

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

Marianne Bertrand

Francis Kramarz

Antoinette Schoar

David Thesmar*

November 10, 2006

Abstract

This paper tests whether politically connected CEOs alter their hiring and firing decisions in order to help incumbent politicians in their reelection 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 and plant creation, and a lower rate of plant destruction, in election years. This is especially true when the firms' operations are located in politically contested areas. We find some evidence suggesting that these election favors follow partisan lines on the political left and are stronger for incumbents who have more political clout within their party. We only find weak evidence that the favors extended by connected CEOs to politicians might in part be reciprocated through privileged access to subsidy programs. Overall, politically connected firms show lower profits as the fraction of their employment located in politically contested areas increases, due to higher wage bills.

*University of Chicago Graduate School of Business, NBER, CEPR and IZA; CREST-INSEE and CEPR; MIT Sloan, NBER and CEPR; HEC and CEPR. We thank Florian Ederer, Mara Faccio, Philippe Maitreau, Andrei Shleifer, Justin Wolfers and seminar participants at CREST, MIT, ESSLE, the NBER Corporate Finance meeting, the CEPR symposium on financial markets, the ASSA meetings, Stanford University and the University of California at Berkeley for many helpful comments on an earlier draft. We are also grateful to Johan Hombert for excellent research assistance.

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 relationship 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 analyze whether firms headed by politically connected CEOs alter 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). We define politically connected firms as those firms whose CEO has served as a government employee before taking a private sector position. The idea is that managers can help incumbent politicians stay in power by accelerating hiring, or postponing layoffs and plant closures before election year. Managers of connected firms might be more willing to engage in such practices because of the favors the incumbent politician have granted them in the past, and the expectations that these favors will continue if the incumbent is re-elected. Alternatively, connected managers could be more susceptible to political pressures to boost employment figures because of personal or ideological ties to politicians.

Our results show that politically connected firms in France indeed hire more workers, set up more plants and avoid plant closures prior to election years especially in politically contested regions. There is limited evidence that these political connections follow partisan lines, if at all the results are only significant for the political left. Moreover, incumbents who have more political clout within their party are more likely to receive business favors in form of employment generation. We only find weak evidence that the favors extended by connected CEOs to politicians might in part be reciprocated through privileged access to subsidy programs. Overall, politically connected firms have lower profits as the fraction of their employment located in politically contested areas increases, due to higher wage bills. These findings suggest that political connections might not only provide benefits to connected firms as suggested in the earlier literature. But in turn these can

create costs, if managers adjust employment decision to further political goals.

The idea that economic variables are manipulated for political purposes is certainly not new. Starting with Nordhaus (1975), a large literature on political business cycles has highlighted the incentives incumbent governments have to use economic policy tools to affect election outcomes.¹ Also, 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 this paper, however, we focus on the behavior of private, *publicly-traded* firms, which are not directly controlled by the government, but whose top managers are connected to politicians.

France offers a unique research setting to study the nexus between business and government, and its possible implications for business decisions. First, social and political connections between CEOs and politicians are very prevalent in France. A substantial fraction of the largest publicly-traded companies in France are managed by former civil servants. We show that former civil servants control 11% of the firms, and 63% of the assets, listed on the French stock market in the 1990s. More than half of these former civil servants also served as an advisor for a Minister in government. More generally, political and business leaders in France tend to share a common educational background, having attended the same very limited set of elite schools (known as “Grandes Ecoles”), namely ENA and Polytechnique. These shared professional and educational experiences are sources of social, political and potentially ideological connections between political and business leaders.

A second important reason to focus on France lies in the quality of the available data. Since even the biggest firms in a country might not have the

ability to affect nationwide employment trends, we will focus on the relationship between city-level politics and city-level business decisions. This requires the ability to measure hiring and firing decisions at a regionally disaggregated level for each firm. The French statistical office compiles very detailed plant level information, which allows us to measure for every firm how many jobs are created and destroyed annually, and how many plants are started and shut down annually, *in a given city*.

