipality, province, region) has its own local government, with both a legislative and an executive branch and a head of the executive (mayor, president of province and governor of region, respectively). Each of these different levels of government has authority over and responsibility for the provision of local public goods and services, administrative authority over the issuing of permits and licenses, and - with the exception of the central government - only modest power to levy taxes.

For each individual in office, in addition to the exact level of government, whether in a council or executive position, date of assuming and leaving office (where the former is left censored to January 1^{st} 1985, and the latter is right censored to December 31^{st} 2011), usual occupation and highest education level, the data also provide each individual's first and last name, and hence the F3C, gender, municipality and date of birth. Importantly, we do not have data on candidates other than those who are elected. The data also provide only imprecise information on party affiliation or on whether an individual comes from a party that is part of the ruling coalition.[5]

Overall, between 1985 and 2011 there are around 137,000 individuals in office every year, for a total of approximately 525,000 individuals and average tenure (in the same or different offices) of around 7 years.[6]

[5] As individuals can hold more than one office simultaneously within the same government (e.g., council member and local commissioner), we assign to each individual the highest office among all those held while we treat the same individual simultaneously holding office in different governments (e.g., a mayor also sitting in parliament) as two separate observations. For married women, there is no explicit rule stating whether they have to use their maiden on their husband's last name, although it is customary for most women to use their maiden name.

[6] The normal length of the legislature in Italy varies between four and five years, depending on the level of government and the period considered. *De facto*, though, the duration of the legislature is often much shorter. Since 1948, i.e., since the first national election, there have been seventeen elections and sixty different national governments.

Table 2 presents yearly average characteristics of politicians. The statistics in the table refer to year X government X individual observations and each observation is weighted by the fraction of the year in office. Not surprisingly, the greatest majority of those in office hold positions in the municipal government, accounting for more than 96 percent of the observations. In contrast, national politicians account for less that 1 percent of the observations. Around 70 percent of individuals are in council positions. The table also shows that politicians are disproportionately males, they have relatively high levels of education compared to the population at large, and many have professional occupations.[7] A comparison between place of birth and place of office shows that around 48 percent of municipal officials serve in their municipality of birth, 85 percent of municipal and provincial officials in their province of birth, and 92 percent of municipal, provincial, and regional officials serve in the region of birth.

2.3. Matched workers-politicians' data

We link workers' data to politicians' data based on municipality of birth and F3C. We discuss in Section 4.7 the ability of this match to deliver reliable estimates of family connections.

We start by transforming the workers' data the data into a yearly panel, with one observation per year for each individual who is ever observed in the social security data. When an individual has no social security record in a year, we assign zero earnings and zero months of work.

Next, for each individual in the INPS sample we compute the total number of individuals in office carrying the same F3C and born in the same municipality, overall and by type (i.e., by level of government, office, gender, tenure, etc.). We restrict to individuals of working age, i.e., not younger than 18 and not older than 65 born in Italy and we also exclude workers with a frequency of the F3C in their municipality of birth (that we derive based on data in Section 2.4) greater or equal than 1,000. We do so in order to avoid very imprecise matches between workers and politicians, and in particular to attenuate the consequences of type-2 error that stems from classifying a large number of unrelated individuals to political families (see Section 2.4 and Appendix A.1).[8]

This delivers an unbalanced panel (due to the age restrictions) of around 800,000 workers and 17 million year X individual observations. The average number of individuals with the same F3C and born in the same municipality in office in a year is 0.4. This is clearly a large number that reflects the fact that our fuzzy matching method identifies families with error. We revert to this issue below.

[7] For comparison, the fraction of the male labor force with a high school degree is 24 percent, while the fraction with a college degree is 11 percent (Istat 2010).

[8] This selection criterion leads us to drop around 12 percent of workers from the INPS data.

2.4. Data on last names and F3Cs

We finally complement our analysis with cross-sectional data on the universe of personal tax returns in Italy, which allow us to derive a distribution of last names by municipality. We use these data to both validate our measure of family ties and, later, to correct for measurement error.

One characterizing feature of Italy, due to its late unification and historical fragmentation, is the very high number of last names (Caffarelli and Marcato 2008). Coupled with a high number of geographical divisions (municipalities) and the low geographical mobility, this implies that a matching based on municipality of birth and F3C has the potential to identify relatively small groups. [9]

The data that we use for this auxiliary exercise come from the universe of personal tax returns in 2005, covering over 39 million individual taxpayers (out of around 60 million inhabitants). Individual tax returns filed by all physical persons in Italy were briefly posted on line by the Italian Revenue Service (*Agenzia delle Entrate*) in 2008 in an attempt to "name and shame" tax evaders, before being removed under a public uproar.Although we do not have access to the micro data on tax returns, we have aggregate data on the number of taxpayers by last name and municipality of *residence*. This gives around 2 million distinct combinations (for comparison, data for the USA report around 6 million last names, see Word et al 2008. In contrast, data for China report only around 7,000 last names, see Liu et al 2012). After some data cleaning, we are left with around 500,000 unique last names, and around 11.5 million last name X municipality of residence interactions. This provides a measure, albeit error-ridden, of the distribution of last names and F3Cs by municipality of *birth*.

The top panel of Table 4 reports the distribution of last names in Italy based on these data. As in many other countries, this distribution is highly skewed to the right (column 1), with 200,000 last names having only one occurrence, and only one last name (Rossi) having more than 130,000 occurrences. An average individual shares the same last name with around 5,000 individuals nationwide (column 3). Within municipalities of residence though, this number falls dramatically, to about 72 individuals.

As said, the INPS data do not provide information on workers' last name, but only on their F3C. Moving from last names to F3C, entails some loss of precision, as the over 500,000 last names in Italy only correspond to less than 10,000 F3Cs. Although this might seem problematic for our analysis, the F3C is still a very good predictor of one's last name within municipality. This is shown in the bottom panel of Table 4. While nationwide an average individual shares his last name with almost 79,000 individuals, within municipality of residence this number falls to around 373. This is because there are on average 3 last names for the same F3C in each place of residence, something that is shown in the last column of the table. In a separate analysis

[9] Around 45 percent of Italians reside in the municipality of birth, 75 percent in the province of birth, and 85 percent in the region of birth (authors' calculations based on the 2001 Italian Population Census).

(available upon request) we show that the relative frequencies of cells in the INPS and tax data line up remarkably well.

3. Econometric Model

3.1. Specification and identification

Having discussed the data, in this section we present the econometric model that guides our empirical analysis. Let y_{iFmt} denote labor outcomes (employment, earnings, etc.) in year t of worker i with F3C F and born in municipality m, and let P_{iFmt} be the number of individuals in office at time t who are genuinely related to individual i through family connections. Ignoring other covariates, our basic model is:

$$y_{iFmt} = \alpha + \beta P_{iFmt} + \varepsilon_{iFmt} \tag{3.1}$$

where

$$P_{iFmt} = \sum_j d_{iFmj} pol_{jt},$$

d_{iFmj} is a dummy equal to one if individual j is related to worker i, pol_{jt} is a dummy equal to one if individual j is in office at time t, ε_{iFmt} is an error term that we assume uncorrelated with P_{iFmt}, while β is the return to family connections to individuals in office, or, which is the same, the additional outcome that each politician j generates among each individual connected along family lines.

The model allows different politicians to benefit different individuals, and the same individual to benefit from multiple connections.

In order to identify β, in our baseline specification we include (103) province (effectively, live-to-work areas in Italy) of birth dummies fully interacted with (27) year dummies (p X t, with $m \in p$), and individual fixed effects, in order to account for individual time invariant heterogeneity. We also include the few individual controls available in our data, namely age and gender (where the latter is clearly identified only when individual fixed effects are not included in the model). The estimate of β is based on a differences-in-differences strategy that relies on a comparison of worker i from "family" Fm, before and after an individual in his family assumes or leaves political office, relative to individuals belonging to "families" that stay (un)connected over the same period.

As a concern remains that unobserved trends in family fortunes might simultaneously lead to movements of a family member in or out of office and an improvement or deterioration in labor market prospects of other members, hence leading a to spurious correlation between y_{iFmt} and P_{iFmt}, and hence a bias in the OLS estimates of β, we provide a number of additional checks. Using an event-study analysis, we investigate whether trends in labor market outcomes can be detected prior to a family member assuming or losing office. We also experiment with very flexible specifications where we interact individual fixed effects with linear time trends or with dummies for shorter sub-periods (2, 4, or 8 years) - hence avoiding to restrict latent trends to be the same across individuals in the same local labor market. We finally investigate heterogeneity along a number of dimensions, in terms of workers, politicians and jobs' characteristics.

3.2. Measurement error

One issue with the estimate of equation (3.1) is that we do not have precise information on the actual number of worker i's family members in office. We proxy this with the number of politicians with the same F3C F and born in the same municipality m as worker i.[10]

This implies that we only have an error-ridden measure of P_{iFmt}. Measurement error arises either because we fail to classify as connected some family members who have a different F3C or municipality of birth (type-1 error) or because we classify as connected some individuals who have the same F3C and municipality of birth but who are not family members (type-2 error). [11] In formulas our measure of the number of family members in office is:

$$\hat{P}_{Fmt} = \sum_{j} s_{iFmj} pol_{jt}$$

where s_{iFmj} is a dummy equal to one if individual j and worker i share the same F3C and are born in the same municipality. [12]

Our final regression model is:

$$y_{iFmt} = \alpha + \beta \hat{P}_{Fmt} + u_{iFmt} \quad (3.2)$$

In Appendix A.1 we show that both types of misclassification (type-1 and type-2 error) induce non-classical measurement error, and that the OLS estimate of β converges in probability to $k\beta$, where:

$$k = 1 - Pr(d_{iFmj} = 1 | s_{iFmj} = 0) - Pr(d_{iFmj} = 0 | s_{iFmj} = 1)$$

This means that $\hat{\beta}$ is bound between $-\beta$ and β. This is because, in the extreme case when all connected individuals are classified as unrelated and all unconnected individuals are classified as related, the estimates of β will be reverted.

[10] We prefer to use municipality of birth as opposed to municipality of election (for municipal elections) as this is more likely to be exogenous to the decision of where to run for office. The concern here is that individuals more likely to engage in nepotistic practices might seek office in municipalities with systematically different characteristics. We interpret our estimates as intent-to-treat estimates that are free of this potential margin of selection.

