The value of local political connections in a low-corruption environment

Mario Daniele Amore[†]
Bocconi University

Morten Bennedsen[‡]
INSEAD

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Abstract

We use exogenous changes in Danish local municipality sizes to identify a large positive effect of political power on the profitability of firms related by family to local politicians. Our difference-in-differences estimate is consistent with a unitary elasticity of connected firms' performance to political power (as measured by population per elected politician). Increasing power boosts firms' operating returns, especially in industries relying heavily on public demand. We confirm our main finding via several alternative models. Focusing on arguably the world's least corrupt country, we highlight the importance of corporate rent seeking at local governmental levels, which account for nearly half of total public expenditures.

JEL: G30, G34, G38, D72, D73

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[†] Assistant Professor, Bocconi University. Email: mario.amore@unibocconi.it

[‡] André and Rosalie Hoffmann Chaired Professor of Family Enterprise and Professor of Economics, INSEAD. Corresponding author email: morten.bennedsen@insead.edu

1. Introduction

Connections between firms and politicians are widespread around the world. Faccio (2006) documents the existence of publicly traded firms with national political connections in 35 of 45 countries; these firms account for nearly 8% of the world's stock market capitalization. She also documents that national political connections are valuable, especially in countries with weak political institutions.

In this paper, we explore the value of local political connections in a low-corruption environment. We use an administrative reform that generates exogenous variations in the size of local municipalities in Denmark to establish the effect of changes in political power on the profitability of firms that have family ties with local politicians. On average, we find that (1) doubling the political power (as measured by population per elected politician) doubles the performance of politically connected firms, and (2) the effect is larger in industries delivering goods and services to the public sector.

Our study makes two contributions to the literature. First, we explore the value of family connections with *local* politicians in a country with strong political institutions: according to the well-respected Corruption Perceptions Index (CPI), Denmark is the world's least corrupt country in four out of the last six years.[1] Previous studies have identified the value of political connections in corrupt countries (e.g., Fisman, 2001; Johnson and Mitton, 2003; Li et al., 2008; Bunkanwanicha and Wiwattanakantang, 2009; Cingano and Pinotti, 2011), for powerful national politicians (Jayachandran, 2006;

[1] The CPI, formulated by Transparency International, ranked Denmark first in 2007, first in 2008, second in 2009, first in 2010, second in 2011, and first in 2012.

Goldman et al., 2009, 2012),[2] and in times of severe financial crisis (Acemoglu et al., 2010). Yet research has failed to establish the general effect of political connections in countries with strong political institutions. For instance, Faccio (2006) finds that political ties in countries with high levels of corruption generate a statistically significant cumulative abnormal return (CAR) of 4.32%, versus an insignificant CAR of −0.02% in countries with low levels of corruption.[3] We contribute to this field by establishing—in a highly accountable institutional environment—the corporate value of family connections with local governments, which account for almost half of total public expenditures.

Second, we provide a novel identification strategy for estimating the effect of political ties on operating performance. Event studies have been used to estimate the market value of political connections (Fisman, 2001; Faccio, 2006; Faccio and Parsley, 2009), but identifying the effect on operating profits is difficult in such studies owing to the challenge of finding appropriate counterfactuals. Our identification strategy exploits exogenous variations in political power for a given firm–politician match. These variations result from an administrative reform, implemented in 2005, whereby 238 Danish municipalities merged into 65 new ones and 33 municipalities were left unchanged. Our difference-in-differences framework establishes how the increase in political power due to the enlargement of local governments increased the profitability of firms connected with local politicians before and

[2] For instance, Goldman et al. (2009) consider senators, members of the House of Representatives, and directors of organizations such as the Central Intelligence Agency. Faccio (2006) and others using similar data (e.g., Faccio et al., 2006; Boubakri et al., 2012a) consider heads of state, ministers, and members of Parliament. Roberts (1990) detects significant benefits from being connected with Senator Henry Jackson, whereas Fisman et al. (2006) find no significant effect of being connected with Richard Cheney. Finally, in a study of connections with politicians in the German parliament, Niessen and Ruenzi (2010) show that connected firms have fewer growth opportunities but slightly superior operating returns.

[3] The notion that political connections pay off disproportionately more in corrupt than in uncorrupt economies is supported by Faccio and Parsley (2009), among others.

after the reform; for counterfactuals, we use similarly connected firms in municipalities unchanged by the reform.

Our approach presents two empirical advantages. First, we can study how exogenous changes in political power affect corporate performance within a given firm–politician match. Second, by focusing solely on connections with winning candidates, we avoid potential endogeneity problems in the formation and disruption of connections. For example, we do not use nonconnected firms or firms connected with unelected candidates, which represent poor counterfactuals because the electoral fates of connected politicians can be affected by corporate outcomes.

A premise for our identification is that the merging of local municipalities creates a positive shock to politicians' power. To support this argument, we show that the ratios of population, governmental budget, and outsourced expenses to elected politicians increased significantly in merging municipalities as compared with unchanged municipalities. We remark that the reform itself was backed up by DKK 1.2 billion to cover transitory expenses in merging municipalities only.[4]

Our estimates indicate that an increase in political power significantly improves the performance of connected firms. The average effect is consistent with an elasticity of firm performance to political power close to unity; thus, a 100% increase in population per politician nearly doubles connected firms' operating returns. This performance increase is also associated with larger firm revenues, and it is greater for firms operating in industries that depend more on public demand. Taken together, these findings support the notion that

[4] The average DKK/USD exchange rate in 2006 was 0.1681. Source: www.statbank.dk.

family ties with the political sector help a firm secure more business with local governments.

We provide additional tests to support the causal interpretation of our findings. First, we show that the municipality mergers did not affect the profitability of nonconnected firms or of firms connected with politicians who ran for local offices but were not elected. Second, we confirm that there were no significantly diverging firm-level trends—as regards merging versus nonmerging municipalities—prior to the reform. Third, we control for selection bias due to increased electoral competition in merging municipalities.[5] For this, we use two exclusion restrictions: the aggregate party vote; and the share of politicians older than 65 in the pre-reform municipalities. Fourth, we employ alternative specifications. We use a matching strategy to address the possibility that the impact of the reform is heterogeneous with respect to observable firm and political characteristics that are unbalanced across merging and unchanged municipalities. In addition, we exploit the sharp discontinuity adopted for selecting which municipalities to merge. Comparing connected firms in municipalities barely above and below the qualifying threshold allows us to mitigate concerns that the merging municipalities are characterized by, for example, declining economic or demographic trends.

We also exploit variation in politician, firm, and municipality characteristics in order to gain further insight concerning the relationship between family connections and corporate rent seeking. Sample split results are consistent with our hypothesis that firms connected to stronger politicians benefit more from that greater political power. Also, these benefits are

[5] For example, if tougher electoral competition in the merging municipalities improves the quality of re-elected politicians and if high-quality politicians are less willing to provide rent to connected firms, then focusing on re-connected firms may underestimate the effect of political power on firm performance.

primarily present among small firms and firms making little profit before the reform, thus indicating that this rent extraction may not be welfare improving. Finally, we show that the strength and persistence of political institutions matter. First, revising municipality borders had a larger connected firm effect in municipalities where the reform increased electoral competition more. Second, the effect is smaller for firms connected to politicians in municipalities that developed "persistent" social capital by virtue of having received, centuries ago, special city rights.

Section 2 describes our data, the institutional features of Danish municipalities, and the 2005 administrative reform. Section 3 presents our identification strategy and provides summary statistics, and Section 4 reports our main results. Section 5 presents a battery of falsification tests together with selection, matching, and discontinuity estimates; Section 6 shows how our average effect varies depending on individual, corporate, and municipality characteristics. Section 7 gives results on alternative corporate outcomes, and Section 8 discusses the implications of our findings.

2. Institutional background and data

2.1. Local governments and the 2005 administrative reform

Municipalities in Denmark are governed by local councils, each of which is headed by a mayor who is elected (by a simple majority) during the council's first meeting. The mayor has the overall responsibility for providing public services in various sectors: primary and secondary education; elder care; healthcare; employment; social services; special education; business services; collective transport and roads; environment and planning;

and, often, provision of electricity, water, and heating. These services account for some 48% of total public expenditures.[6]

Local councils always have an odd number of seats and anywhere from 9 to 31 members (except for Copenhagen, which has 55 members). Municipalities with more than 20,000 inhabitants had a minimum of 19 (resp., 25) seats before (resp., after) the 2005 reform. The election period is four years, and elections are held on the third Tuesday in November. Every new local government starts working on the first of January. The electoral system is proportional, and in most municipalities the parties that run for election are the same as those that run for the national election; however, local parties do exist in some municipalities. The last three local elections took place in 2001, 2005, and 2009.

