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

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Abstract

In this paper we study the effect of family connections to politicians on individuals' labor market outcomes. Using data for Italy spanning over three decades on a sample of almost one million working age individuals plus data on the universe of individuals holding political office, we show that, while in office, politicians are able to extract significant rents in terms of private sector jobs for their family members. We present evidence consistent with the hypothesis that this is a form of corruption, i.e., based on a *quid-pro-quo* exchange between employers and politicians, although an inferior substitute for easier to detect modes of rent appropriation on the part of politicians.

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1 Introduction

This paper combines micro data for Italy over almost thirty years on the universe of around 500,000 individuals holding political office with micro data on a random sample of almost one million working age individuals to estimate the returns - in terms of private sector jobs among family members - to holding political office. We present an array of evidence consistent with the view that the phenomenon we uncover is a form of corruption, i.e., based on a *quid-pro-quo* exchange between firms and politicians, although an arguably inferior substitute for sheer bribing and grafting.

There is plenty of anecdotal evidence that private firms often reserve special treatment to politicians' family members, including in what are typically regarded more mature democracies. The argument goes that, in exchange for, or in expectation of, political favors, firms hire or promote politicians' relatives or grant them higher earnings.[1] Evidence though remains elusive. We bring this argument to empirical scrutiny using data from Italy and investigate the main determinants and correlates of this phenomenon.

Italy appears an ideal case study for our analysis. The roles of family ties and the lack of trust, civic participation and meritocracy in shaping the fabric of society are often seen the root of the country's inability to modernize (Banfield 1958, Pellegrino and Zingales 2014, Putnam et al 1993). Alongside, widespread red tape and a cumbersome bureaucracy create opportunities for corruption, with the country ranking third from the bottom among OECD high-income countries in the Ease of Doing Business index (World Bank 2014) and highest among all Western European countries on the Corruption Perceptions index (Transparency International 2014).

One major advantage of the data that we have assembled for this exercise is that they provide information on each individual's 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 identify "families" based on individuals sharing the same F3C and born in the same municipality.

In order to identify the effect of a family member holding office on individuals' labor market outcomes we exploit the longitudinal nature of the data and the timing of family members' movements into office. In the spirit of a differences-in-differences estimation, we

[1] Allegations of political nepotism against companies often surface in the press, including in the USA. 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).

compare yearly changes in labor market outcomes among individuals whose "family" members enter office to changes in labor market outcomes among otherwise similar individuals who do not experience such entry. Based on this strategy, we find positive and precisely estimated effects of a family member in office on both earnings and employment.

We present an array of evidence to corroborate our claim that the effects we uncover are causal. First, we use an event-study analysis to show that there are no pre-trends in labor market outcomes prior to a family member taking office and that effects manifest precisely in the year in which this family member takes office, and tend to fade out as the end of the mandate approaches. In addition, we show that our results remain unchanged when we restrict the estimation sample to individuals who, at one point over the period of analysis, have a family member in office. This somewhat tempers the concern that we compare families with very different latent trends in the variables of interest. We also include in the model the interaction between individual fixed effects and linear time trends or with dummies for groups of consecutive years. Identification here is extremely demanding as it relies on highly temporally localized changes in the variables of interest. Our results are robust to all these checks.

Despite the very fine-grained partition of the data, our matching method identifies families with error, since not only does it fail to classify some connected individuals (those with a different F3C) as family members but - more importantly - it erroneously classifies some unconnected individuals (those with the same F3C) as family members. We show that this induces a systematic downward bias in our estimates and that one can use information on the distribution of F3Cs in the population to correct the estimates for this source of non-systematic measurement error.

We show in the paper that parameter estimates derived using this fuzzy matching method will be effectively attenuated by the fraction of all those truly related among all those with the same F3C and municipality of birth. So, if one had information on the total individuals in the population with the same F3C and municipality of birth, one could attempt to derive error-free estimates of the parameter of interest. Although we have no such data, we can use information on these frequencies from the INPS sample. Based on this approach, our back-of-the-envelope calculations imply that individuals in office generate on average approximately extra 10,000 euros worth of private sector earnings among their family members carrying the same last name and born in the same municipality. These are likely to be conservative estimates of the overall returns to holding office in terms of family private sector earnings,

as they exclude family members with different last names or born elsewhere.

In the second part of the paper we examine the gradient in the estimated effects as a function of politicians' clout. If the effect we uncover is due to rent extraction, one will expect this effect to be larger the larger the rents accruing to office. Consistent with this, and similar to Brollo et al (2013) and Dal Bó et al's (2006) finding that corruption increases when resources increase, we find that the effect is larger the larger the budget available to the administration and the longer the tenure in office. However, we do not find systematically larger effects associated to higher levels of office (executive versus legislative branch), or to the highest levels of government (regional and national). Although this might indicate that greater rents are not always related to more nepotism, an alternative interpretation is that greater public scrutiny associated with these offices reduces the opportunities for nepotistic practices. We also find that effects are larger in sectors that are more dependent on the public administration, where the returns to nepotistic hiring are presumably higher.

We finally investigate how nepotistic hiring varies with the cost of alternative technologies of rent extraction on the part of politicians. Although the extraction of monetary bribes might be cost-effective ways for politicians to monetize over the rents that accrue to office, if disclosed, payment of bribes will entail a cost, as this is *per se* evidence of misbehavior, while the hiring of a politician's family member is not. It follows that, if the cost of alternative technologies of rent appropriation increases, one will expect parties to shift towards more hidden, harder-to-detect forms of corruption.

In the final part of the paper we bring this argument to empirical scrutiny by exploiting the heterogeneous effect across judicial districts of a major anti-corruption campaign against firms and Politicians entryvolved in payment and receipt of monetary bribes. Historiographical accounts of this campaign suggest that this was initiated by judges and prosecutors with close links to the left-wing faction of the Association of Italian judges and prosecutor). We compare changes in corruption cases prosecuted in each of the 26 judicial districts before and after this anti-corruption campaign. Consistent with increased deterrence, we find a larger fall in the number of corruption cases in districts with a greater baseline vote share for the left. However, we also find a larger increase in the spread of nepotistic hiring in these areas. We take this evidence to suggest that nepotistic hiring is a substitute - and potentially an inferior one - for grafting and monetary bribes. Our result is reminiscent of Olken's (2007) finding that increased corruption monitoring in Indonesia leads to lower corruption but higher nepotistic hiring in publicly funded projects.

Although we are not the first to examine the returns to family connections to politicians, we are arguably the first to investigate returns in the private labor market in a highly corrupt environment. Folke et al (2017) use Swedish register data to investigate earnings of children of elected mayors. They find positive but very modest effects on the probability of finding a private sector job in the municipality where the parent is elected, which they fail to ascribe to an exchange between politicians and firms. Given the notably low levels of corruption in Sweden, the latter seems in fact unlikely. Fafchamps and Labonne (2017) investigate the effect of family connections to local Politicians entry the Philippines and find evidence of such connections having a positive effect on the probability of employment in better paying occupations. However, they cannot distinguish between private and public employment, leaving open the possibility that, similar to Olken (2007), most of the effects found are ascribable to the public sector, where hiring and promotion decisions are under the direct or indirect control of politicians.

Our paper relates and contributes to different streams of literature in both political economy and labor economics. There is considerable evidence of substantial monetary returns to political careers both while in office and after that (Cingano and Pinotti 2013, Fisman et al 2014, Merlo et al 2010), including through the establishment of political dynasties (Dal Bó et al 2009). 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. This happens either by sharing rents with colluding agents or through direct diversion of public resources for personal purposes (Banerjee et al 2012, Brollo et al 2013, Ferraz and Finan 2008, Olken 2007, Olken and Pande 2012).

Connecting the literature on the role of connections with the literature on political careers, 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 2016, Cingano and Pinotti 2013, Fisman 2001). These links appear to be more likely in more corrupt environments, providing indirect evidence that they might directly benefit politicians. Differently from our paper, though, these studies largely focus on connections to shareholders, CEOs and board members and typically refer to small samples of firms.

An established body of literature in labor economics focuses on - and finds evidence indicating 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). A related body of literature uses last names to identify family ties or to measure intergenerational mobility and the concentration of families in specific occupations (e.g., Clark and Cummins 2014, Durante et al 2011).

Through the provision of insurance, information or mechanisms of contract enforcement, family and other informal networks might provide a second best solution to market failures. However, 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, 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).

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).

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 discusses the econometric model. Section 4 presents the main regression results. Section 5 discusses the consequences of measurement error for our estimates. In Sections 6 and 7 we investigate and discuss the determinants of nepotistic hiring. Section 8 finally concludes.

2 Data

2.1 Workers' data

For the purpose of the empirical exercise, we use workers' micro data from the Italian National Institute of Social Security (*Istituto Nazionale della Previdenza Sociale* or INPS) between 1985 and 2011. These are matched employer-employee data that, for each year, record all employment spells and the associated annual earnings for the universe of dependent workers in the private sector, hence excluding self-employment and the public sector.[2] The version

[2] 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. In practice we exclude firms in the public sector (sector ATECO-81 = 90).

of the data we have access to refers to a 1/30 random sample (those born on the first day of each month) of those in INPS each year, around 360,000 individual employment spells per year.

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 (blue collar, white collar and manager) and sector of activity at two-digit level (fifty categories). Unfortunately, other than for an anonymous firm identifier, no additional information is available on the firm.

Importantly, for each worker, the data provide the tax code (*codice fiscale*), which in Italy is calculated as a deterministic function of gender, date and municipality of birth and the first three consonants of the last name (F3C). For women, the F3C is based on the maiden name.

The original data provide information on all employment spells. For computational purposes, we transform the data into on observation per individual per year: we assign to each individual in each year the total number of calendar months worked and total earnings across all jobs, while we assign the characteristics (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 one's own earnings and employment, we also exclude from the sample workers who ever appear in the politicians' data set (see next section). Importantly, note that the INPS data do not allow us to identify family ties, be it spouses or parents and children. As every individual autonomously contributes to his/her social security account, there is no concern that we are confounding spouses' incomes. We now mention this explicitly in the paper (in the data description section). Average real (at 2005 prices) yearly earnings among those with at least one day of social security contributions during the year are about 19,500 euros (around $21,000 USD), with workers working on average ten calendar months (see Table A.1).

