Essays in Empirical Corporate Finance

Adrien Matray

HEC PARIS
Apprendre à oser®

GREGHEC
Laboratoire CNRS - UMR 2959

ECOLE DES HAUTES ETUDES COMMERCIALES DE PARIS

Ecole Doctorale « Sciences du Management/GODI » - ED 533
Gestion Organisation Décision Information

« Essays in Empirical Corporate Finance »

THESE
présentée et soutenue publiquement le 3 octobre 2014
en vue de l'obtention du

DOCTORAT EN SCIENCES DE GESTION

Par

Adrien MATRAY

JURY

Directeur de Recherche :	**Monsieur David THESMAR** Professeur , HDR HEC Paris – France
Rapporteurs :	**Monsieur Daniel PARAVISINI** Professeur Associé London School of Economics and Political Science– Royaume-Uni
	Monsieur Amit SERU Professeur University of Chicago, Booth School of Business – Etats-Unis
Suffragant :	**Monsieur Ulrich HEGE** Professeur HEC Paris – France

Ecole des Hautes Etudes Commerciales

Essays on Empirical Corporate Finance

Ph.D. dissertation submitted by:

Adrien Matray

Committee Members:

Advisors:

Ulrich Hege,

David Thesmar, Research Director

External Members:

Daniel Paravisini (London School of Economics)

Amit Seru (University of Chicago, Booth School of Business)

To Aline, Luc and Louis

Acknowledgements

"You can't just turn on creativity like a faucet. You have to be in the right mood.
- What mood is that?
- Last-minute panic.." (*Calvin and Hobbes*, by Bill Watterson)

This thesis is the result of a long process. If doing a PhD is often perceived as a very lonely journey, I had the chance to have always been surrounded by incredible persons who all made this work possible. The first pages of this thesis are here to thank them all.

First I would like to thank the members of my committee. I'm forever indebted to David Thesmar, my supervisor at HEC for his guidance and advice. David has proved to be a wonderful supervisor during those five years. Especially at the beginning, starting his own research works can be overwhelming and it is invaluable to be able to quickly test his ideas with his supervisor and trust his judgment. David has always available to discuss ideas, provide feedbacks and kindly reorient my work when it was needed. Most importantly, his incredibly broad knowledge of the literature in economics and finance and his general curiosity always pushed me to explore very different topics, as long as they were interesting and about relevant questions. I will keep in mind his most dreaded question among PhD students "Why should we care? Why not working on unemployment in Europe?" as well as some more obscure comments "you don't have enough moustache to run this regression" in my future career.

I'm indebted to Ulrich Hege, for his constant encouragements and attentive guidance and for having always very patiently listened to me when I had doubts. The completion of my PhD would have been much more painful without his extraordinary support. Ulrich always pushed me to think about the "big picture" and my discussions helped me raise the ambition of my papers every time we met.

I'm thankful to Daniel Paravisini, who provided me with a warm and challenging environment at LSE. Thanks to him, my visiting in London has been extremely rewarding.

Daniel was always available to discuss about very various ideas and looked my work with a benevolent eye. He also confirmed me that you can aim for doing careful empirical works without sacrificing the importance and relevance of the question first.

Finally, I would also like to express my gratitude to the last member of my committee: Amit Seru, a researcher that I have followed with interest since the beginning of my PhD and who kindly accepted to fly across the Atlantic to attend my defense.

We know well in finance that time is money. And doing a PhD takes time. I thank the HEC Foundation for funding my scholarship and the LSE for providing me with an office during my visiting. Thanks also to all the persons behind those institutions who made sure I was properly covered by the social insurance, got pay on time, or helped me domesticating the Notilus system or planing the organisation of my PhD defense: Caroline Meriaux, Melanie Romil, Francoise Dauvergne, Celine Rimbault. Frankie Clarke made sure I was not homeless and without computer when I visited the LSE. Thanks to all of you for allowing me to spend most time doing research and teaching while having everything around that works smoothly.

Next, I would like to warmly thank all my co-authors. I have been blessed to work with incredible persons, who always found the patience to support me and without whom the researches made during this PhD could not exist. In addition to being wonderful persons, Johan Hombert, Olivier Dessaint, Claire Celerier and Elizabeth Beasley are amazing researchers, both at the same time extremely creative and very careful and rigorous. I cannot stress enough how much I have learnt by working with them and I do hope to pursue this fruitful collaboration!

I have a special thought for Johan Hombert, who in addition of being the co-author any researcher can dream of, kindly drove me to HEC nearly every week (because yes, you can have a PhD and not a driving licence...). While it made me wake up at 5.30 in the morning, the discussions we had during the drives were largely worth it and will be deeply missed.

The need to find a car to go HEC is due to a small drawback of this institution, it is far away from Paris. But apart from that, the finance department provided me with the best environment possible to do a PhD. PhD students are immediately considered as researchers per se and encouraged to pursue their own ideas, while professors are very easily accessible. I am grateful to all for their generous feedbacks on my work with a spe-

cial thanks to Francois Derrien, Thierry Foucault and Phillip Valta, as well as Laurent Fresard who sadly for the department, decided to leave the American dream in Maryland two years ago but kept provided me with encouragement and feedback from across the Atlantic.

I have been lucky enough to meet outside HEC many professors who all took time to provide feedbacks, challenging discussions, or exposed me to new and motivating ideas. Thomas Piketty has been a wonderful supervisor while I was doing my master at Paris School of Economics, Elise Huilery, Jerome Pouyet, Eric Maurin, Milo Bianchi and Laurent Bach all provided very valuable feedbacks there and contributed in some way or another to my decision to do a PhD. Augustin Landier allowed me to visit Toulouse School of Economics where I could discuss with him, Bruno Biais, Sebastien Pouget and Thomas Chaney. Vicente Cunat, Juanita Gonzalez, Luis Guaricano at LSE (in addition to Daniel Paravisini) and Rui Silva at LBS made my visiting in London as friendly, useful and interesting as it could be. I would like to have a special thank for the well seasoned researchers who take the time to organize summer schools or workshops that provide such a great experience to PhD students. Thanks to Thomas Hellman who organised the NBER entrepreneurship summer camp, to Dimitri Vayanos (summer school on Dysfunctional Finance), to Nick Barberis (summer school on behavioral finance) and to Denis Gromb (the "Denis Gromb workshop" at INSEAD). Finally, I would like to thanks all the professors with whom I had the chance to interact with and discussed about my papers: Antoinette Schoar, Andrei Shleifer, Xavier Gabaix, Martin Brown, Robert Seamans, Geraldo Cerqueiro to name a few. Thanks to all of you for showing me that academia can be a fun, generous and stimulating place to be!

The finance department at HEC contained one additional treasure (in addition to its wonderful professors): the fellow PhD students with whom I spent four years: Jean Noel Barrot, Jerome Dugast, Hedi Benamar, Boris Vallee, Alina Rosu, Michael Spira, Charles Boissel. Your good mood, support, discussions will be missed. The atmosphere of collaboration, exchange, mutual assistance and (yes sometimes) friendly competition helped me grow as a researcher and I'm looking forward to seeing you again in various seminars and conferences around the world. I have a special thought for Michael Spira and Charles Boissel. Michael, I'm sorry we tried so many projects which unfortunately failed and I hope that your decision to go back to the private sector has nothing to do with that and that you will come back soon to academia! Charles, I'm sure that one project will eventually work and that we have many papers ahead of us.

In addition to my friends in the finance department at HEC, I had the chance to enjoy the companionship of other fellow PhD students, whose friendliness and support have been crucial and they deserve my deepest acknowledgment. In no particular order I thank: Thomas Roulet and Samuel Touboul, who showed me that strategy can be interesting. Thomas Bourveau, who did the same with accounting. Mattia Girotti for welcoming me during my visiting in Toulouse. Una Savic, Jesus Gorrin and Pat Akey who shared my aversion for noisy pubs and crappy beers and my taste for French red wine, making my year in London an extremely fun and pleasant experience. Giorgia Barboni who read carefully several times the paper on unbanked households and offered me to go work with her in Brussel on a promising dataset. I also thank Romain Lafarguette, Francois Geerolf, John Kuong, Alberto Teguia, Ryan Lewis, Patrick Kiefer for all the fun I had at Sciences Po and in conferences around the globe. Thank you all for the stimulating discussions, the less stimulating ones, the (way too) numerous drinks we got together and for all the fun we had during those years. There are sadly only 24 hours in a day, but I still hope to find the time to write papers with all of you!

Because there is also a life outside academia (contrary to what some people would like to make us believe), I thank all my friends who were wise enough to stay away from the academic path and without whom I would certainly not have been able to complete this journey. Thanks for being patient enough to keep asking questions about what exactly I'm doing research on (without asking when I will be graduating), and being (seeming?) genuinely interested about the answers. Thanks for the support during troubling times, the joy, the encouragements, the time shared around a good dinner. Thanks for all the stimulated discussions that always allowed me to make sure my work stays in touch with reality or gave me motivations or surprising questions. I sincerely thanks all of you. Those from schools, classe prepa or business school: Benjamin Candoti-Besson, Raphael Moran, Julien Pauchon, Amelie Vautravers, Aurelie Dossier, Nicolas Antraigue, Nicolas Debeuf, Bertrand Girard, Karen Leighton, Olivier Raimbault, Antoine Le-Fevre I can finally tell you that I'm officially done being a student! It took me a few extra years, but my collection of diploma (a priori) ends now. Those with whom I had the pleasure to share some amazing adventures such as creating a review (still edited!) and multiple think tanks that brought me so much and helped me growing: Axel Adida, Francois Andelkovic, Sonia Baudry, Laurent Bach, Gaell Mainguy, Laure Marie Neuburger, Gregoire Postel-Vinay, Jean Marc Schlenker, Francois Taddei, Vincent Schachter, Anne Lise Sibony, Cosimo Winkler. And all the others I had the privilege to meet along the way: David Colle, Stephane Distinguin, Laurent Eymard, Marion Meissonnier, Lucas Pernollet, Pierre Yves Platini, Corentin Sannier, Constance Valdelievre. I will never

been able to express all my gratitude and how much this thesis owns you. Thanks to all of you and the other I'm necessarily forgetting but hopefully will recognize themselves.

Finally, I would like to thank my parents and little brother for their unconditional love along all these years. There is without any doubt a job harder than doing a PhD, it is to be the parents of a PhD student. Mum, dad, you have been incredible in the accomplishment of this (daunting) task. Thank you for all the help, support and assistance you gave me. Thanks for the discussions and questions about my work and why it could be important. Thanks for your patience when I was discoursing on "endogeneity issues" or "selection bias" during family lunch. Those conversations have been among the most important to clarify what I was doing and making sure I was not completely drifting off the tracks. You, who gave me the desire to explore and understand (a bit better) the world, and who transmitted me your curiosity, your love for knowledge and your care about the public good, this thesis is entirely dedicated to you and would not exist without you.

Thank you all!

Introduction

Confidence is what you have before you understand the problem. (Woody Allen)

This thesis is structured around three different topics: banking market, innovation and behavioral finance.

Lending Relationships, innovative firms and the allocation of talents

One of the first questions I have been interested in understands the real effects of banking market structure and banking regulation. This leads me to investigate two different issues. In the first one, I study with Johan Hombert in the first chapter entitled *The Real Effects of Lending Relationships: Evidence from Innovative Firms and Inventor Mobility*, what type of banking market structure is more likely to foster innovation and innovative entrepreneurship. Our intuition was that relationship lending, because it allows lenders and borrowers to acquire subjective and abstract ("soft") information through ongoing personal interaction with borrowers, can foster innovation. By exploiting banking deregulation that happened in the 70s and 80s, we can generate a negative shock to relationships. We show that it reduces the number of innovative firms, especially those that depend more on relationship lending such as small, young, and opaque firms.

Because skilled labor is likely to be mobile, we then study how this negative shock on credit supply for innovative firms affects the allocation of talent within the economy. We find that this credit supply shock leads to reallocation of inventors whereby young and promising inventors leave small firms and move out of geographical areas where lending relationships are hurt.

We also believe that our results provide some policies implication. For instance, if one accepts that the lesson drawn from commercial banking extends to public funding, then governments willing to support innovation by allocating public funds should not rely on a centralized and hierarchical structure, but on local agencies that more able to deal with soft information. It also suggests that governments should pay attention to financial markets in order to attract skilled human capital and to win the "global race for inventors' brains" (Fink, Miguelez, and Raffo 2013). Having a well-functioning local credit market should therefore be in the tool-box of placed based policies design to attract talents (Moretti, 2011).

The reason we find this negative effect of banking deregulation on innovation is because it affects lending relationships. I then became curious to see if another category of the population which is also strongly dependent on soft information could also be harmed by an increase in competition on the banking market: poor and minorities.

Why poor households are unbanked?

