The Cost of Political Connections

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February 27, 2018

Abstract

Using plant-level data from France we document a potential cost of political connections for firms that is not offset by other benefits. Politically connected CEOs alter corporate employment decisions to help (regional) politicians in their re-election efforts by having higher job and plant creation rates, and lower rates of destruction in election years, especially in politically contested areas. There is little evidence that connected firms benefit from preferential access to government resources, such as subsidies or tax exemptions. Connected firms are less profitable in the cross-section and also experience a drop in profitability when a connected CEO comes to power.

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1 Introduction

The nexus between business and government has been a topic of intense public debate and academic research alike. A number of recent papers have documented the financial advantages firms can gain from maintaining close relationships with politicians. Findings by Fisman (2001) Johnson and Mitton (2003) Sapienza (2004) and Faccio, Masulis, and McConnell (2005) suggest that political leaders often use their power to grant economic favors to the firms that are connected to them, which can lead to economic advantages for the connected firms.[1]

In this paper, we explore a potential downside for a firm of having a politically connected CEO: that connections lead CEOs to use firm resources to help connected politicians even if it is not beneficial for the firm. CEOs might have personal benefits from a continued relationship with a politician, or might find it difficult to ignore pressures from politicians that belong to the same political network.[2] We measure politically connected CEOs as those who previously served as close advisor to a top ranking government official. Moving from such positions to the corporate suite has been a common career path for French CEOs which provides high level political access as well as personal relationships.The particular distortions we analyze in this paper are corporate hiring and firing decisions. Prior work has shown that employment conditions are of great importance to voters when deciding whether to re-elect an incumbent politician (see, for example, Wolfers, 2002). Consequently, we test whether connected CEOs maintain employment levels that are above the economically efficient level in order to grant election favors to connected politicians. In doing so, we hope to rule out that connected politicians may engage in this behavior to secure future benefits for the firm. For example, CEOs might be willing to maintain excess employment through the election cycle if they receive more subsidies or lower taxes in return. Similarly, we want to rule out that increased employment through the election cycle is the *result* of receiving more government contracts during that period. Both of these stories would predict that firm sales and potentially profits should go up during this period or even in the long run, which would be close to the prior literature that suggests firms benefit from political connections.

We document three main sets of facts. Using detailed firm and plant level data from France, we first show that business and political elites have strongly overlapping networks and many politicians move to private sector firms after they leave politics. This makes our set up an ideal laboratory for studying the type of political connections we are interested in. Former civil servants controlled

[1] More recent papers have shown that, even in developed countries, access to political networks can affect firm outcomes; see Sapienza (2004), Amore and Bennedsen (2013), Adelino and Dinç (2014), and Schoenherr (forthcoming). A number of papers have also documented the role that banks can play in facilitating political favoritism. See, for example, Khwaja and Mian (2005), Cole (2009), Dinç and Gupta (2011) Dinç (2005).

[2] Our analysis does not allow us to disentangle whether government officials are asking for favors or whether firm executives are providing favors voluntarily. Even if the impetus comes from the politicians, our tests allow us to determine if only connected CEOs respond to the pressure.

11% of the firms (63% of the assets) listed on the French stock market in the 1990s. Second, we find that politically connected firms use employment as a lever to help politicians. Firms with connected CEOs are less likely to fire and more likely to hire workers prior to local elections. We find parallel results for plant closures and openings. These results are particularly strong for areas with contested or close elections where the employment margin might matter more for the incumbent politician. Third, we show that the connected firms do not seem to benefit significantly from being politically connected. Connected firms do not receive higher subsidies or lower (local) taxes, which are decisions that are under the control of local politicians. Furthermore, in the cross-section we do not find that politically connected firms have higher profits or returns on assets than non-connected firms. And finally, when a firm hires a politically connected CEO, firm performance measured as return on assets (ROA) or profits goes down on average, not up. In comparison when a non-connected CEO is replaced by another non-connected CEO, performance does not change significantly. In sum, these politically motivated favors do not seem to create significant benefits for the firms.

Our key empirical strategy is to compare hiring and firing patterns at publicly-traded firms that are managed by politically connected CEOs versus firms whose CEOs are not connected. To identify whether connected CEOs are more likely to grant election favors to incumbent politicians, we test whether there are significant differences in hiring and firing patterns around election times, or in areas that have more contested and close elections. Since typically no one firm can hope to affect nationwide employment patterns, we focus on city-level (mayoral) elections. The advantage of focusing on cities is that (1) they are entities small enough for firm employment decisions to matter and (2) mayors have the power to return favors via regulatory decisions or targeted local tax cuts. Another important benefit of the French setting is that administrative plant-level data are available, which allows us to measure annual job creation and destruction by a given firm in a given city.

Our tests build on the insight from political economy that labor market conditions matter for the re-election chances of politicians (see for instance, Wolfers, 2002, and the references therein). We first confirm in our data that this relationship holds for the French municipal elections we study. Indeed, we find that aggregate changes in local labor market conditions affect the re-election chances of the incumbent party. The effect is especially pronounced for more visible events such as the creation or destruction of whole plants. Therefore, the employment channel appears to be an important factor that can affect a politician's re-election chances.

To attribute any differential growth in job creation or reduced layoffs at the firm level to political motive, our identification strategy uses two important assumptions. First, we assume that politically connected CEOs do not extend political favors all the time but are selective in when and where they use them. The rationale for this is that shareholders might not tolerate very

high levels or long periods of underperformance. CEOs who use firm resources too blatantly for political goals might lose their job or reputation. As a result, connected CEOs should grant these favors predominantly in situations where they can provide the largest political gains. If voters are myopic, one would expect that the positive political impact of additional job creation and new plant openings will be most pronounced close to an election year (and similarly for job destruction and plant closures). These favors will also be more valuable when the jobs are located in areas where the re-election prospects of the political incumbent are less secure.

A second related assumption is that connected CEOs are more likely to respond to the needs of political incumbents than to the needs of opposition candidates. The rationale for this assumption is twofold. To help an opposition candidate, a connected CEO would have to adopt business practices aimed at depressing employment prior to an election, but their benefits would be spread out across the various opposition parties or candidates. Second, a politician who is currently in power likely has more credible ways to commit to reciprocating favors granted by a CEO.

In support of the central hypothesis of the paper, we first find higher employment growth, higher rates of plant creation, and lower rates of plant destruction for firms managed by politically connected CEOs in election years. The effects are especially pronounced if the plants are located in politically contested cities with close elections. Importantly, we show that these employment patterns are robust to controlling for a set of firm characteristics that vary with the political background of the CEO such as firm size and whether the firm was formerly state-owned.

Second, we show that these election favors do not seem to be part of a two-way gift exchange between politicians and connected CEOs. We focus on two of the main levers that politicians have with regards to firms: subsidies and taxes.[3] For example, if subsidies are offered in return for employment favors, we would expect connected firms to be especially likely to receive subsidies when a high fraction of their employment is located in politically contested cities. We do not find evidence for such reciprocity. While politically connected firms have more employees during election years and if they have more plants in contested cities, their sales are not positively affected. This result rejects the hypothesis that the increased job creation by connected firms in election years (or in contested areas) is driven by greater access to government contracts. Similarly, these firms are not more likely to receive subsidies, but taxes seem to be higher (rather than lower) when firms have more employees located in contested areas. Moreover, we do not find that subsidies or tax exemptions are higher in election years.

In addition, we explore whether politically connected firms outperform unconnected firms in

[3]While the corporate income tax is set by the state, municipalities in France set local business taxes such as the "taxe professionnelle." In theory, these local taxes are computed as a fixed percentage of firms' wage bill (Rapport au Premier Ministre, Commission de la Reforme de la Taxe Professionnelle, 2004). In practice though, municipalities can "fine-tune" local business taxes from firm to firm, for example by exempting some firms from paying local taxes for a fixed period, or by tolerating some firms' underestimation of their wage bill.

the cross-section. One could imagine that powerful politicians have other channels more difficult to measure to pay back CEOs who helped their re-election chances. However, we should expect connected firms to have higher performance than unconnected firms if these channels are of first order importance. We do not find evidence to that effect. Depending on the specification, firms with politically connected CEOs typically have between 1 and 2 percent lower ROA than unconnected firms.[4] Even when looking at CEO turnover, we see that a firm that moves from having an unconnected CEO to having a politically connected CEO has an ROA 2.2 percentage points lower than a firm transitioning from one unconnected CEO to another. The latter typically do not see a significant decrease in performance. While our research design does not allow us to study the causal effect of political connections on the performance of the whole firm, since we rely on regional variation, we can relate the performance of connected firms to the fraction of employment they have in politically contested areas. Specifically, we show that the ROA for connected firms decreases as the fraction of their employment located in politically contested cities increases. In line with our hypothesis above, we also show that the lower return on assets can be related to a higher wage bill for these firms. This is may indicate that connected firms have "too many" employees.