Our empirical strategy consists in studying differences in hiring and firing patterns at publicly-

¹See for example Alesina and Rosenthal (1995) or Alesina and Sachs (1988)

traded firms that are managed by connected CEOs compared to other publicly-traded 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. Specifically, we test whether connected CEOs engage in relatively more hiring and less firing during election years; also, we test whether connected CEOs engage in relatively more hiring and less firing in politically unstable areas (especially during election years).

A central assumption underlying our identification approach is that politically motivated employment favors a direct negative effect on a firm's financial outcomes. To the extent that CEOs of publicly-traded firms have only limited discretion to engage in such practices, political favors will be granted selectively, and used predominantly in situations where they can provide the largest political gains. Under this assumption that voters are myopic, a view that is common to many political economy models, 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 politically motivated employment creation will be most 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 underlying our empirical tests is that connected CEOs are more likely to respond to the needs of political incumbents than to the needs of opposition candidates. We can see at least two justifications for this assumption. First, while there is a direct relationship between boosting employment close to an election and improving the re-election chances of the incumbent, business practices aimed at depressing employment close to an election would have a more diffuse effect. They would reduce the re-election chances of the incumbent, 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 verify that changes in local labor market conditions affect the reelection 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. These findings confirm 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.

We then report our main findings: We find higher employment growth, as well as a higher rate of plant creation and a lower rate of plant destruction for firms managed by politically connected CEOs, especially during election years. In addition, politically connected firms create more jobs in politically unstable cities, and are also more likely to create new plants and less likely to destroy existing plants in those cities. These effects again are particularly large in municipal election years. Importantly, we show that these employment patterns are robust to accounting for a set of firm characteristics that systematically vary with the political background of the CEO, such as firm size and whether the firm was formerly state-owned.

We then analyze the nature of the relationship between connected CEOs and politicians. First, we consider the possibility that the political connections follow ideological party lines. We study whether CEOs that are connected to a right-wing government are more likely to bestow favors on right-wing political incumbents, and vice versa for left-wing CEOs. We find some evidence for this behavior, but exclusively for the left wing. 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 find that incumbent mayors who previously served as ministers in the central government receive somewhat larger election favors, even though the overall magnitude of these effects is small. This result suggests that more influential politicians are more able to extract political favors. Third, we explore whether the election favors are part of a two-way gift exchange between politicians and connected CEOs. We focus on two firm outcomes that might be affected by political favors: subsidies and taxes.² 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. However, we only find very limited evidence for reciprocity by politicians. While politically connected firms seem to be somewhat more likely to receive subsidies we do not find that firms managed by connected

²Optimally, 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.

CEOs are more likely to receive tax exemptions when more of their plants are located in politically contested cities.³

Finally, we analyze the implications of our findings for the performance of politically connected firms. 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 is related to a higher wage bill for these firms. Earlier work in other countries has documented the financial benefits firms derive from political connections. Our findings, however, raise the possibility that political connections can also entail 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.

The rest of the paper is organized as follows. Section 2 provides 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. Section 3 introduces the main datasets and describes sample construction. Our main findings are reported

in Section 4 and we conclude in Section 6.

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

³While 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 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.

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.⁴

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

2.2.1 CEO Data

As we explain in more detail in Section 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.

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,

⁴Two 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.

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* having graduated from ENA or Ecole Polytechnique 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).⁵

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.

3 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.

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

⁵As 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.

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.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 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.

4 Findings

4.1 Are Election Results Responsive to Employment Conditions?

The political business cycle literature has shown that voters appear to hold incumbent politicians accountable for current economic performance. In fact, voters sometimes attribute developments at the local- or country-level to incumbent politicians even when the politicians had no control over these developments, such as those driven by weather patterns or world business cycles. One economic performance indicator that is of special interest to voters is employment. This appears especially true in France, where job security and unemployment figures are central themes in every political campaign. In this section, we formally verify that the French electorate does factor in current employment conditions when deciding whether to re-appoint the incumbent party in municipal elections. 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

⁶We 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.