Between 35 to 45% of low income households in the US are unbanked, i.e. possess neither a checking nor a saving accounts. This extremely high figure motivated Claire Celerier and I to try to understand what could be the driver behind that and especially if bank practices, by creating hurdles for the poor (e.g. minimum account balances, overdraft fees, distance between branches or the proliferation of formal steps to open an account) could be an explanation, as it is often suggested. We addressed this question in the second chapter entitled *Unbanked Households: Evidence on Supply-Side Factors.*

We use interstate branching deregulation in the U.S. after 1994 as an exogenous shock on banking supply and find that an increase in bank competition is associated with a large drop in the share of unbanked households. In particular, we find that the effect is even stronger for populations that are more likely to be rationed by banks, such as black households living in "high racial biased" states.

The improved access to bank accounts leads to higher savings rate, while not translating into higher level of indebtness, which suggests that having access to formal banking sector plays a role in asset accumulation for this population and that banking regulation, by affecting the intensity of bank competition, could impact minorities access to financial services.

Understanding knowledge spillovers

The paper with Johan Hombert leads me to study how firm behaviors can affect local productivity, knowledge spillovers and urban agglomeration. In the third chapter entitled *The Local Innovation Spillovers of Listed Firms*, I study the spillovers produced by inefficient listed firms on the cities where they operate. Using an instrument for listed firm efficiency uncorrelated with local markets, I find that when their efficiency declines, they innovate less, which conducts local private firms to innovate less in response and this effect declines sharply with distance. I provide evidence that it comes from knowledge spilling overs, especially when educated workers potentially interact more, when they can move from one firm to another and because they create new firms. Spillovers also happen because thriving cities attract capital and skilled labor potentially used by local private firms.

This last result suggests that finance could be an important factor to explain the huge disparities between cities in terms of economic specialization, entrepreneurship, growth, etc. If capital follows innovation and in return amplifies economic spillovers, initial small differences could increase rapidly. This also may explain why economic integration in Europe (especially with the creation of the Euro) has translated in more divergence across the economies and not convergence as it was initially hoped.

When the Behavioralist Meets Corporate Managers

The last chapter of my thesis is made with Olivier Dessaint and is entitled *Do Managers Overreact to Salient Risks? Evidence from Hurricane Strikes*.

In this paper, we provide empirical evidence that managers exhibit biases when assessing risk. Specifically, we show that managers systematically respond to near-miss liquidity shocks by temporarily increasing the amount of corporate cash holdings. Over time, the perceived risk decreases, and the bias disappears. Such a reaction cannot be explained by the standard Bayesian theory of judgment under uncertainty because the liquidity shock stems from a hurricane landfall whose distribution is stationary. Instead, this reaction is consistent with salience theories of choice (Tversky and Kahneman, 1973, 1974; Bordalo, Gennaioli and Shleifer, 2012a, 2012b, 2013) that predict that the temporary salience of a disaster leads managers to reevaluate their representation of risk and put excessive weight on its probability.

This bias is costly for shareholders because it leads to higher retained earnings and negatively impacts firm value by reducing the value of cash. We examine alternative explanations for our findings. In particular, we find only weak evidence that the possibility of risk learning or regional spillover effects may influence our results.

Contents

Chapter 1

The Real Effects of Lending Relationships: Evidence from Innovative Firms and Inventor Mobility

How the Banking Sector can Shape State Innovative Capacities

Joint work with Johan Hombert (HEC Paris)

1.1 Introduction

Innovation is essential for long-run economic growth (Schumpeter 1942). However, because innovation relies on human capital and intangible assets, its financing is inhibited by informational frictions (Hall and Lerner 2010). This raises the question of which type of financial system is more conducive to the financing of innovation.[1]

The banking literature (e.g., Rajan 1992) contrasts two polar systems – relationship-based versus arm's length – each with its forces and weaknesses in funding innovative firms. On the one hand, in a relationship-based system, lenders are better able to acquire subjective and abstract ("soft") information through ongoing personal interaction with borrowers. Soft information can mitigate information asymmetry, especially for small, opaque firms (Petersen and Rajan 1994) and facilitate the financing of innovation.[2] On the other hand, Rajan and Zingales (2003) argue that a relationship-based system may favor incumbents at the expense of newcomers and prevent the entry of innovative startups. They further assert that an arm's length system may give innovators a better chance of securing a loan, because it allows them to tap a wider circle of potential lenders and facilitates the aggregation of public, hard information dispersed among many investors.

In this paper, we study how the strength of lending relationships affects innovation activity and its distribution across the economy. First, we analyze whether lending relationships foster or impede innovation and test whether the ability of innovators to exchange soft information with lenders is crucial to innovative activities. Second, we

[1]See Hall and Lerner (2010), Lerner, Sorensen, and Stromberg (2011), and Brown, Martinsson, and Petersen (2013) for studies of equity funding of innovation, and Acharya and Subramanian (2009), Amore, Schneider and Zaldokas (2013), Chava et al. (2013), Cornaggia et al. (2013), Mann (2013), and Nanda and Nicholas (2013) for the role of credit market institutions.

[2]Examples of soft information include the competence and trustworthiness of the management, the types of forthcoming investment opportunities, or trade secrets whose public disclosure would reduce a project's value.

contrast the effect of relationships on opaque versus transparent firms and study how it affects the allocation of innovative human capital across firms. This investigation allows us to explore how lending relationships shape the distribution of innovation capacities.

The difficulty when studying the effect of lending relationships is that these relationships are not random. In particular, they are likely to be endogenous to firm characteristics that may be correlated with innovation activity. To overcome this problem, we use the wave of intrastate banking deregulation passed in different states in the United States from the early 1970s to the mid-1990s as a shock to relationships. By removing restrictions on bank expansion within state borders, intrastate deregulation has intensified banking competition (Black and Strahan 2002; Stiroh and Strahan 2003) and increased the size of banks (Jayaratne and Strahan 1998). These developments in local banking markets damage lending relationships. Indeed, Petersen and Rajan (1995) show that competition reduces banks' incentives to invest in relationships as they are less able to reap the rewards of their investment in the future. In addition, Stein (2002) demonstrates that large banks interact more impersonally with their borrowers because they have difficulties dealing with non-verifiable soft information that cannot be easily passed along within the hierarchy.

The staggered timing of banking deregulation across states permits a difference-in-difference identification strategy, which compares innovation activity before and after each deregulation event relative to a control group of states not undergoing a regulation change. We proxy for innovation activity by counting the number of firms filing patents with the U.S. Patent and Trademark Office (USPTO). An appealing feature of USPTO data is that they cover the whole universe of patents, including patents filed by small and private firms.

We find that the number of innovative firms declines after states deregulate their banking system. Exploring the dynamic effect of deregulation, we show that there is no pre-deregulation trend and that the number of innovative firms starts to decline three years after deregulation and ends up 20% below its initial level after 10 years. Given that the average state has 180 firms that file patents in a given year, a state that deregulates its banking system loses an average of 36 innovative firms in the long-run.

To confirm that our shock increases credit constraints for lending relationship-dependent firms, we explore the cross-sectional dimension of our effect. We construct three standard proxies to classify industries by the strength of relationships and find that the decline in innovation is more pronounced in industries where firms rely more on relationships. This effect is even stronger in industries that are more exposed to credit supply shocks: industries that are more dependent on external finance (Rajan and Zingales 1998) and those with fewer tangible assets (Almeida and Campello 2007).

We also define more granular proxies for firms' dependence on lending relationships. Since opaque firms rely more on relationships to exchange soft information with lenders (e.g., Berger et al. 2005), we proxy for relationship dependence at the firm level by its degree of opacity. First, we find that the decline in innovation is concentrated in young firms, as these firms typically produce less hard information. Second, it is also stronger in young industries holding fixed firm age, because it is more difficult to evaluate projects in nascent industries as there are fewer successful projects to which new projects can be compared. Third, we obtain similar results when we use firms' patenting history to proxy for the amount of information that is available to lenders. These results depict a consistent story where opaque, relationship-dependent innovators are more affected by deregulation.

We next turn to the consequences of credit supply shock on the labor market of inventors. Specifically, we study how it affects the allocation of talent and inventor mobility across firms and across states. First, we show that small firms tilt the composition of their innovative workforce away from young inventors after deregulation, consistent with the hypothesis that small firms compensate the loss of soft information by employing more seasoned inventors. Small firms are also less likely to employ promising inventors, suggesting that constrained employers lose their most talented employees.

Second, we study inventor mobility across firms (within state) and across states. We find that inventors working for small firms are more likely to leave after deregulation and, when they leave, they are more likely to switch to a larger employer. Zooming in on the characteristics of moving inventors, we observe that younger and more promising inventors are more likely to change firms following deregulation. Across states, we find that inventors are more likely to relocate to another state when their state of origin deregulates. This effect is stronger for inventors working in small firms and for young and promising inventors. Overall, our results indicate that financial reforms affect not only the level, but also the geographical and across-firm distribution of innovative activity.

Finally, we investigate the identifying assumption behind out analysis, namely, that the timing of deregulation is exogenous to innovation activity. Kroszner and Strahan (1999) show that deregulation is not random and is related to interest group factors such as the prevalence of small banks and of small firms. However, we show that the timing of deregulation is *not* related to various measures of state-specific innovation capacities or pre-deregulation innovation trends, which suggests that deregulation offers a valid quasi-natural experiment as far as innovation is concerned.

Our paper relates to several strands of literature. First, it connects with the literature on the real effects of lending relationships (e.g., Petersen and Rajan 1994; Berger and Udell 1995; Berger et al. 2005; Zarutskie 2006; Detragiache, Tressel, and Gupta 2008). We add to this literature by focusing on an important outcome, innovation, through which the banking sector can impact economic growth.

Second, we contribute to the literature linking bank financing and innovation (Benfratello, Schiantarelli, and Sembenelli 2008; Hellmann, Lindsey, and Puri 2008) and more specifically to the research agenda that seeks to identify the characteristics of debt markets that are more conducive to innovation. Acharya and Subramanian (2009) and Mann (2013) study the effect of creditor rights, while Nanda and Nicholas (2013) investigate the impact of bank distress.

Another channel through which banking regulation can affect innovation is analyzed by Amore, Schneider, and Zaldokas (2013), who focus on another episode of deregulation in the U.S., namely, interstate banking deregulation, which allowed banks to expand across state borders.[3] They show that interstate deregulation enabled banks to better diversify and thus to increase credit supply to innovative firms. Cornaggia et al. (2013) exploit another episode of interstate deregulation to study the dynamics of public firms' acquisitions of small innovative targets. In contrast, we focus on the wave of *intrastate* branching deregulation, which allowed banks to expand within state borders. The reason is that we want to isolate the effect of a shock to lending relationships with no change

[3]See Kroszner and Strahan (2011) for a detailed history of banking regulation and deregulation.

in banks' ability to diversify geographically: arguably, diversification benefits are much lower with within-state expansion.[4]

More closely related to our paper is Chava et al. (2013), who consider both intrastate and interstate banking deregulation. Their focus is different as their goal is to contrast the effects of intrastate deregulation with interstate deregulation. They find, like us, that innovation decreases following intrastate deregulation. In contrast to us, they argue that this effect is due to a reduction of competition in local banking markets. We offer and test an alternative hypothesis: deregulation stifles innovation because by increasing competition, it damages lending relationships. First, our explanation reconciles our findings with those of previous studies that intrastate branching deregulation increases banking competition (Jayaratne and Strahan 1998; Black and Strahan 2002; Stiroh and Strahan 2003; Cetorelli and Strahan 2006). Second, to disentangle Chava et al.'s (2013) hypothesis from ours, we follow the methodology of Black and Strahan (2002) to show that the effect of deregulation operates through an increase in banking competition. Specifically, we show that innovation declines more in states with more concentrated banking markets, because the increase in competition has a greater effect in markets that are initially less competitive. Finally, we provide several additional tests of the relationship hypothesis in the cross-section of industries, firms, and inventors.

Third, we contribute to the burgeoning literature on inventor mobility (Almeida and Kogut 1999; Agrawal, Cockburn, and McHale 2006; Breschi and Lissoni 2009; Marx, Strumsky, and Fleming 2009) and more broadly to the literature on the domestic and international migration of skilled human capital (instance Borjas, Bronars, and Trejo

[4]Among our other robustness tests, we check that our results are robust to controlling for interstate deregulation.

1992; Boustan, Fishback, and Kantor 2010; Docquier and Rapoport 2012). Our paper indicates that finance also determines the migration of talent.