[11] This is possibly aggravated by potential misspellings of politicians' last names or misreporting of workers' tax codes.

[12] Note that $\hat{P}_{Fmt}$ does not vary by i, as we attribute the same politicians to each individual with the same m and F.

In practice, this implies that it is sufficient to rescale the OLS estimates by the factor k to correct for measurement error. In particular one can use, for each "family", the number of individuals with and without the same F3C by municipality (that we derive from the tax records in Section 2.4) and the total the number of genuinely related individuals with and without the same F3C (that we derive below based on simulations) to recover an unbiased estimate of β. We revert to this in Section 4.7.[13]

4. Model Estimates

4.1. Main estimates

In this and the following sections we present estimates of the effect of carrying the same F3C and being born in the same municipality of a politician on labor market outcomes. In Sections 4.2 and 4.3 we use a number of strategies to argue that these estimates carry a causal interpretation. We also show in Section 4.4 how these estimates vary across politicians and in Section 4.5 how they vary across workers and jobs.

Table 5 presents main estimates of β in equation (3.1). Each cell corresponds to a separate regression, while each panel refers to different dependent variables (months of work, and earnings during the year, respectively), while separate columns refer to different specifications. In particular, column (1) includes no controls, while column (2) includes F3C X municipality of birth fixed effects, plus the interaction of province of birth X year dummies in order to control for local labor market conditions. Column (3) additionally includes four age-groups dummies plus a gender dummy. Column (4) finally includes individual fixed effects. Standard errors in these and all other regressions are clustered by municipality of birth in order to allow for correlated effects both within and between "families" in the same municipality.

By and large, the inclusion of additional controls leads to point estimates that are smaller in absolute value but consistently positive and statistically significant at conventional levels. Focusing on the most saturated specification in column (4), this suggests that one politician in office increases yearly months of work of each individual with the same F3C and born in the same municipality by 0.037 months (roughly 1.2 days of work a year, a 0.37 percent increase relative to a baseline number of months of work of around 9.98) and an increase in earnings of 112 euros (a 0.57 percent increase relative to baseline earnings of around 19.500 euros).

For brevity, in the analysis we do not present regression results for the probability of employment. These are close to those obtained for the number of months of work in the year once

[13] One additional concern is that, to be included in the sample, individuals have to have at least one social security spell over the period of observation. This might lead to estimates that are affected by sample selection. The bias stems from the fact that, assuming that β is positive, individuals with a politician in office sometimes during the period of observation will have systematically lower values of the error term u_{iFmt}, inducing a negative correlation between u_{iFmt} and $\hat{P}_{iFmt}$. In Appendix A.3 we show, using both theory and data, that this effect is likely negligible.

appropriately rescaled by the factor of 10, i.e., the average number of months in work among those in the INPS data.

Taken together, this evidence suggests that most of the effects on employment come from new hires (or lower quit rates) rather than increases in months of work among those already in employment. It also appears that earnings gains are larger than employment gains (0.57 versus 0.37 percent). This suggests that either those who benefit from political connections enjoy wage premia, or that these individuals are selected among those with high earnings potential.

4.2. Separate estimates for entry and exit and event study analysis

Estimates in Table 5, columns (1) to (4), exploit both politicians' entry into and exit from office to identify impact effects and restrict these two effects to be the same in magnitude but opposite in sign. However, there should be no presumption that these two effects are the same in magnitude, and much is to be learnt from separately analyzing entry and exit episodes.

For the effect of political connections to be truly causal, one will expect the effect on entry to be positive. One will also expect this effect to manifest only upon a family member assuming office, hence ruling out pre-trends, unless entry into office is anticipated, something that we investigate in the next section.

Similarly, one will also expect this effect to last as long as a connected individual remains in office, implying a negative effect precisely upon exit. The negative effect upon exit though will be smaller than the positive effect upon entry if there is state dependence in employment or earnings - whereby a job today leads to a higher probability of employment or higher earnings tomorrow - or state dependence in political power - whereby those leaving office today transition to other, perhaps more powerful, positions.

To investigate this, we have computed in each year and for each F3C X municipality of birth, the cumulated sum of individuals assuming and leaving office since 1985, denoted respectively by $\hat{P}^{in}_{Fmt}$ and $\hat{P}^{out}_{Fmt}$, and we have included these two regressors separately in the model. Clearly, the difference between these two variables is simply the stock, the number of individuals in office at any point in time $\hat{P}_{Fmt}$, i.e., the regressor in columns (1) to (4) of Table 5.

Column (5) of Table 5 shows the estimated coefficients of these two separate variables. The results show a positive effect of connections that manifests upon entry, of a magnitude very similar to the one in column (4), and a small negative effect upon exit only statistically significant for months of work but not for earnings. Importantly, this is evidence that the effects of nepotism persist after the period when a family member is in office.

In order to add transparency to our identification assumption and to investigate pre-trends and possibly anticipation effects, we complement the regression analysis in Table 5 with an event-study analysis.

We start by focusing on entry episodes. We restrict to individuals in the INPS data who have at least one "family" member joining office between 1985 and 2011, i.e., we ignore never connected individuals. As individuals can experience multiple entries in office among those with the same F3C and municipality of birth over the period, which greatly complicates the analysis, for each individual we focus on the first entry episode in the period 1985-2011. We do so in order in to avoid that previous entry episodes might confound our estimates. The concern remains though that entry episodes before 1985 (which are not observed) or entry episodes after the first one might still affect our estimates. A related concern is that entry episodes might be correlated with exits. To deal with issue we show later that the first entry episode is a good predictor of the number of politicians in office in surrounding years.

In the model, we only include observations in a 5-year window around the event. If by t_1 we denote the time of first entry in office for "family" Fm, we estimate the following equation:

$$y_{iFmt} = \alpha + \sum_{t-t_1=-5}^{5} \beta_{t-t_1} \hat{P}^{in}_{Fmt_1} + u_{iFmt} \tag{4.1}$$

In practice we use the same model as in column (4) of Table 5, i.e., with province X year fixed effects, individual fixed effects and age controls. As we can only identify ten coefficients out of eleven, we restrict the coefficient in the year preceding the first entry episode ($t-t-1=-1$) to zero.

If our identification assumption holds, one will expect the effect at each lead ($t-t_1 = -5,...,-2$) to be equal to zero. This will rule out anticipation effects. One will also expect effects to manifest precisely at the year of entry ($t = t_1$), and possibly to increase over time as those in office acquire more power. One will finally expect the effect to decline after a number of years as individuals gradually leave office.

Estimated coefficients for yearly earnings, together with 95 percent confidence intervals, are reported in Figure 1 (a similar picture for months of work is reported in Appendix Figure A.1). A vertical line refers to the year of first entry (time t_1, i.e., at lag 0). Indeed, one can verify that, prior to entry, there is no trend in labor market outcomes. This evidence rules out that anticipation effects or spurious a correlation between a family's labor market and political fortunes. One can also see that the estimated coefficients become positive exactly at the time of year of entry, they increase over time, presumably as politicians establish themselves, and they start to decline precisely after four years. i.e., towards the end of a normal term.

As said, a concern remains that an entry episode might not be a good predictor of the actual number of family members in office in nearby years. This happens for example is a politician's entry into office is systematically associated to an exit, implying that there would be no effect on the total stock of individuals in office in the family. A related concern is that in equation (4.2) we have included, for each observation at time t, only the entry episode at time t_1, while obviously that same observation might also be influenced by entry episodes occurring at other times.

In order to address both of these concerns, in Appendix Figure A.3, we report results from a similar regression to (4.1), where now the dependent variable is the number of family members in office at time t. One can see that the first entry episode is a strong predictor of the number of family members in office in the following years, with a coefficient close to one after entry, suggesting that state dependence in political office or omission of other entry episodes are not sources of major concern. One can also see that the number of politicians in office declines precisely five years after entry, consistent with political terms being typically four years.

In Figures 2 and A.2 we examine politicians' exit from office and, similarly to entries, we run the following regression:

$$y_{iFmt} = \alpha + \sum_{t-t_N=-5}^{5} \beta_{t-t_N} \hat{P}^{out}_{Fmt_N} + u_{iFmt}, \tag{4.2}$$

where with t_N we denote the time of last exit from office for "family" Fm. Again, we focus on the last exit episode in order to limit the possibility that subsequent exits might confound our estimates.

Similar to what found for entry, we find evidence of this exit episode being a clear and significant predictor of the number of politicians in office in surrounding years (see Appendix Figure A.4). However, differently from what found for entries , there is evidence of a deterioration in outcomes predating the time of exit, which continues after the time of exit itself. This also explains the small and insignificant effect upon exit in column (5) of Table 5. We take this evidence to suggests that exist are somewhat anticipated (which is reasonable given the normal length of a term) and that perhaps they capture some declining trends in family fortunes. This in turn suggests that exit episodes might not be as exogenous to the outcome variable as entry episodes and one should exert some caution in interpreting estimates on exit as truly causal.

4.3. Robustness checks

Before proceeding further, in this section we discuss briefly a number of robustness checks aimed at corroborating the analysis. We revert to a specification were we constrain the effect of entry and exit to be the same but of opposite sign, and we focus on the most saturated specification, as in column (4) of Table 5.

In column (1) of Table 6 we restrict to workers ever connected, i.e., with at least one family member in office during the twenty-seven years period. Identification is based on differential timing of entry or exit into and from office across groups. By restricting to those ever connected, one hopes to limit the concern that those connected have different latent trends in labor market status from those unconnected that might happen to be correlated with their families' political fortunes. This sample selection criterion reduces the sample by almost 50 percent but results are very similar to those found in Table 5.

Results also remain virtually unchanged once we include the interactions of individual fixed effects with a linear time trend in column (2) that capture gradients in employment and earnings across individuals that might be correlated with the number of family members in office. In columns (3) to (5) we include respectively dummies for 8, 4 and 2 years sub-periods interacted with individual fixed effects. Effectively, we allow individuals to have differential non-parametric trends in local labor market outcomes. Note however that identification relies on increasingly close observations around the time of entry or exit of a family member into/from office, hence leading to less precise estimates. Point estimates fall in magnitude compared to those in Table 5, but remain positive and statistically significant at conventional levels. Results (not reported) also show that our estimates are insensitive to the start years used to mark the beginning of each 8-years, 4-years and 2-years time interval.