Finally, we analyze the nature of the relationship between connected CEOs and politicians. Many prior papers emphasize the role of personal connections or social and family networks in politics, where politicians favor people with whom they have close trust relationships (see, for example, Cohen, Coval, and Malloy, 2011; Haselmann, Schoenherr, and Vig, forthcoming). A complementary dimension of political connections which has been studied much less is that CEOs who have worked in politics might have better access to the political system and better understand how to affect political outcomes. Our proxy for political connections is whether a CEO was previously a cabinet member (i.e., a close advisor to a minister). Having been in the cabinet allows these CEOs to establish a large personal network, in particular among politicians. But we cannot exclude that being a cabinet member most likely also proxies for greater political savviness and general political access.

To provide more insight into the nature of the connections, we study two separate dimensions. First, we analyze whether politically motivated favors are stronger when CEOs and politicians are on the same side of the political spectrum. One would imagine that party ties proxy for ideological alignment and for closer personal ties between people since they might have worked together previously. We find only weak evidence for this channel. While the results are slightly more likely to be significant for politicians on the left of the political spectrum, the economic magnitudes are very small overall. Second, we ask whether the strength of politically motivated employment

[4]We obtain this number from the regression presented in Table 7, column 1, and combine it with the average tenure in bureaucracy available in Table 1.

favors varies with the political clout, and hence potential influence, of the political incumbent. In particular, we identify those incumbent mayors who previously served as ministers in the central government. We find that these mayors do seem to receive slightly larger election favors, even though the magnitude of these effects is small. We interpret these findings as suggestive evidence that within the political system we study, general access and an understanding of how the system works might be more important than individual personal connections. But we cannot rule out that both dimensions are at work.

The idea that economic variables are manipulated for political purposes is not new. Starting with Nordhaus (1975), a large literature on political business cycles has highlighted the incentives incumbent governments have to use economic policy to affect election outcomes. While both fiscal and monetary instruments can be used to improve economic conditions prior to an election, politicians can also try to influence decisions by the corporate sector. For example, Shleifer and Vishny (1994) model the interests of politicians in getting state-owned firms to engage in excess employment and pay above-market wages in order to gain greater political support. In contrast the current paper concentrates on the behavior of private, *publicly-traded* firms, which are not directly controlled by the government, but whose top managers are connected to politicians.

Our results also contribute to a large literature on the connections between business and politics. In the US context, a number of papers have shown that political contributions and lobbying expenses are associated with positive results for the lobbying firms. Political favors in the US might take the form of cash contributions, rather than labor market decisions, due to the specific democratic institutions in the US. For example, Adelino and Dinç (2014) show that distressed firms during the Great Recession had larger lobbying efforts and the amount spent on lobbying was positively associated with greater access to stimulus funds. Houston et al. (2014) find similar results for bank loans when firms have board members with political ties. Akey (2015) shows that firms donating to winning candidates in close elections benefit from their donations and display abnormal post-election equity returns. Yu and Yu (2011) and Fulmer, Knill, and Yu (2017) document that political donations improve firm outcomes on particular corporate decisions. Ovtchinnikov and Pantaleoni (2012) show that individuals make political contributions strategically by targeting politicians that can affect their particular local industries.

The rest of the paper is organized as follows. Section 2 starts with a historical perspective on the executive labor market in France, and describes the main features of this market in the 1990s and early 2000s, the time period covered by our study. We also introduce the main datasets and describe sample construction. Our main findings are reported in Sections 3 and 4. We conclude in Section 6.

2 The French Business Elite

2.1 Historical Perspective

While governmental intervention in the French economy grew during World War I, it is only after World War II that the state took de facto control of large sectors of the economy, including most of the financial sector and a number of large manufacturing firms (such as Renault), with the intent to channel resources to priority industries (see Melitz, 1990; Garrigues, 2002). At the same time, the new prestige and power linked with civil service led an increasing number of graduates from top universities in France to take high ranking government jobs. The career paths of these government officials would typically include a short stay in office, a few years spent as advisors to a minister (or "cabinet members"), and finally a promotion to the top executive level in state-owned firms. Importantly, private firms would also hire these individuals, in part because of their highly selective educational background, but also because of their connections with politicians and bureaucrats, a key asset in an economic environment characterized by heavy state intervention.

The state control of the French economy reached its peak in the early 1980s, (L'année politique, 1982), but by the middle of the 1980s, a political consensus was progressively reached around the idea of reducing government intervention. Between 1984 and 1986, the socialist government undertook a number of dramatic reforms in the banking industry (see Bertrand, Schoar, and Thesmar, 2005) and financial markets (see Thesmar and Thoenig, 2004). In 1986, a center right coalition was elected, which implemented a large privatization program. By the late 1990s, only a few firms remained under state control, mainly utilities and transportation companies.

However, despite these reforms, the representation of former civil servants and former cabinet members remained large in publicly-traded firms into the 1990s and early 2000s, the period under study in this paper. First, many of the former civil servants that were heading state-owned firms prior to privatization remained at the helm of these firms post-privatization, and had substantial discretion in appointing their successors, often drawing from the same social networks. Also, many companies that were never state-owned continued to rely on former civil servants to fill their top executive ranks, suggesting a persistent desire to keep close ties with the state.[5]

[5]Two recent books by Garrigues (2002) and Orange (2006) provide ample anecdotal evidence of these two phenomena. Vivendi (former Compagnie Générale des Eaux) for instance, a very large and diversified conglomerate, was run by former civil servants until 2001 although it never was state-owned.

2.2 Who Managed French Publicly-Traded Firms in the 1990s?

2.2.1 CEO Data

As we explain in more detail in Section 2.3, our study covers publicly-traded firms in France over the 1987 to 2002 period. The *DAFSA* yearbook of French listed firms provides the name of the CEO (directeur général or président du directoire) at the head of each of these companies. We used the French editions of the *Who's Who* (1994–1995 and 2000) to obtain information on the educational and professional backgrounds of these CEOs.

For each listed individual, the *Who's Who* contains self-reported information on: parental occupation, place and date of birth, marital status, number of children, and most relevant for us, education and past professional background. Using this information, we hand-coded for each CEO the year of entry in the private sector and, when relevant, years of entry into and exit from the public sector. For positions held in the public sector, we also coded whether the CEO was a cabinet member ("cabinet ministeriel") and, if so, the political orientation (right-wing or left-wing) of the government the CEO served under. When a CEO had multiple such posts in government, we focused on the highest position that was attained. We also used *Who's Who* information to compile for each CEO overall tenure at their current firm and tenure as CEO.

We were able to retrieve such *Who's Who* information for just over 50% of the CEOs in our sample of publicly-traded firms. For those CEOs who were not found in *Who's Who*, we relied on recent directories of all alumni of Ecole Polytechnique (2001) and ENA (2002–2003), two of the most prestigious schools in France, and key feeders of high-ranking government jobs. Hence, the only political connections we are missing are for those former cabinet members who did not go to Polytechnique or ENA, *and* were not in *Who's Who*. Casual knowledge, however, suggests that such a career profile must be extremely rare among the CEOs of large corporations and the data tend to confirm this. For the sample of former cabinet members present in *Who's Who*, we found that a vast majority (more than 90 percent) graduated from either ENA or Ecole Polytechnique.

2.2.2 Descriptive Statistics

Table 1 reports on the political and career backgrounds of the CEOs heading the firms in our sample. As shown in Panel A, 11 percent of all CEOs in our sample had some prior work experience in the French civil service. These ex-civil servants control more than 60% of publicly-traded assets (i.e., book assets of publicly held firms). Panel B shows that half of the former civil servants are also former cabinet members. About two-thirds of these can be linked back to a right-leaning administration, and the remaining third to a left-leaning administration. By definition, former cabinet members are the most likely to have interacted with politicians in the past and therefore

will be the basis for our measure of political connection in our analysis.

As expected, based on our discussion above, column 3 shows that former civil servants are systematically more likely to head previously state-owned firms. 30% of formally state-owned firms (and 76% of formally state-owned publicly-traded assets – not reported in table) are managed by former civil servants. However, former civil servants are also well represented among firms that were never state-owned: 8% of these firms and 46% of their assets are managed by CEOs that were previously in public sector jobs (not reported in table).

There are also systematic differences in CEO background across industrial sectors. For example, former civil servants are over-represented among financial firms (column 5). This is related to the fact that financial firms are under the tutelage of the Ministry of Finance, a greenhouse of top government officials. Finally, an analysis of trends (not reported here) shows that, in spite of a continuing process of deregulation in all sectors of the economy during the sample period, former civil servants remain prevalent in the French top executive ranks by the early 2000s. In fact, we find that former civil servants control a *growing* share of publicly-traded assets over the period under study.