1.1.1 Banking deregulation

[INSERT FIGURE 1.1 ABOUT HERE]

Before 1970, most U.S. states had strong banking market regulations. Branching was either prohibited or strongly limited, with the exception of 12 states that allowed unrestricted statewide branching. Starting in 1970 however, all the other states progressively lifted restrictions on branching within their borders. States generally relaxed restrictions on within-state bank expansion in three steps: by permitting the formation of multibank holding companies, by permitting branching by means of merger and acquisitions (M&As) only, and by permitting unrestricted (de novo) branching, thereby allowing banks to enter new markets by opening new branches. Figure 1.1 illustrates the timing of the deregulation for the three dimensions. There are 82 episodes of deregulation in 39 different states between 1970 and 1994. Because we do not have priors about which of these three steps had the greatest impact, we follow Black and Strahan (2001) and construct a deregulation index. The index equals zero if a state does not permit branching via M&As, de novo branching, or the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within a state.[5] At the end of the deregulation period, in 1994, 38 states had a deregulation index equal to three.

In our main specification, the deregulation index enters linearly into the innovation

[5]While stronger competition and larger bank size limit the ability of lenders to finance soft information-based projects, the lifting of de novo branching restrictions might also reduce distance to the nearest lender, which may counterbalance partially the loss of soft information. If this is the case, then our estimate will underestimate the impact of hurting lending relationships on innovation.

equation, that is, we assume that increasing the index from zero (full regulation) to one has the same effect as moving from one to two, or from two to three (full deregulation). We show in Section 1.5.4 that our results are robust when using the index non-parametrically.

1.1.2 Measure of innovation

We use patents filed with the U.S. Patent and Trademark Office (USPTO) compiled in the National Bureau of Economic Research (NBER) Patents File (Hall, Jaffe, and Trajtenberg 2001) to measure innovation. The data contain all patents granted in the U.S., along with information about the patentee (unique identifier, institutional characteristics, nationality, geographic localization) and about the patent (year of application, technology class, number of citations received). An appealing feature of the NBER Patents File is that it covers the entire universe of patents filed in the U.S., including patents filed by young and private firms. This is important because these firms are more likely to be affected by changes in local banking markets as they typically have less access to national capital markets. Having comprehensive patent data is also needed to assess the effect of banking deregulation on the total amount of innovation produced in each state. This feature of the data allows us to rule out explanations of our results based on a change in the share of innovation conducted by public versus private firms.

While the NBER patent data do not have a standard industry classification, they have a classification based on the technology of patents. We use the two-digit classification, which counts 37 technology classes listed in Table 1.1.[6] With a slight abuse of terminology, we will use the terms "technology class" and "industry" interchangeably in this paper.

[6]We have re-run all our regressions and obtained similar results with the finest three-digit classification, which counts 422 technology classes.

[INSERT TABLE 1.1 ABOUT HERE]

We only keep patents filed by U.S. corporations in our sample, thereby excluding foreign firms, universities, and governmental agencies.[7] We date our patents according to the year in which the patent application was filed. This avoids anomalies that may be created due to a lag between the date of application and the granting date. We consider all patents filed between 1968 (two years before the beginning of the deregulation period) and 1998 (four years after the end of the deregulation period). Our main variable of interest is the number of uniquely identified firms that file at least one patent (hereafter "innovators") at the state-year-industry level.[8] Finally, we follow the banking deregulation literature and exclude Delaware. This leaves us with a balanced panel of 37 industries in 50 states (including the District of Columbia) over 31 years. Table 1.1 reports summary statistics for the number of innovators for each of the 37 industries. There is an average (median) of 5.6 (1) innovators in a given state-year-industry cell with a substantial heterogeneity across both industries and states.

Patents have long been used as an indicator of innovative activity (Griliches 1990); this measure, however, has its drawbacks. Not all firms patent their innovations, because some do not meet the patentability criteria and the firm might rely on secrecy or other means to protect its innovation. In addition, patents measure only successful innovations. Despite these drawbacks, there is nevertheless a strong relationship between R&D and the number of patents in the cross-section of firms (R-squared is 0.9; see Griliches 1990).

[7] We exclude foreign firms because these firms often file patents with the USPTO to protect their innovations on U.S. soil but actually seek financing and do their R&D in their home country (see Acharya and Subramanian 2009).

[8] When an innovator files patents in several industries in a given state and year, we assign the innovator to the industry(ies) in which the innovator filed the largest number of patents (if the maximum is reached for several industries). This assumption is consistent with interpreting the number of innovators as a measure of the extensive margin of innovation, as it avoids double counting.

1.1.3 Identification strategy

Our main specification focuses on the number of innovative firms. The reason is that we want to weight small and large firms equally, because we expect deregulation to affect mostly small firms. Following the innovation literature, we estimate a Poisson model to take into account the counting nature of the dependent variable:[9]

$$E\left[Innovators_{jst}\right] = \exp\left(\alpha_j + \gamma_s + \delta_t + \beta\ Deregulation_{st} + Controls_{st}\right), \tag{1.1}$$

where $Innovators_{jst}$ is the number of innovators in industry j, state s, and year t. $Deregulation_{st}$ is the deregulation index equal to the number of steps of deregulation that have been implemented in state s up to year t, that is, the index is incremented by one for all the years following each deregulation event. α_j, γ_s, and δ_t denote industry, state, and year fixed effects. Industry fixed effects account for the heterogeneity in the propensity to innovate and to patent innovation across industries. State fixed effects capture time-invariant determinants of innovation in the different U.S. states, such as the size of the state, the sectoral composition, and the level of education. Year fixed effects control for aggregate shocks and common trends in innovation activity.[10] We also control for time-varying state characteristics that may affect innovation: annual number of college degrees granted, annual number of doctorates granted, annual amount of federal funds for research and development, and volume of capital invested each year by venture capitalists.[11] The Poisson model is estimated using maximum likelihood and standard

[9]See Hausman, Hall, and Griliches (1984) for a discussion of count data models.

[10]Such common shocks can be caused by changes in the legal and institutional environment at the federal level, such as the creation of the Court of Appeals for the Federal Circuit in 1982.

[11]Data on educational attainment and federal R&D expenses come from the National Science Foundation's CASPAR database, while information on venture capital funds is from VentureXpert.

errors are clustered at the state level to account for serial correlation and correlation within states.

The parameter of interest is β. It measures the permanent effect of one step of deregulation (out of three possible steps) on the number of innovators. The identification of β relies on comparing the number of innovators before and after deregulation relative to a control group of states not experiencing a change in regulation.

[INSERT FIGURE 1.2 ABOUT HERE]

Before running our regressions, we estimate the dynamic effect of deregulation around the event date. In Figure 1.2, we estimate equation (1.1) where the deregulation index is replaced by dummy variables for each year from 10 years before to 10 years after each step of deregulation. Reassuringly, there is no trend before the event date. In particular, the number of innovators 10 years before deregulation is almost equal to the number of innovators at the time of deregulation. This is consistent with our identifying assumption that deregulation is not endogenous to innovation (see also Section 2.5.1). Figure 1.2 also shows that the effect of deregulation starts to materialize three-four years after the event date. There are two possible explanations for this result. First, it can take a couple of years before deregulation reshapes the banking market structure and leads to the development of large banks (Jayaratne and Strahan 1998). On the other hand, the adverse effect of competition on banks' incentives to invest in lending relationships is effective as soon as banks anticipate increased competition, which is at the time of deregulation (or even before if deregulation is anticipated). Second, there is a delay between the time an innovative project is funded and the moment when the firm files the patent application. We run formal statistical tests in the following section.

1.2 Data and Empirical Strategy

1.3 Number of Innovating Firms

1.3.1 Baseline results

[INSERT TABLE 1.2 ABOUT HERE]

We start by investigating the effect of banking deregulation on the number of innovative firms. Results are reported in Table 1.2. Column (1) shows that every deregulation step leads to a statistically significant 9.7% decline in the number of innovators. In column (2), we add time-varying control variables for the level of education, federal R&D spending, and venture capital activity at the state-level. All these variables are significant with the expected sign except federal R&D spending, which is insignificant; this may be due to the fact that federal spending is directed toward moderately innovative states. The coefficient on the deregulation index remains negative at -7.1% and significant at the 5% level. Given that the deregulation index ranges from zero to three, it indicates that the number of innovators drops by a little more than 20% when a state moves from being fully regulated (deregulation index equal to zero) to being fully deregulated (deregulation index equal to three).

In column (3) we exploit the time dimension of the panel more fully to check that we are not capturing a trend. We decompose each of the three components of the deregulation index into four dummy variables associated with four periods around the deregulation date: more than four years before deregulation, the four years preceding deregulation, the four years following deregulation, and more than four years after deregulation. Then, we sum over the three components of the deregulation index to obtain four dummy variables corresponding to the four time periods around each step of deregulation. The deregulation year is the reference year. First, as seen in Figure 1.2, we find that there is no pre-deregulation trend. Second, we also find that it takes some time before the effects of deregulation materialize: the number of innovators decreases by 2.7% in the first four years after deregulation while it decreases by 9% after that.

1.3.2 Relationship dependence

We next turn to our main hypothesis, namely that innovation declines because lending relationships are hurt by deregulation. To test this hypothesis, we ask whether innovators that are more reliant on relationships are more affected by deregulation. One difficulty is that relationships reflect the optimal answer of firms to their specific environment, which may create an endogeneity problem. To overcome this problem, we rely on an empirical strategy that is similar in spirit to Rajan and Zingales (1998). We posit that some industries are naturally more reliant on bank relationships than others, for instance because firms operating in these industries are more opaque and need to exchange soft information with lenders. Thus, we compute proxies of relationship dependence at the industry level across all borrowers in the U.S. By construction, these proxies are constant across states and are therefore exogenous to unobserved state-specific characteristics.

We create three proxies of relationship dependence using the National Survey of Small Business Finances (1987 and 1998), which contains a thorough documentation of firms' relationships with financial institutions.[12] The first proxy is the average distance be-

[12]For more details about this database, see Petersen and Rajan (2002). We provide more detail on how we construct these proxies in Appendix 3.7.1.

tween firms and their main lenders in 1987 (the first year the survey was conducted) at the two-digit SIC level. As shown by Petersen and Rajan (2002), when the distance between the bank lending office and the borrowing firm is longer, they communicate in more impersonal ways and are less able to share soft information. The second proxy is borrowed from Landier, Nair, and Wulf (2009) and is defined as the average increase in distance between banks and borrowers between 1987 and 1998. The idea is that in hard information industries, the distance between banks and borrowers increases more as lenders take advantage of technological developments. The third proxy of relationship dependence is the average length of the relationship between banks and borrowers in 1987 (Petersen and Rajan 1994). The low correlations between these three measures suggest that they are capturing different dimensions of relationship dependence.[13] Finally, we map each of the three variables into the 37 patent classes that we use in our regressions and classify a patent class as relationship dependent if the variable is above the sample median.[14]

We regress the number of innovators on the deregulation index and its interaction with each of the three measures of relationship dependence as well as on the same set of controls and fixed effects as before. The results are reported in Table 1.2, columns (4)-(6). With all three measures, the negative effect of deregulation is stronger in more relationship-dependent industries, while the effect is never significant in industries less reliant on relationships (the reference group in the regression). The difference between

[13]The correlation between (minus) distance and (minus) change in distance is -0.42, between (minus) distance and length of relationship is -0.01, and between (minus) change in distance and length of relationship is 0.23. The fact that the first correlation is negative is due to the fact that distance is measured in 1987 and change in distance is computed between 1987 and 1998, which are negatively correlated if it is easier to increase the distance when the initial distance is small.

[14]We obtain similar results if we split industries into terciles or quartiles of relationship dependence or if we use the continuous variables.

high and low relationship dependent industries is −3.8% when relationships are proxied by average distance with lender, −2.9% when relationships are proxied by change in distance, and −9.1% when using the length of relationships; it is significant at the 1% level with all three proxies. These results lend support to the hypothesis that innovation activity slows down because banking deregulation increases credit rationing for firms that are more reliant on relationship lending.

1.3.3 Sensitivity to credit supply shocks

To provide further evidence that our findings are explained by a credit supply shock, we test whether our effect is stronger for firms that are more sensitive to credit supply shocks. We consider two standard measures of sensitivity to credit supply shocks. The first one is Rajan and Zingales' (1998) external finance dependence defined as the industry average of the fraction of investment that cannot be financed by current cash flows. The second proxy is asset tangibility; it captures more specifically the sensitivity to a shock to lending relationships. Indeed, tangible assets can be pledged to mitigate information asymmetries, which is especially useful when relationships are weak and borrowers cannot share soft information with lenders. For instance, Berger and Udell (1995) show that banks ask for more collateral when they have weaker relationships with the borrower.[15] We measure asset tangibility as the industry average of the ratio of net property, plant, and equipment over total assets. We construct these two variables using Compustat and map the SIC industries into patent classes.

[INSERT TABLE 1.3 ABOUT HERE]

[15]See also Almeida and Campello (2007) and Chaney, Sraer, and Thesmar (2012) for the importance of tangible assets in alleviating credit constraints.