We have also performed a number of additional robustness checks (not reported but available upon request). First, we show that coefficients remain statistically significant at conventional levels if we cluster standard errors at the level of province as opposed to city of birth. Estimates also remain unchanged if we include among the regressors the interaction between province of residence (in addition to province of work) and year fixed effects, or if we further include in the model the interaction between F3C and year fixed effects.[14]

4.4. Politicians engaging in nepotism

Having ascertained that our estimates are robust to the specification used, this section explores to the differential effect of political connections based on the characteristics of politicians and the office they hold. If the effects we find are genuinely due to nepotistic practices, one will expect these to be stronger the greater the rents accruing to a politician's office.

We present regression estimates in Table 8 where, again, we revert to the specification in Table 5, column (4). Columns (1) to (3) report respectively separate estimates on the number of family members in office in council and executive positions, on the number of politicians by number of consecutive terms in the same office (1 term, 2 terms or more) (including the number of individuals in office in 1985 to control for the left censored nature of the data) and on the number of politicians at different levels of government (municipal, provincial, regional and national).

The table illustrates that more powerful politicians tend to generate higher labor market returns among their family members. Column (1) shows that those in the executive positions (whether commissioners in municipal provincial or regional governments, or ministers in the central government, or heads of the executive) generate returns that are around 30 to 50 percent higher than those in council positions. We take this as evidence of those in the executive having

[14] We have also estimated regression coefficients from a model where we include (8,110) municipality of birth X (27) year fixed effects. This allows us to control for the state of the labor market at a very localized level. By including municipality (as opposed to province) of birth X year fixed effects, though, our control group includes individuals with a different F3C in any given municipality (as opposed to any given province). This exacerbates type-1 error (see equation A.1 in the appendix). Indeed, the inclusion of municipality of birth X year fixed effects reduces the point estimates sensibly (a 5-fold reduction for earnings and 30-fold reduction for months of work) although the effects remain positive and typically significant.

more direct control over resources and hence being able to appropriate larger rents.

Column (2) shows that the returns to those connected to individuals in office for 2 terms or more are around three times those found among those connected to politicians in office only in one term. This is evidence of the returns increasing with tenure, although it is possible that those with longer tenure are more powerful or able politicians, including those more able to appropriate rents for themselves and their families (Coviello and Gagliarducci 2013).

A similar positive gradient is found among politicians at higher levels of government (e.g. regional) compared to those at lower levels (e.g. municipal), at least as long as earnings are concerned, although results other than for municipal politicians are typically imprecise, which is unsurprising given that most politicians are municipal ones.

In sum, there is clear evidence of the effects of connections displaying a positive gradient in politicians' clout, which lends further support to our interpretation of the coefficients as measuring rent extraction on the part of politicians.

4.5. Jobs created and workers benefitting from nepotism

Having characterized the type of politicians more prone to engage in nepotistic practices, in this section we finally focus on the characteristics of jobs created and the individuals who benefit from them. Table 7 explores the differential effect of political connections by jobs and workers' characteristics. In columns (1) to (3) we investigate the type of jobs accruing to politicians' family members. We run separate regressions for earnings and the number of months of work and by specific occupation (blue collar, white collar and manager). [15]

There is evidence that the effects decline the higher the occupation: for example political connections are responsible for an additional 44 euros worth of blue collar earnings and 0.029 months of blue collar employment. The same figures for managers are 27 euros and 0.002 months of work. Effects, however, are *proportionally* higher the higher the occupation. As shown in the Table, an average individual in the INPS sample for example makes 4,804 euros of blue-collars' earnings and only 453 euros worth of managers' earnings. These results suggest that jobs created by politicians are disproportionately high-paying jobs. This is likely to explain why we find effects on earnings that are proportionally higher than for months of work (see Table 5).

Unfortunately, with the data at hand there is no way of ascertaining whether these jobs get dispensed to workers with higher than average skills, or whether those acquiring jobs through political connections enjoy wage premia. There is evidence from other papers though that informal connections are particular valuable, and hence more frequently used, among those with poorer labor market prospects and lowers skills (Pistaferri 1999, Kramarz and Skans 2014).

We also investigate whether any heterogeneity exists by age (effectively the only individual

[15] Note that coefficients do not add precisely to those in column (4) of Table 5 due to a small number of observations with missing occupation.

characteristic that is available in our data). Using the same specification as in column (4) of Table 5, in column (4) we interact the effect of the number of total politicians in office by F3C and municipality of birth with workers' age. Estimated effects are positive for younger individuals and they tend to decline with age. Interestingly, the estimated effect on earnings is negative for individuals 55 or older, on the order of -226 (-298+72) euros. Possibly this is due to earlier transitions to retirement, or to transitions to other sectors (the public sector or even political careers) as a result of political connections.

A concern remains that some firms in the INPS data, even if belonging to the private sector, are publicly owned. Most of these firms are owned by municipalities, typically operating as providers in the utilities and transport sectors.[16] Although this would not invalidate our estimates above, the concern remains that a positive coefficient is driven by the direct control that politician, and in particular municipal politicians, exert over firms' hiring. Although, as said, we have very little information on firms' characteristics (as their identity is concealed in the data), we have, information on the firm's sector of activity, although only at one digit level (9 sectors). For ease of reading, in Figures A.5 and A.6 we report the *proportional* increase in yearly earnings and months of employment due to nepotism, by sector. One can see that effects are similar across sectors (on average 0.1 percent) and there is no evidence that these effects are larger in the energy sector. If anything, it appears that the effects are more pronounced in manufacturing.

In sum, it appears that politicians allow preferential access to jobs that are better than the average job, and that younger workers are those who most benefit from these connections. The effect is roughly uniform across sectors, implying that this phenomenon is pervasive in the economy.

4.6. Local budget and nepotism

Having ascertained that nepotism is a pervasive phenomenon in Italy and that it varies as a function of a politicians' clout, in this section we provide a tentative estimate of the fraction of public resources that are siphoned by politicians through nepotistic practices. In particular, we exploit the heterogeneity in the estimated effects and the public budget across municipalities to identify this parameter.

In order to perform this exercise, we follow a two-step procedure. We start by estimating a separate parameter β_m in equation (4.2) for each municipality.[17]

[16] Traditionally, state controlled companies operated largely in the transport and communication sectors (e.g., railways, telecoms), finance (banking) and in specific manufacturing (e.g., steel and airplanes) and energy (e.g., oil) sectors of strategic relevance. Over the course of the 1980s and early 1990s, most of the ownership of these firms though changed from being public to private.

[17] In formulas, we estimate the following model: $y_{iFmt} = \alpha + \beta_m \hat{P}_{Fmt} + u_{iFmt}$. As the model includes province X year effects, in order to ease the computational burden, we run separate regressions for each of the 103 Italian provinces, where we include year fixed effects (plus all other controls, namely individual fixed effects and age group dummies) and we interact the variable $\hat{P}_{mFt}$ with a dummy for each municipality m. Again, we cluster standard errors by municipality of birth.

In a second step we regress these municipality-specific measures of nepotism on municipality-level variables with weights equal to the reciprocal of the square of the standard error of each coefficient, in the spirit of a minimum distance estimator (see for example Card and Lemieux 2001 for an application). In formulas, we estimate the following model:

$$\hat{\beta}_m = \delta_0 + \delta_1 Z_m + v_m \tag{4.3}$$

where Z_m denotes is a measure of public resources per politicians. As we exploit cross-municipality variation in local budget, we restrict to the effect of municipal politicians only. However, we have shown above that most of the effects of nepotism are indeed ascribable to municipal politicians.[18]

In order measure the resources available to those in office we use public expenditure per politician (in logs). We restrict to a measure of discretionary expenditure, defined as total expenditure net of debt service and personnel, as this is easier and hence more likely to be used to foster nepotistic practices. We have also experimented with other measures of the local budget (total expenditure and total revenues per politicians). Results (not reported) are qualitatively similar but they are less precise.

Column (1) of Table 9 presents estimates of equation (4.3) with no additional controls. These and all other regressions are restricted to the municipalities with non-missing values of all included regressors. It appears that a 10 percent increase in resources per politician leads to an increase in the estimated coefficient of 19 euros (=190.078 X 0.1) for earnings and 0.06 (=0.064 X 0.1) for months of work, roughly a 20 percent increase compared to the main estimates in Table 5.[19]

As, clearly, the amount of discretionary spending is not randomly allocated across municipalities and some determinants of spending might exist that are correlated with the amount of nepotism, we include in the model a large number of observable municipality-level characteristics.[20]

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[18] Out of the 8,100 municipalities in the country, we are able to identify separate coefficients for 7,182 of them. This is because, for the residual 918 municipalities, not enough observations are available to identify a municipality-specific coefficient. As a robustness check we have run the pooled regression in Table 5, column (4) on this restricted sample of 7,182 municipalities. Results (not reported but available upon request) are remarkably similar to those obtained for the entire sample.

[19] Note that, for politicians, we use the characteristics of the municipality of birth (as opposed to the one of election). Not only does his greatly simplify the empirical analysis, but it also has the advantage of circumventing the potential non-random allocation of those in office across districts. The concern here is that those with stronger propensity to engage in nepotistic practices might seek office in areas where the return to this activity is higher or where discretionary spending is higher. In this sense, our estimates can be interpreted as intent-to treat estimates of the effect of local resources on the incidence of nepotism. Separate regressions show that discretionary expenditure in the municipality of birth is a strong predictor of discretionary expenditure in the municipality of election, with a coefficient in a log-log regression of 0.58. This is consistent with the fact that around 50 percent of local politicians serve in their city of birth.

[20] These are measures of local economic conditions and of the state of the labor market, namely log income per capita, the log number of firms per capita, the fraction of workers in the public sector and the local unemployment rate. We also control for log total number of politicians, log local population, the fraction of population with a college degree, the fraction of the population that is past working age, plus dummies for

If anything, the inclusion of municipality-level controls leads to estimates, in column (2), that are larger than estimates with no controls in column (1) by between 30 and 42 percent, respectively for earnings and months of work. This suggests that, if anything, determinants of greater discretionary spending are associated to lower nepotism and that the estimates in column (1) are conservative. Finally we include in the model province fixed effects. Identification is across municipalities with the same characteristics within each of the 103 provinces. Once more, point estimates increase when we include these additional controls.

