COPENHAGEN BUSINESS SCHOOL
HANDELSHØJSKOLEN
SOLBJERG PLADS 3
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ISSN 0906-6934

Print ISBN: 978-87-92842-66-4
Online ISBN: 978-87-92842-67-1

Essays on Empirical Corporate Finance

PhD Series 20.2012

Copenhagen Business School
HANDELSHØJSKOLEN

Essays on Empirical Corporate Finance

Mario Daniele Amore

The PhD School of Economics and Management

PhD Series 20.2012

ESSAYS ON EMPIRICAL CORPORATE FINANCE

Mario Daniele Amore

March 2012

PhD Thesis

Department of Economics

Copenhagen Business School

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1st edition 2012
PhD Series 20.2012

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"The Doctoral School of Economics and Management is an active national and international research environment at CBS for research degree students who deal with economics and management at business, industry and country level in a theoretical and empirical manner".

Contents

English summary

The effect of corporate governance and managers on the value of companies has received great attention in the recent public debate. In the academic research, this increased attention has been associated with an effort to develop finer conceptual frameworks and analytical techniques to assess how governance and financial characteristics influence corporate policies and profitability.

While theoretical models represent a successful approach under specific hypotheses, the econometric analysis of corporate governance and managerial characteristics has proven to be extremely challenging. Because governance and managerial characteristics are equilibrium outcomes largely determined by the firm itself, it is methodologically difficult to separate out their determinants from their consequences to infer causal effects. Since its infancy the empirical corporate governance and corporate finance research has faced this problem, which is often responsible for mixed empirical results.

In my dissertation, I adopt a common methodological framework developed in the "program evaluation" literature to shed new light on the effects of governance and managerial characteristics on a variety of corporate policies and, ultimately, firm performance. In particular, I estimate a class of difference-in-differences models deriving the empirical identifications from policy changes that generate "quasi-natural experiments".

The first chapter of my dissertation analyzes the corporate value of political connections between managers and the political sector. Connections with politicians are commonly seen as a valuable asset for companies in corrupt institutional environments. I contribute to this stream of research by analyzing politically connected firms in Denmark, which is typically considered as one of the most transparent countries in the world. Moreover, I offer a new empirical identification based on a difference-in-differences model which exploits the passage of an administrative reform to generate exogenous variations in the decisional power of local politicians connected

with firms through family ties. My findings indicate that political networking is a powerful business strategy even in a non-corrupt environment: firms family-connected with politicians that became more powerful following the administrative reform systematically outperform firms connected with politicians that did not experience any change. Furthermore, I show that the mechanism that generates this corporate performance is partly related to doing more business with the public sector.

The second chapter of my dissertation examines how corporate governance shapes the competitive ability of firms. When competition acts as a disciplining mechanism, as argued by a large strand of governance research, we should expect that, in equilibrium, competition mitigates the negative effects of worse corporate governance. Yet, if worse governance entails managerial slack then, at least in the short term, worse-governed firms may suffer from the inability to timely respond to a sudden increase in competition. My empirical analysis provides strong support for this notion: U.S. firms that are exogenously endowed with worse governance are more vulnerable to a subsequent increase in competitive pressures, as induced by the passage of the U.S. – Canada Free Trade Agreement (CUSFTA) in 1989. Furthermore, I show that one of the channels driving the effect is the increase in financial constraints for worse-governed firms, which may endanger firms' ability to adapt to the new environment and expose them to predatory actions by the competitors.

Although financial constraints influence a broad array of corporate outcomes, innovation expenses have long been considered as one of the investment policies most susceptible to changes in the availability of financial resources. The third chapter of my dissertation contributes to this literature by examining the effect of a wider access to external finance on firms' innovative performance. My empirical approach exploits the passage of banking deregulations in the U.S. during the 1970s and 1980s, which increased the supply of credit for bank-dependent firms, improved the quality of financial intermediation, and provided banks with an opportunity to geographically diversify credit risk. My main result shows that U.S. firms exposed to the deregulation of banking activities across states increased significantly the quantity and quality of

their innovation activities, as measured by patent-based metrics. In exploring the channels behind this effect, I provide evidence that the effect is partly driven by a better diversification of deregulated banks. Moreover, I show that the effect is driven by an increase in innovation inputs associated with a relaxation of financial constraints. Finally, I find that the positive effect of banking deregulations on innovation is not influenced by simultaneous policies that affected the quality of corporate governance.

Dansk resumé

Effekten af virksomhedsledelse og -ledere i forhold til virksomheders værdi er blevet genstand for stor bevågenhed i nyere offentlig debat. Inden for akademisk forskning har denne bevågenhed været knyttet til bestræbelserne på at udvikle et mere udbygget begrebsapparat og analytiske metoder til at vurdere, hvordan ledelse og finansielle karakteristika influerer virksomhedspolitik og -rentabilitet.

Mens teoretiske modeller udgør en udbytterig metode i forhold til specifikke hypoteser, har den økonometriske analyse af virksomhedsledelse og ledelseskarakteristika vist sig at være yderst udfordrende. Idet ledelse og ledelseskarakteristika er resultat af ligevægt, som hovedsageligt er bestemt af firmaet selv, er det metodisk vanskeligt at afgrænse deres determinanter fra deres konsekvenser for at kunne udlede kausale virkninger. Siden dets fremkost har den empiriske forskning i virksomhedsledelse og virksomhedsfinansiering stået over for dette problem, som ofte har været årsag til sammenblandede empiriske resultater.

I min afhandling anvender jeg et alment metodologisk apparat, der er udviklet inden for programevaluering, for at kaste nyt lys over effekterne af styreformer og ledelsesegenskaber i forskellige virksomhedspolitikker og i sidste instans virksomheders præstation. Især estimerer jeg en type ”difference-in-differences” modeller, som udleder de empiriske kendetegn for policyændringer, der generer ”quasi-naturlige eksperimenter”.

Det første kapitel af min afhandling analyserer virksomhedernes værdi af politiske forbindelser mellem ledere og den politiske sektor. Forbindelser med politikere er ofte set som et værdifuldt aktiv for virksomheder i korrupte institutionelle omgivelser. Jeg bidrager til denne forskningsgren ved at analysere politisk forbundne erhvervsvirksomheder i Danmark, der typisk anses som et af de mest åbne lande i verden. Ydermere fremsætter jeg en ny empirisk identifikation baseret på en ’difference-in-differences’-model, som benytter vedtagelsen af en administrativ reform, der sigter mod at generere eksogen variation i beslutningskraften hos lokale

politikere, der er forbundne til firmaer gennem familie. Mine fund indikerer, at politisk networking er en slagkraftig virksomhedsstrategi selv i et ikke-korrupt miljø; firmaer, der er forbundet gennem familie med politikere, der fik mere magt som følge af den administrative reform, klarer sig systematisk bedre end firmaer, som er forbundet med politikere, der ikke oplevede nogen ændring. Derudover viser jeg, at den mekanisme, der generer denne virksomhedspræstation, delvist er forbundet med at gøre flere forretninger med den offentlige sektor.

Andet kapitel af min afhandling undersøger, hvordan virksomhedsledelse former virksomheders konkurrenceevne. Når konkurrence fungerer som mekanisme for disciplin, som en lang streng af ledelsesforskning argumenterer for, bør vi forvente, at konkurrence i ligevægtssituationen mindsker negative effekter af dårligere virksomhedsledelse. Men hvis dårligere styring fører til træg styring, i det mindste på den korte bane, vil dårligt ledede virksomheder lide under en manglende evne til rettidigt at handle på en pludselig konkurrenceøgning. Min empiriske analyse underbygger denne opfattelse: Amerikanske firmaer, der er eksogent udstyret med dårligere styring er mere sårbare over for efterfølgende forøgelse af konkurrencepres, foranlediget af vedtagelsen af ”the U.S. – Canada Free Trade Agreement” (CUSTFA) i 1989. Derudover viser jeg, at en af de faktorer, der styrer effekten, er forøgelse af dårligt ledede virksomheders økonomiske begrænsninger, hvilket kan true virksomheders evne til at tilpasse de nye forudsætninger samt eksponere dem for destruktive adfærd fra deres konkurrenters side.

Selvom finansielle begrænsninger har indflydelse på en lang række af virksomhedens resultater, har udgifter til innovation længe været anset som en af de investeringspolitikker, der er mest sårbare i forhold til ændringer i tilgængeligheden af økonomiske ressourcer. Det tredje kapitel i min afhandling bidrager til denne forskning ved at undersøge effekterne af en bredere adgang til ekstern finansiering af virksomheders innovative performance. Min empiriske tilgang benytter vedtagelsen af bankderegulering i USA gennem 1970erne og 1980erne, der øgede udbuddet af kredit for bankafhængige virksomheder, forbedrede kvaliteten af finansiel formidling samt udstyrede banker med en mulighed for at sprede kreditrisici geografisk. Mit hovedresultat viser, at amerikanske virksomheder, der er udsat for deregulering af bankaktiviteter på tværs af delstater, øgede kvaliteten og kvantiteten af deres aktiviteter inden for innovation signifikant målt ved patentbaserede metrik. I udforskningen af de bagvedliggende kanaler til denne effekt, beviser jeg, at effekten delvist er drevet af en bedre spredning af deregulerede banker. Ydermere viser jeg, at effekten er drevet af en øgning i innovationstilførsel i forbindelse med en lempelse af finansielle begrænsninger. Endelig finder jeg, at den positive effekt af bankdereguleringer vedrørende innovation ikke bliver influeret af samtidige politikker, der påvirker kvaliteten af virksomhedsledelse.

Introduction

The effect of managers and corporate governance mechanisms on the value of companies has received great attention in the recent public debate. In the academic research, this increased attention has been associated with an effort to develop finer conceptual frameworks and analytical techniques apt to assess how governance and financial characteristics influence corporate policies and, ultimately, the performance of firms.

While theoretical models represent a successful approach under specific hypotheses, the econometric analysis of governance and managerial characteristics has proven to be extremely challenging. The problem arises because governance and managerial characteristics are equilibrium outcomes largely determined by the firm itself, and that makes methodologically difficult to separate out their determinants from their consequences in order to infer causal relationships. A positive association, for instance, between observed measures of profitability and corporate governance could indicate that good governance improves firm performance. However, scholars have acknowledged that such inference is typically plagued by two problems. First, companies may adopt effective governance mechanisms in response to good performance, in which case, corporate governance is not the determinant but rather the consequence of firm performance. Second, the quality of corporate governance may be correlated with factors (e.g. CEO's preferences) that are not observed by the researcher but that directly affect firm policies; in this case, one would wrongly attribute the effect of such omitted factors to corporate governance. These two problems represent the most common cases of *endogeneity*, either in the form of reverse causality or in the form of omitted factor bias.

Since its infancy, the empirical corporate governance and corporate finance research has faced these methodological problems, which are often responsible for mixed empirical results.[1] Roberts and Whited recently considered endogeneity as: "the most important and pervasive issue confronting studies in empirical corporate finance".[2] Yet, the importance of establishing causal effect in corporate governance goes beyond the academic research agenda and entails a practical relevance for e.g. investors interested in identifying firm characteristics that yield high returns, and policy-makers that need to assess and compare the effectiveness of different policy interventions.

In my dissertation, I adopt a common methodological framework developed in the "program evaluation" literature to shed new light on the effects of governance and managerial characteristics on a variety of corporate policies and, ultimately, on firm performance. In particular, I estimate a class of difference-in-differences models deriving the empirical identifications from policy changes that generate "quasi-natural experiments". This approach has recently emerged as an effective way to handle the problems plaguing the empirical research in corporate governance and corporate finance. For example, Adams et al. (2010) write that "empirical work will need to continue to devise ways of dealing with the joint endogeneity issue", and explicitly refers to natural experiments generated by legislative changes as an approach that can help in solving this issue.[3] The main advantage of this approach is that, under a relatively small set of assumptions, it permits to control for major confounding effects such as common trends and omitted factors, thereby helping to identify causal relationships.

The first chapter of my dissertation analyzes the corporate value of political connections between managers and the political sector. Connections with politicians are commonly seen as a valuable asset for companies. However, existing research has estimated the value of political connections primarily in corrupt institutional environments, such as Indonesia and Thailand, where these benefits are expected to be

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[1] See e.g. Demsetz and Lehn (1985), Morck et al. (1998) and Demsetz and Villalonga (2001) on the relationship between ownership structures and corporate performance.

[2] Roberts and Whited (2011, pg.1)

[3] Adams et al. (2010, pg. 98)

the largest.[4] By contrast, I concentrate the analysis on Denmark, which the Corruption Perception Index (CPI) ranks as one of the most transparent countries in the world.[5] My empirical analysis is based on a difference-in-differences model which exploits the passage of an administrative reform in Denmark to generate exogenous variations in the decisional power of local politicians that are connected with firms through family ties. My findings indicate that political networking is a powerful business strategy even in a non-corrupt environment: firms family-connected with politicians that became more powerful following the administrative reform systematically outperform firms connected with politicians that did not experience any change. Furthermore, I show that the increase in corporate performance partly arises from doing more business with the public sector, whereas the relaxation of connected firms' financial constraints, documented by previous works in emerging economies (e.g. Claessens et al. 2008), does not play a significant role.

The second chapter of my dissertation examines how corporate governance shapes the competitive ability of firms. When competition acts as a disciplining mechanism, as argued by a large strand of research, we should expect that in equilibrium competition mitigates the negative effects of worse corporate governance. Yet, if worse governance entails managerial slack then, at least in the short term, worse-governed firms may suffer from the inability to timely respond to a sudden increase in competition. My analysis provides strong support for this notion: U.S. firms that are exogenously endowed with worse governance are more vulnerable to a subsequent increase in competitive pressures, as induced by the passage of the U.S. – Canada Free Trade Agreement (CUSFTA) in 1989. Furthermore, my analysis shows that one of the channels driving the effect is the increase in financial constraints for worse-governed firms, which may endanger firms' ability to adapt to the new environment and expose them to predatory actions by the competitors.

[4] See, in particular, Fisman (2001) and Bunkanwanicha and Wiwattanakantang (2009).

[5] The CPI is a ranking of perceived corruption assembled published by Transparency International, a non-governmental organization. Denmark ranked second in 2009, first in 2010, and second in 2011 (source: http://www.transparency.org/policy_research/surveys_indices/cpi).

Although financial constraints influence a broad array of corporate outcomes, innovation expenses have long been considered as one of the investments most susceptible to changes in the availability of financial resources. Starting from this notion, a recent literature has focused on the relationship between finance and innovation (e.g. Atanassov et al. 2007; Bernstein 2011; Brown et al. 2009; Benfratello et al. 2008). In the third chapter of my dissertation, I contribute to this literature by examining the effect of a wider access to external finance on firms' innovative performance. My empirical approach exploits the passage of banking deregulations in the U.S. during the 1970s and 1980s, which increased the supply of credit for bank-dependent firms, improved the quality of financial intermediation, and provided banks with better opportunities to geographically diversify their credit risk. My main result shows that U.S. firms exposed to the deregulation of banking activities across states increased significantly the quantity and quality of their innovation activities, as measured by patent-based metrics. In exploring the channels behind this effect, I provide evidence suggesting that the effect is partly driven by a better diversification of deregulated banks, which encouraged lending to riskier companies. Moreover, I show that the effect is driven by an increase in innovation inputs associated with a relaxation of financial constraints. Finally, I find that the positive effect of banking deregulations on innovation is not influenced by simultaneous policies that affected the quality of corporate governance.

To conclude, the overall contribution of my dissertation is threefold. First, it links three governance and financial characteristics to firms' success, measured using indicators of accounting profitability, market value, and innovative performance. Second, it provides a thorough methodological assessment of the causality relationships using quasi-natural experiments derived from policy changes. Third, it explores the specific mechanisms that generate the performance effects; in particular, the provision of services to the public sector (first chapter) and the changes in financial constraints (second and third chapters).

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Political power and blood-related firm performance

Mario Daniele Amore†
Copenhagen Business School

Morten Bennedsen‡
INSEAD

Abstract

Applying difference-in-differences, matching, discontinuity, and selection models, we use exogenous variations from an administrative reform to identify a positive causal effect of political power on the operating performance of firms that have blood-related ties to local politicians. An increase in power boosts blood-related firm revenues and proves especially valuable in service sectors and when local outsourcing increases, suggesting that the performance increase is related to doing more business with local governments. Focusing on connections between firms and local politicians in the world's least corrupt country, we conclude that political networking is a valuable business strategy even in settings where connections are expected to be least relevant.

JEL Classification: G34
Keywords: political connections, firm performance, family ties

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A previous draft of this paper circulated under the title "Political reforms and the causal impact of blood-related politicians in the world's least corrupt country." We are grateful to Yosef Bhatti and Lene Holm Pedersen for directing us to budget data for Danish municipalities, and to Maria Faccio, Denis Gromb, Rafael Lalive, Ethan Kaplan, Samuli Knûpfer, Gordon Phillips, Francisco Peréz González, Jesper Rangvid, Thomas Rønde and Alminas Zaldokas for useful comments and discussions. We thank participants at the Corporate Governance, Family Firms, and Economic Concentration Conference in Jerusalem (2011), Symposium on Finance, Banking and Insurance in Karlsruhe (2011), Econometrics Society World Congress in Shanghai (2010), EFA Conference in Frankfurt (2010), ESSEC-INSEAD-HEC-PSE Workshop in Financial Economics, Nordic Finance Network Workshop in Lund (2010), Japanese Development Bank Workshop in Tokyo, European Academy of Management in Rome (2010), INSEAD-Georgetown Workshop in Political Economy, Workshop in Corporate Governance and Investment in Barcelona (2010), and CIE Workshop in Industrial Economics (2010), as well as seminar participants at Copenhagen Business School, INSEAD, University of Salamanca, and University of Zurich. We also thank the Danish Social Science Research Foundation for financial support and Pernille Bang at Statistics Denmark for data help. All errors remain our own.

†Ph.D. candidate, Copenhagen Business School. Email: mda.eco@cbs.dk

‡André and Rosalie Hoffmann Chaired Professor of Family Enterprise and Professor of Economics, INSEAD. Email: morten.bennedsen@insead.edu

1. Introduction

We use an administrative reform that exogenously increased the size of a majority of municipalities in Denmark to identify the causal effect of political power on the performance of firms that have blood ties to local politicians. We find that, compared to a control group of firms connected with unchanged municipalities, blood-related firms prosper when the average power of local politicians increases. Furthermore, we provide supporting evidence that this effect is driven by an increase in business activities with the public sector.

A number of studies indicate that political connections have a large positive effect on firm performance in countries with weak institutions (Bunkanwanicha and Wiwattanakantang 2009; Cingano and Pinotti 2011; Fisman 2001; Johnson and Mitton 2003; Li et al. 2008). By contrast, evidence from developed countries is ambiguous. In the U.S., for example, Goldman et al. (2009a) and Jayachandran (2006) find that political connections have a positive and economically relevant value. However, Fisman et al. (2006) show that the effect of being connected with former Vice President Dick Cheney was zero. Acemoglu et al. (2010) argue that political connections may be beneficial but mainly in times of economic distress.

We use a natural experiment to estimate the effect of being connected with local politicians in Denmark, which the well-respected Corruption Perceptions Index (CPI) classifies as the *least corrupt* country over the last four years.[6] In other words, we investigate the value of political ties in an institutional environment where our prior assumption is that such ties are least valuable. Contrary to this expectation, our results support the notion that political networks are of great importance in all countries around the world and at all political levels.

We contribute to the literature in both providing new economic insight and introducing a novel identification strategy. First, we show how exogenous increases in political power affect connected firms. We provide evidence for a positive relationship

[6] The CPI, formulated by Transparency International, ranked Denmark 2nd in 2009, 1st in 2010, and 2nd in 2011.

between district size and connected firms' performance: our unconditional correlations show that doubling a district's population/expenditure/outsourcing improves connected firms' performance by 105/77/80 percent. This picture is consistent with the notion that centralization leads to more rent-seeking (Fisman and Gatti 2002) and lower internal political efficacy (Dreyer Lassen and Serritzlew 2011). Thus, we build a bridge between research on the corporate value of political connections and the vast literature that investigates how the design of political institutions shapes politicians' rent-seeking behavior.[7] Our analysis also complements a larger discussion of how government spending affects firm activities (Cohen et al. 2011).

Second, we propose a novel identification strategy to estimate the causal impact of political connection on firms' operating returns.[8] While event studies have been used to assess the stock price impact of political connections (e.g. Fisman 2001; Faccio and Parsley 2009), identifying the effect on accounting measures of performance has proven to be extremely difficult due to the challenge in choosing appropriate counterfactuals. Our identification exploits exogenous variations in the size of local governments for given connections between firms and politicians. These variations come from the implementation of an administrative reform that took place in Denmark in 2006, whereby 238 municipalities merged into 65 new ones and 33 municipalities were left unchanged. Using a difference-in-differences model, we test how the *enlargement* of local governments affected the profitability of firms connected with local politicians *before* and *after* the reform, using as counterfactuals firms connected *before* and *after* the reform in municipalities that did not change size.

Our identification builds on the notion that an increase in the size of local governments creates a 'positive shock' to politicians' power and thus to the amount of

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[7] See, for example, Fisman and Gatti (2002) on decentralization; Persson et al. (2003), Persson and Tabellini (2000), and Gagliarducci et al. (2011) on electoral rules; Ferraz and Finan (2011) on re-election incentives; Alt and Dreyer Lassen (2003) on the openness of political systems.

[8] Previous studies have relied on cross-sectional comparisons of connected and non-connected firms (Johnson and Mitton 2003; Li et al. 2008; Niessen and Ruenzi 2010), and panel data focusing on politicians or parties losing offices (Cingano and Pinotti 2010; Bunkanwanicha and Wiwattanakantang 2009; Goldman et al. 2009b).

rent that politically connected firms may potentially receive. We validate this notion by documenting that the ratios of population, governmental budget, and outsourced expenses to elected politicians increased significantly more in merging municipalities than in non-merging municipalities. Moreover, the reform in itself was backed up with DKK 1.2 billion to cover transitory expenses in merging municipalities only.[9] Overall, these figures indicate that politicians in treatment districts were endowed with more decisional power and financial resources.

Our approach 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 increased the power of some politicians but not others. Second, we can focus solely on connections with winning candidates in both the treatment and control group. Thus, we avoid potential endogeneity problems 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 affected by corporate outcomes.

Our estimates indicate that connections with local politicians in larger municipalities lead to an economically and statistically relevant increase in firm performance. On average, our benchmark specification reveals that the operating return on assets (OROA) nearly doubles. This effect is particularly pronounced for small firms, firms connected with more powerful politicians, and firms with low prior profitability.

Further validation of a causal relation is derived, as we do not find any significant effect for either non-connected firms or firms connected with politicians who ran for local offices but were not elected. Neither do we find any significant impact for a placebo increase in district size on firm performance prior to the reform year. A challenge to our identification is that the reform may have affected the selection of politicians elected in treatment municipalities in a way that is correlated with the

[9] The average DKK-USD exchange rate in 2006 was 0.1681 (source: www.statbank.dk).

transfer of rent to the connected firms. For example, if tougher electoral competition in the merging municipalities improves the quality of re-elected politicians, and high-quality politicians are less willing to provide rent to connected firms, then focusing on connections with politicians who were re-elected may undermine our results. To mitigate this concern, we adopt a selection model based on two different exclusion restrictions: the aggregate party vote, computed excluding the district where a given politician runs for re-election; and the share of politicians older than 65 years in the old municipalities that formed a given new municipality. The resulting estimates indicate that selection concerns did not significantly affect our findings.

We confirm our findings by employing alternative specifications. First, we use a matching strategy to address the possibility that the impact of the reform is heterogeneous with respect to observable corporate and political characteristics unbalanced across treatment and control municipalities. Second, we exploit a sharp discontinuity that was adopted to select which municipalities to merge. By comparing firms connected with municipalities barely above and below the qualifying threshold, we mitigate the concern that the merging group is characterized by declining economic or demographic performance and, thus, that firms connected with those municipalities are not fully comparable with firms connected with large unchanged municipalities.

We proceed to identify a channel through which companies benefit from connections. First, we show a larger increase in profitability for firms connected with municipalities that outsource a higher proportion of public services to private contractors. Second, we find that the effect is larger in industries that are more dependent on public demand. Third, some indication exists that firms connected with treatment municipalities increase their revenues relative to the industry level. Taken together, these findings suggest that political ties secure firms a larger share of outsourced local service provisions.

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 our identification strategy. In Section 4, we present empirical results. In Section 5, we discuss our findings and conclude.

2. Institutional background and data

2.1. Local governments and the 2005 administrative reform

Municipalities in Denmark are governed by local councils headed by a mayor, who is elected during the first meeting of the council by a simple majority. The mayor has the overall responsibility for the provision of public services in various sectors (in particular, primary and secondary education; elderly 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 approximately 48% of total public expenditure.[10]

Local councils have between 9 and 31 members, with the exception of Copenhagen, which has 55 seats. Municipalities with more than 20,000 inhabitants had a minimum of 19 seats before 2005, and a minimum of 25 seats after 2005, as an effect of the administrative reform. All councils have an odd number of seats. The election period is 4 years, and the elections take place the 3rd Tuesday in November. Every new local government starts working on January 1st. 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, cases may exist of local parties that run only in specific municipalities. The last local elections took place in 2001, 2005, and 2009.

The main input to our identification derives from a change in the geographic borders of Danish municipalities due to the administrative reform implemented with the 2005 elections.[11] Figure 1 maps the municipalities before and after the

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

[11] Counselors in the new municipalities were elected through the local elections in November 2005, but to ensure continued operation in the merging municipalities, the tenure of the previous councils was prolonged by one year, until the end of 2006. In this transitory period, old municipalities transferred administrative entities to the new municipalities and were fully dissolved on January 1, 2007. The newly

administrative reform; and Table 1, Panel A, describes how the reform reduced the number of municipalities.

Since the previous electoral reform in 1974, 271 municipalities had ranged from less than 5,000 to more than 400,000 inhabitants (the old municipalities are represented on the left side of Figure 1). Given the economic and administrative inefficiencies of having 205 municipalities with fewer than 20,000 inhabitants, the 2005 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 $159 km^2$ to $440 km^2$ 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 in Dreyer Lassen and Serritzlew (2010), 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[12], whereas 14 municipalities were split into two parts, with each merging into separate larger municipalities.

2.2. Election, corporate, and management data

We received from the Danish Ministry of the Interior electoral data containing the personal identification number (CPR number) of all candidates in the 2001 and 2005

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elected councils in municipalities not involved in a merger started their activities on January 2006. Five municipalities on the island of Bornholm merged into an island-wide municipality when the debate over the reform started, at the end of 2002; in the empirical analysis, we exclude the few firms connected with these municipalities.

[12] Two municipalities were allowed to stay independent because the ruling coalitions in the neighboring municipalities were of different political orientation, and a few poor municipalities had a hard time finding neighbors willing to merge.

local elections. For each candidate, the data contain information about party affiliation, number of votes received, and whether or not he or she was elected.

To construct our dataset of firms connected with local politicians, we combined a number of other data 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 Economics and Business Affairs. We consider companies with non-negative and non-missing book value of assets that are present in the sample for the period from 2002 to 2008. Unfortunately, the 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. By law, all balance sheets must be approved by external and independent accountants.

The Danish Ministry of Economics and Business Affairs also 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 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 at the Ministry of Interior provided 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 (number and dates of marriages, divorces, and widowhoods). By using this information, we create the family tree behind each top manager and director.

2.3. Politically connected firms

By merging the families behind the top management and directors with the election data, we can identify firms that are blood-related to politicians. By 'blood-related' (i.e. our definition of connection) 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).

Because we have election data for both the 2001 and 2005 local 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 *re-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 either of the two periods, and by firms that were connected in 2001 but not in 2005.

3. Empirical strategy and summary statistics

Our main goal is to measure how exogenous variations in the size of political districts affect the corporate value of political connections. For this purpose, we classify municipalities into 'treatment' municipalities—those that increased in size—and 'control' municipalities—those that did not. We focus only on re-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 political office affects the profitability of connected firms, using as a control group firms that were connected with politicians in non-merging municipalities. This methodology allows us to absorb any general impact of political connections, national elections, and changes in the business environment (e.g., macroeconomic shocks) on corporate outcomes.

The validity of our identification relies on two premises. First, the selection of merging municipalities is not driven by corporate outcomes. We have discussed 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.[13] Second, the increase in district size caused an increase in political rent. We provide evidence that this effect was created by a combination of fewer politicians, more tasks, and larger budgets. Table 1, Panel C, reports three measures of power attributes around the reform in both treatment and control municipalities. The number of inhabitants per elected politician more than doubled in treatment municipalities, whereas it remained unchanged in control municipalities. Expenditures per elected politician increased by a factor of three in treatment districts, whereas control districts only experienced marginal increases. Finally, we find an increase in outsourcing per elected politician that is nearly three times larger in treatment 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[14], and were to a large extent outsourced to private companies. Overall, these arguments provide strong evidence that the reform induced a greater increase in political power and potential for rent-seeking for the average politician in the treated municipalities than in the control municipalities.[15] While we cannot conclude that *every* politician in the treatment municipalities became more powerful, in our empirical analysis we allow for the possibility that this effect is only present among leading politicians or politicians belonging to the ruling coalition.