Table 1.3, column (1), we regress the number of innovators on the deregulation index and its interaction with financial dependence. We find that the interaction term is negative and significant. Therefore, industries that rely more on external financing are more affected by deregulation. This is consistent with the interpretation that the observed drop in innovation is due to a credit supply shock. We next consider the triple interaction of deregulation with financial dependence and with each one of the measures of relationship dependence. In columns (2)-(4), the triple interaction term is negative for all three proxies and is significant at the 5% level for two out of the three proxies. Therefore, among relationship-dependent industries, those that are more reliant on external financing are more affected by deregulation than those that have more internal funds. This result lends further support to the interpretation that the effect of deregulation on relationship-dependent industries operates through a credit supply shock.

A similar picture emerges when we use asset tangibility as a proxy for sensitivity to credit supply shocks. In column (5), the interaction term between deregulation and asset tangibility is positive and significant, which indicates that the negative effect of deregulation is dampened in industries that have more tangible assets. The triple interaction of deregulation with asset tangibility and relationship dependence is positive and significant for all three proxies of relationship dependence. Therefore, among relationship-dependent industries, those with more tangible assets are less affected. This finding is consistent with Berger and Udell (1995), who show that tangible assets which can be pledged to secure loans are substitutes to lending relationships.

1.3.4 Effect depending on lenders' information set

So far we have considered industry-level time-invariant measures of relationship dependence. We now develop more granular measures based on the idea that borrowers who cannot produce easily verifiable information have to rely on relationship lending [see Berger et al. 2005 for a discussion]. Relationships allow opaque firms to exchange soft information with lenders and thus to mitigate information asymmetries (Petersen and Rajan 1994). Therefore, the prediction of the relationship hypothesis is that innovators who cannot produce hard information should be more affected by the weakening of lending relationships brought about by deregulation. To test this prediction, we create a series of measures for the amount of hard information available to lenders.

Information proxy based on age

The first proxy is simply the age of the firm. Young firms have mechanically a shorter track record than old firms, therefore it is more difficult for young innovators to produce hard information such as past patents or historical financial statements. The second proxy is the age of the industry. Regardless of the firm's ability to produce hard information about itself, its projects remain hard to evaluate if it operates in a young industry and the banker remains at arm's length. Intuitively, it is more difficult to assess the quality of a project when there is no similar product on the market than when several firms have already successfully commercialized similar innovations. Therefore, the ageing of an industry produces hard information for all the projects in the industry. In other words, it was more difficult to assess the quality of a project in the computer sector before the emergence of Microsoft, Sun Microsystems, and Apple.

Firm age is calculated as the number of years since the firm first filed a patent application. We identify young firms as those whose age is below the sample median (three years) and old firms as those whose age is above the sample median.[16] Industry age is measured as the median age of innovators in the industry. We classify an industry as young if its age is below the median industry age (also three years) and old otherwise.[17]

To investigate the effect of age, we count the number of young innovators and the number of old innovators at the state-year-industry level. We therefore obtain a four-dimensional balanced panel where the new dimension is the age category of innovators: young or old. We construct a dummy variable equal to one in the young innovator age category, as well as a dummy variable equal to one if the industry is classified as young. We then regress the number of innovators at the state-year-industry-innovator age category level on the two age dummies and their interactions with deregulation, as well as the same controls and fixed effects as in equation (1.1).

[INSERT TABLE 1.4 ABOUT HERE]

The results are reported in Table 1.4. In column (1), we consider the effect of innovator age and find that deregulation essentially affects young innovators. While the effect of deregulation on old innovators (the reference group) is negative and insignificant, the number of young innovators decreases by a significant 5.1% compared to old innovators. In column (2), we consider industry age and find that compared to old industries, the number of innovators in young industries decreases by a significant 7.5% after deregulation. When

[16]The NBER Patents File starts in 1965 but coverage is good only starting in 1968, which creates a truncation problem in the definition of age. To limit this problem, we start the sample period in 1970 when studying the effect of age.

[17]We find similar results in unreported regressions when we use the average age of patents in the industry as an alternative measure of industry age.

we include innovator age and industry age in column (3), both are negative and significant: young industries are more affected holding fixed firm age, and young firms are more affected holding fixed industry age. These results are consistent with the hypothesis that damaging lending relationships is more harmful to firms and industries that do not have much a track record.

Information proxy based on the patent portfolio

Another dimension of an innovative firm's track record is its patenting history. To proxy for the information set of potential lenders, we construct two variables based on observable information about the firm's patent portfolio. To create the first variable, we rely on pairwise citations data, which provide for every patent k the list of patents that cite patent k. Since we know the date when citing patents are granted, we can compute at any point in time the number of citations received by a given patent up to that point. Therefore, we can count the total number of citations received by the patent portfolio of a firm at any point in time. Forty percent of innovator-year observations correspond to firms that have received at least one citation. We classify innovators into two groups depending on whether they have received at least one citation. Finally, we count at the state-industry-year level the number of innovators with no past citations, as well as the number of innovators whose patents have already received citations.

While the first proxy captures the observable quality of the patent portfolio, the second proxy measures the complexity of the patent portfolio. To measure the complexity of a patent, we ask whether it cites a wide body of technological antecedents, in which case external lenders might find it more difficult to assess its quality because this requires expertise in a broad set of industries. To operationalize this idea, we borrow from the measure of patent originality developed by Hall, Jaffe, and Trajtenberg (2001), which is defined as one minus the Herfindahl concentration index of patent classes associated with cited patents. For each innovator-year observation, we compute the average originality across patents filed by the firm up to that year, and we count the number of firms with patent portfolio originality above the median, as well as the number of firms with patent portfolio originality below the median.

[INSERT TABLE 1.5 ABOUT HERE]

Table 1.5 reports the results of the effect of deregulation depending on the amount of information available to lenders. First, we find that firms with cited patents are barely affected by deregulation (negative but insignificant coefficient in column (1)). Conversely, the number of innovators with no cited patents decreases by a significant 8.8% (column (2), the difference with the estimate in column (1) is significant at the 1% level). Therefore, more opaque innovators are more affected by deregulation than innovators with a track record of successful patents. The results in columns (3) and (4) are similar when using the alternative proxy for the lender's information set based on the complexity of patent portfolios. Overall, the results in this section are consistent with the interpretation that deregulation tightens credit constraints for opaque innovative firms that need to share soft information with lenders.

1.4 Labor Market Reallocation

In this section we investigate whether there is a reallocation of inventors following deregulation. We focus on human capital for several reasons. First, labor and especially educated labor is highly mobile both across firms and across geographic areas. Second, our lending relationships hypothesis makes predictions on the characteristics of inventors who move after deregulation and on the characteristics of firms they leave and the firms to which they move. If deregulation entails a loss of soft information for small, opaque firms, we expect those firms to prefer hiring seasoned inventors to alleviate information asymmetry with lenders. Indeed, hiring a seasoned inventor allows the firm to produce more hard information to the lender since her track record of patents is easily verifiable. In addition, if credit constraints increase particularly for small firms, then these firms should lose their talented inventors first as they are more difficult to retain.

The patent data include the names of the inventors for every patent; they do not, however, provide consistent listings of inventor names or unique inventor identifiers. To overcome this problem, Lai, D'Amour, and Fleming (2009) develop a disambiguation algorithm to create unique inventor identifiers, which we use to track inventors over time. Since inventor identifiers are only available starting in 1975, we study inventor mobility over the 1975-1998 period.[18]

We first take a static view and study the composition of the labor force of inventors of small versus large employers. Then, we explore the mobility of investors across firms and across states and ask whether inventors leave small firms to work for bigger firms or for firms located in other states.

[18]The data are available at http://dvn.iq.harvard.edu/dvn/dv/patent and the disambiguation algorithm is discussed in Lai, D'Amour, and Fleming (2009).

1.4.1 Static analysis

We start the analysis at the firm level. Each year, we list for each firm all the authors of patents filed by the firm during that year; when the same inventor is the author of several patents in the same firm-year, she is listed only once. We have 305,034 firm-year observations. We measure employment as the number of inventors at the firm-year level and define small firms as firms with below-median employment (three inventors). To investigate employment composition, we construct three variables at the firm-year level: the fraction of young inventors, the fraction of promising inventors, and the fraction of young promising inventors. We measure inventor age as the time elapsed since the inventor first filed a patent and define young inventors as those who are below the median inventor age (two years old). To proxy for promising inventors, we count the number of patents the inventor will file in the future and define promising inventors as those with a number of future patents above the median (two patents). Young promising inventors are those who are both young and promising.

[INSERT TABLE 1.6 ABOUT HERE]

First, we regress the fraction of young inventors at the firm-year level on the deregulation index, a small firm dummy and its interaction with deregulation, and the same set of fixed effects and controls as in equation (1.1). Consistent with the hypothesis that small firms compensate the loss of soft information by hiring more seasoned inventors, the interaction term between the small firm dummy and deregulation in column (1) in Table 1.6 is negative and significant at the 1% level. Second, in column (3), we find that the fraction of promising inventors also drops by a significant amount in small firms compared to large firms. This is consistent with the idea that constrained employers lose their most talented employees first. Finally, combining these two results, we analyze in column (5) the fraction of young promising inventors and find that it decreases by 0.85 percentage points in small firms relative to large firms following deregulation. Given that young promising inventors represent on average 16% of the inventors in a firm, this means that each step of deregulation reduces the fraction of young promising inventors by $0.85/0.16 = 5\%$ in small firms. All these findings are robust to controlling for firm fixed effects in columns (2), (4), and (6).

1.4.2 Dynamic analysis

The above results offer a static view of the composition of the labor force at the firm level. We now take a more dynamic view and investigate inventor mobility across firms and across states.

To measure inventor mobility we follow Marx et al. (2009) and identify an inventor as changing employers if she files two successive patent applications that are assigned to different firms. Specifically, we start with the patenting history of 630,866 unique inventors. The unit of observation is an inventor-employer-year cell (i, k, t) such that inventor i files at least one patent assigned to firm k in year t. For every subsequent observation, we define a move dummy variable equal to one if the inventor's employer is different from the previous employer;[19] 15.5% of observations correspond to a move. Because the previous observation for the same inventor is needed to detect a move, the first observation of each

[19] When we observe a firm change, we do not know precisely when it occurred within the time interval between the two observations. This is, however, unlikely to be a major problem because the average time between two consecutive observations is only 2.4 years. In the main analysis, we follow Marx, Strumsky, and Fleming (2009) and consider that the move happens at the midpoint of the time window between the two observations. In unreported regressions, we obtain similar results when assuming that the move happens at the first date or at the second date.

inventor is excluded from the analysis; in particular, investors that appear only once are excluded. We are left with 232,091 unique inventors and 577,401 inventor-employer-year observations. We construct two variables to study inventor mobility: a within-state move dummy variable equal to one if the inventor moves to another firm in the same state (13% of observations) and an out-of-state move dummy variable equal to one if the inventor moves to another firm in another state (2.5% of observations).

[INSERT TABLE 1.7 ABOUT HERE]

We start by analyzing within-state mobility in Table 1.7. First, we regress the within-state move dummy on the deregulation index and its interaction with the dummy variable equal to one if the inventor's (initial) employer is below the median size. In addition to the same set of fixed effects and controls as in equation (1.1), we also include inventor age and indicator variables for the inventor's main technology class defined as the industry in which the inventor files the largest number of patents over the sample period. We find that the likelihood that an inventor leaves a small employer increases by 3.8 percentage points (0.013+0.025 in column (1)) following every step of deregulation. This corresponds to a 30% increase relative to the unconditional probability of moving. In contrast, there is no significant effect on mobility away from large firms (the reference group in the regression).

We next investigate whether inventors are leaving small firms to work for larger firms. We focus on the subsample of inventor-employer-year observations corresponding to a change of employer within the state and compute the ratio of new employer size over previous employer size. Column (2) in Table 1.7 shows that after deregulation, the ratio of new employer size over previous employer size goes up by a significant 4.4 percentage points. To characterize which inventors are moving away from small firms to large firms, we interact the deregulation index with each of the small three inventor characteristics as in the static analysis. Column (3) shows that the coefficient on the interaction term with the young inventor dummy is positive and significant. Therefore, after deregulation, young inventors are more likely to move to larger employers compared to older inventors. Similarly, columns (4) and (5) show, respectively, that the new employer over previous employer size ratio also increases more for promising inventors and for young and promising inventors.