2.2 Politicians' data

We combine INPS data with yearly data on the universe of individuals holding political office between 1985 and 2011, at any level of government - local, sub-national and national - whether elected or appointed and whether in the legislative or executive branch.

In addition to the central government composed of the two houses of parliament and

the central government, each geographical entity (8,110 municipalities, 103 provinces and 20 regions) has its own local government, with both a legislative and an executive branch. Each of these different levels of government has 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 gender and age, the level of government, whether in a council or executive position, date of assuming and leaving office (with the former left censored to January 1^{st} 1985, and the latter right censored to December 31^{st} 2011), the data also provide information on municipality of birth and last name, and hence the F3C. Almost the universe of married women use their maiden name when they run for office. 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.

Overall, between 1985 and 2011 there are around 137,000 individuals in office every year, for a total of approximately 525,000 individuals for the entire period. Not surprisingly, the greatest majority of those in office hold positions in the municipal government, accounting for more than 96 percent of the observations (see Table A.2). In contrast, national politicians account for less that 1 percent of the observations. Around 70 percent of individuals are in council positions and the rest in the executive.

2.3 Matched workers-politicians' data

In the empirical analysis that follows we focus on the sample of individuals who, over the twenty-seven years of analysis, make at least one social security contribution and we follow their employment and earnings careers as their family members assume or leave office. To do so, we transform the workers' data into a yearly panel, with one observation per year for each individual who is ever observed in the INPS data. When an individual has no contribution in a year, we assign zero earnings, months of work and employment. We restrict to native born individuals of working-age, i.e. aged 18-65. This leads to an unbalanced panel (due to the age restrictions) of around 725,000 individuals per year and a total of 19 million year X individual observations (see Table A.3). The individuals in our sample account for around 2

percent of the working age population in each year.

3 Econometric model

In this section we present the econometric model that guides our empirical analysis. Let y_{iFt} denote labor outcomes in year t of worker i from "family" F (i.e., with given F3C F and municipality of birth m), and let P_{Ft} be the number of individuals in office at time t with the same F3C and municipality of birth. The regression model is:

$$y_{iFt} = \beta_0 + \beta_1 P_{Ft} + X'_{it}\beta_2 + \mu_i + \lambda_t + u_{iFt} \tag{3.1}$$

where β_1 is the additional outcome that each politician generates among each individual connected along family lines. In the model, we include individual fixed effects (μ_i) and time effects (λ_t). Identification of β_1 is based on a differences-in-differences strategy that relies on a comparison of changes in individuals' labor market outcomes before and after somebody in their family assumes or leaves political office with the same outcomes for individuals who remain (un)connected over the same period.

Importantly, identification of β_1 relies both on entries in as well as exits from office. This model constrains the coefficient on entries to be the same as the coefficient on exits. Below we separate these sources of variation and we argue that variations due to entries are more likely to deliver consistent estimates of the parameter of interest than variations due to exits.

One major challenge associated to the estimation of the parameter of the model is that we have no information on actual family ties but only on whether individuals share the same F3C and municipality of birth with individuals in office. This implies that for individual i from "family" F we only have an error-ridden measure of the number of true family members in office. This error arises because we classify as connected individuals with the same F3C and municipality of birth but who are not family members while we fail to classify as connected individuals who are linked by family ties but who do not share the same F3C. Under the reasonable assumption that the first source of error is negligible, one can show (see Appendix A.1) that the OLS estimate of the parameter β_1 is attenuated by a factor $0 < k = E\left(\frac{D_{Ft}}{N_{Ft}}\right) < 1$, where N_{Ft} denotes the number of individuals from "family" F at time t and D_{Ft} denotes the number of individuals genuinely related to a politician via family ties among them. The intuition for this result is simple: estimates that are based on F3Cs and municipality of birth rather than on actual family ties are diluted by the fraction of those not actually related

among all those classified as connected.

One can also make some progress on the actual return to family connections based on the distribution of the frequency of last names, N_{Ft}, which is known. In particular, one can allow the model parameters to vary across groups of individuals with different frequency of last names in each municipality of birth. In formulas:

$$y_{iFt} = \theta_0 + \theta_1 \left(\frac{P_{Ft}}{N_{Ft}} \right) + X'_{it}\theta_2 + \mu_i + \lambda_t + e_{iFt} \tag{3.2}$$

From the above, the OLS estimate of θ_1 will converge in probability to $E(D_{Ft})\beta_1$. This is an estimate of the *total* return to holding office among truly related individuals. We can exploit the INPS sample to derive an estimate of N_{Ft} for each family. As this is a 2 percent random sample of the working age population, one can simply rescale this number by a factor of 50 to estimate the total private labor market return from holding office.

4 Model estimates

We start by presenting estimates of model (3.1). As said, these are conservative estimates of the parameter of interest. For most of the analysis, we exclude workers with a frequency of the F3C in their municipality of birth in the INPS data greater than 30, the 90th percentile of the distribution. We do so to attenuate the consequences of measurement error. In closing, we turn to the estimates of the total return to holding office on family members' earnings and employment outcomes based on equation (3.2).

4.1 Event-study analysis

Before presenting the model estimates, and in order to add transparency to the analysis and to further probe the validity of the identification assumption, we present event-study analyses of changes in labor market outcomes at the time of entry or exit of "family" members in/from office. This allows us to examine potential pre-trends in labor market outcomes and to directly observe the evolution of labor market outcomes in each year after the election.

We start by focusing on entry episodes. Clearly, to do so, we ignore individuals who remain unconnected throughout the period. As families can experience multiple entries in office over the period, which greatly complicates the analysis, for each family, for the event

study, we focus on the first entry episode in the period 1985-2011. This somewhat tempers the concern that earlier entry episodes are responsible for the observed trends in outcomes.

We include observations in a 11-year window (from -5 to +5) around the time of first entry in office for "family" F, t_1, and we estimate the following equation:

$$y_{iFt} = \beta_0^{in} + \sum_{t=t_1-5}^{t_1+5} \beta_{1,t-t_1}^{in} P_{Ft_1}^{in} + X_{it}'\beta_2 + \mu_i + \lambda_t + v_{iFt}^{in} \tag{4.1}$$

where $P_{Ft_1}^{in}$ is the cumulative number of individuals from "family" F having entered office between 1985 and t_1. The coefficients $\beta_{1,t-t_1}^{in}$ capture trends in outcomes at different leads and lags to/from the time of first entry. As we can only identify ten coefficients out of eleven, we restrict the coefficient in the year before the first entry ($t = t_1 - 1$) to zero.

Estimated coefficients for yearly earnings, together with 95 percent confidence intervals based on standard errors clustered by municipality of birth are reported in Figure 1 (corresponding graphs for months of work and for employment are very similar and are reported in the appendix).[3] A vertical line refers to the year of first entry (time t_1). One can verify that, prior to entry, there is no trend in labor market outcomes. This evidence rules out anticipation effects or a spurious correlation between families' labor market and political fortunes. One can also see that the estimated coefficients become positive exactly at the time of entry, they increase over time, as politicians establish themselves, and they start to decline precisely after four years, i.e., towards the end of an electoral term. We revert to the magnitude of the effects when we present our regression estimates below.

In Figure 2 we examine politicians' exit from office. We perform an exercise similar to the one on entries, by examining trends before and after the last exit episode. Once more, we focus on the last exit episode in order to limit the possibility that subsequent exits might confound our estimates, and we restrict the coefficient in the year after the last exit ($t_N + 1$) to zero.

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 evidence suggests that exits are somewhat anticipated, which is reasonable given the normal length of a term and the fact that information on those running for the election is somewhat known in advance. Because of this, our identification strategy - which is based on a pre-post comparison around

[3] Similar to columns (2) and (3) of Table 1 and all subsequent regressions, we control for individual fixed effects, individuals' age and the interaction of year and province fixed effects.

the event - will fail to identify the effects of interest. For this reason, in the rest of the paper, we focus on variations in the stock of Politicians entry office induced by entries only.

4.2 Main estimates

Having presented evidence from the event-studies, Table 1 reports regression estimates of model (3.1). Each panel refers to different dependent variables (earnings during the year, months of work in the year, and dummy for at least one employment spell in the year, respectively), while separate columns refer to different specifications. As a baseline specification, in column (1) we use as a regressor only the number of individuals in office at any point in time (P_{Ft}). Clearly, this specification is likely to suffer from omitted variable bias, as the probability of being in office and in work/earnings are likely correlated across families. For this reason, in column (2) and similar to the event-studies analysis above, we additionally control for individual fixed effects plus individuals' age (the only time varying individual level variable in the INPS data). We also control for year effects which we further interact with province (effectively live-to-work areas) fixed effects to account for generalized differences in local labor markets outcomes.[4] Finally, in column (3) we report separate coefficients on cumulative inflows and outflows, P_{Ft}^{in} and P_{Ft}^{out}. Note that, relative to the specification in column (3), the specification in column (2) constraints the coefficients on cumulative inflows and outflows to be of the same value and opposite sign. Again, standard errors are clustered by municipality of birth.

When comparing estimates in column(1) and (2), 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 specification in column (2), this suggests that one politician in office increases yearly earnings among all individuals with the same F3C and born in the same municipality by 99 euros (a 1 percent increase relative to baseline level of earnings of around 9,500 euros), months of work by 0.035 (a 0.7 percent increase relative to a baseline value of 4.5), and the probability of employment by 0.3 percentage points (a 0.6 percent increase relative to a baseline value of 48 percent).

Consistent with the event-study evidence, it appears that the positive effect of connections manifests only upon entry (column 3), while there is no evidence that exits are systematically

[4] One might be concerned that our sample inclusion criterion, namely those with at least one social security contribution over the 27 years of analysis, leads to regression estimates that are biased. We address this point in Appendix **??**, where we argue that this concern is most likely second order.

associated to changes in labor market outcomes.[5] Because of this, from now on we will focus on the coefficient on entries only, as in column (3). In the rest of the analysis we also focus only on the most saturated specification, i.e., with the inclusion of individual fixed effects, time X province fixed effects and workers' age.