In sum, these results confirm that greater resources under the control of politicians happen to lead to grater incidence of nepotism. In the following section we revert to these estimates in order to derive back of the envelope calculations on the amount of discretionary spending appropriated by politicians through nepotistic practices.

4.7. Assessing the extent and consequences of measurement error

Having ascertained that the estimates of the parameter of interest are causal, in the sense that they present causal estimates of the effect of being born in the same municipality and carrying the same F3C as one individual in office and having presented heterogeneity in effects across a number of dimensions, we now present calculations on the returns of genuine effect of being related to a politician via family ties. These calculations are tentative, as they are based on a number of assumptions. Still, they should allow us to recover a sense of the magnitudes involved.

Equation A.1 illustrates that given that type-1 error is overall negligible, in order to correct our estimates of measurement error we simply need to rescale our estimated coefficients by the ratio of the number of truly related individuals with the same F3C and the total number of individuals with the same F3C, whether related on unrelated.

For each family we can derive an estimate of the denominator of k using the tax data (see equation A.1).

In order to derive an estimate of the numerator, we have simulated the average number of working age relatives for a middle aged individual (i.e., somebody of parenting age but too young to have grandchildren and too old to have working age parents) (see Appendix A.2). Under a range of plausible assumptions about the number of children born by generation and the rate of geographical mobility, we predict an average number of close consanguineal relatives (children, siblings, nephews and nieces, first cousins and their children - i.e., the ones who

whether a municipality is a region or province capital. We control for potential deterrence effects by including dummies for whether them municipality has a police station (separately for the three police forces in Italy, *Carabinieri*, State Police and *Guardia di Finanza*) and for whether this is a site of a judicial court. Finally, in order to proxy for different level of social capital across areas (see Guiso et al 2004) we include a measure of turnout in local elections, log number of non-profit associations per capita, and a dummy for whether the municipal administration was ever dissolved for Mafia (see Acconcia et al 2014), which proxies for the presence of organized crime. Precise sources and definitions together with descriptive statistics are presented in Appendix A.4 and Table A.1.

are possibly more likely to benefit from political connections) carrying one's last name and born in the same municipality on the order of 2.5. We also estimate a total number of close consanguineal relatives, whether carrying one's last name or not, born in the same municipality of around 6.

Once we average this ratio across all individuals in the INPS data, this gives an estimate for the implied bias (k) of 0.16.[21]

Reverting to our main estimates in Table 5 (but a similar rescaling can be applied to the other tables), this suggests a rise in earnings associated to one extra family member in office on the order of 700 euros per year ($112/0.16$) (i.e., 3.5 percent based on average earnings of 19,500 euros) and a rise in the number of months of work of around 0.23 ($0.037/0.16$, a 2.5 percent increase relative to a baseline or around 10 months).

We can also attempt to derive estimates of the overall number of private sector jobs and the wage bill ascribable to nepotism. Given the above estimates, and assuming that all close consanguineal family members - whether carrying the last name or not - born in one's municipality benefit from these connections, this gives an estimate of the return to holding political office on the order of 4,200 (700 X 6) euros and 1.3 months of work per year.

These estimates are likely to be conservative as they exclude affinal relatives, i.e., spouses of consanguineal relatives as well as consanguineal and affinal relatives of one's spouse born in the same municipality (as well as close relatives born elsewhere, that we ignore throughout). If these individuals also benefit from political connections, the numbers above will need to be multiplied by a factor of 4, implying a yearly return to holding office as high as 16,800 euros, i.e., approximately the monetary return to 0.85 jobs, and 5.2 months of work.

As there are approximately 137,000 individuals in office per year, and each politician generates between 0.13 and 0.52 jobs,[22], this implies that at least 18,000 and as many as 71,000 jobs per year can be ascribed to political nepotism along family lines. This is between 0.18 and 0.67 percent of private sector employment in INPS (around 10 million workers). Similar figures for earnings imply that nepotism accounts for between 0.30 and 1.18 of the total wage bill in INPS.

We can also use the numbers in the previous section to derive a tentative estimate of the amount of resources that politicians divert through nepotistic practices. Using the most conservative estimate of the effect of discretionary spending on nepotism in Table 9, we calculate that

[21] Although one could simply rescale the number of politicians in each family by the relevant factor k for each family to obtain an unbiased estimates of β, we prefer not to do so as both the numerator and the denominator of k are measured with error. Recall in particular that the tax data only provide the distribution of last names by municipality of residence rather than the municipality of birth. This implies that we can only derive an imperfect measure of the actual number of individuals in one's municipality of birth with the same F3C. An additional complication is that we only have information on the universe of taxpayers. These include a large number of individuals out of the labor force, importantly pensioners. We hence correct the denominator by a factor 0.64, which is the fraction of total labor force over the number of taxpayers. Estimates of the labor force are on the order of 25 million - see Istat (2010) - compared to a number of taxpayers of around 39 million. For this reason we prefer to compute an average k across all families.

[22] This is between 1.3 and 5.2 months of work divided by 10, which is the average number of months of work during the year

politicians appropriate at least 4 percent of discretionary spending through these practices.[23]

5. Discussion and Conclusions

In this paper we estimate the effect of family connections to public officials on private labor market outcomes in Italy. Although there is plenty of anecdotal evidence on practices of favoritism in hiring and promotion of public officials' relatives, credible evidence is by and large missing. This is because identifying individuals who are related to those in office is challenging in large enough data samples that are required to identify these effects with reasonable precision.

We circumvent this problem by exploiting a unique piece of information that is available in our data on individuals' municipality of birth and a substring of three characters that compose their last name (F3C), forming part of a worker's tax code. Importantly, although we cannot precisely identify individuals in the data, as we have no precise information on their last names - let aside on their degree of relatedness to politicians - the data allow us to match politicians and workers based on similar last names and place of birth. We show that this method has the potential to identify relatively small groups of individuals, hence offering some promise about its ability to identify, albeit imprecisely, family connections, We discuss at length the consequences of measurement error induced by misclassification and we provide a simple method to correct for it.

Based on this method, our estimates imply that the average individual monetary return to political connections is on the order of 3.5 percent per year. Assuming that all close consanguineal family members born in one's municipality benefit from these connections, we estimate a return to holding political office on the order of 4,200 euros and 1.3 months of work per year, an effect that is four-fold when we also include affinal relatives. Back of the envelope calculations suggests that jobs acquired through nepotism account for at least 0.2 to 0.7 percent of private sector employment in Italy and between 0.3 and 1.2 of the total private sector wage bill and that politicians appropriate at least 4 percent of the local discretionary budget to fund this practice.

We take the evidence in the paper that the estimated effect increases with a politician's clout and with the resources accruing to the administration where he serves to indicate that nepotism is a technology of rent appropriation that helps politicians monetize over their position of power

[23] To get to this number, we use the estimate in column (1), row (1) of Table 9. This implies that a 10 percent increase in discretionary expenditure per politician, around 30,000 euros, leads to an increase in earnings among those with the same F3C and born in the same municipality of around 19 euros (190.078 X 0.1). Correcting for measurement error, this gives an increase of about 119 euros per family member (19 /0.16). Assuming that at least 6 family members benefit from this practice, this is 712 euros worth of private sector earnings. Correcting for the fact that these are ITT estimates (see footnote 19), this gives an effect of 1,228 euros. These estimates imply an effect of around 4 percent of the discretionary budget 1,228/30,000. Results can be significantly larger if one is willing to assume that politicians benefit more than four family members or if one accepts the less conservative estimates in columns (2) and (3) of Table 9.

and a wider measure of the returns to holding office.

Although the available data do not allow us to investigate this directly, we speculate that nepotism is the result of an exchange between firms and politicians (as in Shleifer and Vishny 1994). The question though remains of why politicians resort to the triangulation of private firms to monetize over their position of power. Although direct appropriation of resources is in theory always an option, this is clearly risky. In addition, many of the rents associated to public office stem from the monopoly that officials exert over administrative decisions that affect the utility of other agents in the economy. This implies that, in order to monetize over these rents, officials will need to collude with these agents.

Some related considerations apply to firms. Although payment of bribes in exchange for favors is always an option, this is criminally sanctioned. In addition, to the extent that imperfections exist in the labor market, firms might have an incentive to hire politicians' relatives rather than paying bribes and appropriate part of the rents that stem from such imperfections.

We also speculate that nepotism is a - potentially inferior - substitute for sheer corruption: when corruption and grafting are costly due to high rates of detection, politicians exchange favors with firms in order to monetize over their position of power, although this possibly comes at the cost some rent dissipation or rent sharing with firms. This is reminiscent of Olken (2007), who shows that increased corruption monitoring in Indonesia leads to lower corruption but higher nepotistic hiring in publicly funded projects. Again, although our data do not allow us to provide definite evidence on this, we observe that nepotism is more widespread in areas where corruption is more widespread.[24]

Others might object that firms unilaterally - as opposed to a result of an exchange - decide to hire politicians' relatives, in hope or expectation of deriving a private utility from it, perhaps because these gives access to information that is not publicly available and is valuable to the firm. Appointment or election of a family member to office might also serve as an information revelation mechanism about a family's - and hence a worker's - quality. Although we cannot definitely rule out these alternative explanations, it still remains true that our estimates measure the wider returns to holding office and hence that individuals might be seeking office in expectation of such returns.

Our estimates clearly only refer to nepotism along family lines and exclude other forms of interference with the hiring decisions of private firms on the part of public officials through favoring of "friends" or other associates, including political associates. Also, given the nature of the data, we are unable to measure nepotistic hiring in the public sector or other benefits that accrue to public officials' family members in self-employment. In this sense, our estimates are likely to provide a lower bound for the true effect of nepotism on the labor market and they obviously ignore other margins of inefficiency due to the diversion of resources that are likely

[24] To come to this conclusion we have examined the coefficient on corruption crime rates in model 4.3. The coefficient is consistently negative across specifications and statistically significant at conventional levels. Clearly, one has to be cautious in attaching a causal interpretation to this coefficient.

to result from such practices.

