Politicians, Firms and the Political Business Cycle: Evidence from France

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Abstract

This paper tests whether politically connected CEOs alter their hiring and firing decisions in order to help incumbent politicians in their re-election efforts. We study this question in the context of France, where more than half of the assets traded on the French stock markets are managed by CEOs who were formerly in government. Our results show that publicly-traded firms managed by politically connected CEOs display higher rates of job (plant) creation and lower rates of plant destruction, in election years. These results are larger for firms operating in politically contested areas. We find only limited evidence that these political favors follow partisan lines. Moreover, the results are stronger for incumbents who have more political clout within their party. However, we only find very weak evidence that connected firms benefit from preferential access to government resources such as subsidy programs or tax exemptions. In fact, politically connected firms show lower profits compared to unconnected firms, and this lower performance seems mainly driven by higher labor costs. Overall our results suggest that political connections can have significant costs for the connected firms, which is in contrast to earlier studies that have mainly documented economic benefits from political connections.

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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 economic advantages firms can gain from maintaining close relationships with politicians. Findings by Fisman (2001), Johnson and Mitton (2003), Sapienza (2003) or Faccio, Masulis and McConnell (forthcoming) suggest that political leaders often use their power to grant economic favors to connected firms. These papers also suggest that political connections often result in economic advantages for the connected firms. In this paper, we investigate the reverse direction: benefits that flow from firms to politicians. We study whether firms headed by politically connected CEOs alter their business decisions to grant election favors to politicians. In particular, we focus on politically connected firms' hiring and firing decisions, since prior work has shown that current employment conditions are of great importance to voters when deciding whether to re-appoint an incumbent politician (see for example Wolfers, 2002).

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 for them 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.

We focus our analysis on France, which offers a unique research setting to study the nexus between business and government. Social and political connections between CEOs and politicians are very prevalent in France. We will show below that former civil servants control 11% of the firms, and 63% of the assets, listed on the French stock market in the 1990s. Until recently, most of the literature on political business cycles has focused on national elections and national outcomes (such as GDP growth or employment) and has failed to find strong for political business cycles at that level of aggregation (see Drazen, 2000). Realistically, one does not expect even the largest firms within a country to be able to affect nationwide employment figures; but business decisions by

a given firm could have a significant effect on city-level employment and election results. A small recent literature on political business cycles has started seeking evidence for political business cycles using such micro level outcomes and data from local election (see for instance Dinc, 2003, Mian and Khwaja, 2005, and Cole, 2006). While these papers provide evidence that politicians subsidize the private sector to obtain reelection, our focus is on the reverse direction, i.e. political favors granted by connected mangers to politicians. In practice, this implies that we need to measure hiring and firing decisions at a regionally disaggregated level for each firm. Fortunately, the French statistical office compiles very detailed plant level information, which allows us to measure annual jobcreation and destruction by a given firm in a given city.

Our empirical strategy consists in studying differences in hiring and firing patterns at publicly-traded firms that are managed by connected CEOs compared to other 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 time, or in areas that are politically more contested.

A central assumption underlying our identification approach is that politically motivated employment favors impose a cost on the connected CEOs, e.g. they might lose their job if they too blatantly use firm resources for political goals. Thus we assume that these favors will only be used selectively, and 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 closer to an election year (and vice versa for job destruction and plant closures). Similarly, one would expect that those favors will be more valuable when the jobs are located in areas where the re-election prospects of the political incumbent are less secure. In contrast, in areas where the party in power has a large majority, there will be less political value in boosting employment, since the incumbent can secure re-election without help from the business community.

Another 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, the party currently in power likely

has superior and more credible means (carrots and sticks) to exert pressures on connected CEOs.

In the analysis that follows, we first show that changes in local labor market conditions affect the re-election chances of the incumbent party in municipal elections. The effect is especially pronounced for the more visible events such as the creation or destruction of whole plants. This first result confirms that incumbent politicians benefit from improved labor market conditions around election time, and thus open the possibility that they will rely on their connections to private-sector firms to strengthen employment figures when politically most relevant.