Our identification builds on a change in the geographic borders of Danish local municipalities, a consequence of the administrative reform implemented with the 2005 elections.[7] Fig. 1 maps the municipalities before and after the administrative reform, and Table 1 (Panel A) details how the reform reduced the number of municipalities.

[[INSERT Fig. 1 about Here]]

[[INSERT Table 1 about Here]]

The left side of Fig. 1 shows the municipality map prior to the 2005 reform: 271 municipalities ranging from fewer than 5,000 to more than 400,000 inhabitants. The aim of

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[6] Source: "The Local Government Reform – In Brief," Ministry of the Interior and Health, Department of Economics, 2005.

[7] Counselors in the new municipalities were chosen by the local elections in November 2005. However, to ensure continued operation in the merging municipalities, the tenure of the previous councils was prolonged by one year (i.e., until the end of 2006). In this transitory period, old municipalities transferred administrative entities to the new municipalities and then were fully dissolved on January 1, 2007. The newly elected councils in municipalities *not* involved in a merger commenced their activities on January 2006 (i.e., just as if there had been no reform). Five municipalities on the island of Bornholm merged into an islandwide municipality at the very start of debate over the reform (near the end of 2002); in the empirical analysis, we exclude the few firms connected with these municipalities.

the reform was to increase economic and administrative efficiency by creating larger municipalities—that is, with at least 20,000 inhabitants. The selection of which municipalities to merge was based almost entirely on two criteria (Dreyer Lassen and Serritzlew, 2011): geography and population size.[8] The right side of Fig. 1 and Panel A of Table 1 show the reform's outcome: 238 municipalities were merged into 65 larger municipalities, while 33 mostly large municipalities were left unchanged. Hence the average municipality increased in size from approximately 159 km^2 to 440 km^2 and, in terms of inhabitants, from about 20,000 to 56,000. Panel B of the table reports the reform's effect on the number of municipalities in terms of population size.

2.2. Corporate and management data

To construct our dataset of firms connected with local politicians, we combine a number of sources. Accounting data come from Experian, a private firm that collects the annual reports that all limited liability firms are required to submit to the Danish Ministry of Business Affairs. We consider companies with nonnegative (and nonmissing) book value of assets that are present in the sample for the entire period 2002–2008. Danish law does not require private firms to disclose any more than a few financial items; these include total assets, selected measures of profitability (e.g., operating and net income), and a few variables related to capital structure. Firms need not disclose data on sales or employment,

[8] Some additional factors were at play in rare cases. A few municipalities were split into two parts, with each merging into separate larger municipalities. Two small municipalities remained independent because the ruling coalition in neighboring municipalities was of a different political orientation, and a few poor municipalities had a hard time finding neighbors willing to merge.

but about half of the firms in our sample do so voluntarily. By law, all balance sheets must be approved by external and independent accountants.

We obtain from the Danish Ministry of Business Affairs the personal identification number of all managers and board members in Danish firms from 2001 to 2006. These data include the dates of entering and exiting managerial positions, which firms are obliged to submit to the ministry within two weeks of any changes. For each personal identification number in our sample, the official Danish Civil Registration System provides us with the personal identification number of all close family members. These administrative records contain individual characteristics such as gender, birth and death dates, and marital history. We use this information to create the family tree of each CEO, manager, and director.

2.3. Family networks and political connections

The Danish Ministry of the Interior provides electoral data containing the personal identification number of all candidates in recent local elections. For each candidate, the data contain information on party affiliation, number of votes received, and electoral success (or failure).

By merging this local election data with our data on the families of CEOs and directors, we can identify firms that are family related to local politicians. By *family related* we mean a politician who is a CEO and/or a board director or who is connected by family to a firm's CEO and/or director. The family relations we consider are parent, child, sibling, and current or former spouse(s).

Using election data for 2001 and 2005, we classify firms into four groups. Firms can be connected in both electoral periods (we say they are *re-connected*) or they can be connected in 2005 but not in 2001 (these are the *newly connected*). The other groups consist of (1) firms that were not connected in either period (*unconnected*), and (2) firms that were connected in 2001 but not in 2005 (*disconnected*).

3. Empirical strategy and summary statistics

Our aim is to measure how exogenous variations in the size of local municipalities affect the performance of connected firms. We classify municipalities into "treatment" municipalities (those that increased in size) and "control" municipalities (those that did not). We focus on re-connected firms for two reasons. First, doing so circumvents potential endogeneity problems in the formation and disruption of connections. Second, it allows us to focus on how changes in political power affect rent seeking by connected firms. We estimate a difference-in-differences (DD) model, which allows us to absorb any general impact of election cycles (Bertrand et al., 2007) or changes in the business environment (e.g., macroeconomic shocks) for corporate outcomes. We discuss and extend this approach in the following five remarks.

First, one premise of our methodology is that the increase in municipality size resulted in an increase of political power. Panel C of Table 1 reports the evolution of three measures of political power during the reform period. The number of inhabitants per elected politician more than doubled in merging municipalities but remained unchanged in control municipalities. Expenditures per elected politician increased by a factor of three in merging municipalities, compared with only a marginal change in control municipalities. Finally, outsourcing per elected politician increased six times more in merging than in unchanged municipalities. Note that implementation of the reform required merging municipalities to accomplish some transitory tasks (e.g., integration of IT systems, relocation of administrative and political units and/or of public transportation networks). Expenses for these tasks amounted to almost DKK 1.2 billion and were mostly outsourced to private companies.[9] We therefore have solid evidence that, on average, the reform significantly increased political power in merging municipalities while leaving it practically unaltered in control municipalities.

Second, our identification assumes that the enlargement of local municipalities does not affect firm performance through channels other than political connections. Although we cannot rule out a priori that a merger benefits all firms located in a given municipality—for instance, by fostering economic activity or improving the business environment—our empirical investigation demonstrates that this is not the case; only connected firms benefit from the reform.

Third, our identification may deliver biased estimates if the reform affected the quality of politicians by increasing political competition in merging municipalities. If the quality of re-elected politicians is correlated with delivering rent to the connected firms, then our estimates on performance may be biased (either downward or upward). We solve this challenge by adopting a selection model based on two exclusion criteria for re-election: the aggregate party votes (not counting the politician's own municipality); and the share of council members aged over 65 prior to the election.

[9] Studies of electoral rules and fiscal federalism suggest that centralization can reduce electoral accountability (Fisman and Gatti, 2002). In the context of Denmark, Dreyer Lassen and Serritzlew (2011) document that larger municipalities had a sizable detrimental effect on citizens' political efficacy; this may, in turn, have reduced their ability to hold politicians accountable.

Fourth, our identification requires that the selection of merging municipalities be exogenous to current corporate performance. The two main criteria for selection were population size and geography. Local population patterns are primarily determined by the late 18th-century evolution of industry and localization of the railway network and ports, so these patterns are exogenous to current business conditions. Furthermore, current migration (which could be affected by the current business environment) has only a marginal impact—relative to the historical factors—on municipality population size.

Fifth, our DD identification requires that the pre-reform "parallel trend" hypothesis be valid—in other words, that there be no pre-reform differential trends between treatment and control municipalities that correlate with the performance of connected firms. Suppose that in the years prior to the reform, a variable likely affecting politicians' rent-extraction, e.g. electoral competition, increased more in nonmerging than in merging municipalities. If this differential trend continued during the reform window, then we would be unable to separate the effect of the reform from the effect of the pre-existing diverging trends.[10] Our main defense of the parallel trend hypothesis is to create a placebo test in which we move the window of analysis back three years (i.e., as if the reform transpired in 2002). This is a powerful test because, even though there are many possible diverging trends, what matters for our identification is whether or not they determine diverging trends in firm profitability. Indeed, this placebo test reveals no differences with respect to firm performance between treatment and control groups. Furthermore, we show that the key variables describing

[10] Notice that only diverging trends in pre-reform years constitute a challenge to our identification; in contrast, if the reform creates changes in political competition, then this could be a channel through which revised municipality borders affect connected firm performance.

electoral competition yield no clear evidence disconfirming the parallel trend hypothesis *prior to the reform*.