From the table, it also appears that the effects of a family member entering office are largely due to variations in employment at the extensive rather than at the intensive margin, meaning that the results are due to hiring (or lack of firing) rather than increases in months of work conditional on working. It also appears that the effects are largely due to increases in employment rather than earnings conditional on working. Note however that earnings gains are marginally larger than employment gains (1 versus 0.6-0.7 percent), suggesting that either those who benefit from political connections enjoy wage premia or that these individuals are selected among those with higher than average earnings potential.

4.3 Heterogeneous effects by jobs and workers' characteristics

Table 2 explores the differential effect of political connections by jobs and workers' characteristics. We start by investigating the type of jobs accruing to politicians' family members, running separate regressions by occupation (blue collar, white collar and manager). Note that different occupations correspond to alternative employment outcomes rather than to intrinsic workers' attributes. To perform this analysis, hence, for each occupation we create a separate outcome variable. If an individual is not employed in a certain occupation at time t (either because not employed at all or employed in another occupation), the outcome variable is set to zero. The last row of Table 2, columns (1) to (3), shows average earnings and months of work in each occupation among all individuals in our sample.

We find positive effects of entries in office for each occupation type. For example, political connections are responsible for an additional 54 euros worth of blue collar earnings and 24 euros worth of manager earnings. Consistent with this, the event-study graphs in Figure 3 show positive effects of the first entry episode on both blue and white collars' earnings. Effects on mangers' earnings are small and very imprecisely identified.

An average individual in the sample earns 4,637 euros worth of blue-collar's earnings and 510 euros worth of managers' earnings per year. It follows that connections lead to a proportional increase in blue collars' earnings of 1.16 percent (=54/4,637) and an increase

[5] Estimates of a model with the inclusion of entries only, i,e., excluding exits (not reported but available upon request, are virtually identical to those in column (3) of Table 1.

in managers' earnings of 4.71 percent (=24/510). These results suggest that jobs created by politicians are disproportionately high-paying. This squares well with the evidence discussed above that the effects on earnings are proportionally higher than for employment.

In column (4) we investigate heterogeneous effects by workers' age. Estimated effects are positive for younger individuals and they tend to decline with age. The estimated effect on earnings is negative for individuals older than 55, possibly due to earlier transitions to retirement or to transitions to other sectors. Results from the event-study analysis in Figures 4 confirm these pattern, although there is some evidence of anticipation effects for older workers. Here and in the following we only report event studies for yearly earnings. Event studies for employment and months of work are remarkably similar and for brevity we omit them in the rest of the paper.

In column (5) we investigate heterogeneous effects by workers' gender. We find positive and statistically significant effects for both men and women. Although, in absolute terms, the effect among female workers is about half the effect among male workers, the proportional effects are roughly the same across gender groups (166//11,171=1.5 % for males and 57/6,235=0.9 % for females).

Finally, in column (6) we explore the heterogeneity in effects across politicians of different ages and gender. We restrict to "young" workers, i.e., those aged 18 to 35, as this is the group for which we find the largest effects. We observe that young workers benefit from being connected to either "young" (18-35) or "old" (aged 56 plus) politicians, but not to middle-age politicians (age 36-55). This is consistent with our interpretation that the model estimates genuinely capture family ties, as these politicians are likely to be connected to young workers via siblingship or parenthood, respectively, while middle aged politicians are less likely to have direct family ties to young workers. Even more revealing, we find no effect of "old" female politicians on young workers' careers, while we find an effect of "old" male politicians and of young politicians, irrespective of their gender. Recall that while fathers and children as well as siblings - irrespective of the children and siblings' gender - share the same F3C, mothers and children do not. We see this evidence as providing further support in favor of our claim that our estimates capture causal effects of family ties.

In sum, it appears that political connections grant access to jobs that are better than the average job, and that younger workers are those who most benefit from these connections. We have also provided additional evidence consistent with the claim that our estimates genuinely capture connections via family ties.

4.4 Threats to identification and additional tests

In Table 3 we present further corroborating evidence in favor of our identification assumption. In column (1) we restrict to workers ever connected, i.e., with at least one family member in office during the twenty-seven-year period. This somewhat tempers 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 selection criterion reduces the sample by almost 50 percent but results are very similar to those in Table 1. For earnings, for example, the estimate is around 91 euros relative to an estimate on the entire sample of around 118 euros but differences are not statistically significant.[6]

We also experimented with more flexible specifications where we include the interactions of individual fixed effects with a linear time trend (in column 2). Results remain virtually unchanged compared to column (3) of Table 1. In columns (3) to (5) of this table we also include respectively dummies for 8-, 4- and 2-years sub-periods interacted with individual fixed effects. Note that identification here relies on increasingly close observations around the time of entry in office of a family member. Even focusing on these very localized differences, point estimates remain positive and statistically significant at conventional levels.

5 Implied returns to nepotistic hiring

As said, one way of interpreting the estimates in the previous section is that these are error-ridden estimates of the true effect of family connections. This error is likely to be larger the larger the size of the group.

Recall that, if we had the actual number of individuals with the same F3C and municipality of birth, N_{Ft}, we could standardize the regressor by this number and, from (3.2), obtain a consistent estimate of θ_1, i.e. the overall returns generated by a politicians among all those related. Although we do not have this number, we can exploit the INPS sample to derive an estimate of N_{Ft} for each family. As this is a 2 percent random sample of the working age population, one can hence simply rescale this number by a factor of 50 to estimate the total private labor market return from holding office.

[6] Alternatively, we could have used an RDD, comparing labor market outcomes of families of those who barely won and barely lost in close elections. Although appealing in theory, this approach is unfeasible in this context. The major limitation is that (with the exception of mayoral elections towards the end of the period), we do not have data on candidates other than those who won the election. This problem is further compounded by the circumstance that most of the elections in Italy are held under party rather than individual ballot system.

This is what we do in column (1) of Table 4, which reports estimates of equation (3.2), where the regressor is the number of cumulative entries in office divided by the frequency of each group in the INPS sample. For this, we use the median size for each "family" in the INPS sample across the twenty-seven years of analysis. The point estimate for earnings is 208 euros, which is the total return among those in the INPS sample. Extending to the universe of those of working age, this implies that each politician is able to extract around 10,000 euros of private labor market earnings for his family for each year since entering office (208/0.02). We similarly estimate an overall increase of 4 months of work and 0.25 percentage points worth of private sector employment.

Rather than imposing the coefficient in (3.2) to vary parametrically with N_{Ft}, one can also estimate separate parameters by the frequency of the distribution of F3Cs in each municipality of birth. One will expect larger estimates for smaller groups, as measurement error is less of an issue in this case. In columns (2) to (5) we present separate regressions for sample frequencies 1, 2-5, 6-30 and more than 30. Consistent with what implied by measurement error, estimated effects decline monotonically with the frequency of the F3C. The average return among individuals in groups of sample size 1 is around 131 euros, implying an estimated total effect in the population of around 6,550 euros (131/0.02). The effect among individuals in groups with a sample size of between 2-5 (geometric mean of the distribution of frequencies 0.37) is 86 euros, implying a total effect of around 11,600 euros (86 /(0.02 X 0.37)). Finally, for frequencies between 6 and 30 (geometric mean 0.10) the effect is 34 euros, implying an overall effect of around 17,000 euros (34 /(0.02 X 0.10)). Similar patterns emerge when looking at months of work and employment in the bottom panels of Table 4. Overall, estimates of the total return to office appear to increase with the number of individuals in the group, possibly due to a greater number of family members living in the same municipality for larger groups (i.e, a larger D_{Ft}).

6 Nepotistic hiring and corruption

6.1 Rents in office

In this section we bring ammunition to our claim that the phenomenon we uncover above is based on a *quid-pro-quo* exchange between politicians and firms. We do so by investigating how the incidence of nepotism varies with the resources available to the office where politicians serve and then to their clout. As a first exercise, we augment model (3.1) by including the

interaction between the number of politicians having entered office since 1985 and the amount of the local budget. In formulas, we estimate the following model:

$$y_{iFt} = \beta_0 + \beta_1 P_{Ft} + \beta_2 \; P_{Ft} \; Spending_m + X'_{it}\beta_3 + \mu_i + \lambda_t + u_{iFt} \qquad F \in m \qquad (6.1)$$

where $Spending_m$ is the (log) discretionary expenditure per politician in municipality of birth of worker m. The latter is defined as the average (1993-2004) yearly expenditure net of debt service and personnel. As the model includes individual fixed effects, these absorb variations in $Spending_m$, which is hence not included as a regressor. Because we use information on the local budget, we restrict to the effect of municipal politicians only. However, we have shown above that the majority of politicians serve at the municipality level.

Table 5 presents these estimates of the parameter β_2 in odd numbered columns. As, clearly, the amount of spending is not randomly allocated across municipalities, we also experiment with specifications that include the interaction of the regressor with a large number of observable municipality-level characteristics (see Appendix A.3 and Table A.4). It appears that a 10 percent increase in resources per politician leads to a between 5 and 7 percent increase in yearly earnings compared to the average estimates in Table 1, depending if one includes controls or not (0.1 X 59 / 118 and 0.1 X 86 /118 respectively). A similar pattern can be detected in Figure 5, which reports separate event-study analyses for municipalities below and above the median of discretionary expenditure per politician. Results for months of work and employment are qualitatively similar but typically less precise.

In Table 6 we investigate heterogeneous effects by politicians' clout, as measured by the office where they serve and tenure in office. Column (1) reports the effect of entries in office by number of consecutive terms in the same level of government (first term, second term, etc.). These have to be interpreted as additional effects, as an entry in a second term, for example, requires having concluded a first term in office. Result show a sizeable effect of the first term in office. While there is apparently no additional return to staying in office a second term, there are sizeable premia associated to a third term, with the effects being significant for earnings only. This is evidence of the returns to office 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.

Column (2) reports the effect of the number of politicians entering at different levels of government (municipal, provincial, regional and national). A positive gradient is found

among politicians entering at higher levels of government (provincial) compared to those at lower levels (municipal), although effects manifest only for months of work and employment. Results at the highest levels of government (regional and national) are smaller and typically imprecise, which is unsurprising given that most politicians serve at the local level. Consistent with this last finding, results in column (3) show that access to executive positions generates returns that are around 15 percent lower than those associated to council positions, although point estimates cannot be told apart.