[13] We admit that corporate performance can affect migration across municipalities, but for almost all municipalities this concern is of a second order to the geographic and historical determinants and has not been, we claim, a decisive factor in any of the mergers between municipalities in Denmark.
[14] Source: "The Local Government Reform—In Brief," Ministry of the Interior and Health, Department of Economics, 2005.
[15] Research on political accountability has yielded some additional support for the argument that larger district size is associated with more potential for rent-seeking. Studies of electoral rules and fiscal federalism suggest that centralization and the creation of larger electoral municipalities might imply lower electoral accountability (Fisman and Gatti 2002). In the context of Denmark, Dreyer Lassen and Serritzlew (2010) document that larger districts had a sizeable detrimental effect on citizens' internal political efficacy, which in turn may have reduced their ability to hold politicians accountable.

Our identification assumes that the enlargement of local municipalities does not affect firm performance through channels other than the political connections. Although we cannot *a priori* rule out 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 shows that this effect is not the case; only connected firms benefit from the reform. Another challenge is that the reform may have affected the selection of politicians differently across treatment and control municipalities. For example, the extra-electoral competition induced by having in the merged municipalities a number of seats that is lower than the sum of the seats in the old municipalities may have raised the quality of re-elected politicians. If the quality of re-elected politicians is correlated with delivering rent to the connected firms, our estimates on performance may be biased.[16] To empirically cater to this challenge, we control for the selection in the pool of connections with blood-related politicians re-elected in 2005. We use two exclusion restrictions for the likelihood that a connected politician is re-elected: the aggregated number of votes for the party excluding the politician's own district; and the share of council members above 65 years of age before election in the council where a politician runs for re-election. In Section 4.3, we provide arguments for the validity of these exclusion restrictions.

Table 2, Panel A, illustrates our electoral data. In total, 11,341 individuals ran for office in the 2005 municipal elections. Among them, 8,375 (2.966) ran in treatment (control) municipalities. In total, 2,502 candidates were elected in the 98 municipalities, out of which 1,852 (650) candidates were elected in treatment (control) municipalities. While the ratio of the elected to all candidates is similar across the two groups, the ratios of re-elected to elected candidates in 2001 and to candidates running for reelection are lower in the treatment districts; so, too, are the ratio of candidates

[16] In the case of a positive correlation—i.e., better politicians provide more rents to the connected firms—our estimates will be upward biased because the increase in profitability arises from both the increase in power and the superior quality of politicians. By contrast, in the case of a negative correlation—i.e., better politicians are less willing to deliver rents to the connected firms, perhaps because they are more accountable to voters—our estimates will be downward biased.

running for re-election to candidates elected in 2001. Hence, the evidence in Panel A indicates the possibility of an increase in electoral competition in treatment districts and thus motivates the need to control for selection bias.

Table 2, Panel B, shows the same figures for politically connected firms. Overall, 1,964 firms were connected with candidates in the 2005 elections. The fraction of firms connected with elected candidates relative to all connections is approximately 38 %, with no significant differences across treatment and control groups. The last row of Table 2 describes the set of re-connected firms that we use for establishing the causal link between political connections and corporate performance. Of a total of 419 firms connected with politicians re-elected in 2005, 321 of them were connected in treatment municipalities, and 98, in control municipalities. The ratio of connections with re-elected candidates relative to all connections is approximately 21%, with no significant differences across treatment and control groups.

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 particularly useful, as they are typically used as proxies for the observed quality of politicians (e.g. Brollo et al. 2010; Ferraz and Finan 2011). The average candidate in 2005 is approximately 50 years old, has 13 years of schooling and labor income of 403,284 DKK (Panel A, Column 4). Winning candidates have similar ages and education levels, although they have a higher labor income (Panel B, Column 4). Focusing on changes in politicians' characteristics between 2001 and 2005, we notice that candidates (both the entire pool and the subsample of winners) in treatment municipalities became older, slightly more likely to be male, significantly more educated, and with a higher labor income (Columns 7). Similar changes are present, though they display lower significance, for candidates in control municipalities (Panels A and B, Column 8). Taking the difference between changes in the characteristics of candidates in treatment and control groups (Column 9) indicates that the average candidate in a treatment municipality became significantly more educated. This positive effect, however, becomes insignificant once we focus on winning candidates (Panel B, Column 9), whereas, among these candidates, we find a significant and positive effect increase in labor income. In sum, Table 3 highlights the importance of choosing winning politicians as counterfactuals to minimize the observable differences between firm-connected politicians in treatment and control groups. Further, the small observable differences indicate that unobservable differences may also exist; hence, Table 3 reconfirms the importance of controlling for selection.

Table 4 reports the average characteristics of connected and non-connected firms prior to the implementation of the administrative reform. Our main measure of corporate performance is OROA, computed as the ratio of earnings before interests and taxes (EBIT) to the book value of total assets. An important advantage of using OROA as a measure of operating returns is that, unlike net income-based measures of performance, OROA is unaffected by differences in firms' capital structure. To mitigate the effect of outliers, we drop 1% of observations on the right and left tails of the OROA distribution. 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 median OROA of each 4-digit industry, considering all active firms in our dataset, including those that are not politically connected. For each industry, we require that at least 20 firms exist in a given year; when this restriction is not satisfied at 4-digit, we move to 3-digit or 2-digit level.

In Table 4, Columns (1), (2), and (5), we look at the pre-reform differences between connected and non-connected firms. On average, connected firms are larger and perform less well than do non-connected firms, corroborating the cross-country evidence provided by Faccio (2009) and reconfirming the finding that non-connected firms are weak counterfactuals.[17] In Table 4, Columns (3), (4), and (6), we show that

[17] Examining the industry distribution (untabulated), 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. Further, we find only small differences in the industry distribution of connected firms in treatment and control municipalities: connected firms in treatment

the differences between treatment and control firms are much smaller both in economic and statistical terms, except for a marginal significance in OROA (Column 6). Even if these differences are smaller than those in Columns (1), (2), and (5), the evidence still raises concerns about omitted factor bias. Furthermore, 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 to which he or she is connected.

To circumvent such endogeneity issues, we focus the analysis on firms that are connected with politicians re-elected in 2005 (i.e., politicians who had a place on the municipal council both before and after the 2005 elections, in both treatment and control municipalities). Table 4, Columns (7) - (10), shows that no significant differences exist 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. Taken together with the argument that the selection of merging municipalities was not influenced by corporate characteristics, this lack of differences is strong confirmation of the validity of our counterfactuals.

4. Results

We begin by establishing the effect of increasing district size on blood-related firm performance, using OLS difference-in-differences estimates (Section 4.1). We then proceed to provide evidence for a causal effect by showing that the effect is only present for firms connected to winning politicians and that no significant differences in performance exist between treatment and control firms prior to the reform (Section 4.2). We control for selection concerns in Section 4.3, and provide matching and

municipalities are marginally more present in manufacturing, trade, and transport, whereas firms in control municipalities operate more in other business segments.

discontinuity estimations in Section 4.4. In Section 4.5, we provide evidence of how variations in political power affect connected firm performance, and in Section 4.6 we identify business activities with the public sector as the channel through which connected firms benefit from increased political power. Finally, we analyze firm variations and alternative outcomes in Section 4.7.

4.1. OLS difference-in-differences

Table 5 presents the results of OLS regressions, where the dependent variable is the change in firm profitability around 2005 (the local election year during which the administrative reform was implemented). We consider three years after and three years before, excluding the election year itself. The main variable of interest, called treatment, is a dummy 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 dependent variable and only controlling for regional localization to reduce the scope for omitted factor bias. We compute Huber-White robust standard errors. The treatment effect is 3.25 percentage points and is statistically significant at the 5% level. This result indicates that re-connected firms with merging municipalities experienced, on average, a 3.25 percentage-point improvement in OROA compared with re-connected firms with municipalities that did not change size. This impact becomes marginally higher when we control for lagged assets and operating performance (Columns 2 and 3). In Columns (4) to (6), we employ as dependent variable the change in industry-adjusted OROA around the election year. The results are very similar in size and significance to the unadjusted results, suggesting that our findings are not driven by different industry trends. As the treatment is defined at the municipality level, we allow for correlation of residuals within municipalities by clustering standard errors at the municipality level. We present these estimates in Column (7), using industry-adjusted OROA as the dependent variable. As shown, the treatment effect remains statistically significant at the 5% level.

On the basis of these estimates, we conclude that the increase in profitability is statistically significant and ranges between 3.1 and 3.4 percentage points. Given that the average OROA is 4.2% for all firms and 2.5% for firms connected with re-elected politicians (Table 4), the economic magnitude of such an increase is large. We will explore in more detail the relationship between political power and blood-related firm performance in Section 4.5. For now, we conclude that by increasing the power of politicians in treatment districts, the reform created significant benefits for blood-related companies.

4.2. Falsification and robustness tests

Our identification hinges crucially on the exogeneity of the administrative reform relative to corporate outcomes. However, two additional risks remain to the causal interpretation of our results. The first is whether the effect of enlarged municipalities improves the performance of all connected firms or even non-connected firms. This happens, for example, when a merger positively affects the demand for private services and other goods, or improves accounting standards by allocating more resources to the auditing process. Results in Table 6 help to rule out this interpretation. In Columns (1) and (2), we present results obtained using non-connected firms, whereas in Columns (3) and (4) we use firms that are connected with non-elected candidates. In both cases, we find that the treatment is not significant in either statistical or economic terms. The second issue is about the implicit assumption of parallel trends between treatment and control groups needed for the validity of the DD model. To underline the similarity of the two groups before the implementation of the reform, we propose a falsification test in Columns (5) and (6) that estimates DD regressions in a pre-treatment window centered at $t = -3$. The lack of statistical significance confirms that the two groups were similar before the 2005 elections and confirms, therefore, the validity of the parallel trends hypothesis in our setting.

We perform a number of further checks to assess the robustness of the estimates reported in Table 5. In computing the dependent variable, we have trimmed OROA to 1% on the right and left tails of the distribution to mitigate the concern of outliers. To confirm that our results are not driven by outliers, we further trim the dependent variable to 1% on the right and left tails of the distribution. Alternatively, we run a median regression (computing standard errors by bootstrap, using 500 replications), 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 (for which operating returns are typically an unreliable measure of performance), or firms connected with municipalities that were split into separate larger entities, given that for these firms the effect of a merger is ambiguous. All results from these tests (un-tabulated, but available upon request) are in line with our previous estimates (coefficients range between 1.5 and 4.3 percentage points depending on the specification adopted, and they are at least significant at a 10% level).

4.3. Controlling for selection

As discussed above, the increase in political competition induced by the reform might affect the quality of the re-elected politicians in merging districts in a way that correlates with the ability to transfer rent to the connected firms. In such cases, the effect estimated on the sample of firms connected with re-elected politicians would not only measure the benefits of an increase in political power but also the superior quality of re-elected politicians; and, our estimates would not be able to separate out these two channels. Even if Table 3 did not provide strong evidence for any major change in the observable characteristics of re-elected politicians after the reform, controlling for unobservable differences would nevertheless be worthwhile.

Table 7 reports the results when we use 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 but, at the same time, do not affect corporate performance in any other way than through the rent transferred to the firms. The first is the average number of votes that the politician's party has received in other municipalities, excluding the politician's own municipality. The idea here 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 can serve as common shock that affects all candidates' probability of re-election but does not affect the profitability of connected firms through channels different than the re-election of the connected politicians. The second is the number of elected politicians in 2001 in the same municipality who are older than 65 years in 2005. A higher incidence of old politicians suggests that fewer will stand for re-election; this condition increases the likelihood that a politician who runs for re-election is elected again. Again, we claim that the age distribution of the municipality council in 2001 is independent of the characteristics of a given connected firm.

Table 7, Panel A, provides estimates from the selection equation, in which the dependent variable is a dummy equal to 1 if a firm was connected with a re-elected politician, and the explanatory variables are the two exclusion restrictions (separately reported in Columns 1, 2 and 3, 4) with and without their interaction with the dummy, indicating whether the municipality was treated by the reform or not.[18] Consistent with the idea of tougher competition in districts that were merged by the reform, we observe that the treatment indicator has a negative sign. We also observe that the use of both variables as exclusion restrictions increases the likelihood that a connected politician is

[18] An alternative approach might be to estimate the re-election probabilities on the entire pool of politicians. This method, would, however, introduce another selection problem, concerning a politician's likelihood of political connectedness.

re-elected. However, a difference between the two selection models exists: the aggregate party vote has an impact primarily in the merging municipalities, whereas the age distribution works equally across merged and control districts.

Panel B presents the performance results obtained using both maximum likelihood (ML) and 2-step estimations. As is similar to our baseline results in Table 5, the ML estimates vary from 3.2 to 3.6 percentage points and are significant at a 5% level. The 2-step estimates vary more depending on the exclusion restriction used. Using the aggregate votes gives smaller and less significant results (around 2.7 percentage points, and significant at 10%), but using the age distribution yields results very similar to our baseline OLS estimates.

On the basis of these results, we conclude that controlling for selection of politicians does not alter the effect of an increase in political power on the performance of connected firms.

4.4. Matching and discontinuity estimates

We now investigate whether our findings are robust to the use of alternative estimation methods. We show results based on re-weighting and nearest-neighbor matching (Rosenbaum and Rubin 1983; Abadie and Imbens 2007). The benefit of these approaches is that we not only use re-connected firms with municipalities that do not change in size as counterfactuals, but also, for each firm in the treatment group, we find the most similar firm in the control group, discarding dissimilar observations. By minimizing the distance between the two groups, we reduce the bias induced by differences in observable firm and political characteristics that might be unbalanced across treatment and control groups.

The covariates included in the matching procedure are pre-treatment assets and industry-adjusted operating performance; regional localization; logarithm of age and gender 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 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 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.

We start by showing estimates after re-weighting observations on the basis of the propensity score. Table 8, Column (1), presents the results. The estimate is significant at the 5% level and marginally lower than the OLS estimates, confirming the robustness of our previous results. In Column (2), we match observations with replacement on the covariates directly. In Column (3), we match with replacement on the propensity score and rematch on the covariates, reporting the bias-adjusted results. Column (4) yields results from a 1-to-1 match without replacement. All the estimates are significant both in statistical and economic terms, and range between 2.9 and 3.3 percentage points.

A further concern about our identification approach is that the treatment group may be formed by municipalities with declining economic or demographic performance, and therefore, connected firms with those municipalities will not be fully comparable with firms connected with large unchanged municipalities. Although we have already proved that such potential differences are not reflected in a different profitability between treatment and control firms prior to the reform, we offer two additional 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 (5) and (6), 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 firms connected with municipalities above and below this threshold. As this variable is precisely measured and cannot be manipulated 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 parametrically estimate a linear specification, adding it to the usual set of controls

(Column 7). In Column (8), we further add the interaction between the treatment and the running variable. Results show that the treatment effect is positive at the 5% level and marginally higher than the OLS estimates. In conclusion, all our alternative estimation methods confirm the presence of an increase in political power as a significant and large causal effect on the performance of blood-related firms.

4.5. Variations in political power

We now focus on how the variation in power among districts and politicians impacts connected firms' performance, and on the channel through which the transfer of rent takes place. In Table 5, we noticed that the average performance improvement for firms connected to municipalities that merged was around 3.1 to 3.4 percentage points.

A municipality merger increases political power through many channels, including increasing population, budget, and outsourcing. In the unconditional correlation, we observe that doubling population is correlated with an increase of 105% in connected firms' operating performance. Doubling the local government expenditure is correlated with a 77% increase in performance, and doubling outsourcing expenditures is correlated with a performance improvement of 80%. In Figure 2, we show unconditional averages indicating how connected firm performance is correlated with *actual* variations in these three areas. We split our municipalities at the median level of the three measures; then, we show the mean performance for the groups below and above the median. As shown, the increase in population per politician is positively correlated with the increase in industry-adjusted OROA. However, the correlation between changes in budget size and firm performance is stronger; we find the largest correlation when we focus on the increase in outsourcing in the municipalities. Overall, these correlations are consistent with the notion that connected firms benefit from politicians being more powerful, and suggest that outsourcing was an effective way for transferring rent.

In Table 9, we investigate how variations in political power affect firm performance by studying different types of connections and politicians. In Column (2), we begin by looking at nuclear connections, i.e., where the CEO or board member or his/her spouse or sons/daughters are members of the municipality. We notice that the coefficient for nuclear connections in Column (2) is slightly larger than the one obtained on the full sample (Column 1). Column (3) focuses on powerful politicians, defined as those who won more than the median share of personal votes in a given party and district. Again, the coefficient is marginally higher than the average impact and significant at the 5% level. In Column (4), we look at firms connected to politicians belonging to the mayor's party or coalition. We notice that the coefficient is notably higher than the average impact even if the standard error is larger, likely as a result of a smaller sample. What these sample splits suggest—even if the differences are not statistically significant—is that the benefits to the firm produced by political ties increase with the level of power of the politician involved in the connection.

4.6. Proximity to the 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 by governments and to benefit from financial support provided by the international institutions. 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 an easier access to bank lending (Khwaja and Mian 2005). Goldman et al. (2009b) document that politically connected firms are favorably treated in the allocation of procurement contracts.

Table 10 provides evidence that the public demand plays a major role in determining how political connections create benefits for the companies in our 'corruption-free' environment. In Panel A, Columns (1) and (2), we test the importance of outsourcing for rent transfer. Motivated by the unconditional evidence in Figure 2, our hypothesis is that the increased political power in merging municipalities has a stronger impact on firm performance when municipalities 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 expenditures. 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 with a high outsourcing ratio. One interpretation of this result is that connected firms after the reform have the ability to increase their share of existing outsourcing activity because they are preferentially treated when new procurement contracts are offered.

In Panel B, we further 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 (2010), we analyze the cross-entries between public consumption and industries in the 2-digit Danish Input-Output matrix to classify industries as highly or weakly/not dependent on public demand.[19] Then we run regressions interacting our treatment with a dummy equal to 1 if the firm operates in an industry that has a high dependence on the public sector. The results indicate that the positive effect of merging on operating returns is strongly present in industries that are closely linked to the public sector. Overall, these findings support the notion that connected firms benefit from business relations with the public sector.

4.7. Firm variations and alternative outcomes

In Table 11, we explore the heterogeneity in the treatment effect along firm, industry, and political characteristics. For the sake of comparisons, Column (1) reports our baseline estimate using the full sample. In Columns (2) and (3), we separately analyze small and large firms. While the treatment coefficient is positive in both samples, results indicate that the effect is larger for smaller firms. Because we focus on the corporate value of *local* political connections, the different effect depending on firm

[19] Examples of highest dependence on the public demand are sectors related to education, hospitals, recreational activities, and civil engineering.

size may show up because large firms are more likely to focus their business outside the local district.

In Columns (4) and (5), we also observe that the effect is only present among firms that exhibited worse performance prior to the reform year. This result is consistent with highly profitable firms being more oriented outside the local municipality or, in general, being dependent on their political connections. In Columns (6) and (7), we divide our sample by industries that have a high or low concentration of politically connected firms. In industries where political connections are more common—and, perhaps, where companies have more to offer or gain from interactions with local government—the treatment effect is larger (4.2%) than in industries with low political connections (3.1%).

In sum, Table 11 provides evidence suggesting that local political connections are more valuable for small and less productive firms, and in certain industries. Together with evidence that connected firms are, on average, less profitable (Table 4), this picture indicates that the rent transferred to connected firms reduces social welfare.

In Table 12, we test the impact of blood-related connections on a number of alternative corporate outcomes.[20] In Column (1), we show that firms connected in merged municipalities experience an increase in revenues relative to the industry level; thus, our results on profitability may be related to an increase in market share arising from an increase in business activities. In Column (2), we find that the firms in merged municipalities increase net income to assets, which is used as an alternative measure of performance. In Column (3), we show that no significant effect is had on firm size, measured by changes in total assets; this result rules out the possibility that the differences in performance are merely determined by smaller increases in total assets of treatment firms as compared to control firms. In Column (4), we show that the firms

[20] The number of firms varies across the different columns in Table 12 due to data availability. Our sample is formed mostly by small- and medium-sized private companies, and not all firms publish data beyond that which is legally required.

in merged municipalities experience an increase in liquidity holdings, which is consistent both with the interpretation that these firms retain earnings and/or that they accumulate cash to be able to invest in the new business opportunities as they show up. In Column (5), we focus on the volatility of profits measured by the change in standard deviation of OROA around 2005. We find a positive and marginally significant effect, which suggests that in the post-reform period the positive effect on firm profits was partly driven by transitory expenses that the municipalities faced to reorganize their activities in the first year after the administrative reform. Finally, in Column (6), we test whether any impact on the capital structure of firms occurs, using the ratio of total debt to assets as dependent variable. The treatment is not significant; neither do we find any impact on the maturity structure of debt, measured as the ratio of long-term debt to total debt (unreported). These results suggest that an increase in political rent does not influence locally connected firms through the cost of capital or access to debt financing.[21]

5. Conclusion

We have shown that an exogenous increase in political power improves the performance of connected firms even in an institutional context where the effect may be expected to be negligible. Using an administrative reform to identify exogenous variations in political power, we have documented that being tied to local politicians is extremely important for the profitability of companies even in a country ranked as the least corrupt in the world. Our analysis thus suggests that political networking can be a powerful business strategy 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 developing countries have documented that political connections affect firms' capital structure through lower cost of capital,

[21] In unreported results, we also find no significant effects on wages or employment.

protection in times of financial distress, and easier access to bank credit. Our evidence suggests that doing business with the public sector represents the main channel for transferring political rent to connected firms, supporting the earlier finding of Goldman et al. (2009b). Thus, our analysis suggests that in developed countries with strong institutions, the transfer of rent through political connections is demand driven; connected firms are in a better position to gain from the outsourcing activities of the public sector.

Analyzing the full welfare effects of political connections is beyond the scope of this article. However, our analysis does contain some suggestions that political connections are welfare reducing. First, politically connected firms tend to be less productive before the connection is established. Second, the value of political connections is higher among less profitable firms. Both arguments indicate that political connections may transfer rent from more productive to less productive firms. The welfare reduction is mitigated, however, because our results also indicate that connected firms use the rent to increase their operating efficiency.

Finally, our analysis contributes to the discussion on how to measure corruption. 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 (1) illegal, (2) an attempt to circumvent existing rules, and (3) generally associated with favors extended to particular firms (Bennedsen et al. 2010). We do *not* claim that any of the connected firms or politicians in our study have engaged in illegal behavior. However, our analysis provides evidence that is consistent with the last two elements of the description above, in particular that political connections induce measurable firm-specific benefits which appear to be detrimental for economic welfare. As such, our findings indicate that a significant level of 'legal' corruption can be present, even in the least corrupt country in the world.

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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.

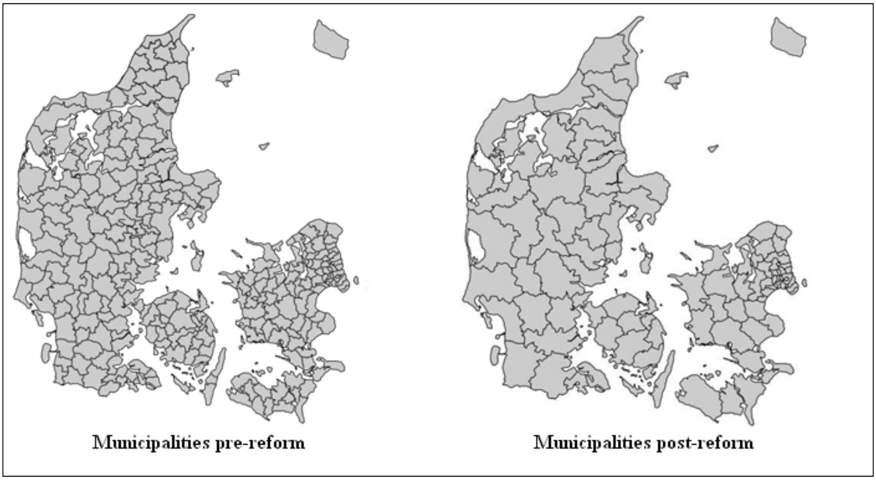

Figure 2. Operating performance and changes in the size of political offices

This chart illustrates the average unconditional change of industry-adjusted OROA around 2005, by above or below-median changes in a municipality's population, expenditures, and outsourcing divided by the number of elected politicians. Outsourcing represents the sum of expenses referred to contractors and other services. Outsourcing and expenditures ratios are computed using budget items from 2004-2007 and election data from 2001-2005. Population ratio is computed using data for 2005-2007 and election data for 2001-2005. Source: Denmark Statistics.

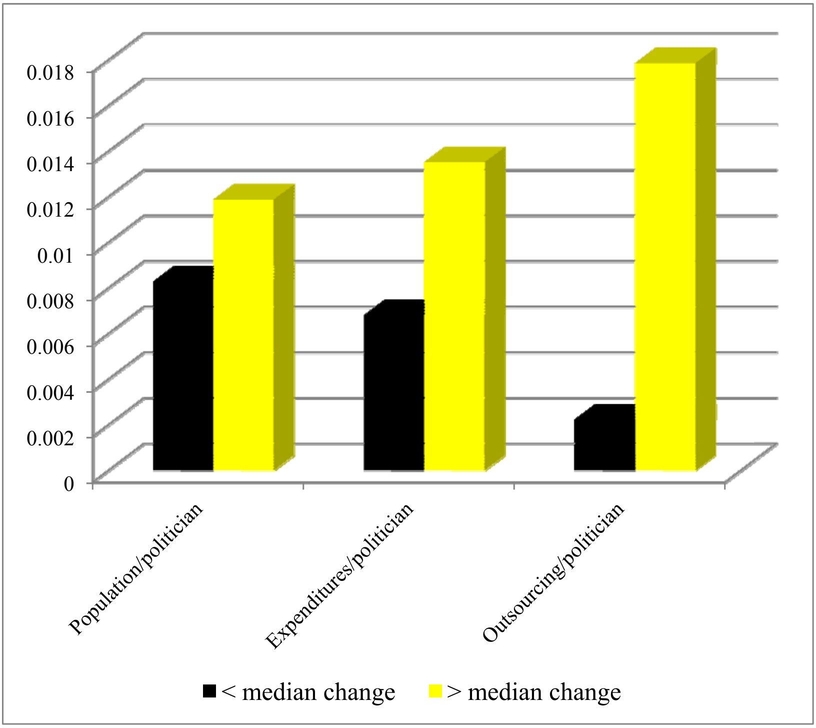

Table 1. Danish municipalities before and after the administrative reform

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 referred to contractors and other services. Outsourcing and expenditures ratios are computed using budget items from 2004-2007 and election data from 2001-2005. Population ratio is computed using data for 2005-2007 and election data for 2001-2005. 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. 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	5,197.7***
	Control	9,515.2	1,0352.3	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. In square brackets, we report a number of ratios. 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 square brackets. Panel B also shows the number of firms connected with politicians re-elected in 2005, by 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 square 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 elected to all candidates	[22%]	[22.1%]	[21.9%]
Re-elected candidates	1,679	1,287	392
Ratio re-elected to all candidates	[14.8%]	[15.3%]	[13.2%]
Ratio re-elected to all candidates running for re-election	[61.3%]	[57.5%]	[78.4%]
Ratio candidates running for re-election to candidates elected in 2001	[62.3%]	[60.3%]	[73.6%]

Panel B. 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 connections with elected to all connections	[38.3%]	[38.9%]	[36.4%]
Firms connected with re-elected candidates	419	321	98
Ratio connections with re-elected to all connections	[21.3%]	[22.1%]	[19.2%]

Table 3. Politicians' characteristics

This table illustrates average differences in observable characteristics for all candidates (Panel A) or winning candidates (Panel B) in 2001 and 2005 elections, by control and treatment municipalities. *, **, and *** denote significance at 10%, 5%, and 1% respectively.

	2001			2005					
	All	Treatment	Control	All	Treatment	Control	Difference (5)-(2)	Difference (6)-(3)	Diff.-in-Diff. (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 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. 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 is reported in square brackets.

		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	9.116	0.901***	0.009	9.663	9.709	9.514	0.195
	(0.008)	(0.051)	(0.057)	(0.111)	(0.052)	(0.124)	(0.118)	(0.126)	(0.293)	(0.317)
	[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	0.026	0.022	0.004
	(0.001)	(0.002)	(0.003)	(0.005)	(0.002)	(0.006)	(0.004)	(0.004)	(0.011)	(0.013)
	[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
	(0.001)	(0.002)	(0.003)	(0.005)	(0.002)	(0.005)	(0.004)	(0.004)	(0.009)	(0.011)
	[49,834]	[1,732]	[1,430]	[501]			[417]	[321]	[96]	
Ln sales	7.749	8.729	8.783	8.583	0.980***	0.210	9.074	9.094	8.991	0.103
	(0.019)	(0.084)	(0.097)	(0.169)	(0.086)	(0.195)	(0.172)	(0.186)	(0.435)	(0.470)
	[15,507]	[909]	[666]	[244]			[227]	[181]	[46]	
Ln employees	1.804	2.346	2.272	2.269	0.542***	0.003	2.636	2.638	2.630	0.008
	(0.008)	(0.046)	(0.051)	(0.103)	(0.047)	(0.115)	(0.114)	(0.122)	(0.278)	(0.303)
	[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 group. The dependent variable is the change in unadjusted OROA around the 2005 elections (three years after minus three years before, excluding the election year) in Columns (1) - (3), and the change in industry-adjusted OROA in Columns (4) - (7). The industry adjustment is computed as the firm's OROA minus the median OROA of the relevant industry. 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, and a set of regional dummies. Additionally, in Columns (2), (3), and (5) - (7), we control for the lagged logarithm of total assets and lagged industry-adjusted OROA. Standard errors are reported in parentheses. Columns (1) - (6) report robust standard errors, whereas in Column (7) standard errors are clustered by the new municipality classification. *, **, and *** denote significance at 10%, 5%, and 1% respectively.