In Table 2, Panel A shows that 11,341 candidates ran in 2005, of which 8,375 (resp., 2,966) ran in treatment (resp., control) municipalities. Altogether, 2,502 candidates were elected in the 98 municipalities, with 1,852 of these in the merging/treatment municipalities and 650 in the unchanged/control municipalities. The ratio of candidates seeking re-election to candidates elected in 2001—as well as the ratio of re-elected candidates to all candidates seeking re-election—is lower for the merging municipalities, in which there is also a smaller share of candidates running for re-election. Therefore, Panel A indicates the possibility of a differential increase in electoral competition *during* the reform period. As already mentioned, this provides a channel through which the political reform could affect firm performance. In addition, it underscores the importance of applying our previously discussed selection model of re-election.

[[INSERT Table 2 about Here]]

Panel B of Table 2 tests directly if there was a diverging trend in political competition *prior to the reform*. Our proxy for political competition is the number of seats in a given municipality divided by the number of candidates running for election. To test the parallel trend hypothesis, we estimate the average change in political competition from 1997 to 2001 (the two elections prior to the reform). Panel B shows that there was no significant diverging trend in political competition before the reform.

Panel C reports the numbers of connected firms. Overall, 1,964 firms were connected with candidates in the 2005 elections. The portion of firms connected with elected candidates relative to all connections is approximately 38%, with no significant differences across treatment and control municipalities. There are 419 re-connected firms with usable observations: 321 with treatment and 98 with control municipalities. This is our primary firm sample. The ratio of connections with re-elected candidates to all connections is about 21%, and there are no significant differences in this figure across groups.

[[INSERT Table 3 about Here]]

Table 3 provides summary statistics for the personal characteristics of all candidates (Panel A) and winning candidates (Panel B) for the years 2001 and 2005. Education and labor income are often used as proxies for candidate qualities (Ferraz and Finan, 2011; Brollo et al., 2012). The average candidate in 2005 is approximately 50 years old, received 13 years of schooling, and has a labor income of DKK 403,284; see column (4) in Panel A. As seen in column (4) of Panel B, winning candidates have a higher labor income. Comparing the candidates in 2001 with those in 2005 (both the entire pool and the subsample of winners), in merging municipalities the latter were older, slightly more likely to be male, significantly more educated, and with a higher labor income; similar but less pronounced differences were evident in control municipalities. In Panel A, column (9) indicates that the average candidate in a merging municipality was significantly more educated than the control counterpart. This effect is not significant for winning candidates (same column in Panel B), where instead we observe a significant and positive difference in labor income changes. In sum, Table 3 highlights the importance of choosing winning politicians as counterfactuals so that observable differences between connected politicians in treatment versus control groups can be minimized. The small differences suggest that unobservable differences may also exist; thus Table 3 confirms the importance of controlling for selection.

Table 4 reports average firm characteristics prior to 2005. Our main measure of corporate performance is operating return on assets (OROA), computed as the ratio of earnings before interest and taxes (EBIT) to the book value of total assets. An important advantage of using OROA as a measure of performance is that, unlike measures based on net income, OROA is unaffected by differences in the capital structure of firms. To mitigate the effect of outliers, we drop 1% of observations in the right and left tails of the OROA distribution. To check for whether differences in OROA are explained by differential industry trends, we also report industry-adjusted OROA. The industry adjustments are calculated using the median OROA of each 4-digit industry while considering all active firms in our data set (i.e., including those that are not politically connected). For each industry, we require the existence of at least 20 firms in a given year; when this requirement is not satisfied at 4-digit level, we move to the 3-digit (or 2-digit) level.

[[INSERT Table 4 about Here]]

Columns (1), (2), and (5) of the table reveal that, on average, connected firms are larger and worse performing than are nonconnected firms; these results support the cross-country findings in Faccio (2010) and confirm that nonconnected firms would serve poorly as counterfactuals. Columns (3), (4), and (6) show that the economic and statistical differences between connected firms with treatment and control municipalities are much smaller; however, the significant difference observed in OROA may still raise concerns about omitted factor bias. This motivates our focus on re-connected firms. According to columns (7)–(10), there are no significant differences between re-connected firms with treatment versus control groups with respect to total assets, performance, sales, or employees. We cannot entirely rule out the possibility of unobserved differences between the two groups, but the lack of significant observable differences suggests that this problem is unlikely to bias our results.

4. Results

4.1. Operating profitability

Table 5 presents our main difference-in-differences results. The dependent variable is the change in firm profitability around the year 2005 (i.e., the local election year during which the administrative reform was implemented). We consider the three years after and the three before, but we exclude the election year itself. The variable of interest, *treatment*, is a dummy set equal to one if the firm is connected with a politician re-elected in a merging municipality, or to zero if the firm is connected with a politician re-elected in an unchanged (control) municipality. The model is estimated via ordinary least-squares (OLS) regressions.

[[INSERT Table 5 about Here]]

In column (1) of Table 5 we report estimates using unadjusted OROA as the dependent variable and controlling only for regional localization (to reduce the scope for omitted factor bias). Since the treatment is defined at the municipality level, we allow for correlation of residuals within municipalities by clustering standard errors at that level. The treatment effect is 0.0325 and is statistically significant at the 5% level. This result indicates that firms re-connected with merging municipalities experienced, on average, a 3.25 percentage points (pct) improvement in OROA over firms re-connected with municipalities of unchanged size. This impact becomes marginally higher when we control for lagged assets and operating performance; see columns (2) and (3). In columns (4)–(6), the dependent variable is the change in industry-adjusted OROA. The results are similar in size and significance to the unadjusted results, which suggest that our findings are not driven by industry trends.

In summary we find that, following the administrative reform, the firms that re-connected with merging municipalities increased their profitability by 3.1–3.4 pct in comparison with firms re-connected with control municipalities. Given that the average connected firms' OROA is 3.3%, the economic significance of such an increase is large; the operating performance of re-connected firms nearly doubles after the reform. To illustrate the magnitude of the impact of increased political power on connected firms' performance, we make a back-of-the-envelope calculation of the elasticity of corporate performance with respect to political power. Table 1 shows that the average increase in population per politician was 132 pct in merging municipalities and 1 pct in unchanged municipalities. Thus, the average difference was 131 pct. Similarly, we find that the relative differences were 179 pct (resp., 172 pct) for the increase in expenditure (resp., outsourcing) per politician. As shown in column (3) of Table 5, the treatment effect is 0.034; this implies an average performance increase of 136 pct for the re-connected firms (having an average OROA of 0.025). Based on these values, the elasticities of connected firm performance to political power are 1 for changes in population per politician, 0.76 for changes in expenditure per politician, and 0.79 for changes in outsourcing per politician. Hence our analysis suggests that, if a politician's constituency doubles, then a re-connected firm will double its performance after the reform. Similarly, if outsourcing per politician doubles, then a re-connected firm increases performance by 79 pct.

4.2. Sales and public demand

Previous studies have examined several channels through which firms benefit from political connections. For example, Faccio et al. (2006) find that connected firms are more likely to be bailed out. Boubakri et al. (2012b) argue that connected firms enjoy a lower cost of equity capital. Other studies show that political connections shape the firm's capital structure (Claessens et al., 2008; Li et al., 2008), mainly through easier access to bank lending (Khwaja and Mian, 2005). In this section, we forge a link between our profitability results and the dependence of a firm's sector on public demand.

As discussed previously, private firms in Denmark are not obliged to report sales data, but half of our sample firms did so. Column (7) of Table 5 indicates that there is a positive treatment effect on net sales. The economic magnitude of the effect is large, although the statistical significance is only 10% (owing, perhaps, to a smaller sample size). This evidence suggests that part of the increase in operating profits stems from higher firm revenues.

