Political Reforms and the Causal Impact of Blood-Related Politicians on Corporate Performance

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Abstract

Estimating the causal effect of political connections on corporate performance is challenging due to lack of appropriate counterfactuals. Applying difference in difference, matching and regression discontinuity estimations techniques we use a political reform that increased the size of a majority of municipalities in Denmark as exogenous variation to identify a positive causal impact of blood related politicians. Political connections increase market share, are more valuable in outsourcing municipalities and in sectors that are highly dependent upon public demand. Focusing on local political connections in the world's least corrupt country, we conclude that political networking is an extremely valuable business strategy even in settings where connections are expected to be least relevant.

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1. Introduction

A number of recent studies have demonstrated a correlation between being connected with powerful politicians and higher firm value and performance, in particular in societies with weak legal and governance institutions. The evidence presented ranges from corporate relationships with influential politicians, such as Senator Henry Jackson in the United States (Roberts 1990), President Suharto in Indonesia (Fisman 2001) and Thaksin Shinawatra in Thailand (Bunkanwanicha and Wiwattanankantang 2009), through country studies of political connections in emerging economies such as China (Calomiris et al. 2010, Fan et al. 2007, Li et al. 2008), Malaysia (Johnson and Mitton 2003), Brazil (Claessens et al. 2008) and Pakistan (Khwaja and Mian 2005), to cross-country studies including emerging and developed economies (Faccio 2006, Faccio et al. 2006, Boubakri et al. 2008a).

Three approaches have been adopted to investigate the effect of political connections on firms' outcomes. The first is based on cross-sectional regressions using non-connected firms as the comparison group (Li et al. 2008, Faccio et al. 2006, Niessen and Ruenzi 2010). The second is represented by event studies focused either on the establishment of new connections, for example through board appointments (Faccio 2006) and marriages (Bunkanwanicha et al. 2008), or on 'shocks' to existing connections, such as changes in the political landscape (Jayachandran 2006, Goldman et al. 2009a, Ferguson and Voth 2008) and sudden deaths (Faccio and Parsley 2009). The third approach relies on panel data, focusing on splits between corporations and politicians or breaks in connections through the loss of political power (Goldman et al. 2009b). Overall, previous studies employ non-connected firms as counterfactuals, either through the cross-sectional or the longitudinal variation in connections.

Many of these studies have convincingly argued, on a case-by-case basis, that political connections have a causal impact on corporate value. However, as a general identification strategy it is problematic to use non-connected firms as counterfactuals because such firms are often very different from connected firms both in terms of observable and unobservable characteristics (Faccio 2009). These differences introduce interpretational problems due to omitted factor and self-selection bias.

In this paper, we offer a novel identification strategy which exploits an exogenous variation in the size of local governments for given connections between firms and politicians. Our exogenous variation in the size of political rent stems from an administrative reform that took place in Denmark in 2006, whereby 238 municipalities merged into 65 new ones (treatment group) and left 33 municipalities unchanged (control group). We investigate how the *enlargement* of local governments influenced the profitability of firms connected *before* and *after* the reform with local politicians, using as counterfactuals firms connected *before* and *after* the reform in municipalities that did not change size.

The premise of our identification strategy is that an increase in the size of local governments creates a 'positive shock' to politicians' power and influence and therefore to the amount of rent politically connected firms may potentially extract. We provide support for this premise by showing that population per politician, governmental budget per politician and outsourced expenses per politician increase significantly in merging municipalities compared to non-merging municipalities. Moreover, the reform in itself was backed up with 1.2 billion DKK to cover merger expenses in merging municipalities only.

Our natural experiment presents two empirical advantages. First, we can keep fixed the connections between firms and politicians over time and identify the value of connections through a positive shock that exogenously impacted some politicians but not others. Second, we can focus solely on connections with winning candidates both in the treatment and control group. Such advantages allow us to depart from previous studies that use changes in connection status, thus avoiding problems from the potential endogeneity in the formation and disruption of connections. Similarly, we do not employ non-connected firms or firms connected with non-elected candidates, which represent poor counterfactuals because the electoral results of connected politicians are potentially endogenous to corporate outcomes.

Using OLS difference-in-differences we find that being connected with politicians in merging municipalities leads to an economically and statistically relevant increase in a firm's profitability. For the average district enlargement, our benchmark specification reveals that the operating return on assets nearly doubles for the average connected firm.

This effect is particularly pronounced for small firms, firms operating in unprofitable industries and firms connected with more powerful politicians.

To address the possibility that the impact of the reform may be heterogeneous with respect to observable corporate and political characteristics that are unbalanced across treatment and control municipalities, we adopt a matching approach. As an alternative estimation strategy, we exploit the sharp discontinuity at 20,000 inhabitants that was adopted as the main criterion to select merging municipalities. Comparing firms connected with politicians in municipalities that are barely above and below this threshold mitigates the concern that the merging group may be formed by municipalities with declining economic or demographic performance, and thus that connected firms in those municipalities may not be fully comparable with firms located in large unchanged municipalities. Further validation of the causal relation is derived since we do not find a positive effect for non-connected firms nor for firms connected with politicians that ran for local office but were not elected. Neither do we find any significant impact for a placebo increase in district size on a firm's performance prior to the implementation of the administrative reform.

An important contribution of our paper is that we estimate a *lower bound* for the value of political connections, which is important to our understanding of the role of political networking as a general business strategy. Most existing studies have investigated situations where the potential benefits of political ties were expected to be highest. This includes the analysis of (a) politicians who are known to be powerful and corrupt (e.g. Suharto in Indonesia[1]); (b) countries with weak institutions and high levels of corruption (e.g. China and Brazil); or (c) connections between the most powerful politicians and the largest publicly traded firms.

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[1] It is insightful to observe exactly how valuable it was for corporations to be linked or controlled by President Suharto in Indonesia. However, according to Wikipedia, Suharto has unofficially been labeled the most corrupt dictator ever: "In May 1999, Time Asia estimated Suharto's family fortune at US$15 billion in cash, shares, corporate assets, real estate, jewelry and fine art. Of this, US$9 billion is reported to have been deposited in an Austrian bank. The family is said to control about 36,000km² of real estate in Indonesia, including 100,000 m² of prime office space in Jakarta and nearly 40% of the land in East Timor. Suharto was placed highest on Transparency International's list of corrupt leaders with an alleged misappropriation of between US $15-35 billion during his 32-year presidency". Thus, it is hardly surprising that firms linked to the Suharto family prospered. On a lesser scale, the same can be said about Thaksin Shinawatra in Thailand and other extremely powerful politicians that the literature has focused on.

For the purpose of this paper we go to the other extreme: we study local political connections for *all firms* in Denmark, which, according to the well respected Corruption Perceptions Index (CPI),[2] has been the *least corrupt* country over the last three years. Thus we investigate the value of political ties in an economic environment where our prior assumption is that such ties are least valuable. Our results, therefore, support the notion that political networks are of great importance in all countries around the world and at all political levels.

Politically connected firms are common all over the world; according to Faccio (2006) they constitute 7.7 pct. of global stock market capitalization. For example, politicians may benefit from improved electoral results (Bertrand et al. 2007) and/or private welfare (Eggers and Hainmueller 2009). Connected firms may benefit from easier access to external finance (Claessens et al. 2008, Boubakri et al. 2008b), lighter taxation (Faccio 2006), financial assistance in times of economic distress (Faccio et al. 2006), favourable treatment in the allocation of procurement contracts (Goldman et al. 2009b), and protection from internal and external competition (Bunkanwanicha and Wiwattanakantang 2009).

An additional contribution of our paper is to identify a clear channel through which small and medium sized companies benefit from connections with local politicians. Our estimates suggest that connected firms in treatment municipalities increase their market share. Furthermore, we find a larger treatment effect for firms connected with municipalities that outsource a higher proportion of public services to private contractors and for firms operating in industries that are more dependent upon public demand. Taken together, these results are consistent with the hypothesis that political ties help to secure a larger share of outsourced local service provisions. In contrast, we find no evidence that political connections shape a firm's capital structure through easier access to debt financing or by supporting firms in financial distress.

