ABSTRACT

Title of dissertation: ESSAYS IN CORPORATE FINANCE

Anne Duquerroy, Doctor of Philosophy, 2016

Dissertation directed by: Professor Michael Faulkender
Department of Finance
Robert H. Smith School of Business

This dissertation presents two essays in Corporate Finance.

In the first essay, I study how political institutions affect corporate investment through the policy uncertainty channel. I examine investment response to changes in the ability of the governing party to implement its political agenda due to checks and balances. I use US gubernatorial elections from 1978 to 2010 and a regression discontinuity design to estimate the causal effects of giving a single party full versus split control over a state government. I find that shifts from a divided to a unified government depress investment and job creation. Investment drops by an average of 3 to 5 percent in the year after the election giving a single party control of the government. The effect is not limited to public firms, is stronger for firms operating in a single state and firms with more irreversible investment. The findings support the hypothesis that moving from divided to unified government translates into policy uncertainty, which in turn affects the investment and employment cycles.

The second essay is joint with William Mullins and Christophe Cahn.

How to support private lending to SMEs during aggregate contractions is a crucial but still open policy question. This paper exploits an unexpected drop in 2012 in the cost of funding bank loans to some firms but not others in France to uncover how banks adjust their SME lending portfolios in a crisis. The cost reduction causes bank debt to rise and payment defaults with suppliers to fall, providing evidence that funding cost can be an effective policy lever. The effect is driven by firms with only one bank relationship, a numerous but understudied group. The size of the effect varies, with additional credit flowing to firms with stronger observables, to high growth firms, to firms with high demand, and to firms with a deeper banking relationship. Further, a richer relationship appears to substitute for stronger observables in the lending decision. Finally, we provide suggestive evidence that, compared to multi-bank firms, single bank firms are particularly credit constrained in crisis periods.

ESSAYS IN CORPORATE FINANCE

by

Anne Duquerroy

Dissertation submitted to the Faculty of the Graduate School of the
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Advisory Committee:
Professor Michael Faulkender, Chair
Professor Liu Yang
Professor Francesco D'Acunto
Professor William Mullins
Professor Ethan Kaplan

Dedication

To Clement, Agathe, Capucine and Timothee.

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1 CHAPTER 1: THE REAL EFFECTS OF CHECKS AND BALANCES : POLICY UNCERTAINTY AND CORPORATE INVESTMENT

1.1 Introduction

Is political compromise good for business? In the wake of the high profile political crisis the US experienced in 2013, which led to a two-week long partial shutdown of the Federal government in October, the question of the real effects of having a split partisan control of the government has been widely debated.

How politics shape economic outcomes is a research question of long standing. The earlier literature focuses on the impacts of politics on the macroeconomy (see Drazen (2001) for a survey), showing strong evidence of partisan business cycles in the US (Alesina and Sachs (1988); Alesina et al. (1997)).

More recently a micro-based literature has explored which underlying channels could drive a causal effect of political cycles on financial markets and firm-level outcomes. Government spending (Belo et al. (2013); Cohen et al. (2011)), campaign contributions (Cooper et al. (2010); Akey and Lewellen (2015)), changes in the degree of political connectedness (Faccio (2006); Fisman (2001)), or political uncertainty (Julio and Yook (2012)) have been shown to matter in explaining how political cycles influence.

Most of these analyses look at the impact of politics on corporate activities through the lens of the partisan business cycle theory, in particular using the party controlling the executive to identify partisan preferences over economic policy. Institutions, and

how they effectively impact the ability of the Democratic or Republican parties to implement their political agenda, have received less attention. Over the past forty years in the US, the periods in which the President party fully controls both the legislative and the executive powers (unified government henceforth) have been more the exception than the rule. [1]

At the state level, half of US states are divided on average between 1978 and 2010.[2] This includes so called split branch governments, where the governor is confronted with majorities from the opposing party in both chambers, as well as split legislatures, when different parties hold the upper and lower houses.

This paper investigates the real impact of this dynamic of within-government counter-balancing on firm investment and hiring decisions. I empirically analyze whether, and to what extent, changes in government party control from a divided (unified) to a unified (divided) government, in the US, at the state level, are associated with adjustments in corporate investment and employment. The first parts of the paper are devoted to estimate causal effects of checks and balances on firm investment and employment policy, while the last part of it investigates policy uncertainty as a potential channel to explain the documented effects.

[1] Since 1977, the US government has been unified in 1977-1981 under the Democratic presidency of James Carter; in 1993-1995, under the Democratic presidency of William J. Clinton; in 2003-2007 under the Republican presidency of George W. Bush and in 2009-2011 under the Democratic presidency of Barack Obama.

[2] As of 2016, nineteen states are in split partisan control situations, including Illinois, Maryland, Massachusetts, Minnesota, New York and Pennsylvania.

State policy matters

US State governments have substantial power in shaping the economic environment in which firms operate. They can affect corporate profitability directly (e.g., through tax code, labor regulations or business incentives policies) or indirectly (through customer demand or sentiment). Legislatures in most states (34 states plus the District of Columbia) can approve tax bills with a simple majority vote in each house. Most states have industry specific or targeted programs designed to incentivize investment within the state (for example, aimed at green energy technology). Minimum wage laws and regulations on collective bargaining, which affect labor costs, also vary at the state level. While firms do not explicitly mentioned elections or partisan politics as a source of risk, state level policy is often identified as a source of uncertainty in the risk factors section of their 10-K reports (cf. examples cited in footnote).[3]

[3]INNODATA, headquartered in NJ, incorporated in DE, 10-K 2012. Provider of business process and IT services "Measures aimed at limiting or restricting outsourcing by US companies are under discussion in Congress and in numerous state legislatures. While no substantive anti-outsourcing legislation has been introduced to date, given the ongoing debate over this issue, the introduction of such legislation is possible. If introduced, our business, financial condition and results of operations could be adversely affected and our ability to service our clients could be impaired".

CARDIAC SCIENCE, headquartered in Washington state, incorporated in Delaware, 10-K 2003. Health Industry (medical devices manufacturer). "Federal, state and local governments have adopted a number of healthcare policies intended to curb rising healthcare costs. There have been and may continue to be proposals by legislators, regulators and third-party payers to keep these costs down. Certain proposals, if passed, could impose limitations on the prices we will be able to charge for our products (...). These limitations could have a material adverse effect on our financial position and results of operations".

ALABAMA GAS CORP, headquartered and incorporated in Alabama, 10-K 2012. Oil & Gaz Company. "Federal, state and local legislative bodies and agencies frequently exercise their respective authority to adopt new laws and regulations and to amend and interpret existing laws and regulations. (...) Currently, there are various proposed law and regulatory changes with the potential to materially mpact the Company. (...) Due to the nature of the political and regulatory processes and based on its consideration of existing proposals, the Company is unable to determine whether such proposed laws and regulations are reasonably likely to be enacted or to determine, if enacted, the magnitude of the potential impact of such laws (...)".

NB: these examples have been generated with the help of Don Bowen, by searching the body of 10-K reports available in the EDGAR SEC database, for keywords related to state level policy such as state legislature, state government, gubernatorial elections, Governor etc.

Though the effects of state government policies are potentially smaller than those of federal policy, they are by no means insignificant both on average (state taxes account for 21% of total corporate income taxes paid (Heider and Ljungqvist (2015)) and especially for a subset of firms. Anecdotal evidence such as the January 2015 decision of Daimler AG to relocate its Mercedes-Benz USAs headquarters from New Jersey to an Atlanta suburb, illustrates the relevance of state politics to corporate decisions, with firms willing to take advantage of low union membership in right-to-work states, and lower corporate taxes. Significant impact of state-level policies on corporate decisions has been documented in the financial literature, in particular for smaller, less geographically diversified, firms (Colak et al. (2015); Wald and Long (2007)).

In this paper I use the outcomes of 408 gubernatorial elections held between 1978 and 2010, in 46 states, as exogenously timed changes in state full versus split partisan control. The elections resulted into 82 staggered switches from divided to unified governments and 64 switches from unified to divided governments. The timing of elections is exogenously determined by the political calendar of each state and is independent of economic conditions. This mitigates endogeneity concerns that changes in investment in the post election year may be induced by changes in business cycles. However, it does not rule out reverse causality, because economic outcomes also directly influence election results.

Establishing causality looking at a small number of Presidential elections is challenging. Shifting the analysis to the state level offers the researcher more power because

changes in partisan control alignment (from a divided to a unified government and conversely) are numerous, spread across time and geographically dispersed across states. Indeed states are affected by the same national macro factors but gubernatorial elections in different states occur in different years so that electoral outcomes are dispersed over the business cycle.

The concerns that the state of the economy influences election outcomes is thus alleviated by the fact that identification relies both on within state time variation as well as on across state variation. In the spirit of a triple difference-in-differences, the first treatment level is the occurrence (or absence) of a gubernatorial election in the state of the firm in a given year, and the second treatment level is whether or not this election, if held, resulted in a switch from a divided to a unified government or from a unified to a divided government.

To address remaining concerns that unobserved state-level economic conditions or anticipated state-level economic conditions may be driving both the election outcomes and the investment/employment cycle, I use a regression discontinuity design. Recent contributions implementing a similar design in gubernatorial elections include Folke and Snyder (2012) and Erikson et al. (2015). As stated by Erikson et al. (2015), "the basic idea behind this, is that in very close elections the party of the governor is decided essentially by a coin flip". Thus, looking at a sub-sample of states that were divided ex-ante, those states that became unified because a governor barely won an election and those that stayed divided because a governor barely lost an election should be nearly identical except on dimensions that are directly affected by

the election outcome. Focusing on these very close elections therefore allows me to estimate the causal effect of a change in full or split government partisan control, as decided by the party of the governor.

Results

I first examine corporate investment sensitivity to changes in government full or split partisan control. Controlling for investment opportunities (Tobin's Q and cash flows), demand (sales-growth), overall economic conditions (GDP growth and unemployment rate) and partisan effects (party of the incumbent or elected governor, incumbency advantage, change in partisan majority among others), I find evidence of a persistent negative relationship between switches from divided to unified government and investment. Specifically, corporate investment rate drops by an average of 2.5 percent in the year following elections, when government switches from unified to divided.

I do not find supporting evidence of a symmetric positive response of investment to reverse switches, from unified to divided government. The coefficient estimates are even often negative, although they are not consistently statistically significant. One explanation seems to be that "switching" is a negative shock in itself. A switch to unified government seems to exacerbate this negative effect further. A switch to divided government is associated with a positive, but not high enough, impact to turn the net effect into a rebound in the investment rate. Results are robust to varying specifications to account for additional firm-level characteristics, state-level economic variables and political controls.

I do not find any evidence that the political platform of the incumbent or of the newly-elected governor affect corporate investment around the election cycle or the investment sensitivity to the switch from divided (unified) to unified (divided) government.

The main caveat of this analysis is that, because the reach of state policy is local, identifying firms or industries more exposed to their home state is critical to properly gauge the potential effects and channels through which state policy may affect corporate decision making. Because Compustat firms are generally multi-state firms their exposure to their headquarter states policy is lower than for single-state firms and estimated coefficients are attenuated. To address this concern I estimate similar regressions on a sub-sample of small firms, under the assumption that they are more likely to be geographically concentrated in their home state. Results are stronger, with a drop in the post-election year investment rate which is twice as large as the average effect.

More importantly, I extend my analysis to private sector firms. I exploit employment data from the Census Bureau's Business Dynamics Statistics, which covers approximately 98 percent of US private employment and removes any location bias that could have been introduced by the use of the Compustat headquarter indicator. I find a similar negative and significant drop in the job creation rate, following a switch from divided to unified governments. The economic magnitude of the effect is weaker than for investment, and translates into a 1.5% decrease in the gross job creation rate. This suggests a wait-and-see effect since job destruction rate is also negatively

affected (though the effect is not consistently statistically significant) so that the net creation rate remains basically unchanged. Younger firms and smaller firms, which have one or two establishments, and for which we are thus almost certain that they are entirely located in a given state, are found to be most affected.

In the next set of results, the policy effect is estimated from the discontinuity that occurs when a gubernatorial candidate wins more than 50% of the votes so that the party affiliated with the Governor in the state legislature switches from being the minority party to being the majority party. The RD estimates support the results from the difference-in-difference approach, and show that giving a single party full control over a state, by winning the governors election, systematically leads to a significant drop in the investment rate of firms headquartered in that state in the post-election year. The magnitude of the effect translates into a 5-7 percent drop when reported to the average investment rate. The results hold with and without covariates and are robust to a placebo test in the year before the election outcomes are known.

The policy uncertainty channel

Although the RD estimates establish that there is a negative effect, they say little about why this is the case. A potential channel that I will explore to explain these results is the policy uncertainty channel. Government policy, including state level policy, represents a large source of uncertainty for many firms. Firms do care about which laws or regulation is likely to be passed and how it may impact their profits and

operation capabilities (cf. examples of 10-Ks citations from the risk factors sections).

The topic has received a lot of interest lately with a growing literature looking at the relation between policy uncertainty and corporate investment (Julio and Yook (2012); Gulen and Ion (2015)) or asset prices (Kelly et al. (2016); Pástor and Veronesi (2013)). While Democratic and Republican parties have different political agendas, the concept of unified government is important in state lawmaking because the governor, the senate and the house all play decisive roles in turning a partisan agenda into state legislation. Election results partially resolve uncertainty related to future government policies, but may not be very informative about the probability that a policy shift will occur. It seems likely that whether a given party fully controls the government affects its ability to pass bills, and that this ability is higher when the elected government is unified. [4] As American politics is polarized, unified governments are likely to yield strong partisan outcomes in the form of more extreme economic policy choices. An example of such choices can be found in the Kansas experiment under the unified government of Republican governor Sam Brownback who enacted a major tax cut for individuals and businesses.[5]

On the contrary, electing a divided government has been theorized as a way for voters

[4] A unified government is not a sufficient condition for legislation to be passed, as another layer of analysis (not yet taken into account in this draft) is the existence of supermajority rules in both legislative bodies. Most states define a supermajority as either sixty percent or two-thirds of seats held by a single party. Supermajority procedures and heterogeneous preferences make gridlock also possible in a unified government.

[5] "With Brownback as governor, Kansas is in the midst of a self-described economic experiment, a project that, whatever you think of its merits, is one of the boldest and most ambitious agendas undertaken by any politician in America. Brownback calls it the march to zero, an attempt to wean his states government off the revenues of income taxes and to transition to a government that is financed entirely by what he calls consumption taxes that is, sales taxes and, to a lesser extent, property taxes." New York Times, August 05 2015

to get middle-ground policy through institutional balancing (Alesina and Rosenthal (1996)). In addition, since passing legislation requires bipartisan support, policy decisions enacted by a divided government are more likely to be durable and to survive majority changes. Democratic Massachusetts governed by Republican William Weld in the 1990s may be an illustration of a relatively smoothly run divided state.

In case no cooperative equilibrium is reached however, the default option is the statu quo, often called gridlock. Under the gridlock theory, conflict between opposing parties in a divided government increases the likelihood of stalemate in the policy-making process (Binder (2003); Bowling and Ferguson (2001)). This assumption echoes the literature on decision making in political system with veto players, which suggests that the potential for policy change decreases as the number of groups with institutional veto power increases (Tsebelis (1995)). An example of such political gridlock is the struggle over the Minnesota budget between Democrat governor Mark Dayton and the Republican controlled legislature which led to three weeks of state government shutdown in 2011.

My identification assumption in this paper is that a switch from a divided to a unified government raises policy uncertainty by ending the status quo regime and increasing the probability of economic policy changes in general, and of more polarized changes in particular. As a result policy uncertainty is assumed to be on average higher in the post election year when government switches from divided to unified relative to other post election outcomes. The main mechanism through which uncertainty can affect investment is through a real option effect. If investment is irreversible,

uncertainty increases the value of waiting for new information before investing in a project. As a result uncertainty creates incentives to postpone investment.[6] To pin down the policy uncertainty channel I conduct several cross-sectional tests to see whether firms that are ex-ante more sensitive to uncertainty have a larger response to shocks induced by switches from divided to unified governments, and from unified to divided governments. Using firm asset tangibility as well as the Kim and Kung (2014) asset redeployability indicator, I find that, consistent with theoretical predictions, the magnitude of the effect is stronger for firms characterized by a higher degree of investment irreversibility. I also test for the reverse pattern, that R&D investments positively respond to increased uncertainty, that has been reported in the literature (Atanassov et al. (2015)).[7] I do not find any significant effect for the average firm, although the smallest firms have a significant 8 percent increase in R&D intensity in the post-election year following a switch from divided to unified government. [8] The time dynamics of the sensitivity of investment to switches from divided to unified government shows that the effect is entirely concentrated in the first year after a governor is elected, which is consistent with the wait-and-see approach suggested by the policy uncertainty channel. In the second and third year uncertainty about government policies is more likely to have fallen, while some of the effects of early measures may already have materialized.

[6]Note that, as underlined by Julio and Yook (2012) the underlying mechanism does not necessarily require an extreme policy outcome to be likely to create incentives to wait. Even a positive change in policy, by reordering the ranking of the expected returns of mutually exclusive projects, can affect how the firm allocates its investment spending.

[7]They specifically find that electoral uncertainty before gubernatorial elections in the US encourages firm-level R&D.

[8]The effect adds up to the positive effect documented by Atanassov et al. (2015) in the year before the election. I confirm their effect both for the average firm and in the small firms sub-sample.

The findings are in line with cross-country evidence (Julio and Yook (2012)) that the effect of elections on investment cycles is larger for countries with fewer checks and balances on executive authority. However, contrary to the papers that look into investments short-term dynamics around election dates, I do not find evidence of a pure timing effect, as characterized by a drop and catch-up effect. My analysis suggests that the investment drop is temporary but is not counter-balanced by an over-investment pattern in the remaining years of a gubernatorial term. The observed decrease, though motivated by precautionary delay, seems to turn into a permanent reduction in the conditional mean investment rate over a gubernatorial cycle.

This paper contributes to the debate on the real effects of politics, and more specifically its effect on firm performance and corporate decisions, and to our understanding of the economic effects of checks and balances. I document novel and robust evidence that changes in full or split partisan control over a state government, within the same political regime, induce cycles in corporate investment and employment. It suggests that partisan alignment between executive and legislative powers may be as relevant to explain corporate outcomes as partisan preferences over policy, through the channel of policy uncertainty.

The paper also contributes to the growing literature on the impact of policy uncertainty on the economy and in particular on the level and the timing of corporate investment. Unlike the findings of the literature centered on short-term investment dynamics around national elections (Julio and Yook (2012); Kelly et al. (2016)), I find that the depressing effect on investment attributable to state elections, though

temporary, is not counterbalanced by a subsequent rebound. Instead, there seems to be a permanent drop in the average corporate investment rate over a gubernatorial cycle. These results highlight the interplay between political cycles and corporate decisions, and the possibility that checks and balances directly influence corporate decision-making.

The remainder of the paper proceeds in as follows. Section 1 briefly reviews the related literature. Section 2 summarizes firm characteristics and the election data. Section 3 develops the empirical predictions and discusses the identification strategy. Section 4 presents the main empirical results related to corporate investment. Section 6 extends the previous analysis to private firms by looking at corporate employment. Section 7 develops the estimation strategy using a Regression Discontinuity Design and section 8 explores the political uncertainty assumption as the underlying mechanism that could explain the results.

1.2 Related literature

This paper relates to the finance literature on the impact of politics, and of policy uncertainty, on firm outcomes and asset prices. It also contributes to the political economy literature on the consequences of divided government.

How politics shape economic outcomes is a research question of long standing.
The earlier literature focuses on the impacts of politics on the macroeconomy (see Drazen (2001) for a survey), showing strong supportive evidence of partisan business cycles in the US with the economy performing better under Democrat than Repub-

lican Presidents, in the first half of their terms (Alesina and Sachs (1988); Alesina et al. (1997)). Empirical regularities of partisan cycles have also been robustly documented in the finance literature. Santa-Clara and Valkanov (2003) find that average excess returns in the US stock market are substantially higher under Democratic than Republican presidencies. Nor business cycles neither the difference in the riskiness of the stock market explain this Presidential stock market puzzle.

More recently a micro based literature has explored which underlying channels could explain a causal effect of political cycles on financial markets and firm-level outcomes. Firm dependence on government spending is one of these mechanisms. Cohen et al. (2011) use changes in congressional committees chairmanship as a source of exogenous variation in federal expenditures channeled to the US states. They find causal evidence of a crowding out effect on the private sector economy in states affected by positive fiscal spending shocks. Belo et al. (2013) show that, conditional on the presidential partisan cycle, firm exposure to government spending predicts the cross section of stock returns. Political connections through relationships or through campaign contributions also explain heterogeneity in firms sensitivity to partisan cycles. Announcements affecting the degree of political connectedness of the firm are followed by significant reactions in stock prices (Faccio (2006); Fisman (2001)). Cooper et al. (2010) show that corporate contributions, in particular the number of candidates supported by a firm, are positively correlated with future excess returns.

On the corporate side, and closely related to this paper, several recent studies have investigated the effect of policy uncertainty on firm investment using national elec-

tion years, in a cross-country framework, as indicators of times of higher political uncertainty. Julio and Yook (2012) document that firms cut investment by 4.8% in election year relative to non-election years, on average, and that investment subsequently rebounds in the post-election year. A similar hold-on and catch-up dynamic is found by Jens (2013) within the US, in the quarters preceding and following a Governor election. Durnev (2010) shows that investment is less sensitive to stock prices during election years. Atanassov et al. (2015) show that, contrary to capital expenditures, government policy uncertainty encourages firm-level R&D. These papers focus on the dynamic of firm outcomes around the election date while previously mentioned research investigates the relation between politics and finance through the lens of partisanship. The role of institutions in general, and of checks and balances in particular, has received less attention.

In line with the election literature, this paper uses the timing of US gubernatorial elections as a source of plausibly exogenous political shocks. However it differs in that it focuses on firms response to particular outcomes of the election, reflecting the extent to which a single party controls the government. While a few papers analyzed the impact of divided government on macro economic outcomes such as growth and inflation (Alesina and Rosenthal (1989)), economic research on the consequences of divided government has mainly focused on propensity to reform. Responding to Mayhew's "Divided We Govern" claim of an absence of policy stalemate in the United-States (Mayhew (1991)), a subsequent body of empirical works suggests that meaningful legislation is less likely to pass under divided than unified government

(Binder (2003); Bowling and Ferguson (2001); Coleman (1999); Howell et al. (2000)). Coleman (1999) and Howell et al. (2000) find evidence supporting the conjecture that, in the US, unified government is much more productive as regards the quantity of important legislative enactments. Consistent with the gridlock hypothesis, Bowling and Ferguson (2001) find that when a governor faced a legislature controlled by the opposition party, divided government did make passage of conflictual policy more difficult. Andersen et al. (2012) find that the budget is 10 to 20% more likely to be late under divided government. The analysis of the economic impact of divided vs. unified governments on corporate policy decisions, at the firm level, has not been explored to the best of my knowledge.

1.3 Data description

1.3.1 Election data

This study considers 408 gubernatorial elections conducted in 46 states in the United States, between 1978 and 2010. It spans up to 8 gubernatorial elections in each state. There are 403 regular gubernatorial elections, whose timing is exogenous. Three special elections are also included. Removing them does not change the results.[9]

The primary source for the US gubernatorial election data is the CQ Press Electronic Library. Election results from 1978 to 2000 have been obtained from List and Sturm

[9]The 2003 California gubernatorial recall election was a special election permitted under California state law. It resulted in voters replacing the incumbent Democratic Governor Gray Davis with Republican Arnold Schwarzenegger. The 2010 Utah special election was conducted to fill the remainder of Jon Huntsmans term who resigned in 2009 to become the United States Ambassador to China. The 2011 West Virginia special gubernatorial election was conducted after Governor Joe Manchin resigned in 2010 to run for the US Senate, following Senator Robert Byrds death in 2010.

(2006), available at: http://personal.lse.ac.uk/sturmd/. Data from 2000 to 2010 have been updated using figures from the Congressional Quarterly database. The data on party control of state governments and legislatures have been obtained from Klarners dataset on Partisan Balance of State governments, available at http://klarnerpolitics.com/datasets/ . The final data includes the election date, the winning governor candidate and her party, whether the incumbent governor participates in the election, the vote margin in the gubernatorial election, the party holding the Senate or the House.

Currently 48 states have four-year terms for their governors. States that have a two-year terms are Vermont and New Hampshire. Arkansas before 1986 and Rhode Island before 1994 also did. They are excluded from my sample so as to compare states with the same political cycle length. Though the four-year length of a governor term is the same, states hold their elections in a staggered way. Thirty-four [10]states hold their gubernatorial elections in even numbered years which are not concurrent with Presidential elections, nine states hold their gubernatorial elections in the same year as presidential elections.[11]

Three states hold their gubernatorial elections in the year before the Presidential election (Kentucky, Louisiana, and Mississippi) and two (New Jersey and Virginia) hold them in the year after. With the exception of Louisiana, gubernatorial elections always take place on the first Tuesday in November. [12]

[10]Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Illinois, Iowa, Kansas, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nebraska, Nevada, New Mexico, New York, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Wisconsin and Wyoming.

[11]Delaware, Indiana, Missouri, Montana, North Carolina, North Dakota, Utah, Washington, and West Virginia

[12]Louisiana is an exception with respect to election dates that can be different because of the open primary system applied to gubernatorial elections.

Like the federal government, each state government is made of a legislative and an executive branch. State legislatures, divided into a lower and an upper chamber, have members elected by voters every two to four years. I am not considering mid-term legislature elections in this study. Since I am interested in the partisan control of state governments, I exclude Nebraska, which has a unicameral legislature of members with no party affiliations. I also drop elections in which one of the top two candidates is an independent. [13]

Party control of state government can exist in various configurations. A government is divided if at least the majority in one of the legislative chambers is from a different party than the governors party. Divided government can occur in a split branch form, when the legislature is Unified against the executive, as it is the case for the US Congress at the national level since Republicans have regained a majority in the Senate after 2014 mid-term elections. divided government may also mean that the legislature is itself divided (so called split legislature) as it was the case at the federal level, right before the 2014 mid-terms, with Republicans holding the House and Democrats the Senate.

As illustrated by figures 1.1 and 1.3, at the state level, over the 33 years of the sample period, swings from divided to unified government are the norm and not the exception. Half of the states have been divided on average over the period studied, with 212 gubernatorial elections held between 1978 and 2010 electing a divided government. In 60% of these elections a Republican governor was elected.

[13]This includes Minnesota (1999-2003); Maine(1995-2003); Connecticut (1991-1995) and Alaska (1990-1994).

[insert Figure 1.1 here]

[insert Figure 1.3 here]

Panel A of table 1 summarizes the election data in more details. 20% of the gubernatorial elections considered in this study resulted in a change from a divided to a unified government while 16% led to the reverse switch. More than 80% of elections, which resulted in a switch ("divided to unified" or "unified to divided"), also elected a non-incumbent governor. 9% of gubernatorial elections led to a switch from a divided state ruled by a Republican governor to a Democratic unified state. 6% of gubernatorial elections led to a switch from a divided government under a Democrat governorship to a Republican unified government. Changes in partisan alignment following gubernatorial elections, switches from divided to unified or from unified to divided, have been frequent and quite evenly distributed over the election cycle (cf. figure 1.2).

[insert Table 1.1 here]

[insert Figure 1.2 here]

Finally, as shown on the political maps (figures 1.4 and 1.5), changes in party unified or divided control over a state government are geographically widespread. All states but Georgia, Kentucky, Montana and the state of New York have at least experienced one change from divided to unified following a gubernatorial election (i.e. not including mid-term election changes).

[insert Figure 1.4 here]

[insert Figure 1.5 here]

The US case is not an exception and many Western democracies have checks and balances. In France, for instance, the term "cohabitation" is used to describe situations in which power is split between a President from one party and a parliament held by the opposition.[14] In parliamentary democracies like Germany it is also frequent that the Bundestag (consisting of members of state governments) has a different party majority than the Bundesrat (the parliament electing the federal government). Since most important laws need a majority in both chambers, legislative deadlock can possibly arise.

1.3.2 State-level data

I obtain state macroeconomic data from various sources. State finance data (budget surplus, public debt and taxes revenues) are from Klarner's State Economic Database, with original data coming from the Historical Database of State Government Finances which is maintained by the US Census of Governments. State-level real GDP growth and unemployment rates are from the Bureau of Labor Statistics (http://sdata.bls.gov).

In the last section I shift the focus of my analysis to corporate employment. I exploit the Census Bureau's Business Dynamic Statistics (BDS), produced by the US Census Bureau, which is publicly available and reports semi-aggregated statistics from the Business Register. BDS data contains annual observations on employment for establishments in the private sector and covers approximately 98 percent of US pri-

[14] Implications are likely to be different from the US case though, because the President has to appoint a prime minister of the opposing party, resulting in a split in the executive but not the legislative power.

vate employment. Information is available at the establishment-level on employment stocks and job flows, for continuing, entering, and exiting establishments. The unit of observation is the establishment defined as the single physical location where business is conducted. [15]

Establishment data is broken down by location (state) of the establishment, and within state by size and by age of the parent firms. [16]

Panel B of table 1.2 describes gross and net job flows from 1978 to 2010. The average job creation rate at the establishment level over the sample period is 17.6% and the average destruction rate is 15.3%, leading to a net job creation of 2.3%. While employment in terms of number of employees is highly concentrated in large firms[17], both creation and destruction rates are skewed towards smaller firms (less than 250 employees) and younger firms (5 year or less) which experiment a gross creation rate around 20%, 7 percentage points higher than for larger and older ones.

1.3.3 Firm-level data

The firm-level sample consists of US publicly listed companies. Data are from the merged CRSP/COMPUSTAT Fundamentals Annual database. Panel A of table table 1.2 reports summary statistics for the firm economics characteristics used in the analysis.

[insert Table 1.2 here]

[15] An establishment is a fixed physical location where economic activity occurs. Firm level data are compiled based on an aggregation of establishments under common ownership by a corporate parent.

[16] Job creations come from either opening establishments or expanding establishments and job destructions from either closing establishments or contracting establishments.

[17] Large firms, with 500 or more employees employ about 50 percent of all workers.

The sample period extends from January 1978 to December 2010. While keeping firms whose headquarters are located in the United States, I filter out observations of firms that were headquartered in Nebraska, because it has a unicameral legislature, as well as those headquartered in New Hampshire, Vermont, Arkansas before 1986 and Rhode Island before 1994 since those states have a 2-year gubernatorial election cycle. Firms headquartered in the District of Columbia and in Hawaii are also discarded.

I exclude financials (SIC between 6000 and 6999) and utilities (SIC between 4900 and 4999) because their cash holdings and investment policy may respond to regulatory supervision. I exclude firm-year observations for which information on capital expenditures, net property, plant and equipment, sales, and total assets is not available. Total assets must be available in t and t-1. Moreover, I exclude observations with negative assets, capital expenditures, share outstanding and stock prices, as well as observations with capital expenditures larger than total assets. Finally, I winsorize all ratios at the 1st and 99th percentiles to reduce the effect of outliers.This leaves an unbalanced panel of 9,558 unique firms over 33 years for a total of 91,202 firm-year observations (though the need to lag certain variables, data availability of additional covariates, as well as gaps in the panel structure of some firms will reduce the sample size used in some of the regressions).

Investment is Capital Expenditures scaled by beginning of fiscal year net Property, Plant and Equipment.[18] The mean (median) firm has an investment ratio of .27 (.22). I use the Tobin's Q investment specification, augmented by controls for firms' char-

[18]This normalization by PP&E is standard in the investment literature (see, e.g., Kaplan and Zingales 1997 or Almeida et al. 2007). My results are robust to an alternative normalization choice by lagged asset value, which yields lower coefficient estimates. Cf. table 1.21 : Robustness to alternative definitions of the investment rate.

acteristics and economic conditions. Given the lack of a suitable empirical measure of marginal q, I use the common measure of book-to-market ratio as a measure for average Tobin's Q to proxy for growth opportunities in business conditions.[19] Cash flows are Net Income before Extraordinary Items plus Depreciation scaled by lagged asset value. Firm size is measured as the natural logarithm of total assets. Sales growth is measured as the log difference in annual sales. Finally I use Research and development expenses as reported in Compustat (xrd) as a measure of R&D, for firms with non missing values. The average firm in the sample has a Q of 1.7, total assets of $1.6 billion, a leverage ratio of 24%, a profitability (ROE) of 2%, a tangibility of 33%, sales growth of 7% and a R&D intensity ratio of 7.3%.

1.3.4 Location data issue

I assign firms to their geographic location based on headquarter address information, which I initially extract from Compustat. One concern with Compustat location data is that it reports a firm's current not its historic headquarter location. And firms may have changed headquarters location. Though, Pirinsky and Wang (2006) show that in the period 1992-1997, less than 3% of firms in Compustat changed their headquarter locations, Heider and Ljungqvist (2015) estimate that for the period 1989-2011 overall Compustat's location data are incorrect for 10% off firms. I cannot

[19]Market Equity is price times shares outstanding. Price is from CRSP, shares outstanding are from Compustat (if available) or CRSP. Book Equity is the book value of stockholders' equity, plus balance sheet deferred taxes and investment tax credit (if available), minus the book value of preferred stock. Depending on availability, I use the redemption, liquidation, or par value (in that order) to estimate the book value of preferred stock. Stockholders' equity is the value reported in Compustat, if it is available. If not, I measure stockholders' equity as the book value of common equity plus the par value of preferred stock, or the book value of assets minus total liabilities (in that order).

correct for measurement error with firms changing headquarters location. It will lead to an attenuation bias pushing downwards the "true" estimate. So any significant estimated effect should be interpreted as a lower bound.

Another concern is that a firm's corporate headquarters may be in one state while its plants and operations are located in other states. Similarly firm clients and suppliers need not be concentrated around the headquarters. As a result geographically diversified firms are less likely to be sensitive to local political shocks, which will attenuate again the magnitude of my estimates. For example, Whole Foods Market is headquartered in Austin, Texas, but has a large number of stores and significant exposure in other states. The effect is likely to be meaningful since, using García and Norli (2012) textual analysis based data measuring firm geographical exposure, I estimate that roughly 18% of firms are geographically diversified at the state level over the period 1993-2008. [20] Limiting the sample period to 1993-2008 and to firms that are geographically focused in their headquarter state based on their measure does not yield, however, any meaningful results.