Event-study analyses in Figures 6, 7 and 8 corroborate the regression results, although there is evidence that by focusing on the first entry event there is a positive effect of a second term in office which is not evident when focusing on all entry episodes.

Taken together, these results show that the estimated effect is larger the larger the resources accruing to office, which lends support to our interpretation of the coefficients measuring rent extraction on the part of politicians. We do not find systematically larger effect associated to longer tenure, higher levels of office (executive versus legislative branch), or to the highest levels of government (regional and national), possibly because increased public scrutiny associated with higher level of office reduces the opportunities for nepotistic practices.

6.2 Public influence over firms

We now investigate effects across firms operating in sectors with different levels of dependence on the public sector. We use Pellegrino and Zingales' (2014) Public Sector Dependence Score, which is based on the number of news articles on regulation policy and government aid and contracts as a percentage of the total number of news articles per sector between 2000 and 2012. The index varies between around 1.5 percent in Basic Metals and Fabricated Metal Products to over 9 percent in Agriculture, Hunting, Forestry and Fishing. While this index is clearly a coarse measure of public influence, it has the advantage of capturing the two main channels through which politics might interfere with firms' activities, i.e., regulation and public transfers.

We proceed in two steps. Similar to the analysis by occupation above, we first derive estimates of the effect of entries in office separately for each of the fifty ATECO-81 two-digit sectors, which is the industrial classification used in the INPS data. In formulas, we estimate the following equation, separately by sector:

$$y_{ijFt} = \beta_{0j} + \beta_{1j} P_{Ft} + X'_{it}\beta_{2j} + \mu_i + \lambda_t + u_{ijFt} \tag{6.2}$$

where y_{ijFt} are outcomes (i.e., earnings or employment) in sector j.

In the second step, we regress the estimated coefficients on the Pellegrino and Zingales' (2014) score (PZ_j), in formulas:

$$\widehat{\beta_{1j}} = \gamma_0 + \gamma_1 PZ_j + v_{jt} \tag{6.3}$$

In this regression, we weight observations by the reciprocal of their standard error, in the spirit of a minimum distance estimator. Point estimates reported in Table 7 are systematically positive, although statistically significant at conventional levels only for earnings. A back-of-the-envelope calculation suggests that moving from the least regulated to the most regulated sector (7.5 percentage points) leads to an increase in the monetary returns to political connections of around 8 percent (=7.5 X 1.106 /108). In Figure 9 we also report event-studies separately by sectors with a different degree of public dependence. For simplicity, we group sectors into two categories: below median (0-.03) and above median (.03-.1) score. The figures confirm the results of Table 7 that the effect of connections is much stronger in sectors that are more dependent on the public administration. In sum, we find evidence of the effects being larger in more regulated sectors, which is consistent with the view that the phenomenon we uncover is driven by a *quid-pro-quo* exchange between firms and politicians.

7 Nepotism vis-à-vis other modes of corruption

The obvious question that remains is why, in an attempt to extract rents that accrue to their office, politicians engage in these practices as opposed to simple grafting or eliciting monetary bribes from firms. We argue that this practice is a substitute, possibly an inferior one, for other, more visible and easier to detect, forms of corruption. Although the returns to nepotistic hiring are presumably lower, as jobs are not necessarily fungible for money, nepotistic practices are also less likely to be discovered and lead to prosecution, and hence their cost is also presumably lower.

In order to bring suggestive evidence in favor of this claim, we exploit a major natural experiment induced by "*Mani Pulite*" (or "Clean Hands"), an aggressive judicial prosecution campaign against cases of corruption linked to payment of bribes to the then majority

parties (Christian Democrats and Socialists) that swept Italy starting in 1992 (see *The New York Times* 1993). Importantly, the focus of the investigations was on payment and receipt of monetary bribes, both because these were apparently very widespread and, more importantly, as illicit transfers and funds represented the primary source of evidence brought by prosecutors in most of these cases.

Clean Hands exploded after a period when the judiciary had been dormant in the face of rampant corruption. A widespread view (see, e.g., *Il Foglio* 2016) is that the campaign was initiated by prosecutors with links to *Magistratura Democratica* (in brief MD), the left-wing faction of the *Associazione Nazionale Magistrati* (in brief ANM), the independent official body that represents the interests of judges and prosecutors. MD had historical ties to the Communist Party, traditionally in opposition to the majority coalition parties (Christian Democrats and Socialists). The campaign eventually led to the collapse of traditional political parties and the overall system of representation that had emerged in post-war Italy.

We exploit the 1988 district-level share of votes for MD in the election for the *Comitato Direttivo Centrale* of the ANM (see www.associazionemagistrati.it), MD_d, to predict how aggressive the Clean Hands campaign was across areas. One will expect the judiciary to more aggressively prosecute cases of payment of monetary bribes in areas where MD was stronger. If nepotistic hiring is a substitute for payment of monetary bribes, one will also expect a rise in nepotistic hiring where MD was stronger.

We investigate the correlation between this variable and a number of outcome variables over three periods (p): 1985-1991 (pre-Clean Hands), 1992-2000 (aftermath) and 2001-2011 (post). Column (1) of Table 8 reports a regression where the dependent variable is the log per capita number of crimes against the public administration (C) prosecuted in each of the twenty-six districts (d) in each year (t). These crimes include wrongdoing on the part of both public officials and private agents, and also include payment and receipt of monetary bribes, as well as grafting. The regression include sub-period dummies and we use a GLS with weights equal to population. In formulas:

$$C_{dt} = \Sigma_p \psi_p I_{tp} + \Sigma_p \psi_{1p} MD_{d0} I_{tp} + \epsilon_{dt} \tag{7.1}$$

where I_{tp} is a dummy that takes the value 1 if year t is in sub-period p and 0 otherwise.

For ease of interpretation, we normalize the MD vote share to the standard deviation across judicial districts. Consistent with increased enforcement, the data show that the

number of prosecuted cases feel more in districts with a higher baseline share of votes for MD: a one standard deviation increase in the 1988 MD vote share led to a reduction in the number of reported number of cases of 8 percent between 1992 and 2000 (not significant at conventional levels) and of 17 percent in the period since 2001.

As a concern remains that trends across areas with different vote shares for MD are correlated with trends in corruption for reasons other than stricter enforcement in columns (2) to (5) of Table 8 we present similar regressions with different dependent variables (see Appendix A.3 and Table A.5 for a description of these variables). In column (2) we report a regression where the dependent variable is total reported crimes per capita (in logs). As the Clean Hands campaign might have affected the selection of politicians or local economic activity, and this might have an independent effect on the spread of corruption, in columns (3) to (5) we report regressions where the dependent variable is, in turn, the fraction of incumbent mayors, the fraction of mayors who are from the incumbent party, and the (log) value added per capita. None of these variables follow trends that are correlated with the pre-campaign share of MD votes in that area. In sum, these regressions are suggestive of the treatment not capturing or producing effects along other relevant confounding margins.

In columns (6) to (8) we finally report regressions where the dependent variable is a measure of nepotistic hiring. In particular, we estimate model (3.1) which by augment by including the three-way interaction between the cumulative number of individuals having entered office since 1985, the 1988 MD vote share in district d where the municipality of birth m is located, and dummies for the three sub-periods. All regressions include additionally the interaction between the cumulative number of individuals having entered office since 1985 and sub-period dummies, the interaction between the 1988 MD vote share and sub-periods dummies plus all other controls as in equation (3.1). In formulas:

$$y_{iFt} = \beta_0 + \Sigma_p \beta_{1p} P_{Ft}\ MD_d\ I_{tp} + \Sigma_p \beta_{2p} P_{Ft}\ I_{tp} + \Sigma_p \beta_{3p} MD_d\ I_{tp} + X'_{it}\beta_3 + \mu_i + \lambda_t + u_{iFt} \qquad F \in d \tag{7.2}$$

The coefficients on the three-way interaction terms, β_{1p}, provide a measure of how the returns to political connections vary differentially over time as a function of the 1988 MD vote share. Focusing on earnings, in column (6), it appears that, while there are no appreciable differences across areas in the pre Clean hands periods, the returns to political connections grow in the subsequent periods, and more so in areas where MD was stronger. Magnitudes are also high: a one standard deviation increase in the MD vote share leads to a rise in the

incidence of nepotistic hiring of between 44 and 12 percent (40 additional euros in the 1990s and 25 euros in the 2000s, relative to a baseline effect of 118 euros in Table 1). Similar results emerge for the number of months of work and employment, although estimates are small not statistically significant for the last sub-period.

In sum, this section provides suggestive evidence in favor of a rationale for nepotistic hiring: when monetary bribing and grafting become more costly, both private firms and officials might prefer harder-to-detect technologies of rent appropriation. This evidence suggests that the availability of alternative forms of exchange between firms and politicians may reduce the effectiveness of monitoring as a tool to contrast corruption (Olken and Pande 2012).

8 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, and it is difficult to establish if these practices are ascribable to a *quid-pro-quo* exchange between politicians and firms.

We show that, while in office, politicians are able to extract significant rents in terms of private sector jobs for their family members. Our back-of-the-envelope calculations imply that holding political office leads to returns among family members on the order of 10,000 euros worth of private sector earnings per year and 4 months of work per year. Our calculations also suggest that jobs acquired through nepotism account for at least 0.4 percent of private sector employment in Italy. These numbers 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. They also only refer to family members born in the same municipality and with the same F3C and clearly exclude relatives born elsewhere or those with a different last name (and hence F3C), including affinal relatives, as well as nepotistic hiring in the public sector. In this sense, these are likely to provide a lower bound for the true effect of nepotism.

We speculate that the phenomenon that we uncover is the result of an exchange between firms and politicians. 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 indeed a technology that helps politicians monetize over

the rents that accrue to office.

The question arises as to why firms resort to nepotistic hiring in exchange for what we claim as being political favors. We speculate and present suggestive evidence in favor of the hypothesis that nepotism is a - potentially inferior - substitute for grafting and monetary bribes: when these are costly, due to high rates of detection, both firms and officials will shift towards harder-to-detect technologies of rent appropriation.