Our final contribution is to confirm the above results through a simple but novel approach: a direct survey conducted among all connected firms in our sample and a group of non-connected firms. We received 170 answers from connected firms and 159 answers

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[2] CPI by Transparency International ranked Denmark 1st in 2007, 1st in 2008 and 2nd in 2009, only marginally behind New Zealand.

from non-connected firms. The survey results confirm that connected firms have a better relationship with local government and that political connections are important for this relationship. In addition, the survey indicates that connected firms are better at dealing with the municipality in areas of 'soft' regulation, including environmental documentation and regulations related to construction and plant expansion projects.

We believe that our findings raise an interesting discussion about how to *measure* corruption. Corruption refers to illegal activities that: a) misuse public funds for private benefits; b) circumvent existing rules; and, c) provide firm-specific benefits (Treisman 2000 and Bennedsen et al. 2010). We do not imply that the activities studied in this paper are illegal; however, they do satisfy the above three descriptive elements of corruption. In particular, since connected firms are found to be less productive and the gains from production are higher in unproductive sectors, there is some evidence that political connections induce a reduction in economic welfare. Thus, our findings indicate that there can be a high level of 'legal' corruption even in the least corrupt country in the world.

In Section 2, we describe our data and the institutional features of the Danish administrative reform. In Section 3, we provide summary statistics and discuss the validity of our identification strategy. In Section 4, we present the empirical results and a number of robustness checks. In Section 5, we identify the channels through which small and medium sized firms benefit from local political connections. In Section 6, we conduct a direct survey asking connected and non-connected firms about the impact of the political reform on their relationship with the local government. Finally, Section 7 discusses our findings and concludes.

2. Data description

2.1. The Danish administrative reform

Danish local governments are responsible for the provision of important public services in various sectors (healthcare, employment, social services, special education, business services, collective transport and roads, environment and planning) and manage

about 48% of total public expenditure.[3] There is a fixed four-year interval between local elections in Denmark. Local elections took place in 2001, 2005 and 2009. We received electoral data from the Danish Ministry of the Interior containing the personal identification number (CPR number) of all candidates in the 2001 and 2005 local elections, their party affiliation, the number of votes and if they were elected or not.

The main input to our identification strategy derives from a change in the geographic borders of Danish municipalities after the local elections in November 2005.[4] Figure 1 maps the Danish municipalities before and after the administrative reform, and Table 1, Panel A, describes how the reform reduced the number of municipalities.

Since the previous electoral reform in 1974, there had been 275 municipalities varying in size from less than 5,000 inhabitants to more than 400,000. (The old municipalities are represented on the left side of Figure 1). Given the economic and administrative inefficiencies of having 205 municipalities with less than 20,000 inhabitants, the reform aimed to create larger and more efficient entities. Table 1, Panel A, and the right side of Figure 1 show the outcome of the reform: 238 municipalities were merged into 65 new and larger municipalities, while 33 mostly large municipalities were left unchanged. As a result of the reform, the size of the average (median) municipality increased from approximately 159km^2 to 440km^2, and in terms of inhabitants from approximately 20,000 (10,000) to 56,000 (49,000). Table 1, Panel B, shows the impact of the reform on the number of municipalities by population size.

As documented by Dreyer Lassen and Serritzlew (2009), the selection of merging municipalities was strictly based on population size, which is arguably exogenous to current firm outcomes. An additional constraint was that the merging municipalities had to be neighbors. Less obvious requirements were applied in few cases, for example, the color of the ruling political constituency in the municipalities and/or differences in income

levels.[5] Finally, 14 municipalities were split into two sections, each of them merging into separate larger municipalities.

2.2. Corporate and management data

We constructed a dataset of firms connected with local politicians, starting out with the totality of Danish firms, which amounted to 53,820 incorporated limited liability companies for the period 2002-2008. We combined several data sources in the process.

Accounting data for all 53,820 firms were collected from Experian, a private firm that collects the annual reports that all limited liability firms are required to submit to the Danish Ministry of Economics and Business Affairs. Unfortunately, Danish law requires private firms to disclose only a limited number of items such as total assets, selected measures of profitability including operating and net income, and a few variables related to capital structure. Other items such as sales or employment are not required to be disclosed, albeit around one third of firms disclose these measures voluntarily.

The Danish Ministry of Economics and Business Affairs provided the personal identification number of all managers and board members in the Danish firms from 1994 to 2007, including the dates of entering and exiting managerial positions. Our data sources can be deemed reliable because they originate from information that firms are obliged to deliver by law and has been approved by external and independent accountants.

We then proceeded to create the family tree behind each top manager and director. For each personal identification number in our sample, the official Danish Civil Registration System at the Ministry of the Interior provided us with the personal identification number of all close family members. These administrative records contain individual characteristics such as gender, dates of birth and death, and marital history (number and dates of marriages, divorces, and widowhoods).

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[5] Two municipalities were allowed to stay independent because the neighboring municipalities were of different political color and a few poor municipalities had a hard time finding neighbors willing to merge.

2.3. Politically-connected firms

By merging the families behind the top management and directors with the election data, we can identify blood-related political connections. By 'blood-related' we mean a politician that either is a CEO and/or a board director, or is family-connected to a CEO and/or a director of a firm. The family relations we consider are parent, child, sibling and current or former spouse(s). We drop connections that ended before or started after the local elections in 2005.

Since we have election data for both the 2001 and 2005 elections, we can classify firms into different groups depending on the connection status through the two election periods. Firms can be connected in both electoral periods – denoted as *staying connected* – or they can be connected in 2005 but not in 2001 – denoted as *newly connected*. The other two groups are formed by firms not connected in any of the two periods, and firms that were connected in 2001 but not in 2005.

3   Empirical strategy and summary statistics

Our main goal is to measure the impact of political connections on firm performance, employing the exogenous increase in the size of local governments from the Danish administrative reform to identify variations in political power.

For this purpose we classify municipalities into 'treatment' municipalities – those which increased in size – and 'control' municipalities – those which did not. We focus only on 'staying connected' firms, i.e. firms that were connected both after the 2001 elections and after the 2005 elections. We exploit the longitudinal nature of our data to estimate a difference-in-differences model (DD hereafter), measuring how the increase in political rent arising from a larger local government affects the profitability of connected firms around the implementation of the reform, using as a control group those firms that were connected with politicians in non-merging municipalities. This methodology allows us to control for aggregate changes in the business environment (e.g. macroeconomic shocks) and connection-specific shocks.

The validity of our identification strategy relies on three premises. The first is that the selection of merging municipalities is not driven by corporate outcomes. We have indicated above that the criteria used to merge municipalities were almost entirely determined by population size and geographic conditions, which were mostly historical in origin and independent of current firm performance.[6]

The second premise is that the increase in district size caused an increase in political rent in treatment municipalities through a combination of fewer politicians, more tasks and larger budgets. Table 1, Panel C, reports three measures of the size of political constituencies before and after the reform in both treatment and control municipalities. The number of inhabitants per politician significantly increased in merged municipalities, whereas it remained unchanged in control municipalities. Expenditure per elected politicians also increased significantly in merged municipalities, indicating that politicians elected in those municipalities managed a larger budget. Finally, in the last row we focus on expenditure on local government activities outsourced to private service providers. We find a significant increase in outsourcing both in treated and control groups; however, the actual increase is more than six times larger in treated municipalities. Thus, Panel C in Table 1 provides strong evidence that the reform provided an exogenous increase in the potential power of the average politician in the treated municipalities relative to control municipalities.

In addition to these figures, the implementation of the reform required merging municipalities to accomplish some transitory tasks (e.g. integration of IT systems or public transportation networks). Expenses for these tasks amounted to almost DKK 1.2 billion, including approximately DKK 750 million for IT adjustments and DKK 175 million for relocations.[7] These activities were to a large extent outsourced to private companies.

Research on political accountability has yielded additional support for the second premise. Studies of electoral rules and fiscal federalism suggest that centralization and the creation of larger electoral municipalities imply lower electoral accountability (Persson and Tabellini 2000), which in turn may encourage political misbehavior (Fisman and Gatti

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[6] We admit that migration across municipalities can be affected by corporate performance, but this is for almost all municipalities a second-order concern to the geographic and historical determinants, and we argue that it has not been a decisive factor in any of the mergers between municipalities in Denmark.

