American Economic Journal: Applied Economics 2020, 12(2): 67–95
https://doi.org/10.1257/app.20170778

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

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This paper studies the effect of family connections to politicians on individuals' labor market outcomes. Using data for Italy spanning more than three decades on a sample of almost one million individuals plus data on the universe of individuals holding political office, we show that politicians extract significant rents, in terms of private sector jobs, for their family members. We present evidence consistent with the hypothesis that this phenomenon is a form of corruption, i.e., a quid pro quo exchange between firms and politicians, although arguably an inferior substitute for easier-to-detect modes of rent appropriation on the part of politicians. (*JEL* D72, D73, J23, K42, M51, Z13)

This paper combines almost 30 years of micro data from Italy on 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 for politicians' family members, including in what are typically regarded as more mature democracies. The argument is that, in exchange for or in expectation of political favors, firms hire or promote politicians' relatives or grant them

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† Go to https://doi.org/10.1257/app.20170778 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

higher earnings.[1] Evidence, though, remains elusive. We bring this argument under empirical scrutiny using data from Italy and investigate the main determinants and correlates of this phenomenon.

Italy appears to be 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 as the root of the country's inability to modernize (Banfield 1958; Putnam, Leonardi, and Nonetti 1993; Pellegrino and Zingales 2017). In addition, widespread red tape and a cumbersome bureaucracy create opportunities for corruption, with the country ranking third from the bottom among OECD high-income countries on 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 (abbreviated as F3C) of an individual's last name and an identifier for their municipality of birth. We identify "families" on the basis of individuals who share the same F3C and are 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 compare yearly changes in labor market outcomes among individuals whose "family" members enter office with changes in labor market outcomes among otherwise similar individuals who do not experience such entry. Using 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 pretrends in labor market outcomes prior to a family member taking office and that effects manifest precisely in the year when 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. Not only does it fail to classify some connected individuals (those with different F3Cs) as family members but—more importantly—it erroneously classifies some unconnected individuals (those with the same F3C) as family

[1] Allegations of political nepotism against companies often surface in the press, including in the United States. One prominent recent case involves the Securities and Exchange Commission's allegations that "JPMorgan…hired the children of high-ranking Chinese officials to help win business (Sevastopoulo, Chon, and Braithwaite 2015).

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 nonsystematic measurement error.

We show that parameter estimates derived by 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 all 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 our sample of data. Based on this approach, our back-of-the-envelope calculations imply that individuals in office generate on average approximately an extra €10,000 worth of private sector earnings among their family members who carry the same last name and were 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 who have different last names or were 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 would expect this effect to be larger the larger the rents accruing to office. Consistent with this, and similar to the findings in Dal Bó, Dal Bó, and Di Tella (2006) and Brollo et al. (2013) 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 with different levels of office (executive versus legislative branch) or with the highest levels of government (regional versus national). Although this might indicate that greater rents are not always related to more nepotism, an alternative interpretation is that the greater public scrutiny associated with these offices limits 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 then 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 a cost-effective way for politicians to monetize over the rents that accrue to office, if disclosed, the payment of bribes will entail a cost, as this is per se evidence of misbehavior, whereas 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 toward more hidden, harder-to-detect forms of corruption.

In the final part of the paper we bring this argument under empirical scrutiny by exploiting the heterogeneous effect across judicial districts of a major anticorruption campaign against firms and politicians involved in payment and receipt of monetary bribes. Historiographical accounts of this campaign suggest that it was initiated by judges and prosecutors with close links to the left-wing faction of the *Associazione Nazionale Magistrati*. We compare changes in corruption cases prosecuted in each of the 26 judicial districts before and after this anticorruption campaign. Consistent with increased deterrence, we find a larger drop 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 the finding in Olken (2007) that increased corruption monitoring in Indonesia leads to less corruption but more 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, Persson, and Rickne (2017) uses Swedish register data to investigate the earnings of children of elected mayors. That paper finds 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) investigates the effect of family connections to local politicians in the Philippines and finds evidence that such connections have a positive effect on the probability of employment in better-paying occupations. However, that work cannot distinguish between private and public employment, leaving open the possibility that, similar to what was observed in Olken (2007), most of the effects found can be ascribed 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 during and after a politician's term in office (Merlo et al. 2010; Cingano and Pinotti 2013; Fisman, Schulz, and Vig 2014), including through the establishment of political dynasties (Dal Bó, Dal Bó, and Snyder 2009). At the extreme, politicians can profit from their position in order to engage in corruption and grafting, i.e., illegal activities connected to their office that yield a private utility. This happens either through sharing rents with colluding agents or through direct diversion of public resources for personal purposes (Olken 2007; Ferraz and Finan 2008; Banerjee, Hanna, and Mullainathan 2012; Olken and Pande 2012; Brollo et al. 2013).

Linking 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; Fisman 2001, Cingano and Pinotti 2013). These links appear to be more likely in more corrupt environments, providing indirect evidence that they might directly benefit politicians. Unlike 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 finds evidence indicating considerable intergenerational persistence in socioeconomic status, income, human capital, occupations (including political occupations), jobs, and even firm control (Bertrand and Schoar 2006; Dal Bó, Dal Bó, and Snyder 2009; Black and Devereux 2011; Durante, Labartino, and Perotti 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., Durante, Labartino, and Perotti 2011; Clark and Cummins 2014).