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

As expected under the view that the electorate is sensitive to employment conditions, 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 ask whether the electorate is also sensitive to city-level employment changes induced by private-sector plants. Using the plant-level dataset described above, we 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 find a positive but statistically insignificant relationship between the 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 in column 8, where each city is weighted by its population. In other words,

⁷We 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.

the creation of new plants in a city helps the incumbent party and the destruction of old plants hurts the incumbent party. 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; column 8).

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 predictively 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.

All in all, 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.

4.2 Do Connected CEOs Grant “Favors” to Political Incumbents?

We now proceed to analyze whether employment patterns at firms managed by connected CEOs systematically differ around election time. For the purpose of this analysis, we define “connected CEO” as a dummy variable that equals 1 if the CEO that currently heads a given publicly-traded firm was a “membre de cabinet” (e.g. held a post in government) 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. Later on, we will also breakdown the group of connected CEOs into two sub-groups: those that served under a right-wing administration and those that served under a left-wing administration.

Under the view that politically connected CEOs grant favors to the incumbent party, we would expect them to create relatively more jobs, open more plants, and shut down fewer plants, around election time. Also, because such favors are more valuable to those incumbents that face more uncertain re-election outcomes, we would expect connected CEOs' job creation and destruction

decisions to deviate especially from that of non-connected CEOs in cities that are politically less stable. 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 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, who are most able to influence aggregate outcomes 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 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 1999.⁸

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 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 state-owned. In other words, we allow for systematically different employment practices by previously state-owned firm 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

⁸We 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.

⁹In 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.

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 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.

4.3 Partisanship, Mayors’ Political Clout, and Gift Exchange

In this section, we report on a few additional results aimed at deepening our understanding of the relationship between connected CEOs and politicians. We first do this by exploiting additional available information about the CEOs’ and local politicians’ backgrounds: specifically, we ask whether politically connected CEOs tend to favor, through their employment practices, those incumbent politicians with whom they share the same political orientation; we also ask whether more powerful political incumbents are especially likely to receive favors from connected CEOs. Second, we investigate, within the constraints of the available data, whether connected CEOs are themselves more likely to be the recipients of favors granted by politicians; in other words, we ask whether the “employment favors” we observe appear to be part of a two-way exchange between politicians and firms.

4.3.1 Do Favors Follow Partisan Lines?

Are CEOs that formerly served under a right-wing government especially likely to alter their employment practices to support right-wing political incumbents, and vice versa for left-wing CEOs? This is the question we address in Table 4.

The dependent variables in Table 4 are the same as in Table 3: change in city-level employment by a subsidiary in a given year, a dummy for whether any plant was created by a subsidiary in a given city and given year and a dummy for whether any plant was destroyed by a subsidiary in a given city and given year. The independent variables of interest are now 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 include 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 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 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. 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.

4.3.2 Do Politically More Powerful Mayors Receive More Employment Favors?

Table 5 investigates another facet of the relationship between connected CEOs and local politicians: are mayors with more political clout receiving larger employment favors from connected CEOs? 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

¹⁰The 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.

(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.

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. While small in magnitude, 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, political clout 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.

4.3.3 Do Politicians Reciprocate the Favors They Received?

It is possible, especially in an environment characterized by poor corporate governance, that ideological, social or personal relationships are enough to explain why connected CEOs would distort employment practices at their firm to help local politicians. However, it is also possible that politically connected CEOs grant such employment favors because of benefits they have received, or expect to receive, in return. Indeed, a number of recent papers have pointed out how political connections can be beneficial to firms. While our data unfortunately does not capture some important firm outcomes that might be affected by political connections (such as access to low interest rate loans or, in our local politics environment, allocation of city contracts), we do however observe

two important levers through which politicians can affect business outcomes: taxes and subsidies. Under the view that the favors granted by connected CEOs are reciprocated by politicians, we would expect firms managed by connected CEOs to be more likely to receive tax exemptions and also to be more likely to receive subsidies. Moreover, we would expect this preferential access to tax exemptions and subsidies to be concentrated among those connected firms whose activities are mostly located in politically contested areas. This is the analysis we perform in Table 6.¹¹