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

2002) and the scope for private businesses to expropriate rent from their political connections. Taken together, these results suggest that the size of the political constituency and the potential for rent extraction substantially increased in treatment municipalities as a result of the reform. While we cannot, from these insights, conclude that *every* politician in the treatment districts became more powerful, in our empirical analysis control we do control for the possibility that only leading politicians or politicians belonging to the ruling party became more powerful.

The third hypothesis for the validity of our identification strategy is that the enlargement of local municipalities does not affect corporate performance through other channels than the political connections. Although we cannot *a priori* rule out that a merger benefits all firms in a given municipality, e.g. by fostering economic activity or improving the business environment, our empirical investigation shows that this is not the case; only connected firms benefit from the reform.

Table 2 illustrates our political data. In total, 11,341 individuals ran for office in the municipal election of 2005. Among them, 8,375 (2.966) ran in treatment (control) municipalities. In total, 2,502 candidates were elected in the 98 municipalities. This figure reflects the following administrative rules that determine the number of seats in municipality councils: (1) Merging municipalities have between 25 and 31 seats; (2) Non-merging municipalities have 19-31 (9-31) seats if more (less) than 20,000 inhabitants.; (3) The municipality of Copenhagen has 55 seats. Note that 1,852 (650) candidates were elected in treatment (control) municipalities, hence the ratio of winners to the total number of candidates is similar in the two groups. This result suggests that the reform did not drastically alter the electoral competition across municipalities.

The second part of Table 2 shows the same figures when we only look at politicians that were connected with firms. Approximately 1 out of 10 candidates are blood-related to firms and this is similar across the two groups of municipalities. The ratio of winning candidates who are connected with companies relative to all candidates is approximately 1 to 6, with no significant differences across groups. This tells us that business connections are correlated with a higher probability of being elected, which provides further evidence that using non-connected or newly-connected firms as counterfactuals might suffer from selection bias.

The last part of Table 2 describes the set of staying connected firms that we use for establishing a causal link between political connections and corporate performance. Of a total of are 383 connected politicians that were re-elected in the 2005 elections, 290 of them were located in treatment municipalities and 93 in control municipalities.

Examining (but not reporting) the industry distribution, we find only minor differences between connected and non-connected firms: connected firms are slightly more present in real estate and slightly less present in insurance and financial sectors. Furthermore, we find very small differences in the industry distribution of connected firms in treatment and control municipalities: connected firms in treatment municipalities are marginally more present in manufacturing, trade and transport, whereas firms in control municipalities operate more in other business segments.

Table 3 measures average characteristics of connected and non-connected firms prior to the implementation of the administrative reform. Our main measure of corporate performance is operating return on assets (OROA), measured as the ratio of earnings before interests and taxes (EBIT) to the book value of total assets. Unlike net income-based measures of performance, OROA is unaffected by differences in firms' capital structure. To check whether differences in OROA are explained by differential industry trends, we also report industry-adjusted OROA. The industry adjustments are calculated using the average and the median OROA of each 2-digit industry, considering all active firms in our dataset, including those that are not politically-connected.

In Table 3, Columns (1), (2) and (5), we look at the average performance before the reform for all non-connected and all connected firms. On average, connected firms are larger and worse performing than non-connected firms, which is consistent with the cross-country evidence provided by Faccio (2009). Overall, in Table 3 there are observable differences between these two groups of firms. Given such economically and statistically significant differences, it is to be expected that a comparison between connected and non-connected firms will suffer from an omitted factor bias, which casts further doubt on an identification strategy based on non-connected firms as counterfactuals.

In Table 3, Columns (3), (4) and (6), we show that the differences between firms in treatment and control municipalities connected with all candidates are much smaller both in economic and statistical terms, except for differences in performance. One observable

factor that may explain the differences in Column (6) is the variation in distribution between the treatment and control municipalities in the ratio of connections to winning and non-winning candidates. Even if differences are smaller than for columns (1), (2), and (5), the evidence still raises concern about the presence of an omitted factor bias. More importantly, a comparison between firms that are connected with winning and non-winning candidates may suffer from reverse causality in the likely event that the probability of a business-connected politician winning a seat is affected by the characteristics of the firm he or she is connected to.

To circumvent such endogeneity issues, we focus the analysis on firms that are connected with politicians re-elected in 2005 in both treatment and control municipalities (i.e. politicians who had a place on the municipal council both before and after the 2005 election). Table 3, Columns (7) - (10), show that there are no statistically significant differences between firms in treatment and control groups in terms of total assets, performance, sales and employees. While we cannot rule out the presence of unobserved differences between the two groups, the lack of significant differences in observable terms suggests that this problem is much less likely to interfere with our results. Together with our argument above that municipalities are allocated to control and treatment groups irrespective of corporate characteristics, this is strong confirmation of the validity of our counterfactuals.

We adopt three estimation techniques. First, we estimate a DD model by means of OLS regressions. Second, we adopt a matching procedure to minimize the distance between *staying connected* firms in the treatment group and *staying connected* firms in the control group, thus reducing the bias induced by e.g. differences in observable characteristics that might be unbalanced across treatment and control municipalities. Third, we exploit the sharp discontinuity at 20,000 inhabitants adopted to select merging municipalities in order to mitigate any concern that the merging group may be formed by municipalities with declining economic or demographic performance and therefore that connected firms in those municipalities are not comparable with firms located in large unchanged municipalities.

4. Results

4.1. OLS difference-in-differences

Table 4 presents the results of OLS regressions where the dependent variable is the change in firm profitability around the 2005 elections as well as the relative implementation of the administrative reform (we consider three years after versus three years before, excluding the election year). Treatment is a binary variable equal to 1 if the firm is connected with a politician re-elected in a treatment district, and 0 if the firm is connected with a politician re-elected in a control district.

In Column (1), we report estimates using unadjusted OROA as the dependent variable and only control for regional localization to reduce the scope for omitted factor bias. We compute Huber-White robust standard errors. The treatment effect is 4.1 pct. and is statistically significant at the 5 pct. level. It indicates that *staying connected* firms in merging municipalities experienced, on average, a 4.1 pct. point improvement in OROA compared with *staying connected* firms in municipalities that did not change size.

This impact becomes marginally higher and significant at the 1 pct. level when we control for lagged assets and operating performance (Column 2). Consistent with mean reversion in profitability, the lagged performance shows a negative coefficient, though it is not statistically significant at the conventional level. In Column (3), we control for industry effects by including a set of 2-digit industry dummies, while in Column (4) we further control for the age of the connected politician, his or her position in the electoral list and the sum of activities that municipalities outsource to private contractors divided by total expenditure. Outsourcing and politician age are positive but not statistically significant at the conventional level. In Columns (5) and (6), we employ as the dependent variable the change in industry-adjusted OROA around the election. We obtain significant and positive results using both median (Column 5) and mean adjustments (Column 6) at the relevant 2-digit industry.

Since the treatment is defined at the municipality level, we allow for correlation of residuals within municipalities by clustering standard errors at the municipality level.[8] We present these estimates in Columns (7) and (8), using industry-adjusted OROA as the dependent variable. Despite an increase in standard errors, the treatment effect is statistically significant at the 5 pct. level.

On the basis of these estimates, we conclude that the increase in profitability is statistically significant and ranges between 3.9 and 4.2 pct. Given that the average OROA in Denmark is 4.2 pct. (Table 3), the economic magnitude of such an increase is very large; for the average staying connected firm it is even bigger due to adverse performance prior to the election. The average re-connected firm has an OROA which is half as large as the average connected firm. Thus for the average re-connected firm the magnitude is in the order of a 200 pct. increase in OROA.

4.2. Falsification and robustness tests

Our identification strategy hinges crucially on the exogeneity of the administrative reform relative to corporate outcomes. However, there are two additional risks to the causal interpretation of our results. The first is that the effect of enlarged municipalities might improve the performance of all connected firms or even non-connected firms. This happens when, for example, mergers positively affect the demand for private services and other goods, or improve accounting standards by allocating more resources to the auditing process. Results in Table 5 help to rule out this interpretation. In Columns (1) and (2), we estimate DD regressions for non-connected firms only, whereas in Columns (3) and (4), we estimate DD regressions for firms connected with non-elected candidates. In both cases we find that the treatment variable is not significant in either statistical or economic terms.