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, the assignment of jobs and the availability of opportunities according to one's name or contacts rather than one's talent might be detrimental to 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 socioeconomic status across generations might also create incentives to divert resources away from productive investment (such as human capital) toward rent-seeking activities (such as the preservation of family ties), to impede geographical mobility and risk-taking, and to 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 levels of economic development; and poorer quality of institutions, including reduced control of corruption (Alesina and Giuliano 2014).

The rest of the paper is organized as follows. Section I describes the data. Section II discusses the econometric model. Section III presents the main regression results. Section IV discusses the consequences of measurement error for our estimates. In Sections V and VI we investigate and discuss the determinants of nepotistic hiring. Section VII concludes.

I. Data

A. *Worker Data*

For the purpose of the empirical exercise, we use worker 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 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, totaling 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, or manager) and sector of activity at the two-digit level (50 categories). Unfortunately, other than an anonymous firm identifier, no additional information is available on the firm.

[2] Since the mid-1990s, a series of reforms have extended the mandate of INPS to include some categories of self-employed workers and public sector workers. Our data refer only to those originally included in the INPS fund. In practice, we exclude firms in the public sector (sector ATECO-81 = 90).

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 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 one 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 highest-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 politician dataset (see next section). Note that the INPS data do not allow us to identify family ties such as spouses, parents, and children. As every individual autonomously contributes to his/her social security account, there is no concern that we are confounding spouses' incomes. 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 (around US$21,000), and workers work on average ten calendar months (see Table A.1 in the online Appendix).

B. *Politician 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, subnational, or national—whether elected or appointed and whether in the legislative or the executive branch.

In addition to 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, level of government, council versus executive position, dates of assuming and leaving office (with the former left-censored to January 1, 1985, and the latter right-censored to December 31, 2011), the data also provide information on municipality of birth and on the last name, and hence the F3C. Nearly all 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; if an individual simultaneously holds office in different governments (e.g., a mayor also sitting in parliament), we designate it 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 the online Appendix). 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 are in the executive branch.

C. *Matched Worker–Politician Data*

In the empirical analysis that follows we focus on the sample of individuals who, over the 27 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 worker 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 $\times$ individual observations (see Table A.2 in the online Appendix). The individuals in our sample account for around 2 percent of the working-age population in each year.

II. 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 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. Let $\mathbf{X}$ denote a vector of individual characteristics. The regression model is

$$y_{iFt} \;=\; \beta_0 + \beta_1 P_{Ft} + \mathbf{X}'_{it}\beta_2 + \mu_i + \lambda_t + u_{iFt}, \tag{1}$$

where β_1 is the additional outcome that each politician generates for 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 into and 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 with the estimation of the parameter of the model is that we have no information on actual family ties; instead we have information 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 have only 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 online Appendix A1) that the ordinary least squares estimate of the parameter β_1 is attenuated by a factor $0 < k = E(D_{Ft}/N_{Ft}) < 1$, 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. 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 on the basis of the frequency distribution 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,

$$y_{iFt} = \theta_0 + \theta_1\left(\frac{P_{Ft}}{N_{Ft}}\right) + \mathbf{X}_{it}'\theta_2 + \mu_i + \lambda_t + e_{iFt}. \tag{2}$$

From the above, the ordinary least squares 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.

III. Model Estimates

We start by presenting estimates of model (1). As stated, these are conservative estimates of the parameter of interest. For most of the analysis, we exclude workers with a frequency greater than 30 of their F3C in their municipality of birth in the INPS data; this represents the ninetieth 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 (2).

A. *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 into/from office. This allows us to examine potential pretrends 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 into office over the period—greatly complicating the analysis—for

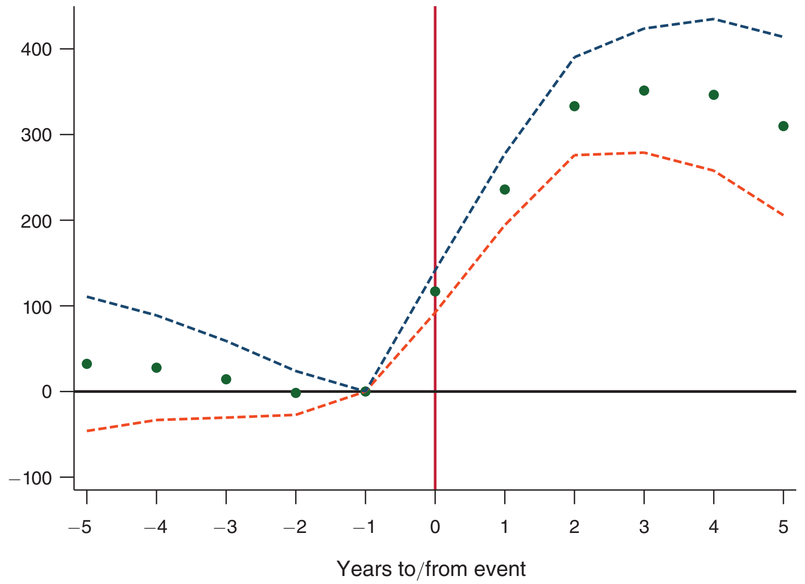

FIGURE 1. EVENT-STUDY ANALYSIS: YEARLY EARNINGS—ENTRY

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 are expressed relative to the effect in the year before entry; 95 percent confidence intervals are reported. See text for details.