[[INSERT Table 6 about Here]]

In Table 6, we investigate how the public sector influences the value of political connections by exploiting the heterogeneity in the sectoral dependence on public demand. Following Cingano and Pinotti (2011), we analyze the cross entries between public consumption and industries in the 2-digit Danish input–output matrix to classify industries as strongly or weakly dependent on public demand.[11] We then interact our treatment with a dummy set equal to one if the firm operates in an industry that depends strongly on the

[11] Sectors that are highly dependent on public demand include education, hospitals, recreational activities, and civil engineering.

public sector (and set equal to zero otherwise). The results reported in columns (1) and (2) indicate that the positive effect of mergers on operating returns is clearly visible in industries that are closely linked to the public sector. Overall, these findings support the interpretation that a connected firm benefits from doing business with the local public sector; hence our findings are consistent with politically connected firms receiving favorable treatment in the allocation of procurement contracts (Goldman et al., 2012).

5. Further causal validation

In this section, we extend the preceding analysis to accommodate a number of identification challenges, including the five discussed in Section 3. The estimation methods and battery of additional tests described here support a causal interpretation of the findings in Section 4.

5.1. Falsification and robustness

One concern with interpreting Table 5 is the possibility that *all* firms benefit from a larger municipality—that is, irrespective of their connection with local politicians. This may happen, for example, if a merger increases the public demand for private services and other goods or in response to the alignment of local and national government policies (as found by Kim et al., 2012).

The results reported in Table 7 help us rule out these interpretations. As columns (1) and (2) show, nonconnected firms experienced no significant profitability changes in conjunction with reform passage, which suggests that the estimates in Table 5 are indeed connection specific.[12] Columns (3) and (4) further confirm this evidence by showing that the firms connected with unelected candidates also experienced no significant change in profitability.

[[INSERT Table 7 about Here]]

A second challenge to the causal interpretation of Table 5 concerns the implicit assumption of parallel trends, which is needed for the DD model to be valid. To highlight the similarity of the two groups *before* implementation of the reform, we propose a falsification test that estimates DD models in a pre-treatment window centered at $t = -3$. The lack of statistical significance in this case, as reported in columns (5) and (6) of Table 6, confirms that the two groups were similar prior to the 2005 elections and thus that the parallel trends assumption is justified.

We perform a number of additional checks to assess the robustness of our estimates in Table 5. In computing the dependent variable, we trimmed OROA by 1% in both the right and left tails of the distribution to mitigate concerns about outliers. To confirm that outliers do not drive our results, we then trim the dependent variable by an additional 1% in the right and left tails of the distribution. We also run a median regression (computing standard errors by bootstrap, using 500 replications). In addition to clustering at the municipality level, we consider an alternative way of computing standard errors based on a block-bootstrap procedure (Bertrand et al., 2004) involving 500 replications. We also exclude firms in financial, insurance, and utilities industries (for which operating returns are typically an unreliable measure of performance), as well as firms connected with

[12] The lack of a negative and significant profitability change suggests that, in our case, government spending did not offset private activities (Cohen et al., 2011).

municipalities that were split into separate larger entities—given that, for such firms, the effect of a merger is ambiguous. Finally, we adopt alternative measures of firm performance, such as the ratio of net income to total assets. All results from these tests (untabulated) are statistically and economically in line with our previous estimates.

5.2. Controlling for the selection of re-elected candidates

We anticipated that the reform-induced increase in political competition might affect the quality of re-elected politicians in merging municipalities in a way that correlates with the ability to transfer rent to the connected firms. In this case, the estimates presented in Section 4 would be capturing not only the benefits of an increase in political power but also the superior quality of re-elected politicians. Although Table 3 indicates no major changes in the *observable* characteristics of politicians re-elected after the reform, it is still important to control for unobservable differences that might affect our findings.

Table 8 reports the results of using Heckman models to control for selection into the pool of connections with politicians re-elected in 2005. We adopt two alternative exclusion restrictions, which are correlated with a connected politician's likelihood of being re-elected in 2005 yet are unlikely to affect corporate performance in any way other than through the rent transferred to the firms by connected politicians. The first restriction is the average number of votes the politician's party has received in other municipalities (i.e., not counting the politician's own municipality). This approach is similar to that of Dal Bó et al. (2009), who use the re-election probabilities of a legislator's current cohort (by state and party) as an instrument for the probability of re-election. In our setting, the idea is that the aggregate votes received by a given party is a common shock that affects all candidates' probability of re-election—but it has no effect on firm profitability except via channels employed by the connected politicians who are re-elected. The alternative restriction is the number of elected politicians in 2001 in the same municipality who are older than 65 years prior to the 2005 election. A higher incidence of old politicians implies that fewer will stand for re-election, so this condition increases the likelihood that a politician who runs for re-election succeeds. We can reasonably assume that the age distribution of the municipality council in 2001 is independent of a given connected firm's characteristics.

[[INSERT Table 8 about Here]]

Table 8, Panel A, provides first step probit estimates; here the dependent variable is a dummy set equal to one only if a firm was connected with a re-elected politician, and the explanatory variables are the two exclusion restrictions—reported separately in columns (1)–(4), with and without their interaction with the dummy—indicating whether the municipality was or was not altered by the reform. Consistent with the idea of tougher competition in municipalities that were merged by the reform, we observe that the treatment indicator has a negative sign. We also observe that using both exclusion restrictions increases the likelihood that a connected politician will be re-elected. However, the two selection models differ in this respect: the aggregate party vote mainly affects merging municipalities whereas the age distribution has comparable effects across merging and control municipalities. Panel B of the table gives the performance results obtained using the Heckman selection model. Much as in our baseline results reported in Table 5, the profitability effects range from 3.2 to 3.4 pct and are significant at the 5% level.[13]

[13] Estimates are only marginally smaller if we use two-step rather than maximum likelihood procedure.

On the basis of these findings, we conclude that controlling for selection concerns does not alter the effect of an increase in political power on the performance of connected firms.[14]

5.3. Matching and discontinuity estimates

We now investigate whether our findings are robust to the use of alternative estimation methods. Toward this end, we report results based on reweighting and nearest-neighbor matching (Rosenbaum and Rubin, 1983; Abadie and Imbens, 2007). With these approaches we find, for each firm connected with a merging municipality, the most similar firm in the control group and thus discard dissimilar observations. The resulting benefit is that, by minimizing the distance between merging and unchanged municipalities, we reduce the bias induced by differences in observable firm and political characteristics that might be unbalanced across the two groups.

The covariates included in the matching procedure are pre-treatment assets and industry-adjusted operating performance; regional localization; gender and (logarithm of) age of the connected politician; and logarithm of his or her position in the electoral list. We compute the matching estimators as follows: (1) we run a probit regression in which the

[14] Another concern is whether business conditions affected pre-reform migration across municipalities, thereby influencing the selection of which municipalities to merge. Table 4 casts doubt on this possibility. In the event of such reverse causality, we should observe that firms connected with merging municipalities perform worse than firms connected with control municipalities; however, Table 4 shows that the two firm types exhibited similar performance before the reform. To mitigate this concern still further, we instrument the merging dummy with the logarithm of municipality population in 1976. Population size in 1976 is negatively correlated with the probability of merger in 2005: historically, then, larger municipalities were less likely to be affected by the administrative reform. At the same time, the exclusion restriction may be satisfied given the small likelihood that municipality population in 1976 had a direct effect on changes in firm performance around 2005. Two-stage least-squares estimates (untabulated) broadly confirm our main finding; the profitability effect is 0.05 pct (6% significant). This evidence helps us rule out the effect of endogenous merging decisions on our results.

dependent variable is the binary treatment and the explanatory variables are the aforementioned covariates; (2) we use the predicted values to construct the propensity score, discarding the few observations outside the common support; (3) we match with replacement firms connected with merging and unchanged municipalities. We then estimate the difference in change of profitability around the election.

We start by showing estimates obtained after re-weighting observations on the basis of the propensity score. The results are presented in column (1) of Table 9. These estimates are significant at the 5% level and marginally lower than the OLS estimates. In column (2), we match observations with replacement on the covariates directly; in column (3), we match with replacement on the propensity score and then rematch on the covariates, reporting the bias-adjusted results. Column (4) yields results from a one-to-one match without replacement. All the estimates are significant both statistically and economically, and they range between 2.9 and 3.5 pct.