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Table 1: Main estimates: number of family members in office and individuals' labor market outcomes

	(1)	(2)	(3)
	Dep. variable: Yearly earnings		
Politicians	439.954***	99.126***	
	(62.005)	(13.817)	
Politicians entry			117.951***
			(14.446)
Politicians exit			-2.615
			(11.708)
	Dep. variable: Months of work in the year		
Politicians	0.106***	0.035***	
	(0.017)	(0.004)	
Politicians entry			0.041***
			(0.005)
Politicians exit			-0.007
			(0.004)
	Dep. variable: Employment		
Politicians	0.010***	0.003***	
	(0.001)	(0.000)	
Politicians entry			0.003***
			(0.000)
Politicians exit			-0.001
			(0.000)
Munic. birth X F3C FE		Yes	Yes
Prov. birth X year FE		Yes	Yes
Indiv. controls		Yes	Yes
Indiv. FE		Yes	Yes

Notes. Columns (1) and (2) report the estimated coefficients from regressions of individual labor market outcomes on the number of individuals currently in office by F3C and municipality of birth (equation (3.1)). Column (3) reports separate coefficients on the cumulative number of individuals by F3C and municipality of birth having entered and exited office since 1985. Specifications in columns (2) and (3) include individual fixed effects, the interaction between province of birth and year dummies and workers' age (in ten year groups). The top panel refers to yearly earnings, the middle panel to months of work during the year and the bottom panel to a dummy for at least one employment spell during the year. Standard errors clustered by municipality of birth in brackets. Sample is restricted to observations with at most 30 individuals with a given municipality of birth and F3C in the INPS sample. Number of observations: 16,584,152. ***, **, *: denote significant at 1, 5 and 10 percent level, respectively.

Table 2: Heterogeneous effects by jobs, workers and politicians' characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
		By workers' occupation		By workers' age	By workers' gender	By politicians' age and gender (young workers)
	Blue collar	White collar	Manager			
	Dep. variable: Yearly earnings					
Politicians entry	53.959***	35.058***	24.436***			
	(6.306)	(9.710)	(7.725)			
Politicians entry X workers 18-35				472.732***		
				(22.043)		
Politicians entry X workers 36-55				52.161***		
				(17.277)		
Politicians entry X workers 56-65				-343.162***		
				(46.749)		
Politicians entry X male workers					164.548***	
					(22.216)	
Politicians entry X female workers					57.119***	
					(13.544)	
Male politicians 18-35 entry						123.257***
						(22.734)
Male politicians 36-55 entry						55.099**
						(21.854)
Male politicians 56-65 entry						128.748***
						(32.916)
Female politicians 18-35 entry						107.792**
						(41.852)
Male politicians 36-55 entry						73.911
						(51.509)
Female politicians 56-65 entry						117.304
						(76.613)
Avg. dep. variable	4,638	4,265	510			
	Dep. variable: Months of work entry the year					
Politicians entry	0.035***	0.005	0.001**			
	(0.004)	(0.003)	(0.001)			
Politicians entry X workers 18-35				0.203***		
				(0.011)		
Politicians entry X workers 36-55				-0.010**		
				(0.005)		
Politicians entry X workers 56-65				-0.073***		
				(0.009)		
Politicians entry X male workers					0.053***	
					(0.006)	
Politicians entry X female workers					0.025***	
					(0.006)	
Male politicians 18-35 entry						0.065***
						(0.012)
Male politicians 36-55 entry						0.014
						(0.011)
Male politicians 56-65 entry						0.059***
						(0.016)
Female politicians 18-35 entry						0.044**
						(0.020)
Female politicians 36-55 entry						0.019
						(0.024)
Female politicians 56-65 entry						0.051
						(0.037)
Avg. dep. variable	3.00	1.76	0.06			
	Dep. variable: Employment					
Politicians entry	0.003***	0.000	0.000**		0.004***	0.002***
	(0.000)	(0.000)	(0.000)		(0.001)	(0.001)
Politicians entry X workers 18-35				0.017***		
				(0.001)		
Politicians entry X workers 36-55				-0.001		
				(0.000)		
Politicians entry X workers 56-65				-0.006***		
				(0.001)		
Politicians entry X male workers					0004***	
					(0.001)	
Politicians entry X female workers					0.002***	
					(0.001)	
Male politicians 18-35 entry						0.006***
						(0.001)
Male politicians 36-55 entry						0.001
						(0.001)
Male politicians 56-65 entry						0.005***
						(0.002)
Female politicians 18-35 entry						0.003
						(0.002)
Female politicians 36-55 entry						0.002
						(0.002)
Female politicians 56-65 entry						0.006*
						(0.003)
Avg. dep. variable	0.32	0.16	0.01			

Notes.The table reports estimates of the coefficients on the cumulative number of individuals by F3C and municipality of birth having entered office since 1985. Columns (1) to (3) refer to regression where the dependent variables are outcomes in each separate occupation. Specifications in columns (4) and (5) allow the effect of the regressor to vary by worker's age and gender, respectively. Specifications in column (5) also include interaction of all controls with a gender dummy. Specifications in column (6) restrict to workers aged 18-35 and presents separate coefficients by politicians' age and gender. Regressions include the same controls as in column (3) of Table 1, including cumulative exists, and, where applicable, the interaction of this variable with the relevant workers and politicians' characteristics. Number of observations: 16,584,152 in columns (1) to (5) and 7,728,309 in column (6). See also text for details and notes to Table 1.

Table 3: Robustness checks

	(1)	(2)	(3)	(4)	(5)
	Ever connected workers only	Add individual linear trends	Add 8-year FE	Add 4-year FE	Add 2-year FE
	Dep. variable: Yearly earnings				
Politicians entry	90.999***	117.440***	115.877***	98.587***	79.582***
	(14.336)	(14.788)	(10.995)	(9.634)	(10.553)
	Dep. variable: Months of work in the year				
Politicians entry	0.035***	0.041***	0.055***	0.047***	0.028***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
	Dep. variable: Employment				
Politicians entry	0.003***	0.003***	0.005***	0.004***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)

Notes. The table reports estimates of the coefficients on the cumulative number of individuals by F3C and municipality of birth having entered office since 1985. All specifications include the same controls as in column (3) of Table 1. Column (1) refers to workers with at lest one "family" member in office during the period of observation. Column (2) includes the interaction of individual fixed effects with a linear time trend. Columns (3) to (5) include the interaction between individual fixed effects and dummies for 8-years (1985-1992, 1993-2010, etc.), 4-years (1985-1989, 1990-1994, etc.), and 2-years (1985-1986, 1987-1988 etc.) periods, respectively. Number of observations: 8,974,990 in column (1) and 16,584,152 in columns (2) to (5). See also notes to Table 1.

Table 4: Heterogeneous effects by frequency of F3C and municipality of birth

	(1)	(2)	(3)	(4)	(5)
	Rescaled by	Frequency			
	frequency	1	2-5	6-30	> 30
	Dep. variable: Yearly earnings				
Politicians entry (per capita)	207.801***				
	(34.489)				
Politicians entry		131.497***	87.506***	33.828**	-11.020
		(42.926)	(20.865)	(15.953)	(10.577)
	Dep. variable: Months of work in the year				
Politicians entry (per capita)	0.074***				
	(0.013)				
Politicians entry		0.050***	0.034***	0.009*	0.000
		(0.017)	(0.008)	(0.005)	(0.003)
	Dep. variable: Employment				
Politicians entry (per capita)	0.005***				
	(0.001)				
Politicians entry		0.003**	0.003***	0.001	0.000
		(0.002)	(0.001)	(0.000)	(0.000)

Notes. Column (1) reports estimates of the coefficients on the ratio between the cumulative number of individuals having entered office by F3C and municipality of birth and the sample frequency of each F3C in each municipality as derived from the INPS data (equation 3.2). Columns (2) to (5) report estimates of the coefficient on the ratio between the cumulative number of individuals having entered office by F3C and municipality of birth, separately by classes of frequencies. All regressions include the same controls as in column (3) of Table 1. Number of observations: 18,474,574, 4,970,470, 6,920,962, 4,692,720 and 1,890,422 respectively in columns (1) to (5). See also notes to Table 1.

Table 5: Heterogenous effects by local discretionary spending

	(1)	(2)	(3)	(4)	(5)	(6)
	Yearly earnings	Yearly earnings	Months of work	Months of work	Employment	Employment
Politicians entry X Spending	58.932***	85.985**	0.024***	0.026	0.002***	0.002
	(10.544)	(42.696)	(0.005)	(0.017)	(0.000)	(0.002)
Additional controls	No	Yes	No	Yes	No	Yes

Notes. The table reports estimates of coefficients on the interaction between the cumulative number of individuals by F3C and municipality of birth having entered office since 1985 and log local discretionary spending per politician (equation 6.1). Regressions refer to municipal politicians only. Specifications in odd-numbered columns include the same controls as in column (3) of Table 1 plus the interaction between cumulative exits and log discretionary spending. Specifications in even-numbered columns include in addition the interaction of cumulative entries and exits with the following municipal time-invarying municipality level controls: log income per capita, log number of firms per capita, fraction of workers in the public sector and local unemployment rate, fraction of 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, dummies for whether the 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, 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. For sources, definitions and descriptive statistics see Appendix A.3 and Table A.4. See also notes to Table 1.