Dependent variable:	OROA			Industry-adjusted OROA			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Treatment	0.0325**	0.0331**	0.0343**	0.0309**	0.0315**	0.0338**	0.0338**
	(0.0148)	(0.0149)	(0.0149)	(0.0149)	(0.0149)	(0.0150)	(0.0147)
Ln assets		-0.0022	-0.0015		-0.0020	-0.0010	-0.0010
		(0.0024)	(0.0022)		(0.0024)	(0.0021)	(0.0021)
Profitability $_{t-1}$			-0.2155**			-0.2189**	-0.2189**
			(0.0911)			(0.0946)	(0.1029)
Number of firms	419	419	419	419	419	419	419

Table 6. 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; 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. 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, 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. Robust standard errors are reported in parentheses. *, **, 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	-0.0013	-0.0013	-0.0032	-0.0015	0.0086	0.0049
	(0.0016)	(0.0015)	(0.0088)	(0.0085)	(0.0124)	(0.0125)
Ln assets		-0.0006*		-0.0012		0.0018
		(0.0003)		(0.0016)		(0.0021)
Profitability $_{t-1}$		-0.2953***		-0.1243***		-0.1997*
		(0.0071)		(0.0498)		(0.1047)
Number of firms	47,814	47,814	1,201	1,201	405	405

Table 7. Controlling for selection

This table shows the impact of the reform once we control for selection into the pool of connected politicians who were re-elected in 2005. In Panel A, each column reports the results from a first-stage probit regression, where the dependent variable is a dummy equal to 1 if the firm is connected with a politician re-elected in 2005 and 0 for connections with politicians not re-elected in 2005. 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, a set of regional dummies, lagged logarithm of total assets, and lagged industry-adjusted OROA. In addition, the specification contains an exclusion restriction, which is the average fraction of votes obtained by the same party of the connected politician in municipalities different from the one of the politician itself (in Columns 1 and 2), or the number of politicians older than 65 years in councils elected in 2001 (Columns 3 and 4). In Columns (1) and (3) we include the exclusion restriction only, whereas in Columns (2) and (4) we further include the interaction between the treatment dummy and the exclusion restriction. Standard errors (which are clustered by the new municipality classification), are reported in parentheses. In Panel B, each column reports the estimates obtained from a Heckman selection model, where in the first step we estimate the probability of being connected with a re-elected politician using the sample of politicians connected with firms that were elected in 2001 and ran again for election in 2005. In Columns (1) and (3), we estimate the selection equation including the treatment dummy and the exclusion restriction as well as the usual firm-level controls. In Columns (2) and (4), we further include the interaction between the treatment dummy and the exclusion restriction. In the first row, we estimate the model by using ML procedure. In the second row, we estimate the model using a two-step (Heckit) procedure. Standard errors 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	-0.2314	-0.9956***	-0.2080	-0.0974
	(0. 1409)	(0.2867)	(0.1436)	(0.1938)
Aggregate party votes	1.9562***	-1.0798		
	(0.4702)	(1.0811)		
Treatment × Aggregate party votes		3.7306***		
		(1.1973)		
Nr. politicians older than 65			0.2273***	0.3393**
			(0.0716)	(0.1528)
Treatment × Nr. politicians older than 65				-0.1442
				(0.1719)

Panel B. Dependent variable: Industry-adjusted OROA

	(1)	**(2)**	**(3)**	**(4)**
Treatment (ML)	0.0326**	0.320**	0.0358**	0.0352**
	(0.0146)	(0.0143)	(0.0153)	(0.0157)
Treatment (2-step)	0.0276*	0.0278*	0.0368**	0.0346**
	(0.0164)	(0.0160)	(0.0169)	(0.0168)
Connections with re-elected politicians	415	415	415	415
Connections with re-elected & non re-elected politicians	641	641	641	641

Table 8. Matching and discontinuity estimates

This table shows the impact of the reform using alternative estimation techniques. In Columns (1) – (4) we compute the average treatment effect 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: 3-year average pre-treatment logarithm of total assets and industry-adjusted performance; regional dummies; logarithm of politician's age; politician's gender; and position on the electoral list. The propensity score 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 (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 re-matching on the propensity score with replacement. In Column (4), we report the treatment effect obtained by using nearest-neighbor matching without replacement, thus only using the treated firms that are closest to the control firms. Estimations in Columns (1) - (4) are always restricted within the common support. In Column (5) and (6), we perform OLS regressions, excluding the 10% smallest (largest) municipalities. 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 inhabitants in 2004, from the threshold at 20,000 inhabitants. In Column (8), we further include the interaction between the treatment dummy and the running variable. In Columns (5) – (8), lagged logarithm of total assets and industry-adjusted OROA and a set of regional dummies are included. 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

	Re-weighting	Nearest-neighbor		1-to-1 match	Excluding smallest municipalities	Excluding largest municipalities	RDD	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment	0.0296**	0.0327**	0.0335*	0.0337*	0.0387**	0.0321**	0.0451**	0.0454**
	(0.0131)	(0.0163)	(0.0191)	(0.0171)	(0.0172)	(0.0153)	(0.0180)	(0.0178)
Number of firms	387	387	387	196	390	377	419	419

Table 9. Variations in political power

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. 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 total assets, lagged industry-adjusted OROA, and a set of regional dummies. In Column (1), we report the estimate obtained from the full sample for the sake of comparison. Nuclear-family connections (in Column 2) are defined as direct connections and connections with spouse and sons. Connections with powerful politicians (in Column 3) include firms connected with politicians who obtained more than the median fraction of votes in their list and in a given municipality. Connections with mayors' coalitions (in Column 4) include firms that are connected with politicians belonging to the same party/coalition as the mayor in a given municipality. Standard errors reported in parentheses are clustered by the new municipality classification. *, **, and *** denote significance at 10%, 5%, and 1% respectively.

Dependent variable: Industry-adjusted OROA

	All firms	Nuclear connections	Connections with powerful politicians	Connections with mayors
	(1)	**(2)**	**(3)**	**(4)**
Treatment	0.0338**	0.0400**	0.0361**	0.0474*
	(0.0147)	(0.0159)	(0.0172)	(0.0280)
Ln assets	-0.0010	-0.0009	0.0005	-0.0035
	(0.0021)	(0.0023)	(0.0026)	(0.0043)
Profitability $_{t-1}$	-0.2189**	-0.2665**	-0.1150	-0.5005***
	(0.1029)	(0.1067)	(0.1211)	(0.1679)
Number of firms	419	364	283	187

Table 10. The role of outsourcing and 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 group. In Panel A, Columns (1) and (2), we separately analyze firms connected with municipalities experiencing a low (high) increase in outsourcing per politician, defined as changes in the lower (upper) quartile of increase in outsourcing per politician around 2005 (2007 minus 2005). 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 total assets, lagged industry-adjusted OROA, and a set of regional dummies. In addition, in Panel B, Columns (1) and (2), we include a dummy equal to 1 if the firm is operating in industries above and below the median fraction of output sold to the public sector relative to total output, as well as its interaction 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 reported in parentheses are clustered by the new municipality classification. *, **, and *** denote significance at 10%, 5%, and 1% respectively.

Panel A. Heterogeneous results depending on outsourcing

Dependent variable: OROA	High increase of outsourcing	Low increase of outsourcing
	(1)	**(2)**
Treatment	0.0406*	0.0168
	(0.0220)	(0.0252)
Ln assets	-0.0019	-0.0003
	(0.0027)	(0.0027)
Profitability $_{t-1}$	-0.3093**	-0.1087
	(0.1231)	(0.1726)
Number of firms	289	128

Panel B. Proximity to the public demand

Dependent variable:	OROA	Industry-adj. OROA
	(1)	**(2)**
Treatment	-0.0032	-0.0050
	(0.0155)	(0.0158)
Treatment*High sectoral dependence	0.0578**	0.0610**
	(0.0282)	(0.0286)
High sectoral dependence	-0.0059	-0.0107
	(0.0232)	(0.0235)
Number of firms	419	419

Table 11. Firm and industry 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 group. The dependent variable is the change in industry-adjusted OROA around the 2005 elections. 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 total assets, lagged industry-adjusted OROA, and a set of regional dummies. In Column (1), we report the estimate obtained from the full sample for the sake of comparison. In Columns (2) and (3), we consider subsamples of small (large) firms, considered as firms having total assets below (above) the median of assets in 2006. In Columns (4) and (5), we consider subsamples of firms below (above) the median OROA in the pre-reform period. In Columns (6) and (7), we consider industries with a high (above median) or low (below median) fraction of connected firms. Standard errors reported in parentheses are clustered by the new municipality classification. *, **, and *** denote significance at 10%, 5%, and 1% respectively.

Dependent variable: Industry-adjusted OROA

	All firms	Small firms	Large firms	Low-profitability firms	High-profitability firms	Industries with high share of connections	Industries with low share of connections
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Treatment	0.0338**	0.0534**	0.0183	0.0643***	-0.0015	0.0422*	0.0314*
	(0.0147)	(0.0249)	(0.0140)	(0.0227)	(0.0175)	(0.0225)	(0.0172)
Ln assets	-0.0010	-0.0007	-0.0063***	-0.0018	0.0014	-0.0046	0.0020
	(0.0021)	(0.0085)	(0.0022)	(0.0023)	(0.0032)	(0.0035)	(0.0022)
Profitability $_{t-1}$	-0.2189**	-0.2482**	-0.0893	-0.2483	-0.0996	-0.2949*	-0.1283
	(0.1029)	(0.1170)	(0.0924)	(0.1658)	(0.1212)	(0.1626)	(0.0901)
Number of firms	419	203	216	209	210	200	219

Table 12. Impact on alternative 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 group. The dependent variables are changes around the 2005 elections of the following variables: logarithm of sales (Column 1); net income to assets (Column 2); ratio of liquid assets to total assets (Column 3); logarithm of total assets (Column 4); standard deviation of industry-adjusted OROA (Column 5); the ratio of total debt to total assets (Column 6). 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 total assets, lagged industry-adjusted OROA, a set of 2-digit industry dummies, and regional dummies. Standard errors reported in parentheses are clustered by the new municipality classification. *, **, and *** denote significance at 10%, 5%, and 1% respectively.

Dependent variable:	Log of Sales	Net income to assets	Total assets	Cash holdings	Profit volatility	Leverage
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	0.3929*	0.0632***	0.1584	0.0638***	0.0196*	0.0236
	(0.2095)	(0.0197)	(0.1323)	(0.0201)	(0.0117)	(0.0329)
Ln assets	-0.0432	-0.0067*	0.0052	-0.0036	0.0018	0.0165***
	(0.0288)	(0.0037)	(0.0187)	(0.0034)	(0.0016)	(0.0061)
Profitability $_{t-1}$	-0.8328	-0.0746	0.5854	0.0474	0.0099	-0.1676
	(0.6326)	(0.1118)	(0.4371)	(0.0956)	(0.0732)	(0.1997)
Number of firms	210	409	419	373	414	195

Corporate governance and international trade shocks

Mario Daniele Amore[†]
Copenhagen Business School

Alminas Zaldokas[‡]
INSEAD

Abstract

We study how the quality of corporate governance affects firms' reaction to changes in the competitive environment. Our identification strategy relies on exogenous variations in both corporate governance and product market competition experienced by U.S. firms in the late 1980s. While the Canada-U.S. Free Trade Agreement of 1989 increased foreign competition, the business combination laws, passed between 1985 and 1991 in thirty U.S. states, weakened corporate governance for firms incorporated in those states. We find that the operating and stock market returns of firms with worse corporate governance were more negatively affected by the increase in competitive pressures. We also find that worse corporate governance impaired the ability of exporters to benefit from the reduction in export tariffs to Canada. These differences in performance are related to the lower financial constraints of well-governed firms.

JEL Classification: G34
Keywords: corporate governance, free trade agreement, competition, financial constraints

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For helpful comments and suggestions we thank Ramin Baghai, Morten Bennedsen, Jeffrey Callen, Alejandro Cuñat, Vicente Cuñat, Alexander Dyck, Lily Fang, Denis Gromb, Maria Guadalupe, Gilles Hilary, Oguzhan Karakas, Karl Lins, Alberto Manconi, Massimo Massa, Randall Morck, David Oesch, Pascalis Raimondos-Møller, Urs Peyer, Gordon Phillips, Jesper Rangvid, Thomas Rønde, Ben Sand, Ana Maria Santacreu, Peter Schott, Yossi Spiegel, Heiwai Tang, Timothy Van Zandt, and Moqi Xu. We also thank conference participants at EEA/ESEM (Oslo), EARIE (Stockholm), the Nordic Finance Network Workshop (Helsinki), the Workshop on Corporate Governance and Investment (Leipzig), as well as participants at Copenhagen Business School and INSEAD brown bag seminars. All remaining errors are ours.

[†]Ph.D. candidate, Copenhagen Business School. Email: mda.eco@cbs.dk

[†]Ph.D. candidate, INSEAD. Email: alminas.zaldokas@insead.edu

1. Introduction

Extensive research has documented that, by shaping agency conflicts within the firm, corporate governance has significant implications for such corporate policies as acquisitions (Masulis et al. 2007), innovation (Aghion et al. 2009; Sapra et al. 2009), cash holdings (Dittmar and Mahrt-Smith 2007; Harford et al. 2008) and debt financing (Klock et al. 2005). Other work underlines the importance of corporate governance in facing an adverse shock such as the Asian financial crisis (Johnson et al. 2000; Mitton 2002). Overall, better-governed firms have higher productivity (Bertrand and Mullainathan 2003) and value (Gompers et al. 2003; Cuñat et al. 2011). We contribute to this literature by examining how corporate governance affects a firm's response to changes in the competitive environment.

The relationship between competition and corporate governance has long been debated theoretically. Hart (1983) formalizes the notion that competition might mitigate managerial slack. Scharfstein (1988) shows, however, that whether competition mitigates or exacerbates managerial slack crucially depends on the assumptions made on the managers' utility function. Schmidt (1997) provides a model in which, by reducing profits and increasing at the same time the liquidation threat, competition has an ambiguous effect on managerial effort. In disentangling empirically the effects of governance and competition on corporate outcomes, we face two major obstacles. First, corporate governance is typically correlated with unobservable factors, which can bias any inference regarding the effect of corporate governance on firm outcomes. Second, because governance and competitive actions are jointly determined in equilibrium, it is difficult to separate out their individual consequences for firms. In order to avoid these problems, we consider an exogenous variation in corporate governance and how it affects a firm's reaction to a subsequent exogenous increase in competitive pressures. These exogenous variations are provided by two policy changes: the Canada-U.S. Free Trade Agreement (FTA) of 1989, which led to a significant increase in foreign competition for U.S. firms; and the business combination (BC) laws, passed by thirty

U.S. states over the period of 1985-91, which worsened corporate governance of firms incorporated in those states by reducing the threat of hostile takeovers.

Using a sample of publicly traded U.S. firms over 1976-95, we find that the FTA's negative effect on operating performance and stock market valuation was greater for firms incorporated in states that had previously passed BC laws. This evidence indicates that worse corporate governance rendered firms either unable or unwilling to respond to changes in the competitive environment. Non-exporters under worsened governance became more vulnerable to the increase in competition induced by lower import tariffs. Moreover, worse-governed exporters did not benefit from the decrease in export tariffs to Canada, even though it increased the size of their product market. The negative effect of the FTA was greater for firms that were small, young, less productive, and located closer to the Canadian border. We establish that the effect of governance was in part due to lower financial constraints of better-governed firms, suggesting that financial constraints became more important after competition strengthened.

The Canada-U.S. FTA provides a plausibly exogenous variation in competition. Contrary to some other free trade agreements, the Canada-U.S. FTA was largely unanticipated and was not accompanied by any other significant economic reform; nor was it a response to prevailing economic conditions (Trefler 2004; Breinlich and Cuñat 2011). In addition, as Canada and the U.S. are main trading partners, the effect of the FTA was economically significant for the U.S. economy.[22] Furthermore, since the agreement consisted mainly of abolishing existing import tariffs that differed across industries, the increase in competition following the FTA had a measurable cross-sectional variation. Similarly, the passage of BC laws induced exogenous variations along an important dimension of corporate governance: the market for corporate control. In particular, BC laws restricted certain transactions (e.g. mergers and asset sales) between firms and their large shareholders for a period of three to five years after

[22] Clausing (2001) finds that a 1% reduction in post-FTA import tariffs was associated with a 10-11% increase in U.S. imports from Canada. He also estimates that the FTA raised annual Canadian exports to the U.S. by $23 billions. Since Canada was the main U.S. trading partner (accounting for about one fifth of total imports) and since there was no trade diversion, the effect of the FTA was substantial for the U.S. economy.

the large shareholder's stake passed a pre-specified threshold. This moratorium had the effect of hindering acquirers' access to target firms' assets and thus limited the former's ability to pay down acquisition debt. By making hostile takeovers more difficult if not impossible, BC laws weakened the overall quality of corporate governance and thereby increased managerial slack (Bertrand and Mullainathan 2003).

We focus first on operating performance. In particular, we interact a dummy indicating whether a firm was incorporated in a state with BC laws and a variable measuring the FTA-induced reduction in import tariffs within the firm's industry. Thus, our identification gains from the staggered implementation of BC laws and also from the different extent to which the trade reform affected different industries. The inclusion of firm fixed effects allows us to control for time-invariant differences in corporate governance and competitive positions. Also, since our identification relies on the interaction between states of incorporation and industries, we can control for the economic conditions of the states where firms are headquartered as well as for general industry trends.

Consistently with the notion that BC laws increased managerial slack, we find that the adoption of BC laws had a significant negative impact on the operating performance (measured by return on assets, or ROA) of firms incorporated in that state: on average, ROA dropped by 1.9% for these firms. The FTA, too, had a negative impact on operating performance. The ROA of firms that experienced an average tariff cut declined by 1.1% after the trade agreement. Examining the combined effect of the two policy changes, we find that the interaction between BC laws and lower import tariffs is also negative and statistically significant. The total effect for firms exposed both to BC laws and an average reduction in import tariffs was a decline of 3.1% in ROA.

After establishing an effect for the average firm, we examine whether our results are more pronounced for firms that are expected to be most affected by BC laws and the FTA. We first posit that non-exporters are affected negatively by trade liberalization because they are less likely to benefit from reduced export tariffs – that is, the FTA affected them primarily through the *import* tariff reduction. In line with this prediction, we find that worse corporate governance amplifies the negative effect of import competition for non-exporters. For exporters, in contrast, we find that worse governance reduces their ability to benefit from the lowered export tariffs to Canada.

Trade liberalizations have been found to induce welfare gains as the market shares are reallocated from the least to the most productive firms (Pavcnik 2002; Melitz 2003). It has also been shown that low-productivity firms are more likely to be taken over because they offer higher potential efficiency gains (Maksimovic and Phillips 2001). Thus, the combination of worse corporate governance and a subsequent increase in foreign competition should harm less productive firms the most. Indeed, our results indicate that, when the competition increases, worse corporate governance is especially harmful to firms with lower total factor productivity.

Gravity models of international trade suggest that trade intensity decreases with distance. We examine the geographic heterogeneity of our results by testing for whether the negative effect of the trade shock (and its interaction with governance quality) depends on the distance between Canada and the U.S. firm's headquarters. We find that both of these negative effects on profitability are concentrated among firms located closer to the Canadian border.

One concern with our results is that passage of the FTA or of BC laws may have been anticipated – in other words, that the "parallel trends" hypothesis required for the validity of our model is violated. We therefore perform a placebo test, which assumes that the FTA was already enacted in 1986 (in fact, negotiations on the agreement began in September 1985), but find no effects of such placebo policy on operating performance. Similarly, we find no significant effects of a placebo implementation of BC laws three years before their actual passage. Another concern is that the size of the tariff reduction was correlated with some pre-FTA industry characteristics and instead of the change in competition with our empirical specification we capture these inherent characteristics. To mitigate this concern, we control for several industry characteristics that are typically associated with trade protection (Guadalupe and Wulf 2010). Our results are robust to the inclusion of the Herfindahl-Hirschman index (HHI), which controls for the domestic competition in U.S. industries, and also to the interaction between the HHI and BC laws (as in Giroud and Mueller 2010). Furthermore, our results are robust to the exclusion of firms incorporated in Delaware and of firms that operate in more than one industry (multisegment firms). Finally, we confirm our findings by adopting an alternative proxy for the quality of corporate governance – the extent of institutional ownership in the firm (Nikolov and Whited 2009) – and an alternative proxy for foreign competition – the industry-level import penetration as instrumented by the real exchange rate (Bertrand 2004).

In addition to these results on operating performance, we document a significant decline in the market value of firms that are affected by tariffs cuts and are incorporated in states with BC laws. First, we confirm the results in terms of market-to-book ratios. Second, we use an event study to show that companies with worse corporate governance had a more negative stock price reaction to the FTA. The trade agreement encountered substantial opposition in Canada, and its fate was determined by a narrow victory of the Progressive Conservative Party in the federal election of November, 1988. Thus, the election date offers a good setting for assessing the stock market reaction to the FTA (Morck et al. 2000; Breinlich 2010). We examine abnormal returns for U.S. firms on the trading days following the election. Our findings indicate that, over a period of six days, stock prices dropped by 1.88% more for firms subject to BC laws than for other firms.

Finally, we examine the channels through which corporate governance might affect firm performance when competition changes. Broadly, such effect can be justified in two ways. Entrenched managers could be taking advantage of a “quiet life”; thus, because of earlier poor actions or unwillingness to react to a shock that requires new actions to be taken, their firms would suffer the most. Also, managers in firms with worse governance might be unduly constrained and thus not able (although willing) to respond appropriately to an increase in competition. We explore the latter explanation by looking into firms’ financial constraints, which play an important role in how firms react to trade liberalization (Manova 2008). First, we find evidence of larger effects on operating performance among the firms that were ex ante the most financially constrained, i.e. firms in industries that rely heavily on external finance, firms without a credit rating, and small and young firms. Second, we test for whether a subsequent exogenous increase in financial need magnified the negative effect on performance for worse-governed firms facing the competitive shock. Examination of the oil spike that occurred during the first Gulf War in 1990 reveals that an unexpected change in credit conditions mostly affected firms that had recently experienced declining tariffs and the introduction of BC laws. Third, looking at the actual changes in the capital of U.S. firms, we find that firms subject to BC laws raised less external finance (both debt and equity) in the post-FTA period than other firms did. When combined, these results support the explanation that increased competition had a more negative effect on worse-governed firms (at least in part) because of the more binding financial constraints they faced.

This paper contributes to several streams of literature. First, our work is closely related to the literature that studies how firms adapt to an increase in competition. It has been shown that more competition leads to outsourcing (Grossman and Helpman 2004), to flatter and more decentralized organizations (Bloom et al. 2010; Guadalupe and Wulf 2010), to greater pay-for-performance sensitivity (Cuñat and Guadalupe 2005, 2009), and to upgrading of technology (Bustos 2011). In demonstrating how firms' responses to trade liberalization are shaped by the quality of their governance, our results indicate that misalignment of incentives between managers and shareholders limits the readiness of firms to face changes in the competitive environment. We thus also extend the work of Khanna and Tice (2000) who show that firms with less agency conflicts (those with higher inside ownership, or the ones that are privately owned) respond less aggressively to the entry of a new rival. Our paper establishes the value effects, i.e. that after a rise in competition worse-governed firms in fact suffer in terms of operating and stock market performance.

Another study that is close to ours is that of Morck et al. (2000), who find that the Canadian firms affected most by the FTA were heir-managed family firms. Following the expansion of export markets, these firms lost their domestic advantage over the widely-owned firms. Here we instead focus on U.S. companies and, in particular, on the corporate governance aspect on the firm's response to a trade shock. In addition, we control for the endogeneity of corporate governance by employing BC laws as a shock to the market for corporate control. We also uncover a channel – namely, the need to raise external funds – that explains why corporate governance matters for a firm's ability to compete in the product market. Finally, we document that the role played by corporate governance in responding to trade liberalization depends on the nature of a firm's operations. For domestic firms, worse governance limits their response to increases in import competition; for exporting firms, worse governance reduces their capacity to benefit from greater business opportunities in Canada.

Our paper is also related to the literature studying whether competition acts as a governance device (Alchian 1950; Stigler 1958). More recent work has provided empirical support to this claim. For instance, Giroud and Mueller (2010) document that BC laws reduced profitability primarily in less competitive industries.[23] But whereas Giroud and Mueller (2010) explore how changes in corporate governance affect firms in a given competitive environment, we investigate governance-induced differences in firms' readiness to compete under increasing import competition in product markets. In fact, we find that increased competition for a given firm does not reduce the importance of worse corporate governance. On the contrary, weaker corporate governance impairs firm's profitability after the rise in competition. This shows that, even if competition is a corporate governance device, it takes time for the threat of being driven out of the market to actually realize.

Finally, our work is related to the literature on heterogeneous firms and international trade (Melitz 2003). Recent research in this field has emphasized the role of credit supply on firms' exports (Manova 2008, 2010; Paravisini et al. 2011). We focus on how different levels of access to financing affect the response of domestic

[23] Kadyrzhanova and Rhodes-Kropf (2011) find that the interaction between industry concentration and corporate governance can be either positively, or negatively associated with a firm's value depending on the type of the governance provisions considered.

producers to an increase in import competition. Our results suggest that corporate governance is one of the factors determining which firms are likely to benefit (or suffer) from trade liberalization.

The paper proceeds as follows. Section 2 describes our data and key variables. Section 3 discusses our empirical methodology. Section 4 presents our main findings on operating performance. Section 5 discusses results on market values. Section 6 looks at the effect of export tariff reduction for exporting firms. Section 7 explores the role of financial constraints. Section 8 concludes.

2. Data and variables

2.1. Data sample

Our data set consists of publicly listed firms located and incorporated in the United States. We restrict our analysis primarily to manufacturing firms (SIC codes up to 4000) because the FTA affected only the tradable sector (Guadalupe and Wulf 2010). We draw our data on firm outcomes from the Compustat data set[24]. We exclude the firms for which net sales or book value of assets are either missing or negative as well as firms in the industries for which we have no data on tariffs. Our sample period ranges from 1976 through 1995 and consists of 3,567 unique firms and 34,279 firm-year observations, although the presence of missing values for control variables reduces the number of observations used in the regressions.

2.2. Measures of corporate governance

A first generation of anti-takeover statutes were passed by some U.S. states in the 1970s. These statutes were deemed unconstitutional by the Supreme Court in 1982, primarily because states exceeded their jurisdictional reach in applying them to firms

[24] We use the Compustat data set of public firms – rather than establishment-level data from the U.S. Census – because most of the financial decisions that drive our results are made at the headquarters and not at individual plants. Note also that, since private firms are typically more constrained financially than are public firms, our results should generalize to the broader array of firms covered by the U.S. Census.

incorporated outside their state. The mid-1980s saw states introducing anti-takeover legislations aimed to firms incorporated in the legislating state, and the practice spread across the country after Indiana's new law was declared constitutional by the Supreme Court in 1987. As reported by Bertrand and Mullainathan (1999), the most stringent of these anti-takeover regulations were BC laws that made hostile takeovers more difficult by restricting an acquirer's access to the target firm's assets for a period of three to five years, thus limiting the ability to use debt to finance the acquisition. We exploit the introduction of BC laws as our key variation in corporate governance.

BC laws were introduced in various U.S. states at different times. Table 1 reports when BC laws were passed in each state as well as the distribution of firms by states of location and states of incorporation.[25] In our sample, only 33.1% of the firms are actually incorporated in their state of location.[26] Twenty states, which account for 15.7% of firm-year observations, never passed a BC law.

Figure 1 illustrates the timing of BC legislation with respect to the FTA. Most of the firms (79.1%) are incorporated in a state that passed a BC law in or before 1989, the year of the FTA. For this reason, we interpret our results as indicating the combined impact of an exogenous worsening of corporate governance and a subsequent increase in foreign competition.[27]

As a robustness check, we use the fraction of institutional ownership as a proxy for the quality of corporate governance. Standard corporate governance indices, such as those constructed by Gompers et al. (2003) and Bebchuk et al. (2009), are unavailable for the period we study. Moreover, Nikolov and Whited (2009) claim that those indices fail to capture latent poison pills which can be introduced without shareholder consent. Hence they suggest that institutional investor ownership is a better proxy for corporate

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[25] Given that firms are affected by BC laws in their state of incorporation, the potential for misclassification arises because Compustat only reports the state of incorporation for the latest year available. However, re-incorporation during the period considered was rare (Romano 1993) and so we assume that no such changes occurred over the sample period.

[26] The table reveals that, as expected, most of our sample firms are incorporated in Delaware; however, in Section 4.3 we demonstrate that our results are robust to the exclusion of Delaware-incorporated firms.

[27] To confirm this interpretation, we perform a robustness check (see Section 4.3) that excludes firms incorporated in states that passed BC laws *after* the FTA.

governance. We draw the annual data on institutional investor holdings from SEC 13 filings recorded in the Thompson Financial CDA/Spectrum database.[28]

2.3. Measures of competition and industry concentration

The FTA abolished existing trade duties between U.S. and Canada. Because these tariffs differed across industries, we quantify how the FTA influenced foreign competition for U.S. firms by using the tariffs on imports from Canada that applied to a given industry *before* the implementation of the FTA. As shown by Clausing (2001), the larger were the import tariffs in place in a given industry, the greater was the competitive shock.

We use each firm's primary four-digit SIC code to identify its industry and thus the relevant tariffs. We extract data on tariffs from the Center for International Data at UC Davis. We start by computing average tariffs in the industry by summing the customs value of imports and duties paid across all sub-industries of each four-digit SIC industry in each year before 1989. We then divide the total duties paid by the total customs value of imports and use this as our proxy for the import tariffs from Canada that each four-digit SIC industry faced in a particular year. The main treatment in our specification is the change from the average import tariffs in the pre-FTA period, computed over the three years prior to the implementation of the FTA (1986-88), to zero tariffs in the post-FTA period (from 1989 onwards). Table 2 lists the twenty industries with the highest tariffs on Canadian imports. The median cut in import tariffs due to the FTA was 3.3% and it ranged between 0% and 36%.