In support of the central hypothesis of the paper, we 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, and especially if the plants are located in politically unstable cities. Importantly, we show that these employment patterns are robust to accounting 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.

We offer a few additional insights on the nature of the relationship between connected CEOs and politicians. First, we consider the possibility that strong ideological preferences among connected CEOs interfere with their business decision-making. We ask whether CEOs that formerly served under a right-wing government are especially likely to bestow favors to right-wing political incumbents, and vice versa for left-wing CEOs. We find some evidence for this, but exclusively on the left wing of the political spectrum. Second, we ask whether the strength of these employment effects vary with the political clout, and hence potential influence, of the political incumbent. In particular, we identify those incumbent mayors that previously served as ministers in the central government. We find that these mayors do seem to receive somewhat larger election favors, even though the magnitude of these effects is small.

Finally, we explore whether these election favors are 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.[1] [2] For example, if subsidies are offered in return for employ-

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[1] Optimally, one would like to study other outcomes, such as access to city contracts. Unfortunately, no such data is readily available for a representative sample of cities.

[2] 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 percentage of firms' wage bill, with this percentage being decided at the city-level and supposed to apply to all firms operating in the city (Rapport au

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ment 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 only find very limited evidence for such reciprocity. While politically connected firms seem to be somewhat more likely to receive subsidies, especially if they are located in contested areas, the same does not hold for tax exemptions. Moreover, we do not find that subsidies or tax exemptions are higher in election years. Our results also reject the view that the increased job creation by connected firms in election years (or in contested areas) is driven by greater access to government contracts, since we do not find any accompanying increase in sales or value added.

We conclude by exploring the implications of our findings for the performance of politically connected firms. Earlier work in other countries has documented the financial benefits firms derive from political connections. Our findings, though, raise the possibility that political connections might also entail some costs to firms. Even though one would expect independent and profit-maximizing boards to appoint connected CEOs only if the net effect of this appointment on performance is positive, possible corporate governance failures or private benefits considerations make this a relevant and mostly empirical issue. While our research design does not allow us to study the causal effect of political connections on performance, we can however relate the performance of connected firms to their exposure to election considerations. Specifically, we show that the rate of return on assets for connected firms decreases as the fraction of their employment located in politically contested cities increases. In accord with much of our analysis above, we show that this negative impact on return on assets can be related to a higher wage bill for these firms.

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 describes sample construction. Our main findings are reported in Sections 3 and 4. We conclude in Section 5.

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 of time, or by tolerating some firms' underestimation of their wage bill.

2 The French Business Elite

2.1 Historical Perspective

Until World War II, French firms mainly relied on engineers to fill in their top executive positions. The tendency for firms to hire managers with engineering background dates back to the 19th century, a time when most French firms were still family-owned. As some of these family-owned firms grew larger, they started to hire professional managers who were engineers from either "Ecole Polytechnique," the top engineering school in France, and "Ecole Centrale" (see Cassis (1999)).

While governmental intervention in the economy started in the 1930s, 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 of channeling resources to priority industries (see Melitz (1990) or Garrigues (2002)). Around the same time, the "Ecole Nationale d'Administration," or ENA, was created to supply the public sector with highly trained civil servants who would typically join the Treasury or the Ministry of Finance. The new prestige and power linked with the civil service also led an increasing number of graduates from the "Ecole Polytechnique" to join the Ministry of Industry. The career paths of the civil servants trained at ENA and "Ecole Polytechnique" would typically include a short stay within government, a few years spent as advisors to a minister ("membre de cabinet") and finally a promotion to the top executive level in state-owned firms. Importantly, private firms would also hire these former civil servants, in part because of their highly selective educational background, but also because of their connections with politicians and the bureaucracy; such connections were viewed as an asset in an economic environment characterized by heavy state intervention.