2.3 Plant Level Data

In addition to the CEO-level information described above, our analysis relies on firm-level and municipal election data. Our firm-level panel dataset covers the period 1987–2002 and is restricted to publicly-traded firms. The *DAFSA* directories provide annual lists of all publicly-traded firms in France. French publicly-traded firms are very often the holding company of a group. Subsidiaries of these holdings are, in general, fully-owned, but registered as separate legal entities. The *DAFSA* directories contain information on a group's consolidated employment and financial statements. This leads to an unbalanced panel of about 700 groups a year, with the mean group in the sample having about 9,800 employees. About a third of these firms operate in the manufacturing sector and about a quarter are in finance, insurance or real estate.

For the bulk of our analysis, we need to supplement this group-level information with other data sets to identify corporate outcomes at a more disaggregated level. For each publicly-traded holding company, we use the LIFI survey (conducted by INSEE, the French statistical office) to find the ownership links to its subsidiaries. Accounting and employment data at the subsidiary level are then obtained from tax filings, which are made available by the tax authorities. All firms, even fully owned subsidiaries, have to file separate financial statements for tax purposes.

In a final step, we extract plant-level information for each of these subsidiaries, available from the SIRENE files maintained by the French statistical office. The SIRENE files provide precise location (city identifier) and total employment for each plant that belongs to a given subsidiary.

From the SIRENE files, which we supplement with the TRANSFER files (also from the French statistical office), we track episodes of plant creation and plant destruction for each subsidiary.[6]

We complement the firm and CEO data with information on municipal election outcomes for the 900 largest cities in France. Municipal elections are held every six years and we obtained data for the 1983, 1989, 1995 and 2001 elections. For each election, the data available at the city-level includes number of registered voters, turnout, and number of votes obtained by each party during the first round of voting.

3 Employment Creation over the Political Business Cycle

3.1 Are Election Results Responsive to Employment Conditions?

In this section, we verify that the French electorate responds to current employment conditions when deciding whether to re-elect the incumbent party in municipal elections. This is an important fact to establish since otherwise there would be no incentives for connected CEOs to engage in politically motivated job creation. The results of this analysis are reported in Table 2.

The dependent variable in all regressions in Table 2 is the change in the fraction of the votes going to the incumbent party between the current and last municipal elections. Cities are weighted equally in columns 1, 2, 5, and 6; they are population-weighted in the remaining columns. In columns 1 to 4, the independent variable of interest is the change in the city-level unemployment rate between the year of the current election and two years prior. Because annual city-level unemployment statistics are only available from 1990 on, we focus on the 1995 and 2001 municipal elections for this analysis.[7]

Consistent with prior studies (e.g., Wolfers, 2002), we find that an increase in unemployment rate is associated with a reduction in the fraction of votes going to the incumbent party. A one standard deviation increase in the unemployment rate two years prior to an election (+2.3 percentage points in the sample) leads to a 1.8 percentage point decrease in the fraction of votes going to the incumbent party. This is true whether cities are equally weighted (column 1) or weighted by their population (column 3). Columns 2 and 4 show that the electorate is especially responsive to unemployment numbers in areas that have a larger share of manufacturing jobs, consistent with the prior that manufacturing jobs would be especially salient to voters.

[6] We use the TRANSFER files in order to separate actual episodes of plant creation and destruction from episodes where a given plant is relocated or changes industry, a distinction that cannot easily be made from the SIRENE files alone.

[7] We obtained unemployment figures at the city-level from UNEDIC, the French unemployment insurance agency. We normalize city-level unemployment with city-level active population, from the 1990 Census.

In the remaining columns of Table 2, we use the plant-level dataset that we will use for most of the tests in the paper to compute city-level employment growth between 1988 and 1989, 1994 and 1995, and 2000 and 2001. Thus, instead of using local unemployment rate to forecast re-election, we use employment growth—as measured by our plant-level data—in each city in the year prior to a municipal election. The longer time series for plant-level data allows us to include 3 election years in this analysis, compared to only 2 in columns 1 to 4.

We show that, like unemployment rate, the electorate is sensitive to city-level employment changes induced by the private-sector job creation. We find a positive but statistically insignificant relationship between the overall change in the fraction of votes going to the incumbent party and city-level employment growth (columns 5 and 7). In columns 6 and 8, we break down this measure of employment growth into three different components: employment growth due to employment changes at already existing and surviving plants, employment growth due to the creation of new plants (always positive by definition), and employment growth due to the destruction of old plants (always negative by definition). We find a more robust relationship between changes in the incumbent party's vote share and employment growth on the extensive margin (e.g., due to plant creation and plant destruction). This pattern is statistically most significant when each city is weighted by its population (column 8). For instance, a one standard deviation decrease in employment growth due to plant destruction ($-.14$) leads to about a 2 percentage point decrease in the fraction of votes going to the incumbent party (or about a third of a standard deviation). In other words, the creation of new plants in a city helps the incumbent party and the destruction of old plants hurts the incumbent party.

Employment growth on the intensive margin does not have a statistically significant effect on the incumbent party's vote share, and the point estimates in fact indicate a surprisingly negative correlation. The fact that voters might be more responsive to employment changes on the extensive margin may not be that surprising as the creation and destruction of plants are more visible events that are, among other things, more likely to be reported by the local media.

Overall, our findings in Table 2 support the view that incumbent politicians should regard current local labor market conditions as a relevant factor in their bid for re-election. Salient events, such as the establishment of a new plant or the closure of an old plant, appear predictive of the voting behavior of the electorate. Hence, CEOs could in theory help incumbent politicians by altering their employment practices around election time, such as by postponing a plant closure. In the next section, we empirically test whether connected CEOs do indeed appear to engage in such practices.

3.2 Do Connected CEOs Grant Favors to Political Incumbents?

We now analyze the main hypothesis of the paper, whether connected CEOs systematically increase employment and plant creation or reduce plant shutdowns around election time to help incumbent politicians. For the purpose of this analysis, we identify a "connected CEO" with a dummy variable that equals 1 if the CEO is a former cabinet member. We focus on former cabinet member because they have worked in direct contact with politicians and therefore are expected to have the tightest political connections. We also repeated the regressions for other measures of political connectedness –for example, any government experience– and find qualitatively similar results.

We assume that politically connected CEOs face constraints in how much they can change employment levels for political reasons, since keeping more employees on the payroll or delaying the shutdown of a plant is costly for the firm. Therefore, we would expect that employment favors are focused around election times and in politically contested areas where the incumbent faces more uncertain re-election outcomes. Tests of these basic hypotheses are reported in Table 3.

The unit of analysis in Table 3 is at the subsidiary-city-year level. We construct three different measures to capture hiring and firing decisions at that level. First, we compute annual employment change in all n plants of a given subsidiary in a given city. Employment change is defined as employment in year t minus employment in year $(t-1)$, divided by the half-sum of employment in year t and $(t-1)$. The mean of this variable is .019. We also construct two dummy variables that focus on employment changes on the extensive margin. Specifically, we construct a dummy variable that equals 1 if the subsidiary created an additional plant in that city in year t, and 0 otherwise; we also construct a dummy variable that equals 1 if the subsidiary shuts down a plant in that city in year t, and 0 otherwise. The mean of the "plant created" dummy is .138, while the mean of the "plant destroyed" dummy is .125.

Panel A of Table 3 shows employment patterns around municipal election time. The independent variable of interest is "Election year×Connected CEO," i.e., the interaction term between an indicator for whether this is a municipal election year and an indicator for whether the ultimate CEO of the company is a former cabinet member. Since municipal elections are held every six years, there are three election years that fall into our sample period: 1989, 1995, and 2001. All regressions include year fixed effects to account for aggregate time shocks to employment. All regressions also include subsidiary fixed effects to account for fixed differences across subsidiaries in employment change, likelihood of plant creation, or likelihood of plant destruction. In addition, we include the city-level mean of the dependent variable of interest to account for local differences in employment patterns.

As was shown in Table 2, connected CEOs are more likely to head firms that were previously state-owned. One could imagine that previously state-owned firms display different employment

patterns in election years, independent of whether or not they are managed by a politically connected CEO. Therefore, we also include in all regressions in Panel A an interaction term between the municipal election year dummy and an indicator for whether the subsidiary belongs to a firm that was previously state-owned. Of course, all interacted variables are also included directly in the regressions.

Finally, in all specifications, we weigh each observation by the fraction of private employment a given subsidiary accounts for in a given city. The rationale behind this weighting scheme is that it puts more emphasis on the behavior of larger employers in an area who should have a bigger impact on aggregate employment at the city-level. Standard errors are corrected to account for arbitrary correlation of the error term between observations that correspond to the same publicly-traded firm.

The findings in columns 1, 3, and 5 are consistent with the hypothesis that connected CEOs create more jobs in election years. In an election year, employment growth is significantly higher at connected firms compared to non-connected firms (column 1). Similarly, column 3 shows that a company managed by a former cabinet member is 5 percentage points more likely to start at least one new plant in an election year. Similarly, column 5 shows that a company managed by a connected CEO is 1 percentage point less likely to destroy a plant in an election year.