We validate that the FTA represented a competitive shock for U.S. firms by estimating its effect on price-cost margins, after controlling for firm size, age, and year and firm fixed effects. Unreported results, as in Guadalupe and Wulf (2010), suggests that more exposure to import tariff cuts indeed leads to a greater decline in the price-cost margin.[29]

[28] All institutional investors with more than \$100 million of securities under management must report their holdings to the SEC on form 13F and must also disclose all common stock positions that exceed 10,000 shares or \$200,000.

[29] By contrast, we do not find any direct effect of BC law passages on firms' price-cost margin.

Because of its bilateral nature, the FTA also improved opportunities for U.S. exports to Canada. To separate this effect from the increase in competition, we use export tariffs data from Trefler (2004) and construct a variable similar to our variable for the import tariffs. Again, we measure the reduction in export tariffs to Canada at the level of U.S. four-digit SIC industry.

Although we consider the import and export tariffs to be zero for all industries after 1989, in some industries the tariffs reductions were phased out over periods as long as ten years following the FTA's passage.[30] Nevertheless, we treat all industries equally regardless of their phase-out schedule.[31] As discussed in Guadalupe and Wulf (2010), this has the advantage of mitigating the potential endogeneity of the phase-out schedule.

We control for existing domestic concentration with the Herfindahl–Hirschman index (HHI) based on the sales distribution of publicly listed firms in each three-digit SIC industry. A higher HHI corresponds to greater industry concentration. We correct for potential misclassifications due to the presence of a single firm in a given industry by omitting 2.5% of the firm-year observations at the right tail of the HHI distribution (cf. Giroud and Mueller 2010). The average HHI in 1988 – that is, one year prior to the passage of the FTA – is around 0.2 (see Panel A of Table 3).

As a robustness check, we adopt the industry-level import penetration as an alternative measure of foreign competition. An industry's import penetration is defined as the dollar value of imports divided by the sum of dollar value of imports and dollar value of domestic production. Because import penetration can be endogenous to industry's profitability, we follow Bertrand (2004) and instrument it using the weighted average of the real exchange rates of the importing countries. In particular, the weights

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[30] Annex 401 of the FTA prescribes the actual phase-out schedules. However, there is anecdotal evidence that many industries lobbied to hasten the phase-out with the first review of the initial schedule adopted just a year after the FTA (see, e.g., "Canadian Trade Pact Accelerated", *New York Times*, March 14, 1989).

[31] Thus, we implicitly assume that (i) firms started adjusting to the new competitive situation immediately following the FTA's passage, and (ii) phase-outs served only to maintain temporary profits. However, untabulated results show that the results are robust to using the actual tariffs, as re-estimated annually after 1989.

for each industry are the shares of each foreign country's imports in the total imports of that industry; thus, the instrument varies both by time and industry.

2.4. Firm outcomes

Our main measure of operating performance is the return on assets, computed as earnings before interest, taxes, depreciation, and amortization (EBITDA) divided by the beginning-of-year book value of assets.[32] To mitigate concerns about outliers, we drop 1% of the firm-year observations from each tail of the ROA distribution, although this procedure does not affect our results.

We also employ the ratio of market value to book value (MB ratio). To compute it we divide the market value of each firm (at the end of its fiscal year) by its book value of common equity. Following Baker and Wurgler (2002), the MB ratio is limited to the interval between 0 and 10.

We define a few of firm characteristics in order to examine whether our hypothesized effect is stronger for firms expected to be more affected by the FTA. First, we sort firms by their total factor productivity (TFP) in 1984, which is estimated via the semi-parametric procedure described in Olley and Pakes (1996).[33] Second, we measure each firm's proximity to the Canadian border; this is proxied by the distance from the largest city in the state of location of the firm's headquarter to the nearest U.S.-Canada border crossing. Finally, when examining the effect of the reduction in export tariffs for exporting firms, we classify exporters as firms that have exports which constitute at least 1% of sales in the pre-FTA period.[34]

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[32] We are primarily interested in how an increase in foreign competition affects the profitability of the firms; however, since profitability is monotonically and positively related to productivity (Imrohoroglu and Tüzel 2011) and since productivity is often proxied by profitability measures in the finance literature (Novy-Marx 2010, Gourio 2007), our results also suggest that a bilateral weakening of trade barriers has, on average, a more negative effect on the productivity of domestic firms with worse corporate governance. In fact, our results on profitability are broadly in line with those based on using a measure of total factor productivity as our dependent variable.

[33] The firm-level variables used to compute TFP are the logarithms of sales, employment, capital expenditures, and property, plants and equipment.

[34] We use a 1% threshold to avoid trivial values in exports. However, our results are qualitatively similar if exporters are simply classified as firms reporting *any* exports (and non-exporters as firms reporting *no*

2.5. Financing

We measure financial constraints in three ways. First, following Rajan and Zingales (1998), we classify firms based on whether the industry in which they operated was above or below the across-industry median of the dollar value of external financial capital raised in 1984 (i.e. one year prior to the passage of the first BC law), normalized by the dollar value of industry assets. Second, we sort our sample by whether or not in 1985 the firms had been assigned a long-term bond rating by Standard & Poors (as reported in Compustat).[35] A bond rating enables firms to access public debt markets and is therefore related to lower credit constraints (Kashyap et al 1994; Faulkender and Petersen 2005). Moreover, as smaller and younger firms are more vulnerable to capital market imperfections (Almeida et al. 2004), we look at the firms at different stages of development (i.e., young and old firms) and different sizes (i.e. small and large firms).

We also provide a test using an exogenous shock that affected the financing needs of some firms: the oil price spike at the end of 1990. In particular, we measure a firm's exposure to this shock by the correlation between daily returns on its stock price and the changes in the West Texas Intermediate (WTI) crude oil spot price, estimated using the data from 1989.

Finally, our measures of external financing activity are based on net changes in debt and equity, estimated as in Hovakimian et al. (2001) and Leary and Roberts (2005). We define the capital raised in a given year as the net change in equity and debt, normalized by the firm's book value of assets in the previous year. We are interested in firms that raise (rather than return) capital, so we consider only positive values of the capital raised. That is, if the net change in debt and equity is negative, we record the capital raised as 0.[36] To deal with outliers, the fraction of capital raised to existing assets is capped at 1. Finally, due to data reliability we follow Leary and Roberts (2005) in restricting our external finance analyses to the period of 1984-95.

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exports) in a given year. Note that due to lack of export data by destination country, we consider the overall export activity and not just export to Canada.

[35] Data limitations necessitate that we use data from 1985 rather than 1984.

[36] However, allowing negative net changes in debt and equity does not substantially alter our results.

We report summary statistics for the main variables of interest in Table 3. Appendix describes all the variables used.

3. Identification strategy

Because corporate governance is an equilibrium outcome that is largely determined by the firm itself, it is difficult to establish a causal link between corporate governance and firm performance. A positive association between profitability and a measure of governance quality, such as board independence, could indeed mean that good governance is beneficial for firm performance. However, such an inference is plagued by three problems. First, companies may adopt effective governance mechanisms in response to good performance, in which case, corporate governance is not the determinant but rather the consequence of firm performance. Second, the quality of corporate governance may be correlated with factors (e.g. CEO's preferences) that are not observed by the researcher, yet directly affect firm policies; in this case, one would wrongly attribute the effect of such omitted factors to corporate governance. Third, if we seek to establish whether corporate governance alters the effect of a changing competitive environment on the firm's performance, then we run into additional problems. For the industries in which good corporate governance becomes increasingly more important for operating performance, competition could intensify. In such a scenario, firms could be improving their governance as the means to improve performance and thus could end up competing more aggressively. Finally, some unobservable factors (e.g., increases in industry's productivity) might be increasing the extent of competition while also cementing the link between corporate governance and performance.

We deal with these concerns by combining two types of difference-in-differences models that establish exogenous variations in both the quality of corporate governance and the intensity of foreign competition. First, we exploit the staggered passage of BC laws in the states of incorporation (Bertrand and Mullainathan 2003; Giroud and Mueller 2010). After controlling for state-level business conditions and firm fixed effects, we assume that the parallel trends between treatment and control groups hold, and thus we are able to identify the effect of worsened corporate governance on a firm's performance.[37] Second, we use passage of the FTA as an exogenous variation in the competitive environment. Even though the timing of the change was uniform, the exposure to the FTA and thus the agreement's effect on competition varied across industries, because pre-FTA tariffs for imports from Canada differed across U.S. industries.[38]

There are several methodological advantages to combining the FTA and the BC laws in order to establish exogenous variations in competitive pressures and corporate governance. First, note that addressing the combined impact of competition and corporate governance on firms solely by means of their cross-sectional measures would leave the analysis open to omitted factor bias. Adopting shocks to competition and governance provides a more tractable way to mitigate this concern than controlling for all potentially omitted variables. Second, one could argue that corporate governance has an effect on the firm's strategy in the product market, and hence on measures of industrial composition. It is therefore difficult to interpret the impact of BC laws and, for example, HHI on firm outcomes if the HHI itself changes in response to BC laws. Using the FTA addresses this concern because BC laws should not have induced immediate systematic increases in import tariffs, which are decided at the international

[37] Because our identification relies on BC laws that were passed a few years before the FTA, one concern is that firms might already have adjusted their internal governance mechanisms, in which case the BC laws (especially those passed early in the period under consideration) should not matter by the time of the FTA. Yet, such concerns mean only that we are estimating a lower bound of the corporate governance effect to the FTA since we are unable to control for the fact that some firms having already reduced their managerial slack. Still, we address this concern in two ways. In the first place, our results are robust to the exclusion of firms that were exposed to the earliest passage of BC laws (in 1985) and thus had the most time to adjust. Second, when looking at the dynamic effect of BC laws on firms, we find that their negative effect on ROA did not diminish, but rather persisted over the years after the BC was passed.

[38] In adopting this approach, we follow Card (1992), who uses a variable to classify cross-sectional units in terms of their exposure to a law change. A statistically significant coefficient for this treatment variable means that it is a good predictor of changes in the dependent variable induced by the policy change (Angrist and Pischke 2008). In our case, the extent of exposure is measured by the average tariffs on Canadian imports that applied in the industry prior to the FTA.

level.[39] Third, using the FTA to establish exogenous and measurable variations in competition circumvents the methodological difficulties of measuring actual competitive pressures.[40] Similarly, BC laws provide a reliable way to assess the effect of corporate governance because consistent firm-level corporate governance measures are lacking for the period surrounding passage of the FTA.[41] Figure 2 depicts our identification strategy in the graphical form.

Our baseline model combines variations induced both by BC laws and the FTA. Whereas each policy taken separately measures the respective impact of changing governance and competition, their interaction identifies the effect on operating returns of exogenously worsened governance and a subsequent increase in foreign competition. Thus, we estimate the following regression:

$$\begin{aligned} ROA_{ijkt} = \alpha_i + \alpha_t + \beta_1 Import\ Tariff\ cuts_{jt} + \beta_2 Export\ Tariff\ cuts_{jt} + \\ + \beta_3 BC_{kt} + \beta_4 BC_{kt} \times Import\ Tariff\ cuts_{jt} + \gamma' \mathrm{X}_{ijkt} + e_{ijkt} \end{aligned} \qquad (1)$$

where i indexes firms, j indexes four-digit SIC industries, k indexes states of incorporation, and t indexes time. The dependent variable ROA_{ijkt} is the return on assets. *Import tariff* $cuts_{tj}$ measures the average level of tariffs on imports from Canada in the industry j before the passage of the FTA, interacted with a dummy, set equal to 1, for the post-FTA period, i.e. *Import tariff* $cuts_{tj}$ is equal to 0 before 1989 and to a positive

[39] If anything, import tariffs decreased slightly over time over the period during which the BC laws were passed.
[40] Many empirical works have stressed the importance of dealing with the endogeneity of product market competition, by using, for example, regulation indexes (Guadalupe and Perez-Gonzales 2011), exchange rates and import tariffs as instruments (Cuñat and Guadalupe 2005), sharp appreciation of currencies (Cuñat and Guadalupe 2009), and policy instruments (Aghion et al. 2005). Note also, that such measures as the HHI and the Lerner index are strongly non-monotonic in the actual competitive situation (Schmalensee 1989), and fail to account for the competitive pressure exerted by potential entrants. An additional issue – that HHI values in the empirical corporate finance research are often based only on public corporations that constitute a small fraction of the universe of firms – is addressed by Ali et al. (2009).
[41] One concern is that BC laws may have had no corporate governance effect and merely made it more difficult to take over inefficient firms. In fact, previous research finds no actual drop in the M&A activity after BC laws were passed (see, e.g., Comment and Schwert 1995; Giroud and Mueller 2010). Garvey and Hanka (1999) suggest that BC laws raise the cost of takeover activity but also the resulting slack increases the payoff from a successful takeover. Therefore, reduced *threat* of takeovers need not reduce actual takeover activity.

value after 1989.[42] *Export tariff cut*$_{tj}$ is the corresponding measure for tariffs on exports to Canada in the industry *j*. We assume that no tariffs remained after 1989, so the coefficient for *Import tariff cuts*$_{tj}$ measures how ROA changed for firms that were exposed to greater foreign competition due to the FTA. BC_{kt} is a dummy, set equal to 1 if the firm's state of incorporation *k* has BC laws in year *t* (and to 0 otherwise). If BC laws do have a negative effect on corporate governance that translates into lower operating returns, then we expect β_3 to be negative. The coefficient for our key variable of interest BC_{kt} × *Import tariff cuts*$_{tj}$ measures how the negative effect of the cut in import tariffs varies as a function of the exposure to BC laws. The null hypothesis for β_4 is that an increase in foreign competition affects firms' returns uniformly, regardless of their governance, i.e. $\beta_4 = 0$. We expect a negative β_4 if worse governance makes firms respond inadequately to increases in competition.

As documented by Giroud and Mueller (2010), firms incorporated in the states with and without BC laws differ in many observable characteristics. For this reason we must control for a number of confounding influences. Our specification includes year dummies, α_t, and firm fixed effects, α_i, to mitigate the scope for omitted factor bias. In addition, our vector of controls, X_{ijkt}, includes firm size, its squared term and firm age.[43] Moreover, we control for the one-year lagged HHI in order to control for the domestic industry concentration.

Also, we control for general conditions at the industry level as well as for contemporaneous economic conditions in the states where firms operate. We do so by estimating state and industry linear trends. In particular, we calculate time-varying averages of the ROA of firms in certain state of location, excluding the firm in question when computing these averages. In a similar fashion, we calculate time-varying averages of the ROA of firms in certain industry, excluding the firm in question when computing these averages. In our robustness checks, we include polynomial terms (up

[42] Following Guadalupe and Wulf (2010), we compute the pre-FTA import tariffs using four-digit SIC averages for the period between 1986 and 1988 as the baseline treatment. As robustness checks, we use alternative procedures, such as those based on three-digit or two-digit SIC codes, averages taken for the period between 1983 and 1988, or averages for the entire period (1976-88) preceding the FTA.

[43] In unreported analyses, we find that our results are unchanged after controlling for lagged leverage.

to cubic) of the state and industry linear trends; also, we follow Guadalupe and Wulf (2010) and control for preexisting industry characteristics that are typically related to trade protection: skill intensity, capital intensity, and TFP growth.[44]

We cluster the standard errors by the state of incorporation, which accounts for arbitrary correlations of residuals across different firms in a given year and state of incorporation, across different firms in a given state of incorporation over time, as well as over different years for a given firm. However, our findings are robust to alternative clustering methods: at the firm level, at the industry level, two-way clustering at the levels of industry and state of incorporation, and by block-bootstrap, as proposed by Bertrand et al. (2004).

4. Operating performance

4.1. Baseline results

Table 4 presents results for the full specification which includes BC laws, a cut in import tariffs and the interaction between them. First, to validate our claim that BC laws are a shock to corporate governance that negatively affected operating returns, we look at the effect of BC law (Column 1). The results, which are in line with those reported by Bertrand and Mullainathan (2003), confirm a negative effect of BC laws on profitability. Meanwhile, consistently with the prediction that the FTA increased foreign competition for U.S. firms, we find that the coefficient for reduced import tariffs is negative and significant; firms exposed to the average (3.3%) tariff reduction saw their ROA decline by 1.1% (the average ROA in our sample is 6.7%). These findings remain unchanged after controlling for the industry HHI (Column 2).

Columns (3) and (4) report our main test by including the interaction between BC laws and import tariff cuts. The coefficient for this interaction term is negative and statistically significant at the 5%, even though the cut in import tariffs by itself is not

[44] Industry controls are extracted from the NBER-CES Manufacturing Industry Database. We compute these controls in the same way as our main tariffs, i.e. by taking the averages for the period 1986-88 and interacting them with a dummy, set equal to 0 before 1989 and set equal to 1 on and after 1989.

significant. The drop in ROA was 3.1% for firms incorporated in states with BC laws *and* exposed to the average cut in import tariffs. Thus, the increase in competition affected operating returns only for firms with recently worsened corporate governance.

In Columns (5) and (6), we add the variable measuring the reduction in export tariffs and its interaction with BC laws, thereby controlling for the fact that the FTA also reduced export tariffs to Canada. Our estimates indicate that the interaction between BC laws and the reduction in import tariffs remains negative and statistically significant, whereas the interaction between BC laws and export tariffs cut is not significant. For the average firm, our findings are thus driven by the increase in foreign competition in U.S. domestic markets and not by the greater ease of exporting to Canada. In Section 6 we examine how this result varies for exporters and non-exporters.

Overall, our findings indicate that worse corporate governance impairs the performance of firms that are subject to greater foreign competition.

4.2. Firm characteristics

We now explore whether the effect is stronger for firms that should have been affected more by the FTA. First, we check whether less productive firms were hurt most. We then show that our effect is present mainly among firms that are more closely located to the Canadian border.

We begin by differentiating firms by their productivity. Models of trade integration with heterogeneous firms (see, e.g., Melitz 2003) suggest that only low-productivity firms are negatively affected by trade liberalization. Moreover, low-productivity firms are ex ante more likely to be taken over because of greater potential efficiency gains (Maksimovic and Phillips 2001). Hence, the passage of BC laws which reduce the threat of takeover should affect them more negatively than high-productivity firms. We thus test whether the negative effect of the interaction between BC laws and trade liberalization on operating returns is mostly prevalent among low-productivity firms.[45] We measure firm-level total factor productivity (TFP) by following the semi-parametric procedure developed in Olley and Pakes (1996).[46] Then, we estimate separate regressions for subsamples of firms with lower and higher TFP than their industry peers in 1984 (Table 5, Columns 1 and 2). The interaction between BC laws and import tariff cuts is negative in both subsamples, but the economic magnitude is more than twice as large for the subsample of low-productivity firms. Our findings thus indicate that low-productivity firms suffered more from the FTA than did other firms and especially so if they were subject to BC laws.

Next, we explore how our results vary depending on the geographic proximity to the Canadian market. Gravity models of international trade imply that the intensity of trade decreases with the distance between the trading partners, so we expect the FTA to have a stronger effect on firms that operate closer to the Canadian border.[47] As BC laws were introduced at the level of state of incorporation, we avoid spurious correlation between distance and the quality of governance. We measure proximity to Canada as the distance from the largest city in the firm's state of location to the closest U.S.-Canada border crossing. We then split the sample according to whether the firms are located closer to or farther from the median distance to Canada (300 miles) and analyze separately the effect of BC laws and reduced import tariffs for both subsamples (Table 5, Columns 3 and 4). We find that the combined effect of tariff reduction and BC laws is statistically significant only for those firms located closer to the Canadian border.

The effect of BC laws on managerial slack was arguably heterogeneous also depending on a firm's ownership structure. For example, family firms may have been less exposed to the negative consequences of BC laws, either because the managers are controlling owner or because the concentration of shares in the hands of families is in itself an effective device to monitor non-family managers. While our estimates quantify

[45] BC laws have a direct effect on firm-level productivity (Bertrand and Mullainathan 2003). We thus sort the firms according to their productivity before the first BC law was passed (in 1984).
[46] Our findings are also robust to computing TFP as Solow residuals from a Cobb-Douglas production function estimated with OLS. Untabulated results are available at request.
[47] Northern and southern states differ along other dimensions as well. For instance, southern states have laxer employment protection laws (Autor et al. 2004) and may therefore be able to adapt more quickly to increases in competition. Such an explanation is in line with our hypothesis.

the effect of worse governance and increase for the average firm, due to lack of data we are unable to explore how this effect is affected by firms' ownership structures.

4.3. Robustness to industry characteristics

We next explore the robustness of our main result in a number of tests. We start by examining whether import tariffs in fact proxy for some inherent industry characteristics. We then look into whether our assumption of parallel trends holds, and also check results while excluding multisegment firms and firms incorporated in Delaware. Finally, we provide alternative computations of standard errors and dependent variables, as well as methods for dealing with outliers.

One concern is that the effect of BC laws could differ across industries for reasons other than competitive pressures. Thus, the reduction in import tariffs might instead reflect some inherent differences across industries that are typically correlated with the protection from foreign competitors. To tackle this issue, we control for a number of industry characteristics as well as for their interaction with the BC law dummy. As we show in Table 6, the inclusion of these controls does not substantially affect our results.

First, industries that are the least competitive globally might be protected by higher import tariffs, yet also be the most affected by the worsening corporate governance. We therefore control for a time-invariant measure of the average import tariffs that a firm faced before the FTA. This variable is related to the static characteristics of the industry such as its global competitiveness. A statistically significant coefficient for the interaction between BC laws and average import tariffs would suggest that the least efficient industries are the most affected by worsening corporate governance. The results are reported in Column (1). This coefficient is not statistically significant, but the interaction between BC laws and the reduction in import tariffs remains significant. Hence, the negative effect stems from changes in competition and not from static industry characteristics.

Second, we provide a specification including the interaction between BC laws and lagged HHI in order to control for the differing effects of BC laws on concentrated versus competitive industries, as documented in Giroud and Mueller (2010). In Column (2) we again find a negative and significant effect for the interaction between BC laws and the tariff cut. The latter result also holds if HHI when estimated contemporaneously.

Third, in Column (3), we follow Guadalupe and Wulf (2010) by including a set of pre-FTA industry characteristics that are typically related to trade protection: skill intensity, capital intensity, and TFP growth over 1986-89, as well as their interaction with the post-FTA dummy. These controls allow us to further absorb the effect of observable industry differences potentially related to the magnitude of the tariffs cut. We find that none of these controls significantly affect our main findings. Finally, in Column (4), we include all controls separately used in Columns (1)-(3) and again find a significant effect for our coefficient of interest.

4.4. Further robustness checks

In Table 7, we assess the robustness of our findings in a number of additional ways. An important concern about our identification strategy is the possible violation of the parallel trends hypothesis concerning the implementation of BC laws and the FTA. Previous literature offers arguments that support the abrupt adoption of both BC laws and the FTA. Romano (1987), who investigates the adoption of BC laws from a political viewpoint, claims that such legislature is typically advocated not by a larger coalition of firms but rather by a single firm facing a threat of imminent takeover. That only a few firms lobbied for BC laws and that they were often adopted during emergency legislative sessions without public hearings should mitigate the concerns for endogeneity (Bertrand and Mullainathan 2003). Moreover, passage of the FTA was highly improbable and unexpected (Guadalupe and Wulf 2010). Its fate was decided in the Canadian federal election, which was won by the Progressive Conservative party, in favor of the FTA, after trailing in the polls to the Liberal party that opposed the agreement. We address these concerns empirically by estimating placebo policy changes three years before their actual passage (Columns 1 and 2). Results indicate that neither placebo BC laws nor the placebo FTA are statistically significant. Their economic effects are also less economically pronounced than our baseline finding in Table 4, Columns (1) and (2). These results confirm that our sample exhibits no diverging trends, due to e.g. selection of worse-governed firms into BC states prior to the actual law passage, that could confound our findings.

An additional concern of our specification is that the control variables (e.g. firm size) might themselves be responsive to the policy changes and/or be correlated with omitted factors. In Column (3), we therefore provide the results for a specification that controls only for time and firm fixed effects.

Moreover, since most of the firms in our sample are incorporated in Delaware (see Table 1), our results could reflect some non-governance related changes in the legislature of Delaware-incorporated firms. Yet the results reported in Column (4) show that our findings are robust also to the exclusion of firms that incorporated in Delaware.

Our treatment measuring the reduction in import tariffs relies on the correct assignment of firms to industries. Since we only use the primary segments reported in Compustat for each firm, the FTA treatments might suffer from measurement errors for firms that are active in multiple segments. To address this concern, we restrict our analysis to single-segment firms, as inferred from the number of segments for which Compustat Segments database reports sales. The results in Column (5) indicate that the interaction between BC laws and reduction in import tariffs is statistically significant at the 10% level and economically relevant.

Additionally, we address the issue of the timing of BC laws. Our baseline results (Table 4) estimate the interaction of an increase in competition with BC laws regardless of whether the BC laws were passed before or after the FTA. As shown in Figure 1, seventeen states (in which 79.1% of our sample firms are incorporated) passed BC laws before the FTA, eight states did so in 1989, while five states passed BC laws in 1990-91. Since we aim to identify how a change in governance affects the response to a subsequent change in competition, our analysis could be biased by including even the few states that passed BC laws after the FTA. Hence, we exclude firms incorporated in states that passed BC laws in 1990 and 1991, and the results, shown in Column (6), are robust to this analysis.

A possible source of selection bias in our estimates is represented by entry and exit effects. New firms may choose where to incorporate depending on whether a BC law was present or not in their headquarter state. Similarly, worse-governed firms in states without a BC law may be more prone to exit from the sample. We reduce these potential biases by estimating our baseline model for a subsample of firms that are present in the dataset from 1981 until 1995 (i.e. the last year in our sample). Results reported in Column (7) confirm our main finding.

We also provide alternative computations of the standard errors. We estimate our baseline regression by clustering at the four-digit industry level (Column 8), to allow for any intra-industry correlation of residuals induced by the FTA. We adopt treatments on two different dimensions and we are interested in the interaction between them. Thus, since our specification is identified at both the industry and the state of incorporation levels, we employ two-way clustering at the levels of industry and state of incorporation. These results are reported in Column (9). In untabulated regressions, we also cluster residuals at the levels of firm and at the state of location. Although the precision of our estimates varies, the interaction between BC laws and reduction in import tariffs remains statistically significant at conventional levels. The interaction coefficient remains significant at the 5% level even when we compute standard errors by block-bootstrap using 200 replications (Bertrand et al. 2004).

Finally, we adopt several ways to deal with outliers. In our baseline estimates, we trim 1% at each tail of the ROA distribution. We obtain similar results, however, if we exclude firms with assets of less than \$1 million, if we trim 1% at each tail of the distribution of total assets, and if we estimate a median regression (including industry fixed effects and bootstrapping standard errors using 100 replications). Our results are also robust to the adoption of alternative measures of performance. These include sales divided by assets, net profit margin (computed as EBITDA divided by sales), ROA after depreciation (computed as operating income after depreciation divided by total assets), and ROE (computed as EBITDA divided by book value of common equity).[48]

4.5. Alternative measures of corporate governance and competition

In addition to using BC laws to identify variations in corporate governance, we use a firm-level measure of corporate governance to show that the FTA had a more negative effect on the worse-governed firms. Hartzell and Starks (2003) find that institutional ownership concentration is associated with greater pay-for-performance sensitivity and lower executive compensation, both of which reduce agency problems between shareholders and management. Furthermore, Ferreira and Matos (2008) show that institutional investors are more likely to invest in better-governed firms. Nikolov and Whited (2009) further claim that, given the measurement problems associated with other proxies, institutional ownership should be the preferred proxy for firm-level corporate governance. Following these studies, we adopt the fraction of institutional ownership in the firm as a proxy for the quality of its corporate governance. We estimate an equation in which *Import tariff cuts*$_{tj}$ is interacted with the fraction of firm's stock owned by institutional investors. Controlling for firm fixed effects allows us to look at within-firm variations in institutional ownership.

The results of this estimations are reported in Table 8.[49] First, Column (1) shows that there is a positive relation between profitability and ownership by institutional

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[48] We conduct a number of additional robustness checks that are available upon request. First, we compute the average tariffs for the entire period before the FTA was passed (from 1976 to 1988). In computing the average tariff for the pre-FTA period, we had to exclude one industry in 1978 that reported an implausibly high tariff. Second, we adopt a symmetric window around the FTA passage (1982-95). Third, we restrict the analysis to manufacturing sectors (SIC between 2000 and 4000). Fourth, we only consider only the cases involving tariff reductions that are not extreme (i.e. strictly larger than 0% and lower than 8%). Fifth, we control for leverage and/or cash holdings. All results confirm our previous findings, in both economic and statistical terms. Finally, we include the square of reduced import tariffs and its interaction with BC laws, and our main results remain largely unchanged. The latter interaction of BC laws with the squared term of reduced import tariffs is not significant by itself, a result that implies the absence of the non-linear effect of reduced import tariffs on operating returns.