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 an 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:

$$(3) \qquad y_{iFt} = \beta_0^{in} + \sum_{t=t_1-5}^{t_1+5} \beta_{1,t-t_1}^{in} P_{Ft_1}^{in} + \mathbf{X}_{it}'\beta_2 + \mu_i + \lambda_t + v_{iFt}^{in},$$

where $P_{Ft_1}^{in}$ is the cumulative number of individuals from "family" F who 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 identify only 10 coefficients out of 11, 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 online 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

[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.

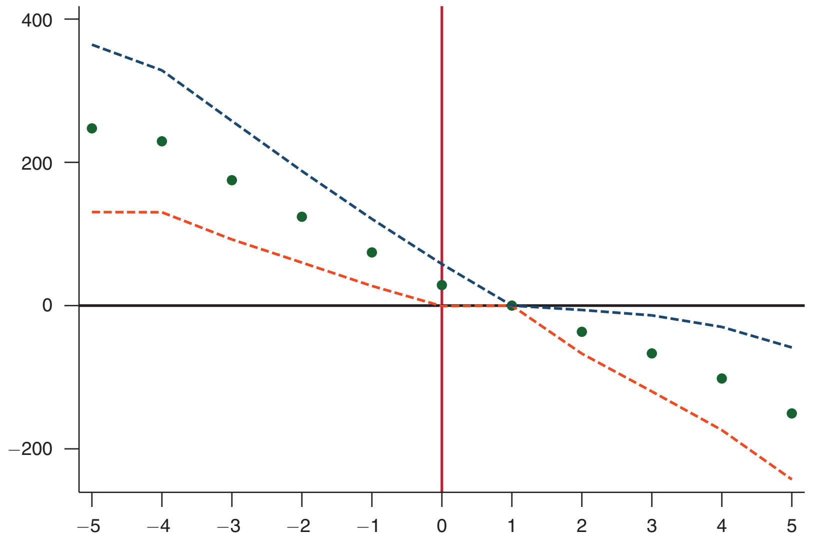

Figure 2. Event-Study Analysis: Yearly Earnings—Exit

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 are expressed relative to the effect in the year after exit; 95 percent confidence intervals are reported. See text for details.

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., toward 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.

Unlike what we found for entries, there is evidence of a deterioration in outcomes predating the time of exit, which continues after the time of exit. 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 in an election is somewhat known in advance. Because of this, our identification strategy—which is based on a pre–post comparison around 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 in office induced by entries only.

B. *Main Estimates*

Having presented evidence from the event studies, in Table 1 we report regression estimates of model (1). Each panel refers to a different dependent variable (earnings during the year, months of work in the year, and a dummy for at least

Table 1—Main Estimates: Number of Family Members in Office and Individuals' Labor Market Outcomes

	(1)	(2)	(3)
Panel A. Dependent variable: Yearly earnings			
Politicians	439.954	99.126	
	(62.005)	(13.817)	
Politicians entry			117.951
			(14.446)
Politicians exit			−2.615
			(11.708)
Panel B. Dependent 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)
Panel C. Dependent variable: Employment			
Politicians	0.010	0.003	
	(0.001)	(0.000)	
Politicians entry			0.003
			(0.000)
Politicians exit			−0.001
			(0.000)
Municipality birth × F3C FE		Yes	Yes
Province birth × year FE		Yes	Yes
Individual controls		Yes	Yes
Individual 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 (1)). Column 3 reports separate coefficients on the cumulative number of individuals by F3C and municipality of birth who entered and exited office since 1985. Specifications in columns 2 and 3 include individual fixed effects (FE), the interaction between province of birth and year dummies, and workers' age (in ten-year groups). Panel A refers to yearly earnings, panel B to months of work during the year, and panel C to a dummy for at least one employment spell during the year. Standard errors clustered by municipality of birth are in parentheses. The 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.

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 probabilities of being in office and in work/earnings are likely correlated across families. For this reason, in column 2 and much like in 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 area) fixed effects to account for generalized differences in local labor market outcomes.[4] Finally, in

[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

column 3 we report separate coefficients on cumulative inflows and outflows, P^{in}_{Ft} and P^{out}_{Ft}. Note that, relative to the specification in column 3, the specification in column 2 constrains 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 we compare estimates in columns 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. If we focus on the specification in column 2, this suggests that one politician in office increases yearly earnings among all individuals who have the same F3C and were born in the same municipality by €99 (a 1 percent increase relative to a baseline earnings level of around €9,500), months of work by 0.035 (a 0.7 percent increase relative to a baseline value of 4.5), and 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); there is no evidence that exits are systematically associated with 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., the one including individual fixed effects, time $\times$ 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 to increases in months of work conditional on working. It also appears that the effects are largely due to increases in employment rather than to 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 from among those with higher-than-average earnings potential.

C. *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, therefore, for each occupation we create a separate outcome variable. If an individual is not employed in a certain occupation at time t (because the individual either is not employed at all or is employed in another occupation), the outcome variable

online Appendix A2, where we argue that this concern is most likely second order.