The unit of observation in Table 6 is at the subsidiary-year level.¹² The first 3 columns of Table 6 focus on taxes and the last 3 columns focus on subsidies. Specifically, we are interested in whether a subsidiary received a tax exemption in a given year; hence, we define a dummy variable that equals 1 if the subsidiary paid a strictly positive tax amount in a given year, 0 otherwise. Similarly, we construct a dummy variable that equals 1 if the subsidiary received any subsidies in a given year, 0 otherwise. In all regressions in Table 6, we include year and (18) industry fixed effects, as well as fixed effects for the publicly-traded firm a given subsidiary belongs to. In addition, we control for the logarithm of total assets and for whether the subsidiary belongs to a firm that was previously state-owned. Standard errors are corrected to account for arbitrary correlation of the error term across observations that correspond to the same publicly-traded firm.

In columns 1 and 4, we first simply relate the tax and subsidy dummies to whether the subsidiary is currently managed by a politically connected CEO. We find that connected firms are about two percentage points more likely to receive any subsidies ($p = .09$); they are also about 1 percentage point less likely to pay any taxes in a given year, but this difference is not statistically significant. This provides some weak support of access to political rents among connected firms.

A more direct test of the “gift exchange” argument outlined above is presented in the remaining columns of Table 6. There, we try to specifically test whether a subsidiary’s access to political rents (e.g. tax exemptions and subsidies) is related to where the subsidiary is located. For each subsidiary, we compute the fraction of their total employment that is located in politically unstable cities. The

¹¹Obviously, our analysis cannot capture any benefits given in the form of personal perks. Moreover, our analysis cannot capture potential benefits that are granted outside our sample period. For example, connected CEOs might have used their political connections to obtain their position in the first place. In return, the connected CEOs might be expected to help the re-election of the party that helped them to obtain their current job.

¹²We do not observe city-level taxes or subsidies in our data set; we can only measure taxes and subsidies at the subsidiary-year level.

coefficient of interest is now that on the interaction term between “Connected CEO” and “Fraction of employment in swing cities.”

In the model estimated in columns 2 and 5, we also include an interaction term between “Fraction of employment in swing cities” and a dummy variable for whether the subsidiary belongs to a firm that was previously state-owned. In both columns, the estimated coefficient on the “Connected CEO*Fraction of employment in swing cities” is statistically significant. However, that coefficient is only of the “expected” sign in column 5. Specifically, connected firms are more likely to receive subsidies as a larger share of their operations is in contested cities; but they are however also more likely to pay taxes as a larger share of their operations is in contested cities.

Columns 3 and 6 respectively replicate columns 2 and 5 but allow for the relationship between firm outcomes and employment location to vary with other firm characteristics. Specifically, the estimated model now includes an interaction term between “Fraction of employment in swing cities” and the logarithm of total assets; also, the model includes interaction terms between “Fraction of employment in swing cities” and the 18 industry dummies. As one can see, the estimated coefficients on “Connected CEO*Fraction of employment in swing cities” are remarkably stable to the inclusion of these additional controls. In summary, only our findings regarding access to subsidies are consistent with a two-way gift exchange between politicians and connected firms.

5 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 of distorting employment decisions, these connected firms should perform better than non-connected ones, and vice versa. Of course, under the view that profit-maximizing boards decide to appoint connected CEOs only if they believe that the benefits of political connections will outweigh their costs, one would expect that, on net, connected CEOs will have a positive net impact on firm performance. However, this needs not be true if there are failures to the corporate governance system. As it turns out, corporate governance was far from perfect in France in the period under study here. The market for corporate control was largely inactive, as most firms were protected from hostile takeovers through a very dense network

of cross shareholding put in place during the privatizations of the late 1980s and early 1990s. Also, until the “Financial Security Act” (Loi de Sécurité Financière) was passed in 2003, institutional shareholders, be they foreign or domestic, did not bother to vote in general assembly. In fact, many observers would argue that, over the 1987-2003 period we study, powerful CEOs were in a position to appoint all directors without much shareholder resistance.¹³