[49] Our main specification restricts the sample of firms with a non-missing value for extent of institutional ownership; however, our results are robust to using the full sample where the missing values of institutional ownership are replaced with 0.

investors. Moreover, in support of our hypothesis, we find that the coefficient for *Import tariff cuts*$_{tj}$ is negative whereas the interaction term for this variable and institutional investor ownership is positive. Thus we find that reduced tariffs have a negative effect only for the firms with a small institutional investor base, i.e. firms with worse governance.[50]

Finally, we employ import penetration at the industry level as an alternative measure of import competition, estimating an equation in which import penetration is interacted with the dummy indicating the passage of BC laws. Import penetration is defined as the ratio of imports to imports plus domestic production in a given industry and year. Because import penetration can be endogenous to an industry's profitability, we follow Bertrand (2004) and instrument it with the weighted average of the real exchange rates of the importing countries. In particular, we construct the weights for each industry from the shares of each foreign country's imports in the total imports of that industry. As in Bertrand (2004), we fix these shares of foreign country's imports at their year 1981 levels. We then use both the current and one-year lagged weighted real exchange rates as instruments for import penetration and use the interaction of these exchange rates with BC laws as an instrument for the interaction of import penetration with BC laws. The results reported in Column (2) show that greater foreign competition reduces profitability, although it mainly affects the profitability of firms with worse corporate governance.

Although the results using alternative measures are in line with our main findings, we prefer to use the FTA and the passage of state-level BC laws as the ways to identify our effect because they provide more exogenous variations in corporate governance and in import competition.

[50] Our results are robust to using membership in the S&P500 index (as in Aghion et al. 2009) to instrument the fraction of institutional ownership.

5. Market values

We complement our results on operating performance by studying stock market reactions to the FTA. If corporate governance indeed reduces the readiness to react to competition and if markets efficiently incorporate this information, then a stock's market value should decrease more for firms with worse corporate governance than for firms with better governance following the implementation of the FTA. We first provide results for the panel estimation of market-to-book ratios. We then conduct an event study on how announcement of the results of the Canadian federal election results, which significantly increased the FTA's probability of being adopted, affected the stock prices of U.S. firms.

5.1. Market-to-book ratios

Our first dependent variable is the ratio of market value of equity to book value of equity (the MB ratio). We again control for firm fixed effects, which absorb firm-level differences in valuation; for average MB ratios of industry and state of operation, which capture industry and local economy trends; and for time-fixed effects, which control for general market movements. The resulting analysis can therefore be interpreted as estimating the effect of BC laws and reduced tariffs on long-term equity returns. The results are reported in Table 9. In Column (1), we report estimates using the same specification as in the ROA regressions. We find that the coefficient for the interaction between BC laws and reduced tariffs is negative and significant: firms exposed to the worse governance experience a decrease in market value following the increases in competition. In Columns (2)-(4) we sequentially include other controls, typically associated with market value: ROA, extent of leverage and ratio of R&D expenditures to sales. We use one-year lagged values in order to reduce the concern that these controls are simultaneously affected by the FTA directly. In Column (5), we include all controls together. Again, our estimates indicate a significant decrease in the market value of firms that are both subject to increasing competitive pressures and incorporated in states with BC laws.

A consistent finding for the market value of equity also eliminates an additional concern regarding our results on profitability. One could argue that firm's investment may follow decreasing returns to scale, in which case a firm could continue to expand even if its average ROA declines. If either the passage of BC laws or the reduction in import tariffs increases the sizes of firms, we could have misinterpreted our findings on ROA. However, the decline in market values of firms incorporated in states with BC laws runs counter to such interpretation of our profitability results.

5.2. Event study

Accounting-based measures can be manipulated in response to worsened corporate governance and the competitive shock. Moreover, the annual panel data cannot fully absorb the endogeneity of the phase-out schedules of tariffs. To mitigate these concerns, we perform an event study to test whether the FTA's adoption had a different immediate impact on the stock prices of U.S. firms incorporated in states with and without BC laws. Morck et al. (2000) and Breinlich (2010) summarize the political events around the implementation of the FTA. Contrary to the political process in the U.S., the debate about the adoption of the FTA was very contentious in Canada. After the agreement was signed between U.S. and Canada in October, 1988, the legislation to implement it stalled in Canada's Senate. Brian Mulroney, Prime Minister at the time, called federal election for November 21, 1988. The FTA was the main issue in the election and the outcome of the election was highly uncertain. Although Progressive Conservatives won the majority, a Gallup poll published two weeks before the election still showed a 12% lead in favor of the Liberal Party, which opposed the implementation of the FTA. The uncertainty surrounding this election offers an ideal context for conducting an event study that examines the U.S. stock market reaction to the FTA.

First, since all firms within the same industry are affected to a similar extent and since their abnormal returns are likely to be correlated (MacKinlay 1997), we form industry-level portfolios. Second, for each of these portfolios we estimate cumulative abnormal stock returns over several event periods surrounding the election date: [-20,-1], [-5,-1], [-1,0], [0,0], [0,1], [0,3], and [0,5], where [-1,0] for example, denotes a two-day event window. Cumulative abnormal returns (CARs) are calculated as the difference between actual returns (extracted from CRSP, the Center for Research in Security Prices) and expected returns, where the latter are projected using a market model with the parameters estimated from 241 to 41 trading days prior to November 21, 1988. We then test whether the average CARs of these 326 industry portfolios are statistically different from zero for each event window.

The results are given in Table 10. Columns (2)-(4) confirm that a greater reduction in tariffs led to a decline in stock prices, a finding that validates our identification strategy. This effect shows up in all the time windows adopted from [0,0] to [0,5]. For instance, the six day return was -1.25% for firms operating in industries subject to large tariffs reductions but was not significantly different from zero for the other firms.

Finally, in the same manner as for the industry portfolios, we form portfolios at the level of state of incorporation, estimate cumulative abnormal stock returns over the same event windows and test for whether the average CARs of these state-level portfolios are statistically different from zero for each event window. In Columns (5)-(7), we document that firms incorporated in states subject to BC laws experienced a larger decline in stock prices. Again, the effect shows up in the various time windows from [0,0] to [0,5]. A six day return was -1.44% for firms subject to BC laws but not significantly different from zero for other firms. Overall, the event study evidence confirms our previous findings that firms incorporated in environments associated with worse corporate governance were less prepared to face an increase in competitive pressures.

6. Exporters

FTA affected both import and export side of trade. We thus separately look into how firms were affected by the reduction in the export tariffs. We first distinguish between exporters and non-exporters. We expect the results for these two groups to differ for two reasons. First, exporters might have benefited from the expanded business opportunities in Canada due to the reduction in export tariffs. Second, exporting firms are typically associated with a high level of productivity (e.g. Clerides et al. 1998, Delgado et al. 2002; Bernard and Jensen 2004), hence the effect of import tariffs should be lower for these firms.[51] To account for both effects, we estimate separate regressions for exporters and non-exporters, including the reduction of both import and export tariffs as well as their interactions with BC laws. It is important for our identification that the firms in our sample do not change their exporting status after 1988 – in other words, that there is no effect from the FTA on the extensive margin to export.

Results, reported in Table 11, Columns (1)-(3) show that our previous findings on import tariff cuts are concentrated in the sample of non-exporters. The interaction between BC laws and import tariffs is both significant (at the 10% level) and economically large for this group of firms. On the contrary, neither export tariffs cut, nor its interaction with the BC laws are significant at conventional levels. Thus, non-exporting firms were negatively affected by the FTA mostly through the increase in competition, and this negative impact was especially strong in environments characterized with poor corporate governance.

On the contrary, we find that for exporters the reduction in export tariffs is positive and significant. This result suggests that exporters were able to benefit from the cut in export tariffs to Canada, which expanded their product market. Moreover, we find that the interaction between the cut in export tariffs and BC laws has a negative coefficient of almost similar size, indicating that even though exporters were positively

[51] Another reason why exporters might have been less affected by reduced import tariffs is that their production inputs are more likely to be imported (Bernard et al. 2009), which means that exporters are more likely to benefit from the reduction in import tariffs on their supplies.

affected by the FTA, worse corporate governance impaired their ability to benefit from the increased opportunities for exporting to Canada.

Due to the data limitations we are not able to distinguish whether the firm is in fact exporting to Canada, or it only exports to the other countries. However, we can look whether our results differ across industries. In particular, some industries have higher fraction of exports to Canada than other industries, and thus we sort exporting firms according to whether their industry mainly exports to Canada. We use data from Schott (2008) and consider industries to be prone to exports to Canada if their share of exports to Canada over all exports in 1985 was larger than 15%. This figure roughly corresponds to the sample median. When we split our sample of exporters, we indeed find that export tariff reduction had a positive effect for exporters that operate in the industries with large export share to Canada (Columns 4 and 5).

7. Why does governance matter?

So far we have shown that worse corporate governance amplifies the negative effect of an increase in competition on operating and stock market performance. These results can be explained in two ways. First, managers that are protected from hostile takeovers can become entrenched and thus exert less effort while an increase in competition would likely require additional effort to remain competitive in the market and to sustain profits. For this reason, firms with worse governance should be the ones affected more negatively by the FTA. Second, managers in firms with worse governance could be more constrained than those in better-governed firms what regards the actions that they can take. Thus, even though these managers are willing, they may be unable to respond adequately to increased competition.

We examine whether the performance of worse-governed firms deteriorated more after the FTA solely because of unwillingness to adapt, or rather also because some constraints jeopardized firm's ability to do so. In particular, we look at one reason why corporate governance is important for how firms react to an increase in competition: a close relationship between corporate governance and financial constraints. Because increased competition requires firms to reorganize their activities, access to external finance may play an important role in adapting to the FTA. Rajan and Zingales (1998) show that, as an industry becomes more dependent on external finance, the availability of outside capital becomes more important. Also, greater credit constraints limit a firm's ability to react to trade liberalizations (Manova 2008). Yet the quality of corporate governance establishes the terms on which firms can raise external funds and that agency problems increase the cost of external finance.[52]

Our procedure is based on three different tests. First, we link our findings on profitability to financial constraints; that is, we check whether firms that were ex ante more financially constrained were more affected by the governance and trade shocks. Second, because financing decisions are endogenous to competition (Phillips 1995; Kovenock and Phillips 1997), we look at an exogenous increase in the need for finance. We want to see whether the latter mainly affected firms exposed to both BC laws and the trade shock, namely, those that we claim to be more financially constrained. In particular, we investigate effects of a sharp spike in oil prices during the first Gulf War in late 1990. Because this oil price spike unexpectedly drained resources of firms with negative exposure to oil prices, the event provides an exogenous source of variation in the need for external financing. Third, we directly test whether firms exposed both to BC laws and to reduced import tariffs actually raised less external finance.

7.1. Financial constraints and performance

First, we check whether firms operating in industries more likely to require external finance suffered more in terms of operating performance. We therefore classify firms based on whether the industry in which they operate was above or below the across-industry median of external financial capital raised in 1984 (i.e., one year prior to

[52] In an extended version of this article, we discuss one explanation for why external finance activity was important after the FTA: industries that raised more external finance were better able to resist price pressure.

passage of the first BC law).[53] Our measure is similar to Rajan and Zingales's (1998) proxy for an industry's financial constraints. The results, which are reported in Columns (1) and (2) of Table 12, indicate that the negative effect on operating returns of reduced import tariffs was mainly concentrated among firms incorporated in states with BC laws *and* operating in industries that are highly dependent on external finance.[54]

Second, we split the firms according to whether they had an S&P long-term debt rating in 1985. Firms can issue public debt only if they have a bond rating and thus access to this additional source of capital reduces financial constraints. Results from these regressions, reported in Columns (3) and (4), also show that the combined negative effect of the FTA and worse corporate governance on operating returns was concentrated among firms that did not have credit rating, i.e. those that were ex ante more financially constrained firms.

Finally, we use firm size and age as indirect measures of financial constraints. As discussed in Almeida et al. (2004), smaller and younger firms are less well known and more vulnerable to capital market imperfections. In Columns (5) and (6), we estimate separate regressions for firms that are smaller or larger than their industry peers. Small (large) firms are defined as firms having assets below (above) the industry median in 1984, one year before the passage of the first BC law. We find that the impact of BC laws is close to zero and insignificant for larger firms, perhaps because their size already rendered takeovers less likely. In contrast, the BC laws had a large and negative effect on the ROA of smaller firms. Although the coefficient for the interaction between BC laws and the FTA is negative and significant at conventional levels for both large and small firms, its economic magnitude is much greater for smaller firms. This result indicates that smaller firms subject to worse corporate governance were most negatively affected by an increase in competition. Columns (7) and (8) report similar results for younger and older firms, again defined relative to the industry medians in 1984.

[53] The average import tariffs were not statistically different for any of the pairs of subsamples studied in this section.

[54] An industry's net change in capital can be assessed by balance sheet measures or instead by security issuances reported in the SDC New Issues database. Our results are consistent using either method. For the sake of brevity, we report results only for a sorting based on the balance sheet measures.

So far we established that interactions between corporate governance and ex ante financial constraints were important factors in firms' responses to increased competition. We now look more carefully at the identification to establish whether financial constraints were one of the main channels through which corporate governance affected the reaction of firms to the FTA.

7.2. Exogenous variation in financing needs

Here we exploit an exogenous variation in the financing needs of U.S. firms. We look into the first Gulf War which began with the invasion of Kuwait by Iraqi armed forces on August 2, 1990, and resulted in a spike in the price of oil that did not subside until U.S. military action commenced in January, 1991. Our test builds on the notion that firms more negatively exposed to oil prices experienced an unexpected drain of resources following the oil price spike.[55] The production costs of such firms might have been tied to oil, or perhaps their business activities relied on discretionary consumer spending and were thus exposed to the inflation shocks. Hence, the effect of the resulting unexpected change in financing needs differed across firms as a function of their exposure to oil prices. Of these firms, we assume that the least financially flexible ones were most hard hit by the oil price spike.[56]

This setting offers an excellent opportunity to test whether a combination of BC laws and the trade shock[57] had an effect on financial constraints. In particular, we test whether the effect on ROA in 1990 was stronger for firms that experienced a combination of (1) large tariffs cut; (2) passage of BC laws; (3) negative exposure to the oil price shock. If the interaction between BC laws and reduced tariffs had no effect on

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[55] Lamont (1997) showed that 1986 oil price shock affected financing costs for the firms dependent on oil.

[56] These claims are in line with the anecdotal evidence. For instance, in an article entitled "Junk Defaults May Rise in U.S. Recession Climate", the news service Reuters reported on August 8th, 1990 that "If oil goes to 30 dollars a barrel and inflation rises in excess of six pct, this will have a general negative impact on the junk bond market" and that "Analysts said that highly leveraged credits with limited financial flexibility will be hard hit as cash coverage for debt repayment declines."

[57] In unreported results, we study the dynamics of the FTA effect and find that it does not diminish over the first years following the adoption. Here we exploit this finding that the trade shock had not yet been fully absorbed by U.S. firms in 1989.

financial constraints, then we should expect the triple interaction *not* to be significant.

We estimate the firm's exposure to oil prices by using the correlation of daily stock returns and changes in the WTI crude oil spot price in 1989. As such, we follow Adler and Dumas (1984) who suggest inferring a firm's exposure to currency risk from how its stock price correlates with exchange rate changes. For the sake of easier interpretation, we take the negative value of this correlation as the firm's exposure to the oil price spike. We estimate the following regression:

$$\begin{aligned} ROA_{ijkt} &= \alpha_i + \alpha_t + \beta_1 Import\ Tariff\ cuts_{jt} + \beta_2 BC_{kt} + \beta_3 BC_{kt} \times Import\ Tariff\ cuts_{jt} + \\ &+ \beta_4 Oil\ spike\ exposure_{it} + \beta_5 BC_{kt} \times Oil\ spike\ exposure_{it} + \\ &+ \beta_6 Import\ Tariff\ cuts_{jt} \times Oil\ spike\ exposure_{it} + \\ &+ \beta_7 BC_{kt} \times Import\ Tariff\ cuts_{jt} \times Oil\ spike\ exposure_{it} + \gamma' \mathrm{X}_{ijkt} + e_{ijkt} \end{aligned} \quad (2)$$

The term *Oil spike exposure*$_{it}$ is equal to the correlation between the oil price and the firm's stock price (estimated as just described) in 1990 and is equal to 0 in all other years. The remaining variables are as defined previously. The coefficient for our key variable of interest here, BC_{kt} × *Import tariff cuts*$_{tj}$ × *Oil spike exposure*$_{it}$ measures whether *Oil spike exposure*$_{it}$ affects performance differently depending on BC_{kt} × *Import tariff cuts*$_{tj}$. The null hypothesis is that BC_{kt} × *Import tariff cuts*$_{tj}$. does not proxy for financial constraints and so an increase in financing needs affected firms' operating returns uniformly, (i.e., that $\beta_7 = 0$). However, we would expect a negative value of β_7 if the interaction between worse governance and increased competition led to greater financial constraints, and thus the firms with more binding financial constraints would experience a more negative effect on performance in response to increased financing needs.

Table 13 reveals a negative and statistically significant effect on ROA for firms that were subject to BC laws and exposed both to the oil shock and the trade shock, in other words for firms that needed finance the most but were the least able to raise it. The result holds if we include our usual control variables as in our baseline specification, or if we control for the export tariff cut.

As coefficient for BC_{kt} × *Import tariff cuts*$_{tj}$ is not significant in these regressions, one could worry that our baseline estimations in Section 4 in fact identify an effect of oil spike rather than the trade shock. However, it should be noticed that the analysis here is performed on a sample of firms that is smaller than that used for previous estimations, because continuous stock price data are needed to estimate *Oil spike exposure*$_{it}$. In order to perform the analysis on the full sample, we replace missing values of *Oil spike exposure*$_{it}$ with zero. The results reported in Column (4) show that coefficients for both BC_{kt} × *Import tariff cuts*$_{tj}$ as well as for its interaction with *Oil spike exposure*$_{it}$ are significant and negative, confirming that worse governed firms were affected most by the trade shock but their performance further deteriorated in 1990 if they were negatively exposed to the oil price changes. The additional drop in ROA for a firm with an average oil spike exposure amounted to 0.5%.

In unreported results we also perform splits similar to those used in our baseline regressions, thereby checking whether β_7 is more negative for firms that are more affected by the trade shock. Indeed, the effect is again concentrated among non-exporters and firms closer to Canada. Moreover, we find a greater effect for firms that are ex ante more financially constrained firms (i.e. firms in industries characterized by higher levels of external financing activity as well as non-rated and younger firms).

7.3. Actual external financing activity

Finally, we look at the actual issuance of new external capital. This analysis does not distinguish between firms that have more constrained supply of finance and firms that demand less external finance. Yet it provides supporting evidence that after the trade shock external financing activity differed for better- and worse-governed firms.[58]

Table 14, Column (1), gives estimates for a specification in which the dependent variable is total net change in capital as a fraction of the firm's book value of assets. In addition to the usual explanatory variables adopted in our profitability regressions, we

[58] In untabulated results, holding the firm effects fixed, we find a positive and significant (at 1%) association between ROA and the external financing activity.

control for the beginning-of-year leverage as well as for the general issuance activity by industry and state of location. First, we show that being subject to BC laws reduces fund-raising.[59] We also show that this reduction is larger for firms that face the trade shock. In fact, firms incorporated in states *without* BC laws increase their external financing activity in response to reduced import tariffs, whereas firms in states *with* BC laws make not changes in that regard.

As a robustness check, we collect data on new capital raised from the SDC New Issues database. In particular, we consider all types of securities (bonds, secondary equity offerings, and other type of securities) that firms issue over the year and that are reported in the database.[60] Our main dependent variable is the proceeds from the issue of securities as a fraction of total assets in the preceding year. We restrict this variable to lie between 0 and 1. If SDC New Issues does not report data on the issuances of a particular firm, we assume that no issuances were made by that firm. Estimates, reported in Column (3), confirm our previous results; following a large reduction in tariffs, a firm is less likely to raise funds if it is incorporated in a state with BC laws.

8. Conclusion

We investigate how the quality of corporate governance affects a firm's performance following an increase in foreign competition. Our empirical approach is based on the intersection of two policies implemented in the United States at the end of 1980s. On the one hand, business combination laws reduced the threat of hostile takeovers, thus rendering ineffective an important corporate governance device: the market for corporate control. On the other hand, the Canada-U.S. Free Trade Agreement increased foreign competition for U.S. manufacturing firms as tariffs on Canadian imports were abolished. We adopt a combination of difference-in-differences models based on the

[59] Consistently with this argument, Qiu and Yu (2009) find that the cost of new debt rose after the passage of BC laws.

[60] One concern is that SDC New Issues does not report borrowing from banks. Therefore, even if firms with worse corporate governance raise less capital via the publicly listed securities, they could substitute it with more capital from the banks.

observations that (i) states passed BC laws at various times (mostly before the passage of the FTA), while (ii) because of the FTA industries experienced different levels of reduction in import tariffs – and thus varying increases in foreign competition.

Our main finding is that corporate governance has a positive effect on a firm's readiness to compete in the product market. The exposure to BC laws magnifies the negative effect of the import tariff cuts on operating returns and firm value. Furthermore, we find that the negative effect of BC laws on operating returns following the trade shock is predominantly concentrated among non-exporters, low-productivity firms, and firms located closer to the Canadian border.

Our evidence is consistent with the "quiet life" notion that managers in firms with worse governance are unwilling to undertake the actions needed to face an increase in competition. We also provide evidence that worse governance exacerbates financial constraints that hamper the ability to react to competitive shocks.

In the face of stronger competition, an increase in firm-level productivity can stem either from a uniform rise in the productivity of all firms or from forcing the least efficient firms to exit the market. Worse-governed firms could thus be less ready to adjust their actions or alternatively they might have a higher probability of going bankrupt. Our findings focus on worse-governed firms improving less than their peers.[61] However, future research could further discriminate between these two explanations and explore the role of other corporate governance mechanisms.

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[61] In fact, after the passage of the FTA we observe somewhat more bankruptcy filings in the states with BC laws. According to the data from BankruptcyData.com and the UCLA-LoPucki's Bankruptcy Research Database, there were 8 filings for Chapter 11 by our sample firms incorporated in states with BC laws (0.24% of the sample firms) versus 20 filings in states without BC laws (0.14%) over 1983-88. However, for 1989-93 there were 94 Chapter 11 filings in states with BC laws (0.70%, an increase by 2.9 times) versus 9 such filings in states without BC laws (0.37%, an increase by 2.6 times). Such anecdotal evidence suggests that the FTA may have had a greater effect on the exit of firms with worse corporate governance than on those with the better governance.

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Appendix. List of variables

Variable Name	Description	Source
	Governance Characteristics	
BC (or BC law present)	Dummy variable, set equal to 1 starting from the year when the BC law was passed by the state where the firm is incorporated (and to 0 otherwise); see Table 1 for the listing of passage dates.	
Institutional ownership	Fraction of firm's outstanding shares that are held by institutional investors.	Thompson Financial CDA/ Spectrum
	Competition Variables	
Pre-1989 tariffs	Average tariffs on imports of Canadian goods during the period 1986-88 for each four-digit SIC industry. For each year tariffs are estimated as the total duties paid across all sub-industries (of each four-digit SIC industry) divided by the total customs value of imports.	UC Davis Center for International Data
Import tariff cuts	Change in the tariffs on imports of Canada. Before 1989 it is equal to 0, in and after 1989 it takes a positive value equal to pre-1989 tariffs (see description of Pre-1989 tariffs variable).	UC Davis Center for International Data
Export tariff cuts	Change in the tariffs on exports of U.S. goods to Canada. Before 1989 it is equal to 0, in and after 1989 it takes a positive value equal to pre-1989 export tariffs. Pre-1989 export tariffs are estimated as the average over 1986-88 for each four-digit SIC industry.	Trefler (2004)
High (resp. low) tariff	Dummy, set equal to 1 if Pre-1989 tariffs exceeds (resp. does not exceed) 0.033 and set to 0 otherwise.	UC Davis Center for International Data
HHI	Herfindahl-Hirschman index, computed as the sum of squared market shares of all publicly listed firms (based on sales), in a given three-digit SIC industry in each year.	Compustat (or U.S. Census)
Import penetration	Dollar value of imports divided by the sum of dollar value of imports plus the dollar value of domestic production in a given four-digit SIC industry.	Schott (2008)
Source-country weighted real exchange rate	Weighted average of real exchange rate of the U.S. dollar versus other currencies. For any given four-digit SIC industry, the weights are the shares of each foreign country's imports in the total imports of that industry, fixed in 1981.	Datastream
PPI	Annual average (M13) of the Producer Price Index for a given four-digit SIC industry.	Bureau of Labor Statistics
	Firm Characteristics	
Ln (age)	=ln(*age*+1), where *age* is the number of years that the firm has been in Compustat.	Compustat
Asset size	=ln(*at*), where *at* is the size of assets, in millions of U.S. dollars.	Compustat
ROA	$=ebitda/at_{t-1}$, where *ebitda* is the earnings before interest, taxes, depreciation and amortization and where *at* is the size of assets.	Compustat
Leverage	=(*dlc*+*dltt*)/*at*, where *dlc* is the amount of financial debt due in one year, *dltt* is the amount of long-term financial debt and *at* is the size of assets.	Compustat
Market-to-book	=(*prcc_f* ×*cshtr_f*)/*ceq*, where *prcc_f* is the market price of a common share at the end of the fiscal year, *cshtr_f* is the number of common shares outstanding and *ceq* is the book value of equity. This variable is limited to the interval between 0 and 10.	Compustat
R&D/Sales	=*xrd*/*sale,* where *xrd* is the amount of R&D expenditures and *sale* denotes the annual sales.	Compustat
Large (resp. small) firm	Dummy variable, set equal to 1 if the firm's asset size of the firm is greater (resp. lower) than the median size of the firms within the firm's three-digit SIC industry in 1984 and set to 0 otherwise.	Compustat
High (resp. low) TFP firm	Dummy variable, set equal to 1 if the firm's total factor productivity (TFP) of the firm is greater (resp. lower) than the median TFP of the firms within the firm's three-digit SIC industry in 1984 and set to 0 otherwise; here TFP is estimated using the procedure described by Olley and Pakes (1996). The firm-level variables used to compute TFP are	Compustat

	the logarithms of sales, employment, capital expenditures, and property, plants and equipment.	
Young (resp. old) firm	Dummy variable, set equal to 1 if the firm's age is greater (resp. lower) than the median within the firm's three-digit SIC industry in 1984 and set to 0 otherwise.	Compustat
Closer to (resp. farther from) the border	Dummy variable, set equal to 1 if the distance from the principal city of the state in which the firm's headquarter is located is less (resp. more) than 300 miles from the nearest road crossing of U.S.-Canada border and set to 0 otherwise.	Various
Exporters (resp. non exporters)	Dummy variable, set equal to 1 if the firm reports an average of at least (less than) 1% of export to sales and set to 0 otherwise. Due to lack of export data by destination country, we consider the overall exports and not only export to Canada.	Compustat
Industries with high (resp. low) exports to Canada	Dummy variable, set equal to 1 if the industry's share of exports to Canada over all exports in 1985 is higher (resp. lower) than 15%.	Schott (2008)

State (Industry) Trends

State-year	Average of the dependent variable across all firms in the same state of location of the firm, where averages are computed excluding the firm in question.	Compustat
Industry-year	Average of the dependent variable across all firms in the same four-digit SIC industry of the firm, where averages are computed excluding the firm in question.	Compustat

Financing Variables

Net change in capital	$=((dd1_t+dltt_t)-(dd1_{t-1}+dltt_{t-1})+(sstk-prstkc))/at_{t-1}$, where *dd1* is the amount of financial debt due in one year, *dltt* is the amount of long-term financial debt, *sstk* is the amount of newly issued common and preferred stock, *prstkc* is the amount of repurchased common and preferred stock and *at* is the size of assets. This variable is limited to the interval between 0 and 1.	Compustat
Net change in equity	$=(sstk-prstkc)/at_{t-1}$, where *sstk* is the amount of newly issued common and preferred stock, *prstkc* is the amount of repurchased common and preferred stock and *at* is the size of assets. This variable is limited to the interval between 0 and 1.	Compustat
Net change in debt	$=((dd1_t+dltt_t)-(dd1_{t-1}+dltt_{t-1}))/at_{t-1}$, where *dd1* is the amount of financial debt due in one year, *dltt* is the amount of long-term financial debt, and *at* is the size of assets. This variable is limited to the interval between 0 and 1.	Compustat
High (resp. low) capital intensive industry	Dummy variable, set equal to 1 if the four-digit SIC industry's net change in capital is greater (resp. lower) than the median net change in capital across all industries in 1984 and set to 0 otherwise.	SDC, Compustat
Bond rating	Dummy variable, set equal to 1 if, in 1985, the firm has been assigned a long-term bond rating by Standard & Poors and set to 0 otherwise.	Compustat
Oil spike exposure	Negative of the correlation between the daily returns on a firm's stock price and changes in the WTI crude oil spot price, where correlation is estimated over 1989.	CRSP, Datastream

Figure 1. Sequence of BC law legislation with respect to the FTA

This figure shows the cumulative number of states and firms subject to BC laws. The vertical line in 1989 indicates passage of the FTA.

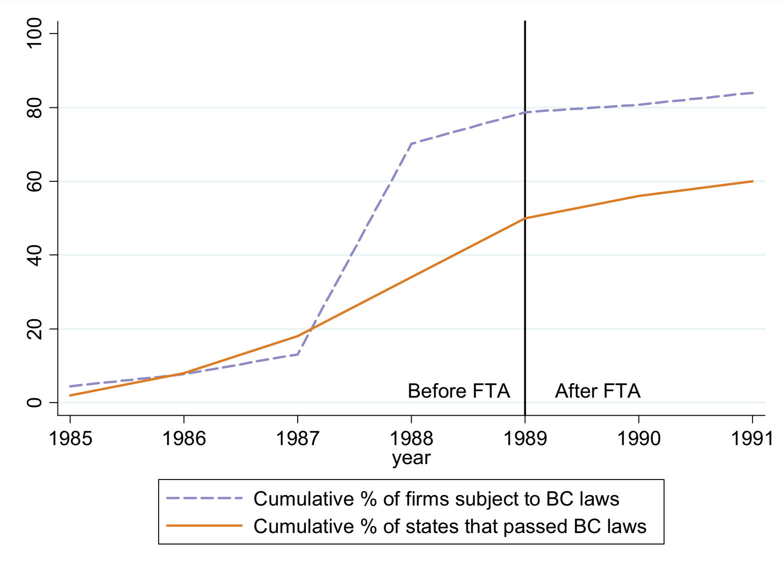

Figure 2. Identification

This figure depicts our identification strategy considering, to simplify the illustration, states that passed BC laws one period before FTA.