Columns 2, 4, and 6 in Panel A of Table 3 replicate columns 1, 3, and 5, respectively, but allow for additional interaction terms between the election year indicator and firm characteristics. Specifically, we saw in Table 2 that firms managed by connected CEOs tend to be systematically larger and appear to operate in a different mix of industrial sectors than firms managed by non-connected CEOs. Therefore, in columns 2, 4, and 6, we allow for employment patterns in election years to systematically vary based on firm size (interaction of the municipal election year dummy with the logarithm of the firm's total assets) and industry (interactions of the municipal election year dummy with 18 industry fixed effects). The introduction of these new interaction terms does not alter the economic or statistical significance of the estimated coefficient on "Election Year×Connected CEO," except in column 6 where we lose statistical significance at conventional levels ($p = .12$).

In regressions not reported here, we also re-estimated each of the regressions discussed above in two separate sub-samples of the data: The sub-sample of firms that were previously state-owned and the sub-sample of firms that were never state-owned. We found the relationship between the political background of a CEO and the employment practices at the CEO's firm in election time held in both sub-samples of the data. Hence, it is not exclusively among previously state-owned firms that political connections matter for employment decisions.

In Panel B of Table 3, we investigate the complementary hypothesis that the granting of employment favors around election time will be especially important in politically contested areas,

e.g., areas where the incumbent party is less certain of being re-appointed. To proceed, we need to categorize municipalities into those that are more or less contested. For that purpose, we define a "swing city" as a city that experienced at least two changes in the identity of the majority party over the three municipal elections that occurred between 1980 and 1999.[8]

Before formally testing this hypothesis, we first ask whether firms managed by connected CEOs differ from firms managed by non-connected CEOs with regard to their employment practices in swing cities. The regressions in columns 1, 3, and 5 of Panel B are similar to those in columns 1, 3, and 5 of Panel A, except that we replace the "election year" indicator with a "swing city" indicator.[9] Interestingly, we find that firms managed by connected CEOs do appear to have different employment patterns in politically unstable areas: they create more jobs in those areas, are more likely to start new plants, and are less likely to destroy old plants. For example, column 3 shows that connected CEOs are more than 1 percentage point more likely to open a new plant in swing cities. Similarly, column 5 shows that connected CEOs are more than 3 percentage points less likely to destroy an existing plant in swing cities. In unreported regressions, we verified that these patterns are robust to allowing for additional interaction terms between the "swing city" indicator and measures of firm size and industry. We also found the same employment patterns to hold when we restrict the sample to those publicly-traded firms that were never state-owned.

Columns 2, 4, and 6 of Panel B confirm that connected CEOs, especially, engage in pro-employment practices around election time when their operations are located in politically contested areas. The coefficient of interest in these regressions is the triple interaction term, "Election year×Swing city×Connected CEO." Note that these regressions also include a triple interaction term between "election year," "swing city," and a dummy variable for whether the firm was previously stated-owned. In other words, we allow for systematically different employment practices by previously state-owned firms in politically unstable areas around election time. All relevant double interaction terms have also been included. As hypothesized, the estimated coefficient on "Election year×Swing city×Connected CEO" is positive and statistically significant in columns 2 and 4 (employment change and plant creation, respectively), and negative and statistically significant in column 6 (plant destruction). We verified that these patterns are robust to including additional interaction terms with other firm characteristics (size, industry). We also found the same patterns to hold in the sub-sample of firms that were never state-owned.

In summary, the findings in Table 3 suggest that the employment practices of connected firms

[8]We also used an alternative measure of how contested a given city is. Specifically, we categorized cities based on how "close" the last election was, where closeness is based on comparing the fraction of votes going to left-wing versus right-wing parties in the first round of voting. We obtained qualitatively similar, but noisier, results using this alternative measure.

[9]In particular, the regressions in columns 1, 3, and 5 of Panel B include an interaction term between the "swing city" indicator and an indicator for whether the firm was previously state-owned.

are affected by the municipal election cycle, and especially so when their operations are located in politically contested areas. We experimented with several other measures of employment changes such as change in levels, dummy variables for large positive shocks to employment (more than 50 jobs created) or large negative shocks to employment (more than 50 jobs destroyed), and number of plants created or destroyed. We found qualitatively similar results for all these measures. Overall, these findings are consistent with our hypothesis that connected CEOs might alter their employment decisions to extend election-related favors to incumbent politicians.

4 Do Firms Gain By Granting Favors to Politicians?

We now analyze why connected CEOs are willing to change employment decisions in their firms to help incumbent politicians stay in power. One explanation is that in an environment characterized by poor corporate governance, CEOs may be able to further their own personal benefits or social networks by helping local politicians. However, it is also possible that politically connected CEOs grant such employment favors in order to ensure economic advantages to the firm that they manage. To analyze this question we focus on two important levers through which politicians can affect business outcomes: lower taxes and larger subsidies. Unfortunately, we do not have data on other potentially important decisions of local government such as the allocation of procurement contracts. But if differential access to these government resources drives our employment results, we would expect that connected firms would show increased sales and value added in politically sensitive areas and in election years. [10]

4.1 Evidence from Operating Outcomes

In Table 4 we investigate these effects on firm outcomes in more detail. Since taxes, subsidies, and sales are accounting-based measures of economic activity, the unit of observation is at the subsidiary-year level. Financial statements are available only at the subsidiary level, not the holding company level. Panel A of Table 4 looks at subsidiary-level outcomes for connected firms prior to election years (Connected CEO×Election Year). And Panel B looks at the outcomes of connected firms in politically contested areas (Connected CEO×Fraction of Employment in swing cities). Compared to prior regressions, the one difference is that we use the fraction of subsidiary employment in a contested area rather than a zero-one variable, since subsidiaries can have plants in several municipal areas. In all regressions in Table 4, we include several controls designed to

[10] Obviously, our analysis cannot capture any benefits given in the form of personal perks or potential benefits that are granted outside our sample period.

account for firm and city fixed heterogeneity, differential response of former government owned enterprises, and large firms in general. Standard errors are corrected to account for arbitrary correlation of the error term across subsidiaries that belong to the same publicly-traded holding.

In column 1 of Panel A we start by replicating our employment results at the subsidiary level. When we regress the log of employment at the subsidiary level on the interaction of Connected CEO×Election Year, as before, we find a strong positive and significant coefficient. We also confirm in column 1 of Panel B that connected CEOs create more jobs in politically contested areas. These results verify that the prior findings hold at the subsidiary level. This is an important robustness check since otherwise one could have been worried that the plant level employment results constitute only strategic reallocation of employees across plants without an effect on the aggregate employment of the subsidiary. For example, in election years a connected CEO could shift employees from plants in politically stable areas to unstable ones to help the political incumbent in that area. However, results in Table 4 confirm that there are strong aggregate effects as well, which could have an effect on the overall performance of the firm.

To analyze whether connected firms receive benefits from these election favors, in columns 2 and 3 of Panel A we repeat the same regression using the log of sales and the fraction of intermediary inputs over sales (one minus gross margin) as dependent variables. If connected firms were benefiting from preferential access to government contracts or other government inputs, we would expect that the increase in employment in election years should be accompanied by an increase in sales or use of intermediate inputs. We do not find any evidence for this hypothesis. Column 2 shows that connected firms do not show any increase in log sales in election years and intermediary inputs over sales seem to decrease (Column 3). This could suggest that firms substitute (potentially cheaper) intermediary inputs for in-house production because of the politically motivated employment creation.

We then investigate taxes and subsidies. In column 4 of Panel A we define a dummy variable that equals 1 if the subsidiary paid a positive tax amount in a given year, and 0 otherwise. We do not find that connected CEOs are more likely to receive tax exemptions in election years. The estimated coefficient on the interaction "Connected CEO×Election Year" is not statistically significant and close to zero. Similarly, in column 5 we construct a dummy variable that equals 1 if the subsidiary received any subsidies in a given year, and 0 otherwise. Again we find no evidence that firms run by connected CEOs are more likely to receive subsidies in election years.

Evidence from columns 4 and 5 in Panel B shows that subsidiaries located in politically contested areas that are run by a connected CEO are *less* likely to receive tax exemptions and are somewhat more likely to receive subsidies, although this second effect is only significant at the 9% level. However, these effects are economically small. Moreover, we do not find an increase in sales or intermediary inputs for firms managed by connected CEOs in politically contested areas

(columns 4 and 5).

Overall, we find only very limited evidence of a two-way gift exchange between politicians and connected firms, since firms in politically contested areas appear to receive (weakly) more subsidies. However, we also find that these firms pay higher taxes and do not show an overall increase in output. Moreover, we do not find support for the idea that the increase in employment at firms managed by politically connected CEOs is mechanically driven by an increase in government spending or other government activities in election years which primarily benefits connected firms.