1.4 Empirical Results

1.4.1 Model and Identification Assumptions

My baseline empirical model to examine the effect of changes in states' government divided unified partisan control on firms' investment decisions is a Q-investment model.

[20] Under the assumption that mentions of US states in a firm's annual report identify locations where the firm has meaningful economic interests, they measure firms geographical dispersion with the number of unique states cited in their 10-K.The percentage of firms that are headquartered in state A but mentioned state B more than 50% of time in their 10-K is on average 18% over the period 1993-2008.

The model, defined in equation (1.1) below, is a standard in the financial literature and has been used in previous research looking at the impact of policy uncertainty on corporate investment thus providing a benchmark for my results (Julio and Yook (2012); Durnev (2010)).

In the next section, I will consider two other specifications for estimating the effect of the change in the unified or divided party control of a state government on investment: a Regression Discontinuity Design (RD) specification with a flexible control polynomial and an non parametric RD in which the sample is restricted to close elections.

$$\begin{aligned} I_{i,s,t} &= \alpha_i + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} \\ &+\beta_2 PostElection_{s,t} * DU_{s,t} + \beta_3 PostElection_{s,t} * UD_{s,t} + \delta_1 TobinQ_{i,s,t-1} + \delta_2 CF_{i,s,t} \\ &+\delta_3 Sale_{i,s,t-1} + \delta_4 Size_{i,s,t-1} + \gamma \Delta GSP_{s,t-1} + c_0 Republican_{s,t} + Region_{R,t} + \epsilon_{i,s,t} \end{aligned} \tag{1.1}$$

Where i indexes firm, s indexes state, R indexes Census regions and t denotes time. The dependent variable is a measure of corporate investment in year t, which is the ratio of capital expenditures in that year scaled by lagged fixed assets (property, plant, and equipment). α_i is a firm fixed effect, $Region_{Rt}$ is a Census region x year fixed effect, and ϵ_{ist} is a random error term assumed to be correlated within firm observations in a given state, as well as across firms observations and potentially heteroskedastic (Petersen (2009)).

My main variables of interest are dummy variables equal to one for firm i, headquartered in state j in year t, if state j government divided/unified partisan control changes following a gubernatorial election in $t-1$, and zero otherwise. Changes in

partisan control occurring after mid-term elections are not considered in this study. Specifically the "Switch to divided" dummy, DU_{st}, is set to one in the post election year t, when government was unified in year $t-1$ and switches to divided as a result of the gubernatorial election held in $t-1$. The "switch to unified" dummy, UD_{st}, is set to one in the post election year t, when government was divided in year $t-1$ and switches to unified as a result of the gubernatorial election held in $t-1$.

The parameters of interest are β_2 and β_3, which respectively capture changes in the conditional investment rate in the period following gubernatorial elections, after government switched from divided (unified) to unified (divided), controlling for firm investment opportunities and state economic conditions. Because of the inclusion of a firm specific effect, identification of β_2 and β_3 comes only from those firms that experience the regime shift. [21]

Following Julio and Yook (2012), I define the post-election year so that the post-election dummy variable takes a value of one for any firm-year in which an election is held no later than two months after the fiscal year beginning of year t and no earlier than 10 months before the fiscal year beginning of year t.[22] The post-election dummy variable requires that approximately 80% or more of a firm's fiscal-year days fall after the election date. All fiscal years for which the election date does not fall within this range have the election dummy set to a value of zero.[23] See 1.6 for a description of

[21] Therefore, restricting our attention to the sub-sample of firms that experience at least one such change in the Regression Discontinuity Design should yield qualitatively similar results.

[22] My results are robust to various cut-off points but the magnitude of the effect is lower for longer overlap periods

[23] As fiscal year ends are mostly concentrated in June and December, it means that for a gubernatorial election held in November of year t, fiscal year t+1 is considered the post election year for firm A whose fiscal year end is December, while fiscal year t+2 will be the post-election year for firm B whose fiscal year end is June.

the matching between election years and fiscal years.

[insert Table 1.6 here]

The baseline regression includes control variables known to affect investment decisions. I control for various measures of time-varying firm characteristics and state economic conditions, as well as for regional time trends. Firm specific variables include Tobin's Q, as an empirical proxy for marginal q, sales growth, firm size (log of firm's total assets value) measured at the end of previous fiscal year, as well as current operating cash flows (CF). I also incorporate controls for the effect of state economic circumstances. As state level economic indicators I include the one-year lagged growth rate of state GDP as well as the change in the state's unemployment rate. I include firm-fixed effects to capture invariant state institutions and unobserved firm characteristics. Because different regions of the country may face heterogeneous economic shocks, I control for region-level trends by allowing the time fixed effects to vary across Census Regions. My time effect thus contains aggregate shocks, including for example changes in federal economic policies and conditions affecting all states in similar ways, as well as shocks that are specific to one geographic region.

To take account of serial correlation over time, within and between firms, within a state, standard errors are clustered at the state level. As a robustness, I also cluster by time to capture the unspecified correlation between observations on firms from different states in the same year (Petersen (2009); Cameron et al. (2008)). The results are basically unchanged. However given the number of time periods is only 33 years this specification is not the preferred one.

To facilitate the comparison of economic magnitudes across covariates, all variables have been normalized by their sample standard deviation prior to running the regression. Each coefficient can thus be interpreted as the change in the investment rate variable (as a proportion of its standard deviation) associated with a one standard deviation increase in right hand side variables.

The key identifying assumption is that conditional on firm-level and state-level covariates and on region x year fixed effects, treated and control firms share parallel trends in the absence of a partisan control regime change. Under the identification assumptions that the state of headquarter is a relevant and well-measured indicator of firms' geographic exposure, that no important observed or unobserved variable (that varies within states across time, while being systematically correlated with switches and having an influence on investment stopping at the state border) has been omitted, and that voter choice is exogenous to expected economic conditions (so that divided states that switch to unified and divided states that stay divided do not systematically differ), then the estimates derived from the OLS specifications will accurately reflect the causal effect of policy choices.

For several reasons the OLS will only be an approximation, a lower bound plausibly, of the ATE. First the parallel trend assumption at the state level is unlikely to hold perfectly, given the differences that do exist between states in the US, and the fact that I am not comparing states starting in the exact same partisan phase of their political cycle before some switch and others do not. In addition given the location caveats that has already been mentioned, my estimate will likely be attenuated. Finally I

cannot totally rule out the possibility of an omitted variable bias, though for the results to be driven by confounders, one would have to believe that switches from divided to unified government were systematically correlated with unobserved within Census region shocks, common to states switching at the same point in time, but not to other states in that region, and that these shocks are not reflected in measures of state-specific business cycles and firm-specific measures of investment opportunities (first moment effects).

To gain confidence in the robustness of the effect, I carry out several robusteness checks, adding more controls for a wide range of state macro economic conditions and political variables, firm observed characteristics as well as varying the definition of the investment rate (endogenous variable). To approach the causality of the effect better, I use a Regression Discontinuity Design to re-estimate my regression in the next section.

1.4.2 Main Results

Sensitivity to switches from divided to unified

Table 2 reports the baseline regression results. A switch from divided to unified government is associated with a lower investment rate in the post election year. The reduction in the conditional investment rate in the year after the election that gives a single party control of the executive and legislative branches of a state government is economically meaningful and statistically significant. Decrease in the conditional investment rate for a firm in a treated state ranges between 2.5 percent and 3.7

percent relative to the average investment rate in states that do not switch to a unified government. A switch from a unified to a divided government is not found to have a significant effect on the conditional mean investment rate.

[insert Table 1.3 here]

I start with a very parsimonious specification and then add control variables in order to examine whether the negative and significant point estimate for the switch from divided to unified remains robust. The coefficient remains negative and significant in all specifications. The first column reports the results of the regression of investment on the post election and the "Switch to unified" dummies alone, controlling for year fixed effects.

The average effect is -0.01 percentage point. As some firms invest more than others and states also differ in the investment incentives they provide to firms, I incorporate firm fixed effects in column 2 to control for unobserved firm and state-level heterogeneity. The results are thus driven by within-firms variations. I also include region-by-year dummy variables to account for potential regional shocks to investment. The results are robust to the inclusion of such effects. Column 4 adds the dummy for the reverse switch of going from unified to divided and column 5 controls for investment sensitivity in the pre-election period.[24] Contrary to the literature on political uncertainty around election dates I do not find any negative effect in the pre-election year for gubernatorial elections, looking at an annual frequency. It is

[24]The pre-election dummy, which is added in (5), takes a value of one if the election date lies between 2 month prior to the end of the fiscal year t and 10 months after the end of fiscal year t. All fiscal years for which the election date does not fall within this range have the election dummy set to a value of zero.

likely the case that Presidential elections have wider implications than gubernatorial elections, specially for large public listed firm. If the effect is really subdued and temporary looking at yearly data may not be high enough a frequency to capture uncertainty effects around gubernatorial elections. Additionally because of the local reach of state-level policy only firms with high exposure to their state are likely to be affected. As a robustness in the last paragraph of this section I estimate the effect of the pre-election year on a sub-sample of small firms. In this case I find results in line with the election/investment literature (Julio and Yook (2012)).

One potential concern is that the first moment of investment opportunities may drive the investment drops as opposed as the second moment. This concern is particularly relevant given a growing body of research suggesting that uncertainty is counter-cyclical (e.g. Bloom (2009)). To address this issue, I introduce standard firm-level covariates of investment (i.e. lagged Tobin Q, cash flows, lagged percentage change in sales and lagged size) that could cause investment rate to diverge for reasons unrelated to the changes in political control of government (column 6). Next I control for local business cycle that can explain different investment patterns accross states within the same region, by adding the lagged change in state real GDP (column 7). Adding such controls has virtually weak effect on the estimated investment sensitivity to switches from divided to unified governments. This implies that the political shocks do not coincide systematically with changes in firm-level characteristics or state-level economic conditions.

To investigate whether the results are indeed due to changes in political control alignment rather than to policy differences between the Democratic and Republican Party, I use an indicator variable for the elected governor's party affiliation. The party dummy is set to one when the newly elected governor is a Republican and zero otherwise. Extensive political controls confirm the robusteness of the effect (See Appendix 1 Tables 1.17,1.18,1.19).

Specifications 7 and 8 are the same but for the clustering of standard errors. In the first case, I cluster standard errors at the state level only, to capture within state cross-sectional correlation. This is a concern, because firms headquartered in the same state are affected by the same political shock, namely, the change from a divided to a unified government. In specification 8, I also cluster by time to capture the unspecified correlation between observations on firms from different states in the same year. The double clustered standard errors are larger than the standard errors clustered by just state, indicating that there is correlation of the residuals within a year. However given I only have 33 time periods and since the results are basically unchanged, I choose a simple firm-clustering specification as my baseline specification.[25] There may be some concern that the auto-correlation in capital expenditures may contribute to the political cycles documented in this paper. The main finding is robust to the inclusion of lagged investment rates (column 9). Finally, to verify that my results are not capturing a Presidential election effect, I run the baseline specification excluding the

[25]I have 33 years (time clusters) and 46 states (state clusters) in most of the specifications. Since the clustered standard error places no restriction on the correlation structure of the residuals within a cluster, its consistency depends on having a sufficient number of clusters. This approach is unbiased as long as there are a sufficient number of clusters, in this case both enough states and enough time periods.

nine states in which governor elections are concurrent with presidential elections.[26] The coefficient on the switch to unified dummy is higher (-0.001) and significant at the 1% level.

[insert Table 1.4 here]

Sensitivity to switches from unified to divided

As mentioned before, going from a unified to a divided government is not found to have a significant effect on the conditional mean investment rate. I can see three explanations that could potentially account for the lack of response to switches from unified to divided governments.

1. First, any change in partisan control regime (i.e. from divided to unified AND from unified to divided) could be a source of uncertainty having in itself a negative effect. While a switch to a unified government may exacerbate this effect, a switch to a divided government may counterbalance it, though not enough to turn it into a rebound in the investment rate. Results presented in table 1.4 explores this possibility and corroborate my assumption. In column 2 I include both a switch dummy which takes a value of 1 after a divided state elects a unified government and after a unified state elects a divided government as well as a specific dummy for the switch from unified to divided. The latest shows up with a positive and significant coefficient of 0.007, not strong enough though to counterbalance the negative switch effect of -0.008.

[26]Delaware, Indiana, Missouri, Montana, North Carolina, North Dakota, Utah, Washington, and West Virginia

2. Another possibility is that the magnitude of the effect of a change in partisan alignment control may not be symmetric, depending which way we go. If the first year of a unified government yields more policy "surprises" than the first term of a divided government, then asymmetry is consistent with rational expectations and Pástor and Veronesi (2013) model in which expected policies do not move the market but unexpected do.

3. Finally, the lower significance of the effect of a switch to divided government may be a power issue since the number of "reverse switches" is lower than the number of switches to unified government in the sample.

1.4.3 The response of investment over time

The primary focus to this point has been on whether firms reduce investment in the year following a gubernatorial election outcome. I now turn to the dynamics of investment around a full gubernatorial term. To address this question, I construct dummy variables based on lead and lags of my variable of interest (i.e. the "T1*Switch" dummy). I then include these dummy variables in the investment baseline specification to provide a more detailed estimation of the pattern of investment over the four years of a gubernatorial cycle. The estimation results are reported in Table 1.5 and illustrated in figure 1.7. The analysis enables to investigate whether there is any pre trend in investment that would differ significantly between treated and control firms and could indicate that firms anticipated the change in partisan control of the state government. In absence of pre trends, in T=0, when neither the results of the elec-

tion nor its effective outcome are realized, we should not see any significant impact on investment. Reassuringly the T0 coefficient $T_0 * 1_{SwitchToUnified}$ is not statistically different from zero. Firms react after the election outcome is fully observed.

[insert Table 1.5 here]

[insert Figure 1.7 here]

A natural question is how persistent is the effect associated with switching to a unified government. If, as suggested in the last part of this paper, a change to a unified government translates into higher policy uncertainty then we should expect the effect to be temporary. Typically in the following years as economic agents have started to learn about government policy choices and about their impact on the economy the effect should lower. As uncertainty is not fully resolved we may not observe a full reversal though.

The results are consistent with this predictions as coefficients estimates for years T=+2 and T=+3 are non statistically different from zero. However the absence of any off-setting rebound in the investment rate suggests that the shift from a divided to a unified government leads to a permanent effect and not a temporal re-allocation of investment around the gubernatorial cycle. It could be that investment starts to increase only gradually or too slowly to capture the effect over my estimation window, which is limited to two years after the switch as another gubernatorial cycle starts after that.

Another explanation is that mid-term elections in year T=2 can change the split vs. full control of a party over the government so that it attenuates my estimates.

Finally a joint test on my four coefficient of interest to test that they are equal to zero yields a p-value of 0.1051. Though I cannot reject the null hypothesis of a pure reallocation effect, it is very close to the 10% cut-off. The same test in different sub-samples explored later in the analysis (small firms, high PP&E ratio) of firms ex-ante more likely to respond to the shock rejects the null.

It would also have been worrisome if the baseline for investment was negative across a unified government term as it could suggest that those states that switched to unified were those for which divided government was associated with a poor economic performance. This is not the case. This suggests that the negative relationship is not driven by a shift in partisan alignment control of government responding to deteriorating economic conditions.

1.4.4 Robustness

In this section, I perform several robustness checks.

Additional State-level controls

First, I estimate the regressions omitting the two final years of the sample period, 2009 and 2010, as there is some concern that the financial crisis itself could be driving the results. The results remain intact. Next, I add further state-level macro controls in my base specification to ensure that state government unified or divided control changes are not triggered by other observed factors that in turn cause firms to adjust their investment rate for reasons unrelated to the change in regime itself. For example, unified government may be more likely to be elected when the state runs a budget

deficit as they have been shown to be able to react and adjust more efficiently.

Table 1.16 in Appendix 1 reports that the results are robust to the inclusion of additional state-level macro variables, such as lagged changes in state unemployment rate, in state budget surplus, state debt outstanding and state tax revenue, as a percentage of GSP. The effect of lagged changes in state unemployment rates is negative, consistent with firms investing less when economic conditions in their home state worsen but not statistically significant as it is collinear with the lagged growth in state gdp. Conditioning on including growth in state gdp as a control only budget surplus and the total debt outstanding reported to gdp have significant additional explanatory power on the investment rate, likely channelling to firms exposed to public spending. Across all these specification the effect of a switch to a unified government remains significant an its magnitude is almost unchanged.

Tables 1.17, 1.18 and 1.19 report estimation results for further political robustness controls. Column 1 and 2 report the estimates from the inclusion of a dummy which is set equal to one when government is divided before the election and stays divided as a result of the election. Columns 3 and 4 report the estimates from the inclusion of a dummy which is set equal to one when government is unified before the election and stays unified as a result of the election. Staying unified or staying divided has no effect on the investment rate and both dummies are positive. As mentioned previously it seems that this is the change from one regime (divided) to another (unified) that matters. Column 5 and 6 look at the effect of a change in the party of the elected governor. Columns 7 to 9 add a "non-incumbent" dummy variable and its interactions

with our treatment variables.

The non incumbent dummy is set to 1 when the elected governor has not served in the past. The assumption is that the election of an incumbent governor should lead to lower uncertainty as firms have prior knowledge of the governor achievements and capabilities as well as of his ability to work with the Legislature. Surprisingly, I find no evidence of variation in the post-election investment rate depending on whether an incumbent or a new governor is elected. Table 1.18 presents estimation results controlling for the party of the elected governor and excluding Southern states.[27]
I control for the partisanship of the elected governor by interacting the party of the governor with the post-election year indicator variable as well as with the indicators of a change to divided or to unified. The results are unchanged.

In table 1.19, I investigate whether the political platform of the incumbent governor affects the investment sensitivity to the switch from divided (unified) to unified (divided) government. For example, if we assume that Republican governors may be more business-friendly, then the election outcome might be different from the firm's perspective, if the state is unified under a Republican legislature or a Democrat one; similarly the effect of going from unified to divided could be different given the party of the incumbent governor. To the extent that right-leaning governors are more market-friendly, we expect that firms view a transition from republican-to-divided as having more potentially bad news than a possible democrat-to-divided transition of power. I control for the partisanship of the incumbent governor by interacting the

[27]South is defined as the 16 states that make the Census South region : DE, FL, GE, MD, NC, SC, VA, WV, AL, KY, MS, TN, AR, LA, OK, TX

party of the leaving governor with the post-election year indicator variable as well as with the indicators of a change to divided or to unified. The governor party dummy is set to one for Democrat governors and zero otherwise. The results are unaffected.

Weak partisan effects are consistent with a second moment effect or uncertainty channel and not with a first moment channel tied to policy preferences under Democrats or Republicans. This absence of partisan effect is not surprising given the fact that previous literature results (Besley and Case (1995)) show weak evidence for the influence of the individual partisanship of the governor on various economic outcomes including tax and spending policies.

Additional firm-level controls, investment definitions and treatment year definitions

For additional robustness, I estimate the regression equation including additional firm-level control variables such as leverage, asset tangibility, cash holdings and age. Table 1.20 in Appendix 1 shows that all these covariates have significant explanatory power but they do not affect the results for our variables of interest. In table 1.21 I use different definitions of the investment rate, scaling capital expenditures by total assets, using the growth rate in the absolute amount of capital expenditures or the first difference in the investment rate.

The switch from a divided to a unified government is still associated with a negative and significant effect on the left hand side investment variable. Finally, in table 1.22, I check that the results are not sensitive to the definition of the post-election year i.e.

to the matching process between fiscal data and election data.

The main specification uses the same definition as in Julio and Yook (2012) which requires the post-election year dummy variable to take a value of one for any firm-year in which an election is held no later than two months after the beginning of the firm fiscal year. I test for different cut-offs, 1 month, 3 months and 4 months. My results are robust, the magnitude of the effect being slightly lower for longer overlap periods.

Overall results are robust to varying specifications to account for additional firm-level characteristics, state-level economic variables and political controls. The main caveat of this analysis remains that, because the reach of state policy is local, identifying firms or industries more exposed to their home state is critical to properly gauge the effects and channels through which state policy may affect corporate decision making. Because Compustat firms are generally multi-state firms their exposure to their headquarter state's policy is lower than for single-state firms and estimated coefficients should be interpreted as an attenuated effect.

To address this concern in the next section I first restrict my sample to a sub-sample of small firms, under the assumption that they are more likely to be geographically concentrated in their home state. More importantly, I then extend my analysis to private sector firms. I exploit employment data from the Census Bureau's Business Dynamics Statistics, which covers approximately 98 percent of US private employment and removes any location bias that could have been introduced by the use of the Compustat headquarter indicator.

1.5 State exposure : Small firms and Employment evidence

1.5.1 Small firms analysis

Table 1.6 presents results obtained by running the baseline investment specification on five sub-samples of the population based on firm size. In column 1 the model is estimated for the smallest firm, defined as firms in the bottom quintile of the total assets distribution over the full sample period so that the sub-sample composition is not changing over time except by entry and exit dynamic. Concerns that those firms have other unobserved characteristics that may have prevented them to grow further over the sample period are alleviated by the presence of firm fixed effects.

[insert Table 1.6 here]

The smallest ones are more likely to have operations concentrated in their home state and hence to be more sensitive to state political cycles. In line with this assumption, results are stronger, with a drop in the post-election year investment rate which is twice as large as the average effect, for the first quintile sub-sample.

In this specific sub-sample, I find a significant drop and rebound in the investment rate, around the election date, which is consistent with the empirical literature findings (Julio and Yook (2012)) on policy uncertainty and investment cycles. Interestingly the magnitude of the effect associated with a transition from a divided to a unified government in the pot election year is about three times larger as the pre-election uncertainty effect on which the literature has focused.

1.5.2 Private sector employment analysis

This section consists in an extension to corporate employment decisions of private firms. As employment can be seen as investment in new workers, partly irreversible because of the costs of hiring, training and firing, jobs creation and destruction should exhibit similar patterns as investment.

Empirical strategy

I exploit data from the Census Bureau's Business Dynamics Statistics, which covers approximately 98 percent of US private employment. Using Compustat would have the double disadvantage of the location issue previously discussed and a limited coverage as it represents only about one-third of employment in the United States (Davis and Haltiwanger (1992)). BDS data contains annual observations on employment for all establishments in the private sector, covering 1978–2010.[28][29]
I follow the same empirical strategy as for investment.

I successively look at job creation rate, job destruction rate and net job creation rate as dependent variables. For each state, job creation (destruction) rate is the ratio of the sum of all employment gains (losses) from establishments located in that state, from year t–1 to year t, including establishment startups (deaths), divided by average of state employment for times t and t-1. All growth rates are based on

[28] I start my analysis in 1978 as in the investment case but results are robust to starting in 1979 because of anomalies in the BDS data that have been pointed out by previous researchers (Moscarini and Postel-Vinay (2012)).

[29] An establishment is a fixed physical location where economic activity occurs. Firm level data are compiled based on an aggregation of establishments under common ownership by a corporate parent.

March–to–March changes and the tabulations for a given year are the changes from the prior year to the current year.

Job creation (destruction) rate is matched to elections based on the location of the establishment. A gubernatorial election occurring on November of year t is matched with changes in employment from March t to March t+1 and the post-election year is matched with changes in employment from March t+1 to March t+2.

Following the literature, I control for state population (natural log of state population) as range in population between states and across time is large and for real gross state product per capita. After controlling for state population, this term can be thought of as a rough proxy for average employee productivity. I also control for the size of the parent firm since smaller firms account for most of the job creations. I use initial firm size[30], which is defined for any given consecutive two–year period as the size at year t–1, to avoid reclassification bias.

Data is agregated at the level of the location (state) of the establishments and broken down within state by size and by age of the parent firms. I will thus estimate this model on two different set of data, the first set with observations at the state x size level and the second set at the state x age level.

[30]In BDS, after computing growth, firms are reclassified into their new size classes, and their new size becomes their initial size in the following period, March of year t to March of year t + 1.

The baseline specification for a group of establishments into firm size or age category k, located in state s, at time t is :

$$JCR_{k,s,t} = a_k + \alpha_s + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta_2 PostElection_{s,t} \times DU_{s,t}$$
$$+\beta_3 PostElection_{s,t} \times UD_{s,t} + \gamma_1 \Delta RealGSP/capita_{s,t-2} + \gamma_2 Size_{k,s,t-1} \qquad (1.2)$$
$$+\gamma_3 StatePopulation_{s,t-2} + c_0 Republican_{s,t} + Region_{R,t} + epsilon_{k,s,t}$$

a_k are firm size or firm age fixed effects. α_s is a state fixed effect. The election year dummy (T0) takes a value of one for any state-year following the year in which a gubernatorial election is held and a value of zero otherwise. As employment rates are reported as of March 12 of each year in the BDS, a gubernatorial election occurring on November of year t is matched with changes in employment from March t to March t+1. The post-election dummy (T1) is defined relative to the election year as T0 + 1 year. The Switch to unified (Divided) dummy is set to 1 when government is divided (unified) the year the gubernatorial election takes place and becomes unified (divided) the year after, as a result of the election. Republican governor is an indicator variable that takes a value of one when the governor is a Republican and zero otherwise.

Results

A weakly significant negative effect is found on the job creation rate following a switch from divided to unified government. The magnitude of the effect varies between -0.3 and -0.25 percentage point. Results by state x size are presented in table 1.7 and results by state x age are presented in table 1.8. The absence of any effects on the job destruction side could point to some asymmetries in the job adjustment process

with more hysteresis on the job destruction side. The net job creation however, whose coefficient estimate is the difference between job creation job destruction, is not statistically significant.

[insert Table 1.7 here]

[insert Table 1.8 here]

In table 1.9, I estimate the same model over 4 sub-samples based on the size of the parent-firm, as measured by its number of employees. The coefficient estimates (-0.45) when the baseline specification is run on a sub-sample of firms with less than 20 employees is twice as large as the average effect and significant at the 10% level. For larger firms the sign of the coefficient is negative but the effect is not statistically significant. Results for larger size firms should be interpreted with precautions because the size criteria is based on the size of the parent firm whose location can be different than the one of the establishments for which the job flows are reported. Looking at the sub-sample of smaller firms however is relevant since the total number of firms and of establishments reported in each state are really close, suggesting that those firms are mainly single establishment firms or multi-establishment firms located in the same state as their parent firm. As such they have a higher exposure to their state policy.

A weakly significant effect is found on the job destruction rate as well for the smallest firms. This could suggest a "wait-and-see" effect since job destruction rate is also negatively affected so that the net creation rate remains basically unchanged.

[insert Table 1.9 here]

Overall, the employment results support the investment empirical findings with switches from a divided to a unified government leading firms to hold back on their employment decisions, both on the creation side and on the destruction side. Consistent with the assumption that state political cycles matter, younger firms and smaller firms for which we are thus almost certain that they are entirely located in a given state, are found to be the most affected.

In the next section, I use a regression discontinuity design to address remaining concerns that unobserved state-level economic conditions or anticipated state-level economic conditions may be driving both the election outcomes and the investment/employment cycle.

1.6 Regression Discontinuity Design

One of the concern for identification is that that voters' decision may not be exogenous to anticipated economic conditions when making their party choice. For example it could be that voters think that divided government are better able to manage economic booms but believe that a unified government is better suited to address crisis times. If anticipated economic conditions are not fully captured by the covariates, then the documented drop in investment may just reflect an expected change from a growth to a recession economic environment, perfectly correlated with a switch to unified government.

To account for this possibility, I use a regression discontinuity design (RD). Recent contributions implementing a similar design in gubernatorial elections include Folke

and Snyder (2012) and Erikson et al. (2015). As stated by Erikson et al. (2015), the basic idea behind this is that in very close elections the party of the governor is decided essentially by a coin flip. Thus, looking at a sub-sample of states that were divided ex-ante, those states that became unified because a governor barely won an election and those that stayed divided because a governor barely lost an election should be nearly identical except on dimensions that are directly affected by the election outcome.

Focusing on these very close elections therefore allows me to estimate the causal effect of full or split government partisan control, as decided by the party of the governor. The policy effect is estimated from the discontinuity that occurs when a gubernatorial candidate wins more than 50% of the vote so that the party affiliated with the Governor in the state Legislature switches from being the minority party to being the majority party.

Empirical strategy and identification assumptions

The RD regression follows the control function approach. As conventional in RD approaches, I use a separate control function on each side of the threshold by including interactions between the treatment variable "*DU*" and the electoral victory margin. The control function is a second or third-order polynomial function of the forcing variable, $V_{s,t-1}$, which is the margin of winning or losing in the gubernatorial election for the candidate whose party is aligned with the majority party elected of the Legislature in state s at time t−1. V is centered at the cut-off point prior to running the regression.

The basic idea behind this specification is that the treatment variable, Switch to Unified, is entirely determined by the forcing variable. Because of this, we can control for potential endogeneity of the treatment variable, and also deal with other problems, such as omitted variable bias, by allowing flexibly in the forcing variable.

The specification is then:

$$I_{i,s,t} = \alpha_i + f(V_{s,t-1}) + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta_2 PostElection_{s,t} \times DU_{s,t}$$
$$\delta FirmControls_{i,t} + \gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t} \qquad (1.2)$$

with

$$f(V_{s,t-1}) = b_1 V + c_1 V^2 + d_1 V^3 + b_2 V \times DU + c_2 V^2 \times DU + d_2 V^3 \times DU$$

Following the literature (e.g. Erikson et al. (2015); Folke and Snyder (2012)), I limit the sample to observations where the parties have between 30% and 70% of the vote share to reduce the possibility of over-fitting the control polynomials by including outliers in the tails of the vote share distribution.

I also restrict my sample to divided states in the pre-election term and to states that switch either to being unified after the election, or to being divided but with the governor being confronted to a majority from the opposite party in both chambers of the Legislature. Basically I drop split legislatures from the divided cases considered in the post election term. This is to ensure that only the governor election results can potentially change the treatment assignment from divided government to unified government or vice versa, as described on figure 1.8.

[insert Figure 1.8 here]

The main drawback from this approach is that it provides global estimates of the regression function that uses data far from the cutoff. As further robustness I also use a non parametric RD approach, which limits the sample to "close" elections i.e. those where the winner's share of the vote is close to 50%. I present results based on different vote margins around 50% to define close elections. I use a 3% and a 2% window i.e. I use observations that are at maximum 3% away or 2% away from the treatment boundary. Though I have too little observations in these windows (35 and 29 elections respectively) for my coefficients to be consistently estimated, presenting the full battery of estimates shows whether or not the estimates found using tight bounds — which have small sample sizes — are stable as we move away from the threshold and increase the sample size.

The identifying assumptions of the approach are the following. First, there has to be some randomness in the final election results. Second, there must not be any sorting around the discontinuity, i.e. there must not be any manipulation of election results by candidates close to the threshold. This assumption will be checked by investigating the smoothness of the density of observations around the threshold. My case departs from standard RD applications using electoral victory margin as the outcome is measured at the firm level, while the close-to-randomization effect under the RD assumption is at the state level. However it provides a nice robustness test of the OLS results derived in section 3.

Given that the switch to a divided or a unified government is the joint outcome from several elections, the Governor election but also all the elections of the members of the

State Lower and Upper House, the assumption that individuals do not have precise control over the forcing variable seems reasonable.

Common practice is to graph the density of the assignment variable to see if there are any discontinuity at the cut-off point, with a very different number of observations just below and above the threshold, driven by the treatment being perceive as desirable or not. However it should be noticed that even if there is some kind of sorting around the discontinuity threshold, it would not necessarily affect the validity of the set up for the outcome of interest as being exogenous to business is different from being exogenous to voters. For finding an imbalance to be a concern one should assume that there exists some unobservable variable that affect the choice of the voters for a unified or divided government and that this variable also directly affect business investment.

As reported in panel B1 of table 1.1, which summarizes the election margin variable, the gubernatorial election margin variable is distributed quite symmetrically about 0, with a mean of -0.2% and a standard deviation of 8.37%, and an inter-quartile range of -6.1 to 6.1. The Republicans control the governorship in 46% of the elections. In more about 45% of the elections in our main sample (53 out of 118) the winning margin is below 5%. However, as illustrated in histograms of figures 8 and 9, the distribution is not as smooth as one could expect in a larger sample size. There are not the exact same numbers of observations in the interval [-4;0) as in the interval [0;4). I have 18 elections below the threshold and 25 above. Because of my limited sample size I cannot rule out that there is no sorting of observations across the threshold. However

the hypothesis seems unlikely to be directly related to the investment outcome since there are more states above than below the threshold, while crossing the threshold is unattractive from the firms' point of view.

[insert Figure 1.9 here]

[insert Figure 1.10 here]

Graphical Analysis

Following previous RD work, we begin with a graphical analysis. Figures 9A and B illustrate the regression discontinuity in the switch to unified government context (fig.10) and in the switch to divided context (fig.11). It plots the estimated change in firm investment rate in t + 1 (T1) as a function of the vote share margin of victory in election t (T0) of the gubernatorial candidate whose party is aligned with the majority party holding the legislature. Each point is an average of changes in firm investment rate in t + 1(T1) for each interval, which is 0.5% wide. In figure 1.11, points to the left of the y axis represent subsequent investment outcomes when previously divided governments stay divided, those to the right are previously divided governments that switch to unified.

As apparent from the figure, there is a discontinuous jump, right at the 0 point, indicating that investment drops in states that barely switched to unified while it remains stable in those that barely stayed divided. As long as states in which the candidates, whose party is aligned with the majority winning the legislature, barely win and states in which they barely lost, are ex ante comparable (on average) in

all other ways, the difference can properly be interpreted as the causal effect of the election outcome i.e. the change from a divided to a unified government.

In figure 1.12, points to the left of the y axis represent subsequent investment outcomes when previously unified governments stay unified, those to the right are for previously unified government that switch to divided.

[insert Figure 1.11 here]

[insert Figure 1.12 here]

Regression Analyses

I now turn to regressions. Table 1.10 presents the main results of the RD estimation for the switch to unified.

[insert Table 1.10 here]

Specification 1 is the baseline specification, estimated on the sub-sample used for the RD. The coefficient estimate of the switch to unified (-.012 ; p-value < 0.05) is higher to the one for the full sample estimation (-0.008), suggesting that OLS under estimate the investment sensitivity somewhat.