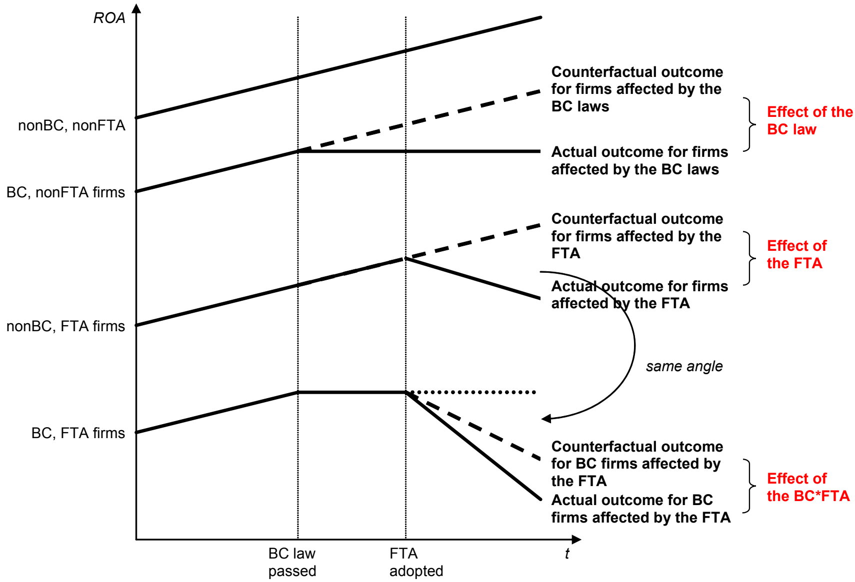

Table 1. States of incorporation and states of location

This table shows the number of firms by state of location (state in which a firm's headquarter is located) and incorporation. "BC year" is the year in which a business combination law was passed in the state.

		Number of firms		Number (%) of firms incorporated in					
State	BC year	State of incorporation	State of location	State of location	(%)	Delaware	(%)	Other states	(%)
Delaware	1988	1956	5	5	100.0	0	0.0	0	0.0
California		199	667	184	27.6	423	63.4	60	9.0
New York	1985	158	297	103	34.7	173	58.2	21	7.1
Colorado		115	160	70	43.8	68	42.5	22	13.8
Minnesota	1987	112	138	96	69.6	37	26.8	5	3.6
Massachusetts	1989	103	199	87	43.7	102	51.3	10	5.0
Nevada	1991	89	18	7	38.9	6	33.3	5	27.8
Texas		83	359	65	18.1	220	61.3	74	20.6
Pennsylvania	1989	77	142	59	41.5	72	50.7	11	7.7
New Jersey	1986	74	200	56	28.0	115	57.5	29	14.5
Ohio	1990	65	117	54	46.2	53	45.3	10	8.5
Florida		55	132	42	31.8	66	50.0	24	18.2
Utah		43	37	23	62.2	10	27.0	4	10.8
Washington	1987	39	53	29	54.7	17	32.1	7	13.2
Michigan	1989	38	89	33	37.1	46	51.7	10	11.2
Virginia	1988	33	51	17	33.3	23	45.1	11	21.6
Maryland	1989	32	44	14	31.8	28	63.6	2	4.5
Wisconsin	1987	30	45	27	60.0	15	33.3	3	6.7
Indiana	1986	28	37	21	56.8	11	29.7	5	13.5
Georgia	1988	26	66	24	36.4	37	56.1	5	7.6
Oklahoma	1991	26	65	23	35.4	31	47.7	11	16.9
Oregon		26	41	21	51.2	12	29.3	8	19.5
Illinois	1989	20	146	14	9.6	117	80.1	15	10.3
Missouri	1986	17	41	10	24.4	23	56.1	8	19.5
Kansas	1989	14	24	10	41.7	7	29.2	7	29.2
North Carolina		14	44	11	25.0	25	56.8	8	18.2
Connecticut	1989	12	95	11	11.6	69	72.6	15	15.8
Tennessee	1988	12	29	9	31.0	15	51.7	5	17.2
Iowa		10	18	8	44.4	6	33.3	4	22.2
Wyoming	1989	8	4	2	50.0	0	0.0	2	50.0
Arizona	1987	7	44	6	13.6	29	65.9	9	20.5
New Mexico		6	8	3	37.5	2	25.0	3	37.5
Rhode Island	1990	6	15	6	40.0	7	46.7	2	13.3
South Carolina	1988	6	17	5	29.4	9	52.9	3	17.6
Louisiana		4	19	1	5.3	12	63.2	6	31.6
New Hampshire		4	19	2	10.5	12	63.2	5	26.3
Mississippi		3	8	3	37.5	4	50.0	1	12.5
Montana		3	5	3	60.0	1	20.0	1	20.0
North Dakota		3	2	1	50.0	0	0.0	1	50.0
Kentucky	1987	2	11	2	18.2	9	81.8	0	0.0
Maine	1988	2	2	2	100.0	0	0.0	0	0.0
South Dakota	1990	2	3	1	33.3	1	33.3	1	33.3
Hawaii		1	4	1	25.0	3	75.0	0	0.0
Idaho	1988	1	6	6	100.0	5	83.3	1	16.7
Nebraska	1988	1	6	1	16.7	4	66.7	1	16.7
Vermont		1	5	1	20.0	3	60.0	1	20.0
West Virginia		1	5	1	20.0	3	60.0	1	20.0
Alabama		0	13	0	0.0	12	92.3	1	7.7
Arkansas		0	9	0	0.0	5	55.6	4	44.4
District of Columbia		0	3	0	0.0	3	100.0	0	0.0
Total		3567	3567	1180	33.1	1951	54.7	436	12.2

Table 2. Industries with the highest tariffs on imports from Canada

This table lists the 20 industries for which the FTA reduced tariffs by the greatest amount.

Four-digit SIC (U.S. 1987)	Industry	Import tariff cuts
3021	Rubber and plastics footwear	36.06%
2326	Men’s and boys’ work clothing	28.88%
3253	Ceramic wall and floor tile	20.00%
2111	Cigarettes	19.33%
2221	Broadwoven fabric mills, manmade fiber and silk	14.53%
2037	Frozen fruits, fruit juices, and vegetables	11.85%
2821	Plastics materials, synthetic resins, and nonvulcanizable elastomers	11.26%
3671	Electron tubes	11.06%
2022	Natural, processed, and imitation cheese	10.46%
3144	Women's footwear, except athletic	10.01%
3171	Women's handbags and purses	9.73%
3229	Pressed and blown glass and glassware, not elsewhere classified	9.31%
2824	Manmade organic fibers, except cellulosic	8.83%
2211	Broadwoven fabric mills, cotton	8.81%
3143	Men's footwear, except athletic	8.55%
3824	Totalizing fluid meters and counting devices	8.06%
2084	Wines, brandy, and brandy spirits	7.83%
2015	Poultry slaughtering and processing	7.77%
3661	Telephone and telegraph apparatus	7.76%
3851	Ophthalmic goods	7.55%

Table 3. Summary statistics

This table gives summary statistics for firm and industry characteristics. Panel A reports mean, median, and standard deviation for average U.S. tariffs on imports from Canada for the period of 1986-88 as well as the HHI index computed in 1988. In Panel B, we report summary statistics for firm variables. See Appendix for the description of all variables.

Panel A. *Competition and concentration measures*

	Mean	Median	Standard deviation
Import tariff cut	0.0445	0.0333	0.0504
Export tariff cut	0.0934	0.0646	0.1144
Herfindahl-Hirschman index (1988)	0.1737	0.1482	0.1210

Panel B. *Firm characteristics*

	Number of obs.	Mean	Median	Standard deviation
Assets size	34,264	3.6303	3.4917	2.3506
Ln (age)	34,279	2.1884	2.1972	0.9611
ROA	33,462	0.0584	0.1181	0.2421
Leverage	33,410	0.1937	0.1534	0.1874
Market-to-book	27,770	1.6435	0.6649	2.4251
Institutional investor ownership	14,428	0.2686	0.2207	0.2189
Import penetration	34,264	0.1276	0.0939	0.1208
Net change in capital	28,581	0.1261	0.0038	0.2579
Net change in equity	29,032	0.0838	0.0001	0.2305
Net change in debt	30,191	0.0576	0	0.1527
Oil spike exposure	1,720	0.0099	.0085	0.0667

Table 4. Main specification

This table reports OLS regressions. In Columns (1) and (2), we include the BC law dummy and the variable measuring the change in import tariffs; in Columns (3)-(6), we include the interaction between BC law dummy and the variable measuring the change in import tariffs. Column (5) also includes a variable measuring the change in export tariffs and Column (6) its interaction with the BC law dummy. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
BC	-0.0268**	-0.0250**	-0.0205*	-0.0189*	-0.0133*	-0.0133*
	(0.0102)	(0.0097)	(0.0105)	(0.0101)	(0.0066)	(0.0066)
BC × Import tariff cuts			-0.4497**	-0.4615**	-0.5595***	-0.5610**
			(0.2132)	(0.2168)	(0.2058)	(0.2614)
Import tariff cuts	-0.3245***	-0.2311**	0.0253	0.1315	0.2134	0.2146
	(0.0958)	(0.0960)	(0.2089)	(0.2078)	(0.2008)	(0.2465)
Export tariff cuts					-0.0045	0.0009
					(0.0219)	(0.0596)
BC × Export tariff cuts						-0.0052
						(0.0559)
Size	0.1061***	0.1094***	0.1061***	0.1093***	0.1080***	0.1080***
	(0.0058)	(0.0055)	(0.0058)	(0.0056)	(0.0065)	(0.0065)
Size squared	-0.0083***	-0.0085***	-0.0083***	-0.0085***	-0.0081***	-0.0081***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Ln (age)	-0.0315***	-0.0205**	-0.0318***	-0.0210**	-0.0208**	-0.0208**
	(0.0056)	(0.0101)	(0.0056)	(0.0099)	(0.0093)	(0.0093)
State-year	0.2205***	0.2220***	0.2192***	0.2205***	0.2059***	0.2059***
	(0.0584)	(0.0623)	(0.0573)	(0.0613)	(0.0560)	(0.0560)
Industry-year	0.1618***	0.1593***	0.1614***	0.1587***	0.1270***	0.1270***
	(0.0320)	(0.0283)	(0.0318)	(0.0281)	(0.0275)	(0.0275)
HHI $_{t-1}$		0.0840***		0.0848***	0.0735***	0.0735***
		(0.0272)		(0.0266)	(0.0264)	(0.0263)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	32,777	29,512	32,777	29,512	29,512	25,001

Table 5. Firm characteristics

This table reports OLS regressions. In Column (1) (resp. Column (2)) we estimate a regression for firms that have TFP greater (resp. lower) than the median TFP of the three-digit industry in which the firm operates in 1984. In Column (3) (resp. Column (4)), we estimate a regression for firms with headquarters located in a state with the principal city closer (resp. farther) than 300 miles to the U.S.-Canada border crossing. All regressions include the control variables used in Column (3) of Table 4. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA				
	High-TFP firms	Low-TFP firms	Closer to border	Farther from border
	(1)	**(2)**	**(3)**	**(4)**
BC	0.0055	-0.0221*	-0.0030	-0.0402**
	(0.0090)	(0.0113)	(0.0070)	(0.0161)
BC × Import tariff cuts	-0.4967**	-0.9504**	-0.4752**	-0.1705
	(0.2045)	(0.3912)	(0.1865)	(0.2458)
Import tariff cuts	0.3097*	0.5680	0.0743	-0.1807
	(0.1709)	(0.4254)	(0.1696)	(0.2830)
Controls	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Number of obs.	11,571	9,755	16,880	15,897

Table 6. Robustness with respect to industry characteristics

This table reports OLS regressions in which we add industry controls to the baseline regression reported in Column (3) of Table 4. In Column (1), we add the interaction between the BC law dummy and average tariffs before 1989. In Column (2), we add the interaction the BC law dummy and the HHI. In Column (3), we add - as industry controls - skill intensity, capital intensity and TFP growth (all averaged for the period 1986 to 1989 and interacted with a dummy, set equal to 1 in the post-FTA period). In Column (4), we add all the controls that were included separately in Columns (1)-(3). Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA

	(1)	**(2)**	**(3)**	**(4)**
BC	-0.0183	-0.0171	-0.0145**	-0.0107
	(0.0128)	(0.0108)	(0.0065)	(0.0096)
BC × Import tariff cuts	-0.4476*	-0.4600**	-0.4963**	-0.4236**
	(0.2233)	(0.2171)	(0.2057)	(0.1852)
Import tariff cuts	0.1377	0.1346	0.2646	0.3052
	(0.2289)	(0.2074)	(0.1949)	(0.2104)
Size	0.1093***	0.1093***	0.1082***	0.1083***
	(0.0056)	(0.0055)	(0.0069)	(0.0069)
Size squared	-0.0085***	-0.0085***	-0.0083***	-0.0083***
	(0.0005)	(0.0005)	(0.0006)	(0.0006)
Ln (age)	-0.0210**	-0.0211**	-0.0226**	-0.0225**
	(0.0098)	(0.0100)	(0.0095)	(0.0095)
State-year	0.2205***	0.2207***	0.1998***	0.1997***
	(0.0613)	(0.0612)	(0.0579)	(0.0580)
Industry-year	0.1587***	0.1592***	0.1340***	0.1342***
	(0.0279)	(0.0278)	(0.0245)	(0.0245)
HHI $_{t-1}$	0.0847***	0.0903***	0.0832***	0.0815**
	(0.0265)	(0.0336)	(0.0269)	(0.0308)
BC × Pre-1989 tariffs	-0.0260			-0.1447
	(0.1855)			(0.1104)
BC × HHI $_{t-1}$		-0.0098		0.0013
		(0.0229)		(0.0229)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry characteristics	No	No	Yes	Yes
Number of obs.	29,512	29,512	26,018	26,018

Table 7. Additional robustness checks

This table reports OLS regressions. In Column (1), we conduct a falsification test that considers a placebo implementation of the BC laws three years before their actual implementation In Column (2), we conduct a falsification test that considers a placebo implementation of the FTA three years before its actual implementation, where standard errors are clustered by four-digit SIC industry. Column (3) provides the results of estimation in Column (3) of Table 4 but without including any time-varying controls. Column (4) excludes firms incorporated in Delaware. Column (5) excludes firms operating in more than one segment. Column (6) excludes the firms incorporated in the states that passed BC laws after the FTA implementation (i.e. BC laws passed in 1990 and 1991). In Column (7), we provide our main estimates for a subsample of firms that are present in the dataset from 1981 to 1995. Column (8) replicates estimations in Column (3) of Table 4 but instead clusters standard errors by four-digit SIC industry, while Column (9) provides two-way clustered standard errors by state of incorporation times four-digit SIC industry. All regressions, except the one in Column (3) include the control variables used in Column (3) of Table 4. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA									
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Placebo BC	-0.0078								
	(0.0091)								
Placebo Import tariff cuts		-0.0836							
		(0.1522)							
BC			-0.0074	-0.0130	-0.0221	-0.0210*	-0.0071	-0.0189**	-0.0189**
			(0.0083)	(0.0089)	(0.0135)	(0.0111)	(0.0090)	(0.0077)	(0.0092)
BC × Import tariff cuts			-0.5255**	-0.5796**	-0.4652*	-0.5003**	-0.6999*	-0.4615**	-0.4615*
			(0.2528)	(0.2214)	(0.2441)	(0.2400)	(0.3951)	(0.2082)	(0.2400)
Import tariff cuts			0.2577	0.1390	0.0693	0.1462	0.5314	0.1315	0.1315
			(0.2366)	(0.2246)	(0.2733)	(0.2266)	(0.3324)	(0.2253)	(0.2404)
Controls	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	29,512	29,512	33,462	14,619	15,983	27,775	11,524	29,512	29,512

Table 8. Alternative measures of competition and corporate governance

This table reports OLS and instrumental variables regressions. In Column (1) we estimate regressions in which corporate governance is proxied by the fraction of the firm's shares held by the institutional investors. In Column (2) we estimate regressions in which foreign competition is proxied by import penetration of the firm's industry. Import penetration is instrumented with the weighted average of the real exchange rates of the importing countries, where weights for each industry are the shares of each foreign country's imports in the total imports of that industry, fixed in 1981. All regressions include the control variables used in Column (3) of Table 4. Control variables are described in Appendix. In columns 1-2 Standard errors are clustered by industry while in columns 3-4 they are clustered by state of incorporation. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA	(1)	(2)
Institutional ownership	0.0640***	
	(0.0191)	
Institutional ownership × Import tariff cuts	0.9574**	
	(0.3952)	
Import tariff cuts	-0.3687	
	(0.2312)	
BC		0.0982
		(0.0599)
BC × Import penetration		-0.8743*
		(0.4679)
Import penetration		2.2823
		(1.4989)
Controls	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Number of obs.	14,011	33,490

Table 9. Market value

This table reports OLS regressions. Market-to-book ratio is estimated as the market value of equity at the end of the fiscal year divided by the book value of common equity; this ratio is limited to the interval between 0 and 10. In Column (1), we report estimates using the same specification as in Column (3) of Table 4. In Columns (2)-(4) we sequentially include other controls (lagged by one year): ROA, extent of leverage and ratio of R&D expenditures to sales. In Column (5), we include all controls together. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: Market to book ratio						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
BC	0.0716	0. 0599	0.0792	0.0634	0.0595	0.0803
	(0.0683)	(0.0704)	(0.0736)	(0.0749)	(0.0701)	(0.0784)
BC × Import tariff cuts	-6.6526**	-9.3694**	-8.1342*	-9.6753**	-9.3674**	-8.2615**
	(2.5484)	(4.3318)	(4.0729)	(4.1935)	(4.3312)	(3.9814)
Import tariff cuts	3.6672	6.1295	5.4033	6.1128	6.1411	5.1769
	(2.2125)	(3.9027)	(3.6081)	(3.7178)	(3.9007)	(3.4771)
Size	0.4731***	0.4756***	0.3934***	0.4790***	0.4690***	0.4024***
	(0.1003)	(0.1015)	(0.0899)	(0.1105)	(0.1038)	(0.1047)
Size squared	-0.0034	-0.0038	0.0024	-0.0031	-0.0032	0.0029
	(0.0071)	(0.0073)	(0.0063)	(0.0078)	(0.0074)	(0.0074)
Ln (age)	-0.2208***	-0.2217***	-0.1977***	-0.1942**	-0.2231***	-0.1643**
	(0.0752)	(0.0752)	(0.0697)	(0.0803)	(0.0740)	(0.0688)
State-year	0.1744***	0.1756***	0.1797***	0.1649***	0.1761***	0.1687***
	(0.0577)	(0.0571)	(0.0589)	(0.0546)	(0.0572)	(0.0555)
Industry-year	0.3058***	0.3056***	0.3105***	0.3095***	0.3057***	0.3124***
	(0.0307)	(0.0306)	(0.0285)	(0.0292)	(0.0305)	(0.0277)
HHI $_{t-1}$	0.9125***	0.9180***	0.8930***	0.8836***	0.9146***	0.8411***
	(0.3000)	(0.3045)	(0.3139)	(0.2832)	(0.3054)	(0.2938)
Export tariff cuts	0.5436	-1.0025	-0.7388	-1.1933	-1.0111	-0.7697
	(0.4565)	(1.5086)	(1.4247)	(1.4295)	(1.5062)	(1.4110)
BC × Export tariff cuts		1.4100	1.9807	1.6981	1.6981	1.5590
		(1.5045)	(1.4180)	(1.4368)	(1.5029)	(1.4131)
ROA $_{t-1}$			0.4612***			0.5780***
			(0.1273)			(0.1521)
Leverage $_{t-1}$				-0.5752***		-0.5367***
				(0.1211)		(0.1129)
R&D $_{t-1}$					-0.1043	1.0297***
					(0.0944)	(0.3442)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	22,091	22,091	21,714	21,747	21,818	21,188

Table 10. Abnormal returns around the 1988 Canadian general election

This table reports the cumulative abnormal returns (CARs) of stocks of U.S. firms. These returns are calculated as the difference between actual holding returns (as extracted from CRSP), and expected returns (projected using a market model with the parameters estimated from 241 to 41 trading days prior to November 21, 1988). Event date [0] in the table corresponds to November 21, 1988. Columns (1)-(3) report results for different equally weighted portfolios, constructed at the three-digit SIC industry level: Column (1) reports results of all industry portfolios; Column (2) reports the average abnormal returns for portfolios of the firms in industries subject to high (i.e. greater than 3.3%) tariff; and Column (3) reports the average abnormal returns for portfolios of the firms in industries subject to low (i.e. lower than 3.3%) tariff. Columns (5)-(6) report results for different equally weighted portfolios, constructed at the state of incorporation level: Column (5) reports the average abnormal returns for portfolios of the firms incorporated in a state that passed a BC law before 1989; and Column (6) reports the average abnormal returns for portfolios of the firms incorporated in a state that passed a BC law in or after 1989. Standard errors are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

CARs around Canadian election results							
	All firms	High tariffs	Low tariffs	Difference: (2)-(3)	BC laws present	No BC laws	Difference: (5)-(6)
Event period	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
[-20,-1]	-0.0035	-0.0138	0.0075	-0.0212*	-0.0013	0.0186	-0.0199
	(0.0064)	(0.0087)	(0.0093)	(0.0127)	(0.0168)	(0.0227)	(0.0332)
[-5,-1]	-0.0024	-0.0063	0.0018	-0.0081	0.0013	-0.0069	0.0081
	(0.0034)	(0.0041)	(0.0055)	(0.0068)	(0.0042)	(0.0087)	(0.0122)
[-1,0]	-0.0003	-0.0045	-0.0021	-0.0024	-0.0050	-0.0037	-0.0012
	(0.0039)	(0.0030)	(0.0040)	(0.0049)	(0.0037)	(0.0045)	(0.0067)
[0,0]	-0.0009	-0.0043**	-0.0030	-0.0013	-0.0057**	-0.0034	-0.0023
	(0.0034)	(0.0018)	(0.0029)	(0.0034)	(0.0026)	(0.0036)	(0.0052)
[0,1]	-0.0016	-0.0086***	-0.0003	-0.0083*	-0.0064*	0.0043	-0.0106
	(0.0037)	(0.0023)	(0.0036)	(0.0043)	(0.0038)	(0.0058)	(0.0083)
[0,3]	-0.0049*	-0.0091***	-0.0003	-0.0088	-0.0135***	0.0020	-0.0155*
	(0.0028)	(0.0032)	(0.0047)	(0.0057)	(0.0047)	(0.0060)	(0.0088)
[0,5]	-0.0034	-0.0125***	0.0063	-0.0188***	-0.0144***	0.0044	-0.0188*
	(0.0032)	(0.0042)	(0.0046)	(0.0062)	(0.0052)	(0.0069)	(0.0101)

Table 11. Exporters

This table reports OLS regressions. We distinguish between exporters and non-exporters (a firm is classified as an exporter if exports constituted at least 1% of its sales prior to the FTA). These regressions also include, as an explanatory variable, the interaction between the change in export tariffs and our BC law dummy. In Column (1) we estimate the regression for the sample of non-exporting firms. In Columns (2)-(5) we estimate regressions for the sample of exporting firms. In Column (4) (resp. Column (5)) we estimate separate regressions for exporting firms in industries with the share of exports to Canada over all exports in 1985 higher (resp. lower) than 15%. All regressions include the control variables used in Column (3) of Table 4. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA

	Non-exporters	Exporters			
				Industries with high exports to Canada	Industries with low exports to Canada
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
BC	-0.0173*	-0.0002	0.0011	-0.0005	0.0054
	(0.0096)	(0.0056)	(0.0056)	(0.0109)	(0.0076)
BC × Export tariff cuts	-0.0242		-0.2653**	-0.3388**	-0.1816
	(0.0554)		(0.1176)	-0.1423	-0.173
Export tariff cuts	0.0419	0.0672**	0.3184***	0.3877***	0.2081
	(0.0587)	(0.0267)	(0.1140)	(0.1193)	(0.1671)
BC × Import tariff cuts	-0.5624*	-0.4729***	-0.0622	0.0955	-0.3185
	(0.3249)	(0.1698)	(0.1721)	(0.7126)	(0.2132)
Import tariff cuts	0.0508	0.1933	-0.1917	-0.044	-0.0963
	(0.2818)	(0.1405)	(0.1403)	(0.7033)	(0.1925)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of obs.	12,542	9,119	9,119	5,011	4,081

Table 12. Financial constraints and performance

This table reports OLS regressions. In Column (1) (resp. Column (2)) we estimate separate regressions for firms in industries with high (resp. low) net change in capital in 1984, one year prior to the first BC laws. Net change in capital is estimated as net change in equity and debt, normalized by the firm's book value of assets at the beginning of the year. In Column (3) (resp. Column (4)) we estimate separate regressions for firms that had (resp. did not have) an S&P long-term debt rating in 1985. In Column (5) (resp. Column (6)) we estimate separate regressions for firms that were smaller (resp. larger) than the median industry size in 1984, one year prior to the first BC laws. In Column (7) (resp. Column (8)) we estimate separate regressions for firms that were young (resp. older) than the median firm in 1984. All regressions include the control variables used in Column (3) of Table 4. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA

	High capital intensive industries	Low capital intensive industries	Firms with credit credirating	Firms without credit rating	Large firms	Small firms	Old firms	Young firms
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
BC	-0.0227	-0.0035	-0.0073	-0.0153	-0.0006	-0.0219*	-0.0021	-0.0238*
	(0.0157)	(0.0072)	(0.0078)	(0.0110)	(0.0077)	(0.0129)	(0.0069)	(0.0130)
BC × Import tariff cuts	-1.0507***	-0.0429	-0.0928	-0.6897***	-0.3875**	-0.8818**	-0.0080	-0.9515***
	(0.2188)	(0.3525)	(0.4714)	(0.2289)	(0.1735)	(0.4357)	(0.4047)	(0.2297)
Import tariff cuts	0.3441	0.0295	0.2066	0.2902	0.2852*	0.4034	0.0627	0.4212
	(0.2364)	(0.3467)	(0.4863)	(0.1962)	(0.1649)	(0.4185)	(0.3986)	(0.2597)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	12,838	11,202	4,577	20,437	12,068	10,445	11,789	10,724

Table 13. Oil price spike

This table reports OLS regressions. We include the BC law dummy, variables measuring the change in import tariffs exposure to oil price spike in 1990, all pairwise interactions as well as triple interaction among them. Column (1) reports the results of a regression without any control variables. Column (2) reports the results of a regression with the control variables used in Column (3) of Table 4, and Column (3) also includes the reduction in export tariffs. Column (4) performs the regression on our full sample, replacing Oil spike exposure variable with 0 where values are unavailable. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable: ROA				
	(1)	**(2)**	**(3)**	**(4)**
BC	-0.0111	-0.0144*	-0.0149*	-0.0097
	(0.0084)	(0.0074)	(0.0078)	(0.0067)
BC × Import tariff cuts × Oil spike exposure	-16.3904*	-16.8277**	-16.7903*	-15.0903*
	(8.2565)	(8.2587)	(8.4216)	(7.7437)
BC × Import tariff cuts	-0.2637	-0.2026	-0.2559	-0.5075**
	(0.3793)	(0.4094)	(0.5493)	(0.2464)
Import tariff cuts × Oil spike exposure	10.5049	11.2219	11.1796	9.3238
	(7.8530)	(7.6550)	(7.8249)	(7.3099)
BC × Oil spike exposure	0.6592	0.7225	0.7218	0.6184
	(0.4115)	(0.4274)	(0.43)	(0.3919)
Oil spike exposure	-0.4132	-0.4764	-0.4723	-0.393
	(0.3993)	(0.4106)	(0.413)	(0.3814)
Import tariff cuts	0.0334	-0.0817	-0.092	0.2178
	(0.3453)	(0.3854)	(0.5216)	(0.2307)
Export tariff cuts			0.0147	
			(0.0968)	
BC × Export tariff cuts			0.0322	
			(0.0982)	
Controls	No	Yes	Yes	No
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Number of obs.	14,847	14,483	14,483	28,370

Table 14. External finance

This table reports OLS regressions. In Columns (1)-(2), we consider the combined net change in equity and debt, normalized by the firm's book value of assets at the beginning of the year. In Column (4), the dependent variable is instead security issuance, which is estimated from data in the SDC New Issues database and is equal to total proceeds from issuance of securities over the year divided by the book value of assets at the beginning of the year. All dependent variables are limited to the interval between 0 and 1. All regressions include the control variables used in Column (3) of Table 4 as well as beginning-of-year leverage and the reduction in export tariffs; in Column (2) we also include the interaction between BC laws and reduction in export tariffs. Control variables are described in Appendix. Standard errors, clustered by state of incorporation, are given in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

Dependent variable:	Net change in equity and debt		Security issuance
	(1)	**(2)**	**(3)**
BC	-0.0436**	-0.0445**	-0.0065
	(0.0165)	(0.0168)	(0.0109)
BC × Import tariff cuts	-0.4452**	-0.6387*	-0.4569**
	(0.1849)	(0.3434)	(0.1933)
Import tariff cuts	0.4900*	0.6526*	0.3014**
	(0.2721)	(0.3636)	(0.1262)
Controls	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Industry and state trends	Yes	Yes	Yes
Number of obs.	17,334	17,334	20,239

Credit Supply and Corporate Innovations

Mario Daniele Amore[†] *Copenhagen Business School*

Cédric Schneider[‡] *Copenhagen Business School*

Alminas Zaldokas[§] *INSEAD*

Abstract

We present causal evidence that financial development plays a key role in technological progress. We focus on firms' innovative performance, measured by patent-based metrics, and exploit the staggered passage of banking deregulations in the U.S. during the 1970s and 1980s as a source of variation in the availability and quality of credit. We find that the deregulation of banking activities across U.S. states had significant beneficial effects on firms' innovation activities. This effect, which does not become evident until some years after deregulations, is partly driven by a greater ability of deregulated banks to diversify credit risk, and by a relaxation of financial constraints for bank-dependent firms.