Table 1: Descriptive statistics, workers - employment spells

	Mean	s.d.
Months of work in the year	9.985	3.377
Yearly earnings	19,521.181	16,502.090
Number of jobs in the year	1.188	0.500
Female	0.327	0.469
Age	37.395	11.115
Area of birth: North	0.461	0.499
Area of birth: Center	0.173	0.378
Area of birth: South + Islands	0.366	0.482
Area of residence: North	0.545	0.498
Area of residence: Center	0.194	0.395
Area of residence: South + Islands	0.261	0.439
City of residence same as birth	0.402	0.490
Province of residence same as birth	0.735	0.441
Region of residence same as birth	0.817	0.387
Area of work: North	0.556	0.497
Area of work: Center	0.196	0.397
Area of work: South + Islands	0.248	0.432
Province of work same as birth	0.681	0.466
Region of work same as birth	0.788	0.409
Blue collar	0.639	0.480
White collar	0.349	0.477
Executive	0.011	0.102
N. observations	9,467,981	
N. individuals	925,211	

Notes. Each observation in the table is one year X individual, and the sample refers to observations with non-zero earnings. Job characteristics refer to the most highly paying job in the year. Categories of variables might not add up to one due to missing values. Yearly earnings are expressed in 2005 euros. Source: INPS data.

Table 2: Descriptive statistics, politicians

	Mean	s.d.
Municipal	0.961	0.194
Provincial	0.024	0.154
Regional	0.008	0.090
National	0.007	0.080
Council	0.699	0.459
Executive	0.301	0.459
1 Term	0.702	0.457
2 Terms	0.209	0.407
$>$ 2 Terms	0.089	0.284
In office in 1985	0.293	0.455
Female	0.138	0.345
Age	44.389	11.267
Primary	0.099	0.299
Junior High	0.241	0.428
High School	0.411	0.492
College	0.247	0.431
Blue collar	0.159	0.366
White Collar	0.338	0.473
Manager	0.080	0.271
Military/Police	0.006	0.078
Physician	0.053	0.225
Professor/Teacher	0.066	0.248
Lawyer/Judge	0.023	0.149
Other occupation	0.060	0.238
Area of birth: North	0.548	0.498
Area of birth: Center	0.137	0.344
Area of birth: South + Islands	0.316	0.465
Area of election: North	0.572	0.495
Area of election: Center	0.137	0.343
Area of election: South + Islands	0.291	0.454
Munic. of election same as birth	0.485	0.500
Province of election same as birth	0.845	0.362
Region of election same as birth	0.917	0.276
N. observations	3,714,808	
N. individuals	525,500	

Notes. Each observation in the table is one year X government X individual. Data are weighted by fraction of year in office. Categories of variables might not add up to one due to missing values. Municipality of office only available for municipal politicians. Province of office only available for municipal and provincial politicians. Region of office only available for municipal, provincial and regional politicians. Source: Ministry of Interior Affairs.

Table 3: Descriptive statistics, matched sample

	Mean	s.d.
Months in work in the year	4.841	5.516
Employed	0.485	0.500
Yearly earnings	9,292.581	14,591.780
Total politicians	0.411	0.966
Total politician >0	0.264	0.441
Total politicians = 1	0.170	0.376
Total politicians = 2	0.054	0.227
Total politicians >2	0.039	0.195
Municipal politicians	0.388	0.931
Provincial politicians	0.013	0.114
Regional politicians	0.005	0.070
National politicians	0.004	0.063
Council politicians	0.297	0.746
Executive politicians	0.114	0.394
N. observations	17,117,062	
N. individuals	806,085	

Notes. Each observation in the table is one year X individual. The data include both employment and non-employment spells. Workers and politicians matched on F3C and municipality of birth. See also notes to Tables 1 and 2.

Table 4: Distribution of last names and F3Cs, individual tax records 2005

	(1)	(2)	(3)	(4)	(5)
Occurrences	Frequency of occurrence	Pop. share	Pop. w/same F3C	Pop. w/same F3C and municipality of residence	Number of last names by F3C and municipality of residence
			Last names		
All	525,054	1.000	5,112	72	
More than 1	320,221	0.995	5,139	72	
More than 5	192,449	0.985	5,190	73	
More than 100	50,007	0.887	5,758	80	
More than 100,000	1	0.003	133,812	755	
			F3C		
All	9,496	1.000	78,562	373	3
More than 1	8,878	1.000	78,563	373	3
More than 5	7,860	1.000	78,570	373	3
More than 100	4,503	0.997	78,801	374	3
More than 100,000	67	0.281	188,427	813	7
More than 300,000	4	0.033	325,703	1,211	10

Notes. The top panel of the table reports the distribution of last names in Italy by nationwide occurrence based on 2005 tax data. Column (1) reports the frequency of last names. Column (2) the associated population shares. Column (3) the number of individuals with the same last name in the entire country and column (4) the number of individuals with the same last name and municipality of residence. The bottom panel reports similar statistics to those in the top panel for the first three consonants of last names (F3C). Column (5) additionally reports the number of last names by F3C and municipality of residence. See also Section 2.4.

Table 5: Main estimates

	(1)	(2)	(3)	(4)	(5)
	Dep. variable: Yearly earnings				
Politicians	378.638***	205.229***	114.637***	111.634***	
	(63.725)	(14.883)	(10.828)	(12.170)	
Politicians in					116.744***
					(12.561)
Politicians out					-9.590
					(11.482)
	Dep. variable: Months of work in the year				
Politicians	0.095***	0.117***	0.054***	0.037***	
	(0.021)	(0.007)	(0.004)	(0.004)	
Politicians in					0.039***
					(0.005)
Politicians out					-0.009**
					(0.004)
Munic. birth X F3C FE		Yes	Yes	Yes	Yes
Prov. birth X year FE		Yes	Yes	Yes	Yes
Indiv. controls			Yes	Yes	Yes
Indiv. FE				Yes	Yes

Notes. Columns (1) to (4) of the table report the coefficients on the number of individuals in office (in any level of government and in any office) with the same F3C and municipality of birth. Individual controls include age-group dummies (in ten year bands) and a female dummy. Column (5) reports separate estimates for politicians' entry into and exit from office. Standard errors clustered by municipality of birth in brackets. Number of observations 17,117,062. ***, **, *: denote significant at 1, 5 and 10 percent level , respectively.

Table 6: Robustness checks

	(1)	(2)	(3)	(4)	(5)
	Ever connected only	Add individual linear trends	Add 8-yrs FE	Add 4-yrs FE	Add 2-yrs FE
	Dep. variable: Yearly earnings				
Politicians	87.599***	110.320***	57.653***	38.625***	30.586****
	(11.015)	(12.420)	(8.157)	(6.460)	(7.733)
	Dep. variable: Months of work in the year				
Politicians	0.031***	0.037***	0.029***	0.020***	0.011***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004))

Notes. The table reports regressions similar to those in column (4) of Table 5. Column (1) excludes families never connected to a politician. Column (2) includes linear time trends interacted with individual fixed effects. Columns (3) to (5) include respectively 8-years (1985-1992, 1993-2010, etc.), 4-years (1985-1989, 1990-1994, etc.), and 2-years (1985-1986, 1987-1988 etc.) dummies interacted with individual fixed effects. Column (6) adds municipality of birth X year fixed effects. Number of observations: 9,291,102; 17,117,062; 17,061,005; 17,002,333; 16,222,848 respectively in columns (1) to (5). See also notes to Table 5.

Table 7: Heterogeneous effects by workers' characteristics

	(1)	(2)	(3)	(4)
	By occupation			By age
	Blue collar	White collar	Manager	
	Dep. variable: Yearly earnings			
Politicians	44.499***	36.615***	26.760***	72.007***
	(6.461)	(8.011)	(7.220)	(26.119)
Politicians X age 26-35				123.223***
				(30.880)
Politicians X age 36-45				86.130*
				(45.397)
Politicians X age 46-55				71.175
				(44.520)
Politicians X age 56-65				-297.789***
				(61.825)
Avg dep. variable.	4,804	4,014	453	
	Dep. variable: Months of work in the year			
Politicians	0.029***	0.006**	0.002**	0.097***
	(0.004)	(0.003)	(0.001)	(0.011)
Politicians X age 26-35				-0.012
				(0.015)
Politicians X age 36-45				-0.082***
				(0.014)
Politicians X age 46-55				-0.091***
				(0.014)
Politicians X age 56-65				-0.109***
				(0.016)
Avg.	3.11	1.67	0.05	

Notes. The table reports regressions similar to those in column (4) of Table 5 where the effects are allowed to vary by workers' characteristics. *Avg.* is the mean value of the dependent variable for each occupation. See also notes to Table 5.

Table 8: Heterogeneous effects by politicians' characteristics

	(1)	(2)	(3)
	By level of office	By tenure	By level of government
	Dep. variable: Yearly earnings		
Council	98.026***		
	(12.553)		
Head + Executive	149.881***		
	(23.794)		
1 Term		104.581***	
		(11.395)	
2 Terms		180.809***	
		(21.603)	
> 2 Terms		347.698***	
		(41.929)	
Municipal			114.183***
			(12.560)
Provincial			61.252
			(50.560)
Regional			188.085**
			(92.365)
National			-70.545
			(123.679)
	Dep. variable: Months of work in the year		
Council	0.034***		
	(0.005)		
Head + Executive	0.045***		
	(0.008)		
1 Term		0.038***	
		(0.004)	
2 Terms		0.054***	
		(0.007)	
> 2 Terms		0.083***	
		(0.014)	
Municipal			0.037***
			(0.005)
Provincial			0.044***
			(0.017)
Regional			-0.005
			(0.032)
National			0.047
			(0.046)

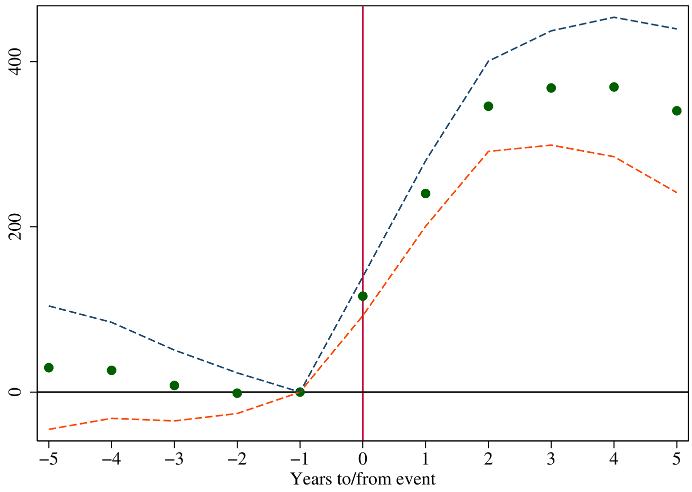
Notes. The table reports regressions similar to those in column (4) of Table 5 where the effects are allowed to vary by politicians' characteristics. Regressions in column (2) additionally include the number of left censored observations (individuals in office in 1985) by F3C and municipality. See also notes to Table 5.