The state control of the French economy reached its peak in the early 1980s, when the newly elected socialist government decided to nationalize the remaining private banks as well as a number of industrial firms, in order to revamp job creation (L'année politique (1982)). However, by the middle of the 1980s, it became clear that the solution to the ensuing economic crisis was less state intervention. Between 1984 and 1988, the socialist government undertook a number of dramatic reforms in the banking industry (see Bertrand, Schoar and Thesmar, forthcoming) and financial markets (see Thesmar and Thoenig, 2004). In 1986, a center right coalition was elected and started 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 graduates of ENA and "Ecole Polytechnique," as well as of former civil servants and former "membres de cabinet," remains large in publicly-traded firms in the 1990s and early 2000s, the period under study in this paper. First, a lot 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, a lot of companies that were never state-owned continued to rely on former civil servants to fill in their top executive ranks, suggesting a persistent desire to keep close ties with the state.[3]

2.2 Who Manages 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), as well as the ENA and Ecole Polytechnique alumni directories, 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 in and exit of the public sector. For positions held in the public sector, we also coded whether the CEO was member of a "cabinet ministeriel" and, if yes, 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, as well as tenure as CEO.

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[3]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.

We were able to retrieve such Who's Who information for a little more than 50% of the CEOs in our sample of publicly traded firms. For those CEOs that were not found in *Who's Who*, we relied on recent directories of all alumni of Ecole Polytechnique (2001) and ENA (2002-2003) to assess whether the CEOs that were not found in *Who's Who* had attended any of these two schools. All former graduates are included in these directories, so we can track with very limited measurement error whether any given CEO in our sample is a graduate of ENA or Ecole Polytechnique. Also, for each graduate, these directories report information on the alumni's career in the civil service, when applicable. We thus also coded as former civil servants all ENA and Ecole Polytechnique graduates with experience in the civil service, as reported in the alumni directories.

While our data collection procedure ensures a good measurement of educational background (e.g. having attended ENA or Ecole Polytechnique), it is theoretically possible that we are not capturing some former experience in the civil service for CEOs that did not graduate from ENA or Ecole Polytechnique. Casual knowledge however suggests that such a career profile (e.g. having worked as a civil servant *without* being and ENA or Ecole Polytechnique graduate) must be extremely rare among the CEOs of large corporations. This is confirmed in the data. For the sample of former civil servants whose career is detailed in *Who's Who*, we found that a vast majority (more than 90 percent) graduated from ENA or Ecole Polytechnique.

2.2.2 Descriptive Statistics

Table 1 reports on the educational and political backgrounds of the CEOs heading the firms in our sample. Seven percent of the CEOs in our sample attended ENA and another 11 percent attended Ecole Polytechnique. As column 2 shows, the CEOs trained at these "Grandes Ecoles" head the largest firms in our sample. More than 90% of the assets listed on the French stock market over the period under study are managed by graduates of ENA or Ecole Polytechnique. Eleven percent of the 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. In addition, half of these former civil servants are also former "membres de cabinet." About two-thirds of these can be linked back to a right-wing administration, and the remaining third to a left-wing administration. By definition, those CEOs that are former "membres de cabinet" 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 the

analysis below.

As somewhat expected based on our discussion above, column 3 shows that former civil servants and graduates of ENA and Ecole Polytechnique are systematically more likely to head previously state-owned firms. Thirty percent 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, graduates of ENA are under-represented among manufacturing firms (column 4), while they (and former civil servants) are over-represented among financial firms (column 5).[4]

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 data set covers the period 1987 to 2002 and is restricted to publicly-traded firms. The DAFSA directory provides annual lists of all publicly-traded firms in France, e.g. including both those traded on the "Premier Marché" and those traded on the "Second Marché." French publicly-traded firms are very often the head of (or holding company for) a group. Subsidiaries of these firms are in general fully-owned, but registered as separate entities. The DAFSA directory contains information on a group's total employment and consolidated financial statement, balance sheet and statement of cash flows. This leads to an unbalanced panel of about 700 firms a year, with the mean firm in the sample having about 9,800 employees. About a third of these firms operates in the manufacturing sector and about a quarter is in finance, insurance or real estate.