Table 6: Heterogeneous effects by office and politicians' characteristics

	(1)	(2)	(3)
	By tenure	By level of government	By level of office
	Dep. variable: Yearly earnings		
Politicians entry 1 Term	103.906***		
	(12.999)		
Politicians entry 2 Terms	10.333		
	(30.240)		
Politicians entry > 2 Terms	107.914**		
	(54.689)		
Politicians entry Municipal		118.856***	
		(14.801)	
Politicians entry Provincial		98.942	
		(62.491)	
Politicians entry Regional		163.226	
		(116.994)	
Politicians entry National		12.092	
		(162.943)	
Politicians entry Council			117.049***
			(14.756)
Politicians entry Executive			102.978***
			(33.908)
	Dep. variable: Months of work in the year		
Politicians entry 1 Term	0.040***		
	(0.005)		
Politicians entry 2 Terms	0.005		
	(0.009)		
Politicians entry > 2 Terms	0.023		
	(0.019)		
Politicians entry Municipal		0.040***	
		(0.005)	
Politicians entry Provincial		0.074***	
		(0.022)	
Politicians entry Regional		0.025	
		(0.047)	
Politicians entry National		0.069	
		(0.054)	
Politicians entry Council			0.043***
			(0.005)
Politicians entry Executive			0.034***
			(0.010)
	Dep. variable: Employment		
Politicians entry 1 Term	0.003***		
	(0.000)		
Politicians entry 2 Terms	0.000		
	(0.001)		
Politicians entry > 2 Terms	0.003		
	(0.002)		
Politicians entry Municipal		0.003***	
		(0.000)	
Politicians entry Provincial		0.008***	
		(0.002)	
Politicians entry Regional		0.003	
		(0.004)	
Politicians entry National		0.006	
		(0.005)	
Politicians entry Council			0.003***
			(0.000)
Politicians entry Executive			0.003***
			(0.001)

Notes. The table reports estimates of the coefficients on the interaction between the cumulative number of individuals by F3C and municipality of birth having entered office since 1985 and office/politicians' characteristics. Regressions include the same controls as in column (3) of Table 1, plus the interaction of cumulative exists with the relevant politicians' characteristics. Regressions in column (1) additionally include the number individuals in office in 1985 by F3C and municipality of birth. Regressions in column (3) additionally include exits and entries from and to other administrative levels. See also notes to Table 1.

Table 7: Heterogenous effects across sectors with different levels of Government dependence

	(1)	(2)	(3)
	Yearly earnings	Months of work	Employment
Politicians entry X Sector government dependence	1.106***	0.096	0.008
	(0.382)	(0.117)	(0.013)

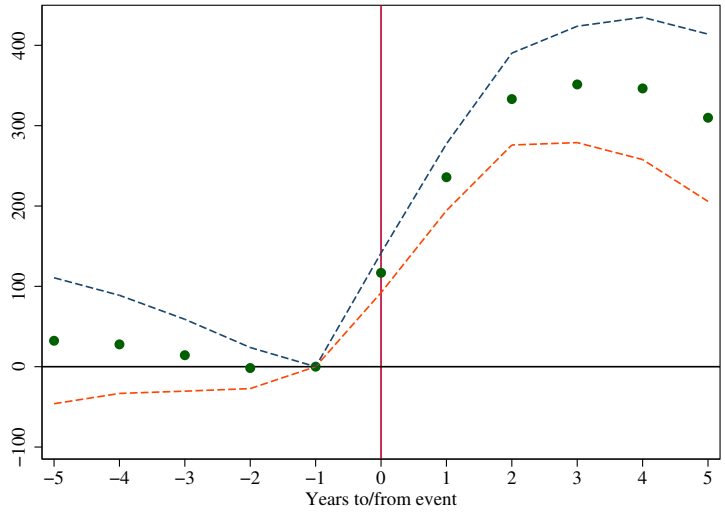
Notes. The table reports estimates of the effect of government dependence by industrial sector on the extent of nepotistic hiring (equation 6.3). Dependent variables are the coefficients from a regression of each dependent variable on the number of individuals by F3C and municipality of birth having entered office since 1985 separately estimated for each ATECO-81 2-digit sector using (equation 6.2). Method of estimation: GLS, with weights equal to the square of the reciprocal of the standard error of each coefficient. See also text for details. Standard errors clustered by twenty-five industrial sectors in Pellegrino and Zingales (2014) in brackets. Number of observations 50. ***, **, *: denote significant at 1, 5 and 10 percent level, respectively.

Table 8: Heterogenous effects across districts with different baseline MD vote share and sub-periods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable:	Crimes against PA (log)	Total crimes (log)	Same mayor	Same party	Value added (log)	Yearly earnings	Months of work	Empl.
Politicians entry X MD X 1985-91	0.040	0.105	-0.018	-0.012	0.627***	18.618	0.006	-0.000
	(0.072)	(0.069)	(0.025)	(0.030)	(0.138)	(23.352)	(0.009)	(0.001)
Politicians entry X MD X 1992-00	-0.084	0.083	0.033	0.022	0.620***	39.706***	0.020***	0.002***
	(0.072)	(0.069)	(0.025)	(0.030)	(0.139)	(13.286)	(0.005)	(0.000)
Politicians entry X MD X 2001-11	-0.172**	0.049	0.007	0.026	0.695***	25.527*	0.004	0.000
	(0.072)	(0.069)	(0.025)	(0.030)	(0.139)	(13.739)	(0.005)	(0.000)

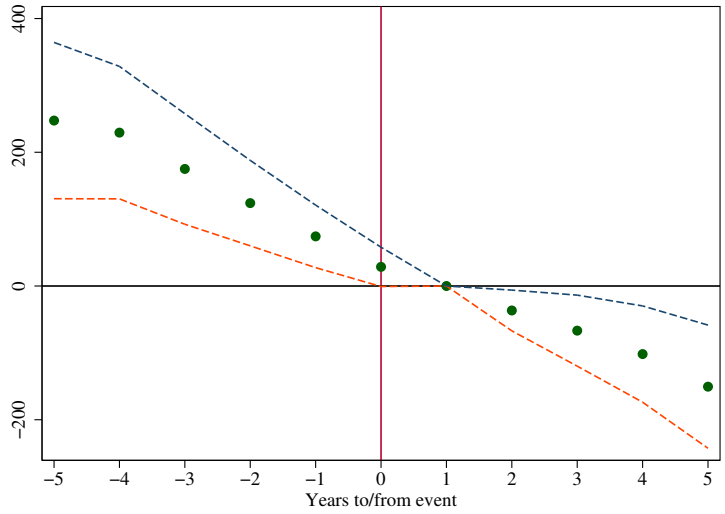
Notes. Columns (1) to (5) report estimates of the effect of the 1988 elections MD (*Magistratura Democratica*) vote share in each judicial district separately by sub-period (1985-1991, 1992-2000, 2001-2011) (equation (7.1). All regressions include sub-period dummies. Dependent variables are: log crimes against the public administration per capita (column 1), log total crimes per capita (column 2), population weighted fraction of municipalities where mayor is an incumbent or from the incumbent mayor's party (columns 3 and 4), log value added per capita (column 5). Number of observations in columns (1) to (5): 78. Method of estimation in columns (1) to (5): GLS, with weights equal to the district population. Columns (6) to (8) reports estimated coefficients on the three way interaction between the number of individuals by F3C and municipality of birth having entered office since 1985, the 1988 MD vote share and dummies for the three sub-periods (equation (7.2). Regressions include additionally the interaction between the cumulative number of individuals having entered office since 1985 and sub-period dummies, the interaction between the 1988 MD vote share and sub-periods dummies, all other controls as in column (3) of Table 1, plus two- and three-way interactions between cumulative exits, the 1988 MD vote share and dummies for the three sub-periods. Column (6) refers to yearly earnings, column (7) to months of work, and column (8) to employment. See also notes to Table 1.

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



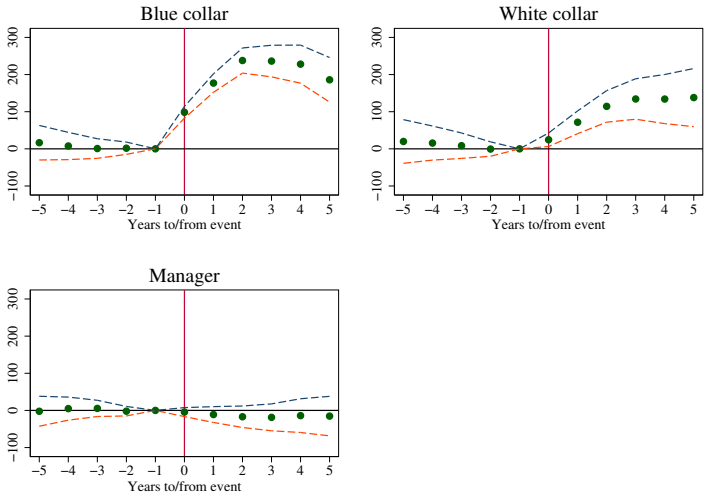
Notes. The figure displays estimated change in yearly earnings at different lags and leads since the time of first entry (denoted by a vertical line). All coefficients expressed relative to effect in year before entry. 95 percent confidence intervals reported. See also text for details.

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



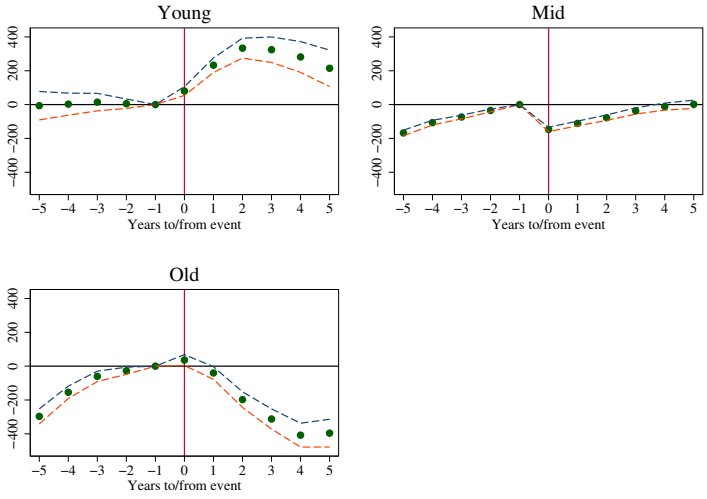
Notes. The figure displays estimated change in yearly earnings at different lags and leads since the time of last exit (denoted by a vertical line). All coefficients expressed relative to effect in year after exit. 95 percent confidence intervals reported. See also text for details.