[INSERT TABLE 1.8 ABOUT HERE]

Another dimension along which the labor force may be reallocated is across geographical areas. In Table 1.8, we investigate whether inventors leave states that deregulate. In column (1), we regress the out-of-state move dummy on the deregulation index and find a positive and significant coefficient. The likelihood that an inventor leaves the state increases by 0.95 percentage point following each step of deregulation. This corresponds to a 35% increase relative to the unconditional probability of the inventor leaving the state. In column (2), we interact the deregulation index with the small firm dummy and find that the interaction term is positive and significant. Therefore, inventors are more likely to leave states that deregulate when they initially work in a small firm, which is consistent with our hypothesis that small firms are hit disproportionately by deregulation. Then, in order to investigate which types of inventors are more likely to move out of deregulated states, we interact the deregulation index with the same three inventor characteristics as before. We find that promising inventors, as well as young and promising inventors are more likely to move out of state after deregulation (columns (3), (5), and (7)). Finally, we consider the triple interaction of deregulation with the small firm dummy with each of the three inventor characteristics. For all three proxies, the triple interaction term is positive and significant, that is, small firms see their young inventors, their promising inventors, and their young and promising inventors move out of state following deregulation (columns (4), (6), and (8)).

Overall, our analysis of inventor mobility indicates that, by damaging lending relationships, banking deregulation leads to labor market reallocation for inventors. This reallocation appears both across firms, as inventors leave small, opaque firms for bigger firms that are less affected by deregulation, and across states. Even more problematic, states that deregulate lose their young and most talented researchers to the benefit of other states.

1.5 Robustness

1.5.1 Is the timing of deregulation random?

In this section, we investigate whether banking deregulation can be considered as a quasi-natural experiment in our setting. Kroszner and Strahan (1999) have shown that the timing of deregulation is not random across states and is related to interest group factors, such as the prevalence of small banks and small firms. This non-randomness would compromise our identification strategy if the timing of deregulation were related to changes in innovation other than through the causal effect of deregulation on innovation that we seek to identify. Of course, it is not possible to test directly the identification assumption. Nevertheless, we conduct several tests to alleviate endogeneity concerns.

As previously mentioned, Figure 1.1 suggests that innovation changes do not lead the adoption of state-level deregulation. To develop more formal tests, we investigate how the timing of deregulation is related to potential determinants of innovation. Following Kroszner and Strahan (1999), we model the "duration of regulation" (or the "time until deregulation") using a Weibull proportional hazards model. The hazard rate function takes the form:

$$h(t, X_t, \beta) = h_0(t) \exp[X_t' \beta], \tag{1.2}$$

where X_t is a vector of time-varying covariates; β is a vector of unknown parameters; and the baseline hazard rate, $h_0(t)$, is pt^{p-1} with shape parameter p. The parameters β and p are estimated by maximum likelihood. Since we consider three steps of deregulation (formation of multibank holding companies, branching by means of M&As, and de novo branching), the covariates vector includes an indicator variable for each type of deregulation. We include in the analysis all state-deregulation step pairs such that the state did not implement this step of deregulation before 1970. We keep state-deregulation step pairs even when the state has still not deregulated in 1994, in which case the duration is right-censored. We are left with 95 state-deregulation step pairs of which 82 are not censored (i.e., deregulation is observed during the sample period).[20] For each state-deregulation step pair, we have one observation for each year up to and including the year of deregulation, which gives us a total of 1,586 observations.

First, we investigate whether the timing of deregulation is related to state characteristics at the beginning of the period that may determine innovation patterns during the sample period. We consider several potential determinants of innovation. The first ones

[20] Excluding the 13 right-censored state-deregulation step pairs from the analysis yields similar results.

are the control variables (number of college graduates, number of doctorates granted, federal funds for R&D, invested VC capital) that we use in all the regressions. Another potential determinant of innovation is the industry mix at the beginning of the sample period. Specifically, we compute for each state the fraction of patents filed in each of the 37 technology classes in 1968 ($w_{s,j}$). Then, we compute the growth rate of the aggregate number of patents in each technology class between 1968 and 1998 (g_j). We define *Predicted innovation growth* as the predicted growth rate of the number of patents given the initial technological specialization ($\sum_j w_{s,j} g_j$). The idea is to capture whether states are in promising industries at the beginning of the period. We consider an alternative measure of the industry mix that does not rely on patent data: *Initial share of innovative industries* is the share of state GDP in the most innovative industries in 1968, where we define the most innovative industries as industries in the top tercile of the total number of patents filed in the industry (all over the U.S.) divided by total industry value added.[21] Finally, we define *Pre-period innovation growth* as the growth rate of the number of patents filed in the state over the eight-year period preceding the sample period.[22] The idea is that, if the innovation process is either persistent or mean-reverting, then the pre-period innovation trend may affect innovation patterns during the sample period.

Second, we want to test whether states deregulate their banking market *following* changes in innovation activity. We define two time-varying measures of innovation at the state level. *Innovation growth* is the growth rate of the number of innovators in the state.

[21]Data on state GDP by industry are from the U.S. Bureau of Economic Analysis. We describe in more details how we construct this variable in Appendix 3.7.1.

[22]Specifically, we compute the growth rate of the number of patents granted between 1963 and 1970. In the rest of the paper we use the application year, but prior to 1969 only the grant year is available in the data. Given that the average (and median) time from application to grant is two years, the 1963-1970 period in terms of grant years corresponds to 1961-1968 in terms of application years, which is the eight-year period preceding the sample period. We use the growth rate in the number of patents rather than in the number of innovators because firm identifiers are missing before 1968.

Share of innovative industries is the share of state GDP in the most innovative industries using the same definition of innovative industries as above, except that now the variable is time-varying instead of fixed in 1968.

[INSERT TABLE 1.9 ABOUT HERE]

Table 1.9 reports the scaled coefficients $\beta^* = -\beta/p$, where p is the Weibull shape parameter. These coefficients have an intuitive interpretation: the log expected time to deregulation is equal to $X_t'\beta^*$, thus β^* coefficients represent the percentage change in the time to deregulation for a one-unit change in the covariates (Kiefer 1998). When state characteristics in 1968 are included one at a time in columns (1) to (4), none of them have significant predictive power for the timing of deregulation except *Pre-period innovation growth*, which is significant at the 10% level. The negative sign indicates that states which experience strong patent growth during the 1960s deregulated their banking market earlier. However, this effect is economically small: a one-standard-deviation increase in pre-period innovation growth (equal to 0.28) results in a $0.3 \times 0.28 = 8.5\%$ decrease in the time until deregulation, or about 10 months. In column (5), we include all the initial state characteristics and we also control for the main variables used by Kroszner and Strahan (1999). The coefficients on the Kroszner-Strahan variables are consistent with their findings: for instance, a larger share of small banks delays deregulation, while a larger share of small firms leads to earlier deregulation.[23] When all these variables are included, all our variables are insignificant. We next consider the time-varying measures of innovation activity and find that they are not significant whether they are included

[23]The exact values of the coefficients differ from Kroszner and Strahan (1999) because we include three types of intrastate branching deregulation (formation of multibank holding companies, lifting of branching restrictions by means of M&As, and of de novo branching restrictions), while Kroszner and Strahan (1999) focus on branching by means of M&As only. Nevertheless, results are very similar.

separately (columns (6) and (7)) or together with the Kroszner-Strahan variables (column (8)). Overall, the timing of deregulation does not appear to be related to innovation activity except maybe with pre-period innovation growth. This may be a problem if there is mean-reversion or persistence in the innovation process. In the next section, we show that controlling for potential mean-reversion or persistence does not affect our results.

1.5.2 Controlling for trends

[INSERT TABLE 1.10 ABOUT HERE]

In this section, we re-estimate our main specification controlling for specific trends and show that our results are robust. First, we control for the innovation trend that is predicted by the initial technological specialization of the state. To do that, we interact *Predicted innovation growth* with a linear time trend and use this predicted trend as a control variable. Column (1) in Table 1.10 shows that our main estimate is barely affected by the inclusion of this additional control, both in terms of economic magnitude and of statistical significance.[24] In column (2), we control for the alternative measure of the initial industry specialization, *Initial share of innovative industries*, which we interact with a time trend. Again, our result is robust. To control for potential persistence or mean-reversion in innovation, we define a pre-period innovation trend as *Pre-period innovation growth* times a time trend and use it as a control variable. Column (3) shows that our result is robust. A more direct approach to control for trends is to estimate the pre-deregulation trend and use it as a control variable. We estimate the pre-deregulation

[24]Using linear-quadratic time trends instead of linear ones yields similar results for all the regressions reported in columns (1)-(4).

trend by computing the average annual growth rate of the number of innovators between 1968 and the first deregulation date; then, we multiply this growth rate by a time trend. When we add this pre-deregulation trend as a control variable in column (4), our result still holds.

Another potential concern is that, in the case of early deregulations, the identification of the effect of deregulation relies on a short pre-deregulation period. To check that these early deregulations do not drive our results, we redefine a deregulation index that is incremented by one unit only for deregulation events occurring in 1976 or later. Column (5) shows that the estimated effect of deregulation is slightly larger in this case.

The last robustness check that we run is inspired by Figure 1.1. Deregulation does not lead to a downward jump in innovation activity, but rather to a declining trend. Therefore, instead of estimating the jump in the number of innovators that occurs on the date of the deregulation, we fit a piecewise linear function whose slope changes at the time of deregulation. We define the post-deregulation trend as $\sum_{k=1}^{3}(t > T_k) \times (t - T_k)$, where k indexes the three types of deregulation and T_k is the deregulation year. In column (6), we drop the deregulation index and replace it by the post-deregulation trend. This specification amounts to fitting a continuous piecewise linear function with a change in slope and no jump at the time of deregulation. We find that the change is trend is negative and significant at the 1% level: following deregulation, the number of innovators declines by 0.83% every year. In column (7), we add the deregulation index, which amounts to allowing for a jump at the time of deregulation. In this case, we find that an insignificant downward jump at the time of deregulation and a subsequent decline of 0.63% per year, significant at the 1% level.

1.5.3 Alternative explanations

In this section, we investigate two alternative explanations for our results. First, Chava et al. (2013) argue that intrastate deregulation leads to an increase in banks' bargaining power due to a weakening of banking competition, which results in a reduction in credit to small firms. To disentangle between this explanation and ours, we note that Chava et al.'s (2013) mechanism operates through a weakening of banking competition, whereas our mechanism operates through an increase in banking competition.[25] To test this competing hypothesis, we follow the methodology of Black and Strahan (2002). The idea is to allow the effect of deregulation to vary with the level of concentration in local banking markets. If the effect of deregulation operates through an increase in banking competition, then it should be stronger in states whose banking markets are more concentrated prior to deregulation, because if the market is already very competitive, the additional increase in competition will have a small impact. Conversely, if the effect of deregulation operates through a decrease in competition, then it should be weaker in states whose banking markets are more concentrated. Following Black and Strahan (2002), we use the Herfindahl index (HHI) of concentration in local markets to proxy for the degree of banking competition at the state level.[26]

[INSERT TABLE 1.11 ABOUT HERE]

[25] Previous studies of intrastate branching deregulation (e.g., Jayaratne and Strahan 1998, Black and Strahan 2002, Stiroh and Strahan 2003, and Cetorelli and Strahan 2006) show that it increases competition.

[26] The local HHI equals the sum of squared deposit market shares across all banks operating in a metropolitan statistical area (MSA). For states with more than one MSA, the local HHI is averaged across all MSAs, weighted by total deposits in each MSA. We thank Phil Strahan for sharing the data with us.

In Table 1.11, column (1), the coefficient on the interaction term between deregulation and HHI is negative and significant. Therefore, the drop in the number of innovators following deregulation is stronger in states with more concentrated local banking markets. In states with low concentration (HHI around 0.1), the effect of deregulation is slightly negative but not significant. Conversely, in states with concentrated local markets (HHI around 0.3), the effect is large and significant ($0.028 - 0.67 \times 0.3 = -17.3\%$). These findings are consistent with our interpretation that deregulation operates through an increase in banking competition, which hurts lending relationships.

Another potential channel through which deregulation may affect innovation is via the M&A market. One possibility is that entrepreneurs innovate in order to sell their startup to a large corporation. If post-deregulation financing is easier to access, then entrepreneurs will have fewer incentives to innovate as they will be able to grow internally. This explanation would also predict that the number of takeovers in innovative industries decreases following deregulation. To test this prediction, we proxy for the number of takeovers by large corporations by using the number of acquisitions made by firms in Compustat operating in innovative sectors. The tests and results are reported in Appendix 1.8.4: depending on the specification, we find that takeover activity either increases or does not change significantly after deregulation. These findings are not consistent with the incentive explanation. Instead, the weak increase in takeover activity is consistent with our interpretation that credit becomes more difficult to access for innovative startups, whose only possibility to grow may be to be taken over.[27]

[27] Another reason why M&A activity increases might be that takeover demand increases. If this is the case, Phillips and Zhdanov (2013) show that it would increase potential targets' incentives to innovate, which would lead us to underestimate the negative effect of deregulation on innovation.

1.5.4 Robustness checks

We run a battery of robustness checks reported in Table 1.11. First, we perform a "placebo" test by re-assigning randomly the deregulation dates to the different states. In column (2), we re-run our baseline regression and find no effect of these fake deregulations.