The second issue is the implicit assumption of parallel trends for the validity of the DD model. To underline that the two groups appear to be very similar before the implementation of the reform, we propose a falsification test in Columns (5) and (6) estimating DD regressions in a pre-treatment window centered at $t = -3$. The lack of

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[8] As described in Table 1, the new districts classification contains a number of clusters that are sufficiently large. As robustness check, we use the old district classification, which contains a larger number of clusters, and the precision of our estimates is only marginally changed.

statistical significance confirms that the two groups were similar before the 2005 elections, and therefore confirms the validity of the parallel trends hypothesis in our setting.

We perform a number of further checks (not reported) to assess the robustness of the OLS estimates reported in Table 4. To confirm that our results are not driven by outliers,[9] we trim the dependent variable to 1% on the right and left tails of the distribution. Moreover, we run a median regression and perform a graphical inspection of residuals to detect influential observations. In addition to clustering at the municipality level, we consider an alternative way of computing standard errors based on block-bootstrap (Bertrand et al. 2004) using 500 replications.

Finally, we exclude firms in financial, insurance and utilities industries or, alternatively, we include a set of 3-digit or 1-digit industry dummies in regressions where the dependent variable is unadjusted OROA. We also exclude firms connected with municipalities that were split into separate larger entities. All results from these tests are statistically significant and in line with our previous estimates (coefficients are ranged between 3.2 and 4.8 pct.).

4.3. Matching and discontinuity estimates

We now investigate whether the DD results are robust when we use alternative estimation methods. In Table 6, Columns (1) - (5), we provide an alternative set of estimations based on nearest-neighbor matching (Rosenbaum and Rubin 1983; Abadie and Imbens 2007). We match staying connected firms in the treatment group with staying connected firm in the control group along corporate and politician characteristics. The benefit of this approach is that we not only use staying connected firms in municipalities that do not change in size as counterfactuals, but for each firm in a treatment group we find the most similar firm in the control group that can serve as benchmark, discarding dissimilar observations. By minimizing the distance between the two groups, this approach reduces the bias induced by differences in observable firm and political characteristics that might be unbalanced across treatment and control groups.

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[9] In the estimates reported in Table 4, we use all firms except one that reported an implausibly large change in profitability around the elections. Keeping this firm in the sample does not alter the statistical significance of our results but leads to larger coefficients.

The covariates included in the matching procedure are pre-treatment assets and industry-adjusted operating performance, regional localization, logarithm of age of the connected politician and his or her position in the electoral list. We compute the matching estimators in the following way: (1) we run a probit regression where the dependent variable is the binary treatment and the explanatory variables are the above-mentioned variables; (2) we use the predicted values to construct the propensity score, discarding the few observations outside the common support; (3) we match with replacement each firm in the treatment group with a firm in the control group and then estimate the difference in change of profitability around the election.

The first column presents the results. The estimate is significant at the 5 pct. level and marginally lower than the OLS estimates, confirming the robustness of our previous results. In Column (2), we use the procedure described in Abadie and Imbens (2007) to correct for the remaining bias due to ex-ante differences between firms in treatment and control groups. In Column (3), we match on the propensity score and rematch on the controls directly, reporting the bias-adjusted results. The last two columns of Table (7) provide results from alternative matching methods (Radius and 1-to-1 match without replacement). All the estimates are significant both in statistical and economic terms, and ranged between 2.3 and 4.5 pct.

A further concern about our identification strategy is that the treatment group may be formed by municipalities with declining economic or demographic performance, and therefore connected firms in those municipalities will not be fully comparable with firms located in large unchanged municipalities. Although we have already proved that such potential differences are not reflected in the different levels of profitability between treatment and control firms before the reform, we offer two ways to address this problem.

First, we exclude the smallest municipalities in the treatment group and the largest municipalities in the control group. Results, reported in Columns (6) and (7), are qualitatively in line with our baseline estimates. Second, we exploit the sharp discontinuity at 20,000 inhabitants that was adopted to select merging municipalities by comparing connected firms located in municipalities that were barely above and below this threshold. Since this variable is precisely measured and it is not manipulable by politicians, it offers an ideal context for a regression discontinuity design. We create the running variable as

the distance in terms of number of inhabitants in 2004 from the threshold and then we estimate a linear and quadratic specification, adding the usual set of controls. Results, reported in Columns (8) and (9), show that the local treatment effect is positive at the 10% level and marginally higher than the OLS estimates.

4.4. Heterogeneous effects and alternative dependent variables

In Table 7, we explore the heterogeneity in the treatment effect along firm, industry and political characteristics. We show the results of OLS regressions where the dependent variable is the change in industry-adjusted OROA for -3 to +3 years around the 2005 election.

In Column (1), we restrict the sample to *nuclear-family connections*, including political ties through the direct election of the manager, or the election of his/her spouse, son or daughter. As expected, the treatment coefficient is larger than the one estimated using the entire sample; when connections are tighter, the positive impact on firm performance is greater.

In Columns (2) and (3), we separately analyze *small and large firms*. While the treatment coefficient is significant in both samples, there is an indication that the effect is larger for smaller firms. Since we measure the impact of local connections on the *ratio* of operating return to assets, the results are consistent with the claim that large firms are more likely to focus their business outside the local district.

In Columns (4) and (5), we divide our sample with respect to industries that have a *high or low concentration* of politically connected firms. In industries where political connections are more common, and perhaps where companies potentially have more to offer or gain from interaction with local government, the treatment effect is larger (4.6 pct.) than in industries with low political concentration (2.2 pct.) In Columns (6) and (7), we also observe that the effect is positive both in *high-profitability and low-profitability* industries; however, it appears to be larger and statistically significant in low-profitability industries. This is consistent with firms in highly profitable industries being more oriented outside the local municipality or profitable firms in general being less dependent on their political connections.

In Columns (8) – (10), we test whether the positive impact on performance varies with *the scope* of the political connection. We start by focusing on *powerful politicians*, defined as politicians that won more than the median share of votes in their party and in a given district. The coefficient, reported in Column (8), is marginally higher than the average impact and significant at the 5 pct. level. In Column (9), we explore connections with *powerful parties*, defined as parties that received more than the median share of votes in a given municipality. The estimated coefficient is not significantly different from the average impact. Finally, when we look at firms connected with politicians belonging to the *mayor's party or coalition* (Column 10), we again find that the coefficient is notably higher than the average impact. Taken together, these tests suggest that the benefits to the firm produced by political ties increase in proportion to the power of the politician involved in the connection.[10]

In Table (9), we test the impact of blood-related connections on alternative corporate outcomes. In Column 1, we find that the increase in district size has a significant impact on net income to assets. In Column (2), we test whether the increase in district size affects the value that connected firms gain from their assets and liabilities by using the return on capital employed (ROCE) as the dependent variable. We compute ROCE by replacing the total assets in the denominator of the OROA with the net assets or the capital employed. The increase in ROCE is 3.1 pct. and is statistically significant at the 10 pct. level. Therefore, there is evidence that the increase in profitability of connected firms entails a real increase in the productivity of firms' net assets. In Column (3), we provide evidence that the increase in district size has some significant (at the 10 pct. level) effect on firm size, measured by changes in total assets; in Column (4) we show that the growth in assets comes primarily from an increase in liquidity holdings.

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[10] In discussing the premises of our identification strategy, we show that, on average, mergers increased the power of politicians. Columns (8)-(10) show that even when the increase in power is heterogeneously distributed across politicians, our analysis still provide the same or even stronger results when we focus on powerful politicians or powerful parties.

5. Identifying channels to transfer political rent

The previous section has established that being connected with larger municipalities causes a statistically and economically relevant improvement in connected firms' operating performance. In this section, we investigate a number of channels through which connected firms may benefit from their political networks.

Previous studies have examined several channels. Faccio et al. (2006) provide cross-country evidence that politically-connected firms are more likely to be bailed out by governments and to benefit from financial support provided by the International Monetary Fund or the World Bank. In a related work, Boubakri et al. (2008b) argue that connected firms exhibit a lower cost of equity capital. Other studies show that political connections shape the firms' capital structure (Claessens et al. 2008, Li et al. 2008), mainly through easier access to bank lending (Khwaja and Mian 2005). Goldman et al. (2009b) provide evidence that politically connected firms are favorably treated in the allocation of procurement contracts; a shift in power from the Democratic to the Republican Party (and the reverse) increases the value of procurement contracts for firms connected with the Republican (Democratic) Party.