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[4]As we mentioned earlier, a large fraction of firms in the financial sector were previously state-owned, so the patterns in column 5 are arguably related to those in column 3.

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 firm, 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, as available in the SIRENE files from 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 also track episodes of plant creation and plant destruction for each subsidiary.[5]

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 political outcomes available at the city-level are: 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

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[5]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 SIREN files alone.

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.[6]

Consistent with prior studies, see Wolfers (2002), we find that an increase in the 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 whether they are 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.

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, we measure employment growth in each city in the year prior to a municipal election. The longer time series for the 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 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

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[6]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.

(-.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 shut downs 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 formerly was a "membre de cabinet" (e.g. held a post in the cabinet) at some point prior to becoming a CEO. We focus on former "membres de cabinet" 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 such as ever having been in government 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 shut down 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.

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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 by 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 any additional plant in that city in year t, and 0 otherwise; we also construct a dummy variable that equals 1 if the subsidiary shut down any 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 studies employment patterns around municipal election time. The independent variable of interest is "Election year*Connected CEO," e.g. 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 "membre de cabinet." 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 we showed 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, independently 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

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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 "membre de cabinet" 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 former "membre de cabinet" is 1 percentage points less likely to destroy any plant in an election year.

Columns 2, 4 and 6 in Panel A of Table 3 respectively replicate columns 1, 3 and 5 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 also 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 based on 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 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 to hold 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

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categorize municipalities into those that are more or less contested. For that purpose, we define as a "swing city" 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.[7]

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.[8] Interestingly, we find that firms managed by connected CEOs do appear to have different employment patterns in politically less stable areas: they create more jobs in those areas, are more likely to start new plants and less likely to destroy old plants. For example, column 3 shows that connected CEOs are more than one 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 any existing plant in swing cities. In regressions not reported here, 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 that on 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

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[7]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.

[8]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.

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 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), number of plants created or number of plants 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.

3.3   Do Political Favors Follow Partisan Lines?

To gain a finer picture of the relationship between connected CEOs and politicians, we first ask whether political connections follow partisan lines. For example, are CEOs that formerly served under a right-wing government especially likely to alter their employment practices to support right-wing incumbents, and vice versa for left-wing CEOs? This analysis will help us to understand whether the political favors are driven by shared ideological believes between CEOs and incumbents. Or alternatively, if connections do not follow partisan lines, it would suggest that other factors such as familiarity with the political process or access to the network of incumbent politicians is important. We investigate this question in Table 4.

The dependent variables in Table 4 are the same as in Table 3: Plant level employment, plant creation and plant shut downs. The independent variables of interest are interactions between the political affiliation of a connected CEO, as determined by the political orientation of the government this CEO previously worked for, 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 also control for the mean of the dependent variable of interest at the city-level. Also, following the same logic as in Table 3, we control for interaction terms between the political orientation of the city mayor (right-wing

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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 evidence of partisan effects on the right-wing of the political spectrum; but we find significant effects on the left-wing. Specifically, among firms managed by left-wing CEOs, employment growth is higher at the plants they operate in cities currently run by left-wing 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 regressions not reported here, 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 4, we restrict the sample to those years and locations where we expect the benefits of higher employment figures to be most beneficial to the political incumbent. 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 politically contested cities. Overall, our findings for these regressions do not differ much from those we obtained in the full sample: we do not find any clear evidence of "partisan effect" on the right-side of the political spectrum, but we observe more pro-labor practices at firms managed by left-wing CEOs when they operate in left-wing cities. However, this effect does not appear economically stronger in these sub-samples of the data (politically unstable cities or in election years) than in the full sample.