We also control for the interstate banking reforms that took place during the 1980s. We define a dummy variable equal to one after a state permits interstate banking, that is, after a state allows banks from other states to buy its banks. First, in columns (3), we find that the effect of intrastate deregulation is not affected when we control for interstate deregulation. Second, interstate deregulation has a small positive but insignificant effect. A possible interpretation is that interstate deregulation has two opposite effects that cancel each other out: on the one hand it damages lending relationships, which tends to stifle innovation as in our paper; on the other hand it allows banks to diversify geographically, which enables banks to fund more risky projects and fosters innovation as in Amore, Schneider, and Zaldokas (2013).

We check whether our results are driven by the choice of a linear deregulation index. First, we replace the index between zero and three by a deregulation dummy that equals one if the state has implemented any step of deregulation, that is, if the index is strictly larger than zero. We show in column (4) that the effect of moving from full deregulation (deregulation dummy equal to zero) to any level of deregulation (deregulation dummy equal to one) reduces the number of innovators by a significant 15%. Second, we replace the deregulation index by three dummy variables for every level of the index between one and three (an index of zero being the reference group). We show in column (5) that the effect of deregulation is monotonic in the intensity of deregulation: an index of one decreases the number of innovators by a significant 12%, an index of two by 19%, and an index of three by 25%. Although the effect is not perfectly linear, this result suggests that the linear specification is a reasonable approximation.

Finally, we check that our results are not driven by the most innovative industries or by the most innovative states. Our results are mostly unchanged when we exclude the most innovative industries (columns (6)) and when we exclude the most innovative states (columns (7)).

1.6 Conclusion

We provide insight into the drivers of innovation and the capacity of an economy to finance its innovative sectors. Employing patent-level and inventor-level data, we show that damaging lending relationships has an adverse effect on the innovation process, which in turn, leads to a labor market reallocation of inventors across firms and states. This suggests that the ability of banks to deal with soft information is key to alleviating information frictions, especially for opaque borrowers like innovative start-ups. While the increase in competition for lending reduces financial constraints for the average firm (Jayaratne and Strahan 1998; Black and Strahan 2002; Cetorelli and Strahan 2006; Kerr and Nanda 2009), which invests in more tangible projects, our results show that it also leads to further tightening of financial constraints for innovative firms. Taken together, these findings indicate that banking deregulation may reshape comparative advantages away from innovative sectors and towards more tangible sectors. This shift in specialization may not necessarily slow down growth, especially in the short run. However, given that innovation generates spillovers, the reshaping of comparative advantages might impede long-run growth.

For instance, our results can shed some light on the drivers of comparative advantages around the world. One puzzle in Europe is why France lags behind Germany in high technology sectors. The structure of their respective banking markets offers a potential explanation. Whereas France is dominated by a small number of large national banks, Germany is characterized by multiple regional banks that have close relationships with their debtors.

In term of public policies, if one accepts that the lesson drawn from commercial banking extends to public funding, then governments willing to support innovation by allocating public funds should not rely on a centralized and hierarchical structure, but on local agencies that more able to deal with soft information.

Finally, we provide evidence that banking markets play a role in the reallocation of skilled human capital across the economy, which may have implications for local productivity, knowledge spillovers, and urban agglomeration (Moretti 2012). Given that governments have become increasingly aware of the importance to win the "global race for inventors' brains" (Fink, Miguelez, and Raffo 2013), understanding the determinants of skilled human capital migration has important policy implications.

References

Acharya, V. and K. Subramanian, 2009, "Bankruptcy Codes and Innovation," *Review of Financial Studies* 22, 4949-4988.

Almeida, P. and B. Kogut, 1999, "Localization of Knowledge and the Mobility of Engineers in Regional Networks," *Management Science* 45, 905-917.

Almeida, H. and M. Campello, 2007, "Financial Constraints, Asset Tangibility, and Corporate Investment," *Review of Financial Studies* 20, 1429-1460.

Amore, M., C. Schneider, and A. Zaldokas, 2013, "Credit Supply and Corporate Innovation," *Journal of Financial Economics* 109, 835-855.

Agrawal, A., I. Cockburn, and J. McHale, 2006, "Gone But Not Forgotten: Knowledge Flows, Labor Mobility, and Enduring Social Relationships," *Journal of Economic Geography* 6, 571-591.

Benfratello, L., F. Schiantarelli, and A. Sembenelli, 2008, "Banks and Innovation: Microeconometric Evidence on Italian Firms", *Journal of Financial Economics* 90, 197-217.

Berger, A., N. Miller, M. Petersen, R. Rajan, and J. Stein, 2005, "Does Function Follow Organizational Form? Evidence From the Lending Practices of Large and Small Banks," *Journal of Financial Economics* 76, 237-269.

Berger, A., and G. Udell, 1995, "Relationship Lending and Lines of Credit in Small Firm Finance," *Journal of Business* 68, 351-81.

Breschi, S. and F. Lissoni, 2009, "Mobility of Skilled Workers and Co-Invention Networks: An Anatomy of Localized Knowledge Flows," *Journal of Economic Geography* 9, 439-468.

Black, S. and P. Strahan, 2001, "The Division of Spoils: Rent-Sharing and Discrimination in a Regulated Industry," *American Economic Review* 91, 814-831.

Black, S. and P. Strahan, 2002, "Entrepreneurship and Bank Credit Availability," *Journal of Finance* 57, 2807-2833.

Borjas, G., S. Bronars, and S. Trejo, 1992, "Self-Selection and Internal Migration in the United States," *Journal of Urban Economics* 32, 159-185.

Boustan, L., P. Fishback, and S. Kantor, 2010, "The Effect of Internal Migration on Local Labor Markets: American Cities during the Great Depression," *Journal of Labor Economics* 28, 719-746.

Brown, J., G. Martinsson, and B. Petersen, 2013, "Law, Stock Markets, and Innovation," *Journal of Finance* 68, 1517-1549.

Cetorelli, N. and P. Strahan, 2006, "Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local U.S. Markets," *Journal of Finance* 61, 437-461.

Chaney, T., D. Sraer, and D. Thesmar, 2012, "The Collateral Channel: How Real Estate Shocks Affect Corporate Investment," *American Economic Review* 102, 2381-2409.

Chava, S., A. Oettl, A. Subramanian, and K. Subramanian, 2013, "Banking Deregulation and Innovation," *Journal of Financial Economics* 109, 759-774.

Cornaggia, J., Y. Mao, X. Tian, and B. Wolfe, 2013, "Does Banking Competition Affect Innovation?" *forthcoming Journal of Financial Economics.*

Detragiache, E., T. Tressel, and P. Gupta, 2008, "Foreign Banks in Poor Countries: Theory and Evidence," *Journal of Finance* 63, 2123-2160.

Docquier, F. and H. Rapoport, 2012, "Globalization, Brain Drain and Development," *Journal of Economic Literature* 50, 681-730.

Fink, C., E. Miguelez, and J. Raffo, 2013, "The Global Race For Inventors' Brains," *Working Paper.*

Griliches, Z., 1990, "Patent Statistics as Economic Indicators: A Survey," *Journal of Economic Literature* 28, 1661-1707.

Hall, B., A. Jaffe, and M. Trajtenberg, 2001, "The NBER Patent Citations Data File: Lessons, Insights and Methodological Tools," *NBER Working Paper.*

Hall, B., and J. Lerner, 2010, "The Financing of R&D and Innovation," *Handbook of the Economics of Innovation*, North-Holland Elsevier Publishers, Amsterdam.

Hausman J., B. Hall, and Z. Griliches, 1984, "Econometric Models for Count Data and an Application to the Patents-R&D Relationship," *Econometrica* 52, 909-938.

Hellmann, T., L. Lindsey, and M. Puri, 2008, "Building Relationships Early: Banks in Venture Capital," *Review of Financial Studies* 21, 513-541.

Jayaratne, J. and P. Strahan, 1998, "The Finance-Growth Nexus: Evidence from Bank Branch Deregulation," *Quarterly Journal of Economics* 111, 639-670.

Kerr, W. and R. Nanda, 2009, "Democratizing Entry: Banking Deregulations, Financing Constraints, and Entrepreneurship," *Journal of Financial Economics* 94, 124-149.

Kiefer, N., 1988, "Economic Duration Data and Hazard Functions," *Journal of Economic Literature* 26, 646-679.

Kroszner, R. and P. Strahan, 1999, "What Drives Deregulation? Economics and Politics of the Relaxation of Bank Branching Restrictions," *Quarterly Journal of Economics* 114, 1437-67.

Kroszner, R. and P. Strahan, 2011, "Regulation and Deregulation of the U.S. Banking Industry: Causes, Consequences and Implications for the Future," NBER Chapters in: *Economic Regulation and Its Reform: What Have We Learned? National Bureau of Economic Research, Inc.*

Lai, R., A. D'Amour, and L. Fleming, 2009, "The Careers and Co-Authorship Networks of U.S. Patent-Holders, Since 1975," *Harvard Institute for Quantitative Social Science.*

Landier, A., V. Nair, and J. Wulf, 2009, "Trade-offs in Staying Close: Corporate Decision Making and Geographic Dispersion," *Review of Financial Studies* 22, 1119-1148.

Lerner, J., M. Sorensen, and P. StrÃ¶mberg, 2011, "Private Equity and Long-Run Investment: The Case of Innovation," *Journal of Finance* 66, 445-477.

Mann, W., 2013, "Creditor Rights and Innovation: Evidence from Patent Collateral," *Working Paper.*

Marx, M., D. Strumsky, and L. Fleming, 2009, "Mobility, Skills, and the Michigan Non-Compete Experiment," *Management Science* 55, 875-889.

Moretti, E., 2012, *The New Geography of Jobs,* ed. Houghton Mifflin Harcourt.

Nanda, R., and T. Nicholas, 2013, "Did Bank Distress Stifle Innovation During the Great Depression?" *forthcoming Journal of Financial Economics.*

Petersen, M., and R. Rajan, 1994, "The Benefits of Lender Relationships: Evidence from Small Business Data", *Journal of Finance* 49, 3-37.

Petersen, M., and R. Rajan, 1995, "The Effect of Credit Market Competition on Lending Relationships," *Quarterly Journal of Economics* 110, 407-443.

Petersen, M., and R. Rajan, 2002, "Does Distance Still Matter? The Information Revolution in Small Business Lending," *Journal of Finance* 57, 2533-2570.

Phillips G., and A. Zhdanov, 2013, "R&D and the Incentives from Merger and Acquisition Activity," *Review of Financial Studies*, 26 34-78.

Rajan, R., 1992, "Insiders and Outsiders: The Choice between Informed and Arm's-Length Debt," *Journal of Finance* 47, 1367-1400.

Rajan, R., and L. Zingales, 1998, "Financial Dependence and Growth',' *American Economic Review* 88, 559-686.

Rajan, R., and L. Zingales, 2003, *Saving Capitalism from the Capitalists*, Random House, New York.

Roberts, M., and T. Whited, 2012, "Endogeneity in Empirical Corporate Finance," *Simon School Working Paper.*

Schumpeter, J., 1942, *Capitalism, Socialism, and Democracy,* Harper & Row, New York.