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

Table 2—Heterogeneous Effects by Jobs, Workers, and Politicians' Characteristics

	By workers' occupation			By workers' age	By workers' gender	By politicians' age and gender (young workers)
	Blue collar	White collar	Manager			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: Yearly earnings						
Politicians entry	53.959	35.058	24.436			
	(6.306)	(9.710)	(7.725)			
Politicians entry × workers 18–35				472.732		
				(22.043)		
Politicians entry × workers 36–55				52.161		
				(17.277)		
Politicians entry × workers 56–65				−343.162		
				(46.749)		
Politicians entry × male workers					164.548	
					(22.216)	
Politicians entry × 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)
Female politicians 36–55 entry						73.911
						(51.509)
Female politicians 56–65 entry						117.304
						(76.613)
Average dependent variable	4,638	4,265	510			
Panel B. Dependent variable: Months of work in the year						
Politicians entry	0.035	0.005	0.001			
	(0.004)	(0.003)	(0.001)			
Politicians entry × workers 18–35				0.203		
				(0.011)		
Politicians entry × workers 36–55				−0.010		
				(0.005)		
Politicians entry × workers 56–65				−0.073		
				(0.009)		
Politicians entry × male workers					0.053	
					(0.006)	
Politicians entry × 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)
Average dependent variable	3.00	1.76	0.06			

(Continued)

Table 2—Heterogeneous Effects by Jobs, Workers, and Politicians' Characteristics (Continued)

	By workers' occupation			By workers' age	By workers' gender	By politicians' age and gender (young workers)
	Blue collar	White collar	Manager			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C. Dependent 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 × workers 18–35				0.017		
				(0.001)		
Politicians entry × workers 36–55				−0.001		
				(0.000)		
Politicians entry × workers 56–65				−0.006		
				(0.001)		
Politicians entry × male workers					0.004	
					(0.001)	
Politicians entry × 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)
Average dependent 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 who entered office since 1985. Columns 1 to 3 refer to regressions in which the dependent variables are outcomes in each separate occupation. Specifications in columns 4 and 5 allow the effect of the regressor to vary by workers' age and gender, respectively. Specifications in column 5 also include interaction of all controls with a gender dummy. Specifications in column 6 are restricted to workers aged 18–35 and present separate coefficients by politicians' age and gender. Regressions include the same controls as in column 3 of Table 1, including cumulative exits 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 text and the notes to Table 1 for additional details.

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

An average individual in the sample earns €4,637 worth of blue-collar earnings and €510 worth of manager earnings per year. It follows that connections lead to a proportional increase in blue-collar earnings of 1.16 percent $(= 54/4{,}637)$ and an increase in manager earnings of 4.71 percent $(= 24/510)$. These results suggest

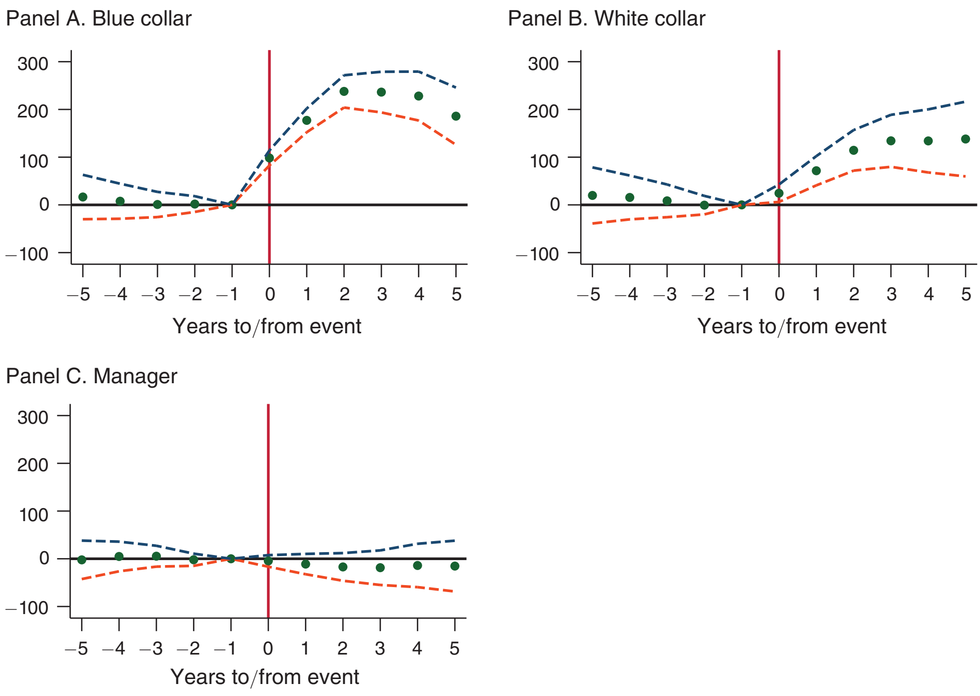

Figure 3. Event-Study Analysis: Yearly Earnings by Occupation—Entry

Notes: The figure reports the same coefficients as in Figure 1, separately by occupation: blue-collar workers (panel A), white-collar workers (panel B), and managers (panel C). See also the notes to Figure 1.

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 the effects on 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 Figure 4 confirm these patterns, although there is some evidence of anticipation effects for older workers. Here and in the following we report event studies only 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 ($166/11{,}171 = 1.5$ percent for males and $57/6{,}235 = 0.9$ percent for females).