In summary, Table 4 offers mixed insights regarding the importance of partisan effects in driving connected CEOs' employment decisions. We find some evidence consistent with a "partisan effect" on the left-wing of the political spectrum, but the timing of this effect and its relative intensity across regions do not match our a priori expectations. But 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

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with the political process.[9]

3.4 Do Politically More Powerful Mayors Receive More Employment Favors?

Table 5 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 either put more pressure on connected CEOs or are able to 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. [10]

In columns 1, 3, and 5 of Table 5, we ask whether connected CEOs engage in more pro-employment practices in cities where the current mayor previously held a ministerial post. The independent 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 same 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 one 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 regressions not reported here, we verified that these findings are robust to including interactions between "Connected CEO"" and other city characteristics, such as population, employment and average income.

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[9]One should also note that what we refer to here as partisanship might still be a reflection of social or personal ties, rather than of an ideological motive. Indeed, one could argue that CEOs that have served under a left-wing government are more likely to have met or befriended individuals that would eventually become left-wing mayors than individuals that would eventually become right-wing mayors.

[10]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 identify any position held as city mayors after serving in the central government.

In the remaining columns of Table 5, 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, 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 5 suggest that mayors with political clout may be able to command more favors from connected CEOs. There are of course many possible reasons for political clout to translate into larger employment favors. For example, it may go hand-in-hand with a larger social network and thus easier access to connected CEOs. Alternatively, mayors with political clout may have a greater ability to return favors to the connected CEOs (and the firms they manage). Our data is unfortunately not rich enough for a full analysis of these different channels. However, in the next section, we do try to provide some more direct evidence on the possibility of a "two-way gift exchange" between politicians and connected firms, e.g. the possibility that politicians somehow reciprocate the employment favors they receive from connected firms.

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. Indeed, a number of recent papers have pointed out how political connections can be beneficial to firms. 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 access to low interest loans or allocation of procurement contracts. But if differential access to these government resources drives

our employment results, we would expect that connected firms show increased sales and value added in politically sensitive areas and in election years. [11]

4.1 Evidence from Operational Outcomes

In Table 6 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 not available for establishments but only at the subsidiary level. Panel A of Table 6 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). The one difference to our prior regressions is that we use the fraction of subsidiary employment that is in contested area rather than a zero-one variable, since subsidiaries can have plants in several municipal areas. In all regressions in Table 6, we include several controls designed to account for firm and city fixed heterogeneity, as well as differential response of former government owned enterprises, as well as large firms in general. Standard errors are corrected to account for arbitrary correlation of the error term across observations that correspond to the same publicly-traded firm.

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 6 confirm that there a strong aggregate effects as well, which potentially could have an effect on the overall performance of the firm.

To analyze whether connected firms receive benefits from these election favors, in Column

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

(2) 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 statistically not significant and close to zero. Similarly, in Column (3) 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. Finally, in Columns (4) and (5) of Panel A we repeat the same regressions using the log of sales and the fraction of intermediary inputs over sales 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 (4) shows that connected firms do not show any increase in log sales in election years and intermediary inputs over sales seem to decrease (Column 5). This could suggest that firms substitute (potentially cheaper) intermediary inputs for in house production because of the politically motivated employment creation.

Evidence from Columns (2) and (3) in Panel B shows that subsidiaries located in politically contested areas, who are run by a politically connected CEO, are less likely to receive tax exemption and are somewhat more likely to receive subsidies, although this second effect is only significant at the 9% level. But 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 only find 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 of government spending or other government activities in election years which primarily benefits connected firms. It appears that connected CEOs are willing to use corporate resources to grant the documented election favors. In the following section we will now investigate whether these decisions are associated with lower firm performance.