Table 9: Discretionary spending and nepotism

	(1)	(2)	(3)
	Dep. variable: Yearly earnings		
Discretionary spending per politician (log)	190.078***	247.962***	331.759***
	(58.550)	(67.610)	(78.663)
	Dep. variable: Months of work in the year		
Discretionary spending per politician (log)	0.064**	0.091***	0.140***
	(0.027)	(0.031)	(0.036)
Additional controls	No	Yes	Yes
Province FE	No	No	Yes

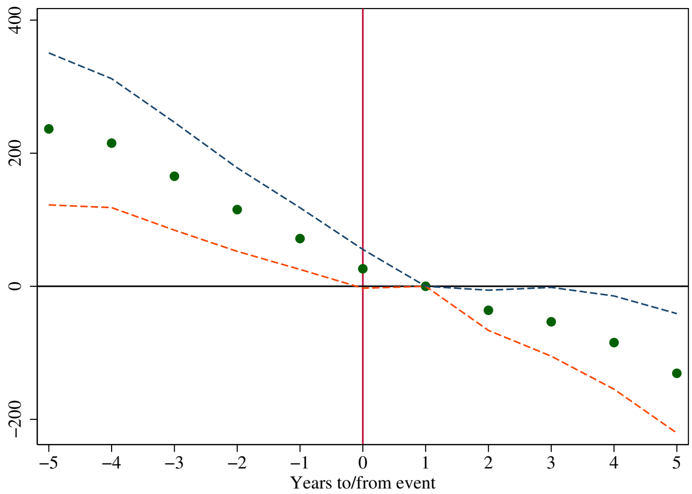
Notes. The table reports minimum distance estimates of the effect of log discretionary spending per politician on the extent of nepotism by municipality. Regressions refer to municipal politicians only. Method of estimation: GLS, with weights equal to the square of the reciprocal of the standard error associated to each coefficient. Additional controls include: log income per capita, log number of firms per capita, fraction of workers in the public sector, local unemployment rate, log total number of politicians, log local population, fraction of the population with a college degree, fraction of the population that is past working age, dummies for whether a municipality is a region or province capital, for whether it has a police station (separately for the three police forces in Italy, *Carabinieri*, State Police and *Guardia di Finanza*) and for whether this is the site of a judicial court, turnout in local elections, log number of non-profit associations per capita and a dummy for whether the municipal administration was ever dissolved for Mafia. See also text for details.

Figure 1: Event-study analysis: Yearly earnings - entry



Notes. The figure displays estimated yearly earnings at different lags and leads since the time of first entry (denoted by a vertical line). All coefficients expressed relative to effect in year before entry. 95 percent confidence intervals reported. See text for detail.

Figure 2: Event-study analysis: Yearly earnings - exit



Notes. The figure displays estimated yearly earnings at different lags and leads since the time of last exit (denoted by a vertical line). All coefficients expressed relative to effect in year after exit. 95 percent confidence intervals reported. See text for detail.

A. Appendix

A.1. Measurement error

As said, in the data we only have an imperfect measure of political connections. This provides an error-ridden measure of P_{iFmt}. In particular, we can only identify politicians carrying the same F3C and born in the same municipality as a worker. In formulas, we only observe:

$$\hat{P}_{Fmt} = \sum_j s_{iFmj} pol_{jt}$$

where s_{iFmj} is a dummy equal to one if individuals i and j have the same F3C F and are born in the same municipality m. It follows that:

$$\hat{P}_{Fmt} = P_{iFmt} + \nu_{iFmt}$$

where

$$\nu_{iFmt} = \sum_j (s_{iFmj} - d_{iFmj}) pol_{jt}$$

and d_{iFmj} is a dummy equal to one if individual i with F3C F is a family member of individual j. It follows that our empirical model is:

$$y_{iFmt} = \alpha + \beta \hat{P}_{Fmt} + u_{iFmt}$$

where $u_{iFmt} = \varepsilon_{iFmt} - \beta \nu_{iFmt}$.

From the above one can derive the implied bias in the OLS estimate of β. Assuming that s_{iFmj} and d_{iFmj} are independent across j's, this estimate converges in probability to βk, where:

$$k = 1 - \frac{Cov(\hat{P}, \nu)}{Var(\hat{P})} = 1 - \frac{Cov(s, s-d)}{Var(s)} = \frac{Cov(s,d)}{Var(s)}$$

Since:

$$Cov(s,d) = Pr(s=1, d=1) - Pr(s=1)Pr(d=1) =$$
$$[Pr(d=1|s=1) - Pr(d=1|s=0)]Pr(s=0)Pr(s=1)$$

and

$$Var(s) = Pr(s=0)Pr(s=1)$$

it follows that:

$$k = 1 - Pr(d=1|s=0) - Pr(d=0|s=1)$$

At given $Pr(s=1)$ and $Pr(d=1)$, k is lower the higher are both type-1, $Pr(s=0|d=1)$, and type-2, $Pr(s=1|d=0)$, errors. Since k varies between -1 and 1, estimates of β are bound between $-\beta$ and β. The intuition for this is straightforward. Type-1 error and type-2 errors imply respectively that connected individuals are erroneously assigned to the control group, and unconnected individuals are assigned to the treatment group, both diluting the estimate of β. In the extreme case when all connected individuals are assigned to the control group and all unconnected individuals are assigned to the treatment group, the estimates of β will be reverted.

The size of both errors will depend on the distribution of F3Cs in a municipality and there is a clear tradeoff between the two. To see this, consider the simple case where the number of genuinely related individuals ($d = 1$) with and without F3C F born in the same municipality m is the same across households (respectively D and $\overline{D}$). In this case:

$$k = 1 - E\left(\frac{\overline{D}}{N_{\overline{F}m}}\right) - E\left(\frac{N_{Fm} - D}{N_{Fm}}\right) \tag{A.1}$$

where N_{Fm} and $N_{\overline{F}m}$ are respectively the number of individuals with and without F3C F in municipality m. One can hence use the simulated number of relatives with the same and with a different F3C born in the same municipality (see Appendix A.2), and the total number of individuals by F3C and municipality (see Section 2.4), to derive an estimate of k.[25]

A.2. Number of family members in the same municipality and with the same F3C: simulations

In this appendix we derive a range of estimates for the number of working age family members with and without the same last name (and hence presumably the same F3C as it is unlikely that family members with different last names have the same F3C) born in the same municipality, D and $\overline{D}$, under a variety of assumptions about the number of children across generations and geographical mobility. We assume that women carry their father's last name (rather than their husband's last name), as the Italian tax code is calculated based on last name at birth, and by and large female politicians use the last name at birth. We focus on a middle-age politician, somebody whose children, siblings, n-th cousins and all their children are of working age, while excluding the parents' generation.

Assuming C children by generation ($C \geq 1$), each individual will have C children, plus $2^n(C-1)C^n$ n-th cousins and $2^n(C-1)C^{n+1}$ n-th cousins' children (n-th cousins once removed) from both parents' sides. For $n = 0$ these formulas provide the number of siblings and nephews/nieces.[26]

To derive the fraction carrying one's last name, we assume a balanced sex ratio. While fathers transfer their last name to daughters, mothers do not. Clearly, the probability of a child carrying a parent's last name is $\frac{1}{2}$ (1 if the parent is male, and 0 if female). Among one's n-th cousins,

[25] As type-1 error is on average negligible, a simplified expression for k that we end up using is $k = E\left(\frac{D}{N_{Fm}}\right)$.

[26] For example, if 3 children are born across generations, each individual will have 3 children, $2^0(3-1)3^0 = 2$ siblings, $2^0(3-1)3^1 = 6$ nephews/nieces, $2(3-1)3^1 = 12$ first cousins, and $2(3-1)3^2 = 36$ first cousins' children.

the fraction of those carrying one's last name is $\frac{1}{2^{2n}}$, while among n-th cousin's children this fraction is $\frac{1}{2^{2n+1}}$.[27]

To derive the number of family members with the same last name born in the same municipality, we assume very simply that the probability of being born in the same municipality as one's parent is p, and that the probability of being born in the same municipality as one's siblings is 1. We also assume that both parents are born in the same municipality, and that the probability of being born in the same municipality as one's ancestor of generation g is p^g. It follows that the probability of an n-th cousin being born in one's municipality is p^{2n}, while for an n-th cousin's children this probability is p^{2n+1}.[28]

We can make some assumptions on the value of C and p in order to derive reasonable estimates of one's family members born in the same municipality. We assume a rate of geographical mobility between 0.35 and 0.75, i.e., a probability p of being born in the same municipality as one's parent between 0.25 and 0.65.[29] We also assume a number of children by generation varying between 1 and 3.

In Figure A.7 we report the simulated number of close consanguineal family members (children, siblings, nephews/nieces, first cousins and their children) born in the same municipality. Clearly, this number increases with both p and C and it varies considerably: from less than 1 when mobility is high and the number of children is small, to around 22 when mobility is low and the number of children is high. On average, assuming a uniform distribution across the range of variation in p and C, we would expect 6 close consanguineal relatives in one's municipality of birth.

In Figure A.8 we report the number of close family members (brothers, sisters, first cousins, and their children) born in one's municipality of birth and sharing the same last name. This number varies between 0 and 7. On average one would expect 2.4 close consanguineal relatives born in the same municipality.[30]

[27] This specializes to 1 for siblings and $\frac{1}{2}$ for nephews/nieces, while this is $\frac{1}{4}$ for first cousins (the children of one's father's brothers) and $\frac{1}{8}$ for first cousins' children (the children of male cousins carrying one's last name).