The literature has also highlighted that political ties can be costly for the firm. Bertrand et al. (2007), in line with the theoretical framework of Shleifer and Vishny (1994), show that connected firms manipulate layoffs and hiring decisions near elections; Fan et al. (2007) and Chaney et al. (2010) find that connected firms have poorer accounting standards and weaker governance institutions.

Table 9 describes some channels through which political connections help small and medium sized companies in our 'corruption-free' environment. Column 1 indicates that an improvement in operating performance is closely related to an increase in market share. Following Blundell et al. (1999), we compute market share as a firm's sales[11] over total industry sales, defining a firm's industry as its principal operating industry at the 3-digit level. Our estimate shows that firms connected in merged municipalities increase their market share by 1.9 pct., which is consistent with the hypothesis that connections are

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[11] Sales are not an item that private firms are required to publish in their yearly statements. This explains the reduced number of firms when we use market share as dependent variable.

beneficial for firms' revenues (Bunkanwanicha and Wiwattanankantang 2009, Cingano and Pinotti 2009).

If the source of the improved market share is that connected firms are better able to win procurement contracts from the respective municipalities, we would expect this effect to be related with the size of the political merger. To test this idea, we split the treatment sample according to how many municipalities were merged. Column (2) contains firms in municipalities where three or less municipalities were merged, while Column (3) contains firms in the sample of more than three mergers. We notice that the coefficient for the treatment effect is about 20 pct. greater than the one obtained using the entire sample.

In the last four columns of Table 9, we test in alternative ways whether connected firms benefit by getting more and/or better procurements contracts. Our hypothesis is that the increased political power in merging municipalities will have a stronger impact on firms' revenues in municipalities that outsource more. To capture this effect, we split merged municipalities into two subsamples according to the ratio of activities outsourced to private contractors divided by total expenditure. In municipalities that have a low outsourcing ratio, we observe a positive treatment effect; however, this effect is much higher and more statistically significant in municipalities that have a high outsourcing ratio. This evidence is consistent with the idea that connected firms after the reform have the ability to increase their share of existing outsourcing activity at the expense of firms that lose their connections and are preferentially treated when new procurement contracts are offered.

We further investigate how the public sector influences the value of political connections by exploiting the heterogeneity in the sectoral dependence upon public demand. Following Cingano and Pinotti (2009), we analyze the cross-entries between the public consumption and industries in the 2-digit Danish Input-Output matrix to classify industries as highly or weakly/not dependent upon public demand. Then we run performance regressions for these two samples separately.

The treatment effect for firms operating in industries that are closely linked to the public sector is above 5 pct. and significant at the 5 pct. level (Column 6). In contrast, when the sectoral dependence is low, the treatment effect is not statistically different from

zero (Column 7). Taken as a whole, the findings reported in Table 9 support the hypothesis that connected firms benefit from business relations with the public sector.

In Table 10, we test whether there is any impact from an increase in political rent on debt financing. We use three different measures of leverage, including total debt (Column 1), short-term debt (Column 2) and long-term debt (Column 3) to total assets. The treatment coefficient is not significant in any of these cases; neither do we find any impact on the maturity structure of debt, measured as the ratio of long-term debt to total debt (Column 4). In addition, we find no supporting evidence (unreported) that political connections protect firms in financial distress. Overall, these results suggest that an increase in political rent does not influence locally connected Danish firms through the cost of capital or access to debt financing. In unreported results we also find that there are no effects on wages or employment.

6. Identifying channels through survey data

The analysis presented above is based on a registered dataset which by nature has limitations in describing the relationship between firms and local governments. To provide a more complete picture of this relationship, we conducted a direct survey aimed at all the politically connected firms in our sample and a group of non-connected firms.[12]

The aim of this survey was to confirm the general insights of the analysis above and to identify potential new channels through which firms benefit from their political networks. We do not claim that such a survey in itself is able to provide more than correlations; however, given the causal analysis above, we believe it can shed light on potential new interpretations.

We asked CEOs and directors a total of 15 questions about their relationship with the local municipality before and after the reform. The questions covered three dimensions: first, their satisfaction with the relationship with the municipality after the reform; second,

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[12] The survey was conducted by Statistics Denmark and neither the individual nor the firm identity is known to the authors of this article. This can technically be done because we conduct the analysis on a server at Statistic Denmark and they have linked personal, managerial and accounting data together without our involvement. When we operate on the server, we cannot identify politicians or companies. Statistics Denmark linked the survey answers to our data in anonymous form.

which factors were important for the firms' relationship with the municipality; third, in which areas the local government was most useful to the companies.

Figure 2 shows the distribution of answers to representative questions within the three dimensions. In general, it was possible to answer on a scale from 1 (most positive) to 4 (least positive) or 5 (a category that the respondent could use if the question was not relevant or he or she did not wish to answer). The columns show the percentages of firms that answered the first two most positive answers.

Columns (1) and (2) show that the vast majority of small and medium sized firms have a positive and constructive relationship with the local municipality. However, firms that are blood related to local politicians have an even better relationship and are even more satisfied than unconnected firms.

Columns (3), (4) and (5) identify the importance of political networks. We asked which factors were most important for the relationship with the municipality. On average, blood-related firms found their political connections to be much more important for the relationship with the municipality than non-connected firms. Most important were the CEOs own connections, followed by directors' connections and then family connections.

The last part of the survey contained questions about how important certain areas were for the firms' perception of the relationship with local governments. Columns (6) and (7) show that collaboration is, in general, important in matters related to 'soft' regulation, such as when firms are expanding, building new facilities, or moving production. The same can be said for the increasingly important areas of documentation of environmental policies and regulations related to environmental practices. The columns show that politically connected firms benefit more from the relationship with the local government in these areas.

The survey data do not allow us to explore these issues more deeply but we believe the answers are consistent with earlier findings that political connections are important and valuable for companies. Furthermore, they are consistent with the view that political connections help corporations in operative and regulative areas beyond the direct effect on sales.

7. Conclusion

Blood relationships with political decision-makers improve firm performance even in an institutional context where the effect is expected to be negligible. By using political reforms to identify variations in political power and rent extraction, we have documented that political ties with local politicians are extremely important for the profitability of small and medium sized companies even in a country ranked as the least corrupt in the world. Our analysis thus indicates that political networking can be a very powerful business strategy across all firm sizes, irrespective of the stability of political institutions.

While political connections are valuable both in developed and developing countries, we argue that the channels through which political rent is transferred to connected firms may vary. Previous studies on less developed countries have documented that political connections affects firms' capital structure through lower cost of capital, protection in times of financial distress and easier access to debt financing. Our evidence suggests that the main channel to transfer political rent to connected firms is represented by procurement contracts, supporting the earlier findings of Goldman et al. (2009b). Hence, there are some indications that in developed countries with strong political institutions the transfer of rent through political connections is demand driven; connected firms are in a better position to gain from public sector outsourcing activity. Beyond the demand effect, our survey responses indicate that there may be a broader range of areas related to soft regulation through which connected firms have a better relationship with local governments.

Analyzing the full welfare effects of political connections is beyond the scope of this article. However, our analysis does contain some partial pointers suggesting that political connections are welfare reducing. First, politically connected firms tend to be less productive before the connection is established; moreover, the staying connected firms that we analyzed were less productive than non-connected firms. Second, we showed that the value of political connections is higher in less profitable industries. Both arguments indicate that political connections may transfer rent from more productive to less productive firms. However, the welfare reduction is mitigated because our results also indicate that connected firms use the rent to increase their productivity and use their capital more efficiently.

Our analysis also contributes to the discussion on how to measure corruption. We study local connections in the least corrupt country in the world. While there is general agreement that a distinctive feature of corruption is the misuse of public office for private gain (Treisman 2000), a clear definition is hard to establish. Corruption encompasses at least three elements: it is illegal, it is an attempt to circumvent existing rules, and it is generally associated with favors extended to particular firms (Bennedsen et al. 2010). We do not wish to imply that any of the connected firms or any of the elected politicians in our sample have engaged in illegal behavior. However, our analysis provides evidence that is consistent with the last two elements of the description above; the rules and guidelines for procurement contracts in Denmark seem to be circumvented by connected firms, and there are measurable firm-specific benefits from being politically connected. When this is the case, it would suggest that that rent transferred to politically connected firms in Denmark constitutes a misuse of public funds for private gain.