4.2 Implications for Performance

Politicians might have additional margins of providing value for firms beyond local subsidies and taxes, for example granting preferential access to city contracts. Many of these will not be directly observable, so we turn to the implications of the above results for the overall performance of connected firms compared to non-connected firms. If the benefits from connections greatly outweigh the costs associated with the employment favors, connected firms should perform better than non-connected ones. Of course, if governance was perfect, profit-maximizing boards should only appoint connected CEOs if the benefits of political connections will outweigh their costs. However, there are many industry reports that corporate governance in France was far from perfect in the period that we study, and powerful CEOs were in a position to appoint related board members without much shareholder resistance.[11]

4.2.1 Cross-Section Performance of Connected Firms

As a first step in Table 5, we document the cross-sectional relationship between firm performance and the presence of a connected CEO. We measure performance as ROA using consolidated accounts to eliminate biases stemming from transfer pricing or double counting that would be present if we looked at subsidiary level performance. We present our results for all firms in the sample, excluding finance, insurance, and real estate. We also present our results for the sub-sample of manufacturing firms.

In Panel A, we first regress group ROA on CEO's experience in the public sector as measured by two variables: A dummy variable equal to 1 if the CEO has previous experience as a civil servant, and a continuous variable measuring the number of years as a civil servant. In model

[11] Orange (2006) provides vivid accounts of such events. Bébéar, a prominent figure in French business, mentions in a book (Bébéar, 2003) that "board members are in general reluctant to fire the president. One general assembly after the other, a CEO has his men appointed on the board of directors. They owe him their seats." Kramarz and Thesmar (2013) provide a more systematic study of the relationship between political connections and board composition.

1, we only control for year fixed effects. We know from the descriptive statistics that politically connected CEOs run systematically different companies (larger, more likely to be formerly state-owned, etc.). So, in models 2 and 3, we further control for firm characteristics that have been identified as relevant determinants of the allocation of politically connected CEOs across firms. In model 2, we add as control variables two-digit industry dummies and the logarithm of the firm's total assets; we also add a dummy variable for whether the firm is listed on the "Premier Marché" (a segment of the stock market reserved for the most actively traded stocks). In model 3, we further control for whether the firm was previously state-owned. Of course, even with these additional controls, we do not want to interpret the findings in a causal fashion, as other unobserved firm characteristics could be correlated with CEO characteristics and have an independent impact on performance.

In this first set of regressions, we find a negative, although fragile relationship between the "past bureaucrat" dummy and current ROA. We also find that tenure in the public service is negatively correlated with firm performance. This effect is more robust and corresponds to a ROA reduction by about 1 percentage point for a CEO with average tenure in the civil service (12 years as shown in Table 1). A longer tenure in the public sector may, in part, reflect that the CEO had a more successful career in government, such as eventually becoming a cabinet member. This leads us to a second set of regressions in Panel B.

In Panel B, we correlate performance with a dummy variable for past public service experience *without* cabinet membership and a dummy variable for cabinet membership. We find that the negative correlation between firm performance and public sector experience is mostly driven by those CEOs that were at some point a cabinet member. The negative correlation is robust to the addition of firm characteristics for the sub-sample of manufacturing firms. Manufacturing firms managed by former cabinet members have rates of ROA that are about 2–3% below that of the average firm in their industry, size, and formerly-state-owned status category.

Finally, Panel C breaks down the set of cabinet members into those that served under a right-wing administration and those that served under a left-wing administration. We find roughly the same negative correlation between firm performance and prior cabinet membership for right-wing and left-wing CEOs. There is no effect of party affiliation.

In summary, the results in Table 5 suggest a strong negative correlation between a firm's performance and the political-connectedness of the CEO that manages that firm. These results are surprising since they run counter to many other papers that have shown a positive correlation between performance and political connections. Of course, it is possible that these results are driven by omitted variables if connected CEOs are systematically chosen to head firms that are economically weaker.

4.2.2 Performance and CEO Turnover

To test whether politically connected CEOs tend to be hired by lower-performing firms but create value for the firm, in Table 6 we study changes in firm-level ROA around episodes of CEO turnover. In doing so, we contrast three different types of turnovers: those where neither the old nor the newly appointed CEO are politically connected, those where the old CEO is not connected but the newly appointed one is, and those where the old CEO is connected but the newly appointed one is not.[12]

This analysis allows us to control for firm characteristics that are fixed over time, thereby alleviating some of the concerns raised by the analysis performed in Table 5. We find no systematic change in ROA when a non-connected CEO replaces another non-connected CEO (row 1 of Table 6). The largest changes in ROA are observed when a connected CEO replaces a non-connected CEO. On average, such a turnover episode is associated with a statistically significant 2.5% drop in ROA. Interestingly, though, we do not find the replacement of a connected CEO by a non-connected CEO to be associated with an improvement in ROA. In fact, the estimated change in ROA is also negative in this case. However, the magnitude of the change is smaller and not statistically significant. There are also fewer such transitions.

In summary, we find no strong evidence of a positive correlation between firm performance and the appointment of a connected CEO in France. If anything, profitability seems to drop.

4.3 Performance and Labor Expenses

However, our empirical approach in Table 6 could still be subject to the possibility that the appointment of a connected CEO is endogenous to *changes* in firm performance, or changes in other firm characteristics that are related to performance. Therefore, we try to establish a more direct link between these firm level outcomes and the fraction of employment a firm has in politically contested areas. Given that political consideration seems to affect hiring and firing decisions, but we do not find increased sales (as shown in Table 4), we would expect to see lower performance in connected firms that have more plants/jobs in politically contested areas. In addition, we would expect larger labor-related expenses to be an important driver of this lower performance.

We investigate this finer prediction in Table 7, where each row corresponds to a different regression. We perform this analysis both in the sample of subsidiaries (Panel A) and in the sample of publicly-traded firms (Panel B). Reported for each regression is the estimated coefficient on the interaction between the *Connected CEO* dummy and the fraction of the firm's city-level

[12]We focus here on firms that experience only one CEO turnover episode during the sample period to ensure enough management stability before and after the turnover to attribute corporate performance to management.

employment that is located in politically contested cities, as defined above. The dependent variable for each regression is listed on the left side of each row. All regressions also include year dummies, a dummy for *Connected CEO*, two-digit industry dummies, controls for whether the firm was formally state-owned, and the log of total assets. We also include interactions of the former state-ownership dummy and industry dummies with the fraction of the firm's city-level employment that is located in politically contested cities. Finally, in rows 1 and 2, where we consider labor outcomes, we also control for the ratio of sales over total assets. Standard errors are clustered at the firm-level and all observations are equally weighted.

Panel A starts with wage bill over total assets. We already know from Table 3, Panel B that for given assets, employment in connected firms tends to be higher in subsidiaries located in politically contested areas. This pattern is confirmed when we move from employment to wage bill over assets. We show that all else equal, a subsidiary would increase its wage bill over total assets by more than 2 percentage points if it moved from operating exclusively in politically stable cities to operating exclusively in politically contested cities. Similarly, in row 2, we find that the sales to assets ratio is not different for politically connected subsidiaries in contested cities. For such firms, operating profits should thus be smaller for a given level of assets. This is why, in row 3, we turn to subsidiary-level ROA as the measure of profitability. As expected, we find that subsidiaries managed by connected CEOs perform more poorly when a higher fraction of their employment is located in contested cities. The estimated coefficient on the interaction term of interest is -.012. Hence, all else equal, the ROA of a connected firm would increase by about 1.2 percentage points if that firm moved all of its operations from politically contested cities to politically uncontested cities. How much of this ROA effect can be attributed to the higher labor costs reported above? In row 4, we use as a dependent variable ROA *p*lus wage bill over total assets. The estimated coefficient on the interaction term of interest becomes marginally positive (but statistically insignificant). In summary, connected firms are less profitable and higher wage bills appear to explain most of this gap in profitability.

In Panel B, we focus on performance at a higher level of aggregation: consolidated accounts of publicly-traded firms (instead of subsidiaries). Rows 6 and 7 respectively replicate rows 4 and 5. For these regressions, we compute the fraction of the publicly-traded firm's total city employment that is located in unstable cities. The patterns in rows 6 and 7 are very similar to those in rows 4 and 5 (though less precise): the higher the fraction of employment in contested cities, the lower the ROA for firms managed by connected CEOs; this negative effect disappears after one factors out labor costs from the ROA calculation. Hence, overall, the point estimates in rows 4–7 suggest that higher labor expenses are an important component of the lower performance of politically connected firms.

In sum, these results suggest that to a first order, the higher labor costs created by connected

CEOs over the political cycle do not seem to be offset by other benefits that firms receive in return.