Finally, in column 6 we explore the heterogeneity in effects across politicians of different ages and genders. 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" (56+)

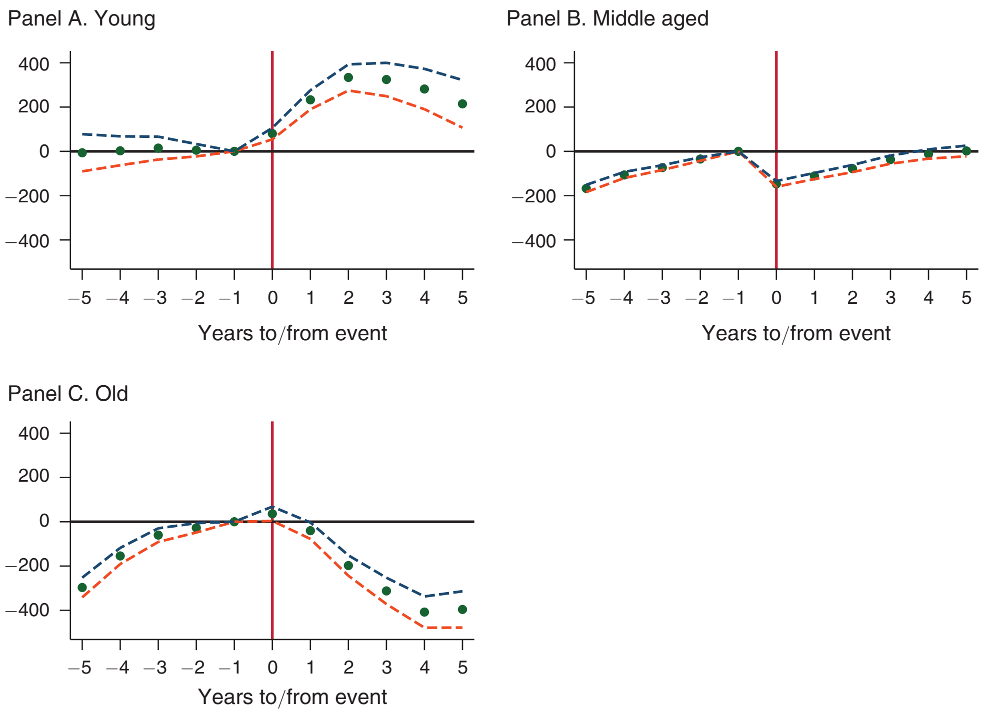

Figure 4. Event-Study Analysis: Yearly Earnings by Workers' Age—Entry

Notes: The figure reports the same coefficients as in Figure 1 separately by workers' age intervals: 18–35 (panel A), 36–55 (panel B), and 56–65 (panel C). See also the notes to Figure 1.

politicians, but not to middle-aged (36–55) politicians. 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 benefit most from these connections. We have also provided additional evidence consistent with the claim that our estimates genuinely capture connections via family ties.

D. *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., those with at least one family member in office during the 27-year period. This somewhat tempers

Table 3—Robustness Checks

	Ever-connected workers only (1)	Add individual linear trends (2)	Add eight-year FE (3)	Add four-year FE (4)	Add two-year FE (5)
Panel A. Dependent 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)
Panel B. Dependent 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)
Panel C. Dependent 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 least one "family" member in office during the period of observation. Column 2 includes the interaction of individual fixed effects (FE) with a linear time trend. Columns 3, 4, and 5 include the interaction between individual fixed effects and dummies for eight-year (1985–1992, 1993–2010, etc.), four-year (1985–1989, 1990–1994, etc.), and two-year (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 the notes to Table 1.

the concern that those who are connected have different latent trends in labor market status from those who are 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 relative to an estimate on the entire sample of around €118, but the differences are not statistically significant.[6]

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

IV. Implied Returns to Nepotistic Hiring

As stated, 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

[6] Alternatively, we could have used a Regression Discontinuity Design, comparing labor market outcomes of the 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 toward 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 a party rather than an individual ballot system.

Table 4—Heterogeneous Effects by Frequency of F3C and Municipality of Birth

	Rescaled by frequency	Frequency			
		1	2–5	6–30	>30
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent 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)
Panel B. Dependent 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)
Panel C. Dependent 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 (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 in column 1; 4,970,470 in column 2; 6,920,962 in column 3; 4,692,720 in column 4; 1,890,422 in column 5. See also the notes to Table 1.

and, from (2), obtain a consistent estimate of θ_1, i.e., the overall returns generated by a politician 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, we can simply rescale this number by a factor of 50 to estimate the total private labor market return from holding office.

This is what we do in column 1 of Table 4, which reports estimates of equation (2), in which the regressor is the number of cumulative entries into 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 27 years of analysis. The point estimate for earnings is €208, 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 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 (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 would 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 would be 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, implying an estimated total effect in the population of around €6,550 (131/0.02). The effect among individuals in groups with a sample size of between 2 and 5 (geometric mean of the distribution of frequencies 0.37) is €86, implying a total effect of around €11,600 ($86/(0.02 \times 0.37)$). Finally, for frequencies between 6 and 30 (geometric mean 0.10) the effect is €34, implying an overall effect of around €17,000 ($34/(0.02 \times 0.10)$). Similar patterns emerge when one looks at months of work and employment in panels B and C 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}).