[28] For example, this probability is 1 with respect to a sibling and p with respect to a nephew/niece, as s/he will have a probability p of being born where one of his parents (i.e., one's sibling, and hence the individuals himself) was born. The probability of two first cousins being born in the same municipality is p^2, as this is the joint probability that the children of two brothers are born in the same municipality. Similarly, for first cousins' children this probability is p^3.

[29] Data from the 2001 Italian Population Census show that around 45-50 percent of individuals live in one municipality of birth, with this number being roughly constant across generations (and slightly lower for women, who are typically tied movers). Assuming that children are born in one's municipality of residence, this provides a rough estimate for p.

[30] We can similarly derive estimates of the number of consanguineal family members carrying a different last name, $\overline{D}$. As in practice these individuals account for a very small fraction of those classified as unrelated, i.e., because the last term in equation (A.1) is close to zero (see footnote 25), for brevity we do not report these simulations.

A.3. Selection bias

One final concern that arises is related to the structure of the data, which is made of individuals with at least one social security spell over the period. Model estimates are at risk of suffering from selection bias.

To quantify the overall bias, let us start from our model in equation (4.2):

$$y_{iFmt} = \alpha + \beta P_{Fmt} + u_{iFmt} \tag{A.2}$$

where we ignore the measurement error issue (see Section A.1).

Let $A_i = \{Max_{t=1,..T}(y_{iFmt}) > 0\}$ define the event that determines inclusion in the sample, with the associated complementary event $B_i = \{y_{iFm1} < 0, y_{iFm2} < 0, .., y_{iFmT} < 0\}$, such that $Pr(A_i = 1|F,m) = 1 - Pr(B_i = 1|F,m)$.

Let:

$$W_{Fm} = \frac{Pr(B_i = 1|F,m)}{1 - Pr(B_i = 1|F,m)}$$

Given the selection rule, we only observe the empirical counterpart to:

$$E(y_{iFmt}|A_i = 1, F, m, t) = \alpha + \beta P_{Fmt} + h_{Fmt}$$

where $h_{Fmt} = -E(u_{iFmt}|B_i = 1, F, m, t)W_{Fm}$ and we have exploited the fact that:

$$E(u_{iFmt}|A_i = 1, F, m, t) = -E(u_{iFmt}|B_i = 1, F, m, t)W_{Fm}$$

which follows from the assumption that $E(u_{iFmt}|F,m,t) = 0$. Assuming independence of u_{iFmt} across time within individuals, it follows:

$$h_{Fmt} = -E(u_{iFmt}|u_{iFmt} < -\alpha - \beta P_{Fmt}) \left(\frac{\Pi_s Pr(u_{iFms} < -\alpha - \beta P_{Fms})}{1 - \Pi_s Pr(u_{iFms} < -\alpha - \beta P_{Fms})} \right)$$

Although the sign of the bias is indeterminate absent further assumptions on the distribution of u, it is easy to show that the bias tends to disappear as T grows, as $\Pi_s Pr(u_{iFms} < -\alpha - \beta P_{Fms})$, and hence W_{Fm}, are likely to become small as T increases. This is simply because the more observations there are for an individual, the less likely is that this individual will not have a positive draw of y_{iFmt} in any given time period, and hence will not be included in the sample.

This can also be directly tested using data. One can use information on the underlying population (from the tax data, see Section 2.4) and the number of politicians by family (from the politicians' data, see Section 2.2) to test whether the probability of inclusion in the INPS sample (i.e., the fraction of individuals with a given F3C in each municipality in the INPS data over the number of people in the population) is correlated with the vector $P_{Fm} = P_{Fm1}, P_{Fm2}, ..., P_{FmT}$.[31]

[31] Around 5 percent of families in the INPS or politicians data do not appear in the tax data. This slight discrepancy is most likely due to the fact that while the tax data report municipality of residence, the workers and politicians' data report municipality of birth.

We have regressed this fraction on the vector P_{Fm} using GLS with weights given by the number of individuals in each cell in the tax data. We also include in the model F3C plus municipality effects. Standard errors are clustered by municipality. The regressions include 3.9 million observations. Estimated coefficients with associated 95 percent confidence intervals are reported in Figure A.9. No clear pattern is detectable in the data, with some coefficients being positive and other negative and hardly ever individually significant.

A.4. Municipality characteristics

In this section we describe the municipal-level variables that we use in Table 9 (see also Table A.1 for descriptive statistics).

Discretionary exp.: municipal expenditure excluding debt service and personnel per year (in 2000 euros), average between 1993 and 2004 (source, Ministry of Interior Affairs).

Income per capita: personal income as of 2005 (source, Ministry of Interior Affairs).

Firms: number of productive activities registered to the Chamber of Commerce as of 2005 (source, Ministry of Interior Affairs).

Pct. unemployment: municipal unemployment rate as of 2013 (source, Istat). Computed as a projection, based on census data, of the unemployment rate at Local Labor District level (*Sistemi Locali del Lavoro*) at the municipal level.

Pct. public sector employment: share of public sector employment as of 2001 (2001 Population Census).

Pct. college: percentage of the resident population 6-years old and over with a college degree or more as of 2011 (source, 2011 Population Census). *Elderly index*: ratio of resident population above 65 over population below 14 as of 2005 (source, Ministry of Interior Affairs).

Population: resident population as of 2001 (source, 2001 Population Census).

Region capital: dummy indicating if the municipality holds the regional government seat.

Province capital: dummy indicating if the municipality holds the provincial government seat.

CC station: dummy indicating if the municipality hosts at least one *Carabinieri* station as of 2015 (source, IPA Indice delle Pubbliche Amministrazioni).

PS station: dummy indicating if the municipality hosts at least one *Polizia di Stato* station as of 2015 (source, IPA Indice delle Pubbliche Amministrazioni).

GDF station: dummy indicating if the municipality hosts at least one *Guardia di Finanza* station as of 2015 (source, IPA Indice delle Pubbliche Amministrazioni).

Court: dummy indicating if the municipality hosts a court as of 2015 (source, Ministry of Justice).

Subsidiary court: dummy indicating if the municipality hosts a subsidiary court as of 2015 (source, Ministry of Justice).

Crime per capita: total number of crimes reported to the judiciary authority per 1,000 individuals, average between 2004 and 2009 (source, Istat).

Corruption crimes per capita: total number of corruption crimes reported to the judiciary authority per 1,000 individuals, average between 2004 and 2009 (source, Istat).

Municipal government dissolved for Mafia: dummy indicating if the municipal government was ever (i.e., since 1991) dissolved due to Mafia infiltration (source, Ministry of Interior Affairs).

Non-profit organizations: number of non-profit organizations (voluntary associations, social cooperatives and foundations, excluding church based organizations) in the municipality (source, 2011 Population Census).

Politicians: total number of available seats in the council and in the executive, average between 1985 and 2011 (source, Ministry of Interior Affairs). The number of elected municipal officials varies discontinuously with population size (see Gagliarducci and Nannicini 2013), from 12 councilors and 4 executives in municipalities with less than 3,000 inhabitants, to 50-60 councilors and 14-16 executives in cities with more than 500,000 inhabitants.

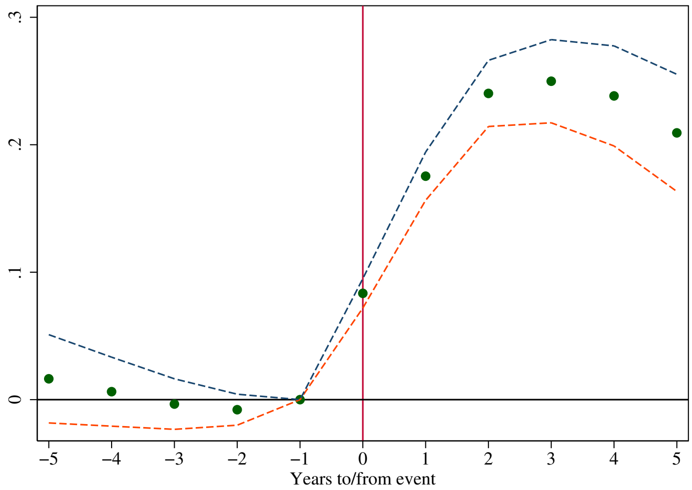
Voters' turnout: percentage of voters over total registered voters in municipal elections, average between 1993 and 2010 (source, Ministry of Interior Affairs).

Table A.1: Municipality characteristics

	Mean	s.d.
Discretionary exp. per politician (log)	11.540	0.926
Income per capita (log)	9.480	0.228
Firms per capita (log)	-2.623	0.328
Pct. unemployment	12.247	6.074
Pct. public sector employment	9.704	9.023
Pct. college	7.400	2.708
Elderly index	185.158	149.213
Population (log)	7.949	1.252
Region capital	0.003	0.053
Province capital	0.016	0.127
CC station	0.478	0.500
PS station	0.044	0.205
GDF station	0.063	0.244
Court	0.019	0.138
Subsidiary court	0.034	0.181
Crimes per (1,000) capita	0.028	0.021
Corruption crimes per (1,000) capita	0.001	0.020
Municipality dissolved for Mafia	0.026	0.159
Non-profit organizations per (1,000) capita (log)	1.490	0.606
Pct. voters' turnout	79.546	8.049
Politicians per capita (log)	-4.947	1.032

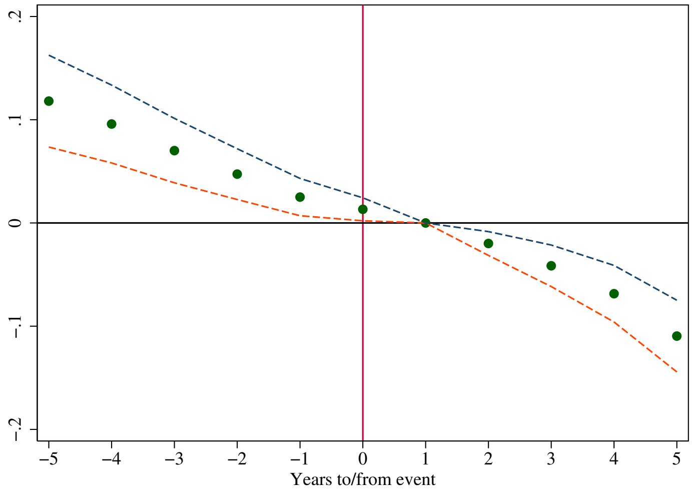
Notes. Number of observations: 7,182. See Section A.4 for a description of the variables and sources.