Figure 3: Event-study analysis: Yearly earnings by occupation - entry



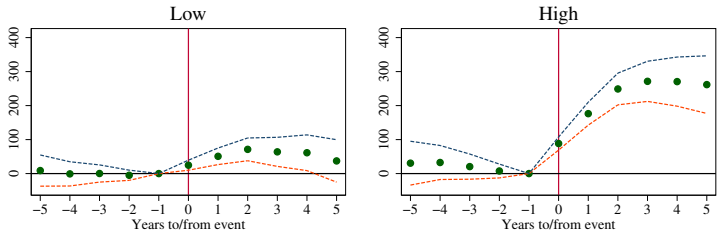
Notes. The figure reports the same coefficients as in Figure 1 separately by occupation: blue collars (top left), white collars (top right) and managers (bottom left). See also notes to Figure 1.

Figure 4: Event-study analysis: Yearly earnings by workers' age - entry



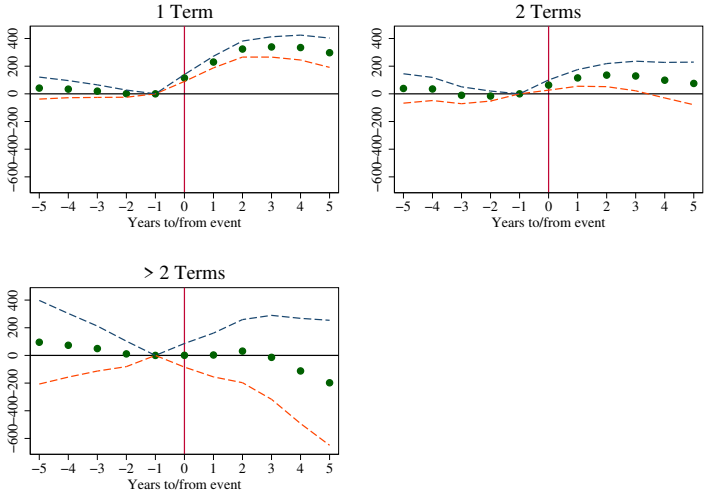
Notes. The figure reports the same coefficients as in Figure 1 separately by workers' age intervals: 18-35 (top left), 36-55 (top right), 56-55 (bottom left). See also notes to Figure 1.

Figure 5: Event-study analysis: Yearly earnings by discretionary spending - entry



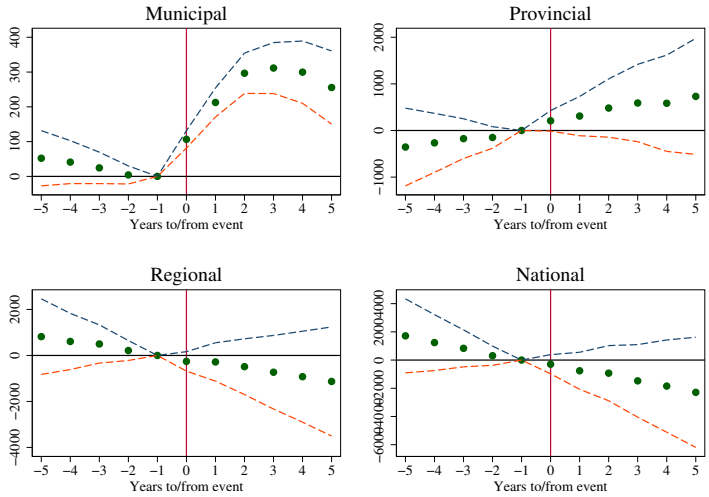
Notes. The figure reports the same coefficients as in Figure 1 separately by level of (per politician) discretionary spending: below median (left), above median (right). Estimates refer to municipal politicians only. See also notes to Figure 1.

Figure 6: Event-study analysis: Yearly earnings by tenure - entry



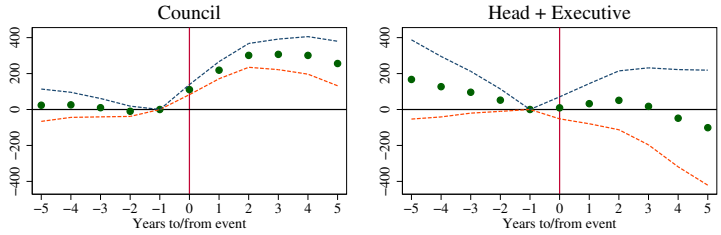
Notes. The figure reports the same coefficients as in Figure 1 separately by consecutive terms in office: first term (top left), second term (top right), more than two terms (bottom left). See also notes to Figure 1.

Figure 7: Event-study analysis: Yearly earnings by level of government - entry



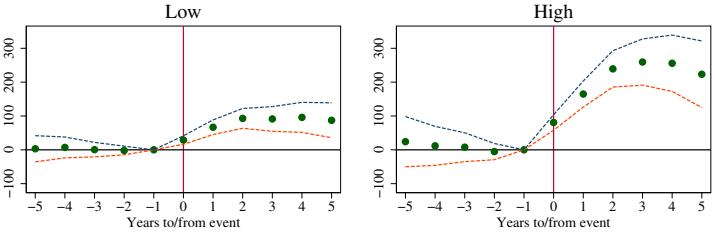
Notes. The figure reports the same coefficients as in Figure 1 separately by level of government: municipal (top left), provincial (top right), regional (bottom left) and national (bottom right). See also notes to Figure 1.

Figure 8: Event-study analysis: Yearly earnings by level of office - entry



Notes. The figure reports the same coefficients as in Figure 1 separately by levels of office: council (left) and executive (right). See also notes to Figure 1.

Figure 9: Event-study analysis: Yearly earnings by government dependence - entry



Notes. The figure reports the same coefficients as in Figure 1 separately by levels of government dependence as mesured by Pellegrino and Zingales (2014): below median (left)and above median (right). See also notes to Figure 1.

A Appendix

A.1 Measurement error

Suppose that our true model is:

$$y_{iFt} = \beta_0 + \beta_1 P_{it} + u_{iFt}$$

where for simplicity we have omitted the other regressors, including the fixed effects. As said, in the data we only have an imperfect measure of family connections. In particular, we can only identify politicians carrying the same F3C and born in the same municipality as a worker, denote this by P_{Ft}. This provides an error-ridden measure of P_{it}, the actual number of politicians related to individual i. In formulas, we only observe:

$$P_{Ft} = \sum_j s_{iFj} pol_{jt}$$

where pol_{jt} is a dummy if individual j is in office in year t and s_{iFj} is a dummy equal one if individuals i and j share the same F3C and are born in the same municipality. It follows that:

$$P_{Ft} = P_{it} + \nu_{iFt}$$

where:

$$\nu_{iF} = \sum_j (s_{iFj} - d_{iFj}) pol_{jt}$$

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

$$y_{iFt} = \beta_0 + \beta_1 P_{Ft} + v_{iFt}$$

where $v_{iFt} = u_{iFt} - \beta_1 \nu_{iFt}$.

From the above one can derive the implied bias in the estimate of β_1 in this last equation. Assuming that s_{iFj} and d_{iFj} are independent across j's, this estimate converges in probability to $k\beta_1$, where:

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

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 is either type-1, $Pr(s = 0|d = 1)$, or type-2, $Pr(s = 1|d = 0)$, error. Since k varies between -1 and 1, estimates of the parameter converge in probability to a value between $-\beta_1$ and β_1. The intuition for this is straightforward. Type-1 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_1. 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_1 will be reverted.

The size of both errors will depend on the distribution of F3Cs in a municipality and there is a clear trade-off between the two. As type-1 error is likely to be negligible, as truly connected individuals will represent a negligible share of all those with a different F3C, a simplified expression for k is:

$$k \approx Pr(d = 1|s = 1) \tag{A.1}$$

i.e., the probability that an individual with the same F3C and municipality of birth as a politician is a true family member. This probability has an empirical counterpart, $E\left(\frac{D_{Ft}}{N_{Ft}}\right)$, where N_{Ft} is the number of individuals in "family" F (i.e with a given F3C and born in a given municipality) and D_{Ft} is the total number of individuals genuinely related to politicians among them.

A.2 Selection bias

One concern in relation to the estimates of model (3.1) arises from 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 illustrate the source of bias (and ignoring the issue of measurement error), let us start from our model in equation (3.1) and let us assume that $E(v_{iFt}|\mathbf{P}_F) = 0$, where $\mathbf{P}_F = (P_{F1}, P_{F2}, ..., P_{FT})$:

$$y_{iFt} = \beta_0 + \beta_1 P_{Ft} + v_{iFt} \tag{A.2}$$

Let $A_i = \{Max_{t=1,..T}(y_{iFt}) > 0\}$ define the event that determines inclusion in the sample, with the associated complementary event $B_i = \{y_{iF1} < 0, y_{iF2} < 0, .., y_{iFT} < 0\}$, such that $Pr(A_i = 1|\mathbf{P}_F) = 1 - Pr(B_i = 1|\mathbf{P}_F)$.

Let:

$$W_{iF} = \frac{Pr(B_i = 1|\mathbf{P}_F)}{1 - Pr(B_i = 1|\mathbf{P}_F)}$$

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

$$E(y_{iFt}|A_i = 1, \mathbf{P}_F) = \beta_0 + \beta_1 P_{Ft} + h_{iFt}$$

where $h_{iFt} = -E(v_{iFt}|B_i = 1, \mathbf{P}_F)W_{iF}$ and we have exploited the fact that:

$$E(v_{iFt}|A_i = 1, \mathbf{P}_F) = -E(v_{iFt}|B_i = 1, \mathbf{P}_F)W_{iF}$$

which follows from the assumption that $E(v_{iFt}|\mathbf{P}_F) = 0$. Assuming independence of v_{iFt} across time within individuals, it follows:

$$h_{iFt} = -E(v_{iFt}|v_{iFt} < -\beta_0 - \beta_1 P_{Ft}, \mathbf{P}_F) \left(\frac{\Pi_s Pr(v_{iFs} < -\beta_0 - \beta_1 P_{Fs}|\mathbf{P}_F)}{1 - \Pi_s Pr(v_{iFs} < -\beta_0 - \beta_1 P_{Fs}|\mathbf{P}_F)} \right)$$

Although the sign of the bias is indeterminate in the absence of further assumptions on the distribution of u, it is easy to show that the bias tends to disappear as T grows, as $\Pi_s Pr(v_{iFs} < -\beta_0 - \beta_1 P_{Fs}|\mathbf{P}_F)$, and hence W_{iF}, are likely to become small. 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_{iFt} in any given time period, and hence will not be included in the sample.