5 What Do Connections Stand For?

Our main proxy of political connectedness is whether a CEO was formerly a cabinet member. As mentioned above, the appeal of this definition is that CEOs who previously held such a position should have broad access to political networks and knowledge of the system. Having been a cabinet member, however, might also mean that these CEOs have personal connections to politicians. To better understand whether personal dynamics play an important role in these connections we now investigate two dimensions: (1) Do party affiliations between CEOs and politicians matter? One could imagine that personal trust relationships follow party lines and also make it more likely that the politician and CEO might have worked in the same administration. (2) Does the political clout of the politician matter in explaining the provision of political favors? If political connections depend on personal relationships, we should not necessarily expect that more powerful politicians receive more favors. However, if CEOs are looking to use the political favors for personal benefits, we would expect to see more powerful mayors receive more election favors.

5.1 Do Political Favors Follow Partisan Lines?

We first analyze in Table 8 whether political connections follow partisan lines. For example, are CEOs that formerly served under a right-leaning government especially likely to alter their employment practices to support right-leaning incumbents, and vice versa for left-leaning CEOs? The dependent variables in Table 8 are the same as in Table 3: Plant level employment, plant creation, and plant shutdowns. The independent variables of interest are interactions between the political affiliation of a connected CEO, as determined by the political orientation of the government for which this CEO previously worked , and the political affiliation of the city mayor, as determined by the identity of the party that received the most votes in the first round of voting in the last municipal election. All regressions include city and subsidiary fixed effects, and control for the mean of the dependent variable of interest at the city-level. Following the same logic as in Table 3, we also control for interaction terms between the political orientation of the city mayor (right-wing or left-wing) and an indicator for whether the subsidiary belongs to a firm that was previously state-owned. We use the same weighting scheme and same approach to compute standard errors as in Table 3.

When we consider all years and cities (columns 1, 4, and 7), we find no significant evidence of partisan effects on the right of the political spectrum; on the left, however, we find significant

but economically small effects. Among firms managed by left-leaning CEOs, employment growth is higher at the plants they operate in cities currently run by left-leaning mayors (column 1). Similarly, a subsidiary managed by a left-wing CEO is about 5 percentage points more likely to create a new plant and 4 percentage points less likely to destroy any plant in a city run by a left-wing mayor (columns 4 and 7, respectively). In unreported regressions, we obtain qualitatively similar results when we allow for additional interaction effects between firm characteristics (such as size and industry) and the political orientation of the city mayor. In the remaining columns of Table 8, we restrict the sample to municipal election years in columns 2, 5, and 8; and in columns 3, 6, and 9, we restrict the sample to politically contested cities. As before, we do not find any evidence of a partisan effect on the right of the political spectrum, but we observe some political employment favors when CEOs and politicians are on the left. However, the economic magnitude of this effect is very small. The results are much weaker than in Table 3 where we based our identification on political connections independent of party affiliation.

These findings do not support an interpretation where connected CEOs use corporate resources to merely further their ideological causes. Since political favors appear to extend across party lines, the results suggest that these networks proxy more generally for access to government or familiarity with the political process. We also believe that these results make less plausible an interpretation that political connections are mainly about social or personal ties. If personal ties played a predominant role, one could argue that CEOs and mayors that have served in either a left- or right-leaning government together are more likely to have met or befriended each other previously. Here again, the fact that connections are stronger across party lines than within, makes it less plausible that the personal side of connections plays an important role.

5.2 Do Politically More Powerful Mayors Receive More Employment Favors?

Table 9 investigates another facet of the relationship between connected CEOs and local politicians: Do politicians with more political clout receive larger employment favors from connected CEOs? An affirmative result would suggest that more powerful politicians can provide larger benefits in return for political favors. To proxy for political clout, we identified the set of mayors that previously served as ministers in a central government. [13]

In columns 1, 3, and 5 of Table 9, we ask whether connected CEOs engage in more employment favors in cities where the current mayor previously held a ministerial post. The independent

[13]The list of all ministers from 1958 on was retrieved from Yvert (2002). We then used the 1994 to 2000 issues of the *Who's Who* in France to obtain the political career of these individuals, and in particular, to identify any position as city mayor after serving in the central government.

variable of interest is the interaction between "Connected CEO" and "Mayor was minister." In all regressions, we include as controls year and subsidiary fixed effects, as well as the city-level mean of the dependent variable of interest. We also allow for previously state-owned firms to have differential employment practices in cities where the current mayor was a minister in the past. We use the same weighting scheme and method to compute standard errors as in Table 3.

While the magnitude of these effects is small, we do find in all three regressions evidence consistent with the idea that more powerful mayors receive larger employment favors from connected CEOs. For example, connected CEOs are about 1 percentage point more likely to create a new plant in cities run by mayors that previously held a ministerial position compared to other cities (column 3).

In the remaining columns of Table 9, the coefficient of interest is that on the triple interaction term, *Connected CEO*×*Mayor was minister*×*Election year*. We also allow for previously state-owned firms to have different employment practices in cities run by former ministers in election years; all relevant double interaction terms have also been included in the model. The estimated effects in columns 2, 4,and 6 are consistent with the idea that connected CEOs are especially likely to create additional jobs in cities run by former ministers in election years. Only in column 6 is the estimated coefficient on the triple interaction term of interest not statistically significant at conventional levels.

Overall, the results in Table 9 suggest that mayors with political clout receive more employment favors from connected CEOs. There are, of course, many possible reasons for political clout to translate into larger employment favors. For example, mayors with political clout may have a greater ability to return favors to the connected CEOs (and the firms they manage). However, the results of the prior section showed no evidence of a two-way gift exchange between politicians and connected firms. One possibility—although we cannot prove it directly—is that politically connected CEOs may grant favors to powerful politicians in order to personally benefit from it. Another possibility is that these CEOs are deceived, as politicians do not return favors as expected. This second explanation is consistent with the idea that politicians can extract favors by giving the illusion of power, and politically connected CEOs are more likely to buy this illusion.

6 Conclusion

While previous research has focused on the advantages firms can derive from maintaining connections to politicians, we consider an orthogonal channel, i.e., that CEOs with better political access can use corporate resources to help an incumbent politician stay in power. We use France as our research setting since a large fraction of publicly-traded assets are managed by CEOs whose past

professional experience involved serving in government. Our results suggest that political connections between CEOs and politicians may indeed factor into important corporate policies, such as job (plant) creation and destruction. Publicly-traded firms managed by politically connected CEOs adjust their employment and plant creation (and destruction) practices in ways that are consistent with helping incumbent politicians in their bid for re-election. Specifically, both employment growth and the rate of plant creation increases at connected firms in election years, while the rate of plant destruction decreases. These practices are particularly strong in election years and in cities that are traditionally more contested. Consistent with the idea that these employment practices might be detrimental to firm performance, we find that accounting performance at firms managed by connected CEOs is lower than non-connected firms and decreases as the fraction of plants that are located in contested areas increases. We show that the lower performance is mostly driven by higher labor costs.

While politicians may, in part, return favors to connected firms through the granting of subsidies, we do not find a net positive effect of political connections on firm performance in the French context. Moreover, we do not think that our employment results are driven by increased access to government contracts for connected firms since there is no accompanying increase in sales or value added around election times for these firms. In fact, analysis of both the cross-section of firms and CEO turnover reveal a negative correlation between firm performance and CEOs' connections to the political leadership. While our research design does not allow us to verify the causal effect of political connections on firm performance (since performance effects cannot be tied closely to the timing of elections), it does seem to suggest that in the French context— different from analysis in other countries—political connections might have large costs or lower benefits.

One can conjecture that the difference between our findings for France and some of the earlier papers is driven by the quality of the institutions across countries or the fact that France is a stable democracy. Maybe it is easier for corrupt politicians to bestow large favors on connected businesses in countries with a powerful dictator who has a large amount of discretion or where the rule of law is less established, while this political patronage is more limited in established democracies such as France. However, more research will be needed to understand the nature of this political gift exchange and the institutional and other constraints on the level of interaction between politics and business.

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Table 1:
Who Manages French Publicly-Traded Firms in the 1990s?
Descriptive Statistics

	(1) Fraction Firms	(2) Fraction Assets	(3) Fraction Old SOEs	(4) Fraction Manuf.	(5) Fraction Finance
Panel A: All CEOs					
Former bureaucrats	0.11	0.63	0.30	0.10	0.15
Observations	1,289	902	501	247	308
Panel B: Former Bureaucrats					
Tenure in the civil service (years)	12.2	15.8	12.7	9.6	15.0
Former cabinet member	0.50	0.80	0.50	0.42	0.53
of which:					
Right-wing government	0.37	0.54	0.36	0.33	0.41
Left-wing government	0.14	0.25	0.16	0.09	0.13
Observations	1,289	902	501	247	308

Sources: *DAFSA* yearbook of corporations listed on the French stock market 1987–2002, supplemented with information from the French editions of the *Who's Who* (1994–1995 and 2000) and alumni directories of ENA and Ecole Polytechnique.