[[INSERT Table 9 about Here]]

One other concern with our identification is that the merging group may be formed by municipalities with declining economic or demographic trends; in this case, the firms connected with those municipalities would not be fully comparable to firms connected with large municipalities unaffected by the reform. We have already shown that such potential differences are not reflected in a different pre-reform profitability between re-connected firms in treatment and control group. Nonetheless, we further address this concern in two ways. First, we exclude the smallest merging municipalities and the largest municipalities in the control group. The results—reported in columns (5) and (6)—are qualitatively in line with our baseline estimates. Second, we exploit the sharp discontinuity at 20,000 inhabitants (which the Danish government adopted when selecting which municipalities to merge) by comparing firms connected with municipalities above and below this threshold. Because this variable is precisely measured and cannot be manipulated, it offers an ideal context for a regression discontinuity design. We create the running variable as the distance from the threshold in terms of number of inhabitants in 2004; then we parametrically estimate a linear specification, adding the running variable to the usual set of controls; see column (7). In column (8), we add the interaction between treatment and the running variable. The treatment coefficient is positive at the 5% level and marginally higher than the OLS estimates.

In conclusion, all our alternative estimation methods—which are proposed to address specific challenges to our identification strategy—confirm that increased political power has a significant and positive impact on the performance of connected firms.

6. Heterogeneous effects

Now that we have established the causal link between political power and connected firm performance, we show how the average effect documented in Table 5 varies depending on the characteristics of politicians, firms, and municipalities.[15] In column (1) of Table 10, we look at nuclear connections—that is, cases in which the CEO or board member (or his/her spouse or sons/daughters) is a member of the municipality. We observe that the treatment coefficient for nuclear connections is slightly larger than the average impact obtained on the full sample in Table 5, column (6). Column (2) focuses on powerful politicians, defined

[15] The results of this section should not be interpreted in statistical terms but rather as differences in economic magnitude across subsample coefficients.

as those who won more than the median share of personal votes in a given party and municipality. Again, the coefficient is marginally higher than the average impact and is significant at the 5% level. In column (3), we look at firms connected to politicians who belong to the mayor’s party or coalition. Here the coefficient is noticeably higher than the average impact, although the standard error is larger (likely the result of a smaller sample). What these sample splits suggest is that the benefits to the firm that result from political ties are increasing in the power of the connected politician.

[[INSERT Table 10 about Here]]

We then analyze small and large firms separately. We believe that firm size can play an important role in determining the magnitude of our result. For instance, smaller firms may rely more intensively on public procurement contracts from the municipality where they operate, while large firms have a greater inclination to extend their business outside the local municipality. Also, the family network behind smaller firms may be geographically more concentrated, and the interests of family members more aligned, thus making it easier to transfer rent from political office to the company. Overall, we posit that smaller firms benefit more from the reform. In columns (4) and (5), we find that the treatment coefficient is positive in both subsamples, but consistent with our arguments the effect is greater for smaller firms.

Next, we analyze how profitability levels prior to the reform year shape our finding. A priori it is not clear how our finding should vary with pre-reform profitability levels. If, following a resource-based view, firms specialize in either being efficient or relying on connections, then it is indeed possible that firms can have similar profitability levels before the reform. However, we have empirically shown that connected firms have a lower profitability than nonconnected firms (Table 4). Hence, it is possible that firms that rely more on connections have lower profitability and will benefit more from the reform. In columns (6) and (7), we find that indeed the profitability increase is present only among firms that exhibited low performance prior to the reform year. This result seems to be consistent with the idea that the least productive firms benefited the most from an increase in political power.[16]

In columns (8)–(11), we investigate the extent to which the strength of political institutions affects rent extraction by connected firms.[17] Danish political institutions exhibit high accountability on average, yet there is some variation across municipalities. Our aim is to investigate whether a connected firm benefits more from the reform if connected with a merging municipality with weaker political institutions. The first step is to apply our measure (from Table 2) of political competition, defined as the ratio of council seats to the number of candidates in a given municipality. Recall that, according to Table 2, there is no pre-reform diverging trend in political competition. We now compute changes within merging municipalities from the 2001 to the 2005 elections. Municipalities above the median level of change saw a relatively small increase in political competition. We expect that it is easier to extract rent from the increase in political power in such municipalities. The results reported in columns (8) and (9) confirm this hypothesis: in municipalities with

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[16] We confirm this result for firms with low or high pre-reform productivity. We compute total factor productivity (TFP) as residuals obtained from OLS estimates of a Cobb–Douglas production function with logarithm of sales as the dependent variable and logarithm of labor and capital (in addition to year and industry dummies) as explanatory variables. We then average TFP over the pre-reform years and create subsamples of firms with low or high TFP with reference to the median threshold. The results (untabulated) confirm our finding that the firms gaining the most from an increase in political power are those that were less productive at the pre-reform stage.

[17] We thank the referee for suggesting this idea.

little change in electoral competition, connected firms benefit more from a merger than do firms in municipalities where electoral competition increases.

Second, we exploit historical variations in institutional quality. Starting from the medieval age, a number of Danish towns obtained the title of "privileged city" (Købstad) from the royal crown.[18] Privileges included the right of self-administration, the right to have local courts, tax breaks, an exemption from national military service, and sometimes freedom of religion. We posit that these privileges—especially the right of self-administration—fostered the creation of social capital in the community, which in turn improved institutional quality in a persistent manner.[19] We therefore anticipate a greater profitability effect stemming from the reform for municipalities *without* a Købstad.[20] Indeed, columns (10) and (11) show that the treatment effect is large and significant (resp., small and insignificant) in municipalities without (resp., with) a Købstad. Thus, we conclude that the social and political capital created through the Købsteder rights centuries ago is persistent enough to reduce the ability of politically connected firms to extract rent.

7. Alternative corporate outcomes

Finally, in this section we ask whether being connected with merging municipalities affects corporate characteristics other than profitability. Table 11, column (1) shows that there is no significant effect on firm size as measured by changes in total assets. Hence our results

[18] It must be emphasized that, because most of these towns received their rights before the 18th century, several of today's large cities did not have a Købstad and several cities that had a Købstad are small; the title has become purely symbolic and no longer has any real administrative content.

[19] A similar argument is presented in Guiso et al. (2008), who document that the creation of free city-states during the Middle Ages in Italy fostered the creation of social capital and argue that it explains much of the current variation in social capital across Italian regions.

[20] Købstad municipalities are classified as of 1921, but our results are similar if we classify them as of 1801.

are not driven by a differential increase in the total assets of treatment and control firms. Column (2) shows that the firms in merging municipalities experience an increase in cash holdings; this finding is consistent with the interpretation that such firms retain earnings and accumulate cash for investing when new business opportunities present themselves. In column (3) we find a positive and marginally significant effect on the volatility of profits (as measured by the change in standard deviation of OROA around the election year). This finding suggests that the increase in profits around the reform was driven, in part, by the government's funding of the transitional expenses entailed by reform. Column (4) tests for the impact on leverage, where the dependent variable is the ratio of total debt to assets. It is clear that the reform had no significant effect on debt; neither did it affect the maturity structure of debt as measured by the ratio of long-term to total debt. These findings indicate that the increase in political power did not influence locally connected firms through access to debt financing. Furthermore, we were unable to discern any significant effect of changes in political power on the wage and employment policies of re-connected firms (untabulated results).

[[INSERT Table 11 about Here]]

8. Discussion

Using a novel identification based on exogenous changes in the size of local Danish municipalities, we establish the causal effect of variations in political power on the performance of firms that are connected by family with local politicians. Our mean estimates are consistent with an elasticity of firm performance to political power (where the latter is measured by the ratio of population per elected politician) of about one. Thus, our estimates suggest that political networking at the local level can be a powerful business strategy even in a country with low levels of corruption. This finding is important because (1) local budgets constitute a large fraction of any country's total public expenditures, and (2) there is little extant evidence concerning whether, in a transparent institutional environment, local political connections can affect firm performance.

Political connections are valuable all over the world, but the channels through which political rent is transferred to connected firms vary from one country to another. Previous studies have documented that political connections affect firms' capital structure through lower cost of capital, protection in times of financial distress, and easier access to bank credit. Our results indicate that doing business with the public sector is the main channel for transferring rent to connected firms, which supports the recent finding of Goldman et al. (2012). So in countries with strong institutions, the transfer of rent through political connections appears to be driven by demand; connected firms are in a better position to gain from the outsourcing activities of the public sector.