The results for the RD specifications show quite stable point estimates for the RD, in the range -0.012 to -0.020 (see columns 1 to 6). The magnitude of the effect translates into a 4.5 percent to 7.5 percent drop when reported to the average investment rate. The results hold with and without covariates and are robust to a placebo test in the year before the election outcomes are known (cf. figure 10 in next section). Finally, when looking at very close races (columns 7 and 8), the non parametric approach

yields a negative effect of a similar order of magnitude. Given the small number of observations though the result is not robust.

To have more observations one should explore the possibility of using a modified discontinuity design, with treatment resulting for the joint outcome of multiple assignment variables, not limited to governor elections but also including elections of each senate, and house members.

Table 1.11 presents the results of a similar analysis for the switch from unified to divided. The assignment variable is now the share of the votes received by the gubernatorial candidate whose party is not} aligned with the party controlling the house and the senate. In this specification, the policy effect is estimated from the discontinuity that occurs when a gubernatorial candidate wins more than 50% of the vote so that the party affiliated with the Governor in the state Legislature switches from being the majority party to being the minority party. The coefficient estimates are very similar to the estimates from the main OLS specification. They are slightly negative, quite stable across the specifications and none of them is statistically significant. The post election dummy in this case is never significant either which may suggest that these races were ex-ante less uncertain than the elections of the first sub-sample.

[insert Table 1.11 here]

Robustness

To check the validity of the regression discontinuity design, I conduct a placebo test using the exact same sample of states but looking at investment rate in the election

year i.e. before the election results are known. There is no discontinuity that could have indicated hat the effect was anticipated or that the effect is driven by ex-ante already present differences between those states (figure 1.13). As suggested in Imbens and Lemieux (2008), I then only look at one side of the discontinuity (when government switch to unified) and take the median of the margin of victory, which is equal to 6.5%, and I test for a discontinuity in that part.There is none (cf. figure 1.14).

Finally I also test whether other covariates exhibit a jump at the discontinuity. I do not find any evidence of discontinuity at the margin of victory threshold, as illustrated by figure 1.15. This is confirmed by a formal regression analysis using the covariates as a left-hand side variable. The RD estimates support the results from the difference-in-difference approach, and show that giving a single party full control over a state, by winning the governor's election, systematically leads to a significant drop in the investment rate of firms headquartered in that state in the post-election year. Although the RD estimates enable us to gain confidence in establishing that there is a negative effect of a change from a divided to unified government on corporate investment, they say little about why this is the case. In the rest of the paper I explore the assumption that policy uncertainty may be one of the underlying mechanisms driving the results.

[insert Figures 1.13,1.14, 1.15 here]

1.7 The Policy Uncertainty Channel

In this section, I investigate the assumption that policy uncertainty is one of the potential channels through which a change from a divided to a unified government may lead to measurable changes in economic activity.

Government policy, including state level policy, represents a large source of uncertainty for many firms. Firms do care about which laws or regulation is likely to be passed and how it may impact their profits and operation capabilities (cf. examples of 10-Ks citations from the risk factors sections). The topic has received a lot of interest lately with a growing literature looking at the relation between policy uncertainty and corporate investment (Julio and Yook (2012); Gulen and Ion (2015)) or asset prices (Pástor and Veronesi (2013); Kelly et al. (2016)). While Democratic and Republican parties have different political agendas, the concept of unified government is important in state lawmaking because the governor, the senate and the house all play decisive roles in turning a partisan agenda into state legislation.

Election results partially resolve uncertainty related to future government policies, but may not be very informative about the probability that a policy shift will occur. It seems likely that whether a given party fully controls the government affects its ability to pass bills, and that this ability is higher when the elected government is unified.[31]

[31] A unified government is not a sufficient condition for legislation to be passed, as another layer of analysis – not yet taken into account in this draft - is the existence of supermajority rules in both legislative bodies. Most states define a supermajority as either sixty percent or two-thirds of seats held by a single party. Supermajority procedures and heterogeneous preferences make gridlock also possible in a unified government.

As American politics is polarized, unified governments are likely to yield strong partisan outcomes in the form of more extreme economic policy choices. An example of such choices can be found in the "Kansas experiment" under the unified government of Republican governor Sam Brownback who enacted a major tax cut for individuals and businesses.

On the contrary, electing a divided government has been theorized as a way for voters to get middle-ground policy through institutional balancing (Alesina and Rosenthal (1989); Fiorina (1991)). In addition, since passing legislation requires bipartisan support, policy decisions enacted by a divided government are more likely to be durable and to survive majority changes. Democratic Massachusetts governed by Republican William Weld in the 1990s illustrates a relatively smoothly run divided state. In case no cooperative equilibrium is reached however, the default option is the status quo, often called "gridlock".

Under the gridlock theory, conflict between opposing parties in a divided government increases the likelihood of stalemate in the policy-making process Binder, 2003; Bowling, 2001). This assumption echoes the literature on decision making in political system with veto players, which suggests that the potential for policy change decreases as the number of groups with institutional veto power increases (Tsebelis (1995)). An example of such political gridlock is the struggle over the Minnesota budget between Democrat governor Mark Dayton and the Republican controlled legislature which led to three weeks of state government shutdown in 2011.

1.7.1 Empirical strategy and identification assumption

My identification assumption in this section is that a switch from a divided to a unified government raises policy uncertainty by ending the status quo regime and increasing the probability of economic policy changes in general, and of more polarized changes in particular. As a result policy uncertainty is assumed to be on average higher in the post election year when government switches from divided to unified relative to other post election outcomes.

The main mechanism through which uncertainty can affect investment is through a real option effect. If investment is irreversible, uncertainty increases the value of waiting for new information before investing in a project. As a result uncertainty creates incentives to postpone investment.[32] To pin down the policy uncertainty channel I conduct several cross-sectional tests to see whether firms that are ex-ante likely to be more sensitive to uncertainty have a larger response to shocks induced by switches from divided to unified governments, and from unified to divided governments.

1.7.2 Investment irreversibility

In this subsection I explore the predictions of the real option theory of investment that firms that exhibit higher investment irreversibility are especially sensitive to uncertainty.

When investment expenditures are irreversible (because capital is firm or industry

[32]Note that, as underlined by Julio and Yook (2012) the underlying mechanism does not necessarily require an extreme policy outcome to be likely to create incentives to wait. Even a positive change in policy, by reordering the ranking of the expected returns of mutually exclusive projects, can affect how the firm allocates its investment spending.

specific) or partially irreversible (because of "lemons" problems and low resale value), and can be delayed, allowing the firm to wait for new information about market conditions before committing resources, then greater uncertainty creates incentives to wait before investing (Pindyck (1991)). Uncertainty about future investment opportunities raises the opportunity cost of investing (the value of the option to invest) and creates incentives for firms to delay capital expenditures. The intuition behind this prediction is that the region of inaction for investment policies expands when uncertainty increases, and more so for firms whose capital is more difficult to liquidate in bad times (Bloom (2009)).

If the sensitivity of investment to changes in party alignment control of state government operates through the uncertainty channel then we should expect firms with more irreversible investments to be more affected.

I use the ratio of fixed assets to total assets measured as PPE divided by total assets as a proxy for investment irreversibility. Following the literature (e.g. Gulen and Ion (2015)) I interpret firms with higher ratios of fixed to total assets as having higher adjustment costs. I rank firms based on their fixed assets ratio and assign them to terciles of the distribution and run three separate regressions fro each of the sub-samples. Table 1.12 presents the results.

[insert Table 1.12]

[insert Table 1.13]

Consistently with the policy uncertainty assumption the magnitude of the coefficient is larger in the sub-sample with a higher PP&E ratio (third tercile) - a proxy for

higher investment irreversibility - than in the rest of the population. It is about twice the average population effect. As asset tangibility may be positively correlated with firm size, and as large firms are more geographically diversified the total effect may be attenuated. I then restrict my sample to small firms (table 1.13) (total assets below the median of the population distribution) and split it further to estimate the model over two sub-samples of "high tangibility" and "low tangibility" firms, based on their PP&E ratio being above or below the median of the ratio distribution in the small firms sample.

The magnitude of the negative effect of a switch to a unified government increase to 0.16 percentage point in the sub-sample of firms which are both more exposed to their home state and have a higher degree of irreversibility. Though the findings are in line with the assumption of a policy uncertainty channel, t-tests for statistical difference of the effect across these sub-samples are not significant as standard variation is high.

The main caveat of using the PP&E ratio is that it does not take into account other determinants of adjustment costs such as asset specificity or mobility. To address this concern, I then use an industry-level measure proposed by Kim and Kung (2016) as a proxy for the salability of assets across industries. The intuition is that firms in industries with more redeployable assets will benefit from a more active second hand market for these assets and will therefore be able to recover a higher proportion of their investments.

[insert Table 1.14]

Kim and Kung (2016) construct their measure of asset redeployability based on the

salability of assets across industries. They measure the across-industry redeployability of a given asset by computing the proportion of industries in which the asset is used (using the Bureau of Economic Analysis 1997 capital flow table). The more industries use an asset, the more redeployable is the asset and thus the higher the redeployability score. A lower redeployability score is interpreted as higher costs of reversing investment decisions in the absence of a liquid secondary market to sell the assets. I find results in line with the predictions of the literature on irreversible investment for a sub-sample of small firms i.e. for firms that are likely to be more sensitive to state-level policy.

As shown in table 1.14, firms with less redeployable assets decrease investment significantly more in response to elections leading to a shift from a divided to a unified government than their counterparts with more redeployable assets. In this specific sub-sample, the magnitude of the effect is twice as big as the drop in investment in the pre-election period, typically documented in the literature using electoral cycles as a proxy for policy uncertainty.

Overall the results are consistent with the assumption of policy uncertainty rising with a change in regime from a divided to a unified government and translating into an negative effect on investment, through a "wait-and-see" channel.

1.7.3 Policy Uncertainty and R&D intensity

In this subsection I test for the "bright sides of political uncertainty" (Atanassov et al. (2015)) that R&D investments respond positively to increased uncertainty, in

line with previous empirical literature findings (Atanassov et al. (2015) [33]). Results are reported in table 1.15.

I keep the same specification but the dependent variable in all regressions is now R&D intensity, defined as the ratio of R&D expenditure to total assets. I do not find any significant effect for the average firm. However the smallest firms have a significant 8 percent increase in R&D intensity in the post-election year following a switch from divided to unified government. The effect adds up to the positive effect documented by Atanassov et al. (2015) in the year before the election. I confirm their effect both for the average firm and in the small firms sub-sample.

[insert Table 1.15]

1.8 Conclusion

This paper contributes to the debate on the real effects of politics, and more specifically its effect on firm performance and corporate decisions, and to our understanding of the economic effects of checks and balances.

Using a sample of 408 US gubernatorial elections held between 1978 and 2010, which resulted into 82 staggered switches from divided to unified governments, I document novel and robust evidence that changes in full or split partisan control over a state government, within the same political regime, induce cycles in corporate investment and employment.

[33] They specifically find that electoral uncertainty before gubernatorial elections in the US encourages firm-level R&D.

Controlling for investment opportunities (Tobin's Q and cash flows), demand (sales-growth), overall economic conditions (GDP growth and unemployment rate) and partisan effects (party of the governor, incumbency, change in partisan majority among others), I find evidence of a persistent negative relationship between switches from divided to unified governments and investment. Specifically, corporate investment rates drop by an economically meaningful average of 3 to 5 percent in the year following elections when government switches from unified to divided, relative to investment rates in other post-election years.

These results are robust to various empirical specifications including a regression discontinuity design using the discontinuity that occurs when a gubernatorial candidate wins more than 50% of the votes, leading his party to switch from the minority to the majority party in the state legislature.

Evidence of a higher sensitivity for smaller firms, as well as capital intensive firms supports the hypothesis that changes in partisan control alignment translate into changes in the degree of policy uncertainty, which in turn affects the investment and employment cycles. It suggests that partisan alignment between executive and legislative powers may be as relevant to explain corporate outcomes as partisan preferences over policy, through the channel of policy uncertainty.

More broadly, these results highlight the interplay between political cycles and corporate decisions, and the possibility that checks and balances directly influence corporate decision-making and that political compromise or political gridlock leads to smoother business cycles.

Figure 1.1: State legislatures under divided government, 1978-2010

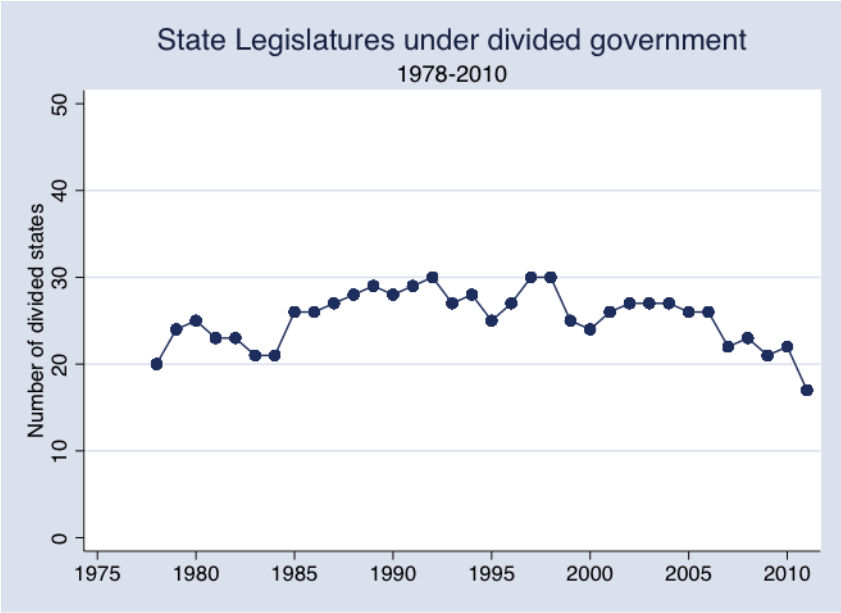


Figure 1.1 tracks the number of US states out of the 46 states in the sample, which have a divided government, for each year, over the sample period 1978-2010.

Figure 1.2: Gubernatorial Elections, Partisanship and State Government Control Changes over time, 1978-2010

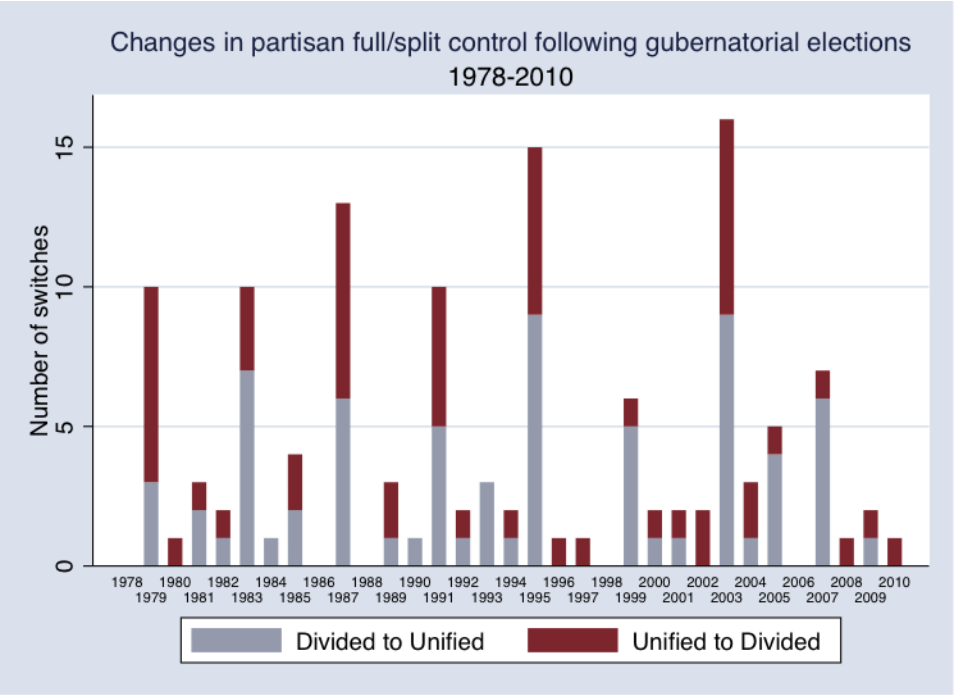


Figure 1.2 plots changes in partisan alignment following gubernatorial elections over time, for the 46 states in the sample, over the sample period 1978-2010.

Figure 1.3: Partisan control of state legislatures, 1978-2010

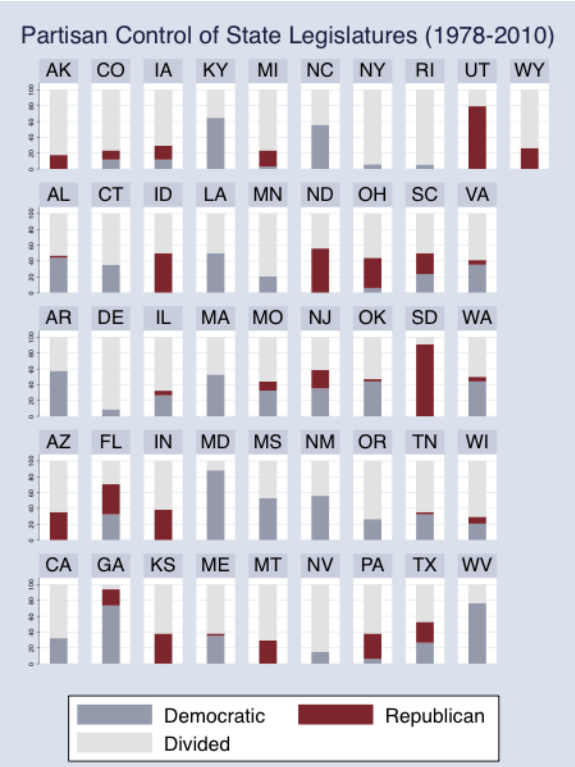


Figure 1.3 represents the percentage of time a state has been governed by a unified Democratic government, a unified Republican government or a divided government, for the 46 states in the sample, over the sample period 1978-2010.

Gubernatorial elections and geography of changes in partisan control of state governments, 1978-2010.

Figure 1.4: Number of Switches from divided to unified.

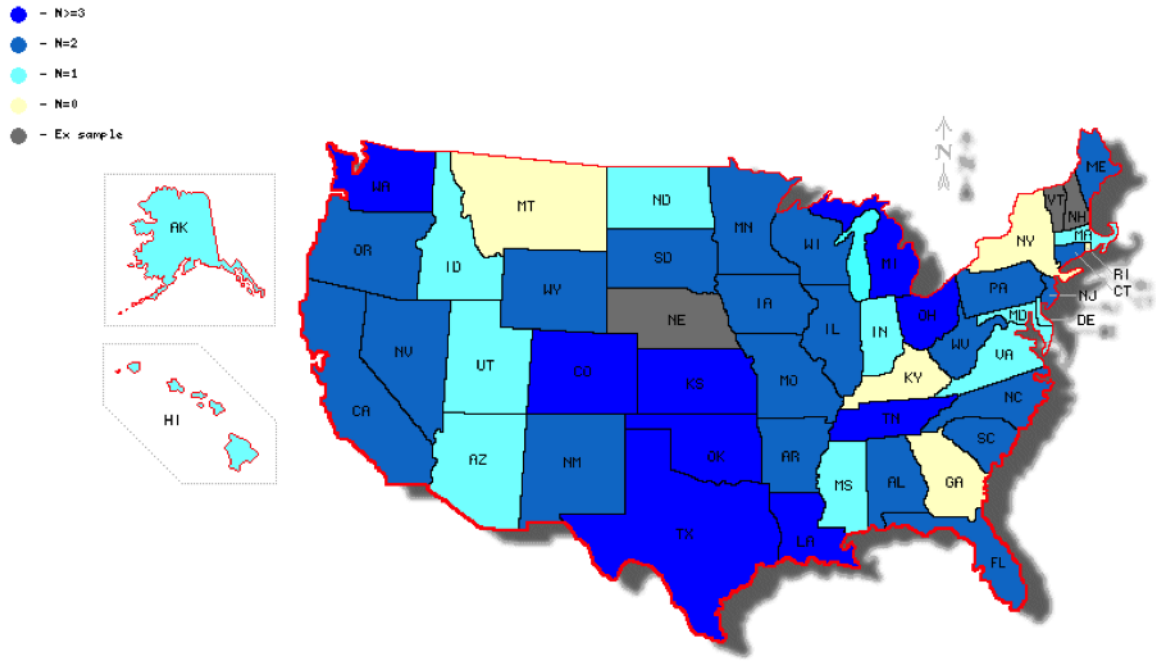
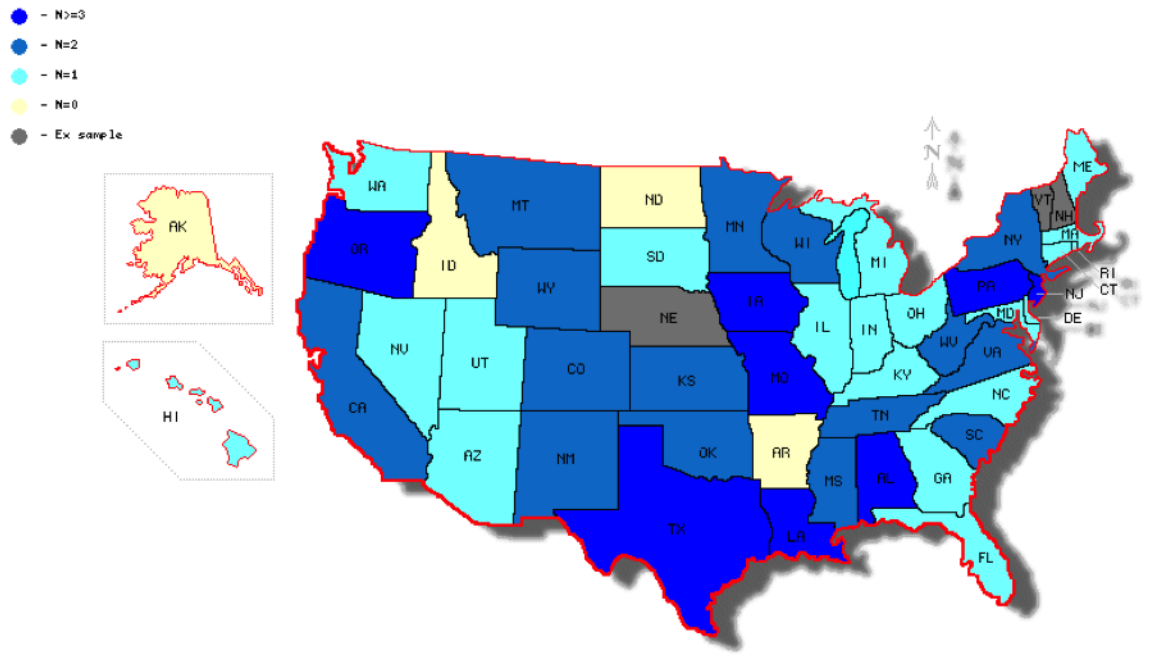


Figure 1.5: Number of Switches from unified to divided.



Figures 1.4 and 1.5 represent the number of switches from divided to unified, and from unified to divided, by state, that occurred as a result of a gubernatorial election, over the period 1978 to 2010.

Figure 1.6: Matching election years with fiscal years

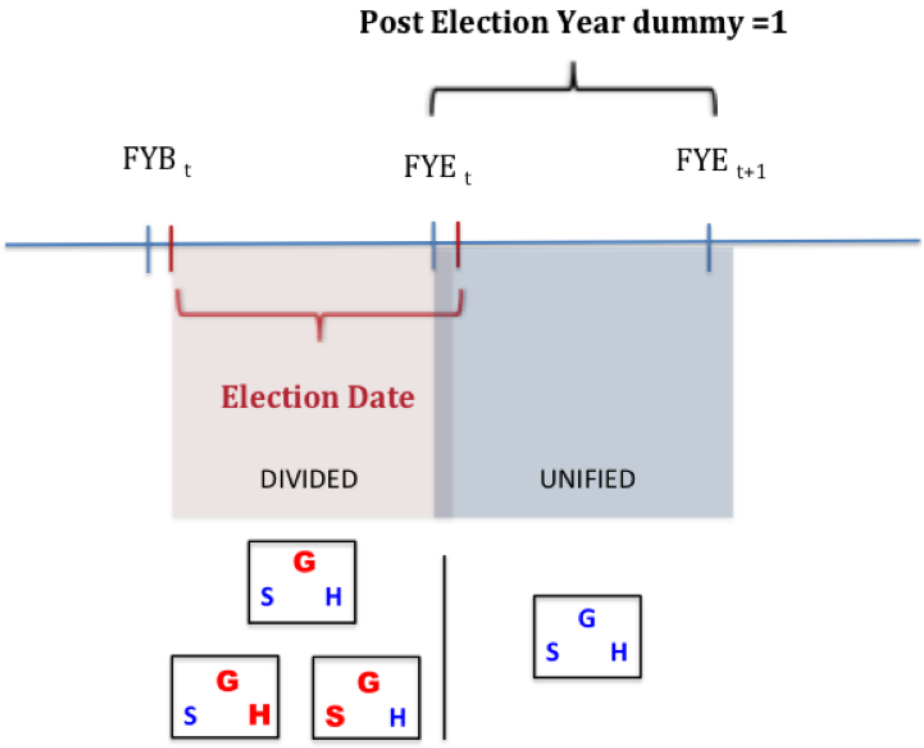


Figure 1.6 demonstrates the construction of the post-election year dummy for each firm given the firm's fiscal year beginning and end. The post-election dummy variable takes a value of one for any firm-year in which an election is held no later than 2 months after the fiscal year beginning of year t and no earlier than 10 months before the fiscal year beginning of year t. The post-election dummy variable requires that approximately 80% or more of a firm's fiscal-year days fall after the election date. All fiscal years for which the election date does not fall within this range have the election dummy set to a value of zero.

Figure 1.7: Time dynamic of investment

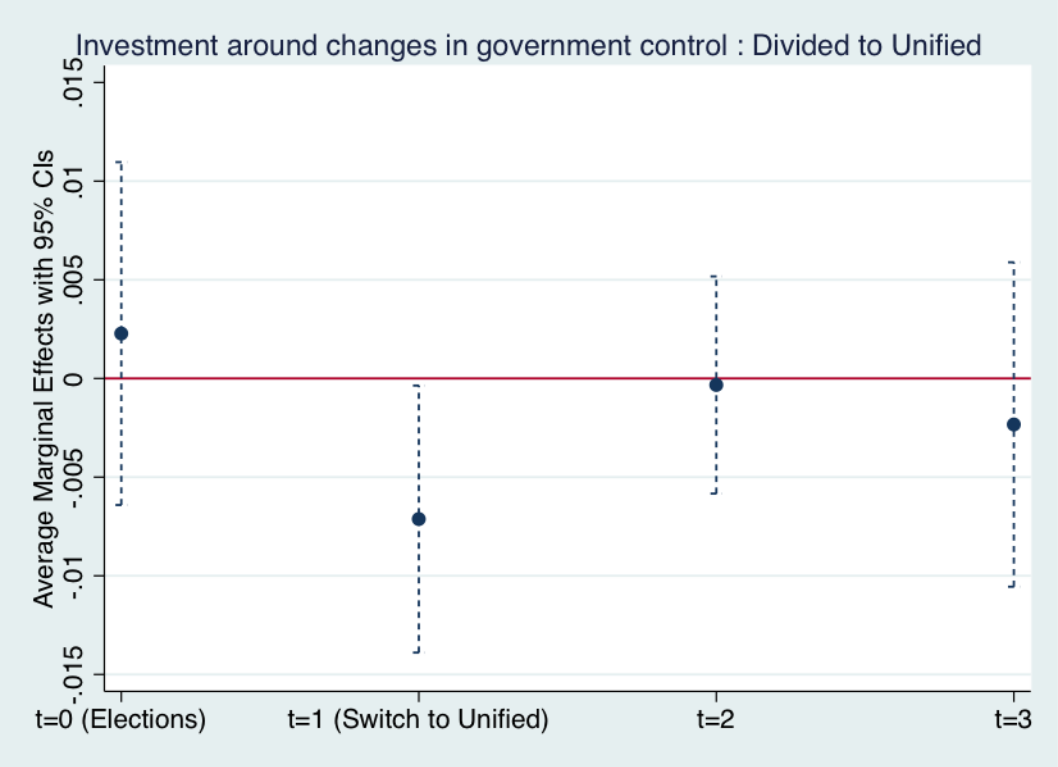
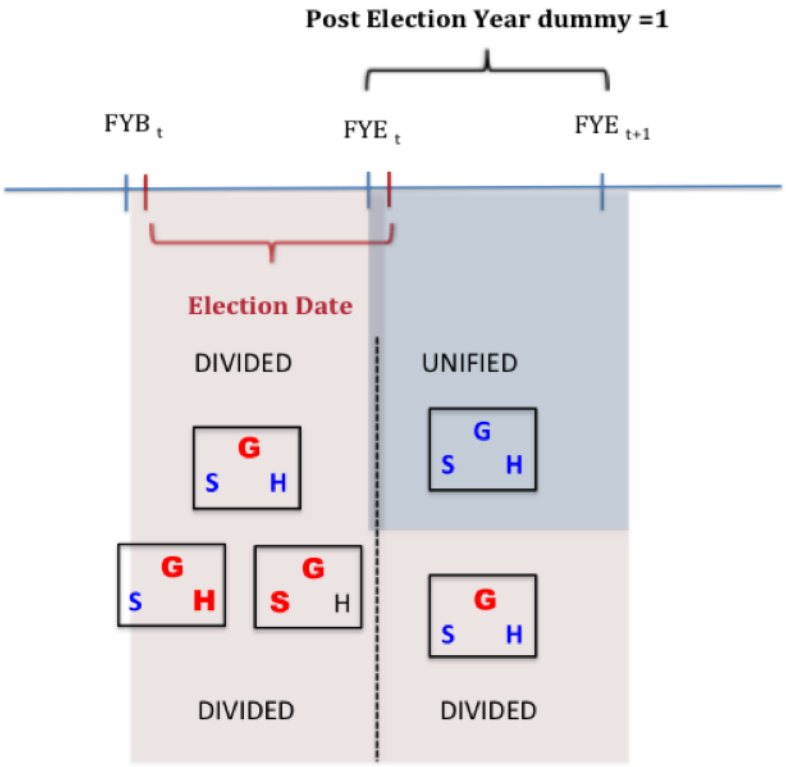


Figure 1.7 illustrates the effect for the switch from divided to unified.

Figure 1.8: Regression Discontinuity Design: sample selection



For the RD, the sample is restricted to divided states in the pre-election term, and to states that end up to be either unified after the election, or to be divided with the governor being confronted to a majority from the opposite party in both chambers of the Legislature. Split legislatures are dropped from the divided cases considered in the post election term. This is to ensure that only the governor election results can potentially change treatment assignment from divided government to unified government or vice versa.

Distribution of the assignment variable

Figure 1.9: Histogram with 2 percent bins

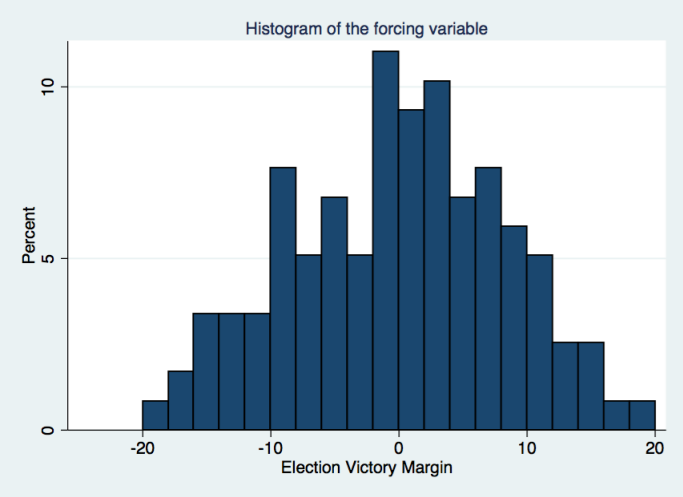


Figure 1.10: Histogram with 4 percent bins

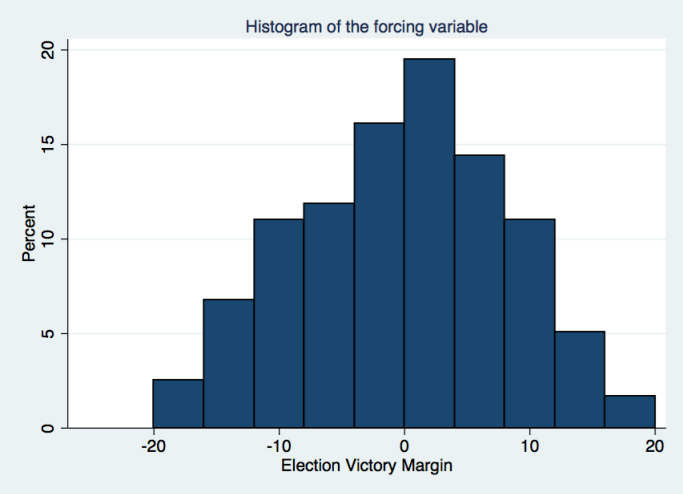


Figure 1.11: Regression Discontinuity: Switch to Unified - Binned averages of changes in investment rate as a function of the electoral margin.