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4.2 Implications for Performance

In the final section of the paper, we turn to a discussion of 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 and vice versa. 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, the market for corporate control in France was largely inactive from the mid 1980s till early 2000, which is the period that we study, and powerful CEOs were in a position to appoint all directors without much shareholder resistance.[12]

4.2.1 Cross-Section Performance of Connected Firms

As a first step in Table 7, we document the relationship between firm performance and the presence of a politically connected CEO at the corporate head. We measure consolidated accounting performance as return on assets (ROA) at the group level to eliminate biases stemming from transfer pricing or double counting that would be present if we look at subsidiary level performance. We present our results for all firms in the sample, excluding the finance, insurance and real estate (FIRE) sectors of the economy, since ROA is not seen as a meaningful measure of performance for these firms. We also present our results for the sub-sample of manufacturing firms.

We first simply regress firm ROA on CEO background characteristics in the cross section, controlling only for year fixed effects (model 1 in Table 7). We know from the descriptive statistics that politically connected CEOs head systematically different companies (larger, more likely to be formerly state-owned, etc.). So, in columns 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 2-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

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[12]Orange (2006) provides vivid accounts of such events. In a recent book, Claude Bébéar (2003), a prominent figure in French business, mentions 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 (2006) provide a more systematic study of the relationship between political connections and board composition.

Marche." 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 impose any causal interpretation on the estimated relationships between firm performance and CEO characteristics, as other unobserved firm characteristics could be correlated with CEO characteristics and have an independent impact on performance.

We consider various CEO background characteristics that we expect to be correlated with their level of political connections, going from the more general, e.g. ENA graduation as a proxy for social connections to the political elites, to the more precise, e.g. whether the CEO was formally a "membre de cabinet" (the variable we use to define "connected CEO" in the paper).

ENA graduation is associated with about 1.5% lower ROA (Panel 1). Most of this lower performance, however, can be explained by the industry, size, and previous state-ownership of the firms that ENA graduates manage. In the second panel, we correlate performance to both ENA graduation and whether the CEO is a former civil servant. We find a more robust negative correlation between prior public sector experience and performance, especially in the sub-sample of manufacturing firms. While this correlation is weakened by the addition of firm characteristics, it stays negative and significant under all estimation models in the sample of manufacturing firms.

Panel 3 assesses whether the negative correlation between performance and past public sector experience relates to the number of years the CEO spent in the public sector. We find that tenure in the public service is negatively correlated with firm performance. 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 "membre de cabinet." Consequently, in Panel 4, 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 "membre de cabinet." The negative correlation is robust to the addition of firm characteristics for the sub-sample of manufacturing firms. Manufacturing firms managed by former "membres de cabinet" have rates of return on assets that are about 2% below that of the average firm in their industry, size and former-SOE status category.

Finally, Panel 5 breaks down the set CEOs that were formally "membre de cabinet" into those that served under a right-wing administration and those that served under a left-wing administra-

tion. We find roughly the same negative correlation between firm performance and prior cabinet membership for right-wing and left-wing CEOs.

In summary, the results in Table 7 suggest an overall negative correlation between a firm's performance and the political-connectedness of the CEO that manages that firm. However, to the extent that we cannot control for all the performance-relevant firm characteristics, this negative correlation cannot be regarded as causal. In particular, it is possible that connected CEOs are systematically chosen to head firms that are economically weaker, as these firms may be most in need of the subsidies or other benefits associated with political connections.

4.2.2   Performance and CEO Turnover

In Table 8 we go a step further and 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.[13]

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 7. However, our empirical approach in Table 8 is still 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.

We find no systematic change in ROA when a non-connected CEO replaces another non-connected CEO (row 1 of Table 8). 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.

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[13]We focus here on firms that experience only one CEO turnover episode during the sample period. This is done to ensure enough management stability before and after the turnover to "attribute" corporate performance to management. This restriction however does not affect our results economically or statistically.

In summary, we find no strong evidence of a positive correlation between firm performance and the appointment of a connected CEO in France. In fact, our findings in Tables 7 and 8 are more suggestive of a negative correlation. This is in contrast with results in earlier papers that have focused on the performance implications of the firm-politics networks. For example, both Fisman (2001) and Faccio (2006) find positive stock market valuation effects for firms that are politically connected. Our evidence suggests that the net benefits of political connections might be lower in France, maybe because in a established democracy with stable institutions the benefits from political patronage could be naturally more restricted than in a dictatorship like Indonesia, where the dictator presumably has a large amount of discretion.