As a first step, we simply document the relationship between firm performance and the presence of a connected CEO at the corporate head. We perform this exercise in Table 7, where the unit of observation is a publicly-traded firm in a given year. We use return on assets (ROA) as a measure of accounting performance. We present our results for all firms in the sample, excluding the finance, insurance and real estate (FIRE) sectors of the economy, as we do not expect ROA to be as meaningful a 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, controlling only for year fixed effects (model 1 in Table 7). Of course, we know from earlier descriptive statistics that politically connected CEOs head systematically different companies (larger, more likely to be formerly state-owned, etc.). So, under 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 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 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, to the more

¹³Orange (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.

precise, e.g. whether the CEO was formally a “membre de cabinet” (the variable we use to define “connected CEO”) in the rest of this analysis).

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 administration. 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.

Table 8 goes a step further in studying the impact of CEOs' political connections on firm performance. In that table, we study changes in firm-level ROA around CEO turnover episodes. In doing so, we contrast three different turnover cases: 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.¹⁴

This analysis allows us to fully 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.

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 we focus on a democracy where election considerations, and the subsequent costs they impose on connected firms, might be more important. However, we stress again that our empirical design in both Tables 7 and 8 falls short of estimating the true causal effect of appointing

¹⁴We 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.

a connected CEO for performance.

In our last table, Table 9, we try to more formally establish that these surprising ROA results might indeed be related to the impact of political pressures on the employment practices of connected firms. Based on our findings above, we would expect lower performance 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 two labor outcomes: the logarithm of total employment and wage bill over total assets. This allows us to verify that our earlier findings (derived at the city-subsi- diary-year level) are robust to this alternative structure of the data (subsidiary-year level). Row 1 indicates that, everything else equal, a subsidiary managed by a connected CEO would have about 8% higher employment if it moved from operating exclusively in stable cities to operating exclusively in unstable cities ($p = .079$). Row 2 indicates that, 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. Row 3 shows that these labor effects do not reflect differential sales patterns. The dependent variable in row 3 is sales over total assets. The estimated coefficient on the interaction term of interest is negative in row 3, even though not statistically significant.

In row 4, we turn to subsidiary-level ROA as the outcome measure. 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 5, 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.

6 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 distort their business decisions to help these political leaders get reelected. 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 creation and destruction. Publicly-traded firms man-

aged 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 municipal 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 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. 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), it does seem to suggest that differently from the analysis in other countries political connections might have a large cost

More research will be needed to understand whether e favors are mostly returned in the form of private benefits, especially in a weak corporate governance context. In particular, we would like to learn more in subsequent work about the process by which these former bureaucrats land in top jobs in publicly-traded firms, and the role that political connections play in this process.