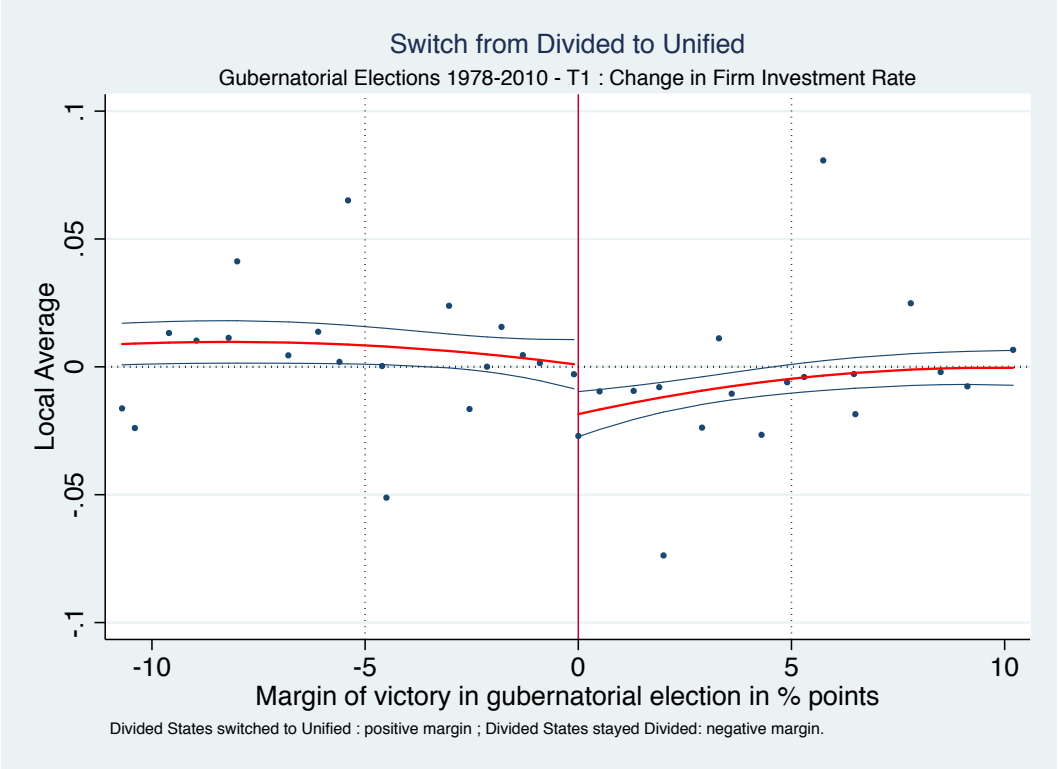


Figure 1.11 plots the average annual within-firm change in investment rate net of the contemporaneous change in the firm's Census region (to remove the influence of time-varying changes in regional economic conditions or nation-wide variation in business conditions that affect all states simultaneously). Each dot corresponds to the unconditional mean of changes in firms' investment rate between t and t+1 within intervals of 0.01 margin of victory in gubernatorial election t. The solid line represents the predicted values of a second-order polynomial fit, with the blue lines showing the 95% confidence intervals.

Figure 1.12: Regression Disconinuity: Switch to Divided - Binned averages of changes in investment rate as a function of the electoral margin.

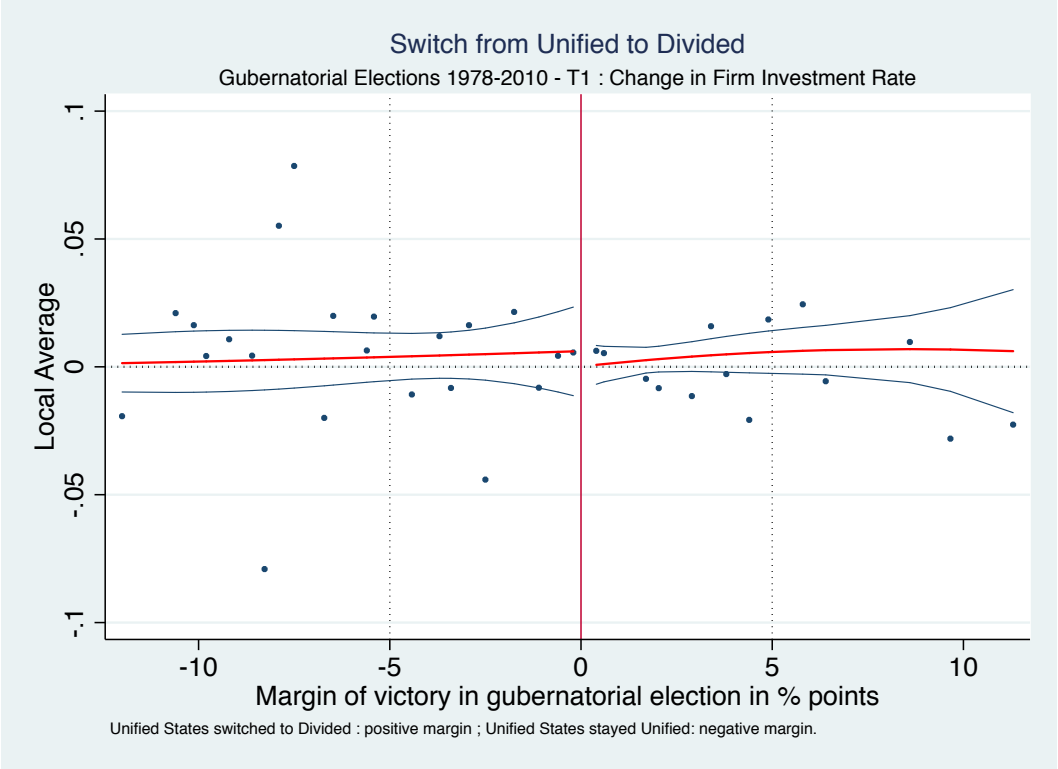


Figure 1.12 plots the average annual within-firm change in investment rate net of the contemporaneous change in the firm's Census region (to remove the influence of time-varying changes in regional economic conditions or nation-wide variation in business conditions that affect all states simultaneously). Each dot corresponds to the unconditional mean of changes in firms' investment rate between t and t+1 within intervals of 0.01 margin of victory in gubernatorial election t. The solid line represents the predicted values of a second-order polynomial fit, with the blue lines showing the 95% confidence intervals.

Figure 1.13: Regression Discontinuity: Switch to Unified - Placebo test in T0.

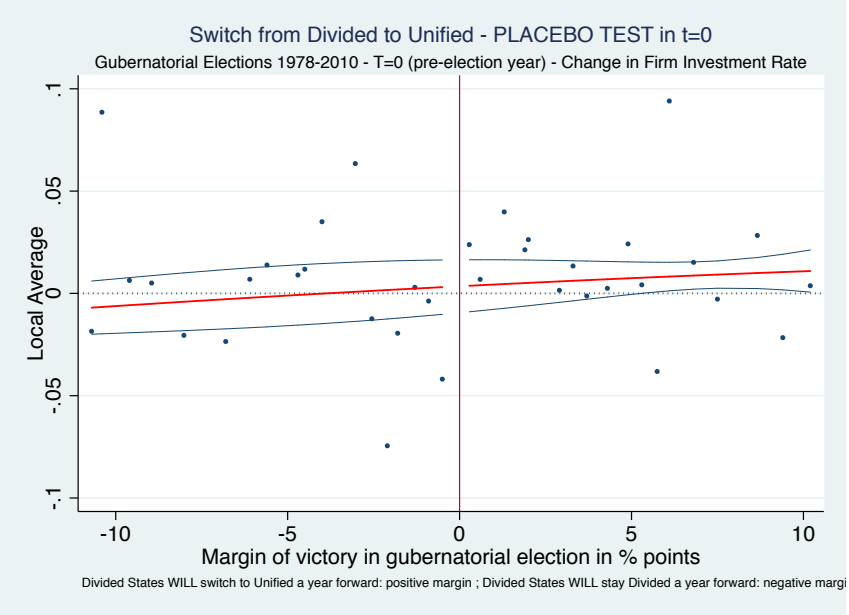


Figure 1.14: Regression Discontinuity: Switch to Unified - Placebo test using median voteshare as the threshold.

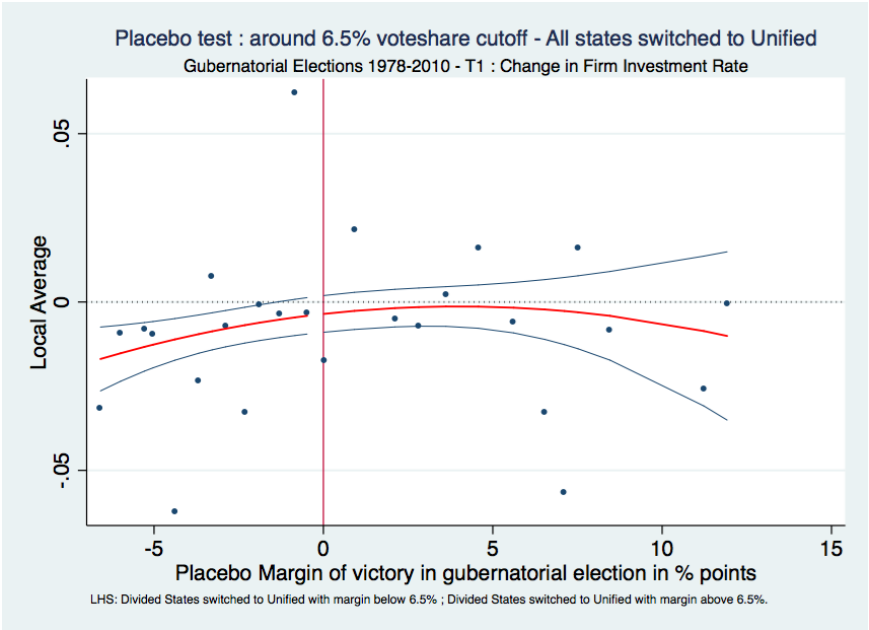
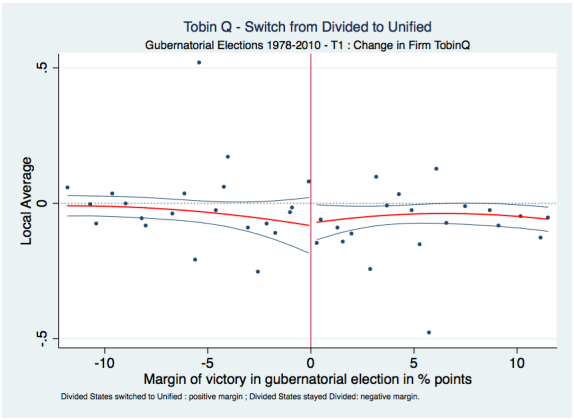
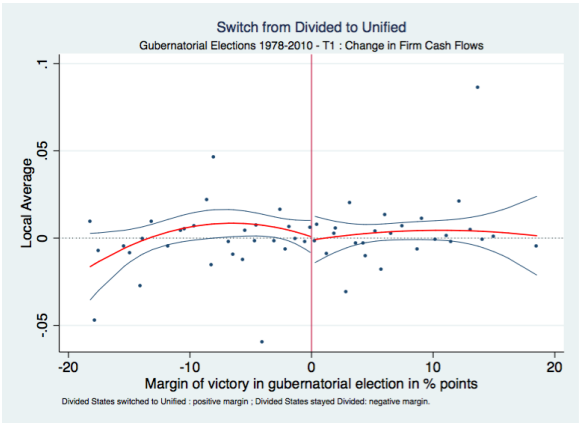


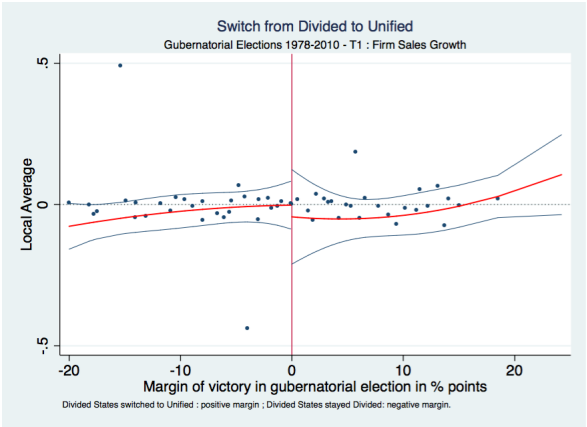
Figure 1.15: Switch to Unified - Effects on other covariates



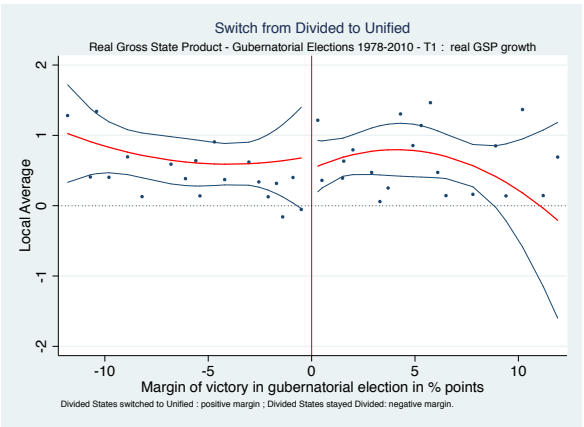
(a) Tobin's Q



(b) Cash Flows



(c) Sales



(d) State GDP

Table 1.1: Election Summary Statistics

Panel A : Election Characteristics

Panel A1 reports various summary statistics for gubernatorial elections held between 1978 and 2010 in 46 states with bicameral legislature.

	Number of elections	Percentage
Gubernatorial elections	408	100
Divided government elected	212	51.9
Switch from Unified to Divided	64	15.8
Switch from Divided to Unified	82	20.1
Switch from Unified to Unified	114	27.9
Switch from Divided to Divided	148	36.3
Non incumbent governor elected	231	56.6
Non incumbent elected * Switch from Unified to Divided	61	14.9
Non incumbent elected * Switch from Divided to Unified	68	16.7
Republican governor elected	199	48.8
Republican gov. elected * Divided government	118	28.9
Switch from Democrat to Republican gov.	77	18.9
Switch from Democrat to Republican gov. * Switch from Unified to Divided	42	10.3
Switch from Democrat to Republican gov. * Switch from Divided to Unified	24	5.9
Switch from Republican to Democrat gov.	60	14.7
Switch from Republican to Democrat gov. * Switch from Unified to Divided	12	2.9
Switch from Republican to Democrat gov. * Switch from Divided to Unified	37	9.1

Panel B1 : Divided to Unified Analysis

Panel B report summary statistics for the margin of victory of gubernatorial elections used in the Regression Discontinuity section. Elections in Panel B1 (B2) led to a transition from a divided (unified) to a unified government (positive margin of victory) or a transition from a divided (unified) to a simple divided government (negative margin of victory). A 5% margin range implies that the vote share of the gubernatorial candidate whose party is aligned with the party controlling the house and the senate is between 45% and 55%.

	N	Mean	p25	Median	p75	Std.
All elections	118	-0.02	-6.1	0.343	6.1	8.372
Republican gov. elected	55	-1.743	-8.016	-1.7	3.6	8.157
Margin within 5%	53	0.462	-1.7	0.386	3.204	2.876

Panel B2 : Unified to Divided Analysis

	N	Mean	p25	Median	p75	Std.
All elections	99	-3.347	-9.2	-3.24	2.59	7.367
Republican gov. elected	52	-0.884	-5.824	0.35	3.622	6.849
Margin within 5%	47	0.326	-2.7	0.4	3	3.009

Table 1.2: Firm Summary Statistics

Panel B : Financial Characteristics

Panel B reports summary statistics for the firm characteristics used in the analysis. Total Assets is book value of assets. Investment is measured as firm-level yearly capital expenditures normalized by beginning-of-fiscal-year firm net property, plant, and equipment (ppent). Asset tangibility is the ratio of net PP&E over end-of-fiscal-year total assets. Tobin's Q is the ratio of the market value of assets to the book value of assets. Cash flow is defined as earnings before interest and taxes minus taxes and interest expense plus depreciation and amortization scaled by beginning-of-fiscal-year total assets. Sales growth is measured as the log difference in yearly sales (sale). Cash holding is measured as cash and short term investments (che) divided by beginning- of-fiscal-year total asset. Return on asset (ROA) is calculated as yearly net income (ib) divided by one fiscal year lagged total asset. Return on equity (ROE) is calculated as yearly net income divided by one fiscal year lagged book equity. Leverage is long term debt + short term debt (dltt + dlc) scaled by beginning-of-fiscal-year book value of assets. R&D is measured as firms' research and development expenditures as reported in Compustat (xrd).

	Mean	Median	Std.	N.firm-year
Total Assets	1,586	119	11,429	91,202
CapEx/PPE	0.27	0.22	0.20	91,202
Asset Tangibility	0.33	0.27	0.25	91,202
Cash holdings/Total Assets	0.16	0.08	0.21	91,199
Leverage	0.24	0.21	0.21	90.951
Sales growth	0.07	0.07	0.22	80,800
ROE	0.02	0.09	0.31	91,202
ROA	0.008	0.04	0.16	91,202
Tobin's Q	1.685	1.28	1.23	91,202
R&D/Total Assets	0.07	0.03	0.10	54,229

Panel C : State-level statistics on gross and net employment flows

Panel C reports summary statistics for state-level gross and net job flows. The unit of observation is the establishment defined as the fixed physical location where economic activity occurs. Size and age categories are based on the age of the parent firm. For each state, job creation (destruction) rate is the ratio of the sum of all employment gains (losses) from establishments located in that state, from year t-1 to year t, including establishment start-ups (deaths), divided by average of state employment for times t and t-1. All growth rates are based on March to March changes.

	Mean	Median	Std.	N. firm-year
Job creation rate in %	17.6	15.8	7.7	17,400
----for firms < 250 emp.	21.4	19.1	8.2	
----for firms >=250 emp.	13.7	13.1	4.5	
----for start ups	20.4	19.2	6.02	
----for firms > 5 year old	13.2	13.0	2.84	
Job destruction rate in %	15.3	15.2	4.6	17,400
----for firms < 250 emp.	17.4	17.3	3.4	
----for firms >=250 emp.	13.2	12.7	4.7	
----for start ups	25.2	24.4	6.2	
----for firms > 5 year old	14.6	14.1	3.5	
Net job creation rate in %	2.3	1.5	6.9	17,400
N. Firms (1,000)	100	69	101	
N. Establishments (1,000)	123	84	125	

Table 1.3: Checks & Balances and Corporate Investment

This table presents estimates from investment regressions of the type :

$$I_{i,s,t} = \alpha_i + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta 2 PostElection_{s,t} * DU_{s,t} + \beta_3 PostElection_{s,t} * UD_{s,t}$$
$$+\delta_1 TobinQ_{i,s,t-1} + \delta_2 CF_{i,s,t} + \delta_3 Sale_{i,s,t-1} + \delta_4 Size_{i,s,t-1} + \gamma \Delta GSP_{s,t-1} + c_0 Republican_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$$

i indexes firm, s indexes state, R indexes Census region and t denotes time. $Region_{Rt}$ is Census region x year fixed effects. The dependent variable is the investment rate defined as Capex scaled by lagged PPE. Tobin's Q is the ratio of the market value of assets to the book value of assets. CF is cash flow scaled by beginning-of-year total assets. Size is the logarithm of total assets. Sales growth is growth in sales over the previous year. %change in GSP is the percentage change in real gross state product for a given state over the previous year. The post-election dummy (T1) takes a value of one for any firm-year in which an election is held no later than 2 month after the fiscal year beginning of year t and no earlier than 10 months before the fiscal year beginning of year t. All fiscal years for which the election date does not fall within this range have the T1 dummy set to a value of zero. The Switch to unified (Divided) dummy is set to 1 when government is divided (unified) the year the gubernatorial election takes place and becomes unified (divided) the year after, as a result of the election. Republican is an indicator variable that takes a value of one when the elected governor is a Republican. In the last specification, I exclude the nine states in which governor elections are concurrent with presidential elections. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) No Firm FE	(2) Firm FE	(3) Region x Time FE	(4) Switch to Div.	(5) Election	(6) Firm Controls	(7) State Controls	(8) Time clustering	(9) Lagged Inv.	(10) Ex Presidential elections
Dependent var.: CAPXt/PPEt-1										
Republican Governor							0.001	0.001	0.001	-0.001
							(0.002)	(0.002)	(0.002)	(0.002)
Election Year					-0.001	0.001	0.001	0.001	0.001	0.002
					(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Post Election Year T1	0.003	0.001	0.001	0.001	0.001	0.003	0.003	0.003	0.003	0.003*
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
T1 * Switch to Unified	-0.010***	-0.006	-0.006*	-0.007*	-0.007*	-0.008***	-0.007**	-0.007*	-0.008**	-0.010***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
T1 * Switch to Divided				-0.004	-0.004	-0.003	-0.003	-0.003	-0.002	-0.000
				(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
$TobinQ_{t-1}$						0.047***	0.047***	0.047***	0.041***	0.048***
						(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
CF_t						0.037***	0.037***	0.037***	0.036***	0.036***
						(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
Sales $growth_{t-1}$						0.017***	0.017***	0.017***	0.010***	0.017***
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Size $_{t-1}$						-0.045***	-0.046***	-0.046***	-0.052***	-0.043***
						(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
%Δreal GSP $_{t-1}$							0.009***	0.009***	0.008***	0.010***
							(0.002)	(0.002)	(0.002)	(0.002)
$Investment_{t-1}$									0.200***	
									(0.009)	
N	90193	90027	90027	90027	90027	69295	69295	69295	68016	56747
R-squared	0.019	0.373	0.375	0.375	0.375	0.444	0.444	0.444	0.469	0.447
Time FE	YES	YES	no	no	no	no	no	no	no	no
Ind. FE	no	no	no	no	no	no	no	no	no	no
Firm FE	no	YES	YES	YES	YES	YES	YES	YES	YES	YES
Region x Time FE	no	no	YES	YES	YES	YES	YES	YES	YES	YES
SE state clusters	46	46	46	46	46	46	46	46	46	30
SE time clusters								33	33	

Table 1.4: Corporate Investment and Changes in State Partisan Control

This table presents estimates from investment regressions of the type :

$$I_{i,s,t} = \alpha_i + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta 2 PostElection_{s,t} * DU_{s,t} + \beta_4 PostElection_{s,t} * SWITCH_{s,t} + \delta FirmControls_{i,t} + \gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$$

i indexes firm, s indexes state, R indexes Census region and t denotes time. $Region_{Rt}$ is Census region x year fixed effects. The dependent variable is the investment rate defined as Capex scaled by lagged PPE. The Switch dummy takes a value of 1 when the election results in a change in the split vs. full partisan control i.e. Switch=1 in states where government switches from unified to divided AND in states where government switches from divided to unified. Switch to unified (divided) dummy is set to 1 when government is divided (unified) the year the gubernatorial election takes place and becomes unified (divided) the year after, as a result of the election. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Checks and Balances and Corporate Investment
Changes in partisan control:"Switch"
1978 - 2010

	(1)	(2)	(3)
Dependent variable: **CAPXt/PPEt-1**			
Post Election Year T1	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)
T1 * 1 {Switch}	-0.005**	-0.008***	-0.002
	(0.002)	(0.003)	(0.003)
T1 * Switch to Unified			-0.007*
			(0.004)
T1 * Switch to Divided		0.007*	
		(0.004)	
N	68016	68016	68016
R-squared	0.469	0.469	0.469
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes
SE state clusters	46	46	46

Table 1.5: The time dynamic of investment

This table presents estimates from investment regressions of the type :

$$I_{i,s,t} = \alpha_i + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta_2 Tk_{s,t} * DU_{s,t} + \beta_3 Tk_{s,t} * UD_{s,t}$$
$$+\delta FirmControls_{i,t} + \gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$$

i indexes firm, s indexes state, R indexes Census region and t denotes time. $Region_{Rt}$ is Census region x year fixed effects. The dependent variable is the investment rate defined as Capex scaled by lagged PPE. Years T(-1), T0 and T2 are defined relative to the post-election year (T1) as, respectively, T1 - 1 year, T1 + 1 year and T1 + 2 years. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **t=0 (Election year)**	(2) **t=1 (Treatment year)**	(3) **t=2**	(4) **t=3**	(5) **All**
Dependent variable: **CAPXt/PPEt-1**					
T=0 * Switch to Unified	0.004				0.002
	(0.004)				(0.004)
T=+1 * Switch to Unified		-0.007**			-0.007**
		(0.003)			(0.003)
T=+2 * Switch to Unified			0.001		-0.000
			(0.002)		(0.003)
T=+3 * Switch to Unified				-0.001	-0.002
				(0.004)	(0.004)
T=0 * Switch to Divided	-0.001				-0.001
	(0.005)				(0.006)
T=+1 * Switch to Divided		-0.002			-0.004
		(0.003)			(0.003)
T=+2 * Switch to Divided			0.002		0.003
			(0.003)		(0.004)
T=+3 * Switch to Divided				0.006	0.006*
				(0.004)	(0.003)
N	69803	69295	67163	65130	65130
R-squared	0.443	0.444	0.446	0.448	0.448
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp
Firm FE	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	46	46

Table 1.6: Checks and Balances and Corporate Investment, analysis across firm sizes

Table 1.6 presents estimates for the baseline model specification for sub-samples based on the size of the firms as measured by total assets. In column 1 the sample is restricted to the smallest firms, whose total assets is in the first quintile of the population distribution over the sample period. Columns 2, 3, 4 and 5 use samples made , respectively, of the second, third, fourth and fifth quintiles. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
T1 * Switch from Divided to Unified	-0.020**	-0.008	-0.002	-0.008	-0.005
	(0.008)	(0.008)	(0.007)	(0.005)	(0.004)
T1 * Switch from Unified to Divided	-0.016	-0.007	-0.001	-0.000	-0.001
	(0.011)	(0.007)	(0.007)	(0.006)	(0.004)
T0 = Election Year	-0.007**	-0.001	-0.002	0.004*	0.006***
	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)
T1 = Post Election Year	0.009*	0.009	-0.004	0.002	-0.000
	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
N	11617	13411	14121	14937	16121
R-squared	0.387	0.408	0.437	0.501	0.549
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes
SE state clusters	45	43	43	44	40

Table 1.7: Checks & Balances and Job Creation (by state*firm age)

This table presents estimates from investment regressions of the type :

$$JCR_{k,s,t} = a_k + \alpha_s + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta_2 PostElection_{s,t} \times DU_{s,t} + \beta_3 PostElection_{s,t} \times UD_{s,t}$$
$$+\gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$$

s indexes the state of the establishment, k indexes the age category of the parent firm, R indexes Census region and t denotes time. Region_R,t is Census region-year fixed effects. The main dependent variable is the JCR_{kst}, the job creation rate in state s between year t and year t-1, in percentages. For each state, JCR_{kst} is the ratio of the sum of all employment gains from expanding establishments from year t-1 to year t divided by average of state employment for times t and t-1. %change real GSP/capita is the percentage change in real gross state product per capita for a given state from year t-2 to and t-1.The election year dummy (T0) takes a value of one for any state-year following the year in which a gubernatorial election is held and a value of zero otherwise. As employment rates are reported as of March 12th of each year in the BDS, a gubernatorial election occurring on November of year t is matched with changes in employment from March t to March t+1. The post-election dummy (T1) is defined relative to the election year as T0 + 1 year. The Switch to unified (Divided) dummy is set to 1 when government is divided (unified) the year the gubernatorial election takes place and becomes unified (divided) the year after, as a result of the election. Republican governor is an indicator variable that takes a value of one when the governor is a Republican and zero otherwise. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **Job Creation Rate**	(2) RegionxYear FE	(3) Switch to Divided	(4) Election year	(5) State controls	(6) **Job Destruction Rate**	(7) **Net Job Creation Rate**	(8) **Estab. Exit Rate**	(9) **Estab. Entr Rate**
Republican Governor					0.019	-0.004	0.023	-0.024	0.019
					(0.068)	(0.107)	(0.126)	(0.084)	(0.026)
T0 = Election Year				-0.032	-0.013	-0.102	0.089	0.001	0.016
				(0.102)	(0.100)	(0.122)	(0.180)	(0.065)	(0.018)
T1 = Post Election Year	0.189*	0.204**	0.137	0.132	0.166	-0.027	0.193	-0.010	0.013
	(0.101)	(0.100)	(0.103)	(0.111)	(0.111)	(0.116)	(0.178)	(0.056)	(0.039)
T1 * Switch from Divided to Unified	-0.389**	-0.321*	-0.260*	-0.261*	-0.274*	-0.114	-0.161	-0.062	-0.025
	(0.155)	(0.163)	(0.153)	(0.153)	(0.137)	(0.171)	(0.247)	(0.083)	(0.040)
T1 * Switch from Unified to Divided			0.317	0.314	0.320	0.130	0.189	0.058	0.196
			(0.249)	(0.249)	(0.242)	(0.238)	(0.361)	(0.124)	(0.198)
%change realGSP/capita $_{t-2}$					0.321***	-0.307***	0.628***	-0.231***	0.001
					(0.048)	(0.054)	(0.074)	(0.075)	(0.024)
N	13045	13045	13045	13045	13045	13045	13045	13045	13045
Adjusted R-squared	0.714	0.722	0.722	0.722	0.724	0.771	0.316	0.923	0.677
Firm Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No	No	No	No
Region x Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of State clusters	46	46	46	46	46	46	46	46	46

Table 1.8: Checks & Balances and Job Creation (by state*firm size)

This table presents estimates from investment regressions of the type :

$$JCR_{k,s,t} = a_k + \alpha_s + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta_2 PostElection_{s,t} \times DU_{s,t} + \beta_3 PostElection_{s,t} \times UD_{s,t}$$
$$+\gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$$

s indexes the state of the establishment, k indexes the size category of the parent firm, R indexes Census region and t denotes time. Region_R,t is Census region-year fixed effects. The main dependent variable is JCR_{kst}, the job creation rate in state s between year t and year t-1, in percentages. For each state, JCR_{kst} is the ratio of the sum of all employment gains from expanding establishments from year t-1 to year t divided by average of state employment for times t and t-1. %change real GSP/capita is the percentage change in real gross state product per capita for a given state from year t-2 to and t-1.The election year dummy (T0) takes a value of one for any state-year following the year in which a gubernatorial election is held and a value of zero otherwise. As employment rates are reported as of March 12th of each year in the BDS, a gubernatorial election occurring on November of year t is matched with changes in employment from March t to March t+1. The post-election dummy (T1) is defined relative to the election year as T0 + 1 year. The Switch to unified (Divided) dummy is set to 1 when government is divided (unified) the year the gubernatorial election takes place and becomes unified (divided) the year after, as a result of the election. Republican governor is an indicator variable that takes a value of one when the governor is a Republican and zero otherwise. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **Job Creation Rate**	(2) RegionxYear FE	(3) Switch to Divided	(4) Election year	(5) State controls	(6) **Job Destruction Rate**	(7) **Net Job Creation Rate**	(8) **Estab. Exit Rate**	(9) **Estab. Entr Rate**
Republican Governor					-0.079	0.042	-0.120	-0.049	-0.082
					(0.071)	(0.086)	(0.097)	(0.052)	(0.052)
T0 = Election Year				-0.097	-0.073	-0.058	-0.015	-0.031	-0.016
				(0.083)	(0.084)	(0.103)	(0.156)	(0.039)	(0.049)
T1 = Post Election Year	0.061	0.023	-0.039	-0.056	-0.001	-0.009	0.008	0.007	-0.052
	(0.077)	(0.074)	(0.080)	(0.085)	(0.078)	(0.088)	(0.133)	(0.053)	(0.050)
T1 * Switch from Divided to Unified	-0.352**	-0.256	-0.199	-0.201	-0.247*	-0.094	-0.153	-0.145*	-0.031
	(0.132)	(0.161)	(0.148)	(0.148)	(0.129)	(0.149)	(0.191)	(0.076)	(0.072)
T1 * Switch from Unified to Divided			0.291	0.285	0.312	-0.030	0.343	-0.036	0.179
			(0.212)	(0.211)	(0.195)	(0.137)	(0.260)	(0.068)	(0.148)
%change realGSP/capita $_{t-2}$					0.466***	-0.262***	0.728***	-0.187***	0.131**
					(0.041)	(0.069)	(0.067)	(0.025)	(0.059)
N	17928	17928	17928	17928	17928	17928	17928	17928	17928
Adjusted R-squared	0.808	0.813	0.813	0.813	0.815	0.533	0.545	0.711	0.660
Firm Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No	No	No	No
Region x Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of State clusters	46	46	46	46	46	46	46	46	46

Table 1.9: Checks & Balances and Job Creation - Young vs. Old firms

Table 1.9 presents estimates for the same specification as in table 12 for sub-samples of the establishment population for which the parent firm is 5 year-old or less ("Start-ups") and for which the parent firm is older than 5 year old ("Others"). The left hand side variables are the job creation rate and the job destruction rate in state s between year t and year t-1, in percentages. For each state, JCR_{kst} is the ratio of the sum of all employment gains from expanding establishments from year t-1 to year t - including establishment startups - divided by average of state employment for times t and t-1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Job Creation Rate				**Job Destruction Rate**			
	N<20	20<=N<250	250<=N<2500	N>=2500	N<20	20<=N<250	250<=N<2500	N>=2500
T1 = Post Election Year	0.289	0.027	-0.240	-0.081	0.302	0.039	-0.194	-0.117
	(0.174)	(0.090)	(0.160)	(0.155)	(0.212)	(0.152)	(0.253)	(0.257)
T1 * Switch from Divided to Unified	-0.449**	-0.304*	-0.200	-0.035	-0.445*	-0.102	-0.154	0.091
	(0.215)	(0.164)	(0.231)	(0.233)	(0.258)	(0.256)	(0.382)	(0.337)
T1 * Switch from Unified to Divided	-0.024	0.139	0.550*	0.584	0.004	0.272	0.287	0.808
	(0.316)	(0.294)	(0.283)	(0.383)	(0.424)	(0.406)	(0.391)	(0.575)
N	4482	4482	4482	4482	4482	4482	4482	4482
Adjusted R-squared	0.929	0.738	0.404	0.253	0.860	0.527	0.220	0.159
Fixed effects	State	State	State	State	State	State	State	State
-	Region x Year	Region x Year	Region x Year	Region x Year	Region x Year	Region x Year	Region x Year	Region x Year
-	Size	Size	Size	Size	Size	Size	Size	Size
State controls	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)	Gov.party g(real gsp per capita)
Number of State clusters	46	46	46	46	46	46	46	46

Table 1.10: Regression Discontinuity Design - Switch from divided to unified

This table presents estimates from investment regressions of the type :

$I_{i,s,t} = \alpha_i + f(V_{s,t-1}) + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta 2 PostElection_{s,t} \times DU_{s,t}$
$+\delta FirmControls_{i,t} + \gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$
with
$f(V_{s,t-1}) = b_1 V + c_1 V^2 + d_1 V^3 + b_2 V \times DU + c_2 V^2 \times DU + d_2 V^3 \times DU$