V. Nepotistic Hiring and Corruption

A. *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 with their clout. As a first exercise, we augment model (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:

$$(4) \quad y_{iFt} = \beta_0 + \beta_1 P_{Ft} + \beta_2 P_{Ft} Spending_m + \mathbf{X}'_{it}\beta_3 + \mu_i + \lambda_t + u_{iFt}, \quad F \in m,$$

where $Spending_m$ is the (log) discretionary expenditure per politician in the municipality of birth of worker m. It 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 municipal 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 online Appendix A3 and Table A.4). It appears that a 10 percent increase in resources per politician leads to a 5 to 7 percent increase in yearly earnings compared with the average estimates in Table 1, depending on whether one includes controls or not ($0.1 \times 59/118$ and $0.1 \times 86/118$, 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 their tenure in office. Column 1 reports the effect of entries into office by number of consecutive terms in the same level of government (first term, second term, etc.). These have to be interpreted as additional

Table 5—Heterogeneous Effects by Local Discretionary Spending

	Yearly earnings (1)	Yearly earnings (2)	Months of work (3)	Months of work (4)	Employment (5)	Employment (6)
Politicians entry × 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 (4)). 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 also include the interaction of cumulative entries and exits with the following municipal time-invariant 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 the population with a college degree, fraction of the population that is past working age, dummies for whether a municipality is a region or province capital, 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 nonprofit associations per capita, and a dummy for whether the municipal administration was ever dissolved for mafia. For sources, definitions, and descriptive statistics see online Appendix A3 and Table A.4. See also the notes to Table 1.

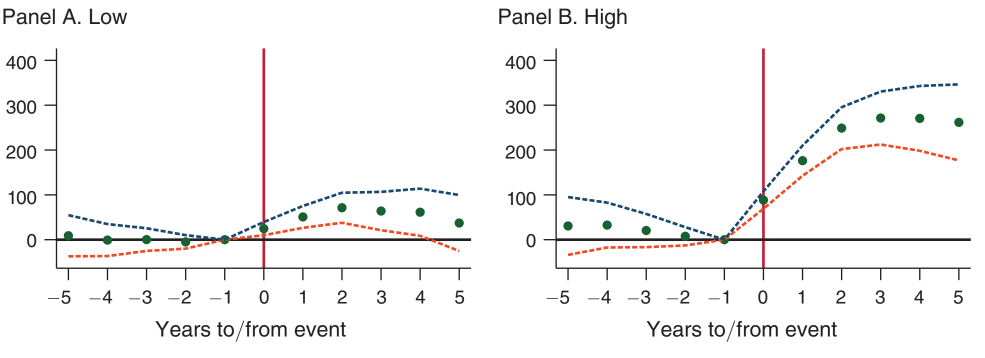

Figure 5. Event-Study Analysis: Yearly Earnings by Discretionary Spending—Entry

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

effects, as an entry in a second term, for example, requires having concluded a first term in office. Results show a sizable effect of the first term in office. While there is apparently no additional return to staying in office for a second term, there are sizable premia associated with 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 with those entering at lower levels (municipal), although effects manifest only for months of work and employment. Results at the highest levels of

Table 6—Heterogeneous Effects by Office and Politicians' Characteristics

	By tenure (1)	By level of government (2)	By level of office (3)
Panel A. Dependent 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)
Panel B. Dependent 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)

(*Continued*)

government (regional and national) are smaller and are 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 with 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 when one focuses on the first entry event there is a positive effect of a second term in office that is not evident when one focuses on all entry episodes.

Table 6—Heterogeneous Effects by Office and Politicians' Characteristics (Continued)

	By tenure (1)	By level of government (2)	By level of office (3)
Panel C. Dependent 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 exits 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 the notes to Table 1.

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 effects associated with longer tenure, higher levels of office (executive versus legislative branch), or the highest levels of government (regional and national), possibly because increased public scrutiny associated with higher level of office limits the opportunities for nepotistic practices.

B. *Public Influence over Firms*

We now investigate effects across firms operating in sectors with different levels of dependence on the public sector. We use the 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 and more than 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: regulation and public transfers.

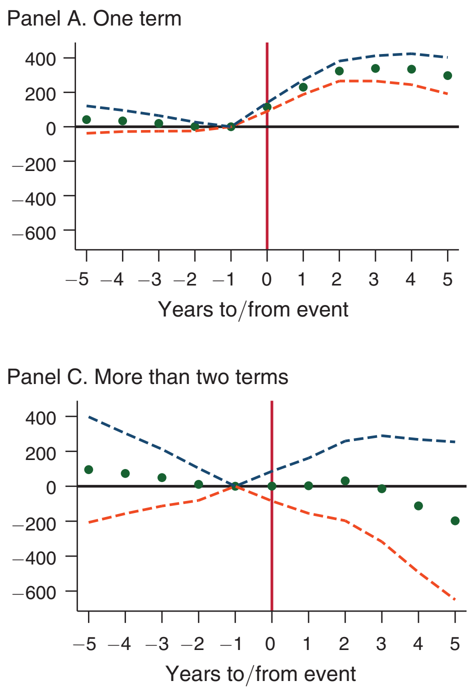

Figure 6. Event-Study Analysis: Yearly Earnings by Tenure—Entry

Notes: The figure reports the same coefficients as in Figure 1, separately by consecutive terms in office: one term (panel A), two terms (panel B), and more than two terms (panel C). See also the notes to Figure 1.