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Figure 1
Danish municipalities before and after the administrative reform

Municipalities pre-reform   Municipalities post-reform
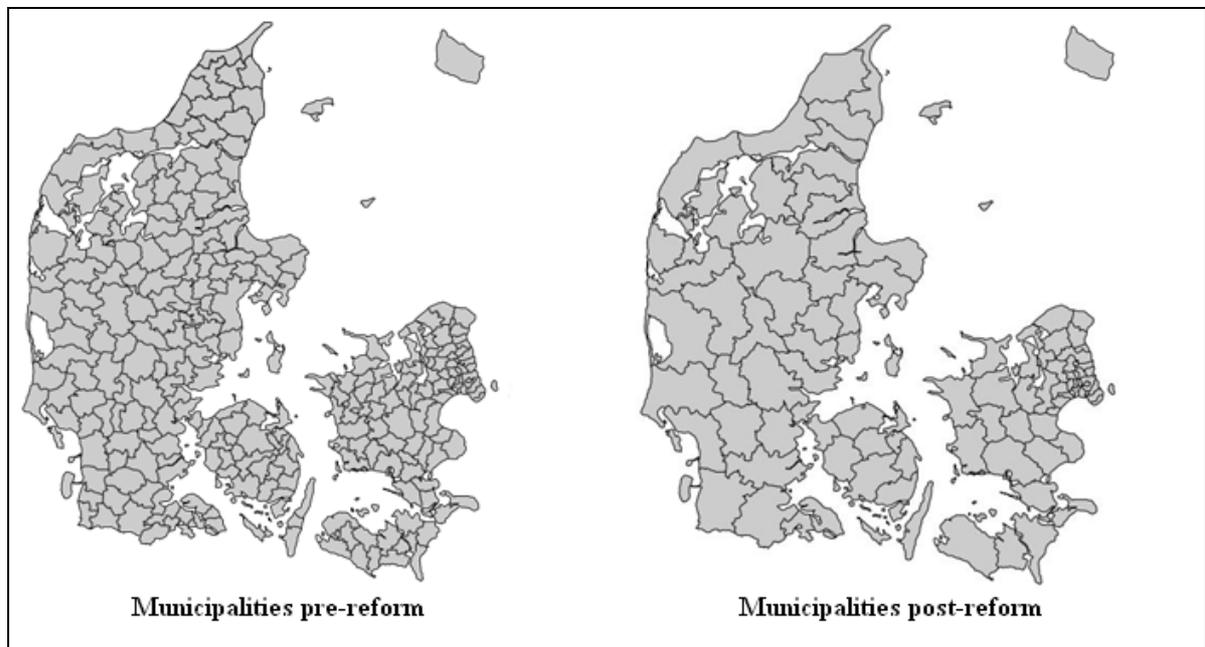
Figure 2
Survey results for the evaluation of the business relationship between firms and local governments

This chart illustrates the main results of the survey analysis, distinguishing between connected firms (firms connected with winning candidates in 2005 elections) and non-connected firms. For each question it was possible to respond on a Likert-scale from 1 (highest) to 4 (lowest), and 5 for "don't know; don't want to respond". Each column represents the fraction of firms that responded the first and the second highest answers to each question. The first question asks respondents to assess the quality of the business relationship between the firm and the municipality. The second question asks to evaluate the satisfaction with the municipality after the implementation of the administrative reform in 2005. The third, fourth and fifth questions ask about the importance of a firm's own network, a directors' network and a family network in determining the quality of the business relationship with the municipality. The remaining two columns report firms' answers about the importance of certain areas of collaboration with the municipality. In particular, the sixth column shows the relevance of the cooperation around environmental control and green accounts; the seventh column shows the relevance of the cooperation around construction, expansion and moving activities carried out by the firm.
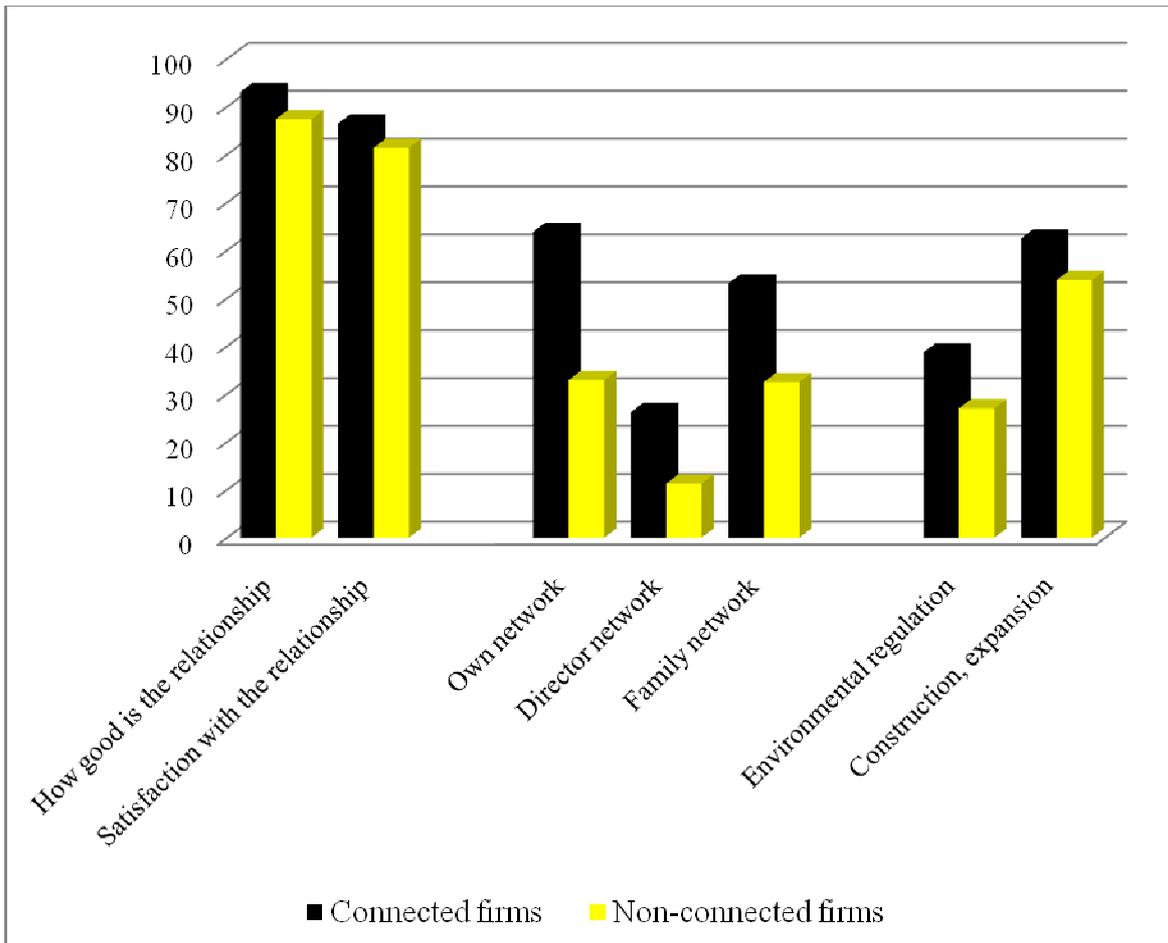
Table 1
Danish municipalities before and after the administrative reform

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 per inhabitant mean levels of number of elected politicians; total expenditures in DKK; and, outsourcing in DKK by treatment and control municipalities. "Before" refers to figures in 2004 and elected politicians in the 2001 elections. "Post" refers to municipality budgets in 2007 (population in 2005) and the number of elected politicians in the 2005 elections. Data source: Denmark Statistics. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Panel A. Number of municipalities

	Before	After
Total	271	98
Treatment municipalities	238	65
Control municipalities	33	33

Panel B. 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	1798.7	1021.8***
	Control	2323	2344	21
Expenditures/politicians	**Treatment**	30066.6	88474.2	58407.6***
	Control	106093.9	122154.4	16060.5
Outsourcing/politicians	**Treatment**	2879.6	8078.3	5197.7***
	Control	9515.2	10352.3	837***

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. The fraction of elected candidates to the total number of candidates is reported in squared brackets. Panel B shows the number of politically connected firms in the 2005 elections. The fraction of firms connected with elected candidates to the total number of firms connected to running candidates is reported in squared brackets. Panel C shows the number of firms connected with politicians re-elected in 2005, in control and treatment municipalities. The fraction of firms connected with re-elected candidates to total number of firms connected to running candidates is reported in squared brackets.