In this regression discontinuity specification, the sample is limited to gubernatorial terms of divided states that either stayed divided or switched to unified. I drop non-contested elections (those in which one party won more than 70% of the votes). Specification 1 is the baseline specification estimated on the sub-sample used for the RD. Specifications 2 and 3 add the share of the votes received by the gubernatorial candidate whose party is aligned with the party controlling the house and the senate. Specification 2 includes a quadratic functional form for the assignment variable (election margin) and specification 3 a cubic form. Both polynomial forms have interaction terms with the treatment variable (Switch to unified) so that a two-degree polynomial or three-degree polynomial is estimated on each side of the cutoff. Column 4 and 5 consider close gubernatorial elections, within a 3% and 2% margin. A 3% margin implies that the vote share of the gubernatorial candidate whose party is aligned with the party controlling the house and the senate is between 47% and 53%.All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	**RDD** 2nd-order polynomial	**RDD** 3rd-order polynomial	**RDD** 3rd-order polynomial 15% margin	No controls	No region x time FE	**RDD** 3% Margin	**RDD** 2% Margin
T=1: Post Election Year	0.006**	0.006*	0.006*	0.005	0.009***	0.007	-0.005	-0.003
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)
T1 * Switch from Divided to Unified	-0.012***	-0.017**	-0.020**	-0.018*	-0.016*	-0.016	-0.011*	-0.013
	(0.004)	(0.007)	(0.008)	(0.009)	(0.009)	(0.015)	(0.006)	(0.008)
N	26110	26110	26110	23202	30388	30388	6796	5686
Adjusted R-squared	0.503	0.503	0.503	0.508	0.462	0.435	0.620	0.639
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
Region x Time FE	yes	yes	yes	yes	yes	no	yes	yes
Firm controls	yes	yes	yes	yes	None	no	yes	yes
State controls	yes	yes	yes	yes	None	no	yes	yes
Number of State clusters	41	41	41	40	40	40	28	24
Number of Elections	144	144	144	126	126	126	35	29
Number of Divided to Unified	79	79	79	70	70	70	16	14

Table 1.11: Regression Discontinuity Design - Switch from unified to divided

This table presents estimates from investment regressions with the same specification as in table 15, except that the partisan control switch of interest is from unified to divided:

$I_{i,s,t} = \alpha_i + f(V_{s,t-1}) + \beta_0 Election_{s,t} + \beta_1 PostElection_{s,t} + \beta 2 PostElection_{s,t} \times UD_{s,t}$
$+\delta FirmControls_{i,t} + \gamma StateControls_{s,t} + Region_{R,t} + \epsilon_{i,s,t}$
with
$f(V_{s,t-1}) = b_1 V + c_1 V^2 + d_1 V^3 + b_2 V \times UD + c_2 V^2 \times UD + d_2 V^3 \times UD$

In this regression discontinuity specification, the sample is limited to gubernatorial terms of divided states that either stayed divided or switched to unified. I drop non-contested elections (those in which one party won more than 70% of the votes). Specification 1 is the baseline specification estimated on the sub-sample used for the RD. Specifications 2 and 3 add the share of the votes received by the gubernatorial candidate whose party is aligned with the party controlling the house and the senate. Specification 2 includes a quadratic functional form for the assignment variable (election margin) and specification 3 a cubic form. Both polynomial forms have interaction terms with the treatment variable (Switch to divided) so that a two-degree polynomial or three-degree polynomial is estimated on each side of the cutoff. Column 4 and 5 consider close gubernatorial elections, within a 3% and 2% margin. A 3% margin implies that the vote share of the gubernatorial candidate whose party is aligned with the party controlling the house and the senate is between 47% and 53%. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Baseline	**RDD** 2nd-order polynomial	**RDD** 3rd-order polynomial	**RDD** 3rd-order pol. Election margin<15%	**RDD** No polynomial Election Margin 5%
T=1: Post Election Year	0.002	0.003	0.003	0.005	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
T1 * Switch from Unified to Divided	-0.002	-0.003	-0.003	-0.006	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
N	20406	20406	20406	19093	10872
Adjusted R-squared	0.525	0.525	0.525	0.535	0.584
Firm FE	YES	YES	YES	YES	YES
Region x Time FE	YES	YES	YES	YES	YES
Number of State clusters	42	42	42	42	36
Number of Elections	150	150	150	127	66
Number of Divided to Unified	44	44	44	43	33

Table 1.12: Investment Irreversibility and the Policy Uncertainty Channel

Table 1.12 presents estimation results based on the baseline specification.The model is estimated separately for sub-samples of firms sorted over the sample period, based on their fixed assets ratio defined as PP&E over lagged total assets. In column 1 the sub-sample is made of firms with a PP&E ratio in the first tercile of the ratio distribution in the population. Columns 2 and 3 look at the second and third terciles. All other variables are defined as before. Only the coefficients of interest are reported. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
Dependent variable: **CAPXt/PPEt-1**			
T1 * Switch from Divided to Unified	-0.008	-0.007*	-0.012***
	(0.007)	(0.004)	(0.004)
T1 * Switch from Unified to Divided	0.001	-0.003	-0.003
	(0.008)	(0.004)	(0.004)
T1 = Post Election Year	0.001	0.002	0.005*
	(0.003)	(0.003)	(0.003)
N	19909	24305	23802
R-squared	0.426	0.449	0.512
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes
SE state clusters	44	43	46

Table 1.13: Investment Irreversibility and the Policy Uncertainty Channel for small firms

Table 1.13 presents estimation results based on the baseline specification.The model is estimated on a the subsample of small firms (total assets below sample median). Low tangibility firms have below median fixed assets ratio (defined as PP&E over lagged total assets). High tangibility firms have above median fixed assets ratio (defined as PP&E over lagged total assets). All other variables are defined as before. Only the coefficients of interest are reported. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) Low Tangibility	(2) High Tangibility
Dependent variable: **CAPXt/PPEt-1**		
T1 * Switch from Divided to Unified	-0.007	-0.016**
	(0.008)	(0.006)
T1 * Switch from Unified to Divided	-0.001	-0.009
	(0.008)	(0.006)
T0 = Election Year	-0.008*	0.003
	(0.005)	(0.003)
T1 = Post Election Year	0.001	0.010**
	(0.004)	(0.005)
N	14348	16496
R-squared	0.396	0.417
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes
Region x Time FE	Yes	Yes
SE state clusters	42	45

Table 1.14: Investment Redeployability and the Policy Uncertainty Channel

Table 1.14 presents estimation results based on the baseline specification. The sample is restricted to firms in the industries identified as "low redeployability" and "high redeployability" in Kim and Kung (2014) and the analysis compares the average effect and the effect on small firms (first quartile of total assets distribution). Columns 1 and 2 look at the 15 most redeployable industries, for all and small firms. Columns 3 and 4 look at the 15 less redeployable industries, for all and small firms. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
	15 most redeployable industries		**15 least redeployable industries**	
	All	Small firms	All	Small firms
Dependent variable: **CAPXt/PPEt-1**				
T1 * Switch from Divided to Unified	0.005	0.009	-0.007	-0.028**
	(0.008)	(0.029)	(0.007)	(0.013)
T1 * Switch from Unified to Divided	0.008	0.027	0.001	0.006
	(0.010)	(0.038)	(0.007)	(0.016)
T0 = Election Year	-0.003	-0.004	-0.003	-0.011*
	(0.005)	(0.013)	(0.003)	(0.006)
T1 = Post Election Year	0.003	0.015	0.001	0.000
	(0.004)	(0.015)	(0.004)	(0.008)
N	5267	1065	14154	3503
R-squared	0.415	0.407	0.458	0.385
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp
Firm FE	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes
SE state clusters	38	28	41	36

Table 1.15: R&D intensity and the Policy Uncertainty Channel

Table 1.15 presents estimation results based on the baseline specification.The model is estimated separately for sub-samples of firms sorted over the sample period, based on their fixed assets ratio defined as PP&E over lagged total assets. In column 1 the sub-sample is made of firms with a PP&E ratio in the first tercile of the ratio distribution in the population. Columns 2 and 3 look at the second and third terciles. All other variables are defined as before. Only the coefficients of interest are reported. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **All firms**	(2) **Small firms** Total assets <p50	(3) **Smallest firms** Total assets <p25
Dependent variable: **R&D/Total Assets t**			
T1 * Switch from Divided to Unified	0.001	0.003	0.006**
	(0.001)	(0.003)	(0.003)
T1 * Switch from Unified to Divided	-0.000	0.000	-0.002
	(0.001)	(0.002)	(0.004)
T1 = Post Election Year	0.000	0.001	0.002
	(0.001)	(0.001)	(0.001)
T0 = Election Year	0.002***	0.003***	0.005***
	(0.001)	(0.001)	(0.002)
N	40792	18257	8512
R-squared	0.851	0.846	0.837
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes
SE state clusters	46	45	44

Appendix : Robustness Checks

Table 1.16: Robustness - Additional State Macroeconomic Controls

Table 1.16 presents estimation results from various robustness tests based on the baseline specification. In column 1 the baseline specification is run on a shorter sub-period excluding the financial crisis. Column 2 to 5 add additional state-level macro variables i.e. the lagged change in employment rate, the change in state budget surplus, debt outstanding and state tax revenue (as a percentage of GSP). Only the parameters of interest are reported. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **Ex-Crisis 1978-2007**	(2) **ΔUnemp.**	(3) **Tax $_{s,t-1}$**	(4) **ΔTax**	(5) **Budget Deficit $_{s,t-1}$**	(6) **ΔBudget Deficit**	(7) **Debt/GSP $_{s,t-1}$**	(8) **ΔDebt/GSP**
Dependent variable: **CAPXt/PPEt-1**								
T1 * Switch from Divided to Unified	-0.0062*	-0.0069**	-0.0069**	-0.0070**	-0.0069**	-0.0069**	-0.0072**	-0.0069**
	(0.0031)	(0.0030)	(0.0030)	(0.0030)	(0.0030)	(0.0030)	(0.0030)	(0.0030)
T1 * Switch from Unified to Divided	-0.0043	-0.0025	-0.0026	-0.0027	-0.0019	-0.0026	-0.0027	-0.0026
	(0.0029)	(0.0029)	(0.0029)	(0.0029)	(0.0028)	(0.0029)	(0.0029)	(0.0029)
Change in Unemployment $_{t-1}$		-0.0005						
		(0.0009)						
Taxes as %GSP$_{t-1}$			0.0004					
			(0.0028)					
Change in Taxes as %GSP$_{t-1}$				0.0014				
				(0.0013)				
Budget Surplus as %GSP $_{t-1}$					0.0039**			
					(0.0015)			
Change in Budget Surplus as %GSP $_{t-1}$						0.0013		
						(0.0013)		
Total Debt Outstanding as %GSP$_{t-1}$							0.0064**	
							(0.0026)	
Change in Total Debt Outstanding as %GSP$_{t-1}$								0.0005
								(0.0009)
N	63410	69295	69295	69295	69295	69295	69295	69295
R-squared	0.4474	0.4441	0.4441	0.4441	0.4442	0.4441	0.4442	0.4441
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	46	46	46	46	46

Table 1.17: Robustness - Partisanship Controls

Table 1.17presents estimation results from various robustness tests based on the baseline specification. Column 1 and 2 report the estimates from the inclusion of a dummy, which is set equal to one when government is divided before the election and stays divided as a result of the election. Columns 3 and 4 report the estimates from the inclusion of a dummy which is set equal to one when government is unified before the election and stays unified as a result of the election. Column 5 and 6 look at the effect of a change in the party of the elected governor. Columns 7 to 9 add a "non-incumbent" dummy variable and its interactions with our treatment variables. The non incumbent dummy is set to 1 when the elected governor has not served in the past.. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Divided to Divided		**Unified to Unified**		**Republican to Democrat**	**Democrat to Republican**	**Non Incumbent governor**	-	-
Dependent variable: **CAPXt/PPEt-1**									
T1 * Switch from Divided to Divided	0.003	0.000							
	(0.002)	(0.003)							
T1 * Switch from Unified to Unified			0.003	0.001					
			(0.002)	(0.002)					
T1 * Switch from Rep to Dem					-0.001				
					(0.003)				
T1 * Switch from Dem to Rep						-0.000			
						(0.003)			
Non incumbent gov							-0.002	-0.002	-0.002
							(0.001)	(0.001)	(0.001)
T1 * Non incumbent gov								-0.001	0.000
								(0.003)	(0.003)
T1 * Switch from Divided to Unified		-0.008**		-0.008**					-0.008**
		(0.003)		(0.003)					(0.003)
N	69295	68016	68016	68016	69295	69295	68016	68016	68016
R-squared	0.444	0.469	0.469	0.469	0.444	0.444	0.469	0.469	0.469
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	46	46	46	46	46	46

Table 1.18: Robustness - Political Controls

Table 1.18presents estimation results from various robustness tests controlling for the party of the elected governor and excluding Southern states. Column 1 excludes the 16 states that make the Census South region : DE, FL, GE, MD, NC, SC, VA, WV, AL, KY, MS, TN, AR, LA, OK, TX. Columns 2 to 5 control for the partisanship of the elected governor by interacting the party of the governor with the post-election year indicator variable as well as with the indicators of a change to divided or to unified. The governor party dummy is set to one for Democrat governors and zero otherwise. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) Exclude South	(2)	(3)	(4)	(5)
Dependent variable: **CAPXt/PPEt-1**					
T1 * Dem.Gov		-0.003*	-0.002	-0.003	-0.003
		(0.002)	(0.002)	(0.002)	(0.002)
T1 * Dem.Gov * Switch from D to U				0.005	
				(0.006)	
T1 * Dem.Gov * Switch from U to D					-0.002
					(0.005)
T1 * Switch from Divided to Unified	-0.009**		-0.007**	-0.011**	
	(0.004)		(0.003)	(0.005)	
T1 * Switch from Unified to Divided	-0.003				0.000
	(0.004)				(0.004)
N	48375	68016	68016	68016	68016
R-squared	0.463	0.469	0.469	0.469	0.469
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes
SE state clusters	30	46	46	46	46

Table 1.19: Robustness - Incumbent party

Table 1.19 presents estimation results from various robustness tests controlling for the party of the incumbent governor. Columns 1 to 5 control for the partisanship of the incumbent governor by interacting the party of the leaving governor with the post-election year indicator variable as well as with the indicators of a change to divided or to unified. The governor party dummy is set to one for Democrat governors and zero otherwise. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable: **CAPXt/PPEt-1**				
T1 * Dem.Gov	-0.003	-0.004	-0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
T1 * Dem.Gov * Switch from D to U			-0.003	
			(0.006)	
T1 * Dem.Gov * Switch from U to D				0.008
				(0.006)
T1 * Switch from Divided to Unified		-0.009***	-0.008**	
		(0.003)	(0.004)	
T1 * Switch from Unified to Divided				-0.005
				(0.005)
N	67082	67082	67082	67082
R-squared	0.470	0.470	0.470	0.470
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	46

Table 1.20: Robustness - Additional firm controls

Table 1.20 reports the results of various robustness checks. Column 1 reports the specification that includes leverage (short term and long term debt scaled by total assets). Column 2 adds tangibility (Net PP&E over Total Assets), column 3 adds cash holdings (cash and short-term investments scaled by total assets), and columns 4 and 5 report specifications controlling for the age of the firm. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) Leverage	(2) PPE/TA	(3) Cash	(4) Age	(5) Age^2
Dependent variable: **CAPXt/PPEt-1**					
T1 * Switch from Divided to Unified	-0.0083***	-0.0074**	-0.0068**	-0.0067**	-0.0067**
	(0.0027)	(0.0028)	(0.0028)	(0.0028)	(0.0028)
T1 * Switch from Unified to Divided	-0.0020	-0.0028	-0.0027	-0.0027	-0.0026
	(0.0030)	(0.0029)	(0.0031)	(0.0031)	(0.0031)
$Leverage_{t-1}$	-0.0235***	-0.0117***	-0.0104***	-0.0101***	-0.0099***
	(0.0010)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
$PPEn/Total\ Assets_{t-1}$		-0.0577***	-0.0535***	-0.0537***	-0.0539***
		(0.0065)	(0.0062)	(0.0062)	(0.0062)
$Cash_{t-1}$			0.0321***	0.0322***	0.0322***
			(0.0011)	(0.0011)	(0.0011)
ln(Age)				0.0092**	-0.0553*
				(0.0045)	(0.0291)
$[ln(Age)]^2$					0.0185**
					(0.0082)
N	67842	67842	67839	67839	67839
R-squared	0.4743	0.4870	0.4957	0.4957	0.4958
Firm controls	Tobinq, CF g(sales), size Lagged inv.	Tobinq, CF g(sales), size Lagged inv.	Tobinq, CF g(sales), size Lagged inv.	Tobinq, CF g(sales), size Lagged inv.	Tobinq, CF g(sales), size Lagged inv.
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	46	46

Table 1.21: Robustness - Alternative measures of corporate investment

Table 1.21 reports the estimates of investment regressions using alternative definitions of corporate investment. Column 1 uses delta capital expenditures scaled by beginning-of-year total assets. Column 2 uses he percentage change in capital expenditures relative to the previous year. Columns 3 to 5 uses capital expenditures scaled by beginning-of-year total assets. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	ΔCapEx$_{it}$/PPE$_{t-1}$	**%Change in CapEx**	**CapEx$_{it}$/TA$_{it-1}$**	**CapEx$_{it}$/TA$_{it-1}$** Control for lagged CapEx	**CapEx$_{it}$/TA$_{it-1}$** Smallest firms (<p25)
Dependent variable: defined in head of column					
T1 * Switch from Divided to Unified	-0.013***	-0.060***	-0.001	-0.002***	-0.005**
	(0.003)	(0.015)	(0.001)	(0.001)	(0.002)
T1 * Switch from Unified to Divided	0.001	0.017	-0.001	-0.001	-0.005*
	(0.004)	(0.015)	(0.001)	(0.001)	(0.003)
T1 = Post Election Year	0.003	0.005	0.001*	0.001	0.003**
	(0.004)	(0.015)	(0.000)	(0.001)	(0.001)
T0 = Election Year	0.000	0.008	0.001**	0.001	-0.000
	(0.002)	(0.011)	(0.000)	(0.000)	(0.001)
N	68016	67866	69295	68016	14584
R-squared	0.100	0.144	0.637	0.684	0.585
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size Lagged capx ratio	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	46	45

Table 1.22: Robustness - Alternative measures of post-election year

Table 1.22 reports the estimates of investment regressions using alternative definitions of the post-election year. 1 month definition: the post-election dummy (T=1) takes a value of one for any firm-year in which an election is held no later than 1 month after the fiscal year beginning of year t. 3 months definition : the post-election dummy (T=1) takes a value of one for any firm-year in which an election is held no later than 3 months after the fiscal year beginning of year t. 4 months definition : the post-election dummy (T=1) takes a value of one for any firm-year in which an election is held no later than 3 months after the fiscal year beginning of year t. All independent variables are divided by their sample standard deviation to facilitate economic interpretation. Robust standard errors are clustered by state or by state and year, following Cameron et al. (2008) and Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	All firms			**Small firms**		
	1 month	3 months	4 months	1 month	3 months	4 months
Dependent variable: **CAPXt/PPEt-1**						
T1 * Switch from Divided to Unified	-0.007**	-0.006*	-0.006**	-0.021***	-0.018**	-0.017**
	(0.003)	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)
T1 * Switch from Unified to Divided	0.000	-0.002	-0.003	-0.012	-0.013	-0.016**
	(0.003)	(0.003)	(0.003)	(0.009)	(0.008)	(0.008)
T1 = Post Election Year	0.002	0.002	0.002	0.008*	0.004	0.003
	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)	(0.005)
N	69206	69442	69512	14560	14625	14644
R-squared	0.444	0.444	0.444	0.388	0.388	0.388
Firm controls	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size	Tobinq, CF g(sales), size
State controls	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)	Gov.party, g(real_gsp)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region x Time FE	Yes	Yes	Yes	Yes	Yes	Yes
SE state clusters	46	46	46	45	45	45

2 CHAPTER 2 : SME CREDIT EXPANSION IN A CRISIS

This chapter is joint with William Mullins (University of Maryland) and Christophe Cahn (Banque de France).

2.1 Introduction

Banks play the central role in reducing the asymmetric information costs of lending to SMEs, making the bank-firm relationship of crucial importance [34]. Nonetheless, SMEs have long complained that banks reduce the availability of credit in bad times, pushing many such firms into bankruptcy. How to support private lending to SMEs in times of aggregate contractions is a crucial but still open policy question. This paper exploits an unexpected drop in the cost faced by banks of funding loans to some firms in France to uncover how banks adjust their SME lending portfolio in response, and which firms are most affected by bank belt-tightening in crises.

As part of a suite of unconventional policy moves the European Central Bank (ECB) and the Banque de France introduced the Additional Credit Claims (ACC) framework in early 2012, which immediately reduced the cost to banks of funding loans to a subset of firms (by about 60 basis points). We report several novel findings.

Firstly, the fall in the cost of funding loans is rapidly transmitted into an increase in the amount of bank credit to SMEs, and, in the two subsequent years, a corresponding

[34] See for example Stiglitz and Weiss (1981), Fama (1985), Diamond (1991), and James (1987) on the role of banks in lending to small firms; Paravisini (2008), Khwaja and Mian (2008), and Jiménez et al. (2014) for evidence of difficulties in replacing bank financing; Sharpe (1990), Rajan (1992), Petersen and Rajan (1995, 1994), and Berger and Udell (1995) for the early work on bank relationships.

drop in the likelihood of payment defaults to suppliers. That is, lowering banks' cost of funding loans in a crisis period causes credit to SMEs to increase and further causes a reduction in payment defaults, relative to comparable firms. Importantly, this is after removing all bank-level capital or liquidity shocks (using bank-month fixed effects), so our results reflect the adjustments to credit made by banks in response to a pure change in the cost of lending in a crisis period.

We also find evidence that the effect is stronger for firms with only a single bank relationship – especially those with stronger observables, and with deeper lending relationships. Our findings suggest that before the shock (but still in a time of financial system crisis) single-bank firms were more constrained, potentially because adverse selection makes single-bank firms near-captives of their banks, and thus much more vulnerable to liquidity shocks affecting their lender (Detragiache et al. (2000)).

Policies to increase bank lending to firms in recessions have been an area of major policy activism in recent years: for example, in 2012 the United Kingdom introduced the Funding for Lending Scheme, while the Eurosystem introduced the ACC framework and subsequently the Targeted Long-Term Refinancing Operations.[35] Our results indicate that changing the cost paid by banks to fund commercial lending is an effective policy lever to induce credit expansion to SMEs in crises. This is in contrast to central-bank-supplied liquidity not tied to lending that has been found to be largely ineffective at expanding lending to firms in other countries (e.g. Iyer et al. (2014)),

[35]The TLTRO allowed banks to borrow funds to lend to households and companies. Banks could borrow up to 7% of their loans to companies and individuals (excluding mortgages). See Eichengreen (2015) for more details. The Bank of Japan implemented a similar policy to the ACC in 2009 and 2010.

or only of benefit to the largest firms (e.g. Andrade et al. (2015)). The efficacy of a policy that induces banks to increase lending to existing borrowers also challenges the general argument that lowering the cost of funds would be unlikely to compensate banks for the high risk of borrower default (e.g. Eichengreen (2015)).

To our knowledge, we are the first to provide clear evidence on SME credit expansions in crisis periods, and particularly how banks expand their lending portfolios in times of financial crisis. We also provide evidence that firms with only one bank relationship are particularly affected by bank credit contractions, highlighting a disadvantage to the archetypal close banking relationship that only manifests in crisis periods.

The shock that we exploit provides an unusually valuable opportunity to uncover how banks adjust their lending to firms in crisis periods, and to explore a successful lever to generate a credit expansion during an aggregrate contraction – a topic of crucial importance for policy. In February 2012 the Banque de France (BdF) changed which commercial loans could be used as collateral by banks in borrowing from the BdF - they reduced the minimum credit rating required of the borrower for the loan to be eligible collateral by one notch. This means that the shock creates clear treatment and control groups: firms in the two credit ratings on either side of the new eligibility threshold. Both groups are closely comparable: they have similar credit ratings and very clear common trends in ex ante credit growth.

Effect dominated by single-bank firms

Thus, the natural experiment we exploit operates at the firm–credit rating level, allowing us to examine the effect of a change in the cost of bank funds on lending for all SMEs in the nearby credit rating categories. By contrast, the influential literature on shocks to bank liquidity largely excludes firms with only one bank relationship from their sample for econometric reasons.[36]

One should expect a difference in the effect of the shock we consider on multi-bank firms versus single bank firms: firstly single-bank firms are unavoidably exposed to any shock affecting their lender and thus more likely to be constrained in a crisis period (Detragiache et al. (2000)). Secondly the banks of one bank firms have hold up power over their borrowers (especially during crises when switching is particularly difficult), and so may take advantage of it to charge higher rates (Santos and Winton (2008)) or instead choose to protect these rents for the future by providing additional funding in these periods (Bolton et al. (2013)).[37] Which effect dominates is an empirical question.

[36]Namely, use of the within-firm estimator to control for firm demand, together with a bank-level shock. For example, see Gan (2007), Schnabl (2012), and Iyer et al. (2014). Khwaja and Mian (2008) are an exception: their main results focus on firms with more than one bank but they also consider the effects of their bank shock on all firms, arguing that those estimates are a lower bound on the real effects. Paravisini (2008) also examines a sample including single bank firms, but finds larger lending effects for firms with multiple banks, and cautions that his results are for "normal times," as opposed to a crisis period.

[37]The literature provides some support for looking separately at single bank firms. In the Detragiache et al. (2000) model, firms choose between two regimes: single or multiple banking, largely based on the probability of a bank liquidity shock which causes premature liquidation. Petersen and Rajan (1994) report that in the cross section additional bank relationships are associated with higher interest rates and lower credit availability, and that strong relationships may provide an "informational monopoly, so that cost reductions are not passed on to the firm" but instead manifest as quantity changes (p35). Houston and James (1996) also find differences in debt behavior between single and multi-bank firms in a sample of public firms.

Our central result is precisely that the firms excluded from the bank shock literature – firms with only one bank relationship – that are most affected by a drop in the cost of funds for lending. We find a 7% increase in debt for single bank firms, and a 2.5% increase for 2 and 3 bank firms. Single bank firms are younger and smaller than multibank firms, are a large fraction of the firm population (making up 83% of firms in France), and employ a large part of the workforce (∼38% of private sector workforce in France), which makes enhanced credit access for such firms likely to have material effects both on innovation and economic activity more broadly. [38]

Which single-bank firms are most affected?

Not all single bank firms are equally affected by the reduced cost of funding loans. Banks adjust their lending portfolio as existing loans mature or as firms request credit – our shock provides a window into this process. We find that additional credit flows on average to firms with stronger observables: firms with lower leverage, older firms, and firms that are net providers of trade credit (rather than users). We find no evidence of loan ever-greening: the effects of the shock do not vary with proxies for size, interest coverage or profitability in our sample. High-growth firms also see especially large increases in their debt growth (of 15 to 30 percentage points) relative to ineligible high-growth firms, and while the effect is especially pronounced for single bank firms, it is also present for multi-bank borrowers. Because high growth firms

[38] In 2008, 83% of the population of French firms – including micro firms – had a single-bank relationship. This number is highly correlated with firm size with 86% of micro firms, 39% of SMEs and 21% of large firms had only one bank relationship (Banque de France, 2010). The employment figure is an approximation based on the percentages of single-bank firms by size category (large, SMEs and micro firms) and by number of employees in each private sector category in France in 2012. (Insee, 2012)

generally have high credit demand, this differential effect provides evidence consistent with these firms being credit constrained ex ante.

Bank relationships provide some benefit within single bank firms

The banks of firms with a single relationship are particularly likely to have developed the relationship, given the reduced scope for information externalities benefiting other banks, or strategic default behavior by borrowers. Thus, single bank firms with a well-established banking relationship would be most likely to see their lending increase (see for example Petersen and Rajan, 1994). We find evidence that banks value soft information acquired in a banking relationship: firms which maintain both long relationship and information intensive relationships - by engaging in a wider range of transactions with their bank – see their debt respond more to the positive shock.

We next consider whether richer relationships are a substitute for or a complement to firm observables such as leverage, age and net provision of trade credit. We find suggestive evidence that relationships substitute for such observables. Single bank firms with a wider range of transactions do not see any additional credit if their observables suggest they are better borrowers. By contrast, firms without a wide range of transactions with the bank show a heterogeneity of effect associated with standard observables: firms receive more credit if they have low leverage, are older, or are net providers of trade credit. In short, it seems that richer bank relationships are valuable for single bank firms. [39]

[39]In unpublished results we do not find similar quantity effects of banking relationships for multi-bank firms, even when looking at their main bank. As the overall effect of the ACC reform is weaker for multibank borrowers this may be because of low power. Alternatively, it could suggest that

Payment default

Our final finding is that firms that are eligible for the ACC framework are 0.6% less likely to default on payments to their suppliers than non eligible firms, in the two years following the shock. This reduced rate of default actually begins to have a detectable effect only six months following the shock, at which point the reduced probability of a default event is over 1 percent relative to ineligible firms. Overall, the finding that the fall in the cost of bank funds causally reduced defaults to suppliers suggests that bank belt tightening may itself induce defaults in borrowers that propagate through their supplier networks in crisis periods. The evidence is consistent with Boissay and Gropp (2013), who show credit constrained firms pass on adverse liquidity shocks by defaulting on their suppliers.

Single-bank firms are on a much more negative time trend ex ante, and the trends are not affected by shock

So far we have focused on differences within the single bank category across treatment and control groups, rather than comparing single bank firms to those with multiple bank relationships. This is because firms with only one banking relationship are very likely to differ from those with multiple banks on unobservable dimensions as well as on observables. However, some comparison of the effects of the shock across these categories is warranted, with the caveat that we can no longer be confident that these differences are causal.

information externalities when a firm has multiple borrowers are such that relationships are less valuable (Rajan, 1992), and so adjustments may occur more on the price than the quantity margin (Petersen and Rajan, 1994).

We find a striking difference in the time trends in bank lending to single versus multiple bank firms: the former have consistently negative average debt growth in the four year period we examine (2010-2013), while for multiple bank firms debt growth is stable or increasing. Moreover, the differential time-trends are largely unaffected by a change in the cost of bank lending, suggesting a deeper difference in how these borrowers are viewed by banks, despite having identical credit ratings. This is consistent with Detragiache et al. (2000) which present multiple bank relationships as an insurance mechanism against bank liquidity shocks. [40]

Generally, single-bank firms look more credit constrained ex ante

Additional evidence points to single bank firms being more credit constrained than firms with multiple bank relationships, although the evidence for this is suggestive, not causal. Single bank firms' debt increases much more in response to the reduction in bank funding cost, as described earlier. The total amount lent to the single bank firms in the sample also declines steadily over time, suggesting banks are not rolling over the debt of single-bank firms in this period.[41] These patterns do not hold for multi-bank firms. Further, only 29% of single bank firms have undrawn credit lines worth over five percent of their debt stock in 2011, while 50% of multi-bank firms have such lines.

We provide additional evidence for the presence of financing constraints for single-

[40] They model the firm's likelihood of choosing to have only one bank as increasing in profits, bank recovery rate after default, and the probability of an idiosyncratic bank liquidity shock – single bank firms are forced to prematurely liquidate if their bank is hit by a shock.

[41] Only 14% of single bank firms see their debt grow by over ten percent month on month in the pre-period (2011) in comparison to 23% of multibank firms, which is consistent with single bank firms being less likely to have their debt rolled over when it is nearing maturity.

bank firms by testing the effect of the ACC on a sample of firms that have ex-ante high demand. We construct a measure of firm demand by looking at a subsample of firms that saw a high number of non-trivial debt increases in the year preceding the shock. Single bank firms in the top quartile of this measure see especially large differential increases in lending in response to the shock ($\sim$ 20 percent) and this is even greater ($\sim$ 30 percent) for smaller firms, which are generally believed to be more opaque. Interestingly, we find no response in the debt of multibank firms to this measure differentially across the eligible (treatment) and ineligible (control) groups, suggesting that high demand firms with several banks did not need the ACC framework to obtain more lending, and so are likely less financially constrained.

Implications of single bank firm credit constraints for policy and literature

If single bank firms are indeed more credit constrained than multi-bank firms in bad times, this has implications both for policy and for the academic literature. Firstly, policies to induce bank lending to firms may be more effective if oriented towards them in bad times, especially given the potentially contagion-reducing effects via reduced supplier non-payment reported here. Of course, it is unclear whether such policies are welfare enhancing overall, but they appear to be consistently popular with policymakers. Secondly, the view in the empirical literature on relationship banking that fewer and stronger relationships lead to better access, weakly lower prices and lower collateral requirements may need a caveat: having only one bank relationship may be very disadvantageous in crisis periods. In the light of the evidence on the

bank lending channel – that the real effects of aggregate shocks (or bank specific shocks) can be aggravated by the financial constraints of banks –single bank firms may be particularly vulnerable to shocks to their bank, as in the Detragiache et al. (2000) model.

A recent stream of papers focusing on the role of relationship banks during recessions has found mixed evidence, with some papers finding a protective role for relationships (Bolton et al. (2013); Deyoung et al. (2015)), while others find limited effects or even the opposite (Jiménez et al. (2014); Santos and Winton (2008)). While directly relevant to our finding that single bank firms, with their deeper relationships, seem to be at a disadvantage in recessions, these papers cannot distinguish the different dynamics of single bank lending during recessions from that of multi-bank firms because of their data or empirical strategies. In an important paper, Bolton et al. (2013) find that relationship banks (identified by low distance from their borrowers' headquarters) in Italy provide continuation financing for their borrowers in crisis periods, unlike transaction banks. However, for econometric reasons this study focuses exclusively on firms with more than one banking relationship.[42] Similarly, Deyoung et al. (2015), find that a small subset of relationship-focused US community banks increased their lending to SMEs during the financial crisis, unlike the majority of banks. But their data is aggregated at the bank level, and so presents an average across firms with all numbers of bank relationships.