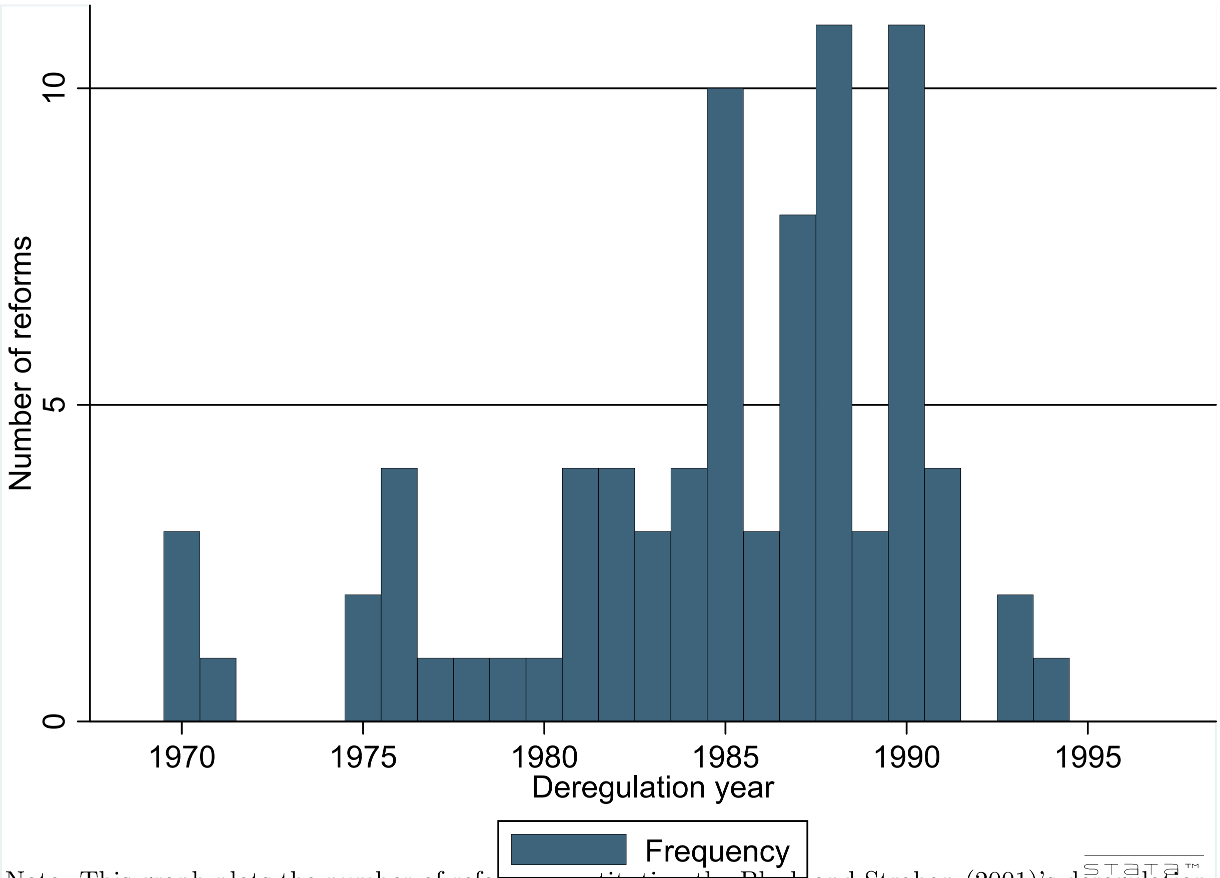
Stein, J., 2002, "Information Production and Capital Allocation: Decentralized vs. Hierarchical Firms," *Journal of Finance* 57, 1891-1921.

Stiroh, K., and P. Strahan, 2003, "Competitive Dynamics of Deregulation: Evidence from U.S. Banking," *Journal of Money, Credit and Banking* 35, 801-828.

Zarutskie, R., 2006, "Evidence on the Effects of Bank Competition on Firm Borrowing and Investment," *Journal of Financial Economics* 81, 503-537.

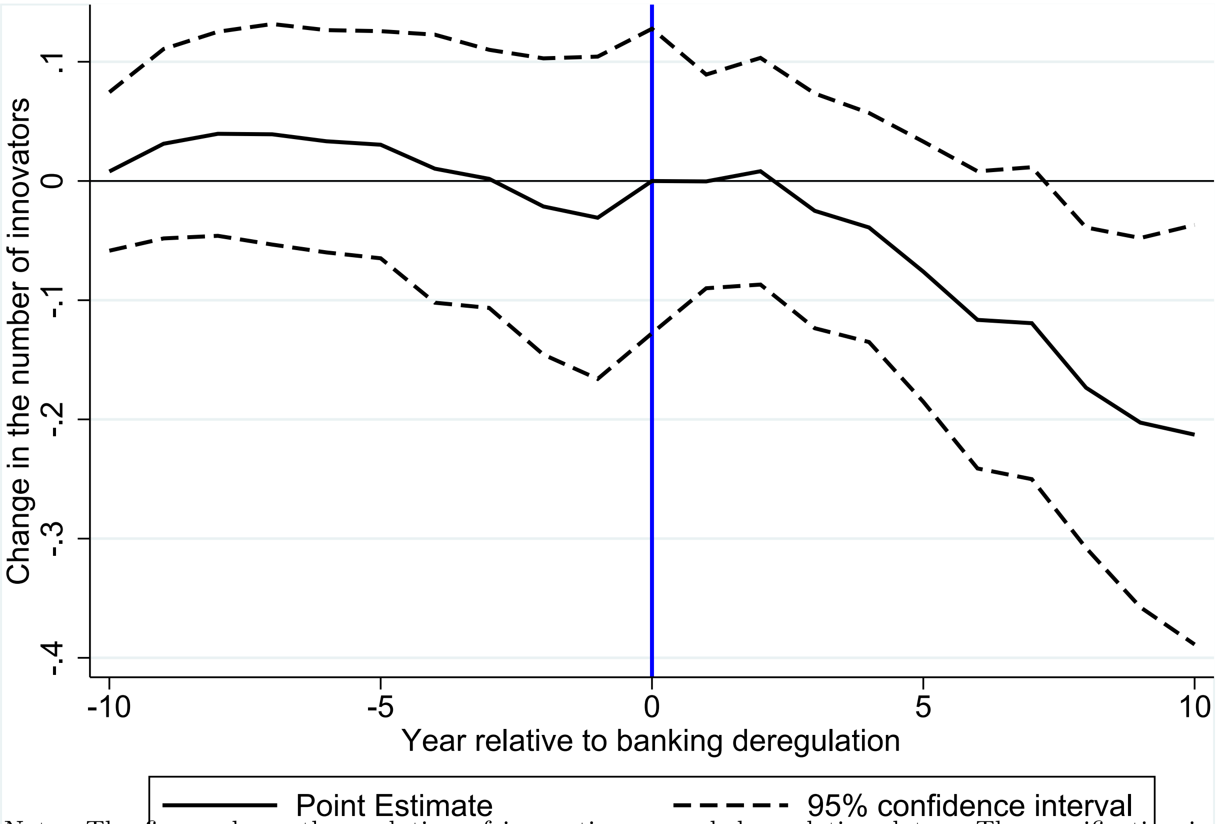
1.7 Figures and Tables

Figure 1.1: Timing of intrastate deregulation



Note: This graph plots the number of reforms constituting the Black and Strahan (2001)'s deregulation index that took place each year of the sample period. The reforms constituting the deregulation index are: a state allows the formation of multibank holding companies; branching by M&As; unrestricted (de novo) branching.

Figure 1.2: Effect of banking deregulation on innovation



Note: The figure shows the evolution of innovation around deregulation dates. The specification is the same as equation (1.1) except that the deregulation index is replaced by a collection of variables $\sum_{s=1}^{3} I^s(k)$, where $I^s(k)$ is a dummy equal to one exactly k years after (or before if k is negative) the state implements a given step of deregulation $s \in \{1, 2, 3\}$. The solid line plots the point estimates for $k = -10, \ldots, 10$, using the deregulation year $k = 0$ as the reference year. The dashed lines plot the 95% confidence interval and standard errors are clustered at the state level.

Table 1.1: Summary statistics: Number of innovators per industry

	Obs.	Mean	Std.Dev.	25^{th}	50^{th}	75^{th}
Panel A: All industries						
	57,350	5.6	13	0	1	5
Panel B: By industry						
Agriculture, food, and textiles	1,550	1	1.6	0	0	1
Coating	1,550	3.1	4.8	0	1	4
Gas	1,550	1.3	2	0	1	2
Organic compounds	1,550	2.6	5	0	1	3
Resins	1,550	3.8	5.8	0	1	5
Other chemical	1,550	20	28	3	9	24
Communications	1,550	9.2	20	1	3	10
Computer hardware and software	1,550	5.4	17	0	1	5
Computer peripherials	1,550	1.6	6.4	0	0	1
Information storage	1,550	2.2	8.8	0	0	1
Other computers and communications	1,550	1.3	5.2	0	0	1
Drugs	1,550	6.7	18	0	1	5
Surgery and medical instruments	1,550	8	19	0	2	9
Biotechnology	1,550	.34	.97	0	0	0
Other drugs and medical	1,550	1.5	3.8	0	0	2
Electrical devices	1,550	6.6	11	0	2	7
Electrical lighting	1,550	2.9	6.2	0	1	3
Measuring and testing	1,550	7	12	1	3	8
Nuclear and X-rays	1,550	2.6	5.9	0	1	3
Power systems	1,550	6.1	9.9	0	2	8
Semiconductor devices	1,550	1.4	6.1	0	0	1
Other electrical and electronic	1,550	4.6	8.2	0	2	5
Material processing and handling	1,550	14	19	2	7	19
Metal working	1,550	7.2	11	1	3	8
Motors and engines	1,550	6.2	9.4	0	2	8
Optics	1,550	2.1	5.1	0	0	2
Transportation	1,550	6.2	9.1	1	3	8
Other mechanical	1,550	13	19	2	6	17
Agriculture, husbandry, and food	1,550	5.8	7.6	1	3	8
Amusement devices	1,550	2.4	4.6	0	1	3
Apparel and textile	1,550	3.8	5.4	0	2	5
Earth working and wells	1,550	3.8	8.3	0	1	4
Furniture and house fixtures	1,550	6	8.7	0	2	8
Heating	1,550	3.7	4.9	0	2	5
Pipes and joints	1,550	2.9	4.8	0	1	3
Receptacles	1,550	6.5	9.6	1	3	9
Miscellaneous	1,550	24	33	3	10	32

Note: Panel A reports summary statistics on the number of innovators at the state-year-industry level across all industries. Panel B reports the same statistics by industry.

Table 1.2: Number of innovators: the effect of banking deregulation and relationship dependence

	Poisson model: Number of innovators					
				Relationship dependence proxy		
				Distance with lender	Increase in distance with lender	Length of relationship with lender
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation	-.097***	-.071**		-.054	-.057	-.024
	(.037)	(.036)		(.033)	(.036)	(.025)
Deregulation (≤t-5)			.024			
			(.032)			
Deregulation (t-4,t-1)			-.015			
			(.018)			
Deregulation (t+1,t+4)			-.026***			
			(.0096)			
Deregulation (≥t+5)			-.09***			
			(.024)			
Deregulation × Relationship dependence				-.038***	-.029***	-.091***
				(.011)	(.01)	(.024)
College graduates		.51***	.54***	.52***	.53***	.52***
		(.2)	(.19)	(.18)	(.19)	(.17)
PhD graduates		.44**	.36**	.35**	.33*	.35**
		(.18)	(.15)	(.17)	(.19)	(.16)
R&D federal expenses		.045	.046	.035	.012	.08*
		(.042)	(.044)	(.036)	(.042)	(.043)
VC funds		.027**	.028**	.027**	.019	.022*
		(.013)	(.013)	(.013)	(.014)	(.013)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,350	57,350	57,350	57,350	57,350	57,350
Pseudo-R2	.74	.74	.74	.75	.75	.75

Note: 50 U.S. states, 37 industries, 1968-1998. We estimate a Poisson model in which the dependent variable is the number of innovators at the state-industry-year level. All regressions include state, year and industry fixed effects. In column (1) the only explanatory variable is the deregulation index which ranges from zero (full regulation) to three (full deregulation). In column (2) we add the state-year-level numbers of college degrees granted, doctorates granted, amount of federal R&D spending, and dollar amount of invested VC capital. In column (3) we split the deregulation index into four sub-periods. In columns (5) to (7) we interact the deregulation index, state and year fixed effects, and control variables with a dummy variable equal to one if industry relationship dependence is above median. In column (5) relationship dependence is proxied by (minus) average distance from main lender; in column (6) it is proxied by (minus) average change in distance from main lender; in column (7) it is proxied by average length of relationship with main lender. Standard errors are clustered at the state level.

Table 1.3: Number of innovators: the effect of sensitivity to credit supply

	Poisson model: Number of innovators							
		Relationship dependence proxy				Relationship dependence proxy		
		Distance with lender	Increase in distance with lender	Length of relationship with lender		Distance with lender	Increase in distance with lender	Length of relationship with lender
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deregulation	-.082**	-.066	-.062	-.054	-.28***	-.29**	-.19**	-.064
	(.04)	(.042)	(.038)	(.055)	(.11)	(.12)	(.079)	(.07)
Deregulation × Financial dependence	-.13**	-.099*	-.086**	-.065				
	(.053)	(.052)	(.039)	(.073)				
Deregulation × Relationship dependence		-.04***	-.057***	-.016		-.23*	-.32*	-.4**
		(.013)	(.014)	(.031)		(.13)	(.17)	(.19)
Deregulation × Relationship dependence × Financial dependence		-.12**	-.13**	-.0073				
		(.048)	(.056)	(.05)				
Deregulation × Asset tangibility					.73***	.76**	.47**	.12
					(.27)	(.31)	(.23)	(.24)
Deregulation × Relationship dependence × Asset tangibility						.78*	1*	1.2**
						(.46)	(.57)	(.59)
State-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,350	57,350	57,350	57,350	57,350	57,350	57,350	57,350
Pseudo-R2	.76	.77	.77	.77	.77	.77	.77	.77

Note: 50 U.S. states, 37 industries, 1968-1998. We estimate a Poisson model in which the dependent variable is the number of innovators at the state-industry-year level. All regressions include the deregulation index, state, year and industry fixed effects, and the same set of state-year control variables as in Table 1.2. In column (1) the deregulation index, state and year fixed effects, and control variables are interacted with industry financial dependence. In columns (2) to (4) they are also interacted with industry relationship dependence and with relationship dependence times financial dependence. In column (2) relationship dependence is proxied by (minus) average distance from main lender; in column (3) it is proxied by (minus) average change in distance from main lender; in column (4) it is proxied by average length of relationship with main lender. In columns (5) to (8) financial dependence is replace by asset tangibility. Standard errors are clustered at the state level.