4.2.3 Performance and Labor Expenses

Finally, in Table 9 we try to establish a more direct link between these surprising ROA results and the possibility that political pressures play a role in on the employment practices of connected firms. Given that political consideration seem to affect job allocation, but not economic activity (as shown in Table 6), we would expect lower performance (profitability) for connected firms that have more plants/jobs in unstable areas. In addition, we would expect larger labor-related expenses to be an important driver of this lower performance.

Each row in Table 9 corresponds to a different regression. We perform this analysis both in the panel of subsidiaries (Panel A) and in the panel 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 employment that is located in politically contested cities, as defined above. The dependent variable for each regression is that listed in the relevant row. All regressions also include year dummies, a dummy for "Connected CEO," 2-digit industry dummies, a dummy for whether the firm was formally state-owned, the logarithm of total assets. Moreover, we also interact the former state-ownership dummy as well as the 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 6, panel B, column

1, 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. Everything 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 stable cities to operating exclusively in unstable cities. We know, however, from Table 6, panel B, column 2, that sales are not higher for such subsidiaries. In row 2, we look at the sales/assets ratio as the outcome variable and find the same result: this ratio is not different for politically connected subsidiaries in unstable cities. For such firms, operating profits should thus be smaller, for 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 as a higher fraction of their employment is located in unstable areas. The estimated coefficient on the interaction term of interest is -.012 ($p = .01$). Hence, everything else equal, the ROA of a connected firm would increase by about 1.2 percentage points if that firm moved its operations from exclusively politically unstable cities to exclusively politically stable 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 *plus* 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 as a higher fraction of their operations is in areas where they are likely exposed to more political pressures; 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: 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 (even though less precise): firms managed by connected CEOs have lower ROA the higher the fraction of their employment in unstable cities; this negative effect disappears after one factors out labor costs from the ROA calculation. Hence, overall, the point estimates in rows 4, 5, 6 and 7 all suggest that higher labor expenses are an important component of the lower performance of politically connected firms.

5 Conclusion

While previous research has focused on the advantages firms can derive from maintaining connections to politicians, we consider the opposite perspective in this paper. We investigate whether CEOs that have ties to the political leadership are ready to take political business cycles into account when making employment decisions to help political incumbents 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 cities that are traditionally more contested. Consistent with the idea that these employment practices might be disruptive to firm performance, we find that accounting performance at firms managed by connected CEOs are 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 however 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, both an analysis of the cross-section of firms or events of CEO turnovers 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, differently from the 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			
ENA graduates	0.07	0.52	0.18	0.04	0.13
Polytechnique graduates	0.11	0.41	0.23	0.12	0.10
Former bureaucrats	0.11	0.63	0.30	0.10	0.15
Obs.	11,567	7,795	1,645	3,328	2,080
		Panel B: Only CEOs that are former bureaucrats			
ENA graduates	0.54	0.79	0.55	0.32	0.78
Polytechnique graduates	0.39	0.43	0.44	0.58	0.25
Tenure in the civil service	12.2	15.8	12.7	9.6	15.0
Former "membre de cabinet" of which:	0.50	0.80	0.50	0.42	0.53
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
Obs.	1,289	902	501	247	308

Sources: *DAFSA* yearbook of corporations listed on the French stockmarkets 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; firms are 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; column 4 focuses on the subset of firms in the manufacturing sector; column 5 focuses on the subset of firms in the financial sector. In Panel B, these various samples are further restricted to the subset of firms that are managed by former bureaucrats.