Panel A. Electoral results in 2005 elections

	Total	Treatment	Control
All candidates	11341	8375	2966
Winning candidates	2502 [22%]	1852 [22.1%]	650 [21.9%]

Panel B. Firms connected with politicians in 2005 elections

	Total	Treatment	Control
All candidates	1622	1196	426
Winning candidates	575 [35.5%]	426 [35.6%]	149 [35%]

Panel C. Firms connected with politicians re-elected in 2005 elections

	Total	Treatment	Control
	383 [23.6%]	290 [24.2%]	93 [21.8%]

33

Table 3
Summary statistics for years prior to elections

This table reports summary statistics for the three years prior to 2005 elections. Column (1) refers to non-connected firms. Columns (2)-(6) refer to firms connected with all candidates. Columns (7)-(10) refer to the sample used in the identification strategy, formed by firms connected with politicians re-elected in 2005. Logarithm of assets is the natural logarithm of the book value of total assets. 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 (2-digit industry dummies). Ln sales and Ln employees are respectively the natural logarithm of net sales and employees. Firm-clustered standard errors are reported in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% respectively. The number of firms for each year is reported in squared brackets.

| | Non connected firms | All candidates in 2005 elections | | | | | Politicians re-elected in 2005 elections | | | |
		Connected firms	Connected firms in treatment municipalities	Connected firms in control municipalities	Difference (2)-(1)	Difference (3)-(4)	Connected firms	Connected firms in treatment municipalities	Connected firms in control municipalities	Difference (8)-(9)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln assets	8.1899	9.047	9.038	9.072	0.857***	-0.034	9.517	9.537	9.455	0.082
	(0.008)	(0.054)	(0.063)	(0.114)	(0.054)	(0.129)	(0.118)	(0.126)	(0.293)	(0.317)
	[52084]	[1619]	[1193]	[426]			[382]	[290]	[92]	
OROA	0.042	0.035	0.039	0.024	-0.007***	0.015**	0.021	0.025	0.011	0.014
	(0.001)	(0.003)	(0.003)	(0.006)	(0.003)	(0.006)	(0.007)	(0.007)	(0.016)	(0.017)
	[51374]	[1603]	[1181]	[422]			[382]	[290]	[92]	
Ind. adj.OROA	0.006	0.001	0.003	-0.007	-0.006**	0.011*	-0.009	-0.006	-0.018	0.012
	(0.001)	(0.003)	(0.003)	(0.005)	(0.003)	(0.006)	(0.007)	(0.007)	(0.016)	(0.017)
	[51374]	[1603]	[1181]	[422]			[382]	[290]	[92]	
Ln sales	7.724	8.575	8.613	8.472	0.851***	0.142	8.887	8.852	9.0009	-0.156
	(0.019)	(0.096)	(0.1122)	(0.187)	(0.098)	(0.218)	(0.184)	(0.198)	(0.455)	(0.494)
	[15711]	[696]	[509]	[187]			[196]	[153]	[43]	
Ln employees	1.800	2.298	2.3378	2.179	0.498***	0.158	2.573	2.519	2.729	-0.210
	(0.008)	(0.049)	(0.056)	(0.106)	(0.050)	(0.120)	(0.119)	(0.128)	(0.279)	(0.305)
	[25595]	[894]	[672]	[222]			[209]	[154]	[55]	

Table 4
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 group. The dependent variable is the change in unadjusted OROA around the 2005 elections (three years after - three years before, excluding the election year) in columns (1)-(4), and the change in industry-adjusted OROA in columns (5)-(8). The industry adjustment in columns (5) and (7) is computed as the firm's OROA minus the median OROA of the relevant industry at 2-digit classification, whereas in columns (6) and (8) it is made by subtracting from the firm OROA the mean OROA of the relevant industry at 2-digit classification. Explanatory variables are a treatment dummy equal to 1 for firms connected with politicians re-elected in municipalities touched by the reform and 0 for firms connected with politicians re-elected in control municipalities, lagged logarithm of assets and profitability, the position of the politician in the electoral list (computed as Ln (1+position in the electoral list)), logarithm of age of the connected politician at the year of 2005 elections, and the outsourcing ratio of the municipality where the political connection is located (constructed as post-reform level, by dividing the sum of expenses referred to contractors and other services by total expenditures). Standard errors are reported in parenthesis. Columns (1)-(6) report robust standard errors, whereas in columns (7)-(8) standard errors are clustered at the municipality level, using the new municipality classification. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable:	OROA				Industry-adjusted OROA		Industry-adjusted OROA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.0409**	0.0419***	0.0391**	0.0433**	0.0421***	0.0414**	0.0421**	0.0414**
	(0.0161)	(0.0160)	(0.0186)	(0.0195)	(0.0160)	(0.0161)	(0.0190)	(0.0183)
Ln assets		0.0024	0.0015	0.0014	0.0023	0.0022	0.0023	0.0022
		(0.0025)	(0.0028)	(0.0027)	(0.0024)	(0.0025)	(0.0026)	(0.0026)
Profitability$_{t-1}$		-0.1391	-0.1077	-0.1209	-0.1353	-0.1327	-0.1353	-0.1327
		(0.1040)	(0.1099)	(0.1098)	(0.1067)	(0.1068)	(0.1118)	(0.1120)
Ln age				0.0397				
				(0.0488)				
Position in electoral list				0.0002				
				(0.0101)				
Outsourcing				0.0029				
				(0.0019)				
	Robust standard errors						Clustered standard errors	
Industry dummies	No	No	Yes	Yes	No	No	No	No
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	381	381	381	379	381	381	381	381

Table 5
Falsification tests

Columns (1) and (2) report the results of OLS regressions using the sample of non-connected firms. Columns (3) and (4) report the results of OLS regressions using the sample of firms connected with non-elected 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 re-elected politicians in 2005, and 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, where the year $t = -3$ is excluded. The explanatory variables are in all regressions a treatment dummy equal to 1 for firms connected with politicians in municipalities touched by the reform and 0 for firms connected with politicians in control municipalities, lagged logarithm of assets and industry-adjusted OROA, and a set of regional dummies. Robust standard errors are reported in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable: industry-adjusted OROA						
	Non-connected firms		**Firms connected with non-elected candidates**		**Connected firms: pre-treatment period**	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.0002	-0.0001	-0.0087	-0.0059	0.0241	0.0233
	(0.0015)	(0.0014)	(0.0098)	(0.0091)	(0.0217)	(0.0188)
Ln assets		0.0006*		-0.0005		0.0017
		(0.0003)		(0.0016)		(0.0032)
Profitability$_{t-1}$		-0.3005***		-0.2107***		-0.4902***
		(0.0068)		(0.0560)		(0.0672)
Number of firms	50653	50653	1025	1025	373	373

Table 6
Matching and discontinuity estimates

This table shows the impact of the reform using alternative estimation techniques. In Columns (1) – (5) we report the average treatment effect on the treated (ATT) computed using matching estimators. The dependent variable is the change in industry-adjusted OROA around the 2005 elections. Firms in treatment and control municipalities are matched according to the following variables: lagged logarithm of assets and industry-adjusted performance, regional dummies, logarithm of politician's age and his position on the electoral list. In Column (1) the ATT is computed using one nearest-neighbor with replacement matching on the propensity score, which is estimated by running a probit regression where the dependent variable is the binary treatment and explanatory variables are the above-mentioned controls. In Column (2) we report the bias-adjusted difference, to account for differences in the propensity score of treatment firms and their nearest matches. In Column (3) we report estimates using bias-adjustment, matching on controls directly and re-matching on the propensity score. Column (4) reports the ATT computed using Radius matching; Column (5) reports the ATT using nearest-neighbor matching on the above-mentioned controls without replacement, therefore computing the ATT by only using the treatment firms that are closest to the control firms. Estimations are always restricted within the common support. In Column (6) and (7) we perform OLS regressions excluding the 25% smallest (largest) municipalities on the treatment (control) side. In Columns (8) and (9) we perform a regression discontinuity design using as running variable the distance, in terms of inhabitants in 2004, from the threshold at 20.000 inhabitants. In Columns (6) – (9) lagged logarithm of assets and industry-adjusted OROA, and a set of regional dummies are included. Standard errors are reported in parenthesis. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable: industry-adjusted OROA