JEL Classification: G20, G30, O30

Keywords: credit supply, banking deregulations, innovation, patents

For insightful comments and suggestions we thank Morten Bennedsen, Bruno Cassiman, Andrea Fosfuri, Denis Gromb, Jens Iversen, Paloma Lopez-Garcia, Andrea Mina, Jesper Rangvid, Thomas Rønde, Ben Sand, Battista Severgnini, as well as participants at the Zvi Griliches Research Workshop in Economics of Innovation (Barcelona), Druid Winter Conference (Cambridge), and participants at the INSEAD brownbag seminar.

[†] PhD candidate, Copenhagen Business School. Email: mda.eco@cbs.dk

[‡] Associate professor, Copenhagen Business School. Email: csc.eco@cbs.dk

[§] PhD candidate, INSEAD. Email: alminas.zaldokas@insead.edu

1. Introduction

Schumpeter argued that developed and well-functioning financial systems are essential for promoting innovation and long-term economic growth. As discussed by Jarayatne and Strahan (1996), this relationship arises via two possible mechanisms. First, a pure volume effect results when financial intermediaries channel savings to investment (Bencivenga and Smith 1991). Second, financial systems can increase the productivity of that investment by allocating funds to the most qualified firms (Greenwood and Jovanovic 1990; King and Levine 1993a). Our contribution is to establish the causal effect of financial development on firms' innovative performance.

Recent works document a positive association between innovation and various financing sources, such as equity issues (Atanassov et al. 2007; Brown et al. 2009), venture capital (Kortum and Lerner 2000), bank credit (Ayyagari et al. 2011; Benfratello et al. 2008; Winston Smith 2011) and internal resources (Himmelberg and Petersen 1994). As shown in Figure 1, our data also point to a positive correlation between patenting activity and loan supply in the U.S. While this evidence suggests that a wider access to external finance may favor innovation, the empirical study of finance's effects on innovation is plagued by the endogeneity of financial development. Arguably, general economic conditions, industry characteristics and other unobserved factors may influence both firms' innovation activities and credit availability. In addition, firms with higher value-added projects may have better access to financing.

To overcome these concerns, we employ the passage of deregulations in the U.S. banking industry during the 1970s and 1980s. While *intrastate* deregulations eased bank branching within a given state, *interstate* deregulations allowed banks to enter different states. We exploit the staggered passage of deregulations to construct time variations in financial development across U.S. states. Banking deregulations induced exogenous variations in financial development for at least three reasons. First, banking deregulations, in particular those across states, facilitated banks' geographic diversification (Demyanyk et al. 2007; Goetz et al. 2011), thus encouraging lending to riskier projects. Second, deregulations strengthened competition, which is thought "to have improved allocative efficiency by allowing capital to flow more freely toward project yielding highest returns" (Kerr and Nanda 2009). Third, deregulations improved banks' efficiency and quality of loan portfolios (Jayratne and Strahan 1996, 1998), and also encouraged the adoption of better screening technologies allowing interest rates to reflect the underlying risk more accurately (Dick and Lehnert 2010).

Our main result indicates that banking deregulations caused a relevant increase in firms' innovative performance, as measured by the number of successful patents filed, which stemmed from across-state, rather than within-state, deregulations of credit markets. We also find an increase in the *relevance* of the patenting activity, measured by citations received from future patents, and in its *originality* and *generality*, suggesting that a wider access to external finance led firms to change the technological nature of their research projects.

These findings remain significant after controlling for common patenting trends, firm fixed effects and other confounding factors. In particular, controlling for the stock of research and development (R&D) we observe that following deregulations firms made a more productive use of their existing innovation inputs – though these positive effects became larger a few years after deregulations were enacted.

We claim that the channel behind our main findings relates to a greater willingness of banks to take risk once they become more diversified geographically, following interstate deregulations. Out-of-state banks may be thus willing to lend at more favorable terms – all the more so if credit risk in the deregulating state is less correlated with their existing exposure. We find that most of the increase in the patenting activity occurred in states whose economies exhibited least comovement with the overall U.S. economy. Moreover, the effect on innovation was highest in those states where new out-of-state banks were entering from the states least comoving with that state.

Although changes in the supply of credit can affect financing and investment decisions of a wide array of firms (Lemmon and Roberts 2010), the effects of bank credit shocks are most pronounced among firms with worse access to other segments of the capital markets (Leary 2009). Existing studies also show that bank debt is particularly important for young and informationally opaque firms (see e.g. Johnson 1997; Hadlock and James 2002). Our findings indicate that bank-dependent firms, e.g. firms that are young and do not have a bond rating enabling access to the public debt market, shift their investment from capital expenditures towards R&D expenses following interstate deregulations.

A large literature has tested the effect of deregulations on entrepreneurship (Bertrand et al. 2007; Black and Strahan 2002; Cetorelli and Strahan 2006; Kerr and Nanda 2009, 2010) and industry reallocation (Acharya et al. 2011; Bertrand et al. 2007). Recent works have also identified significant effects of banking deregulations on the corporate policies of U.S. publicly listed firms (Correa and Suarez 2009; Francis et al. 2011). Our first contribution is to highlight for the first time that interstate deregulations had beneficial effects on firms' innovative activities.

Along this line, we also contribute to a broad research on the relationship between financial development and economic growth (King and Levine 1993b; Jayaratne and Strahan 1996; Demirguc-Kunt and Maksimovic 1998).[62] While some determinants of this relationship, such as firm entry and entrepreneurial activities, have been widely analyzed (Black and Strahan 2002; Guiso et al. 2004; Cetorelli and Strahan 2006; Kerr and Nanda 2009), there is significantly less empirical evidence on the role of technological progress. In a closely related work, Benfratello et al. (2008) show that banking development increased the probability of process innovation. We extend this work in two ways. First, we adopt policy changes in the U.S. banking industry as a natural experiment to establish the *causal* effect of financial development on firms' innovation activities. Second, by focusing on corporate patenting, we are

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[62] See Levine (2005) for a comprehensive review of this literature.

able to establish an effect not only on the volume but also on the *technological nature* and *relevance* of the innovation activities pursued.

Finally, our study complements a recent literature on how variations in the access to external finance affect corporate policies (Campello et al. 2010; Duchin et al. 2010; Leary 2009; Lemmon and Roberts 2010). Although we are unable to test whether the increase in innovation stems directly from bank lending, or indirectly, i.e. from non-bank institutions or investors that in turn benefited from banking deregulations, our results reinforce the notion that changes in the supply of credit have strong effects on firms' corporate policies.

Section 2 describes the policy changes that transpired in the U.S. banking industry. Section 3 presents the data. Section 4 outlines our empirical methodology. Section 5 presents our main finding that banking deregulation increased the number and quality of firm innovation. We then provide evidence on two channels explaining the increase in patenting: the improved ability of banks to diversity credit risk thus lending to riskier borrower (Section 6), and the increase in innovation inputs following a relaxation of financial constraints (Section 7). Section 8 concludes.

2. Deregulations in the U.S. banking industry

The geographic expansion of banking activities in the U.S. has been historically restricted by laws such as the McFadden Act of 1927 and the Douglas Amendment to the Bank Holding Company Act of 1956. However, during the 1970s and 1980s U.S. states passed a number of deregulations of branching and interstate banking activities which effectively terminated the restrictions on the expansion of banks across and within states. Our identification strategy exploits the staggered passage of these banking deregulations.

Table 1 illustrates the timeline of deregulations by state and year. As shown, U.S. states first reduced restrictions on branching within states, and then barriers to banking across states. The first intrastate deregulations were passed in the early 1970s, while the first state passing an interstate deregulation was Maine in 1978, followed by Alaska and New York in 1982. The wave of deregulations continued until the passage, in the mid-1990s, of the interstate banking provisions of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBEEA).[63]

Deregulations were associated with several major improvements in the credit market and set the stage for the emergence of "expansion-minded banks" (Black and Strahan, 2002). Kerr and Nanda (2009) show that the total number of banks fell from the mid-1970s to the mid-1990s but this reduction was mostly driven by a drop in the number of small local banks. By contrast, the fraction of large banks increased over this period. Jayratne and Strahan (1998) document that these changes were associated with greater banks' efficiency. For example, they show that loan losses decrease by about 29 basis points in the short run and about 48 basis points in the longer run while operating costs decrease by about 4.2 percent initially and about 8 percent in the longer run. Furthermore, they argue that most of these reduced costs were passed along to bank customers in the form of lower loan rates. Such major improvements in both the quality and the availability of credit are thought to have produced benefits for a broad array of economic activities, including the financing of innovation activities. Existing evidence at the firm level already indicates that banking deregulations were relevant for the creation and closures of new ventures (see e.g. Kerr and Nanda 2009; 2010) as well as for the corporate policies of publicly traded firms (Francis et al. 2011).

The two types of deregulation had distinct implications for the U.S. banking sector. Intrastate branching deregulations reduced entry barriers into new markets within the passing states and made it easier for banks to gain control over other banks' assets, either by mergers and acquisitions or by opening new branches within a state. The effect was to reduce the banks' ability to raise prices above their marginal cost in local markets and, in some cases, even to break local monopolies (Kerr and Nanda 2009).

[63] The IBEEA was passed in 1994, went into effect on 30 September 1995 and became effective on 1 June 1997 (Carow and Heron 1998).

Interstate deregulations allowed banks to enter different states. Prior to interstate deregulations, only bank holding companies located within a state could buy banks chartered in that state; however, after deregulations bank holding companies operating in other states were allowed to do so. Indeed interstate deregulations let to an expansion of banks across state borders. Since out-of-state banks were typically endowed with more capital, the expansion of banks across states led to an increase in loan supply. Using state-level data on commercial bank loans for the period 1976-1995 provided by the Federal Deposit Insurance Corporation (FDIC) we find that, after controlling for year and state fixed effects, interstate deregulations were associated with a 8% increase in total net loan supply. Moreover, because out-of-state banks were endowed with sophisticated monitoring technologies, interstate deregulations encouraged lending to risky borrowers (Dick and Lehnert 2010). Finally, lower restriction to banking across states improved the scope for geographic diversification (Goetz et al. 2011), allowing banks to finance more freely risky projects yielding higher returns without increasing the overall risk. Given that innovation is inherently a risky activity and out-of-state deregulated banks' were better able to geographically diversify such risk, we expect the effects of deregulations on firms' innovative performance to stem from interstate deregulations, rather than intrastate deregulations.

A concern with our identification strategy is that deregulation passages may have been correlated with pre-existing trends in financial development or product markets within the legislating states. If that is the case, our empirical approach would simply reflect pre-deregulation trends rather than an increase in innovation due to the exogenous changes in credit markets. We rule out this concern in two ways. First, we draw on existing information on the political economy of deregulations. Kerr and Nanda (2009) argue that "interstate deregulations were driven in part by the savings and loan crisis in the early 1980s when federal legislators allowed failed banks and thrifts to be acquired by banks in any state, regardless of state laws governing these transactions. These changes paved the way for bilateral negotiations between states to allow interstate banking in order to foster larger, diversified banks that were less susceptible to failure". Second, we empirically show that deregulations did not have a significant effect on innovations in the years prior to the actual deregulation passages (see Table 5, Panel A) and this finding reinforces the idea that our estimates do not merely reflect pre-deregulation trends.

Another concern is whether banking deregulations should have any effect on innovative activity since a debt contract might be ill-suited to finance such activities as innovation that have uncertain returns (Atanassov et al. 2007; Stiglitz 1985). Yet, public firms might resort to private debt to fund innovation when they incur costs to raise capital in public markets. Indeed, funding innovation with public capital might provide sensitive information to the competitors (Bhattacharya and Ritter 1983; Maksimovic and Pichler 2001), or it can be costly to manager because of low tolerance for failure in the public markets (Ferreira et al. 2011).

3. Data and summary statistics

We measure innovation by using successful patent applications, which represent a widely-used approach to quantify a firm's innovative performance (Griliches 1990). Figure 1 shows the state-level non-parametric relationship between U.S. patenting activity, computed from the U.S. Patent and Trademark Office (USPTO), and loan supply data from the FDIC. While the evidence is suggestive of a strong positive association, the endogeneity concerns discussed in the introduction complicate the causal interpretation. We establish causal effects by exploiting the exogenous variations in credit markets provided by the passage of banking deregulations.

We start by collecting data at the firm level from the Compustat dataset, which contains a rich set of financial characteristics for U.S. publicly traded firms. We focus the analysis on the period 1976-1995, which covers all years when interstate deregulations were passed and also a large time period of intrastate branching deregulations. We do not extend our sample after 1995 to avoid confounding the effects with the Reagle-Neal Act, which was passed in 1994 and went into effect at the end of 1995. Another advantage of ending the sample in 1995 is that our analysis is not contaminated by the dramatic increase in cash flow and equity financing of R&D activities experienced by young U.S. firms during the second half of the 1990s (Brown et al. 2009). Following the literature on U.S. banking deregulations, we exclude observations in Delaware and South Dakota because these states were subject to special tax incentives. We also exclude firms with negative or zero book value of assets and sales, and firms headquartered outside the U.S.[64] Finally, we consider SIC codes up to 4000 (mostly manufacturing firms). Thus, we exclude industries such as financial services or utilities, which typically operate under specific regulations, or the software industry, which is primarily dependent upon non-debt sources of financing such as equity issuances and venture capital.[65] In fact, as documented in Scherer (1983) and more recently in Balasubramanian and Sivadasan (2011), the bulk of patenting activity occurs within the manufacturing sector, which for this reason has been the focus of many existing studies (e.g. Hall and Ziedonis, 2001; Hall et al. 2005).

Next, we match Compustat firms with the patent dataset assembled at the National Bureau of Economic Research (NBER), which contains information on the patents awarded by the USPTO and all citations to these patents (Hall et al. 2001; Bessen 2009). We also construct several firm characteristics such as logarithm of sales, capital-to-labor ratio, R&D stock[66], return on assets (ROA), firm age, cash holdings and asset tangibility. In addition, we construct the Herfindahl-Hirschman Index (HHI)

[64] A concern arises from the fact that Compustat only reports the last state of operations, and we may be unable to observe changes of headquarter from a state to another that are potentially endogenous to the deregulation passages. However, using data on headquarter relocations from the Compact Disclosure database, Pirinsky and Wang (2006) argue that most of headquarter changes are driven by mergers and acquisitions. After excluding these and other major restructuring events, they found 118 relocations from a sample of more than 4000 firms in the period 1992-1997. Our results are robust to excluding firm-year observations with asset or sales growth exceeding 100%, which are typically associated with mergers, restructuring and other major corporate events (Almeida et al. 2004).

[65] In unreported analyses, we replicated our findings with the inclusion of wholesale (SIC 5000-5199) and retail trade (SIC 5200-5999).

[66] The R&D stock is computed following the conventional 15% depreciation rate used in the related literature (see e.g. Hall et al. 2005). Also, we use linear interpolations to replace missing values of R&D; however, our results are robust to leaving those observations missing or treating them as zeros.

to control for the impact of industry concentration on innovation. The HHI is based on the distribution of revenues of the firms in a particular three-digit SIC industry. A higher HHI implies a higher concentration. We correct for potential misclassifications due to the presence of a single firm in a given industry by dropping 2.5% of the firm-year observations at the right tail of the HHI distribution (Giroud and Mueller 2010). The detailed construction of each variable is described in Appendix.

Table 2 reports summary statistics for the sample we use in the empirical analysis, obtained after dropping observations with missing values in the key variables. As documented in previous works on the Compustat-NBER patent dataset, citation statistics are very skewed. In our sample, the average number of patents is approximately 10 but the median is 1.

4. Methodology

We use a difference-in-differences model to explore the causal relationship between firm innovation and banking deregulations. The important advantage of using a difference-in-differences model is that we can control for omitted variables and absorb nation-wide shocks or common trends that might affect the outcome of interest.

We expect the positive effect on innovation to arise primarily from interstate deregulations, since banks improved their ability to diversify credit risk, thereby lending to riskier companies without increasing the overall risk. We use a binary variable *interstate deregulations*$_{jt}$ which is equal to one if a firm *i* is headquartered in a state *j* which has passed an interstate deregulation at time *t*, and zero otherwise.[67] *Interstate deregulations*$_{jt}$ captures the effect of interstate deregulations on firm patenting comparing outcomes before and after each deregulation year, *vis-à-vis* deregulations passed later. To control for the potential effects of intrastate deregulations, we use a binary variable *intrastate deregulations*$_{jt}$ which is equal to one

[67] As shown by Bharath et al. (2007), public firms have a strong propensity to borrow from local lenders, so we assume that firms should be primarily affected by the banking deregulations in the state of their headquarters.

if a firm i is headquartered in a state j which has passed an intrastate deregulation at time t, and zero otherwise.

Our first approach consists in using the logarithm of patent counts as dependent variable and estimating OLS regressions. However, one concern with this approach is that it does not appropriately deal with firms that have zero patents. To avoid losing these observations we employ count data models, which are widely used in the econometric analysis of patents. Following Hausman et al. (1984), we hypothesize that the expected number of patents of a firm i applied for in year t is an exponential function of both types of deregulations, contained in T_{jt}, and firm- and industry-specific characteristics.[68] More specifically, we estimate conditional-mean Poisson models:

$$E\left[Y_{ijt} \middle| T_{jt}, X_{it-1}\right] = \exp\left(\alpha + \beta T_{jt} + \delta X_{ijt-1} + \eta_i + \tau_t\right) \quad (1)$$

We estimate fixed-effect Poisson models by Quasi-Maximum-Likelihood (QMLE), which provide consistent estimates as long as the conditional mean is correctly specified even if the true underlying distribution is not Poisson (Wooldridge 1999). Since our deregulation treatments are defined at the state level, we cluster standard errors at the state of location. Following the literature on the production function of patents (see e.g. Galasso and Simcoe 2011; Aghion et al. 2009), our baseline specification includes a vector X_{ijt-1} of time-varying firm controls, such as firm sales[69] and capital-labor ratio, which are lagged by one year to reduce simultaneity concerns. Furthermore, we control for firm idiosyncratic effects denoted by η_i.

In additional analyses, we include other controls such as firm age and tangibility, to control for existing dependence and access to bank credit; return on assets (ROA), to control for cash flow positions; cash holdings, to control for differences in liquidity; and HHI, to absorb the effect of concentration in the industry where a firm operates.

[68] Our main results do not change if we estimate models in which interstate and intrastate deregulation treatments are included separately.
[69] In unreported analyses, we control for firm size in alternative ways such as by including the square of firm sales, or replacing firm sales with the logarithm of total assets.

Depending on the specification, we also control for the current stock of R&D. As stressed by Aghion et al. (2009), not controlling for the R&D stock implies that the coefficient of the variable of interest on the right-hand side will reflect both the increase in R&D expenses and the productivity of R&D. By contrast, when the R&D stock is included in the specification, the effect of the variables of interest can be interpreted as an effect on the innovative productivity of firms.

Given that the U.S. patenting activity increased substantially starting from the mid 1980s (see e.g. Hall 2004), it is important to control for aggregate trends. First, we include a full set of year dummies, denoted by τ_t in equation (1). Second, we control for industry linear trends by including in the specification the annual three-digit SIC industry averages of the dependent variable, computed after excluding the firm in question. Third, we assess the robustness of our findings to the inclusion of linear geographic trends, computed as annual averages of each headquarter's state after excluding the firm in question. In unreported analyses, we also check that our results are robust to including polynomial terms of industry and geographic linear trends.

5. Innovation activity

This section presents our empirical results. First, we show the results obtained on the number of patents successfully filed by the firms. Second, we analyze the quality of innovation, by exploiting information on the citations that each patent received from subsequent patent applications. We proceed by estimating the dynamic effects of banking deregulations and discussing a number of robustness checks.

5.1 Innovation outputs

Table 3, Panel A, shows OLS estimates using the logarithm of patent counts as the dependent variable. In Column (1), we show that the interstate deregulations had a positive effect on the innovation outputs. In particular, allowing out-of-state banks to enter the state increased innovation activity for a firm located in the state by 21%.

Meanwhile, the coefficient of intrastate deregulations is not statistically different from zero.

While in Column (1) we only control for industry and year fixed effects, in Column (2) we add the logarithm of sales and capital-to-labor ratio. Controlling for these firm characteristics reduces the magnitude of the interstate coefficient, which is however more precisely estimated and becomes significant at 1%. In Column (3), we further control for R&D stock. As expected, the stock of R&D has a positive and significant effect on patenting; however, the interstate deregulation coefficient remains significant at 1%. In Column (4), we confirm our findings by including a host of controls that may potentially affect innovation, such as firm age, HHI, ROA, tangibility and cash holdings. The interstate deregulations coefficient remains both statistically and economically relevant, indicating a 19% increase in patenting.

In Columns (5) - (8), we adopt a more restrictive specification that instead of industry fixed effects includes firm fixed effects. As expected, restricting the identification to within-firm variations leads to sensibly smaller deregulation coefficients, but the statistical significance is confirmed at 1% level. The economic magnitude of the effect is relevant as well: the most restrictive specification (Column 8), indicate a 12.7% increase in patenting. As found above, the intrastate deregulations had no relevant effects.

In Table 3, Panel B, we provide estimates from fixed-effect Poisson QMLE regressions, which take into account that patent counts are skewed to zero.[70] Similar to our OLS results, the most restrictive specification (Column 4) indicates a 14% increase in patenting following interstate deregulations.

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[70] An alternative approach could be to use transformations of the dependent variable to avoid losing observations with zero patents (as in Table 3, Panel A). For example, in unreported analyses we have estimated OLS regressions using the logarithm of (1 + patent counts) as dependent variable. However, these transformations are arbitrary and often not robust to alternative methods.

5.2 Innovation quality

Our results so far suggest that interstate deregulations caused an increase in firms' innovation activities as measured by the raw number of patents granted. However, the existing research has demonstrated that patents differ greatly in "value" and that simple patent counts do not capture the relative importance of the underlying inventions (Harhoff et al. 1999). In this section, we measure innovation by weighing each patent using the number of future citations received from subsequent patents (Trajtenberg 1990). Forward citations reflect the economic and technological "importance" as perceived by the inventors themselves (Jaffe et al. 2000) and knowledgeable peers in the technology field (Albert et al. 1991). Because forward citations suffer from truncation problems, we adopt patent counts weighted by truncation-adjusted citation counts from the NBER data (see e.g. Hall et al. 2001; Hall et al. 2005).[71]

Results reported in Table 4, where we use cite-weighted patent counts as dependent variable, indicate that interstate deregulations lead to a significant and economically relevant increase in the quality of patenting, whereas intrastate deregulations have an insignificant effect. Hence, not only the number of patents have increased but their average quality has risen as well, suggesting that the effect did not purely come from the larger supply of financing and thus lower rationing of projects being financed. We further argue that the average increase in the quality of innovations stems from a rise in the risk of innovative projects being financed.

5.3 Dynamic effects

Although U.S. states passed deregulation legislations at specific points in time, the real consequence of interstate deregulations on credit supply caused by the *actual* entry of banks in the new states may manifest after several years. Even patenting an innovation

[71] The problem arises from the fact that "citations to a given patent typically keep coming over long periods of time, but we only observe them until the last date of the available data" (Hall et al. 2005). Besides the use of truncation-adjusted citation counts, the problem is mitigated by the inclusion of year fixed effect. In fact, our results are robust to the adoption of unadjusted citation counts.

is the outcome of a process that sometimes can require several years. A specification that compares raw outcomes before and after deregulations, as the one used in the previous sections, may not be well-suited to capture these potential dynamic effects.

We test for dynamic effects by drawing on specifications similar to Kerr and Nanda (2009). First, we construct a dynamic difference-in-differences model employing a set of dummies that measure the distance in years from each deregulation passage, using as reference group the period three years or earlier before deregulations. Results, reported in Table 5, Panel A, show that the coefficient prior to the interstate deregulation is small and statistically insignificant, thus confirming that our results are not driven by diverging pre-deregulation trends. By contrast, the post-deregulation coefficients are all positive and significant at conventional levels. Importantly, they become larger as we move forward from the reform year, with the largest effect corresponding to six and seven years after interstate deregulations.

Second, we allow the effect of deregulations on innovation to linearly grow over time using a variable equal to zero up to the deregulation year and then equal to the number of years since a deregulation was passed, capping the treatment effect at 8 years. Results, reported in Table 5, Panel B, confirm that interstate deregulations had a positive growing impact on firms' patenting activity.

5.4 Robustness checks

We test the validity of our findings in several ways. We do not tabulate the results described in this section, but they are available upon request. We start by addressing the concern that other policies potentially affecting innovation were adopted around the same period of the banking deregulations. In the late 1980s, 30 U.S. states passed a set of business combination (BC) laws that reduced the threat of hostile takeovers thus weakening the governance role of the market for corporate control (Giroud and Mueller 2010; Bertrand and Mullainathan 2003). These laws might affect our analysis through the effect of corporate governance on the managerial incentives to innovate[72], and that effect would not be captured by our specification since BC laws impacted firms at their state of incorporation. To mitigate this concern, we control for a dummy equal to one if firms were incorporated in the states that passed a BC law, from the year of the passage onwards, and zero otherwise. Our results indicate that the positive effect of banking deregulations on firm innovation is not confounded by the passage of BC laws.

Re-examining findings in Black and Strahan (2002), Wall (2004) shows that the effect of deregulations on entrepreneurship was positive in some U.S. regions but significantly negative in others. We check in several ways how our results depend on regional effects. First, we our findings are unchanged if we augment our specifications with regional trends, computed as year averages of the dependent variables by region excluding the firm in question.[73] Second, we estimate region-specific deregulation effects. Results from this last exercise show that interstate deregulation coefficients are all positive, though their statistical and economic significance is not homogeneous across U.S. regions.

We perform a number of additional robustness checks that further validate our findings. First, we include state-level time varying characteristics as additional controls. In particular, we include lagged GDP growth and the logarithm of population, obtained from the U.S. Bureau of Economic Analysis. Second, we exclude observations corresponding to the year of interstate deregulations. Third, we show that our results are robust to the inclusion of linear state trends centering the identification on discontinuities surrounding the interstate deregulations (Kerr and Nanda 2009). Fourth, to better isolate the effect of interstate deregulations, we exclude those states

[72] The effect of corporate governance on innovation is ambiguous. Some empirical studies indicate that worse corporate governance reduces the incentives to innovate (Atanassov 2009). Chemmanur and Tian (2011) argue that some degree of managerial entrenchment isolates CEOs from short-term pressures, thus inducing them to focus on long-term value creation and innovate more. Sapra et al. (2011) show that the effect of corporate governance on innovation follows a U-shaped relationship.

[73] Regions are defined according to the four-grouping classification provided by the U.S. Census: west, midwest, northeast and south (http://www.census.gov/geo/www/us_regdiv.pdf).

that passed intrastate deregulations within a year of interstate deregulations. Fifth, we restrict the analysis to firms that remain in the sample for at least 5 (10 or 15) years to purge the analysis from firm entry and exit. Sixth, we use contemporaneous rather than lagged controls. Seventh, we exclude firms headquartered in California and Massachusetts, since these states have a particularly high innovation activity. Eighth, we extend our sample up to 1997, i.e. the year when the implementation of the IBEEA finally enacted a nation-wide deregulation of the banking sector. Ninth, we allow for heterogeneous time and state effects by interacting all the covariates with year and interstate treatment dummies.

6. Innovation risk

6.1 Technological nature and risk of innovation

So far our results indicate that firms subject to a wider access to high-quality credit patent more innovations that are also relevant, as suggested by the overall number of future citations received. In this section, we explore the technological nature and riskiness of innovation. First, we combine citations with information on patents' technological fields. Second, we check if there is a simultaneous increase in both high-quality and low-quality patents. Finally, we analyze the volatility of successful patenting.

Technological fields, defined by the USPTO, consist of about 400 main (3-digit) patent classes. We use the *generality* and *originality* indexes, developed by Trajtenberg et al. (1997) and computed by Hall et al. (2001), to capture the fundamental nature of the research being patented. The generality index is equal to $1 - \sum_{j}^{n_j} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations received by a patent i that belong to the patent technology class j out of n_i patent classes. The index will take high values (high generality) if a patent receives citations from subsequent patents that belong to many different technological fields. The originality index is constructed in a similar way, but its computation relies on the citations made rather than citations received; it will take a high value if a patent cites other patents that belong to many different fields (high originality).

We use these two indexes as separate dependent variables and estimate a specification similar to the one used in Table 4. Our results, reported in Table 6, indicate that interstate deregulations had a positive and significant effect on the generality of patents: firms subject to the interstate treatment exhibited a higher propensity to patent within a broader technological field (Columns 1 and 2). The same result is found for the originality of patents (Columns 3 and 4).