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 (0.28)	-0.32 (0.30)	-0.66 (0.38)	-0.36 (0.51)	.	.	.	.
Change in city unemp. rate × % emp in manufacturing	.	-0.75 (0.30)	.	-0.69 (0.41)	.	.	.	.
% emp. in manufacturing	.	0.01 (0.01)	.	0.00 (0.01)	.	.	.	.
City employment growth	.	.	.	.	0.03 (0.04)	.	0.06 (0.05)	.
City employment growth - intensive margin	.	.	.	.	.	-0.04 (0.07)	.	-0.09 (0.08)
City employment growth - due to plant creation	.	.	.	.	.	0.10 (0.06)	.	0.16 (0.07)
City employment growth - due to plant destruction	.	.	.	.	.	0.08 (0.06)	.	0.16 (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 to 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 to 4 only use the 1995 and 2001 municipal elections. In columns 5 to 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.

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
Obs.	237958	216526	257618	239877	257618	239877
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
Obs	237958	229638	257618	253061	257618	257517

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," 0 otherwise. "Election year" is a dummy variable that equals 1 if the year is a municipal election year, 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, 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. See text for details.

Table 4:
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
Obs	211791	28577	162689	235991	47587	181580	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, e.g. 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 "membre de cabinet" under a LW (RW) 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. See text for details.

Table 5:
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 × Election Year	.	0.005	.	0.006	.	-0.003
		(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 × Mayor was minister	-0.000	-0.000	0.000	-0.000	0.001	0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Firm was formally state-owned × Mayor was minister × Election year	-	-0.001	-	0.000	-	0.001
		(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
Obs	229724	229724	322625	322625	322615	322615
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," 0 otherwise. "Mayor was minister" is a dummy variable that equals 1 if the city mayor previously held a ministerial post, 0 otherwise. "Election year" is a dummy variable that equals 1 if the year is a municipal election year, 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. See text for details.

Table 6:
What Do Firms Gain By Granting Favors to Politicians ?

Dependent Variable:	employ. (1)	Sales (2)	Inputs/sales (3)	Taxes (4)	Subsidies (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
Number of Obs	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
Number of Obs	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, 0 otherwise. "Subsidies" is a dummy variable that equals 1 if the subsidiary received any subsidies in that year, 0 otherwise. "Connected CEO" is a dummy variable that equals 1 if the CEO was formally a "membre de cabinet," 0 otherwise. "Fraction of employment in swing cities" measures the fraction of the subsidiary's employment that is 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. See text for details.

Table 7:
Performance of Firms Managed by Former Bureaucrats

| | Model 1 | | Model 2 | | Model 3 | |
	Real	Manuf	Real	Manuf	Real	Manuf
CEO is ENA graduate	-0.015	-0.014	-0.003	-0.009	-0.003	-0.009
	(0.009)	(0.014)	(0.008)	(0.013)	(0.008)	(0.014)
CEO is ENA graduate	0.002	-0.000	0.001	-0.001	0.000	-0.000
	(0.009)	(0.014)	(0.008)	(0.014)	(0.001)	(0.014)
CEO was bureaucrat	-0.021	-0.020	-0.005	-0.014	-0.005	-0.016
	(0.005)	(0.007)	(0.005)	(0.008)	(0.005)	(0.009)
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)
CEO was bureaucrat,	-0.012	-0.011	-0.000	-0.006	-0.001	-0.008
not "membre de cabinet"	(0.007)	(0.010)	(0.007)	(0.010)	(0.007)	(0.010)
CEO was "membre de cabinet"	-0.030	-0.033	-0.010	-0.028	-0.011	-0.030
	(0.005)	(0.007)	(0.005)	(0.008)	(0.005)	(0.008)
CEO was bureaucrat,	-0.012	-0.011	-0.000	-0.006	-0.001	-0.003
not "membre de cabinet"	(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 return on assets. 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 Marche," 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. See text for details.

Table 8:
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 a "on cabinet" CEO. See text for details.

Table 9:
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," 0 otherwise. In Panel A (B) "Fraction of employment in swing cities" is the fraction of the subsidiary (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, 2-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." See text for details.