7 References

- Alesina, Alberto, and Howard Rosenthal, 1995, *Partisan Politics, Divided Government, and the Economy*, Cambridge University Press: Cambridge, U.K.
- Alesina, Alberto and Jeffrey Sachs, 1988, "Political Parties and the Business Cycle in the United States 1948-1984," *Journal of Money, Credit, and Banking*, vol 20, 63-82.
- Bébéar, Claude (2003) *Ils vont tuer le capitalisme*, Editions Plon: Paris, France.
- Bertrand, Marianne, Antoinette Schoar and David Thesmar, "Banking Deregulation and Industry Structure: Evidence From the French Banking Reforms of 1985," *Journal of Finance*, forthcoming.
- Cassis, Youssef, 1999, *Big Business in Europe*, Oxford University Press: Oxford, U.K.
- Faccio, Mara, Ronald W. Masulis and John J McConnell, "Political Connections and Government Bailouts," *Journal of Finance* (forthcoming).
- Faccio, Mara, 2006, "Politically-Connected Firms," *American Economic Review*, vol. 96 (1), 369-386.
- Fisman, Raymond, 2001, "Estimating the Value of Political Connections," *American Economic Review*, vol. 91, 1095-1102.
- Garrigues, Jean, 2002, *Les Patrons et la Politique: De Schneider a Seilliere*, Editions Perrin: Paris, France.
- Johnson, Simon and Todd Mitton, 2003, "Cronyism and Capital Controls: Evidence from Malaysia," *Journal of Financial Economics*.
- Kramarz, Francis, and Thesmar, David, 2006, "Social Networks in the Board Room," CEPR Discussion Paper No. 5486.
- Melitz, Jacques, 1990, "Financial Deregulation in France," *European Economic Review*, vol 34, 394-402.
- Nordhaus, William, 1975, "The Political Business Cycle," *Review of Economic Studies*, vol 42, 169-190.
- Orange, Martine, 2006, *Ces messieurs de Lazare*, Editions Albin Michel: Paris, France.
- Sapienza, Paola, 2003, "The Effects of Government Ownership on Bank Lending," *Journal of Financial Economics*.
- Shleifer, Andrei and Robert W. Vishny, 1994, "Politicians and Firms," *The Quarterly Journal of Economics*.

Yvert, Benoit, 2002, *Dictionnaire des Premiers Ministres et Présidents du Conseil*, Editions Perrin: Paris, France.

Wolfers, Justin, 2002, "Are Voters Rational? Evidence from Gubernatorial Elections," Research Paper No. 1730, Stanford University.

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

	(1)	(2)	(3)	(4)	(5)
	Fraction Firms	Fraction Assets	Fraction Old SOEs	Fraction Manuf.	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”	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
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?

	<i>Dependent Variable:</i> <i>Change in % votes to incumbent party</i>							
	(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?

<i>Dependent Variable:</i>	<i>Employ. Change</i>		<i>Plants Created</i>		<i>Plants Destroyed</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Election year × Connected CEO	0.040 (0.023)	0.032 (0.016)	0.054 (0.030)	0.065 (0.030)	-0.034 (0.013)	-0.025 (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 city × Connected CEO	.023 (.009)	-.001 (.008)	.012 (.005)	-.000 (.002)	-.033 (.019)	-.006 (.012)
Election year × Swing city × Connected CEO	.	.020 (.012)	.	.008 (.004)	.	-.018 (.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?

<i>Dependent Variable:</i>	<i>Employ. Change</i>			<i>Plants Created</i>			<i>Plants Destroyed</i>		
	All	Elec. Years	Swing Cities	All	Elec. Years	Swing Cities	All	Elec. Years	Swing Cities
Sample:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LW CEO × LW City	0.015 (0.008)	0.010 (0.005)	0.029 (0.008)	0.057 (0.029)	0.013 (0.024)	0.019 (0.015)	-0.043 (0.023)	-0.048 (0.023)	-0.033 (0.018)
RW CEO × RW City	-0.013 (0.011)	-0.005 (0.021)	-0.019 (0.011)	0.003 (0.009)	-0.021 (0.024)	0.008 (0.011)	-0.006 (0.008)	-0.023 (0.026)	-0.006 (0.010)
Firm was formally state-owned × RW City	-0.009 (0.013)	-0.043 (0.032)	-0.020 (0.013)	0.002 (0.015)	0.047 (0.033)	-0.002 (0.004)	0.008 (0.014)	0.007 (0.005)	0.010 (0.015)
Firm was formally state-owned × LW City	0.004 (0.013)	-0.011 (0.018)	-0.052 (0.020)	-0.023 (0.008)	-0.018 (0.019)	-0.031 (0.012)	0.003 (0.014)	0.041 (0.036)	0.018 (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?