In contrast to these results suggesting a positive role for relationships in crisis peri-

[42]Albertazzi and Marchetti (2010)and Sette and Gobbi (2015) find protective results and also focus exclusively on multi-bank borrowers.

ods, Jiménez et al. (2014) find that Spanish banks are more likely to approve loan applications from new borrowers when they had a working relationship with the borrower in the past , but the effect is small and they find no differential effect of lending relationship over the cycle. Further, Santos and Winton (2008) report that banks in recessions opportunistically raise interest rates by more than is justified by risk alone, exploiting the hold-up power generated by the relationship. However, their data is for large firms only: listed corporations and syndicated loan users that have ready access to non-bank finance, making it difficult to apply to SMEs.

Using loan applications data for new borrowers, Jiménez et al. (2014) compare the relative importance of the bank and firm lending channels over the cycle. While they offer evidence that firm balance-sheet strength matters, in crisis and in good times, for building new lending relationships (extensive margin of the balance sheet channel), we investigate the intensive margin. We offer new evidence on heterogeneous effects of the firm-lending channel for existing borrowers conditional on the structure of information available to lenders (i.e. monopoly vs. shared information). Investigating the intensive margin is more appropriate for us, as our research question focuses on the use of information by banks in their loan portfolio allocation decisions. We are thus able to explore heterogeneity of the effects of our shock along different characteristics of the bank lending relationship for existing relationships and we show how the interaction between soft and hard information differentially affect loans renewal decisions and how soft information sometimes dominates firm balance sheet strength.

More generally, this paper relates to the vast literature on the bank-lending channel.

Extensive evidence supports the view that banks pass on monetary policy tightening (e.g. Kashyap et al. (1993, 1994);Jiménez et al. (2012)) [43] and unexpected liquidity shocks (Peek and Rosengren (2000); Khwaja and Mian (2008); Chava and Purnanandam (2011); Schnabl (2012)) to their borrowers. Much less is known however about adjustments to positive liquidity shocks and in particular, of highest interest from a policy perspective, about how expansions work in periods of aggregate contraction, the focus of this paper.[44]

This paper also contributes to the literature on the real effects of the lending channel, which analyses how firm level outcomes are affected by bank supply shocks.[45] To our knowledge we are the first to show how positive liquidity shocks in crisis periods create real benefits in the form of positive spillovers to firm suppliers.

From a policy perspective, a firm-level focus is critical to assess the policy effectiveness and its distributional effects, as loan level effects can be offset by firm-level adjustments when firms have multiple lenders (Jimenez et al. (2014)).[46] Interestingly our

[43]Jiménez et al. (2012)analyze the extensive margin of lending with loan applications data and offer micro-based evidence of an operative bank-lending channel, which varies with the strength of bank balance sheet (capital and liquidity).

[44]Paravisini (2008)examines a lending program in Argentina to support lending to SMEs in poorer regions. The expansion in available external finance had a substantial positive effect on the credit supply of constrained banks, but cautions that the reported effects are for good times. For France, Andrade et al. (2015) find evidence that the ECB long-term refinancing operations (LTROs) implemented by the ECB in 2011 and 2012 had a combined positive and significant impact on the overall net credit supply to large borrowers.

[45]The literature typically finds that real economic activity such as firm investment and inventory decisions (Kashyap et al. (1993, 1994); Chava and Purnanandam (2011)), firm investment composition (Garicano and Steinwender (2015)), as well as firm employment decisions (Greenstone et al. (2014); Chodorow-Reich (2014)) are significantly negatively affected by tight monetary policy or exogenous negative shocks to credit supply.

[46]Jimenez et al. (2014)find a positive loan-level effect of the positive credit supply shock induced by increased access to securitization in Spain in 2004-2007, but no effect at the aggregate firm level and no real effects on sales or employment, suggesting that firms take advantage of improved terms of credit to reduce their interest burden but not to increase liabilities overall.

paper offers also a way to look at those borrowers who are unable to undo the bank lending shocks : the smallest firms (Khwaja and Mian (2008); Iyer et al. (2014)).

2.2 The Additional Credit Claim reform

2.2.1 The Additional Credit Claim framework

All borrowing by banks from the Eurosystem (credit operations such as open market operations, use of the marginal lending facility and intraday credit) needs to be secured with eligible collateral. The Eurosystem allows banks to pledge marketable [47] and non-marketable securities as collateral in its credit operations (Tamura and Tabakis (2013)). The minimum credit quality requirement for eligible credit claims (i.e. bank loans) is equivalent to a rating of 4+ in the Banque de France's rating scale,[48] or a long-term rating of BBB-/Baa3 from S&P/Moody's. Collateral is pledged by a borrowing bank at a national central bank and enters a borrower-specific pool of collateral against which it can borrow from the Eurosystem (collateral is not

[47] Marketable assets consist in marketable debt instruments complying with the eligibility criteria defined by the Eurosystem-wide eligibility rules, known as the "General Framework". e.g. Central government or central bank debt instruments, Covered bank bonds, Bank and corporate debt instruments, Asset-backed securities. Bank loans to high credit quality firms are eligible non-marketable securities.

[48] The Banque de France assigns credit ratings to all French non-financial companies with a minimum turnover of €0.75 million and accounting statements. The rating reflects the overall assessment of firms ability to meet their financial commitments at a three-year horizon, and is used as to select the loans that banks are allowed to use as collateral for their refinancing with the Eurosystem. Ratings are based on firms' accounting statements, as well as information on supplier/customer trade bill payment incidents, bank loans reported by credit institutions, and legal information as well as other sources. Firms are broken down into the following classes by default probability:3++ (highest), 3+, 3, 4+, 4, 5+, 5, 6, 7, 8, 9 to P (in bankruptcy). The Banque de France does not receive any payment from rated companies and always informs companies of their rating, although the rating is not public. Finally, the rating is reviewed at least yearly after receipt of firm financial statements, and whenever a significant new development is brought to the attention of the Banque de France.

tied to a specific operation)[49].

In response to a liquidity crisis in the Eurozone interbank funding market in 2011 and as part of a broader set of non-standard monetary policy measures to improve liquidity the ECB allowed National Central Banks to accept additional credit claims (ACC) as collateral on December 8th 2011. The ECB also implemented two so-called long-term refinancing operations (LTROs) with 3 year maturities. The first LTRO took place on 21 December 2011, before the implementation of the ACC framework. The second operation took place on 29 February 2012, after the French ACC framework was approved. . On February 9th 2012, the ECB approved the criteria proposed by seven national central banks [50], including the Banque de France, for the implementation of the ACC framework, which is temporary, but has been extended to at least September 2018. The Banque de France implementation of the ACC lowered the minimum eligible credit rating from 4+ to 4 (corresponding to a maximum probability of default of 1% at one year) and extended the framework to new categories such as stand-alone guaranteed mortgages.[51]

2.2.2 Estimating the size of the ACC shock

The total value (after haircuts) of the collateral pledged with the Banque de France by 54 banks at the end of 2011 was €412.8 billion (22% of total value of collateral pledged with the Eurosystem), among which credit claims represented 36% or €150

[49]Since October 2008 no quantity restrictions apply to Eurosystem open market operations if the borrower provides sufficient collateral.

[50]Approved Central Banks are Central Bank of Ireland, Banco de Espana, Banque de France, Banca d'Italia, Central Bank of Cyprus, Oesterreichische Nationalbank and Banco de Portugal.

[51]See: Eligibility Criteria regarding Additional Credit Claims. https://www.banque-france.fr/uploads/tx_bdfgrandesdates/2012-02-9-eligibility.pdf

billion (Bignon et al. (2016) - See table **??** in Appendix). In France, the ACC reform made available an additional pool of corporate credit claim collateral of about €90 billion (total outstanding amount of loans that became eligible in February 2012), which, according to Bignon et al. (2016) corresponds to an individual collateral shock for French banks that could have represented 4.8% to 15.1% of their drawn credits. In practice the use of the ACC was more limited for corporate credit claims (20% of pledged ACC loans for total of €9 billion after applying the haircut schedules specified in the French ACC framework) than for stand-alone residential mortgages made eligible at the same period.[52]

Our estimate for the size of the fall in the cost of funding for banks when the ACC program launched is the spread between the Euribor-12month rate interbank borrowing rate, and the ECB main refinancing rate at which banks could obtain loans using the newly eligible collateral. The Euribor-12month was 1.5% on average in March 2012 and the main refinancing rate was 1%, so the spread was 50 basis points. We have not been able to improve on this back-of-the-envelope calculation, as there are several difficulties in estimating the true market cost of funding for French banks. Firstly, we do not know the maturity of the loans against which the ACC claims are pledged and this information is hard to obtain, as collateral is not tied to a specific operation. The maturity at which banks can borrow from the Eurosystem ranges from three years (the second VLTRO which occurred at the same time as the ACC introduction) to one week. Secondly, the Euribor benchmark that we are using as the

[52]Haircuts vary from 17% to 65% depending on the characteristics of the loans. See : https://www.banque france.fr/fileadmin/user_upload/banque_de_france/Eurosysteme_et_international/cp-20130718-bce-reexamine-son-dispositif-de-controle-des-risques.pdf

market borrowing rate is a eurowide reported interest rate; thus it is not an observed rate, nor is it specific to French banks (Anecdotal evidence suggests that French banks were participating in the interbank market in this time, while banks from euro area periphery countries were not.) Thirdly, interbank rates reflect rates for unsecured lending, while the ECB refinancing rate is secured (albeit with collateral that cannot be used in any other contexts). Finally, since October 2008, refinancing operations with the Eurosystem were on a full allotment basis and at a fixed rate. By contrast, interbank market rates vary with the risk profile of the borrower and with supply and demand conditions, making it likely that borrowing large amounts would push actual rates beyond Euribor levels.

Over the course of 2012 the Euribor-OIS spread fell, following the massive injections of liquidity by the ECB; by the end of 2012 it seems clear that the advantage of the ACC as a source of below-market-cost funding had largely disappeared. Thus, the shock we exploit lasts, at most, for ten months (February-December 2012).

2.3 Empirical challenges and identification strategy

2.3.1 Identification strategy

We investigate the causal effects of a positive credit supply shock on treated firms and closely comparable non-treated firms to show how bank changed their lending portfolio during the crisis. While the collateral reform was not targeted at small firms in particular, we restrict our attention to SMEs so as to provide new insights about the availability of credit for the most opaque, and thus likely the most constrained

firms, and more particularly on single-bank borrowers.

As illustrated by figure 2.1, our empirical strategy exploits the fact that the new collateral framework reduced the costs to banks of lending to some types of SMEs (firms rated 4) - by lowering refinancing costs for loans to the newly eligible counterparts -, but not to others that are closely comparable (firms rated 5+). Using firms whose loans are ineligible to be pledged as collateral as our control group, we focus on the behavior of newly eligible firms before and after 2012 to provide difference-in-differences estimates of the impact of the program on various firm-level outcomes.

France provides an ideal setting for this study as this is a banking economy and we use a sample of SMEs which are typically bank dependent. In our sample less than 1% of the firms have access to public debt markets so that they were not able to substitute bank debt by increased bond financing. We focus especially on single-bank firms as they are entirely exposed to any liquidity shock to their bank (Detragiache et al. (2000)) and cannot counterbalance it by accessing funds from other banks. Furthermore their lender has an informational monopoly and this private information is not observable by other banks so that the cost of switching to a new lender is potentially very high, specially during crises.

Our main empirical challenge is to isolate supply effects from credit demand and business cycles effects in a time of crisis. We overcome these hurdles by taking advantage of the great features of the ACC shock, which varies at the firm (rating) level, and of the richness of our individual firm credit data, available at a monthly frequency in the French Credit Register. To overcome the demand/supply identification challenge,

the literature on the bank-lending channel typically look at cross-sectional differences in bank responses to common liquidity shocks (e.g. Kashyap et al. (1994); Kashyap and Stein (2000)) or they restrict their attention to firms that have at least two banking relationships, which allows the empiricist to control for unobservable firm loan demand (Gan (2007); Khwaja and Mian (2008); Andrade et al. (2015); Schnabl (2012)). This means that the effects on SMEs, which generally have a single banking relationship, have not been well established.

On the contrary, our paper exploits a positive supply shock, the ACC reform of January 2012, which varies at the firm level (rating level) and not at the bank level. Our strategy has several advantages. First, it means that the economic interpretation is more direct, because banks' response to the shock likely reflects their normal adjustment process to a change in the cost of funds in recession periods, rather than the more disordered reaction of banks to emergency conditions generated by large unexpected liquidity shocks. Second, the shock is not vulnerable to concerns raised by Khwaja and Mian (2008) and Paravisini (2008) regarding the within firm estimator: in particular, that banks respond differently, and that this response is correlated with the bank-level shock in some way. Third we do not exclude single-bank firms, which have typically been ignored in the existing literature. Finally we can study firm-level outcomes (and not firm*bank level) as our identification strategy does not rely on variations in shocks to bank finance within firms but comes from variations in the cross-section of firms, within a common lender. Looking at the firm level is critical to assess whether and how lending shocks are transmitted to the economy.

Indeed, loan level results can be misleading as the loan-level bank-lending channel can be undone by firm-level adjustments of multibank firms, which reallocate their borrowing portfolio across banks to take advantage of improved terms of credit and reduce their interest burden but without increasing liabilities overall (Jimenez et al. (2014)).

No differential trends unrelated to credit availability

We use a triple difference approach in the spirit of Banerjee and Duflo (2013). We focus on the change in the growth rate in firm drawn debt, before and after the reform for newly eligible firms (ACC) and for non eligible firms in the closest credit rating bucket (Rating 5+).

[insert Figure 2.1]

Our main identification assumption is that treated and untreated firms share similar trends and that their credit trend does not systematically differ around the reform period apart from the effect of the reform. Figures 2.2, 2.7 and 2.8show the average growth rate in debt for treated and untreated firms in the whole population.

Control firms look similar to treatment firms in terms of their growth rate in debt prior to the reform. The treated and control groups follow parallel trends prior to the reform and diverge at the time of the reform. We then separate our sample between single-bank and multibank firms. The growth rate in drawn credit of treated firms rises following the introduction of the ACC, most particularly in the single-bank subsample (figure 2.7).

[insert Figure 2.2]

[insert Figure 2.7]

[insert Figure 2.8]

We confirm the parallel trend assumption more rigorously in a regression set up, where we interact the treatment dummy with a time trend in the pre-treatment period. Table 2.3 reports the results of this test. Single-bank ACC firms do not show any evidence of a differential pre-trend in debt growth. For multibank however we reject the null hypotheses that the coefficient estimate of the trend is zero with a much narrower margin, looking at the one-year period before the reform.

[insert Table 2.3]

Exogeneity of rating, mixing between treatment and control groups and attenuation bias

We estimate an Intention-To-Treat effect based on the rating of the firm that makes it eligible to treatment but we do not observe firms that are actually treated or not treated i.e. whose loans are pledged or unpledged.

We can rule out selection bias as treatment is based on the Banque de France internal credit rating, which is exogenously set by the Central Bank. One could still be concerned of a possible change in the attribution criteria of ratings after the ACC - for example making it more difficult to get a rating of 4 - so that increased credit availability for this category of firms after the ACC would reflect a composition effect and a change in the quality of firms rated 4. To address this issue, and also because

after February 2012 rating becomes endogenous to treatment and can directly be affected by enhanced or restricted access to extra credit, the composition of our treatment and control groups is based on firm rating before the ACC date (January 2012).[53]

The drawback of this approach is that firm rating varies over time and firms can get downgraded or upgraded in the post-treatment period. As a result we have some mixing between our treatment group and our control group, which creates some type I and type II errors. This is true both in the pre and post treatment periods. The stronger the bias in our estimates the further away we go from the ACC date (February 2012). Looking at the occurrences of rating downgrade and upgrade over time in the year after the ACC reform we show that we actually face an attenuation bias.

[insert Figure 2.5]

[insert Figure 2.6]

As shown by figure 2.5, after one year about 20% of firms that were initially rated 4 have been downgraded at least once and have not been eligible to treatment for at least one month. By considering them as part of the treated group we underestimate our intention to treat effect. Symmetrically, about 25% of 5+ firms have been upgraded at least once over the year following the ACC (see figure 2.6). As a result they are eligible to treatment while we are considering them as controls. By considering them as part of the control group we underestimate our intention to treat effect.

[53]Results are robust to defining samples based on November 2011 or December 2011 firm credit rating.

2.3.2 Model

We estimate a reduced-form equation of the form :

$$g_{ijkt} = \alpha_i + \beta ACC_i \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{ijkt} \tag{2.1}$$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. The sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal credit risk rating scale of the Banque de France). The dependent variable is the cumulative growth rate in the outstanding amount of drawn credit, $g(Debt)_{ijkt}$ defined below.

One difficulty in using our data is that we do not observe new loans but only monthly outstanding amounts of credit. To proxy for new credit we thus look at the growth rate in outstanding amounts. Because a large part of changes in credit outstanding amount is driven by periodic amortization of the debt, monthly growth rates are too volatile and noisy to isolate significant changes. We thus use a cumulative growth measure. We measure the growth rate in debt g_{ijkt} for firm i banking with main bank k in month t as follows: $g_{ijkt} = (D_{ijkt} - \bar{D}_{ijk2011s1})/E_{ijkt}$ where $E_{ijkt} = 0.5 * (D_{ijkt} + \bar{D}_{ijk2011s1})$, where D_{ijkt} is the outstanding amount of drawn credit (short-term bank credit and long-term loans) in month t for firm i borrowing from bank k .$\bar{D}_{ijk2011s1}$ is the average

level of debt of firm i in the first 6 months of 2011.[54]

Our growth rate measure is inspired by the labor literature analyzing establishment and firm dynamics (See Davis et al. (1996)) and is bounded between -2 and +2. Since we use a cumulative growth measure it is not symmetric around zero but still smooths the distribution a lot compared to a traditional growth rate measure. Taking the last 6 months of 2011 or year 2010 as period of reference does not affect our results (cf. robustness table 2.15 in Appendix). Using a traditional cumulative growth rate does not change our conclusions either but give larger coefficient estimates due to a small borrower effect as small credit exposures tend to be much more volatile than large ones.

The 1_{ACC_i} dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. Our parameter of interest is β , the estimated intent-to-treat (ITT) effect of the reduction in bank funding costs induced by ACC, on a sample of newly eligible borrowers. The OLS estimate of β is an unbiased estimate of the ITT effect under the parallel trend assumption that changes in firm borrowings over time would have been identical in both treatment and control groups in the absence of the ACC.

We saturate the model with a set of firm (α_i), bank*month ($Bank_{kt}$) and industry*quarter fixed effects (Ind_{jT}). Firm fixed effects remove the average difference in credit growth between ACC and 5+ firms and allows us to control for unobserved

[54]When the firm drawn credit amount reaches zero for the second consecutive month we replace our growth measure with a missing so that it does not get stuck to -2. We also exclude firms with 0 debt in the first semester of 2011 to avoid having some obervations always set to +2.

(time-invariant) firm fundamentals that proxy for credit demand. Though risk or investment opportunities may vary over time our estimation period is limited to two years so that it should not be a main concern. Furthermore for demand factors to alter our estimates, one would have to believe that demand changes are occurring in our sample in a way that is systematically different across both rating groups, even after taking out the effect of bank time-variant heterogeneity and of macroeconomic fluctuations at the industry level.

Given our short period of estimation, one year before and one year after the ACC, we do not include firm-specific control variables such as size, age and financial statement variables as they are annual variables and almost all their explanatory power is absorbed by our firm fixed effect.

We fully account for both observed and unobserved time-varying bank heterogeneity by saturating the specifications with bank * month fixed effects. For multibank firms the fixed effect is defined with respect to their main lender. The average share of borrowings from main bank in the total firm drawn credit is 78% in 2011. Identification comes from comparing the changes in lending in the same period by the same bank to firms of different rating.

As mentioned previously the implementation of the ACC reform was concurrent with exceptional extra liquidity supplied by the Central Bank (second VLTRO) and thus made available for lending to newly eligible borrower (4-rated firms) after February 2012. As a result, at the beginning of our post-treatment period, the comparison of newly eligible (rating 4) vs. non-eligible firms (rating 5+) captures the joint effect

of the LTRO and of the ACC but is not driven by the differences between banks in terms of VLTRO intakes (because of bank*month fixed effects). Within the same bank, there is no obvious reason why VLTROs should differentially affect 4-rated borrowers and 5+-rated borrowers but for the ACC. Bank*month fixed effects also absorb heterogeneity in bank responses to the ECB announcement of outright open market operations (OMTs) in August 2012.[55]

Finally we control for any trends that might affect the French economy at the industry level by including industry*quarter fixed effects.

We cluster the standard errors at the firm level to address auto-correlation issues (Bertrand et al. (2004)).

In a second time, to investigate dynamic of the effect over time we extend our sample period from 2010 to 2013 and saturate our model with interactions of the ACC effect and of monthly dummies in each months but the first 12 months of our estimation period. We estimate a β_t for each time period to follow the monthly dynamic of the effect of the ACC reform on lending to single-bank SMEs. The estimated equation is of the form :

$$g_{ijkt} = \alpha_i + \sum_{t>Jan11} \beta_t 1_{ACC_i} \times t + Bank_{kt} + Ind_{jT} + \epsilon_{ijkt} \tag{2.2}$$

[55] Acharya et al. 2015, show that OMT program announcement led to an increase in bank health and in turn that banks with improved health increase credit supply to low quality borrower. Bank sensibility to OMT depends on how exposed to Sovereign Debt in Portugal, Spain and Greece they were.

2.4 Data and Summary Statistics

2.4.1 Data description and sample composition

This study considers a sample of independent (one legal unit) Small and Medium Size Enterprises (SMEs)[56]. For the sake of our identification strategy we restrict our attention to SMEs with a rating of 4 and 5+ on the internal rating scale of the Banque de France. The data spans a period between 2010 and 2013. The level of observation in our data is a unique firm x month combination, for firms having some positive bank debt over the period 2011-2012. Our primary data sources are the French national credit register (monthly data on outstanding amount of bank credit), available at the Banque de France, the FIBEN individual company database (yearly financial statement data) and the FIBEN internal credit rating database of the Banque de France.

Firm-level credit rating

For the purpose of this paper, we assign firms in our treatment or control groups based on their rating[57] in January 2012[58]. To this end we select firms with an active

[56]SMEs are defined by the French Law of Modernization of the Economy (LME) of 2008. SMEs are firms with less than 250 employees, an annual turnover of less than EUR 50 million and balance sheet assets totaling less than EUR 43 million.

[57]The French Central Bank attributes credit ratings to a large number of resident non-financial firms. Around 270,000 companies (of which over 4,700 groups assessed on the basis of their consolidated accounts) are rated in this manner. Financial products are not rated; ratings are not made available to the public. Credit ratings are used by commercial banks to evaluate whether a firm's bank debt is eligible to refinancing operations for Eurosystem monetary policy operations. The rating is an overall assessment of a company's ability to meet its financial commitments over a three-year horizon, based on its financial statements as well as on qualitative information. Rating information is updated on a daily basis, should an incident impact the firm's ability to meet its financial commitments, or on a yearly basis for the annual review, provided firm accounting information is made available to the Central Bank.

[58]Results are robust to selecting control and treatment groups based on November 2011 or December 2011 ratings.

credit rating over the period of interest (2010-2013). We exclude firms with inactive ratings i.e. firms whose financial information has not been updated since 23 months or more and we require each borrower to have a credit rating of either 4 (newly eligible to be pledged as collateral under the ACC i.e. treated firms), or 5+ (closest rating category, one notch below, for non eligible firms i.e. control firms).

A rating of 4 corresponds to a 1% probability of default at a 1-year horizon. Firms in these three rating categories represent about 50% of the total sample of SMEs with an active credit rating as of January 2012, with 22.1% having a rating of 4 (ACC), 12.6% a rating of 5+ (one notch below) and 15.6% a rating of 4+ (one notch above). Because we cannot require firms to stay in their rating category after the ACC date, as rating becomes endogenous to treatment, there is some mixing between our control and treatment groups as firms get upgraded and downgraded after February 2012. We have shown in the previous section it creates an attenuation bias of our policy effect.

Firm accounting data

This study considers a sample of independent (one legal unit) SMEs. Independent SMEs[59] are identified using Banque de France available information on firm financial linkages (structure of ownership). We restrict our attention to independent SMEs to avoid any double-counting issues in the financial statements[60] and to exclude effects coming from intra-firm liquidity flows between holdings and SME subsidiaries. This

[59]SMEs that do not belong to a group and are mono legal entity.

[60]We are using financial statement information and bank-firm credit data that are reported at the legal unit level and are not consolidated.

way we also make sure mergers, divestitures or acquisitions effects do not affect our results. Accounting data comes from FIBEN, a Banque de France database, which is based on fiscal documents.[61]We exclude micro-firms[62] from our sample as well as agriculture, financials, utilities and public sector firms. We also eliminate firms with special legal status and only keep limited liability firms i.e. SA and SARL, which make 97% of our selected SME sample. We drop firms with negative debt, negative or zero total assets and missing number of employees. We use firm size (log of total assets or number of employees), age, leverage, tangible investment rate and trade credit use as independent variables. All firm characteristics variables are winsorized at the 0.5 th and 99.5 th percentiles throughout the analysis.

Firm-bank credit data

We merge yearly financial statement data with individual credit data from the French national credit register (CCR)[63] available at the Banque de France. CCR covers extensively bank exposures to firms at the bank-firm level on a monthly basis. [64]

[61]FIBEN includes all French firms which sales at least equal to EUR 75,000. In 2004, FIBEN covered 80% of the firms with 20 to 500 employees, and 98% of those employing more than 500 employees.

[62]Under the LME definition micro-firms have less than 10 employees and sales and total assets not exceeding EUR 2 million.

[63]Financial intermediaries, including all resident credit institutions, investment firms, and other public institutions, have the legal obligation to report any risk exposure (e.g., credit claims) over EUR 25,000 on a corporate counterpart as defined by a legal unit and referenced by a national identification number (SIREN).

[64]"In practice, a significant methodological change regarding the scope of this reporting threshold happened in April 2012. Before this date, a bank had to report its bilateral exposures larger than EUR 25,000 as measured at the level of its local branches. After this date, a bank has to report any bilateral exposure that is greater than EUR 25,000 as measured at the level of the whole bank"Andrade et al. (2015). Following Andrade et al. (2015), we correct for this break by looking at the information available at the bank branch-firm level. We dropped all bilateral branch-firm links with a total exposure smaller than EUR 25,000 and then collapse this homogenized database at the bank-firm level .

Reporting statements are not limited to bank loans, they include undrawn credit lines as well as guarantees, and specific operations (medium and long-term lease with purchase option, factoring, securitized loans).

We first collapse exposures at the local bank branch (aggregating so-called "guichets" exposures) to identify the main branch lender of a firm. Then we collapse credit exposures at the level of banking groups (in French: GEAs, for "groupe economique d'appartenance") in order to assess the effect of the ACC policy at the firm level and not at the firm*bank level, since we are interested in the overall effect of the policy. Indeed firms with multiple bank relationship can react by adjusting their sources of financing in equilibrium so that firm*bank level effects are not informative of the aggregate lending channel (Jimenez et al. (2014)). We use the word "bank" in the rest of the paper to refer to banking group (GEA) and will specify local branch when we refer to a finer level of granularity within lenders.

We require main banking groups to be present in the sample throughout the whole period so as to make sure they are not affected by bankruptcy, restructuration or merger. Finally, an implicit requirement of the difference-in-difference strategy is that the firms are present in the pre and post period. We thus require firms to maintain a bank relationship from March 2011 to February 2013 i.e. one year before and one year after the ACC reform. We analyze changes in the growth rate of drawn credit at the firm x month level, over the period during which the firm has some positive bank credit liability.

Payment default data

Finally we use individual payment default data on trade bills coming from the CIPE (Centrale des Incidents de Paiement), maintained by the Banque de France. The CIPE database contains information related to all defaults on trade bills for all private nonfinancial businesses that operate in France.[65]

In CIPE a "default" is defined as a failure for a firm to pay, in time or in totality, its trade creditors. Payment defaults are recorded on a daily basis. This data has been used in other academic studies (Barrot (2016); Boissay and Gropp (2013); Aghion et al. (2012)) and payment defaults have been shown to be negatively and significantly correlated with a firm's access to future loans (Aghion et al. (2012)).

About 2.5% of single-bank firms in our sample experience at least one payment incident in 2011. Not all industries are concerned by payment incidents. Construction and Trade have the highest default rates in our sample, while conversely, Services and Real estate firms experience fewer payment incidents. When an incident on a payment of a trade bill occurs, we have information on the identity of the supplier, the customer, the amount of default, the due date of payment and the motive for the non-payment (dispute, omission, illiquidity, or insolvency). We restrict our attention to payment incidents triggered by insolvency issues (liquidation of the firm) or by liquidity shortages leading a firm to, totally or partially, miss a payment to one of its suppliers[66].

[65]Banks have the legal obligation to report to the Central Bank any default on trade creditors within four business days. CIPE receives and centralises declarations by credit institutions of trade bill payment incidents and then made it available to all commercial banks.

[66]We exclude claim disputes with specified reasons but take into account failure to repay a trade bill in totality under the alleged reason of a dispute.

Results are robust to different definitions of default. Our default dependent variable is a dummy variable equal to one when the firm has experienced at least one payment incident during the month, and to zero otherwise.

2.4.2 Summary statistics

Table 2.1 presents descriptive statistics that compare treated (firms with a rating of 4) and control firms (firms with a rating of 5+) in the year prior to the reform (2011). T-tests present the t statistic of a test of the difference in means between the treated and the control groups. Panel A shows the summary statistics for the overall sample, while Panel B and C are restricted, respectively, to single-bank firms and multibank firms.

Treatment and control firms

Overall SMEs in our sample are mature firms with a median age of 19 years and median total assets of about €1300 thousand. The average firm employs around 20 employees, has about €400 thousand in drawn credit with a leverage ratio slightly above 20%. It has 2 bank relationships and the length of its lending relationship with its main lender is around 8 years. Finally it should be noted that less than 1% of the firms in our sample has access to debt on financial markets (no bond financing). They are thus pure bank financed firms. They are, however, very heterogeneous in terms of their use of debt finance as the difference between the average and the median of total credit borrowed by these firms indicates, in all of the tables. Other sources of external finance available for these firms include trade finance and leasing.

Treated firms are significantly different from control firms in that they tend to be a little bit younger, more leveraged, with a shorter maturity of their debt. Their cumulative growth rate in debt with respect to the first semester of 2011 and measured by g(Debt) as defined earlier, is not statistically different than the one of control firms. Similar relationships hold when comparing treated and control firms within the single-bank subsample or within the multibank subsample. The probability of missing a payment on a trade bill (Payment default) is slightly higher in the control group (5+ firms) than the default probability in the treatment group (ACC firms), though this difference does not hold within the single-bank subsample.

Single-bank and multibank firms

We define single-bank firms as firms borrowing from only one bank (banking group) in 2011. N-bank firms borrow from more than N-1 bank and from less or N banks on average in 2011. Within a banking group firms can borrow from several local branches. We define as the main local branch, the branch whose average share of loans to firm i in 2011 is the largest among firm i's local lenders. A single-bank firm can thus also have several local lenders (less than 10% do).

Main bank is the banking group whose average share of drawn credit to firm i is the largest among firm i's bank lenders in 2011. A total of 22 banking groups or standalone banks, as identified by their GEA, appear in our sample in 2011 for single-bank firms. The distribution of these groups' market share of (drawn) corporate credit is very skewed to the left and 9 banks represent 97 % of drawn credit in 2011 in our sample. Figure 2.3 shows that on average, contrary to SMEs borrowing from multiple lenders,

single-bank SMEs experienced a declining trend in their borrowings in 2011. The average amount of outstanding credit granted to single-bank borrowers is downward sloping while the trend is flat for 2-bank firms and positive for multibank firms with more than 2 lenders.

[insert Figure 2.3]

Panel D of table 1 presents descriptive statistics that compare single-bank firms and multibank firms in 2011. More than 40% of our sample is made of single-bank firms that are typically excluded from other research papers using the within firm estimator (e.g. Gan (2007); Schnabl (2012); Andrade et al. (2015)) on multibank firms to disentangle between supply and demand effects.

The sample includes 2,407 single-bank firms and 3,361 firms multibank firms. Single-bank firms are significantly different from multibank firms in almost every observable dimensions but their proportion of ACC firms vs. 5+ firms. Single-bank firms are younger and smaller. These features along with their single-bank characteristics make asymmetries of information especially relevant for them. They default slightly less on payment to their suppliers than their multibank counterparts. They are less levered and experienced a stronger contraction in their use of credit in 2011 than multibank firms, with an average growth rate in bank debt 1.5 percentage point lower than for multibank in 2011 (-5.8% vs. -4.2%) as illustrated in figure 2.4.

Single-bank characteristics as well as the fact they are on a much more negative credit time trend ex-ante suggest that single-bank firms could have been more credit constrained than their multibank counterparts during the 2011 crisis. It could also

reflect that they had a lower demand for credit maybe as a result of a higher beta with the economy, and in this case we should not expect treated single-bank borrowers to react to the ACC reform that creates new incentives to lend to ACC eligible borrowers. This makes single-bank firms sample especially interesting to show evidence of a potential difference in the allocation of credit by banks during the crisis between borrowers with different degree of information asymmetry and loan liquidity.

To understand how banks allocate their lending portfolio in times of crisis we compare the intensity of their response to the one of multibank firms and we explore heterogeneity in the response to the ACC shock within the single-bank sub-sample.

2.5 Empirical results

2.5.1 Average impact of the ACC reform

Figure 2.2 shows the average growth in debt for the whole sample of SMEs, from 2010 to 2014. All three different rating categories clearly show parallel trends in the pre-period, as mentioned earlier, while presenting a positive effect in growth in drawn debt concurrent with the timing of the ACC reform.

[insert Figure 2.2]

In figure 2.7, we plot the average of our measure of growth rate in drawn debt for treated and for control firms, from 2010 to 2014, in the subsample of single-bank firms only. The graph shows that the two groups follow parallel trends prior to the ACC reform. The difference between the green line (5+ firms i.e. control firms) and the blue line (treated firms) widens after March 2012 while it is almost non-existent

in the pre-period. This confirms that 5+ firms are similar to ACC firms in terms of their credit growth prior to the reform.

Figure 2.8 illustrates the same effect of the ACC reform but in the subsample of multibank firms. While treated and control firm still show parallel trend before the ACC reform the effect of the ACC is weaker in the post reform period.