We believe that our identification strategy offers several advantages in establishing causal links from political connection to firm performance. First, we are able to mitigate selection concerns through our choice of counterfactuals and also by adopting suitable variables that allow us to control for the selection of re-elected politicians. Second, we use matching and regression discontinuity techniques to reinforce the causal interpretation of our findings. Third, our framework allows for estimating the elasticity of rent extraction with respect to political power; it could also be adopted to analyze more broadly, for instance, the rewards to political connections. Fourth, our approach can be replicated in any country where the boundaries of local municipalities have been redefined (e.g., Sweden and Japan), and it can be used to assess the effects of even supernational reforms—for example, the increasing decisional power of the European Parliament.

Analyzing the full welfare effects of political connections is beyond the scope of this paper. However, our analysis does suggest that political connections reduce welfare overall. Politically connected firms tend to be less productive before the reform, and the value of political connections is higher among less productive firms. Hence, political connections may serve to transfer rent from the most productive to the least productive firms. The ensuing welfare reduction is mitigated to the extent that connected firms use the rent to increase their operating efficiency.

Finally, our analysis contributes to the discussion of how to measure corruption. There is general agreement that a distinctive feature of corruption is the misuse of public office for private gain (Treisman, 2000), but a clear definition has proved to be elusive. Corruption involves at least three elements: it is illegal, an attempt to circumvent existing rules, and generally associated with favors extended to particular firms (Bennedsen et al., 2012). We certainly do not claim that any of the connected firms or politicians in our study has engaged in illegal behavior. However, our analysis provides evidence consistent with the latter two elements just mentioned; in particular, political connections induce measurable firm-specific benefits that may reduce economic welfare. Our findings thus indicate that an appreciable level of legal "corruption" is present even in the world's least corrupt country.

Figure 1. Danish municipalities before and after the administrative reform

This chart illustrates the map of Danish municipalities before and after the administrative reform of 2005. Source: Wikipedia.

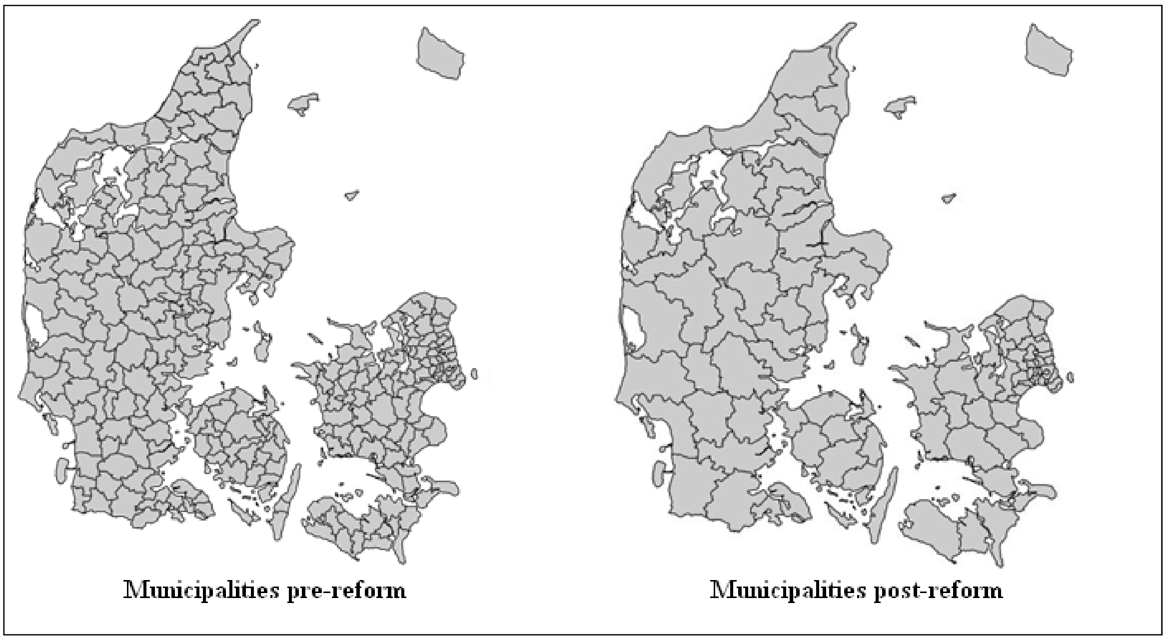

Table 1. Danish municipalities before and after the administrative reform

Panel A illustrates the impact of the Danish administrative reform on the number of municipalities by treatment and control groups. Panel B reports changes in the number of municipalities by population size. Panel C compares the average municipality's outsourcing, expenditures, and population divided by the number of elected politicians, by treatment and control municipalities. Outsourcing represents the sum of expenses from using contractors and other services. Outsourcing and expenditure ratios are computed using 2004–2007 budget items and 2001–2005 election data. Population ratio is computed using 2005–2007 administrative data and 2001–2005 election data. Source: Denmark Statistics. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A. Number of municipalities

	Before	After
Total	271	98
Treatment	238	65
Control	33	33

Panel B. Number of municipalities by population size

	Before	After
> 100,000	4	6
50,000–100,000	13	28
30,000–50,000	24	39
20,000–30,000	25	18
10,000–20,000	77	3
5,000–10,000	114	1
< 5,000	14	3

Panel C. Measures of political power

		Before	After	Difference After − Before
Population/politicians	Treatment	776.9	1,798.7	1,021.8***
	Control	2,323	2,344	21
Expenditures/politicians	Treatment	30,066.6	88,474.2	58,407.6***
	Control	106,093.9	122,154.4	16,060.5
Outsourcing/politicians	Treatment	2,879.6	8,078.3	5,198.7***
	Control	9,515.2	1,0352.3	837.1***

Table 2. Electoral results and political connections

Panel A illustrates the electoral results of the administrative elections held in 2005 by control and treatment municipalities; we report several ratios in brackets. Panel B illustrates the difference between treatment and control groups with respect to the electoral competition prior to 2005 (i.e., using data from the 1997 and 2001 administrative elections); standard errors are given in parentheses. Electoral competition is calculated as the number of council seats divided by the number of candidates. Panel C shows the number of politically connected firms in the 2005 elections; the ratio of firms connected with elected candidates to the total number of firms connected to any candidate is reported in brackets. Panel C also shows the number of firms connected with politicians that were re-elected in 2005 (by control and treatment municipalities). The ratio of firms connected with re-elected candidates to the total number of firms connected to any candidate is reported in brackets.

Panel A. Results of the 2005 administrative elections			
	Total	Treatment	Control
All candidates	11,341	8,375	2,966
Elected candidates	2,502	1,852	650
Ratio of elected to all candidates	[22.1%]	[22.1%]	[21.9%]
Re-elected candidates	1,679	1,287	392
Ratio of re-elected to all candidates	[14.8%]	[15.4%]	[13.2%]
Ratio of re-elected to all candidates running for re-election	[61.3%]	[57.5%]	[78.4%]
Ratio of candidates running for re-election to candidates elected in 2001	[62.3%]	[60.3%]	[73.6%]

Panel B. Electoral competition before the 2005 administrative elections			
	Treatment	Control	Difference
Change from 1997 to 2001	0.008	0.005	0.003
	(0.004)	(0.007)	(0.011)

Panel C. Connections between firms and politicians in the 2005 administrative elections			
	Total	Treatment	Control
Firms connected with all candidates	1,964	1,453	511
Firms connected with elected candidates	752	566	186
Ratio of connections with elected to all connections	[38.3%]	[38.9%]	[36.4%]
Firms connected with re-elected candidates	419	321	98
Ratio of connections with re-elected to all connections	[21.3%]	[22.1%]	[19.2%]

Table 3. Politicians' characteristics

Panels A and B illustrate average differences in observable characteristics for, respectively, all candidates and winning candidates in the 2001 and 2005 elections, by control and treatment municipalities. Standard errors are given in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