A.3 Municipality and Province characteristics

In this section we describe the municipal level and judicial district-level variables that we use in Table 5 (see also Table A.4 for descriptive statistics) and in Table 8 (see also Table A.5 for descriptive statistics).

Municipal variables:

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

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

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

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 (source: 2001 Italian General Census of Population and Housing).

Pct. college: percentage of the resident population six years old and over with a college degree or more as of 2011 (source: 2011 Italian General Census of Population and Housing). *Elderly index*: ratio of resident population above sixty-five over population below fourteen years old as of 2005 (source: Ministry of Interior).

Population: resident population as of 2001 (source: 2001 Italian General Census of Population and Housing).

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).

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

Crimes against public administration per capita: total number of crimes against the public

administration 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).

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

Local politicians: total number of available seats in the council and in the executive, average between 1985 and 2011 (source: Ministry of Interior). The number of elected municipal officials varies discontinuously with population size, 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).

Judicial district-level variables:

MD vote share 1988: vote share for *Magistratura democratica* in the 1988 election of the *Comitato Direttivo Centrale* of the ANM (source: Associazione Nazionale Magistrati).

Total crimes per capita: total number of crimes reported to the judiciary authority per 1,000 individuals, average by sub-periods (1985-1991, 1992-2000 and 2001-2005) and across districts (source: Istat).

Crimes against public administration per capita: total number of crimes against the public administration reported to the judiciary authority per 1,000 individuals, average by sub-periods (1985-1991, 1992-2000 and 2001-2005) and across districts (source: Istat).

Same mayor: elected mayor is an incumbent, average by sub-periosds (1985-1991, 1992-2000 and 2001-2011) and across districts obtained using city population size as weights (source: Ministry of Interior).

Same party: elected mayor is from the incumbent mayor's party, average by sub-periods (1985-1991, 1992-2000 and 2001-2011) and across districts obtained using city population size as weights (source: Ministry of Interior).

Value added per capita: value added per capita at province level, available for 1981, 1991 and 1999, average across districts obtained using province population size as weights (source: Istituto Guglielmo Tagliacarne).

Table A.1: Descriptive statistics, workers - employment spells

	Mean	s.d.
Months in work in the year	9.985	3.378
Yearly earnings	19,507.505	16,504.393
Number of jobs in the year	1.188	0.500
Female	0.327	0.469
Age	37.401	11.117
Area of birth: North	0.461	0.498
Area of birth: Center	0.173	0.378
Area of birth: South + Islands	0.366	0.482
Blue collar	0.639	0.480
Clerk	0.333	0.471
White collar	0.017	0.128
Manager	0.011	0.102
N. observations	9,440,711	
N. individuals	927,606	

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 A.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.

Table A.3: Descriptive statistics, matched sample

	Mean	s.d.
Months in work in the year	4.830	5.515
Employed	0.484	0.500
Yearly earnings	9,439.504	15,034.743
Total politicians	0.778	1.926
Total politician > 0	0.327	0.469
Total politicians $= 1$	0.169	0.375
Total politicians $= 2$	0.065	0.247
Total politicians > 2	0.093	0.291
Municipal politicians	0.728	1.824
Provincial politicians	0.025	0.160
Regional politicians	0.013	0.115
National politicians	0.013	0.118
Council politicians	0.565	1.440
Executive politicians	0.213	0.665
N. observations	19,011,083	
N. individuals	895,456	

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 A.1 and A.2.

Table A.4: 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)	-2.622	0.328
Pct. unemployment	12.263	6.072
Pct. public sector employment	9.718	9.029
Pct. college	7.394	2.705
Elderly index	185.257	149.927
Population (log)	7.945	1.252
Region capital	0.003	0.052
Province capital	0.016	0.126
CC station	0.477	0.500
PS station	0.044	0.204
GDF station	0.063	0.244
Court	0.019	0.137
Subsidiary court	0.034	0.180
Total 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)	1.491	0.607
Pct. voters' turnout	79.541	8.039
Politicians per capita (log)	-4.944	1.033

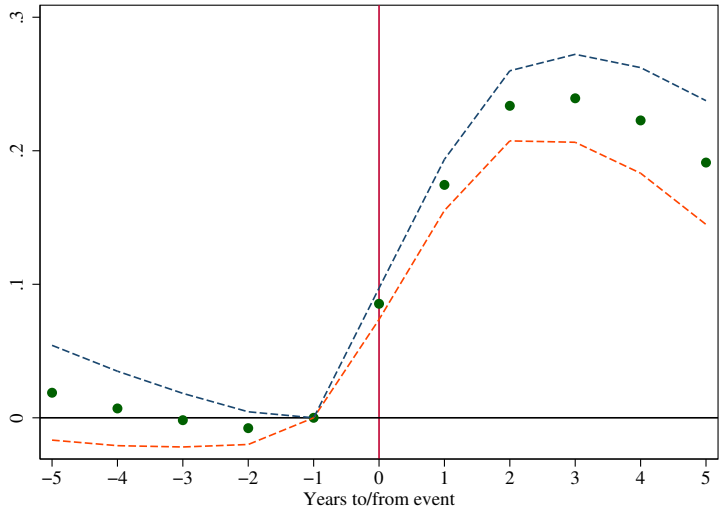
Notes. Number of observations: 6,245. See Section A.3 for a description of the variables and sources.

Table A.5: Judicial district characteristics

	Mean	s.d.
MD vote share 1988	0.186	0.102
Crimes against PA per (1,000) capita	0.035	0.021
Total crime per (1,000) capita	41.647	14.670
Same mayor	0.382	0.143
Same party	0.625	0.183
Value added per capita	65,297.096	56,524.209

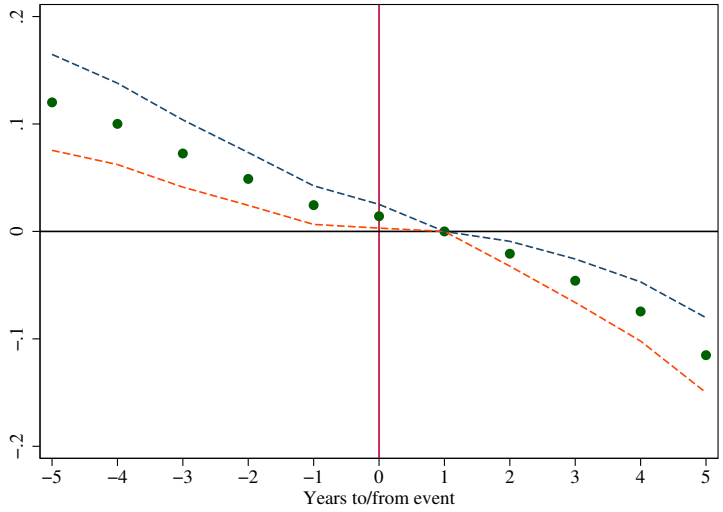
Notes. Number of observations: 78. See Section A.3 for a description of the variables and sources.

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



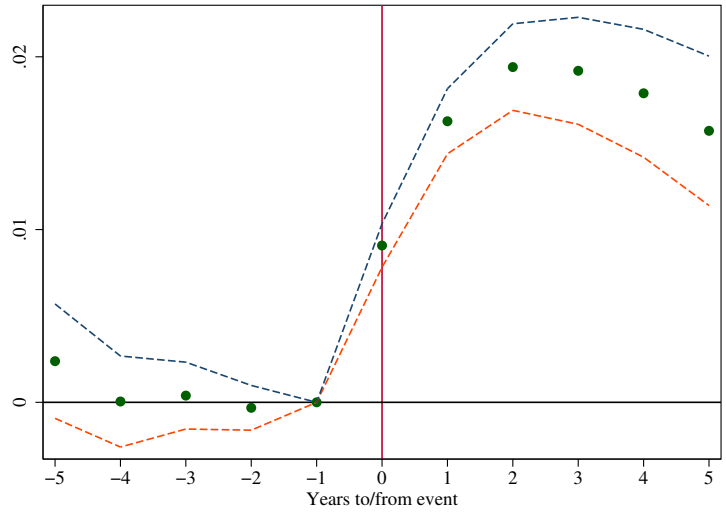
Notes. The figure displays the estimated change in number of months of work at different lags and leads since the time of first entry (denoted by a vertical line). All coefficients expressed relative to effect in year before entry. 95 percent confidence intervals reported. See also text for details.

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



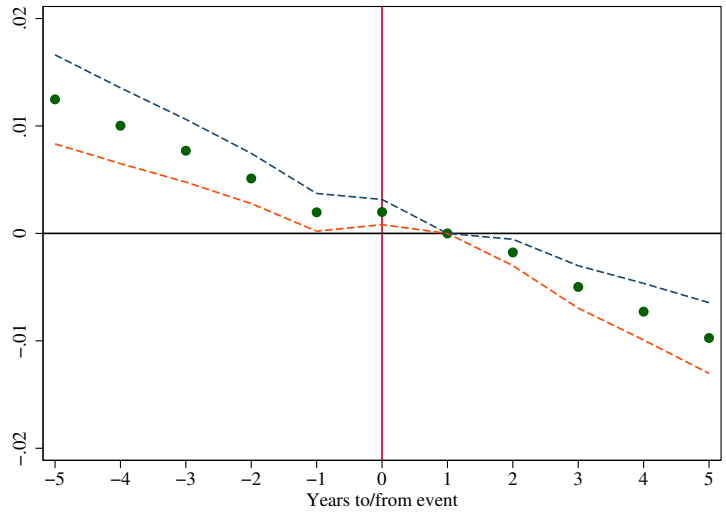
Notes. The figure displays the estimated change in number of months of work at different lags and leads since the time of last exit (denoted by a vertical line). All coefficients expressed relative to effect in year after exit. 95 percent confidence intervals reported. See also text for details.

Figure A.3: Event-study analysis: Employment - entry



Notes. The figure displays the estimated change in the employment probability 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 also text for details.

Figure A.4: Event-study analysis: Employment - exit



Notes. The figure displays the estimated change in the employment probability 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 also text for details.