[insert Figure 2.7]

[insert Figure 2.8]

Main empirical results

Table 2.4 presents the results of the Difference-in-differences estimation of the impact of the ACC framework (2012) on firm borrowings for the average firm. We find that the reform reduced the costs for banks to lend to newly eligible borrowers.

Following literature evidence that single and multibank firms have different credit binding constraints in crisis time (Detragiache et al. (2000)), we focus on single-bank and multibank firms separately.

We first look at single-bank firms and find a significant positive effect on newly eligible borrowers in our baseline specification (column 3). The growth debt in debt is 6.6% higher for treated firms than for control firms in the year after the ACC reform, after controlling for firm time invariant characteristics and for time trends in banks' observable and unobservable characteristics. The stability of the coefficient of interest and its significance in all specifications (1) to (3), when we progressively saturate the model with different set of fixed effects, helps address the concern that borrowers also

differed along unobserved dimensions that are driving the effect.

In columns (4) to (6) we turn to multibank firms and find a weakly significant effect of 2.5% when we aggregate both 2-bank and 3-bank groups together. Firms with more than 3 banks show no effect. In column (7), our dependent variable is the growth in debt drawn from the main lender only. The magnitude of the coefficient estimate (0, 020) suggests that the multibank effect is almost entirely driven by the main bank. We can thus rule out that a weak effect for multibank firms is due to firm-level adjustments of their borrowing portfolio across banks.

Finally, we estimate the effect of the ACC reform on the whole sample of firms (column 10) conditional on the number of bank relationships and confirm our previous results. Treated single-bank firms experienced an 6.8% higher credit growth than their control counterpart and the effect is significantly higher than for multibank treated firms.

[insert Table 2.4]

These results support our assumption that single-bank firms were more rationed for bank loans than multibank firms in 2011.

Interestingly the Post*Single-bank coefficient is negative and significant, with a larger magnitude than the effect of the policy itself (close to 10%). It shows that the differential time-trends between single and multi bank firms are largely unaffected by a change in the cost of bank lending, suggesting a deeper difference in how these borrowers are viewed by banks, despite having identical credit ratings. The negative time path of credit also suggests that single-bank firms were constrained and that in

the post ACC period the economic decision faced by the bank is, in general, whether to roll over existing debt, rather than whether to finance new projects.

Table 2.5 estimates equation 2.3.2in a different sample made of micro firms only and not included in our main analysis. It shows that the effect of the ACC policy did not materialize for the smallest firms or so called French TPE.

[insert Table 2.5]

We also verify that the effect on lending is not concentrated in short-term credit only. Table 2.7 shows that both long-term and short-term debts are positively affected by the ACC reform for firms using both short and long-term debt or only financing themselves short-term (though we have a power issue due to the very low number of firms in this category). The effect is much weaker however in the subsample of single-bank firms that do not use short-term debt.

[insert Table 2.7]

The effect of the ACC reform over time

We then estimate the time dynamic of the effect around the event date, taking advantage of our monthly data. We run equation 2.3.2 over an extended time period (2010m1 to 2013m12) and present the results for the coefficient estimates in figure 2.9.

[insert Figure 2.9]

Figure 2.9 shows that the effect of ACC starts to materialize after the event, in March 2012 with the largest intensity from May 2012 to August 2012, before fading out (coefficient estimates are still significantly positive but their magnitude is stable and our growth measure is with respect to the first semester of 2011). Given that the collateralization process is not immediate, a gradual effect over the months following the reform is expected. After August 2012, the combined effect of the VLTRO and of the announcement of Outright Monetary Transactions (OMT) by Governor Draghi[67] contributed to alleviate interbank market tensions resulting in a decline in EURIBOR-OIS spreads . We can reasonably assume that it made central bank funding relatively less attractive at that time and that the cost of funding advantage of the ACC disappears for banks of high enough quality to borrow in the interbank market.

Single bank firms look more credit constrained ex ante

Next we examine whether the growth in debt for treated single-bank firms resulted in new debt (or rolled-over debt) or in debt drawn from existing credit lines. Though undrawn debt is not pledgeable under the ACC there could still be a differentiated effect on undrawn lines for treated and control firms to the extent treated firms might want to draw on their existing lines of credit and refinance with a new loan at a more attractive rate. In 2011 only 29% of single bank firms have undrawn credit lines worth over five percent of their debt stock in 2011, while 50% of multi-bank firms have such lines.

[67] "Conditionally on fiscal adjustments or precautionary programs enforcement by candidate countries, the ECB is allowed to trade in secondary sovereign bond markets with "no ex ante quantitative limits". See Dubecq et al. (2016)for an analysis of the effects of Eurosystem unconventional monetary policy on the euro interbank market liquidity

Table 2.6 shows that the dynamic of credit lines has not changed for single-bank firms around the ACC reform of February 2012 (column 1). The magnitude of the coefficient when we consider total firm debt i.e. drawn as well as undrawn is close to the one of the effect on drawn credit alone. This supports the view that single-bank firms were more credit-constrained ex-ante and that they needed new debt financing or the roll over of loans arrived at maturity and could not use credit lines buffer. On the contrary it seems that most of the ACC effect for multibank firms comes from drawing on existing lines of credit. The effect on undrawn credit lines suggests access to better credit terms for multibank through a revealed preference argument.

[insert Table 2.6]

2.5.2 Bank allocation of lending and the use of information

Effect of the ACC reform conditional on hard information

We then analyze how the impact of the reform varies, within single-firms, with hard and soft information (Petersen (2004)) about the firm. We want to understand how did bank allocate lending among borrowers, whether they use hard information, soft information or both and to what extent, to discriminate among borrowers.

By hard information we mean quantifiable information based on financial disclosures. Using loan applications data for new borrowers, Jiménez et al. (2014) offer evidence that firm balance-sheet strength matters in crisis time to build new lending relationships. We show that it matters in the intensive margin as well by analyzing how response to our positive supply shock is related to firm characteristics that proxy for

financial strength and degree of riskiness.

We rank firms based on leverage, trade credit use, profitability, interest coverage ratio, tangibility of assets and age, in 2011, prior to the reform. For each of these factors, we then create a dummy, which equals one when the firm lies in the top half of the 2011 distribution. For all characteristics but leverage this dummy can be interpreted as signaling relatively lower risk borrowers based on hard information available to the bank. The interpretation will be reversed in the case of leverage. We then run the same OLS regressions as before with an additional interaction term, "Above Median", which captures the intensity of financial strength based alternatively on firm leverage, trade credit, profitability, interest coverage, tangibility of assets and age.
The estimated equation is :

$$g_{ijkt} = \alpha_i + \beta_0 Post_t \times AboveMedian_i + \beta_1 1_{ACC_i} \times Post_t$$

$$+\beta_2 1_{ACC_i} \times Post_t \times AboveMedian_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t} \qquad (2.3)$$

Table 2.8 presents the triple difference estimates of the effect of the ACC reform on lending to firms conditional on our six proxies.

[insert Table 2.8]

Within single-bank firms we find that the effect of the ACC reform is stronger for firm with low leverage, firms who are trade credit net lenders as well as for older firms. We did not find any positive or negative impact of profitability and interest coverage ratio. These findings suggest that only the safest firms among single-bank lenders benefitted from the ACC supply shock. Contrary to evidence for other European

countries (Iyer et al. (2014)), we rule out evergreening or zombie lending to riskiest borrowers.

We next consider a different sample of so called "gazelles" or high-growth firms. These firms play a critical role in job creation (Haltiwanger et al. (2013)), which makes it particularly important from a policy point of view to know whether and to what extent a reduction in banks' cost of funding was channeled to them under the form of more credit availability.

Firms with highest sales growth are selected based on their growth rate in sales, in each of the years 2009, 2010 and 2011. We define gazelles either firms with sales growth is at least equal to 10% (columns 1 and 2 of table 2.9) in each of these three years or firms in the top quartile of the sales growth distribution in each of these three years (columns 3 and 4 of table 2.9). Lending to treated gazelle firms increased by 18% more than for untreated ones. The effect is about three times higher than the ACC effect on credit for the average firm and is even higher within single-bank firms.

[insert Table 2.9]

Finally we construct an indicator of the intensity of the banking relationship between a firm and its lender in 2011, the year prior the ACC reform (February 2012), to identify frims with ex-ante high demand. We measure intensity as the ratio of the number of months the growth rate in a firm credit is greater than 10% (proxy for new loans) reported to the number of months the firm is in the sample (i.e. has positive

bank debt) over the period.[68]We then rank firms based on their intensity of lending in 2011 and identify firms in the top quartile of the distribution as "High Intensity" firms. Our intensity captures a time varying demand effect which is not well controlled for by our firm fixed effect. By selecting firms with ex ante higher, statisfied, demand, we should expect them to keep having intense borowing activity ex post (which will be captured by our Post coefficient). If this is a pure demand effect then firm credit should keep growing at the same path and there is no reason why ACC firms should grow faster than control firms. We find evidence of this in the multibank subsample (columns 4 to 6). For single-bank however the ACC differentially increase debt growth of treated firms by 18%. The effect is not capturing frequent rolling over of short-term debt as the magnitude of the growth in long-tem debt is about the same (column 2). The effect for smallest borrowers (below median of total assets distribution in 2011) which are likely more constrained as they suffer from higher information asymmetry is stronger, which is consistent with our interpretation of ACC relaxing financing constraints for some types of borrowers.

[insert Table 2.10]

Effect of the ACC reform conditional on soft information

In this part we investigate the role of relationship lending and of soft information in the transmission of the ACC supply shocks to single-bank borrowers. Literature findings suggest that the soft information channel should be especially relevant in the

[68]Our results are robust to the choice of different periods of references, with varying lengths, in 2010 and 2011 and to selecting firms in the top half of the intensity distribution - with a weaker magnitude of the effect in this case. Cf. Robustness table 2.16 in Appendix.

context of SMEs whose access to external finance is highly impaired by information asymmetry (Berger and Udell (1995); Petersen and Rajan (1994)). The acquisition of soft information should mitigate information asymmetry and help borrowers' access to credit. By soft information we mean non-measurable, borrower-specific information that is acquired by the lender over time (length of relationship) through repeated interactions with the firm (intensity of the lending relationship) and across a range of different products (scope of relationship).

The early literature on the benefits of relationship banking has shown that longer bank-firm relationships are correlated with increased credit availability (starting with Petersen and Rajan (1994)) and cheaper access to credit (e.g. Berger and Udell (1995)). However, as relationship lenders acquire private information that cannot easily be shared, it also creates a hold-up problem (Rajan (1992)). Most recent literature contributions have looked at the effect of relationship lending over the cyle but evidence are still mixed. Consistent with Rajan's prediction that hold-up problems should increase with any increase in the firm's risk of failure, Santos and Winton (2008) show that, in downturns, relationship banks exploit their market-power over bank-dependent borrowers by raising prices relatively more than for non-dependent borrowers. On the bright side, Bolton et al. (2013) present theoretical and empirical evidence, in Italy, that the information advantage acquired by relationship lenders (identified by low distance from their borrowers' headquarters) allows them to provide "continuation lending" to profitable firms during a crisis.

We use several proxies for the acquisition of soft information. First we consider

the length of the relationship. This is not our preferred measure as it has several drawbacks. It is not informative on how close lenders and borrowers are and how often they interact so that a long relationship could still be informationally poor. In addition our data is censored as we cannot measure the length of the relationship before 1998. Finally, even censoring our maximum observable relationship duration to 14 years, the average single-bank firm in our sample has a relationship length of about 8 years. The vast majority of firms in our sample thus have long bank relationships and length is not a discriminating enough characteristics.

We thus turn to other measures such as the scope of the lending relationship. We define our Scope variable as a dummy, which takes the value one if the firm has other interactions with the bank based on different types of financial services such as leasing, factoring, commercial paper or securitized loans. The results presented in table 2.8 show no significantly higher effect for treated firms with a wider relationship scope or with a longer relationship (we use a 4-year cut-off to follow the literature that commonly used 3 or 4 years to qualify a long relationship, e.g. Bhue et al. (2016)). Though the magnitude of the coefficient is larger for long relationships, coefficients reported in column 2 and 3 are not statistically different.

We do find evidence of a differential effect of ACC however when we combine both dimensions and look at scope and length together.

There is a mechanical correlation between long and wide relationship so that there are very few wide relationships within the short length subsamples. Among long bank relationships however the wider the scope the largest the effect emphasizing the

importance of the richness of the information set acquired on the quality of borrowers (column 5) in loan granting decisions.

Despite bank loans being their main source of external finance firms with a wider relationship scope could have compensated a reduction in bank credit supply during the crisis with other sources of credit like trade credit. Their pure credit financed counterparts did not have this option. Even though wide scope firms were less likely to be as rationed as the narrow scope firms, wider scope firms, with long lending relationship, benefit the most from the ACC supply shock which shows banks value soft information and used it to discriminate across borrowers in their loan allocation process.

The fact that firms with strong lending relationships experienced a larger increase in debt compared to the average firm however may also suggest that relationship lending for single-bank firms was not working as a countercyclical buffer during the crisis and that these firms were somewhat credit rationed.

Finally in table 2.12 we do a horse race between hard and soft information using our preferred measure of soft information (scope of the lending relationship) to test whether they are complement or substitutes. As expected the ACC effect on credit availability does vary with firm characteristics that proxy for quality and risk (columns 1 to 6 of table 2.12) for firms on which bank has a pure credit exposure and thus a less rich set of private information (narrow scope). The positive effect of the ACC is concentrated on low-leverage firms, net trade credit lenders and on all firms but the youngest ones. However bank do not seem to differentiate accross borrowers in the

same manner when they have a wider and more information intensive relationship with the borrower.

Our findings supports the view that, in downturns, when there is a positive supply shock, strong soft information dominates.

2.5.3 The effect of the ACC on payment default

We then look at potential spillover benefits of the ACC, through supply chain linkages, by analyzing the effect of the ACC reform (February 2012) on payment default of firms to their suppliers.

We estimate equation 2.3.2 and our dependent variable is a dummy variable equal to one when the firm has experienced at least one payment incident during the month, and to zero otherwise. We restrict our attention to payment incidents triggered by insolvency issues (liquidation of the firm) or by liquidity shortages leading a firm to, totally or partially, miss a payment to one of its suppliers. Because our sample is composed of high credit quality firms selected based on their credit ratings, payment default are rare events.

[insert Table 2.13]

Panel A and B of Table 2.1 shows that the median firm does not experienced any default to its suppliers and that the probability of a trade payment default is higher for 5+ firms in general (3.1%) than for ACC firms in 2011 (2.7%). Within the sample of single-bank firms however, there is no difference between both categories of firms. The average default rate is 2.5% for single-bank firms. Multibank firms have

a significantly higher rate of trade payment default of 2.9% but they are also bigger firms with more trade relationships.[69]

Table 2.13 shows that the ACC shock reduces the probability that single-bank firms miss a payment to their own suppliers relative to untreated firms. Columns 4 and 5 show that there is no pre-trend as the effect is insignificant in the year prior to the reform. Firms that are eligible for the ACC framework are 0.6% less likely to default on payments to their suppliers in the two years following the shock than firms that are not. This reduced rate of default actually begins to have a detectable effect only six months following the shock, at which point the reduced probability of a default event is over 1 percent relative to ineligible firms.

The results give additional support to our assumption that single-bank firms had ex-ante liquidity constraints during the crisis and that a positive supply shock helped them alleviating it. From a policy perspective these findings matter as the benefits of the ACC supply shock go beyond directly treated firms and spillover to their suppliers. There could also be a multiplier effect for the treated firm as payment defaults have been shown to be negatively and significantly correlated with a firm's access to future loans (Aghion et al. (2012)).

Overall, the finding that the fall in the cost of bank funds causally reduced defaults to suppliers suggests that bank belt tightening may itself induce defaults in borrowers that propagate through their supplier networks in crisis periods, in line with the findings of Boissay and Gropp (2013).

[69]As our payment default dummy is defined on the occurrence of one default and is not scaled by the size of the trading activity of the firms, any comparison between single and multibank firms should be done with a lot of precautions.

2.6 Conclusion

This paper provides cleanly identified micro-evidence on how banks adjust SME lending portfolios in crisis in response to a drop in their cost of funding loans. We find evidence that the cost of funding loans is effective as a policy lever and provide novel evidence of a causal relationship between increased bank lending and reduced payment defaults to suppliers, potentially reducing contagion effects.

We examine how bank responses vary with the extent of the private information advantage they have about the quality of a given borrower. We find that the effect of the ACC supply shock is concentrated on firms for which lenders have an informational monopoly i.e. single-bank borrowers.

Within single-bank firms the effect is stronger among older firms, low-leverage firms, among firms that are net providers of trade credit and among firms with an information intensive banking relationship (a long relationship and a wide scope relationship).

Our findings can be read at two levels. Firstly, when hit by a positive supply shock, banks use the private information acquired during the relationship in conjunction with hard information to allocate the marginal dollar of lending to borrowers. Firm balance sheet strength matters for the transmission of monetary shocks and so do lending relationships. These findings are in line with the literature on the firm balance sheet channel (Jiménez et al. (2014)) as well as the literature on the benefits of relationship lendings (Petersen and Rajan (1995)).

We contribute to the literature by extending these results to the group of single-bank borrowers and by providing novel evidence that for firms with longest and deepest

bank relationships soft information use may be a substitute rather than a complement (Deyoung et al. (2015)) for balance sheet strength.

Secondly relationship banking does not however appear to provide a strong insurance function for single-bank firms in times of crisis as our evidence suggest that they were more credit constrained ex-ante. We compare single-bank and multibank responses to the ACC shock.

While the results cannot be causally interpreted - as having one or several lenders is an endogenous decision, the determinants of which are beyond the scope of this paper – we present a series of evidence consistent with single-bank firms being ex-ante more financially constrained. Firms in our sample are purely bank-financed so there is no substitution effect possible between bank debt and public debt. In this context a higher reaction to the shock suggests that these borrowers were financially constrained ex ante.

Supporting this assumption, we also show that, within single-bank firms, the growth in credit is not fuelled by drawing on existing credit lines and that firms with an ex-ante higher demand (based on our novel intensity indicator as well as on their sale dynamism i.e. "gazelles") experienced a larger effect than the average firm.

Our findings thus shed light on the darker sides of relationship banking and give empirical support to the idea that single-bank borrowers are particularly vulnerable to shocks to their bank, as in the Detragiache et al. (2000) model.

More generally we contribute to the empirical literature on relationship banking by amending the view that, in downturns, closer relationships lead to better credit access

(Bolton et al. (2013)). Having only one bank relationship may be very disadvantageous in crisis periods.

Beyond the pure financing effect the policy has some positive spillovers through the supply chain via lower default rate on trade payment. There could also be a multiplier effect for the treated firms as payment defaults have been shown to be negatively and significantly correlated with a firm's access to future loans (Aghion et al. (2012)). It is unclear though whether policies such as the ACC are welfare enhancing as we have not explored yet promising future research directions looking at the effects of the policy on the other firms in the economy (in particular possible crowding out effects on untreated firms) or assessing the real effects of such policies on investment or employment .

Figure 2.1: Empirical Design

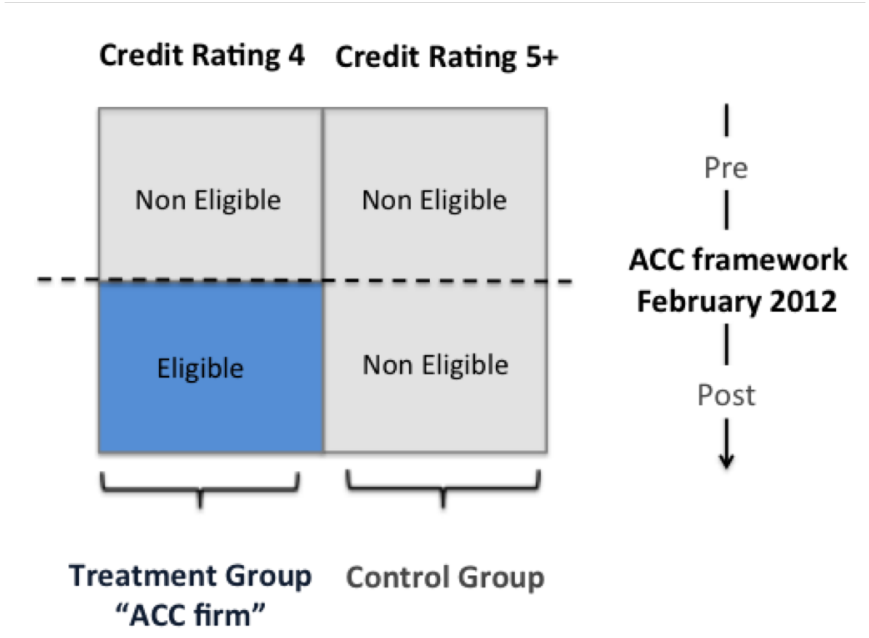


Figure 2.1 illustrates the empirical design for our Intention to Treat estimates. Assignment to treatment and control group is based on firm credit rating in January 2012 i.e. one month before the Additional Credit Claim (ACC).

Figure 2.2: Trends in Credit Growth among treated and control firms

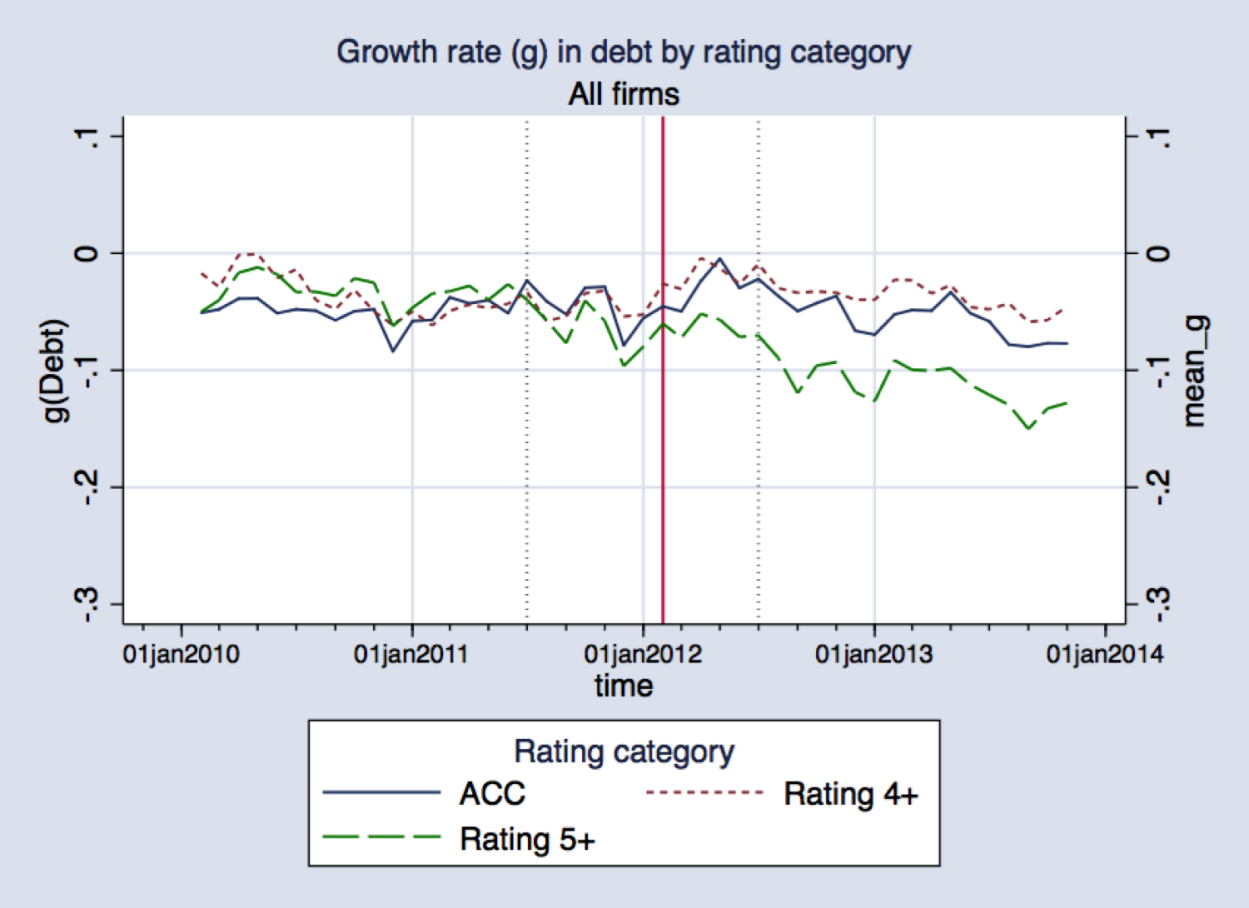


Figure 2.2 shows the average growth rate in debt around the ACC reform (February 2012) in the treated group and in two control groups. Assignment to treatment and control groups is based on firm credit rating in January 2012 i.e. one month before the Additional Credit Claim (ACC). The treated group is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms). The first control group is made of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The second control group is made of 4+ rated firms (closest always eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). For each point in time, we plot the unconditional average of the cumulative growth rate in the outstanding amount of drawn credit $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The vertical line denotes the adoption of the ACC reform (February 2012).

Outstanding amount of credit and Credit growth by number of lending relationships

Figure 2.3: Average outstanding among of drawn debt by number of Lending relationship

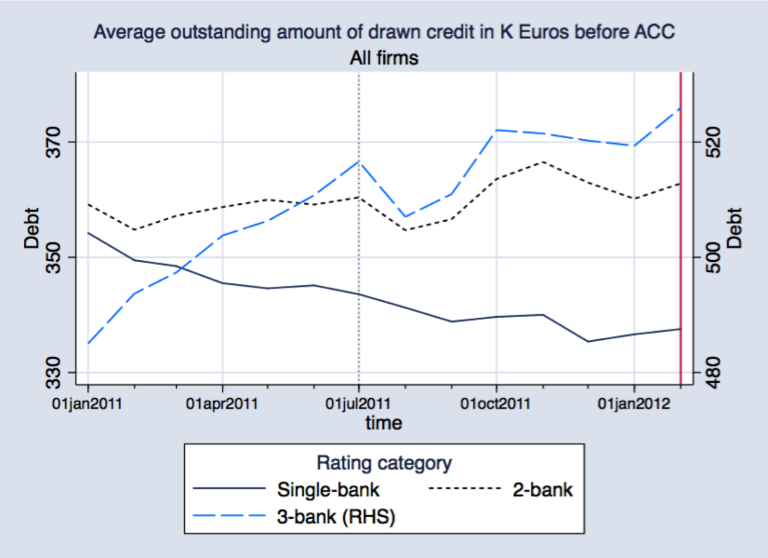


Figure 2.4: Average Credit Growth by number of Lending relationship

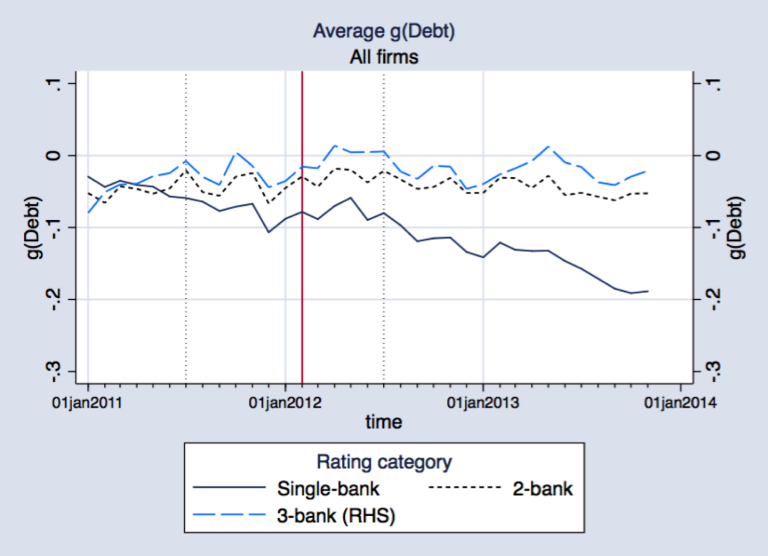


Figure 2.3 shows the average outstanding amount of drawn debt for subsamples of firms based on their number of lending relationships. For each point in time, we plot the unconditional average of the outstanding amount of drawn credit D_{ijkt} reported in the Credit Register. Figure 2.3 shows the average growth rate in debt for subsamples of firms based on their number of lending relationships. For each point in time, we plot the unconditional average of the cumulative growth rate in the outstanding amount of drawn credit $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. Single-bank firms have one lending relationship on average in 2011. 2-bank (3-bank) firms have more than one and less than two (three) lending relationships on average in 2011. The vertical line denotes the adoption of the ACC reform (February 2012).

Probability of a change in credit rating in 2012

Figure 2.5: Rating Downgrade

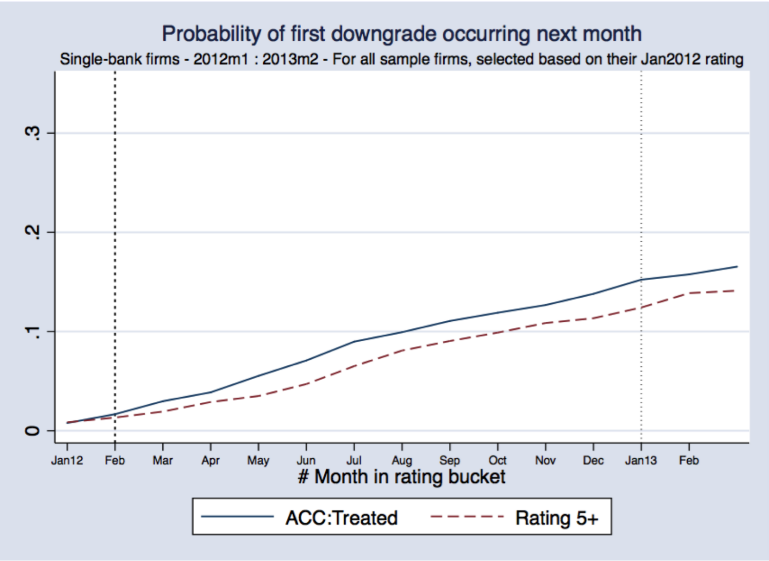


Figure 2.6: Rating upgrade

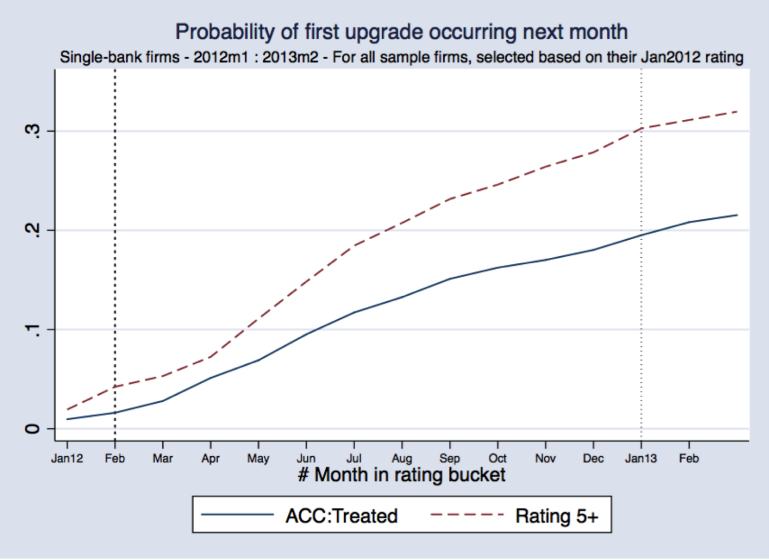


Figure 2.5 shows the percentage of firms that experienced a downgrade of their credit rating in 2012 for treated and control firms. Assignment to treatment and control groups is based on firm credit rating in January 2012 i.e. one month before the Additional Credit Claim (ACC). The treated group is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms). The control group is made of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). Figure 2.5 shows the percentage of firms that experienced an upgrade of their credit rating in 2012 for treated and control firms. After the first occurence of a change in rating (downgrade for figure 2.5 and upgrade for figure 2.5 the firm is removed from the sample. The dotted vertical line denotes the adoption of the ACC reform (February 2012).

Trends in Credit Growth among treated and control firms, by number of Lending relationships

Figure 2.7: Trends in Credit Growth among treated and control single-bank firms

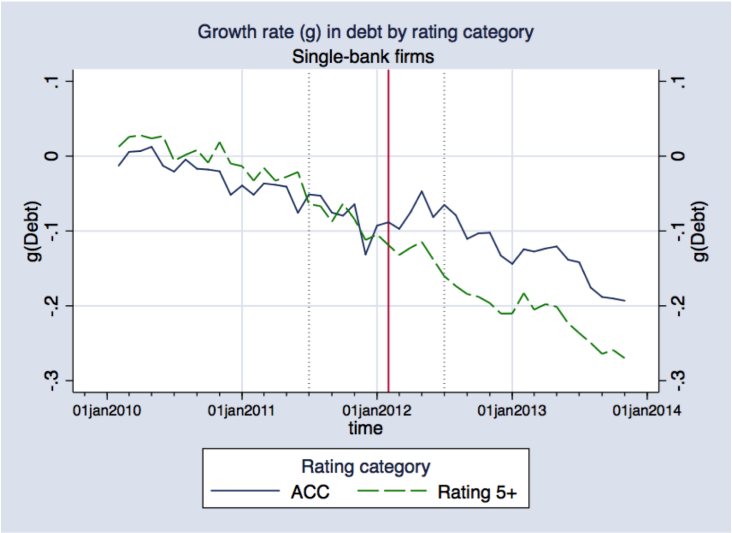


Figure 2.8: Trends in Credit Growth among treated and control multibank firms

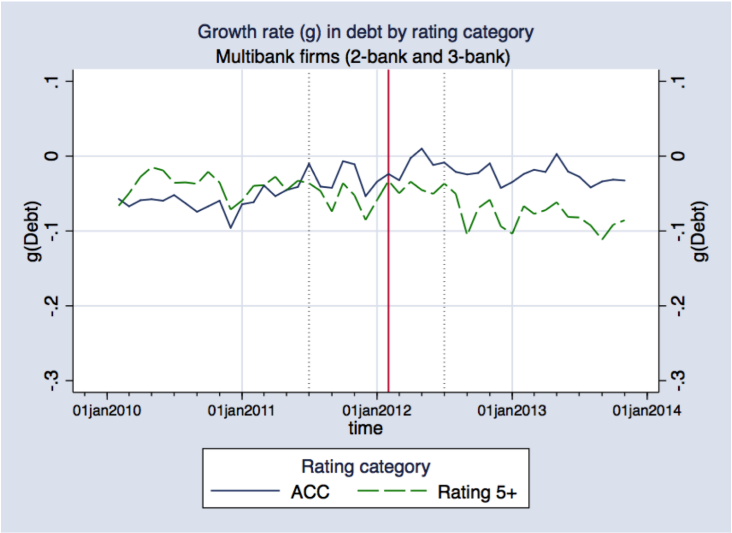


Figure 2.7 shows the average growth rate in debt around the ACC reform (February 2012) for treated and control single-bank firms. Figure 2.7 shows the average growth rate in debt around the ACC reform (February 2012) for treated and control multibank firms. Single-bank firms have one lending relationship on average in 2011. Multibank firms have more than one and less than three lending relationships on average in 2011. Assignment to treatment and control groups is based on firm credit rating in January 2012 i.e. one month before the Additional Credit Claim (ACC). The treated group is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms). The control group is made of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). For each point in time, we plot the unconditional average of the cumulative growth rate in the outstanding amount of drawn credit $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The vertical line denotes the adoption of the ACC reform (February 2012).