	2001			2005			Difference		
	All	Treatment	Control	All	Treatment	Control	(5) − (2)	(6) − (3)	(7) – (8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. All candidates									
Age	49.00	49.08	48.54	49.51	49.75	48.82	0.67***	0.28	0.39
	(0.089)	(0.097)	(0.233)	(0.112)	(0.065)	(0.241)	(0.159)	(0.335)	(0.371)
Women (%)	28.71	28.13	31.49	29.53	28.09	33.61	−0.04	2.12*	−2.16
	(0.356)	(0.396)	(0.837)	(0.428)	(0.491)	(0.867)	(0.630)	(0.012)	(0.013)
Education (months)	155.04	154.06	159.46	160.13	159.44	162.09	5.38***	2.63***	2.75***
	(0.268)	(0.300)	(0.611)	(0.305)	(0.352)	(0.613)	(0.462)	(0.866)	(0.982)
ln(Labor income)	12.20	12.24	12.04	12.29	12.35	12.08	0.11***	0.04	0.07
	(0.011)	(0.012)	(0.030)	(0.014)	(0.015)	(0.031)	(0.020)	(0.043)	(0.048)
Panel B. Winning candidates									
Age	50.17	50.30	49.50	50.40	50.82	49.22	0.52*	−0.28	0.80
	(0.144)	(0.153)	(0.441)	(0.203)	(0.223)	(0.457)	(0.270)	(0.635)	(0.690)
Women (%)	27.05	25.87	34.13	27.21	24.84	34.00	−1.03	−0.13	−0.90
	(0.670)	(0.718)	(1.904)	(0.889)	(1.004)	(1.859)	(1.234)	(0.266)	(2.932)
Education (months)	157.03	156.17	162.09	162.19	161.02	165.51	4.85***	2.92	1.93
	(0.499)	(0.544)	(1.287)	(0.628)	(0.723)	(1.255)	(0.905)	(1.798)	(2.012)
ln(Labor income)	12.69	12.69	12.66	12.83	12.88	12.70	0.19***	0.04	0.14**
	(0.013)	(0.042)	(0.008)	(0.017)	(0.019)	(0.041)	(0.023)	(0.059)	(0.063)

Table 4. Summary statistics

This table reports summary statistics for the three years prior to the 2005 administrative elections. Column (1) refers to nonconnected firms, while columns (2)–(6) refer to firms connected with any candidate. Columns (7)–(10) refer to the sample that was used in the identification strategy, which is formed by firms connected with politicians re-elected in 2005. ln(Assets)is the natural logarithm of the book value of total assets, and OROA is the ratio between operating income and book value of assets; industry-adjusted OROA is computed as firm OROA minus the median OROA of the relevant industry. ln(Sales) and ln(Employees) are the natural logarithms of (respectively) net sales and employees. Robust standard errors are reported in parentheses. The number of firms is reported in square brackets. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

		All candidates in 2005					Politicians re-elected in 2005			
	Non-connected firms	Connected firms	Firms connected with treatment municipalities	Firms connected with control municipalities	Difference (2) − (1)	Difference (3) − (4)	Connected firms	Firms connected with treatment municipalities	Firms connected with control municipalities	Difference (8) − (9)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln(Assets)	8.222	9.123	9.125	9.117	0.901***	0.008	9.663	9.709	9.514	0.195
	(0.005)	(0.030)	(0.034)	(0.064)	(0.030)	(0.072)	(0.068)	(0.073)	(0.169)	(0.184)
	[50,356]	[1,967]	[1,456]	[511]			[421]	[323]	[98]	
OROA	0.042	0.033	0.036	0.026	−0.009***	0.010**	0.025	0.025	0.022	0.003
	(0.000)	(0.002)	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.003)	(0.008)	(0.009)
	[49,834]	[1,732]	[1,430]	[501]			[417]	[321]	[96]	
Ind.-adj. OROA	0.007	−0.000	0.001	−0.005	−0.007***	0.006	−0.006	−0.006	−0.007	0.001
	(0.000)	(0.002)	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.003)	(0.008)	(0.008)
	[49,834]	[1,732]	[1,430]	[501]			[417]	[321]	[96]	
ln(Sales)	7.749	8.729	8.783	8.584	0.980***	0.199*	9.074	9.095	8.991	0.104
	(0.012)	(0.051)	(0.058)	(0.104)	(0.052)	(0.119)	(0.103)	(0.111)	(0.265)	(0.287)
	[15,507]	[909]	[666]	[244]			[227]	[181]	[46]	
ln(Employees)	1.804	2.346	2.372	2.269	0.542***	0.103	2.636	2.638	2.630	0.008
	(0.005)	(0.028)	(0.031)	(0.061)	(0.028)	(0.068)	(0.068)	(0.072)	(0.166)	(0.181)
	[25,019]	[1,084]	[814]	[270]			[233]	[178]	[55]	

Table 5. Difference-in-differences estimates

This table reports the results of OLS regressions using the sample of firms connected with politicians re-elected in 2005 in the treatment and control groups. The dependent variable is: the change in OROA around the 2005 elections (three years after minus three years before, excluding the election year) in columns (1)–(3); the change in industry-adjusted OROA in columns (4)–(7); and the change in the logarithm of firm sales in column (8). The industry adjustment is computed as the firm's OROA minus the median OROA of the relevant industry. In all regressions, the explanatory variables are a treatment dummy set equal to one for firms connected with politicians in municipalities affected by the reform (or to zero for firms connected with politicians in control municipalities) and a set of regional dummies. Additionally, depending on the specification, we control for the lagged logarithm of total assets and lagged industry-adjusted OROA. Column (7) also includes a set of 2-digit industry dummies. Standard errors (clustered by the new municipality classification) are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable:	OROA			Industry-adjusted OROA			ln(Sales)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.0325**	0.0331**	0.0355**	0.0309**	0.0315**	0.0338**	0.3929*
	(0.0151)	(0.0152)	(0.0145)	(0.0152)	(0.0153)	(0.0147)	(0.2095)
$\ln(\text{Assets})_{t-1}$		-0.0022	-0.0011		-0.0020	-0.0010	-0.0432
		(0.0025)	(0.0021)		(0.0025)	(0.0021)	(0.0288)
$\text{Profitability}_{t-1}$			-0.2274**			-0.2189**	-0.8328
			(0.1029)			(0.1029)	(0.6326)
Number of firms	419	419	419	419	419	419	210

Table 6. Dependence on public demand

This table reports the results of OLS regressions using the sample of firms connected with politicians re-elected in 2005 in the treatment and control groups. In addition to the explanatory variables used in column (3) of Table 5 (coefficients not reported), we include a dummy set equal to one only if the firm operates in industries above or below the median ratio of output sold to the public sector relative to total output—as well as the interaction of this dummy variable with the main treatment dummy. The sectoral dependence is computed at the 2-digit industry level using the input–output matrix issued by Denmark Statistics in 2006. Standard errors (clustered by the new municipality classification) are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable:	OROA	Industry-adj. OROA
	(1)	(2)
Treatment	-0.0026	-0.0050
	(0.0155)	(0.0158)
Treatment×High sectoral dependence	0.0591**	0.0610**
	(0.0285)	(0.0286)
High sectoral dependence	-0.0071	-0.0107
	(0.0234)	(0.0235)
Number of firms	419	419

Table 7. Falsification tests

Columns (1) and (2) report the results of OLS regressions using the sample of nonconnected firms; columns (3) and (4) report the results of OLS regressions using the sample of firms connected with unelected candidates in 2005. The dependent variable in columns (1)–(4) is the change in industry-adjusted OROA around the 2005 reform. Columns (5) and (6) show the results of OLS regressions using the sample of firms connected with politicians re-elected in 2005; the dependent variable is change in industry-adjusted OROA in a pre-treatment period, computed as the difference between the average three-year profitability after $t = -3$ minus the three-year average before. In all regressions, the explanatory variables are a treatment dummy set equal to one for firms connected with politicians in municipalities affected by the reform (or to zero for firms connected with politicians in control municipalities) and a set of regional dummies. Additionally, in columns (2), (4), and (6) we control for the lagged logarithm of total assets and lagged industry-adjusted OROA. Standard errors (clustered by the new municipality classification) are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Industry-adjusted OROA						
	Nonconnected firms		Firms connected with unelected candidates		Connected firms: pre-treatment period	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.0013	-0.0013	-0.0032	-0.0015	0.0086	0.0044
	(0.0016)	(0.0015)	(0.0087)	(0.0079)	(0.0118)	(0.0128)
$\ln(\text{Assets})_{t-1}$		-0.0006*		-0.0012		0.0020
		(0.0003)		(0.0016)		(0.0024)
$\text{Profitability}_{t-1}$		-0.2953***		-0.1243***		-0.2248**
		(0.0071)		(0.0511)		(0.1012)
Number of firms	47,814	47,814	1,201	1,201	405	405