We proceed in two steps. Similar to the analysis by occupation above, we first derive estimates of the effect of entries into office separately for each of the 50 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:

$$y_{ijFt} = \beta_{0j} + \beta_{1j} P_{Ft} + \mathbf{X}'_{it} \beta_{2j} + \mu_i + \lambda_t + u_{ijFt}, \tag{5}$$

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,

$$\hat{\beta}_{1j} = \gamma_0 + \gamma_1 PZ_j + v_j. \tag{6}$$

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 ($= 7.5 \times 1.106/108$). In Figure 9 we also report event studies separately by sectors with a different degree of public dependence. For simplicity, we group sectors into

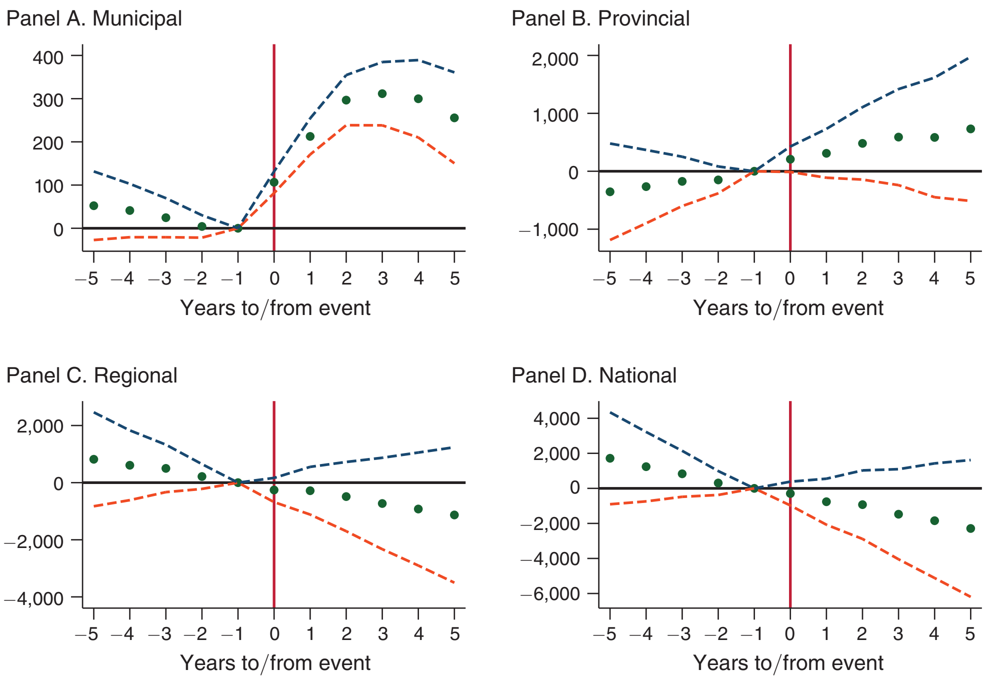

Figure 7. Event-Study Analysis: Yearly Earnings by Level of Government—Entry

Notes: The figure reports the same coefficients as in Figure 1, separately by level of government: municipal (panel A), provincial (panel B), regional (panel C), and national (panel D). See also the notes to Figure 1.

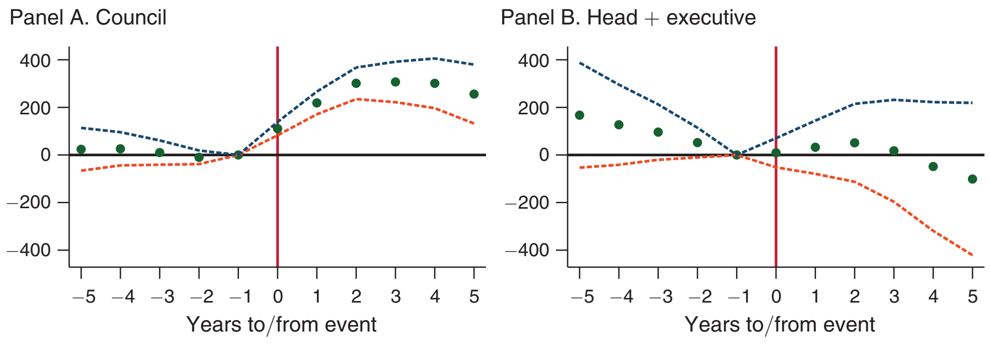

Figure 8. Event-Study Analysis: Yearly Earnings by Level of Office—Entry

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

two categories: below-median (0–0.03) and above-median (0.03–0.1) 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.

Table 7—Heterogeneous Effects across Sectors with Different Levels of Government Dependence

	Yearly earnings (1)	Months of work (2)	Employment (3)
Politicians entry × 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)). 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 two-digit sector using equation (5). The method of estimation is generalized least squares, with weights equal to the square of the reciprocal of the standard error of each coefficient. See text for details. Standard errors clustered by 25 industrial sectors in Pellegrino and Zingales (2014) are in parentheses. Number of observations: 50.