Figure 2.9: Monthly Dynamic of the Effect of the ACC reform (February 2012) on Lending to single-bank SMEs

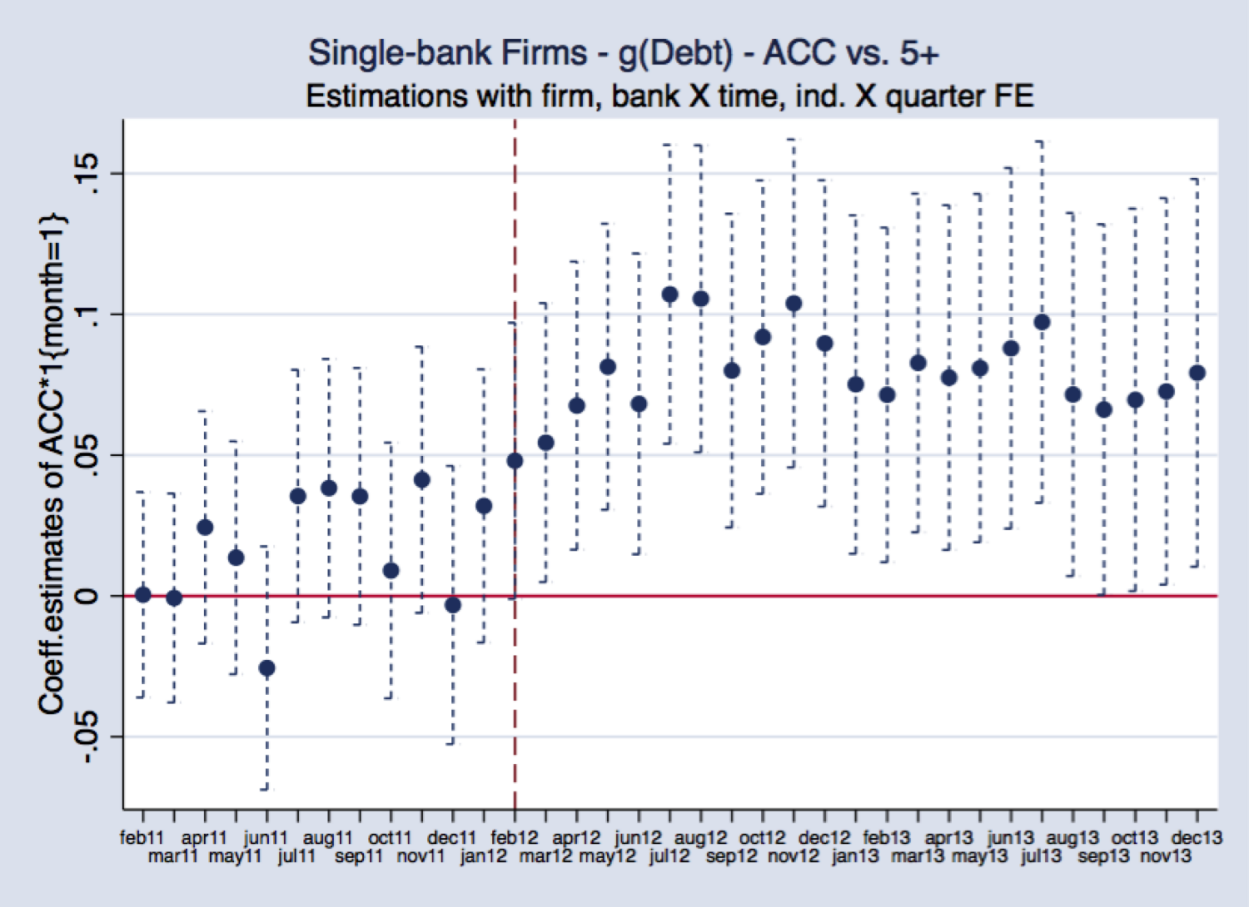


Figure 2.9 shows the evolution of lending to single-bank firms around the ACC reform date. The specification is the same as equation 2.3.2 except that it is estimated over 2010-2013 and the $1_{ACC_i} \times Post$ variable is replaced by a collection of variables $1_{ACC_1} \times \sum_{t>jan2011} 1_t$ where 1_t is a monthly dummy. We plot the point estimates from February 2011 (12 months prior the ACC reform) to December 2013. The dashed lines plot the 95% confidence interval and robust standard errors are clustered at the firm level.

Table 2.1: Summary Statistics

Panel A: Firm-level statistics (2011) – All firms

This table presents firm-level statistics for all firms. Firms rated 4 or ACC firms are the treated group (4,622 firms). Firms rated 5+ are the control group (2,441 firms).

	ACC firms				5+ firms				Stat.Diff.
	Obs.	Mean	Median	Std. dev.	Obs.	Mean	Median	Std. dev.	p-val (clust)
Age	53495	22.43	19	15.39	28588	19.5	19.5	19.5	0.000
Total Assets	53495	2664	1367	11438	28588	2691	1386	17131	0.945
Nb. Employ.	53495	21.58	16	18.79	28588	21.21	16	18.74	0.440
Bank debt	53495	435	189	911	28588	565	258	1024	0.000
Leverage	53495	0.19	0.15	0.15	28588	0.26	0.21	0.19	0.000
Short-term debt / Total debt	53495	0.33	0.08	0.39	28588	0.37	0.2	0.39	0.000
g(Debt)	53495	-0.045	-0.022	0.417	28588	-0.048	-0.022	0.356	0.474
Nb. of bank relationships	53495	2.03	2	1.18	28588	2.16	2	1.33	0.000
Share of main lender (banking group)	53495	0.823	0.925	0.211	28588	0.832	0.934	0.203	0.064
Length of main bank relationship (in years)	53495	8.155	7.417	4.127	28588	7.802	6.75	3.993	0.001
Payment default dummy	53495	0.027	0.000	0.162	28588	0.031	0.000	0.172	0.059

Panel B: Firm-level statistics (2011) – Comparison between single-bank and multibank firms

This table presents summary statistics in 2011 for single-bank firms (2,407) and multibank firms (3,361) firms.

	Single-bank firms				Multibank firms				Stat.Diff.
	Obs.	Mean	Median	Std. dev.	Obs.	Mean	Median	Std. dev.	p-val (clust)
Age	27924	19.21	17	13.89	41325	22.12	19	15.25	0.000
Total Assets	27924	1858	1166	4991	41325	2525	1311	10728	0.001
Nb. Employ.	27924	16.43	13	12.67	41325	21.4	17	17.88	0.000
Bank debt	27924	413	154	738	41325	426	206	919	0.541
Leverage	27924	0.19	0.152	0.34	41325	0.2	0.17	0.15	0.000
Short-term debt / Total debt	27924	0.22	0.16	0.19	41325	0.39	0.27	0.39	0.002
g(Debt)	27924	-0.058	-0.03	0.38	41325	-0.042	-0.019	0.413	0.000
ACC	27924	0.662	1	0.473	41325	0.667	1	0.471	0.000
Share of main lender (banking group)	27924				41325	0.78	0.82	0.18	0.704
Length of main bank relationship (in years)	27924	7.83	6.58	3.95	41325	8.04	7.25	4.15	0.047
Payment default dummy	27924	0.025	0.000	0.155	41325	0.029	0.000	0.169	0.013

Table 2.2: Summary Statistics for treated and control groups

Panel C: Firm-level statistics (2011) – Single-bank firms

This table presents summary statistics in 2011, for single-bank firms. Firms rated 4 or ACC firms are the treated group (1,604 firms). Firms rated 5+ are the control group (803 firms).

	ACC firms				5+ firms				Stat.Diff.
	Obs.	Mean	Median	Std. dev.	Obs.	Mean	Median	Std. dev.	p-val (clust)
Age	18480	20.5	18	13.87	9444	16.69	13	13.56	0.000
Total Assets	18480	1776	1108	5396	9444	2019	1332	4079	0.223
Nb. Employ.	18480	16.82	14	12.71	9444	15.67	13	12.57	0.036
Bank debt	18480	311	128	560	9444	612	236	967	0.000
Leverage	18480	0.18	0.13	0.16	9444	0.29	0.22	0.22	0.000
Short-term debt / Total debt	18480	0.19	0.00	0.34	9444	0.18	0.00	0.33	0.487
g(Debt)	18480	-0.061	-0.033	0.413	9444	-0.052	-0.027	0.305	0.162
Length of main bank relationship (in years)	18480	8	6.92	4.01	9444	7.49	6.25	3.8	0.001
Payment default dummy	18480	0.024	0.000	0.154	9444	0.025	0.000	0.157	0.779

Panel D: Firm-level statistics (2011) – Multibank firms

This table presents summary statistics in 2011, for multibank firms with 2-bank or 3-bank in 2011. Firms rated 4 or ACC firms are the treated group (2,379 firms). Firms rated 5+ are the control group (1,182 firms).

	ACC firms				5+ firms				Stat.Diff.
	Obs.	Mean	Median	Std. dev.	Obs.	Mean	Median	Std. dev.	p-val (clust)
Age	27574	22.84	20	15.38	13751	20.68	18	14.89	0.000
Total Assets	27574	2675	1332	12580	13751	2224	1239	5327	0.140
Nb. Employ.	27574	21.54	17	17.97	13751	21.12	17	17.68	0.517
Bank debt	27574	404	195	896	13751	469	229	961	0.056
Leverage	27574	0.18	0.15	0.14	13751	0.24	0.2	0.17	0.000
Short-term debt / Total debt	27574	0.37	0.24	0.39	13751	0.42	0.34	0.39	0.002
g(Debt)	27574	-0.039	-0.018	0.427	13751	-0.048	-0.02	0.384	0.161
Nb. of bank relationships	27574	2.12	2	0.65	13751	2.11	2	0.64	0.536
Share of main lender (banking group)	27574	0.78	0.81	0.19	13751	0.8	0.83	0.18	0.014
Length of main bank relationship (in years)	27574	8.11	7.25	4.18	13751	7.9	7.08	4.07	0.159
Payment default dummy	27574	0.028	0.000	0.168	13751	0.031	0.000	0.175	0.161

Table 2.3: Paralell Trend Tests

ACC vs. 5+ firms (one notch lower)

This table presents estimates of a differential time trend effect for the treated firms (ACC) in the period prior to the ACC reform. We estimate the following equation :
$g_{ijkt} = \alpha_i + \beta 1_{ACC_i} \times t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$
i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. The sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in the outstanding amount of drawn credit, $g(Debt_{ijkt})$ defined as $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Single-bank		**2-bank**	**3-bank**	**Multibank**
	2010m3-2012m2	2011m3-2012m2	2011m3-2012m2	2011m3-2012m2	2011m3-2012m2
Dep. var:g(Debt)$_{ikt}$					
ACC * t	0.0015	0.0025	0.0036	0.0013	0.0027
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
N	53990	28112	26151	15559	41710
R-squared	0.2461	0.3751	0.3441	0.3526	0.3453
Firm FE	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes
N.Cluster	2405	2394	2231	1322	3553

Table 2.4: Effect of the ACC reform (February 2012) on Lending to SMEs

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect (Intention-to-Treat) of the ACC reform (2012) on the growth in outstanding credit amount to SMEs. We follow a DID strategy and estimate the following equation :
$g_{ijkt} = \alpha_i + \beta 1_{ACC_i} \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. The sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in the outstanding amount of drawn credit, $g(Debt_{ijkt})$ defined as $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. Post is a post-treatment dummy, which is equal to 1 in each month after February 2012. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	1-BANK Firm,Time FE	- BankxTime	- IndxQuarter	**2-BANK**	**3-BANK**	**2-3 BANK**	**2-3 BANK** g(Main)	**4 BANK +**	**Conditional on N Lending Rel.**
Dep. var:$g(Debt)_{ikt}$									
$ACC*1_{\{t>=March2012\}}$	0.070***	0.069***	0.066***	0.029	0.020	0.025*	0.020	-0.007	0.022**
	(0.016)	(0.016)	(0.016)	(0.018)	(0.024)	(0.014)	(0.014)	(0.024)	(0.010)
$ACC*1_{\{t>=March2012\}}$*SingleBank									0.046**
									(0.019)
$1_{\{t>=March2012\}}$*SingleBank									-0.099***
									(0.013)
N	58384	58384	58384	54335	32347	86682	86089	26811	171877
R-squared	0.461	0.467	0.469	0.445	0.468	0.451	0.446	0.456	0.452
Time FE	yes								
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Bank X Time FE		yes	yes	yes	yes	yes	yes	yes	yes
Ind X QuarterYY FE			yes	yes	yes	yes	yes	yes	yes
N.Cluster	2407	2407	2407	2240	1327	3567	3558	1095	7069

Table 2.5: Effect of the ACC reform (February 2012) on Lending to Micro firms

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in outstanding credit amount for a sample of micro firms. Under the French Law of Modernization of the Economy (2008) micro-firms have less than 10 employees and sales and total assets not exceeding 2 million EUR. We follow a DID strategy and estimate the following equation :

$g_{ijkt} = \alpha_i + \beta 1_{ACC_i} \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. The sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in the outstanding amount of drawn credit, $g(Debt_{ijkt})$ defined as $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. Post is a post-treatment dummy, which is equal to 1 in each month after February 2012. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
	1-BANK	**2 and 3-BANK**	g(main)
Dep. var:g(Debt)$_{ikt}$			
ACC*1$_{\{t>=March2012\}}$	-0.011	0.026	0.016
	(0.016)	(0.026)	(0.026)
ACC eligible firm			
N	38987	22526	22423
R-squared	0.471	0.474	0.478
Firm FE	yes	yes	yes
Bank X Time FE	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes
N.Cluster	1599	923	921

Table 2.6: Effect of the ACC reform (February 2012) on Undrawn Lines of Credit

Effect on g(Undrawn Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in undrawn debt to SMEs. We follow a DID strategy and estimate the following equation :

$g_{ijkt} = \alpha_i + \beta 1_{ACC_i} \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. The sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in the outstanding amount of undrawn credit in columns 1 to 4. The dependent variable is the cumulative growth rate in the sum of drawn + undrawn credit in columns 5 to 8. $g(Debt_{ijkt})$ is defined as $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. Post is a post-treatment dummy, which is equal to 1 in each month after February 2012. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Single-bank	**2-bank**	**3-bank**	**Multibank**	**Single-bank**	**2-bank**	**3-bank**	**Multibank**
Dep. var:g(Debt)$_{ikt}$	g(Undrawn)	g(Undrawn)	g(Undrawn)	g(Undrawn)	g(All)	g(All)	g(All)	g(All)
ACC*1$_{\{t>=March2012\}}$	-0.007	-0.034*	-0.032	-0.033**	0.059***	0.010	0.012	0.011
	(0.012)	(0.018)	(0.027)	(0.015)	(0.015)	(0.017)	(0.022)	(0.013)
ACC eligible firm								
N	58384	54335	32347	86682	58384	54335	32347	86682
R-squared	0.270	0.294	0.302	0.293	0.476	0.473	0.486	0.475
Time FE								
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes	yes	yes	yes
N.Cluster	2407	2240	1327	3567	2407	2240	1327	3567

Table 2.7: Effect of the ACC reform (February 2012) on single-bank firms conditional on debt maturity

Effect on g(Medium and Long-term Debt) and g(Short-term Debt)
ACC vs. 5+ firms (one notch lower) - 2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in short and long-term debt to SMEs. We split our main sample into three subsamples based on firm financing maturity. The first subsample is made of We follow a DID strategy and estimate the following equation :

$g_{ijkt} = \alpha_i + \beta 1_{ACC_i} \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. The sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in the outstanding amount of undrawn credit in columns 1 to 4. The dependent variable is the cumulative growth rate in the sum of drawn + undrawn credit in columns 5 to 8. $g(Debt_{ijkt})$ is defined as $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. Post is a post-treatment dummy, which is equal to 1 in each month after February 2012. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	ST Debt$_{2011}$=0 MLT Debt$_{2011}$>0	**ST Debt$_{2011}$>0** MLT Debt$_{2011}$ >0			**ST Debt$_{2011}$>0** MLT Debt$_{2011}$ =0
Dep. var:g(Debt)$_{ikt}$	g(debt)	g(debt)	g(ST debt)	g(MLT debt)	g(debt)
ACC*1$_{\{t>=March2012\}}$	0.029*	0.110***	0.093**	0.113***	0.116
	(0.018)	(0.027)	(0.042)	(0.032)	(0.082)
N	30121	21457	16491	20699	4514
R-squared	0.602	0.452	0.383	0.620	0.367
Firm FE	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes
N.Cluster	1274	907	893	906	226

Table 2.8: Effect of the ACC reform (February 2012) on single-bank firms conditional on hard information

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to single-bank SMEs. We follow a DIDID strategy and estimate the following equation :

$$g_{ijkt} = \alpha_i + \beta_0 Post_t \times AboveMedian_i + \beta_1 1_{ACC_i} \times Post_t + \beta_2 1_{ACC_i} \times Post_t \times AboveMedian_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit, $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. $AboveMedian_i$ is a dummy equal to 1 for firms in the top half of the 2011 distribution of leverage (1), (Account Receivables - Debt Payables)/Assets (2), Ebitda/Total Assets (3), Ebitda/Interest Expenses (4), Age (5), Tangible CapEx/Total Assets (6). Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Leverage	**(Receivables - Payables) /TA**	**Profitability**	**Interest Coverage**	**Age**	**Collateral (Tangible CapEx)**
Dep. var:g(Debt)$_{ikt}$						
ACC*1$_{\{t>=March2012\}}$	0.108***	0.033*	0.061***	0.053***	0.040**	0.075***
	(0.031)	(0.019)	(0.021)	(0.018)	(0.020)	(0.021)
ACC*1$_{\{t>=March2012\}}$*Above p	-0.102***	0.067**	0.010	0.029	0.053*	-0.026
	(0.034)	(0.033)	(0.031)	(0.035)	(0.032)	(0.032)
1$_{\{t>=March2012\}}$*Above p50	-0.018	-0.008	-0.011	-0.017	-0.007	0.058**
	(0.028)	(0.026)	(0.023)	(0.028)	(0.025)	(0.024)
N	56093	54661	56093	56093	56093	56093
R-squared	0.467	0.471	0.464	0.464	0.465	0.465
Firm FE	yes	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes	yes
N.Cluster	2407	2387	2407	2407	2407	2407

Table 2.9: Effect of the ACC reform (February 2012) on "Gazelles"

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

Gazelle firms are firms with highest sale growth over the 3 years preceeding the ACC reform. This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to high growth firms or "gazelles". We follow a DIDID strategy and estimate the following equation :
$g_{ijkt} = \alpha_i + \beta_0 Post_t \times HighGrowth_i + \beta_1 1_{ACC_i} \times Post_t + \beta_2 1_{ACC_i} \times Post_t \times HighGrowth_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit, $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. Firms with highest sales growth are selected based on their growth rate in sales, in each of the years 2009, 2010 and 2011. $HighGrowth_i$ is a dummy equal to 1 when firm sales growth is at least equal to 10% (columns 1 and 2) or in the top quartile of the sales growth distribution in each of these three years (column 3 and 4). Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **Sales Growth >10%** All	(2) **Sales Growth >10%** Single-bank	(3) **Top 25% growing** All	(4) **Top 25% growing** Single-bank
Dep. var:g(Debt)$_{ikt}$				
ACC*$1_{\{t>=March2012\}}$	0.030***	0.073***	0.030***	0.074***
	(0.010)	(0.017)	(0.010)	(0.017)
ACC*$1_{\{t>=March2012\}}$*HighGrowth	0.149**	0.270*	0.160**	0.183
	(0.063)	(0.152)	(0.067)	(0.159)
$1_{\{t>=March2012\}}$*HighGrowth	0.011	-0.035	-0.024	-0.037
	(0.050)	(0.135)	(0.058)	(0.149)
N	149067	50364	149067	50364
R-squared	0.450	0.468	0.450	0.468
Firm FE	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes
N.Cluster	6388	2165	6388	2165

Table 2.10: Effect of the ACC reform (February 2012) on single-bank firms conditional on intensity of credit demand

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to single-bank SMEs . We follow a DIDID strategy and estimate the following equation :

$$g_{ijkt} = \alpha_i + \beta_0 Post_t \times HighIntensity_i + \beta_1 1_{ACC_i} \times Post_t + \beta_2 1_{ACC_i} \times Post_t \times HighIntensity_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit, $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. We measure credit demand intensity in 2011 as the number of month in which debt grows by more than 10% reported to the number of months of presence (positive debt) of the firm in 2011: $Intensity_i = N_{g(Debt_{it})>0.1}/N_{D_{it}>0}$. Our dummy$HighIntensity_i$ takes the value of 1 for firms lying in the top quartile of the intensity indicator distribution. Smallest firms lie in the bottom half of the 2011 distribution of total assets. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Single-bank firms $1_{\{Intensity>=p75\}}$			**Multibank firms** $1_{\{Intensity>=p75\}}$		
Dep. var: $g(Debt)_{ikt}$	g(debt)	g(MLT debt)	Smallest firms g(debt)	g(debt)	g(MLT debt)	Smallest firms g(debt)
$ACC*1_{\{t>=March2012\}}$	0.024	0.031*	0.016	0.016	0.044**	0.045
	(0.015)	(0.016)	(0.043)	(0.014)	(0.019)	(0.031)
$ACC*1_{\{t>=March2012\}}*1_{\{Intensity\}}$	0.186***	0.188***	0.279***	-0.031	0.008	0.013
	(0.052)	(0.070)	(0.093)	(0.036)	(0.060)	(0.071)
$1_{\{t>=March2012\}}*1_{\{Intensity\}}$	0.015	-0.080	-0.015	0.358***	0.461***	0.338***
	(0.042)	(0.056)	(0.071)	(0.030)	(0.051)	(0.055)
N	56093	51883	14047	83257	74930	20821
R-squared	0.469	0.661	0.428	0.462	0.677	0.427
Firm FE	yes	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes	yes
N.Cluster	2407	2220	613	3566	3255	903

Table 2.11: Effect of the ACC reform (February 2012) on single-bank firms conditional on soft information : Length and Scope of lending relationships

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to single-bank SMEs . We follow a DIDID strategy and estimate the following equation :
$g_{ijkt} = \alpha_i + \beta_0 Post_t \times WideScope_i + \beta_1 1_{ACC_i} \times Post_t \times NarrowScope_i + \beta_2 1_{ACC_i} \times Post_t \times WideScope_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$
i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit, $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. LR stands for "Length of Relationship" and measures the duration of the relationship between a firm and its bank. $NarrowScope_i$ is a dummy equal to 1 for firms on which banks have an exposure limited to credit lending. $WideScope_i$ is a dummy equal to 1 for firms on which banks have additional exposure through leasing, factoring, commercial papers or securitization. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) **Scope of LR** -	(2) **Length of LR** -	(3) **Length of LR** 4y or less	(4) **Length of LR** More than 4y	(5) **Length and Scope** 4y or less	(6) **Length and Scope** More than 4y
Dep. var:g(Debt)$_{ikt}$						
ACC*1$_{\{t>=March2012\}}$	0.056***	0.053	0.041	0.070***	0.077**	0.050***
	(0.016)	(0.036)	(0.038)	(0.017)	(0.035)	(0.019)
ACC*1$_{\{t>=March2012\}}$*WideScope	0.053				-0.175	0.098**
	(0.046)				(0.127)	(0.048)
ACC*1$_{\{t>=March2012\}}$*Length>4y		0.016				
		(0.040)				
1$_{\{t>=March2012\}}$*WideScope	0.008				0.226**	-0.032
	(0.036)				(0.111)	(0.035)
N	56093	56093	11295	44797	11295	44797
R-squared	0.465	0.465	0.481	0.465	0.483	0.465
Firm FE	yes	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes	yes
N.Cluster	2407	2407	486	1921	486	1921

Table 2.12: Effect of the ACC reform (February 2012) on single-bank firms conditional on soft and hard information

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to single-bank SMEs . We follow a DIDID strategy and estimate the following equation :

$g_{ijkt} = \alpha_i + \beta_0 Post_t \times AboveP_i + \beta_1 1_{ACC_i} \times Post_t + \beta_2 1_{ACC_i} \times Post_t \times AboveP_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit, $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk2011s1})}{0.5*(D_{ijkt} + \overline{D}_{ijk2011s1})}$. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. $AboveP_i$ takes the value of 1 for firms lying in the top half of the distribution of the variable in the head of the column but for Age where $AboveP_i$ = 1if the firm is above the first quarter of the age distribution. Columns 7 to 12 estimate the model for a subsample of firms on which banks have an exposure limited to credit lending (Narrow Scope). Columns 1 to 6 estimate the model for the subsample of firms on which banks have additional exposure through leasing, factoring, commercial papers or securitization (Wide Scope). Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Wide scope					**Narrow scope**				
		Lev	TradeC	Age	Profit		Lev	TradeC	Age	Profit
Dep. var:g(Debt)$_{ikt}$										
ACC*$1_{\{t>=March2012\}}$	0.105**	0.109	0.071	0.099	0.084	0.058***	0.086***	0.030	0.012	0.058***
	(0.043)	(0.081)	(0.063)	(0.088)	(0.061)	(0.016)	(0.032)	(0.020)	(0.024)	(0.017)
ACC*$1_{\{t>=March2012\}}$*Above p		-0.033	0.046	0.007	0.037		-0.081**	0.058*	0.066**	0.000
		(0.091)	(0.087)	(0.102)	(0.086)		(0.035)	(0.035)	(0.032)	(0.046)
$1_{\{t>=March2012\}}$*Above p		-0.101	-0.005	0.001	-0.020		-0.043	-0.001	-0.014	-0.011
		(0.074)	(0.066)	(0.081)	(0.065)		(0.029)	(0.028)	(0.024)	(0.036)
N	11440	11440	11440	11440	11440	44653	44653	44653	44653	44653
R-squared	0.420	0.422	0.420	0.420	0.420	0.486	0.489	0.487	0.487	0.486
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
N.Cluster	513	513	513	513	513	1894	1894	1894	1894	1894

Table 2.13: Effect of the ACC reform (February 2012) on Payment default

ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on payment defaults of single-bank SMEs to one of their trade suppliers. We follow a DID strategy and estimate the following equation :
$Default_{ijkt} = \alpha_i + \beta 1_{ACC_i} \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is a dummy variable equal to one if firm miss a payment to one of its suppliers because of liquidity or solvency issues. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	All firms	**Low Default Industry**	**High Default Industry**	**--- Dynamic**	**Time Trend 2011m3:2012m2**
Dep. var: Payment Default Dummy$_{ikt}$					
ACC*1$_{\{t>=March2012\}}$	-0.0062**	0.0003	-0.0074**		
	(0.0030)	(0.0018)	(0.0036)		
ACC*1$_{\{Sep11<=t<=Feb12\}}$				-0.0057	
				(0.0046)	
ACC*1$_{\{Mar12<=t<=Aug12\}}$				-0.0052	
				(0.0048)	
ACC*1$_{\{Sep<=t<=Feb13\}}$				-0.0113**	
				(0.0052)	
ACC*1$_{\{Mar13 <= t\}}$				-0.0128**	
				(0.0053)	
ACC * t					-0.0004
					(0.0003)
N	82275	4152	70450	70450	52743
R-squared	0.1929	0.0875	0.1922	0.1922	0.2207
Firm FE	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes
N.Cluster	2461	126	2105	2105	2461

Appendices to Chapter Two

Table 2.14: Composition of collateral pledged with the Banque de France in 2011 and in the first half of 2012

Amounts are calculated after haircuts, in EUR bn.

	Total	**Mean**	**Std.**	**p50**
2011 (54 MFIs *)				
Marketable securities	199.4	3.70	8.7	0.2
Non marketable	63.7	1.	6.5	0.0
Credit Claims	149.7	2.8	7.5	0.0
Total	412.8	7.6	15.4	0.8
2012s1 (59 MFIs)				
Marketable securities	168.8	2.9	6.3	0.3
Non marketable	13	0.2	0.6	0.0
Credit Claims	152.9	2.6	7.2	0.0
ACC	45	0.8	3.4	0.0
Total	379.7	6.4	14.3	1.0

Source : ECB, Bignon et al. (2016)
(*) Monetary and Financial Institutions

Table 2.15: Robustness : Different definitions of growth in credit

Effect of the ACC reform (February 2012) on single-bank firms

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to single-bank SMEs . We follow a DID strategy and estimate the following equation :
$g_{ijkt} = \alpha_i + \beta ACC_i \times Post_t + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$

i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit. We use $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk})}{0.5*(D_{ijkt} + \overline{D}_{ijk})}$ in columns 1 and 2. $\overline{D}_{ijk}$is the firm average outstanding amount of debt in the second semester of 2011 in column 1. $\overline{D}_{ijk}$is the firm average outstanding amount of debt in 2011 in column 2. Column 3 and 4 use a cumulative growth definition of g, with the base peirod being the fist semester of 201 in column 3 and the whole year 2011 in column 4. Finally the dependendent variable in column 5 is leverage. The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
	g2011s2	g2011	$(D_{ikt}-D_{2011s1})/D_{2011s1}$	$(D_{ikt}-D_{2011})/D_{2011}$	Leverage D_{ikt}/TA_{2011}
Dep. var:g(Debt)$_{ikt}$					
ACC*$1_{\{t>=March2012\}}$	0.063***	0.066***	0.136***	0.110***	0.016***
	(0.016)	(0.016)	(0.024)	(0.021)	(0.003)
ACC eligible firm					
N	58213	58384	58291	58346	57924
R-squared	0.301	0.328	0.557	0.379	0.941
Firm FE	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes
N.Cluster	2384	2407	2395	2402	2407

Table 2.16: Robustness : Intensity of lending relationship

Effect of the ACC reform (February 2012) on single-bank firms conditional on soft information

Effect on g(Debt) - ACC vs. 5+ firms (one notch lower)
2011m3- 2013m2

This table presents DID estimates of the effect of the ACC reform (2012) on the growth in debt to single-bank SMEs . We follow a DIDID strategy and estimate the following equation :
$g_{ijkt} = \alpha_i + \beta_0 Post_t + \beta_1 ACC_i \times Post_t + \beta_2 ACC_i \times Post_t \times HighIntensity_i + Bank_{kt} + Ind_{jT} + \epsilon_{i,s,t}$
i indexes firm, j indexes industry, k indexes bank (or main lender for multibank firms), t denotes time in months and T denotes Quarters. $Bank_{kt}$ is a (main) bank x month fixed effects. Within single-bank firms the sample is made of 4-rated firms (newly eligible borrowers or "ACC firms" i.e. treated firms) and of 5+ rated firms (closest non eligible borrowers on the internal Credit Risk Rating scale of the Banque de France). The dependent variable is the cumulative growth rate in drawn credit. We use $g_{ijkt} = \frac{(D_{ijkt} - \overline{D}_{ijk})}{0.5*(D_{ijkt} + \overline{D}_{ijk})}$ where $\overline{D}_{ijk}$is the firm average outstanding amount of debt either in the first semester of 2011 (columns 1 to 3), in the whole year 2011 (column 4) or in the second semester of 2011 (column 5). The 1_{ACC_i}post-election dummy takes a value of one for any firm with a rating of 4 as of January 2012 and zero otherwise. $Post_t$ is a post-treatment dummy, which is equal to 1 in each month after February 2012. We measure intensity in 2011 as the number of month in which debt grows by more than 10% reported to the number of months of presence (positive debt) of the firm in 2011: $Intensity_i = N_{g(Debt_{it})>0.1}/N_{D_{it}>0}$. Our dummy$HighIntensity_i$ takes the value of 1 for firms lying in the top half of the intensity indicator distribution in column 1 and in the top quartile in other columns. Robust standard errors are clustered by firm following Petersen (2009). Associated t-statistics are reported in brackets. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1) Intensity **g(debt)**	(2) Period **g(debt)**	(3) Period **g(debt)**	(4) g definition **$g(debt)_{2011}$**[(a)]	(5) g definition **$g(debt)_{2011s2}$**[(b)]
	Intensity>=p50	T=2010	T=2010-2011m6	T=2011	T=2011
Dep. var:$g(Debt)_{ikt}$					
$ACC*1_{\{t>=March2012\}}$	0.029*	0.033**	0.009	0.036**	0.036**
	(0.017)	(0.016)	(0.014)	(0.016)	(0.016)
$ACC*1_{\{t>=March2012\}}*1_{\{Intensity>=p\}}$	0.070**	0.163***	0.148***	0.144***	0.133***
	(0.033)	(0.047)	(0.048)	(0.050)	(0.050)
$1_{\{t>=March2012\}}*1_{\{Intensity>=p\}}$	0.010	-0.113***	0.218***	-0.013	-0.021
	(0.025)	(0.035)	(0.040)	(0.038)	(0.037)
N	56093	56093	56093	56093	55929
R-squared	0.465	0.465	0.483	0.320	0.293
Firm FE	yes	yes	yes	yes	yes
Bank X Time FE	yes	yes	yes	yes	yes
Ind X QuarterYY FE	yes	yes	yes	yes	yes
N.Cluster	2407	2407	2407	2407	2384

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