Figure A.1: Event-study analysis: Months of work - entry



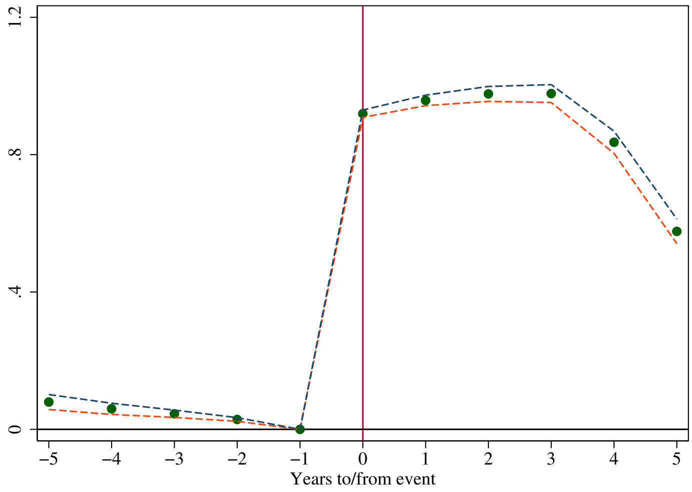
Notes. The figure displays the estimated number of months of work at different lags and leads since the time of first entry (denoted by a vertical line). All coefficients expressed relative to effect in year before entry. 95 percent confidence intervals reported. See text for detail.

Figure A.2: Event-study analysis: Months of work - exit



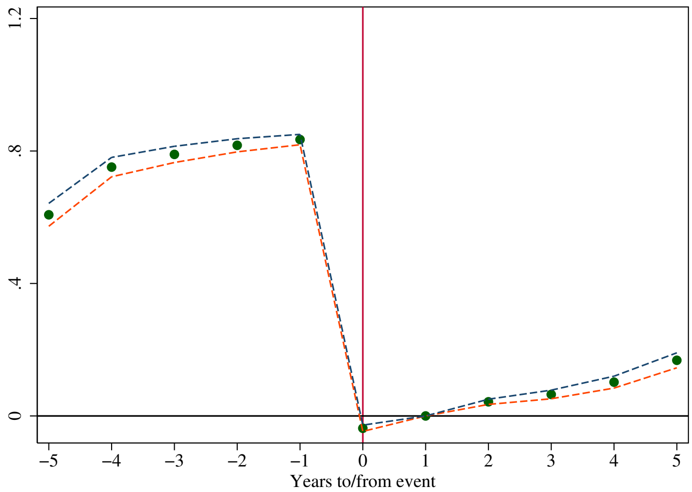
Notes. The figure displays the estimated number of months of work at different lags and leads since the time of last exit (denoted by a vertical line). All coefficients expressed relative to effect in year after exit. 95 percent confidence intervals reported. See text for detail.

Figure A.3: Event-study analysis: Number of family members in office - entry



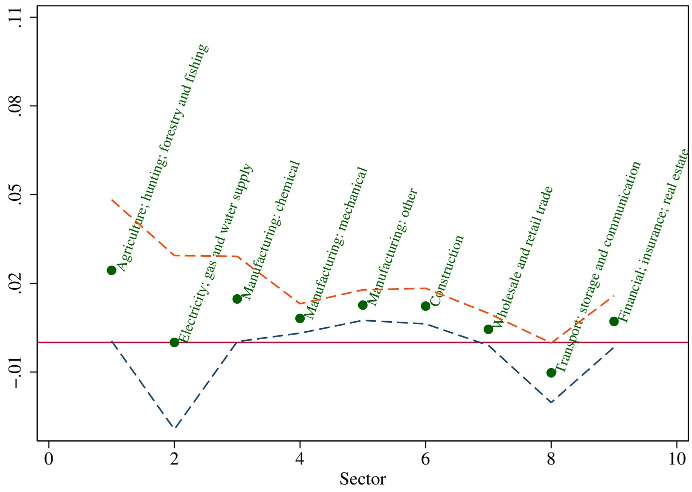
Notes. The figure displays the estimated number of family members in office in each year at different lags and leads since the time of first entry (denoted by a vertical line). All coefficients expressed relative to effect in year before entry. 95 percent confidence intervals reported. See text for detail.

Figure A.4: Event-study analysis: Number of family members in office - exit



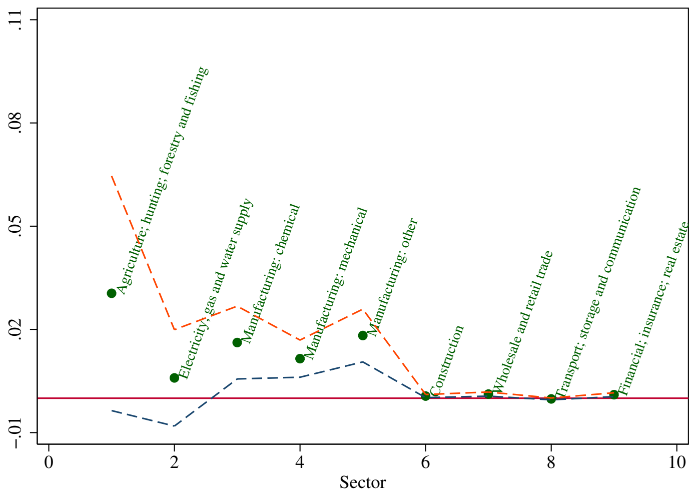
Notes. The figure displays the estimated number of family members in office in each year at different lags and leads since the time of last exit (denoted by a vertical line). All coefficients expressed relative to effect in year after exit. 95 percent confidence intervals reported. See text for detail.

Figure A.5: Heterogeneous effects by sector of activity: Yearly earnings



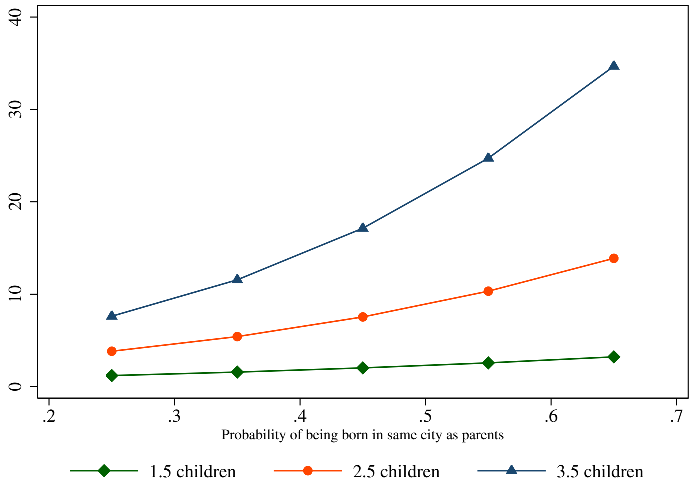
Notes. The figure displays the *proportional* increase due to nepotism, by sector. 95 percent confidence intervals reported. See text for detail.

Figure A.6: Heterogeneous effects by sector of activity: Months of work



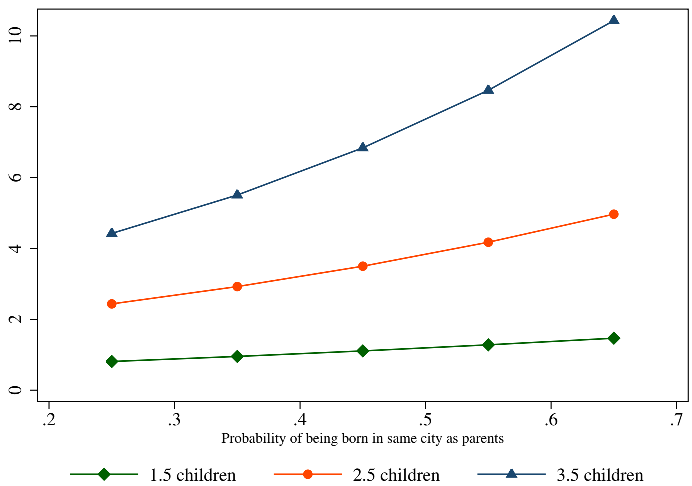
Notes. The figure displays the *proportional* increase due to nepotism, by sector. 95 percent confidence intervals reported. See text for detail.

Figure A.7: Simulated number of close consanguineal relatives born in one's municipality



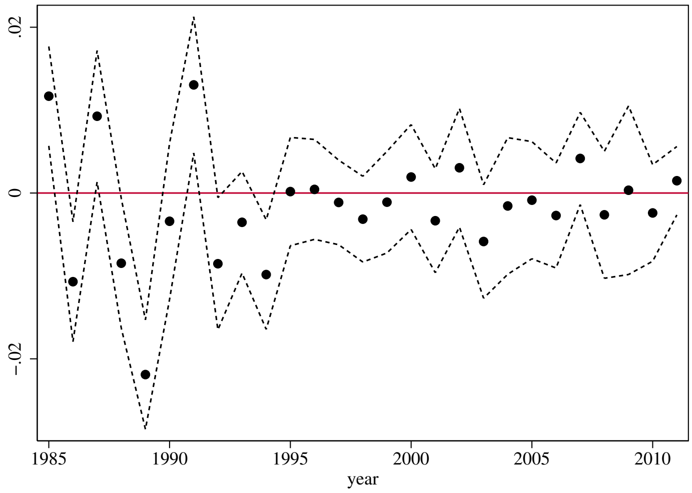
Notes. The figure displays the simulated number of close family members (children plus siblings, first cousins and their children) born in one's municipality. See text for detail.

Figure A.8: Simulated number of close consanguineal relatives born in one's municipality and carrying one's last name



Notes. The figure displays the simulated number of close family members (children plus siblings, first cousins and their children) born in one's municipality and carrying one's last name. See text for detail.

Figure A.9: Testing for selection



Notes. The figure reports the estimated coefficients and the associated 99 percent confidence intervals from a regression of the fraction of individuals in the INPS data by cell (F3C X municipality of birth) on the number of politicians per cell in each year between 1985 and 2011. Regressions include F3C plus municipality of birth fixed effects and are weighted by the size of each cell. Standard errors clustered by municipality of birth. See also Appendix A.3.

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