Note: Reported in each cell is the fraction of publicly-traded firms managed by a CEO with the characteristic listed in that row. Firms are equally-weighted in columns 1, 3, 4, and 5 and asset-weighted in column 2. Columns 1 and 2 cover all publicly-traded firms; column 3 focuses on the subset of previously state-owned firms (SOEs); column 4 focuses on the subset of firms in the manufacturing sector; and column 5 focuses on the subset of firms in the financial sector.

Table 2:
Are Election Results Responsive to Employment Conditions?

	Dependent Variable: Change in % votes to incumbent party							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in city unemp. rate	-0.75	-0.32	-0.66	-0.36	.	.	.	.
	(0.28)	(0.30)	(0.38)	(0.51)				
Change in city unemp. rate	.	-0.75	.	-0.69	.	.	.	.
× % emp in manufacturing		(0.30)		(0.41)				
% emp. in manufacturing	.	0.01	.	0.00	.	.	.	.
		(0.01)		(0.01)				
City employment growth	.	.	.	.	0.03	.	0.06	.
					(0.04)		(0.05)	
City employment growth	.	.	.	.	.	-0.04	.	-0.09
- intensive margin						(0.07)		(0.08)
City employment growth	.	.	.	.	.	0.10	.	0.16
- due to plant creation						(0.06)		(0.07)
City employment growth	.	.	.	.	.	0.08	.	0.16
- due to plant destruction						(0.06)		(0.07)
Weighted by city population?	No	No	Yes	Yes	No	No	Yes	Yes
Year F.E.?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,787	1,787	1,787	1,787	2,598	2,541	2,598	2,541

Sources: Municipal election data from Interior Ministry; city unemployment rates from UNEDIC; city employment growth constructed from the SIRENE files; city population from the 1990 census; % employment in manufacturing in a department is constructed from the French Labor Survey (Enquête Emploi). See text for details.

Note: The dependent variable in all regressions is the change in the fraction of votes going to the incumbent party between the current municipal election and the previous one. The various parties are aggregated into a "left-wing" group (PC, PS, Verts, MRG, divers gauches) and a "right-wing" group (UDF, RPR, divers droites). In columns 1–4, the independent variable of interest is the change in city unemployment rate between the current election year and two years prior; in columns 2 and 4, this variable is interacted with the fraction of employment in manufacturing in the department the city belongs to. Because city-level unemployment figures are only available from 1990 on, columns 1–4 only use the 1995 and 2001 municipal elections. In columns 5–8, the independent variable of interest is the change in private-sector employment in the city between the current election year and one prior; in columns 6 and 8, this change in employment is broken down into changes due to continuing plants, plants created, and plants destroyed. Standard errors are in parentheses. F.E., fixed effect.

Table 3:
Do Connected CEOs Grant Employment Favors to Political Incumbents?

Dependent Variable:	*Employ. Change*		*Plants Created*		*Plants Destroyed*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Election Years						
Election year × Connected CEO	0.040	0.032	0.054	0.065	-0.034	-0.025
	(0.023)	(0.016)	(0.030)	(0.030)	(0.013)	(0.017)
Election Year × firm was formally state-owned?	Yes	Yes	Yes	Yes	Yes	Yes
Election year × firm size and industry F.E.?	No	Yes	No	Yes	No	Yes
Year F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
Subsidiary F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
City-mean of dep. var.?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.09	0.09	0.32	0.45	0.22	0.26
Observations	237,958	216,526	257,618	239,877	257,618	239,877
Panel B: Swing Cities						
Swing city × Connected CEO	.023	-.001	.012	-.000	-.033	-.006
	(.009)	(.008)	(.005)	(.002)	(.019)	(.012)
Election year × Swing city × Connected CEO	.	.020	.	.008	.	-.018
		(.012)		(.004)		(.008)
Election year × Connected CEO?	No	Yes	No	Yes	No	Yes
Election Year × Swing city?	No	Yes	No	Yes	No	Yes
Swing city × firm was formally state-owned?	Yes	Yes	Yes	Yes	Yes	Yes
Election year × Swing city × firm was formally state-owned?	No	Yes	No	Yes	No	Yes
Year F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
Subsidiary F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
City-mean of dep. var.?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.09	0.09	0.32	0.32	0.27	0.27
Observations	237,958	229,638	257,618	253,061	257,618	257,517

Note: Each observation in the dataset corresponds to a subsidiary in a given city in a given year. "Employment change" is defined as employment in year t minus employment in year $(t-1)$, divided by the half-sum of employment in year t and $(t-1)$. "Plants created" is a dummy variable that equals 1 if the subsidiary created an additional plant in that city in year t, and 0 otherwise. "Plants destroyed" is a dummy variable that equals 1 if the subsidiary shut down a plant in that city in year t, and 0 otherwise.

"Connected CEO" is a dummy variable that equals 1 if the CEO was formally a "membre de cabinet," and 0 otherwise. "Election year" is a dummy variable that equals 1 if the year is a municipal election year, and 0 otherwise. "Swing city" is an indicator variable that equals 1 if the city experienced at least two changes in the identity of the majority party over the sample period, and 0 otherwise. In each regression, all interacted variables are also included directly. All observations are weighted by the fraction of the firm's employment in total city employment. Standard errors are in parentheses and are corrected for clustering of the error term at the publicly-traded firm (or group) level. F.E., fixed effects. See text for details.

Table 4:
What Do Firms Gain By Granting Favors to Politicians ?

Dependent Variable:	*Employ.*	*Sales*	*Inputs/Sales*	*Taxes*	*Subsidies*
	(1)	(2)	(3)	(4)	(5)
Panel A: Election Years					
Connected CEO	-0.032	0.062	0.013	-0.010	0.013
	(0.050)	(0.040)	(0.007)	(0.009)	(0.011)
Connected CEO	0.081	-0.001	-0.011	0.004	0.002
× Election Year	(0.029)	(0.029)	(0.004)	(0.005)	(0.002)
Former SOE × election year	Yes	Yes	Yes	Yes	Yes
Size	Yes	Yes	Yes	Yes	Yes
Year, Group, Industry F.E.?	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.32	0.59	0.13	0.11	0.14
Observations	84,640	73,017	73,017	81,837	81,837
Panel B: Swing Cities					
Connected CEO	-0.114	-0.033	0.016	-0.031	0.005
	(0.068)	(0.060)	(0.012)	(0.018)	(0.011)
Connected CEO	0.228	0.158	-0.012	0.042	0.025
× Fraction emp. in swing cities	(0.085)	(0.235)	(0.016)	(0.021)	(0.015)
Fraction emp. in swing cities × firm size, firm former SOE	Yes	Yes	Yes	Yes	Yes
Year, Group, Industry F.E.?	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.54	0.61	0.13	0.17	0.16
Observations	84,640	73,017	73,017	81,837	81,837

Notes: Each observation in the dataset corresponds to a given subsidiary in a given year. "Employment" is the log of subsdiary employment. "Sales" is the log of subsidiary sales. "Inputs/Sales" is the ratio of consumption of intermediary inputs (sales minus value added) to total sales. "Taxes" is a dummy variable that equals 1 if the subsidiary paid any taxes in that year, and 0 otherwise. "Subsidies" is a dummy variable that equals 1 if the subsidiary received any subsidies in that year, and 0 otherwise. "Connected CEO" is a dummy variable that equals 1 if the CEO was formally a "membre de cabinet," and 0 otherwise. "Fraction of employment in swing cities" measures the fraction of the subsidiary's employment in cities that experienced at least two changes in the identity of the majority party over the period under study. In each regression, all interacted variables are also included directly. Standard errors are in parentheses and are corrected for clustering of the error term at the publicly-traded firm (or group) level. F.E., fixed effect; SOE, state-owned firm. See text for details.

Table 5:
Performance of Firms Managed by Former Bureaucrats

	Model 1		Model 2		Model 3	
	Real	Manuf.	Real	Manuf.	Real	Manuf.
Panel A: Past Bureaucrat or Not						
CEO was bureaucrat	-0.014	-0.011	0.001	-0.006	0.000	-0.007
	(0.005)	(0.008)	(0.005)	(0.008)	(0.006)	(0.009)
Tenure in bureaucracy	-0.009	-0.014	-0.009	-0.015	-0.010	-0.016
(in decades)	(0.006)	(0.008)	(0.005)	(0.007)	(0.005)	(0.007)
Panel B: Cabinet Member						
CEO was bureaucrat,	-0.012	-0.011	-0.000	-0.006	-0.001	-0.008
not cabinet member	(0.007)	(0.010)	(0.007)	(0.010)	(0.007)	(0.010)
CEO was cabinet member	-0.030	-0.033	-0.010	-0.028	-0.011	-0.030
	(0.005)	(0.007)	(0.005)	(0.008)	(0.005)	(0.008)
Panel C: Party Affiliation						
CEO was bureaucrat,	-0.012	-0.011	-0.000	-0.006	-0.001	-0.003
not cabinet member	(0.007)	(0.010)	(0.007)	(0.010)	(0.007)	(0.007)
RW CEO	-0.031	-0.034	-0.011	-0.029	-0.012	-0.030
	(0.006)	(0.008)	(0.005)	(0.008)	(0.005)	(0.008)
LW CEO	-0.027	-0.030	-0.008	-0.026	-0.009	-0.031
	(0.009)	(0.012)	(0.007)	(0.016)	(0.008)	(0.017)
Observations	5,846	2,559	5,549	2,419	5,333	2,300

Note: The unit of observation is a publicly-traded firm in a given year. This table reports regressions of firm performance on CEO characteristics, focusing on their education and career in the civil service. The dependent variable in all regressions is ROA. Models 1, 2, and 3 vary with respect to the list of (non-reported) additional controls. Model 1 only controls for year dummies; Model 2 further controls for industry dummies, log of firm assets, and listing on the "Premier Marché." Model 3 further controls for whether the firm was formerly state-owned. Each model is estimated both on the whole sample of listed firms and on the sub-sample of non-financial, non-real estate firms. Standard errors are in parentheses and are corrected for clustering of the error term at the individual (CEO) level. RW, right-wing; LW, left-wing. See text for details.