<i>Dependent Variable:</i>	<i>Employment Change</i>		<i>Plants Created</i>		<i>Plants Destroyed</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Connected CEO × Mayor was minister	0.007 (0.003)	0.001 (0.001)	0.010 (0.005)	0.001 (0.001)	-0.002 (0.001)	-0.000 (0.001)
Connected CEO × Mayor was minister × Election Year	.	0.005 (0.002)	.	0.006 (0.003)	.	-0.003 (0.002)
Connected CEO × Election year	.	-0.011 (0.011)	.	0.003 (0.021)	.	0.000 (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 (0.001)	-0.000 (0.000)	0.000 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)
Firm was formally state-owned × Mayor was minister × Election year	-	-0.001 (0.001)	-	0.000 (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:
Do Politicians Reciprocate Favors?
Taxes and Subsidies

<i>Dependent Variable:</i>	<i>Taxes</i>			<i>Subsidies</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
Connected CEO	-0.008 (0.009)	-0.030 (0.017)	0.026. (0.013)	0.016 (0.008)	0.002 (0.010)	0.001 (0.010)
Connected CEO × Fraction emp. in swing cities	-	0.043 (0.021)	.036 (0.014)	-	0.029 (0.016)	0.029 (0.014)
Firm was formally state-owned × Fraction emp. in swing cities	-	0.003 (0.029)	-0.023 (0.016)	-	0.051 (0.017)	0.039 (0.018)
Fraction emp. in swing cities × firm size an industry fixed effects?	No	No	Yes	No	No	Yes
Year F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
Group F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.?	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R ²	0.37	0.40	0.41	0.30	0.17	0.18
Number of Obs	84640	84640	84640	84640	84640	84640

Notes: Each observation in the dataset corresponds to a given subsidiary in a given year. “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 (0.009)	-0.014 (0.014)	-0.003 (0.008)	-0.009 (0.013)	-0.003 (0.008)	-0.009 (0.014)
CEO is ENA graduate	0.002 (0.009)	-0.000 (0.014)	0.001 (0.008)	-0.001 (0.014)	0.000 (0.001)	-0.000 (0.014)
CEO was bureaucrat	-0.021 (0.005)	-0.020 (0.007)	-0.005 (0.005)	-0.014 (0.008)	-0.005 (0.005)	-0.016 (0.009)
CEO was bureaucrat	-0.014 (0.005)	-0.011 (0.008)	0.001 (0.005)	-0.006 (0.008)	0.000 (0.006)	-0.007 (0.009)
Tenure in bureaucracy (in decades)	-0.009 (0.006)	-0.014 (0.008)	-0.009 (0.005)	-0.015 (0.007)	-0.010 (0.005)	-0.016 (0.007)
CEO was bureaucrat, not “membre de cabinet”	-0.012 (0.007)	-0.011 (0.010)	-0.000 (0.007)	-0.006 (0.010)	-0.001 (0.007)	-0.008 (0.010)
CEO was “membre de cabinet”	-0.030 (0.005)	-0.033 (0.007)	-0.010 (0.005)	-0.028 (0.008)	-0.011 (0.005)	-0.030 (0.008)
CEO was bureaucrat, not “membre de cabinet”	-0.012 (0.007)	-0.011 (0.010)	-0.000 (0.007)	-0.006 (0.010)	-0.001 (0.007)	-0.003 (0.007)
RW CEO	-0.031 (0.006)	-0.034 (0.008)	-0.011 (0.005)	-0.029 (0.008)	-0.012 (0.005)	-0.030 (0.008)
LW CEO	-0.027 (0.009)	-0.030 (0.012)	-0.008 (0.007)	-0.026 (0.016)	-0.009 (0.008)	-0.031 (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 "non 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:		
<i>Dependent Variable:</i>	<i>Log employment</i>	.083 (.047)
	<i>Wage bill/total assets</i>	.021 (.011)
	<i>Sales/total assets</i>	-.021 (.028)
	<i>ROA</i>	-.012 (.004)
	<i>ROA + wage bill/total assets</i>	.004 (.015)
Panel B: Group-level Analysis:		
<i>Dependent Variable:</i>	<i>ROA</i>	-.028 (.018)
	<i>ROA + wage bill/total assets</i>	.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 1 if 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.” Also included in regressions in rows and 2 is sales over total assets. See text for details.