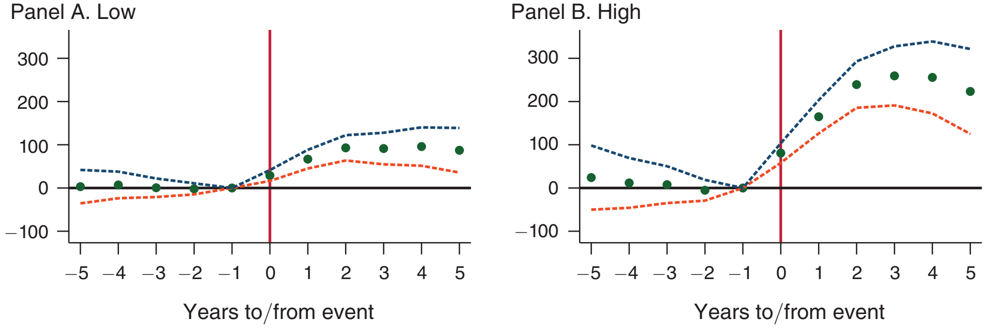

Figure 9. Event-Study Analysis: Yearly Earnings by Government Dependence—Entry

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

VI. 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 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* (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 *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, because 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., Chirico 2016) is that the campaign was initiated by prosecutors with links to *Magistratura Democratica* (MD), the left-wing faction of the *Associazione Nazionale Magistrati* (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 postwar Italy.

We exploit the 1988 district-level share of votes for MD in the election for the *Comitato Direttivo Centrale* of the ANM (see http://www.associazionemagistrati.it), MD_d, to predict how aggressive the Clean Hands campaign was across areas. One would 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 would 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–Clean Hands). Column 1 of Table 8 reports a regression in which the dependent variable is the log per capita number of crimes against the public administration (C) prosecuted in each of the 26 districts (d) in each year (t). These crimes include wrongdoing on the part of both public officials and private agents, as well as payment and receipt of monetary bribes and grafting. The regression includes subperiod dummies, and we use a generalized least squares approach with weights equal to population. In formulas,

$$C_{dt} = \sum_p \psi_p I_{tp} + \sum_p \psi_{1p} MD_d I_{tp} + \epsilon_{dt}, \tag{7}$$

where I_{tp} is a dummy that takes the value 1 if year t is in subperiod 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 fell 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 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 online Appendix A3 and Table A.5 for a description of these variables). In column 2 we report a regression in which 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, 4, and 5 we report regressions in which the dependent variable is, respectively, the fraction of incumbent mayors, the fraction of mayors who are from the incumbent party, and the (log) value added

Table 8—Heterogeneous Effects across Districts with Different Baseline MD Vote Share and Subperiods

Dependent variable:	Crimes against PA (log) (1)	Total crimes (log) (2)	Same mayor (3)	Same party (4)	Value added (log) (5)	Yearly earnings (6)	Months of work (7)	Employment (8)
Politicians entry × MD × 1985–1991	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 × MD × 1992–2000	−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 × MD × 2001–2011	−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 MD vote share in each judicial district separately by subperiod (1985–1991, 1992–2000, 2001–2011) (equation (7)). All regressions include subperiod dummies. Dependent variables are log crimes against the public administration (PA) per capita (column 1), log total crimes per capita (column 2), population-weighted fraction of municipalities where the mayor is an incumbent or is from the incumbent mayor's party (columns 3 and 4), and log value added per capita (column 5). Number of observations in columns 1 to 5: 78. The method of estimation in columns 1 to 5 is generalized least squares, with weights equal to the district population. Columns 6 to 8 report 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 subperiods (equation (8)). Regressions additionally include the interaction between the cumulative number of individuals having entered office since 1985 and subperiod dummies, the interaction between the 1988 MD vote share and subperiod dummies, all other controls as in column 3 of Table 1, and two- and three-way interactions between cumulative exits, the 1988 MD vote share, and dummies for the three subperiods. Column 6 refers to yearly earnings, column 7 to months of work, and column 8 to employment. See also the notes to Table 1.

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 report regressions where the dependent variable is a measure of nepotistic hiring. In particular, we estimate model (1), which we 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 subperiods. All regressions additionally include the interaction between the cumulative number of individuals having entered office since 1985 and subperiod dummies, the interaction between the 1988 MD vote share and subperiod dummies, and all other controls as in equation (1). In formulas,

$$(8) \qquad y_{iFt} = \beta_0 + \sum_p \beta_{1p} P_{Ft} MD_d I_{tp} + \sum_p \beta_{2p} P_{Ft} I_{tp} + \sum_p \beta_{3p} MD_d I_{tp} + \mathbf{X}'_{it}\beta_3 + \mu_i + \lambda_t + u_{iFt}, \quad F \in d.$$

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. When we focus 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 (an additional €40 in the 1990s and €25 in the 2000s, relative to a baseline effect of €118 in Table 1). Similar results emerge for the number of months of work and employment, although estimates are small and not statistically significant for the last subperiod.

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 combat corruption (Olken and Pande 2012).

VII. 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 whether 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 worth of private sector earnings per year and four 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 refer only 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 refer only 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 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 are 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 toward harder-to-detect technologies of rent appropriation.

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