Table 6:
Change in Performance Around CEO Turnover

	Before	After	Difference	Diff in Diff
From: Non-Cabinet	0.003	-0.000	-0.003	
To: Non-Cabinet	(0.003)	(0.003)		
	752	710		
From: Non-Cabinet	-0.006	-0.031	-0.025	**-0.022**
To: Cabinet	(0.011)	(0.007)		
	71	51		
From: Cabinet	-0.017	-0.031	-0.014	**-0.011**
To: Non-Cabinet	(0.004)	(0.006)		
	25	22		

Note: To clarify the analysis, the above results focus on the subset of firms that change CEO once and only once during their presence in the sample. For each firm-year observation, we compute performance as the difference between ROA and mean ROA of all firms in the same year. We thus difference out time effects. For each firm, we then compute the average performance for all years pre-turnover (before), and for all years post CEO turnover (after). We then group firms into three categories: (1) firms for which the CEO was not a cabinet member either before or after the turnover episode; (2) firms who switched from non-cabinet member to cabinet member; and (3) the reverse transition. For each category, we compute the average performance both before and after transition, and report the standard errors in parentheses. We also report the number of observations used. We use 168 transitions from a "non-cabinet" CEO to another "non cabinet" CEO, 14 transitions from a "non-cabinet" to a "cabinet" CEO and 6 transitions from a cabinet CEO to an "on cabinet" CEO. See text for details.

Table 7:
Performance of Firms Managed by Former Bureaucrats
Based on the Location of their Plants

		Coefficient on: Connected CEO × Fraction emp. in swing cities
Panel A: Subsidiary-level Analysis		
Dependent Variable:	*Wage bill/total assets*	.021
		(.011)
	Sales/total assets	-.021
		(.028)
	ROA	-.012
		(.004)
	ROA + wage bill/total assets	.004
		(.015)
Panel B: Group-level Analysis		
Dependent Variable:	*ROA*	-.028
		(.018)
	ROA + wage bill/total assets	.013
		(.055)

Notes: Each observation in the subsidiary-level analysis corresponds to a subsidiary in a given year. Each observation in the group-level analysis corresponds to a group in a given year. Each row corresponds to a separate regression. "Connected CEO" is a dummy that equals is the CEO was formally a "membre de cabinet," and 0 otherwise. In Panel A (B) "Fraction of employment in swing cities" is the fraction of the subsidiary's (group or publicly-traded firm's) employment that is located in cities that experienced at least two changes in the identity of the majority party over the period under study. Also included in all regressions are: year fixed effects, two-digit industry fixed effects, a dummy for "Connected CEO," a dummy for "formally state-owned," log (total assets), and interactions of the "formally state-owned" dummy and industry fixed effects with "fraction of employment in swing cities." Standard errors are reported in parentheses. See text for details.

Table 8:
Do Employment Favors Follow Partisan Lines?

Dependent Variable:	*Employ. Change*			*Plants Created*			*Plants Destroyed*		
Sample:	All	Elec. Years	Swing Cities	All	Elec. Years	Swing Cities	All	Elec. Years	Swing Cities
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LW CEO	0.015	0.010	0.029	0.057	0.013	0.019	-0.043	-0.048	-0.033
× LW City	(0.008)	(0.005)	(0.008)	(0.029)	(0.024)	(0.015)	(0.023)	(0.023)	(0.018)
RW CEO	-0.013	-0.005	-0.019	0.003	-0.021	0.008	-0.006	-0.023	-0.006
× RW City	(0.011)	(0.021)	(0.011)	(0.009)	(0.024)	(0.011)	(0.008)	(0.026)	(0.010)
Firm was formally state-owned	-0.009	-0.043	-0.020	0.002	0.047	-0.002	0.008	0.007	0.010
× RW City	(0.013)	(0.032)	(0.013)	(0.015)	(0.033)	(0.004)	(0.014)	(0.005)	(0.015)
Firm was formally state-owned	0.004	-0.011	-0.052	-0.023	-0.018	-0.031	0.003	0.041	0.018
× LW City	(0.013)	(0.018)	(0.020)	(0.008)	(0.019)	(0.012)	(0.014)	(0.036)	(0.014)
Year F.E.?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subsidiary F.E.?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-mean of dep. var.?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.09	0.28	0.09	0.34	0.36	0.33	0.27	0.61	0.27
Observations	211,791	28,577	162,689	235,991	47,587	181,580	235991	47587	181580

Note: Each observation in the dataset corresponds to a subsidiary in a given city in a given year. In columns 2, 5, and 8, we restrict the sample to municipal election years. In columns 3, 6, and 9, we restrict the sample to the set of "swing" cities, i.e., cities that experienced at least two changes in the identity of the majority party over the sample period. "Employment change" is defined as employment in year t minus employment in year $(t-1)$, divided by the half-sum of employment in year t and $(t-1)$. "Plants created" is a dummy variable that equals 1 if the subsidiary created any additional plant in that city in year t, and 0 otherwise. "Plants destroyed" is a dummy variable that equals 1 if the subsidiary shut down any plant in that city in year t, and 0 otherwise. "LW (RW) City" is a dummy variable that equals 1 if left-wing (right-wing) parties received the most votes in that city in the last municipal election. "LW (RW) CEO" is a dummy variable that equals 1 if the CEO was formally a cabinet member under a left-wing (right-wing) government. In each regression, all interacted variables are also included directly. All observations are weighted by the fraction of the firm's employment in total city employment. Standard errors are in parentheses and are corrected for clustering of the error term at the publicly-traded firm (or group) level. F.E., fixed effect. See text for details.

Table 9:
Do More Powerful Mayors Receive More Employment Favors?

Dependent Variable:	*Employment Change*		*Plants Created*		*Plants Destroyed*	
	(1)	(2)	(3)	(4)	(5)	(6)
Connected CEO × Mayor was minister	0.007	0.001	0.010	0.001	-0.002	-0.000
	(0.003)	(0.001)	(0.005)	(0.001)	(0.001)	(0.001)
Connected CEO × Mayor was minister	.	0.005	.	0.006	.	-0.003
× Election Year		(0.002)		(0.003)		(0.002)
Connected CEO × Election year	.	-0.011	.	0.003	.	0.000
		(0.011)		(0.021)		(0.002)
Mayor was minister × Election year	.	-0.000	.	-0.001	.	0.001
		(0.001)		(0.001)		(0.001)
Firm was formally state-owned	-0.000	-0.000	0.000	-0.000	0.001	0.001
× Mayor was minister	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Firm was formally state-owned ×	-	-0.001	-	0.000	-	0.001
Mayor was minister × Election year		(0.001)		(0.001)		(0.002)
Year F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
Subsidiary F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
City-mean of dep. var.?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	229,724	229,724	322,625	322,625	322,615	322,615
R^2	0.07	0.07	0.20	0.20	0.22	0.22

Note: Each observation in the dataset corresponds to a subsidiary in a given city in a given year. "Employment change" is defined as employment in year t minus employment in year $(t-1)$, divided by the half-sum of employment in year t and $(t-1)$. "Plants created" is a dummy variable that equals 1 if the subsidiary created any additional plant in that city in year t, and 0 otherwise. "Plants destroyed" is a dummy variable that equals 1 if the subsidiary shut down any plant in that city in year t, and 0 otherwise. "Connected CEO" is a dummy variable that equals 1 if the CEO was formally a "membre de cabinet," and 0 otherwise. "Mayor was minister" is a dummy variable that equals 1 if the city mayor previously held a ministerial post, and 0 otherwise. "Election year" is a dummy variable that equals 1 if the year is a municipal election year, and 0 otherwise. In each regression, all interacted variables are also included directly. All observations are weighted by the fraction of the firm's employment in total city employment. Standard errors are in parentheses and are corrected for clustering of the error term at the publicly-traded firm (or group) level. F.E., fixed effect. See text for details.