Showing that following deregulations firms patented in broader technological classes, our results suggest that a wider access to external finance led to a more ambitious innovation policy, which in turn may entail potential failures. We perform additional analyses showing that firms' patenting activity indeed became riskier. First, we study whether there was a simultaneous increase in patents that received many citations as well as few citations in the future. We estimate quantile regressions at different percentiles of the distribution of the logarithm of cite-weighted patent counts. In line with our notion of increased risk, our results, reported in Table 7, Panel A, show that the effect is present both at low deciles (e.g. 30% and 40%) and high deciles (e.g. 80%).

Second, we analyze the volatility of successful patenting. Specifically, we adopt as dependent variable the standard deviation of the logarithm of cite-weighted patent counts computed in the pre- and post-interstate deregulation periods, restricting the analysis to firms that are present at least two years in each period. Then, we estimate a regression including the interstate deregulation dummy together with the usual controls, averaged over the pre- and post-deregulation period, and the firm fixed effects. Results reported in Table 7, Panel B, indicate that interstate deregulations led to an increase in the volatility of successful patenting. By contrast, we do not find any relevant result for intrastate deregulations (untabulated).

6.2 Banks' geographic diversification

One of the channels that can explain the higher patenting activity of firms after the entrance of new banks is that out-of-state banks were better able to finance riskier projects as they were less exposed to the background risks of the state's economy. At the same time, credit in this state provides out-of-state banks an opportunity to diversify their loan portfolio, for instance, due to a different industry composition of the state. We use three empirical tests that provide empirical support to this argument.

In our first test, we separate the states according to how their economic activity comoves with the rest of the U.S. economy. Here we expect that states that are least correlated with the activity of other states would provide highest diversification benefits for entering banks and thus would experience highest increase in patent quality. In particular, we extract a coincident index that summarizes state-level economic indicators from the Federal Reserve Bank of Philadelphia. The coincident index combines data on nonfarm payroll employment, average hours worked in manufacturing, unemployment rate, and wage and salary disbursements deflated by the consumer price index (U.S. city average). The trend for each state's index is set to the trend of its gross domestic product (GDP) so that long-term growth in the state's index matches long-term growth in its GDP (Crone and Clayton-Matthews 2005). We estimate the correlation of a state's economy to the rest of the U.S. from the monthly values of the coincident indices of the states as well as the coincident index of the U.S. over 1979-1984, before interstate deregulations started to come into effect. We then interact this correlation variable that we call *Diversification 1* with the interstate treatment. In Table 8, Columns (1) and (2), we show that the increase in patenting quality primarily rose in the states with the recent history of least covariation with the rest of the U.S.

Our second test relies on the locations of banking institutions that enter the state. We investigate whether the effect on innovation was highest in those states where new out-of-state banks were entering from the states least comoving with the state in question. In particular, for each pair of states we estimate the correlation of their monthly values of the coincident indices over 1979-1984. We then calculate the weighted average of these comovement measures across all out-of-state banking institutions operating in the state, based on the location of their bank holding companies. As a weight for each institution, we use the assets it has in the state as a fraction of the total assets in the state held by out-of-state banking institutions. We estimate such a measure for each state and each year. We call this variable *Diversification 2*.[74] Our data on the banking institutions comes from the Reports of Condition and Income (Call Reports) that provide information on the financial activities as well as the ownership structures of each banking institution. All banking institutions regulated by the Federal Deposit Insurance Corporation, the Federal Reserve, or the Office of the Comptroller of the Currency are required to file the Call Reports. Since this data is only available to us starting from 1986, we conduct the analysis on a subsample between 1986 and 1995. In Table 8, Columns (3) and (4), we report that when we interact *Diversification 2* with our interstate treatment dummy, we find that the increase in patenting quality was mainly evident in the states that experienced the entry of the banks from the states with the least comoving economic indicators.

7. Innovation inputs and bank dependence

If easier access to credit was a channel through which banking deregulations affected innovation, we expect the effect to be more prevalent among firms that faced higher costs to raise external finance or that were more dependent from bank credit prior to deregulations. Moreover, if a wider access to external finance made the need of physical collateral less relevant to finance future investment, we expect that following deregulations firms changed their investment in favor of R&D expenses.

To test these notions, we focus the analysis on the intensity of expenditures in innovation inputs, measured as the ratio of R&D to total investment (i.e. the sum on

[74] Correlation between *Diversification 1* and *Diversification 2* is 0.2.

R&D and capital expenditures). We first perform the analysis for the whole sample and then focus on the more financially constrained firms, adopting a number of standard proxies for bank dependence and financial constraints. First, we consider firm age. Because old and well-established firms can access the public debt market or easily raise equity, they should not be influenced by changes in bank credit supply. By contrast, young firms, which are typically more financially constrained due to asymmetric information problems, are expected to respond more to changes in bank credit. We construct the interaction between interstate deregulations and a dummy equal to one for firms that were young at the time of the interstate deregulation. We define as young those firms that are present for less than 10 years in Compustat (Rajan and Zingales 1998; Cetorelli and Strahan 2006).

As shown in Table 9, while there effect was positive but statistically insignificant for the average firm (Columns 1 and 2), Columns (3) and (4) indicate that the interaction of interstate deregulations and young firms is positive and significant. Young firms subject to interstate deregulations experience a 5 percentage points increase in R&D relative to total investment. Given that the average R&D to total investment ratio is 0.42, this increase is economically relevant.

Next, we sort firms according to whether in 1985 they were assigned a long term bond rating by Standard&Poors.[75] By allowing firms to access public debt markets, a bond rating is related to lower credit constraints (Kashyap et al 1994; Almeida et al. 2004; Faulkender and Petersen 2006; Denis and Sibilkov 2010) and, consequently, lower responsiveness to changes in bank finance (Leary 2009). We construct an indicator equal to one if a firm reports a bond rating and positive debt, and equal to zero if a firm is not assigned to a rating or it has no debt.

Columns (5) and (6) show that the interaction between this dummy and the interstate deregulations treatment displays a positive and significant coefficient. Results so far show that the effect of shifts in bank credit supply are relevant for firms that are young and constrained in accessing the public debt market. This evidence is

[75] 1985 is the first year when the coverage of S&P ratings in Compustat started.

consistent with previous findings that bank credit is most relevant for less-established and informationally opaque firms (Hadlock and James 2002).

In conclusion, our results indicate that interstate deregulations had a positive effect on innovation inputs depending on firms' financial constraints: the effect was present primarily among younger firms and firms with worse access to other segments of the credit market.[76]

8. Conclusion

While the relationship between economic prosperity and financial development has been widely debated, establishing the direction of causality remains a challenging task. We focus on firms' innovative performance as a driving force of technological progress and growth, and exploit the passage of banking deregulations in the U.S. during the 1970s and 1980s to generate exogenous variations in financial development. Banking deregulations, in particular those that removed restrictions to the geographic expansion of banks, allowed banks to better diversify their loan portfolios, increased the availability and quality of credit, and induced the adoption of screening and monitoring technologies.

Our results indicate that interstate deregulations played a beneficial role in spurring firms' innovation activities, as measured by patent-based metrics. Furthermore, we find that the effect was not imminent and was mainly driven by bank-dependent firms, which reacted to the deregulations by changing their investment policy in favor of R&D expenses. Finally, we provide evidence that the increase in firms' innovation activities is associated with a better ability of out-of-state banks to diversify credit risk.

[76] Notice that the results of this section do not change if we include interactions between firm and intrastate deregulations; the interactions with interstate deregulations remain significant and with similar coefficient, whereas neither intrastate deregulations nor the interactions are statistically significant.

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Appendix. List of variables

Name	Description	Source
	Innovation variables	
Patent counts	Count of a firm's number of patents for the period 1976-1995	NBER
Ln (Patent counts)	Logarithm of a firm's number of patents for the period 1976-1995	NBER
Cite-weighted patent counts	Count a firm's number of patents for the period 1976-1995 weighed by future citations received and adjusted for truncation (as described in Hall et al. 2001; Hall et al. 2005)	NBER
Ln (Cite-weighted patent counts)	Count a firm's number of patents for the period 1976-1995 weighed by future citations received and adjusted for truncation (as described in Hall et al. 2001; Hall et al. 2005)	NBER
σ (Cite-weighted patent counts)	Standard deviation of the logarithm of a firm's count of number of patents for the period 1976-1995 weighed by future citations received and adjusted for truncation. Standard deviations are computed in the pre-and post-deregulation period, keeping in the computation firms that remain in each period at least two years	NBER
Originality index	Equal to $1 - \sum_j^{n_j} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations made by a patent *i* that belong to the patent technology class *j* out of n_i patent classes. Technology classes are defined by the USPTO and consist of about 400 main patent classes (3-digit level). The index will take high values (high originality) if a patent cites other patents that belong to many different technological fields	NBER
Generality index	Equal to $1 - \sum_j^{n_j} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations received by a patent *i* that belong to the patent technology class *j* out of n_i patent classes. The index will take high values (high generality) if a patent receives citations from subsequent patents that belong to many different technological fields	NBER
Ln (R&D Stock)	Logarithm of (cumulative R&D expenditures), computed assuming a 15% annual depreciation rate	Compustat
R&D to total investment	Ratio of R&D expenses to total investment, computed as the sum of CAPEX and R&D expenses	Compustat
	Firm and industry characteristics	
Ln (Sales)	Logarithm of a firm's sales	Compustat
Ln (K/L)	Logarithm of capital to labor ratio, where capital is represented by property, plants and equipment (PPE), and labor is the number of employees	Compustat
Ln (Age)	Logarithm of (1+age), where age is the number of years that the firm has been in Compustat	Compustat
ROA	EBITDA to total assets, dropping 1% of observations at each tail of the distribution to mitigate the effect of outliers	Compustat
Cash holdings	Cash and marketable securities to total assets	Compustat
Tangibility	1- (intangible assets to total assets)	Compustat
Industry HHI	Herfindahl-Hirschman Index, computed as the sum of squared market shares of all firms, based on sales, in a given three-digit SIC industry in each year. We drop 2.5% of observation at the right tail of the distribution to mitigate potential misclassifications (Giroud and Mueller 2010)	Compustat
Young firms	Dummy variable equal to one if a firm was present for less than 10 years in Compustat at the time of the interstate deregulation, and zero otherwise	Compustat
Credit constrained firms	Dummy variable equal to one if a firm report a S&P bond rating in 1985, and zero otherwise	Compustat
	Industry and geographic linear trends	
Industry trends	Average of the dependent variable across all firms in the same three-digit SIC industry of a given firm, where averages are computed excluding the firm in question	Compustat
Geographic trends	Average of the dependent variable across all firms in the same state of location of the firm, where averages are computed excluding the firm in question	Compustat

Banking deregulations variables

Interstate/Intrastate deregulations	Dummy variables equal to one from the deregulation year onwards, and zero for the period prior to deregulations	

Diversification variables

Diversification 1	State economy's comovement with the rest of the U.S., measured as the correlation of state's coincident index to the U.S. coincident index. We estimate it from the monthly values of the indices over 1979-1984. The coincident index combines data on nonfarm payroll employment, average hours worked in manufacturing, the unemployment rate, and wage and salary disbursements deflated by the consumer price index (U.S. city average)	Federal Reserve Bank of Philadelphia
Diversification 2	Weighted average of the comovement between the state and the states where the bank holding companies of its out-of-state banks are located. We estimate the pairwise correlations between all states from the monthly values of the coincident indices over 1979-1984. We then calculate the weighted average of these comovement measures across all out-of-state banking institutions operating in the state, based on the location of bank holding company. As a weight for each institution, we use the assets it has in the state as a fraction of the total assets in the state held by out-of-state banking institutions. Due to data limitations, this measure is constructed for the period 1986-1995	Federal Reserve Banks of Philadelphia and Chicago

Figure 1.
Relationship between innovation and credit supply

This graph shows the non-parametric (lowess smoothing) relationship between total net loan supply and patenting activity in the U.S. using state-year observations from the mid 1980s to mid 1990s. The line reports the local linear regression fit computed using a bandwidth of 0.8.

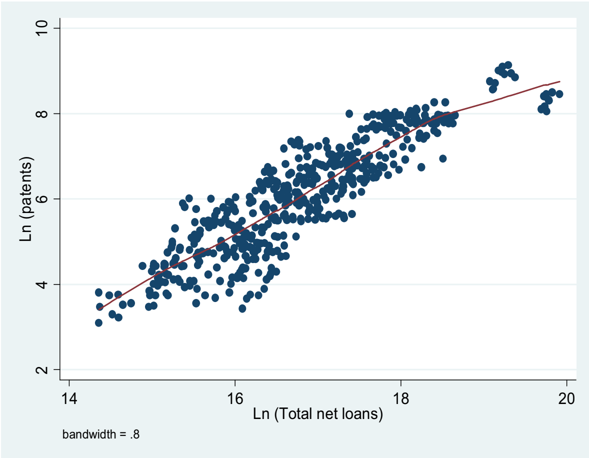

Table 1.
U.S. states by intrastate and interstate deregulations

This table illustrates the timing of intrastate and interstate deregulations in the U.S. states. Deregulations passed in 1975 or earlier are listed as “Before 1976”.

Year	Intrastate deregulations	Interstate deregulations
Before 1976	Maine, Alaska, Rhode Island, North Carolina, Virginia, District of Columbia, Nevada, Maryland, Idaho, Arizona, South Carolina, Delaware, California, Vermont, South Dakota	-
1976	New York	-
1977	New Jersey	-
1978	-	Maine
1979	Ohio	-
1980	Connecticut	-
1981	Utah, Alabama	-
1982	Pennsylvania	New York, Alaska
1983	Georgia	Connecticut, Massachusetts
1984	Massachusetts	Rhode Island, Utah, Kentucky
1985	Tennessee, Oregon, Washington, Nebraska	North Carolina, Ohio, Virginia, District of Columbia, Nevada, Maryland, Idaho, Georgia, Tennessee, Florida
1986	Mississippi, Hawaii	Arizona, New Jersey, South Carolina, Pennsylvania, Oregon, Michigan, Illinois, Indiana, Missouri, Minnesota
1987	Michigan, New Hampshire, West Virginia, North Dakota, Kansas	California, Alabama, Washington, New Hampshire, Texas, Oklahoma, Louisiana, Wyoming, Wisconsin
1988	Florida, Illinois, Texas, Oklahoma, Louisiana, Wyoming	Delaware, Vermont, South Dakota, Mississippi, West Virginia, Colorado
1989	Indiana	New Mexico, Arkansas
1990	Kentucky, Missouri, Wisconsin, Montana	Nebraska
1991	Colorado, New Mexico	North Dakota, Iowa
1992	-	Kansas
1993	Minnesota	Montana
1994	Arkansas	-
After 1994	Iowa	Hawaii

Table 2.
Summary statistics

This table illustrates summary statistics. Patent counts represent a firm's number of patents. Cite-weighted patent counts represent a firm's patents weighted by the number of future citations and adjusted for truncation. Ln (R&D) is the logarithm of R&D expenditures. R&D/Investment is the ratio of R&D expenditures to total investment, computed as the sum of R&D and capital expenditures. Ln (Sales) is the logarithm of a firm's sales. Ln (K/L) is the logarithm of capital to labor ratio. Ln (Age) is the logarithm of 1 plus the number of years a firm has been in Compustat. ROA is return on assets, measured as the ratio of earnings before interest and depreciation (EBITDA) divided by the book value of assets. See Appendix for a full description of each variable.

	Number of observations	Mean	Standard deviation	Median
Innovation measures				
Patent counts	22,400	10.418	40.681	1
Cite-weighted patent counts	22,400	159.008	775.394	0
Ln (R&D)	21,894	1.590	1.485	1.135
R&D to total investment	21,688	0.427	0.266	0.400
Other firm characteristics				
Ln (Sales)	22,367	4.304	2.418	4.230
Ln (K/L)	22,180	2.840	0.986	2.811
Ln (Age)	22,400	2.525	0.785	2.565
ROA	22,178	0.089	0.202	0.134

Table 3.
Innovation outcomes

This table reports regression results for number of patents. Panel A reports OLS regression results using Ln (Patents) as dependent variable while Panel B reports Poisson regression results using patent counts as the dependent variable. Columns (4) and (8) in Panel A and Column (4) in Panel B include an additional set of firm and industry lagged controls. Specifically, they include: Ln (Age), HHI, ROA, tangibility, cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix. Standard errors clustered by state of operation are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Panel A. OLS estimates

Dependent variable: Ln (Patent counts)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interstate deregulations	0.2142**	0.1799***	0.2054***	0.1947***	0.1515***	0.1387***	0.1295***	0.1274***
	(0.0909)	(0.0435)	(0.0461)	(0.0449)	(0.0383)	(0.0330)	(0.0358)	(0.0354)
Intrastate deregulations	-0.1427	-0.0755	-0.0747	-0.0735	-0.1166	-0.0713	-0.0544	-0.0576
	(0.1049)	(0.0511)	(0.0497)	(0.0464)	(0.0827)	(0.0524)	(0.0414)	(0.0429)
Ln (Sales)		0.4492***	0.0848***	0.1384***		0.3414***	0.1777***	0.2117***
		(0.0220)	(0.0173)	(0.0250)		(0.0356)	(0.0477)	(0.0552)
Ln (K/L)		0.1385***	0.0417*	-0.0692**		0.0624	0.0209	-0.0107
		(0.0324)	(0.0233)	(0.0322)		(0.0441)	(0.0400)	(0.0459)
Ln (R&D stock)			0.4961***	0.4532***			0.3439***	0.3396***
			(0.0316)	(0.0365)			(0.0721)	(0.0777)
Industry fixed effects	Yes	Yes	Yes	Yes	No	No	No	No
Firm fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes	No	No	No	Yes
Number of obs.	11,272	11,272	11,272	11,272	11,272	11,272	11,272	11,272

Panel B. Poisson estimates

Dependent variable: Patent counts

	(1)	(2)	(3)	(4)
Interstate deregulations	0.1611**	0.1414***	0.1413***	0.1421***
	(0.0696)	(0.0453)	(0.0452)	(0.0408)
Intrastate deregulations	-0.1524	-0.1315**	-0.1145*	-0.1076**
	(0.1262)	(0.0590)	(0.0588)	(0.0528)
Ln (Sales)		0.7093***	0.4423***	0.4896***
		(0.0618)	(0.0775)	(0.0861)
Ln (K/L)		0.2566***	0.2392***	0.1872**
		(0.0646)	(0.0792)	(0.0796)
Ln (R&D stock)			0.4185***	0.3783***
			(0.1226)	(0.1233)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes
Number of obs.	18,011	18,011	18,011	18,011

Table 4.
Innovation quality

This table reports Poisson regression results using cite-weighted and truncation-adjusted patent counts as the dependent variable. Column (4) includes an additional set of firm and industry lagged controls. Specifically, it includes: Ln (Age), HHI, ROA, tangibility, cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix. Standard errors clustered by state of operation are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable: Cite-weighted patent counts

	(1)	**(2)**	**(3)**	**(4)**
Interstate deregulations	0.1412**	0.1014**	0.0949**	0.0974**
	(0.0710)	(0.0477)	(0.0448)	(0.0439)
Intrastate deregulations	-0.0416	-0.0028	0.0229	0.0184
	(0.1475)	(0.0678)	(0.0669)	(0.0610)
Ln (Sales)		0.6895***	0.3180***	0.3724***
		(0.0577)	(0.0686)	(0.0897)
Ln (K/L)		0.2437***	0.2083***	0.1794**
		(0.0621)	(0.0763)	(0.0741)
Ln (R&D stock)			0.6019***	0.5817***
			(0.1053)	(0.1139)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes
Number of obs.	17,892	17,892	17,892	17,892

Table 5.
Dynamic effects

This table reports Poisson results using a dynamic specification. We use as dependent variable future cite-weighted and truncation-adjusted patent counts. In Panel A, the response to interstate and intrastate deregulations is modeled by leads and lags consolidated into two-year increments extending from two years before to eight years or more after the deregulations. Coefficients for leads are relative to the period three years or earlier before deregulations. Columns (3) and (4) include all controls as in Table 4, Column (8). Interstate and intrastate effects in Columns (1) and (2), as well as in Columns (3) and (4), are simultaneously estimated, although they are reported in separate columns to save space. In Panel B, years since interstate and intrastate deregulations are variables equal to the number of years after the deregulation passages, with a long-term effect at eight years, and equal to zero before the deregulations. Standard errors clustered by state of operation are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Panel A. Time dummies

Dependent variable: Cite-weighted patent counts

	(1)	(2)	(3)	(4)
	Interstate deregulations	Intrastate deregulations	Interstate deregulations	Intrastate deregulations
Years 1-2 before deregulation	0.1483	-0.1173	0.0600	-0.0752
	(0.0998)	(0.1014)	(0.0518)	(0.0473)
Deregulation year and one after	0.2616*	-0.1343	0.1639**	-0.0507
	(0.1359)	(0.1488)	(0.0714)	(0.0769)
Years 2-3 after deregulation	0.3291*	-0.2169	0.2230*	-0.0914
	(0.1880)	(0.2047)	(0.1161)	(0.1017)
Years 4-5 after deregulation	0.4423**	-0.3067	0.3147**	-0.1465
	(0.2144)	(0.2501)	(0.1352)	(0.1313)
Years 6-7 after deregulation	0.5967**	-0.3292	0.4608***	-0.1659
	(0.2546)	(0.2895)	(0.1712)	(0.1517)
Years 8+ after deregulation	0.4525*	-0.4873*	0.3577*	-0.2755
	(0.2635)	(0.2732)	(0.1839)	(0.1760)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	Yes
Number of obs.	17,990	17,990	17,990	17,990

Panel B. Linear treatment effects

Dependent variable: Cite-weighted patent counts

	(1)	(2)
Years since Interstate	0.0538*	0.0600***
	(0.0289)	(0.0149)
Years since Intrastate	-0.0060	-0.0200
	(0.0147)	(0.0170)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry trends	Yes	Yes
Additional controls	No	Yes
Number of obs.	17,892	17,892

Table 6.
Patenting and technological fields

This table reports Poisson results using as dependent variable the originality index (Columns 1-2) and generality index (Columns 3-4). The construction of these indexes and control variables is described in Appendix. Standard errors clustered by state of operation are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable:	Originality index		Generality index	
	(1)	**(2)**	**(3)**	**(4)**
Interstate deregulations	0.1547*	0.1283***	0.1366**	0.1420***
	(0.0798)	(0.0460)	(0.0636)	(0.0496)
Intrastate deregulations	-0.1678	-0.1221**	-0.0938	-0.0321
	(0.1270)	(0.0532)	(0.1385)	(0.0590)
Ln (Sales)		0.4936***		0.5142***
		(0.0754)		(0.0830)
Ln (K/L)		0.2101**		0.1578**
		(0.0882)		(0.0763)
Ln (R&D stock)		0.3545***		0.4209***
		(0.1177)		(0.1350)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of obs.	17,305	17,305	16,654	17,305

Table 7.
Patenting risk

Panel A reports quantile regression results using Ln(Cite-weighted patent counts) as dependent variable. Interstate and intrastate treatments are jointly estimated but coefficients are reported separately in Columns (1) and (2). We include the full set of controls as in Table 4, Column (8). Coefficients, unreported to save space, are available upon request. Panel B reports OLS regressions using as dependent variable the standard deviation of Ln(Cite-weighted patent counts) in the pre- and post-interstate deregulation periods. Controls are constructed as average in the pre- and post-interstate deregulation periods. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Panel A. Quantile regressions

Dependent variable: Ln(Cite-weighted patent counts)

Percentile	(1) Interstate deregulations	(2) Intrastate deregulations
10^{th}	-0.0966	0.0604
	(0.1304)	(0.0917)
20^{th}	0.0148	-0.0221
	(0.0965)	(0.0680)
30^{th}	0.0931	-0.0028
	(0.0780)	(0.0545)
40^{th}	0.1588*	0.0453
	(0.0927)	(0.0644)
50^{th}	0.1832**	0.0439
	(0.0745)	(0.0517)
60^{th}	0.1752**	0.0717
	(0.0697)	(0.0481)
70^{th}	0.1411**	0.0804*
	(0.0677)	(0.0465)
80^{th}	0.1417*	0.0934*
	(0.0752)	(0.0510)
90^{th}	0.0822	0.0702
	(0.0749)	(0.0513)

Panel B. Volatility regressions

Dependent variable: σ(Ln (Cite-weighted patent counts))

	(1)	(2)
Interstate deregulations	0.0900*	0.1126**
	(0.0509)	(0.0558)
Ln (Sales)	-0.0577	-0.0249
	(0.0363)	(0.0492)
Ln (K/L)	-0.0366	-0.0288
	(0.0581)	(0.0586)
Ln (R&D stock)		-0.0504
		(0.0510)
Year FE	Yes	Yes
Industry trends	Yes	Yes
Number of obs.	862	862

Table 8.
Diversification benefits

This table reports Poisson regression results using as dependent variable cite-weighted and truncation-adjusted patent counts. Diversification 1 refers to state economy's comovement with the rest of the U.S., measured as the correlation of state's coincident index to the U.S. coincident index. We estimate it from the monthly values of the indices over 1979-1984. The coincident index combines data on nonfarm payroll employment, average hours worked in manufacturing, the unemployment rate, and wage and salary disbursements deflated by the consumer price index (U.S. city average). Diversification 2 refers to weighted average of the comovement between the state and the states where the bank holding companies of its out-of-state banks are located. We estimate the pairwise correlations between all states from the monthly values of the coincident indices over 1979-1984. We then calculate the weighted average of these comovement measures across all out-of-state banking institutions operating in the state, based on the location of bank holding company. As a weight for each institution, we use the assets it has in the state as a fraction of the total assets in the state held by out-of-state banking institutions. Due to data limitations, Diversification 2 is only available for a subsample in the period 1986-1995. The construction of the control variables is described in Appendix. Standard errors clustered by state of operation are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable: Cite-weighted patent counts

	(1)	**(2)**	**(3)**	**(4)**
Interstate deregulations	0.5490***	0.4152***	0.1074	0.1257
	(0.1873)	(0.1005)	(0.1107)	(0.0927)
Interstate deregulations × Diversification 1	-0.5936**	-0.4471***		
	(0.2714)	(0.1357)		
Interstate deregulations × Diversification 2			-0.2044*	-0.2281**
			(0.1134)	(0.0954)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Industry trends	Yes	Yes	Yes	Yes
Number of obs.	18,665	18,665	7,664	7,664

Table 9.
R&D intensity and financial constraints

This table reports OLS regression results using as dependent variable the ratio of R&D expenses to total investment, computed as the sum of R&D and capital expenditures. In Columns (3) and (4), we interact the interstate deregulation with a dummy equal to one if the firm was present for 10 years or less in the Compustat dataset at the time of the interstate deregulation. In Columns (5) and (6), we use the interaction with a dummy equal to one if the firm report an S&P bond rating in 1985. Standard errors clustered by state of operation are reported in parentheses. *, ** and *** denote significance at 10%, 5% and 1% respectively.

Dependent variable: R&D to total investment

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Interstate deregulations	0.0016	0.0043	-0.0132	-0.0098	-0.0234**	-0.0184*
	(0.0077)	(0.0080)	(0.0104)	(0.0105)	(0.0098)	(0.0095)
Interstate deregulations × Young firms			0.0512***	0.0494***		
			(0.0117)	(0.0117)		
Interstate deregulations × Credit constrained firms					0.0343***	0.0301***
					(0.0076)	(0.0072)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Number of obs.	21,322	21,322	13,838	13,838	13,714	13,714

Conclusion

My dissertation empirically investigates the effect of a set of managerial and financial characteristics on corporate outcomes. Methodologically, my dissertation builds on ”quasi-natural experiments” derived from legislative changes, which enable the estimation of causal relationships. From a conceptual standpoint, my dissertation contributes to the econometric research exploring the vast heterogeneity in corporate policies and the determinants of firm performance.

The first chapter provides evidence that firms family-connected with the political sector improved substantially their profitability following an increase in the political power of the politicians they were connected to. The chapter also suggests that the increase in profitability arises from a better ability of connected firms to do business with the public sector. My analysis goes beyond the prevailing focus on corrupt environments and/or national politicians, and instead analyzes connections with local politicians in a country such as Denmark, which is typically considered as one of the least corrupt in the world. Analyzing the full welfare effects of political connections is beyond the scope of this chapter. However, some results suggest that political connections are welfare reducing. First, politically connected firms tend to be less productive before the connection is established. Second, the value of political connections is higher among less profitable firms. Both arguments indicate that political connections may transfer rent from more productive to less productive firms. The welfare reduction is mitigated, however, since connected firms use the rent to increase their operating efficiency.

The second chapter links corporate governance to the competitive ability of firms. Using both accounting and stock market measures of corporate performance, my econometric analysis indicates that U.S. firms endowed with worse corporate governance become significantly less able to face a sudden increase in foreign competition. The mechanism behind this inability is closely related to changes in financial constraints: firms subject to worse governance are less able to raise external funds when competition strengthens.

The third chapter explores how changes in the supply of credit and quality of financial intermediation influence firms' innovative ability. By employing the passage of banking deregulations in the U.S. during the mid 1970s and 1980s, I provide evidence of a significant increase in the quality and quantity of corporate innovations, as measured by patent metrics, following the deregulation of banking activities across U.S. states. This effect can be traced to a better ability of banks to geographically diversify credit risks thereby increasing lending to risky customers, such as constrained firms with high innovation potential. Finally, in line with the notion that innovation is one of investments most sensitive to changes in financial constraints, I show that easier access to credit leads firms to increase their R&D expenditures relative to total investment.

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