Table 8. Controlling for the selection of re-elected candidates

This table shows the impact of the reform after using a Heckman selection model to control for selection into the pool of connected politicians who were re-elected in 2005. In Panel A, each column reports the results from first step probit regressions in which the dependent variable is a dummy set equal to one if the firm is connected with a politician re-elected in 2005 (or to zero otherwise). In all regressions, the explanatory variables are a treatment dummy set equal to one for firms connected with politicians in municipalities affected by the reform (or to zero for firms connected with politicians in control municipalities), a set of regional dummies, lagged logarithm of total assets, and lagged industry-adjusted OROA. The specification also contains an exclusion restriction: in columns (1) and (2), it is the average fraction of votes obtained by the same party of the connected politician in municipalities other than that of the focal politician; in columns (3) and (4), it is the number of politicians older than 65 years in councils elected in 2001. Columns (1) and (3) incorporate the exclusion restriction only, whereas columns (2) and (4) add the interaction between the treatment dummy and the exclusion restriction. Standard errors (clustered by the new municipality classification) are reported in parentheses. Panel B reports the performance estimates obtained using the Heckman selection model. Each column in Panel B corresponds to the different first-stage regression of Panel A. Standard errors (clustered by the new municipality classification) are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A. Dependent variable: Firm connections with re-elected politicians in 2005

	(1)	(2)	(3)	(4)
Treatment	-0.2506*	-0.9772***	-0.1978	-0.0650
	(0. 1414)	(0.2867)	(0.1433)	(0.1938)
Aggregate party votes	2.0303**	-0.8773		
	(0.7427)	(1.0865)		
Treatment×Aggregate party votes		3.5726***		
		(1.2043)		
Nr. politicians older than 65			0.2094***	0.3435**
			(0.0715)	(0.1533)
Treatment×Nr. politicians older than 65				-0.1727
				(0.1723)

Panel B. Dependent variable: Industry-adjusted OROA

	(1)	(2)	(3)	(4)
Treatment	0.0320**	0.3216**	0.0342**	0.0335**
	(0.0142)	(0.0141)	(0.0149)	(0.0152)
Firm connections with re-elected politicians	419	419	419	419
Firm connections with politicians running for re-election	641	641	641	641

Table 9. Matching and discontinuity estimates

This table shows the impact of the reform using alternative estimation techniques. The dependent variable is the change in industry-adjusted OROA around the 2005 elections. In columns (1)–(4) we compute the average treatment effect using matching estimators. Firms connected with treatment and control municipalities are matched according to the following variables: three-year average pre-treatment logarithm of total assets and industry-adjusted OROA; regional dummies; logarithm of connected politician's age; politician's gender; and the logarithm of connected politician's position in the electoral list. The propensity score is estimated by running a probit regression where the dependent variable is the binary treatment and where the explanatory variables are the aforementioned controls. In column (1), we report results from weighed least squares using the estimated propensity score as weights. In column (2), the treatment effect is computed using one nearest neighbor with replacement matching directly on the covariates. In column (3), we report the treatment effect obtained by matching on the covariates and then re-matching on the propensity score with replacement. In column (4), we report the treatment effect obtained by using nearest-neighbor matching without replacement, so in this case we use only the treated firms that are closest each control firm. All the estimations reported in columns (1)–(4) are restricted within the common support. In columns (5) and (6), we perform OLS regressions excluding (respectively) the largest and smallest 10% of municipalities in terms of number of 2004 inhabitants. In columns (7) and (8), we adopt a control function approach by including as covariate the running variable computed as the distance (in terms of number of 2004 inhabitants) from the threshold of 20,000 inhabitants. In Column (8), we also include the interaction between the treatment dummy and the running variable. In Columns (1) and (5)–(8), we include the explanatory variables used in column (3) of Table 5 (coefficients not reported). Standard errors, which in columns (5)–(8) are clustered by the new municipality classification, are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Industry-adjusted OROA

	Weighted OLS	Nearest-neighbor		1-to-1 match	Excluding largest municipalities	Excluding smallest municipalities	RDD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.0294**	0.0341**	0.0324*	0.0353*	0.0387**	0.0321**	0.0451**	0.0454**
	(0.0131)	(0.0170)	(0.0168)	(0.0170)	(0.0172)	(0.0154)	(0.0180)	(0.0178)
Number of firms	387	387	387	196	390	377	419	419

Table 10. Politician, firm and municipality variations

This table reports the results of OLS regressions using the sample of firms connected with politicians re-elected in 2005 in the treatment and control groups. The dependent variable is the change in industry-adjusted OROA around the 2005 elections. In all regressions, explanatory variables are a treatment dummy set equal to one for firms connected with politicians in municipalities affected by the reform (or to zero for firms connected with politicians in control municipalities), lagged logarithm of total assets, lagged industry-adjusted OROA, and a set of regional dummies. In column (1), "nuclear" family connections comprise direct connections and connections with spouse or offspring. In column (2), connections with "powerful" politicians include firms connected with politicians who obtained more than the median fraction of votes in their race and in a given municipality. In column (3), connections with mayors' coalitions include firms that are connected with politicians belonging to the same party/coalition as the mayor in a given municipality. In columns (4) and (5) we consider subsamples of small and large firms, respectively, defined as firms having total assets below or above the median of assets in 2006; in columns (6) and (7), we consider subsamples of firms below or above (respectively) the median OROA in the pre-reform period. In columns (8) and (9), we consider subsamples of firms connected with municipalities below or above (respectively) the change in electoral competition between the 2001 and 2005 municipality elections. Electoral competition is computed as the ratio of council seats to the number of candidates. In columns (10) and (11), we consider subsamples of firms connected with municipalities where none or (respectively) at least one of the merging municipalities had a Købstad title as of 1921. Standard errors (clustered by the new municipality classification) are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Industry-adjusted OROA

	Nuclear connections	Powerful politicians	Mayors	Small firms	Large firms	Low-profit firms	High-profit firms	Low changes in electoral competition	High changes in electoral competition	Non-Købstad	Købstad
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treatment	0.0400**	0.0361**	0.0474*	0.0534**	0.0183	0.0643***	-0.0015	0.0427**	0.0198	0.0388**	0.0103
	(0.0159)	(0.0172)	(0.0280)	(0.0249)	(0.0140)	(0.0227)	(0.0175)	(0.0193)	(0.0183)	(0.0176)	(0.0176)
ln(Assets) $_{t-1}$	-0.0009	0.0005	-0.0035	-0.0007	-0.0063***	-0.0018	0.0014	-0.0026	0.0005	-0.0031	0.0037
	(0.0023)	(0.0026)	(0.0043)	(0.0085)	(0.0022)	(0.0023)	(0.0033)	(0.0033)	(0.0021)	(0.0028)	(0.0028)
Profitability$_{t-1}$	-0.2665**	-0.1150	-0.5005***	-0.2482**	-0.0893	-0.2484	-0.0996	-0.2179	-0.2179**	-0.1868	-0.2603**
	(0.1067)	(0.1211)	(0.1679)	(0.1170)	(0.0924)	(0.1659)	(0.1212)	(0.1516)	(0.0829)	(0.1364)	(0.1278)
Number of firms	364	283	187	203	216	209	210	219	200	277	142

Table 11. Alternative corporate outcomes

This table reports the results of OLS regressions using the sample of firms connected with politicians re-elected in 2005 in the treatment and control groups. The dependent variables are changes around the 2005 elections in the following variables: logarithm of total assets, column (1); ratio of liquid assets to total assets, column (2); standard deviation of industry-adjusted OROA, column (3); and ratio of total debt to total assets, column (4). In all regressions, explanatory variables are a treatment dummy set equal to one for firms connected with politicians in municipalities affected by the reform (or to zero for firms connected with politicians in control municipalities), lagged logarithm of total assets, lagged industry-adjusted OROA, a set of 2-digit industry dummies, and regional dummies. Standard errors (clustered by the new municipality classification) are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable:	Total assets	Cash holdings	Profit volatility	Leverage
	(1)	(2)	(3)	(4)
Treatment	0.1574	0.0654***	0.0197*	0.0233
	(0.1328)	(0.0203)	(0.0117)	(0.0328)
$\ln(\text{Assets})_{t-1}$	0.0051	-0.0038	0.0019	0.0165***
	(0.0187)	(0.0034)	(0.0016)	(0.0061)
$\text{Profitability}_{t-1}$	0.5581	0.0584	0.0067	-0.2156
	(0.4427)	(0.0971)	(0.0745)	(0.2350)
Number of firms	419	373	414	195

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