	Nearest-neighbor	Nearest-neighbor (bias-adjusted)	Nearest-neighbor (bias-adjusted)	Radius	1-to-1 match	Excluding smallest municipalities	Excluding largest municipalities	RDD linear	RDD quadratic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.0389**	0.0287*	0.0315**	0.0229***	0.0450**	0.0535**	0.0307*	0.0427*	0.0569*
	(0.0173)	(0.0152)	(0.0148)	(0.0080)	(0.0181)	(0.0251)	(0.0181)	(0.0237)	(0.0340)
Number of firms	349	349	349	349	186	331	302	381	381

Table 7
Firm, industry and political characteristics

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 group. The dependent variable is the change in industry-adjusted OROA around the 2005 elections. The explanatory variables are a treatment dummy equal to 1 for firms connected with politicians in municipalities touched by the reform and 0 for firms connected with politicians in control municipalities, lagged logarithm of assets and industry-adjusted OROA, and a set of regional dummies. Nuclear-family connections (in Column 1) are defined as direct connections, and connections with spouse and sons. Industries with high (low) share of connected firms are those above (below) the median share of connected firms computed at 2-digit level. High (low) profitability industries are those above (below) the median OROA at 2-digit level in the whole sample, including non connected firms. Connections with powerful politicians include firms connected with politicians that got more than the median number of votes in their list in a given municipality. Connections with big parties include firms connected with politicians belonging to parties that got more than the median number of votes in a given municipality. Mayor affiliations include firms connected with politicians belonging to the same party/coalition of the mayor in a given municipality. Standard errors reported in parenthesis are clustered at the municipality level, using the new municipality classification. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable: industry-adjusted OROA

	Firm characteristics			Industry characteristics				Political characteristics		
	Nuclear connections	**Small firms**	**Large firms**	**Low share of connections**	**High share of connections**	**Low profitability**	**High profitability**	**Powerful politicians**	**Big parties**	**Mayor affiliations**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.0512**	0.0526*	0.0257**	0.0220	0.0460**	0.0434**	0.0351	0.0482**	0.0410*	0.0488**
	(0.0201)	(0.0281)	(0.0121)	(0.0239)	(0.0177)	(0.0217)	(0.0219)	(0.0211)	(0.0213)	(0.0214)
Ln assets	0.0024	-0.0001	-0.0074***	-0.0069**	-0.0036	0.0039	0.0015	0.0007	0.0040	0.0053
	(0.0028)	(0.0104)	(0.0024)	(0.0034)	(0.0036)	(0.0026)	(0.0046)	(0.0034)	(0.0030)	(0.0045)
Profitability$_{t-1}$	-0.1595	-0.1528	-0.0520	-0.1503	-0.1594	-0.2833**	-0.0431	-0.0528	-0.0839	-0.2291*
	(0.1203)	(0.1380)	(0.0779)	(0.1909)	(0.1202)	(0.1413)	(0.1640)	(0.1629)	(0.1382)	(0.1176)
Number of firms	328	189	192	142	239	188	193	251	292	178

Table 8
Impact on alternative dependent variables

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 group. The dependent variables are changes around the 2005 elections of the following variables: Net income to assets (Column 1); ROCE, computed as the ratio of operating income to the sum of book value of equity plus book value of debt (Column 2); cash holdings, computed as the ratio of liquid assets to total assets (Column 3); logarithm of total assets (Column 4). The explanatory variables are a treatment dummy equal to 1 for firms connected with politicians in municipalities touched by the reform and 0 for firms connected with politicians in control municipalities, lagged logarithm of assets and industry-adjusted OROA, and a set of 1-digit industry and regional dummies. Standard errors reported in parenthesis are clustered at the municipality level, using the new municipality classification. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable:	Net income to assets	ROCE	Cash holdings	Total assets
	(1)	(2)	(3)	(4)
Treatment	0.0496**	0.0313*	0.0421**	0.2595*
	(0.0206)	(0.0169)	(0.0184)	(0.1346)
Ln assets	0.0039	-0.0116***	-0.0007	0.0565**
	(0.0042)	(0.0033)	(0.0042)	(0.0215)
Profitability$_{t-1}$	-0.0427	-0.3206***	0.0429	0.3469
	(0.0885)	(0.0765)	(0.0850)	(0.3686)
Number of firms	364	172	338	381

Table 9
Market share, merging size and contracting

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 group. The dependent variables are changes around the 2005 elections of market shares in Columns (1) - (3) and OROA in Columns (4) - (7). Market share is computed as firm's sales over total industry sales, defining firm's industry as its principal operating industry at 3-digits level and excluding firms with unreported industry classification. The explanatory variables are a treatment dummy equal to 1 for firms connected with politicians in municipalities touched by the reform and 0 for firms connected with politicians in control municipalities, lagged logarithm of assets and industry-adjusted OROA, and a set of regional dummies. The sample used in Column (2) consists of firms located in control municipalities and in treatment municipalities where the merging involved three or fewer municipalities (the median aggregation among treated municipalities), whereas Column (3) considers the mergers that involved more than three mergers. In Columns (4) and (5) we analyze the impact of treatment on OROA separately in municipalities above and below the median outsourcing ratio after-reform. In Column (6) and (7) we consider firms operating in industries above and below the median fraction of output sold to the public sector relative to total output. The sectoral dependence is computed at 2-digit industry level using the Input-Output matrix issued by Denmark Statistics in 2006. Standard errors reported in parenthesis are clustered at the municipality level, using the new municipality classification. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable:	Market share			Industry adjusted OROA			
	All mergers	Small mergers	Large mergers	High outsourcing	Low outsourcing	High sectoral dependence	Low sectoral dependence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.0196*	0.0208*	0.0250*	0.0424**	0.0330	0.0551**	0.0079
	(0.0113)	(0.0112)	(0.0135)	(0.0201)	(0.0244)	(0.0215)	(0.0139)
Ln assets	-0.0046**	-0.0055**	-0.0076***	0.0021	0.0036	0.0039	-0.1254
	(0.0020)	(0.0027)	(0.0027)	(0.0030)	(0.0039)	(0.1328)	(0.1284)
Profitability$_{t-1}$	0.0055	-0.0084	0.0087	-0.1131	0.0015	0.0016	0.0043
	(0.0251)	(0.0195)	(0.0309)	(0.1284)	(0.1277)	(0.0030)	(0.0029)
Number of firms	188	123	107	251	222	250	223

Table 10
Impact on capital structure

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 group. The dependent variables are changes around the 2005 elections of the following variables: Total debt over total assets in (Column 1); short-term debt over total assets (Column 2); Long-term debt over total assets (Column 3); long-term debt over total debt (Column 4). The explanatory variables are a treatment dummy equal to 1 for firms connected with politicians in municipalities affected by the reform and 0 for firms connected with politicians in control municipalities, Logarithm of assets, ratio between fixed assets, lagged total assets and industry-adjusted OROA, and a set of industry and regional dummies. Standard errors in parenthesis are clustered at the municipality level, using the new municipality classification. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable:	Total debt/ total assets	Short-term debt/ total assets	Long-term debt/ total assets	Long-term debt/ total debt
	(1)	(2)	(3)	(4)
Treatment	0.0050	-0.0071	-0.0305	-0.0130
	(0.0275)	(0.0252)	(0.0242)	(0.0393)
Ln assets	0.0229***	0.0025	0.0120**	0.0101
	(0.0064)	(0.0039)	(0.0053)	(0.0068)
Fixed assets/total assets	0.0794	0.0165	0.1512	0.1651
	(0.1156)	(0.0695)	(0.1181)	(0.1514)
Profitability$_{t-1}$	0.2195	0.1101	-0.2266**	-0.1893*
	(0.2401)	(0.1713)	(0.1119)	(0.1088)
Number of firms	170	325	173	173