Table 1.4: Number of innovators: the effect of age

	Poisson model: Number of innovators in age category		
	(1)	(2)	(3)
Deregulation	-.042	-.002	.024
	(.045)	(.029)	(.036)
Deregulation × Young firm	-.051*		-.049**
	(.027)		(.024)
Deregulation × Young industry		-.075***	-.079***
		(.017)	(.017)
State-year controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	107,300	107,300	107,300
Pseudo-R2	.69	.46	.69

Note: 50 U.S. states, 37 industries, 1970-1998. We estimate a Poisson model in which the dependent variable is the number of innovators at the state-industry-year-age category level, where the two age categories are young innovators (less than or equal to three years) and old innovators (more than three years). All regressions include the deregulation index, state, year and industry fixed effects, and the same set of state-year control variables as in Table 1.2. In column (1) the deregulation index, state and year fixed effects, and control variables are interacted with the young innovator category dummy. In column (2) they are interacted with the with the young industry dummy. In columns (3) they are interacted with both the young innovator category dummy and the young industry dummy. Standard errors are clustered at the state level.

Table 1.5: Number of innovators: the effect of lenders' information set

	Poisson model: Number of innovators whose patent portfolio is					
	high quality ($\geq$1 citations)	low quality (0 citations)		transparent (low originality)	opaque (high originality)	
	(1)	(2)	(2)−(1)	(3)	(4)	(4)−(3)
Deregulation	-.042	-.088**	-.045***	-.054**	-.082***	-.041*
	(.033)	(.037)	(.016)	(.024)	(.031)	(.025)
State-year controls	Yes	Yes		Yes	Yes	
State FE	Yes	Yes		Yes	Yes	
Year FE	Yes	Yes		Yes	Yes	
Industry FE	Yes	Yes		Yes	Yes	
Observations	42,550	42,550		42,550	42,550	

Note: 50 U.S. states, 37 industries, 1976-1998. We estimate Poisson models at the state-industry-year level. All regressions include the deregulation index, state, year and industry fixed effects, and the same set of state-year control variables as in Table 1.2. In column (1) the dependent variable is the number of innovators with zero past citations; in column (2) it is the number of innovators whose patents have already received at least one citation. In column (3) the dependent is the number of innovators with patent portfolio originality below median; in column (4) it is the number of innovators with patent portfolio originality above median. Standard errors are clustered at the state level.

Table 1.6: Firm-level employment composition

	Fraction of young inventors		Fraction of promising inventors		Fraction of young promising inventors	
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation	.0086***	.014***	-.0029	.0043	.0015	.0077***
	(.0024)	(.0036)	(.0042)	(.0027)	(.0014)	(.002)
Small firm	.0095*	-.054***	-.0047	.14***	-.0066**	.051***
	(.0047)	(.008)	(.0052)	(.0076)	(.0031)	(.0049)
Deregulation × Small firm	-.0084***	-.024***	-.01***	-.025***	-.0085***	-.021***
	(.0021)	(.0042)	(.002)	(.0029)	(.0011)	(.0021)
State-year controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes
Observations	305,034	305,034	305,034	305,034	305,034	305,034
Adjusted-R2	.026	.21	.038	.24	.023	.074

Note: Inventor data compiled by Lai, D'Amour, and Fleming (2009), 1975-1998. We estimate OLS models at the firm-year level. All regressions include the deregulation index, a small firm dummy equal to one if firm size (measured by number of employed inventors) is below median size, the interaction between the deregulation index and the small firm dummy, state, year and industry fixed effects, and the same set of state-year control variables as in Table 1.2. Even-numbered columns also include firm fixed effects. In columns (1) and (2) the dependent variable is the fraction of inventors employed by the firm whose age is below median age in the population of inventors ("young inventors"). In columns (3) and (4) the dependent variable is the fraction of inventors employed by the firm who will file in the rest of their career a number of patents larger than median ("promising inventors"). In columns (5) and (6) the dependent variable is the fraction of inventors employed by the firm who are both young and promising. Standard errors are clustered at the state level.

Table 1.7: Inventor mobility within state

	Within state move dummy *(All inventors)*	New employer size/ Previous employer size *(Only moving inventors)*			
	(1)	(2)	(3)	(4)	(5)
Deregulation	.013	.081**	.02	.06*	.049
	(.0089)	(.031)	(.03)	(.034)	(.032)
Deregulation × Small previous employer	.025***				
	(.0074)				
Deregulation × Young inventor			.046***		
			(.016)		
Deregulation × Promising inventor				.051***	
				(.017)	
Deregulation × Young promising inventor					.059***
					(.017)
Small previous employer	.062***				
	(.02)				
Young inventor			.08*		
			(.041)		
Promising inventor				.014	
				(.037)	
Young promising inventor					.08*
					(.043)
Inventor-year controls	Yes	Yes	Yes	Yes	Yes
State-year controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	577,401	75,962	75,962	75,962	75,962
Adjusted-R2	.08	.25	.25	.25	.25

Note: Inventor data compiled by Lai, D'Amour, and Fleming (2009), 1975-1998. We estimate OLS models at the inventor-year level. All regressions include state, year and industry fixed effects, the same set of state-year control variables as in Table 1.2, inventor age, and indicator variables for the inventor's main technology class. In column (1) we regress a dummy variable equal to one if the inventor moves to another employer in the same state as her previous employer on the deregulation index, a small employer dummy equal to one if employer size (measured by number of employed inventors) is below median size, and the interaction between the deregulation index and the small employer dummy. In columns (2) to (4) we restrict the sample to observations corresponding to a move to another state. In column (2) we regress the ratio of new employer size over previous employer size on the deregulation index. In column (3) we add a young inventor dummy equal to one if inventor age is below median and the interaction term between the deregulation index and the young inventor dummy. In column (4) we replace the young inventor dummy by a promising inventor dummy equal to one if the inventor will file in the rest of her career a number of patents larger than median. In column (5) we use instead a young promising inventor dummy equal to one if the inventor is both young and promising. Standard errors are clustered at the state level.

Table 1.8: Inventor mobility out-of-state

	Dependent variable: Out of state move dummy							
			Inventor characteristic:					
			Young inventor		Promising inventor		Young promising inventor	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deregulation	.0095***	.0066**	.0069***	.0053**	.0072**	.005*	.0069**	.0049*
	(.0031)	(.0028)	(.0024)	(.0022)	(.0031)	(.0029)	(.0028)	(.0026)
Deregulation × Small employer		.0056***		.0034		.004**		.0039**
		(.0018)		(.002)		(.0019)		(.0017)
Deregulation × Inventor characteristic			.00012	-.0018*	.0042***	.0025*	.0071***	.0043***
			(.00089)	(.001)	(.00095)	(.0014)	(.0012)	(.0015)
Deregulation × Small employer				.0033*		.004*		.0055*
× Inventor characteristic				(.0018)		(.0021)		(.0027)
Small employer		.0094		.021*		.013		.015
		(.0095)		(.012)		(.012)		(.01)
Inventor characteristic			.0024	-.067	.025***	.03**	.0074	.024*
			(.0058)	(.)	(.0075)	(.012)	(.0097)	(.013)
Small employer × Inventor characteristic				-.019		-.01		-.027
				(.013)		(.014)		(.018)
Inventor-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	577,401	577,401	577,401	577,401	577,401	577,401	577,401	577,401
Adjusted-R2	.026	.028	.031	.032	.027	.028	.029	.031

Note: Inventor data compiled by Lai, D'Amour, and Fleming (2009), 1975-1998. We estimate OLS models at the inventor-year level. In all regression the dependent variable is a dummy variable equal to one if the inventor moves to another employer located in a different state than her previous employer. All regressions include state, year and industry fixed effects, the same set of state-year control variables as in Table 1.2, inventor age, and indicator variables for the inventor's main technology class. In column (1) the explanatory variable of interest is the deregulation index. In column (2) we add a small employer dummy equal to one if employer size (measured by number of employed inventors) is below median size and its interaction with the deregulation index. In column (3) we add instead a young inventor dummy equal to one if inventor age is below median and its interaction with the deregulation index. In column (4) we include the small employer dummy, the young inventor dummy, the interaction between the small employer dummy and the young inventor dummy, and the interaction between each these three variables and the deregulation index. In columns (5) and (6) we replace the young inventor dummy by a promising inventor dummy equal to one if the inventor will file in the rest of her career a number of patents larger than median. In columns (7) and (8) we use instead a young promising inventor dummy equal to one if the inventor is both young and promising. Standard errors are clustered at the state level.

Table 1.9: Determinants of banking deregulation

	Duration model for the time until deregulation							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State characteristics in 1968								
College graduates	0.018				-0.2			
	(0.21)				(0.5)			
PhD graduates	-0.29				-0.5			
	(8.7)				(13)			
R&D federal expenses	-0.18				-0.73			
	(0.19)				(0.45)			
VC funds	0.3				0.35			
	(0.56)				(1.2)			
Predicted patent growth		-0.55			1.7			
		(0.74)			(1.4)			
Initial share of innovative industries			-0.47		-1.9			
			(0.34)		(1.2)			
Pre-period patent growth				-0.3*	-0.53			
				(0.16)	(0.4)			
Time-varying variables								
Innovation growth						-0.068		-0.075
						(0.16)		(0.16)
Share of innovative industries							-0.38	-0.58
							(0.4)	(0.39)
Kroszner-Strahan variables								
Small bank asset share					7.5***			7.1***
					(1.9)			(1.5)
Capital ratio of small banks					7.6			11***
					(6.4)			(4.1)
Relative size of insurance					2.3**			1.7*
					(1)			(0.87)
Small firm share					-7.3**			-4.1*
					(3)			(2.2)
Share of Democrats					0.099			0.14
					(0.11)			(0.11)
Unit banking indicator					0.11			0.19**
					(0.17)			(0.095)
Change in bank insurance power indicator					-0.17			-0.2**
					(0.13)			(0.094)
Type of deregulation								
Multibank holding companies	-0.67***	-0.68***	-0.65***	-0.69***	-0.71***	-0.67***	-0.64***	-0.66***
	(0.14)	(0.13)	(0.13)	(0.12)	(0.15)	(0.13)	(0.13)	(0.15)
M&A branching	-0.38***	-0.38***	-0.38***	-0.38***	-0.39***	-0.38***	-0.37***	-0.38***
	(0.085)	(0.085)	(0.085)	(0.085)	(0.096)	(0.086)	(0.083)	(0.088)
Observations	1,586	1,586	1,586	1,586	1,586	1,586	1,586	1,586
p-value of χ^2	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01

Table 1.9 (continued)

Note: The hazard model is Weibull, where the dependent variable is the log expected time to deregulation. All variables are measured for each state in each year. *Predicted patent growth* is the patent growth over the sample period predicted by the state share of patents in each technology class in 1968. *Initial share of innovative industries* is the share of state GDP in 1968 in industries in the top tercile of the patents/value added ratio. *Pre-period patent growth* is the growth rate in the number of patents filed between 1963 and 1970. *Innovation growth* is the growth rate of the number of innovators. *Share of innovative industries* is the share of state GDP in industries in the top tercile of the patents/value added ratio. *Small bank asset share* is the percent of banking assets in the state held by banks below the median size of banks in each state in each year. *Capital ratio of small banks* is the capital to assets ratio of small banks minus that of large banks. *Relative size of insurance* relative to banking plus insurance in the state is measured as gross state product from insurance divided by gross state product from insurance plus banking. *Small firm share* is the percent of all establishments in the state that have fewer than twenty employees. *Share of Democrats* is the share of the three bodies of state government controlled by Democrats. *Unit banking indicator* equals one for states with unit banking restrictions. *Change in bank insurance power indicator* equals one if the state changed the law to permit banks to sell insurance during the sample period. *Multibank holding companies* equals one for deregulation step permitting the formation of multibank holding companies. *M&A branching* equals one for deregulation step permitting branching by M&As. Standard errors are clustered at the state level.

Table 1.10: Controlling for innovation trends

	Poisson model: Number of innovators						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deregulation	-.061**	-.054**	-.06**	-.075**			-.028
	(.029)	(.026)	(.028)	(.038)			(.031)
Deregulation post-1975					-.074**		
					(.037)		
Post-deregulation trend						-.0083***	-.0068***
						(.0025)	(.0023)
Predicted innovation trend	.04*						
	(.024)						
Initial share of innovative		-.065***					
industries × Time trend		(.019)					
Pre-period patent growth			.029				
× Time trend			(.019)				
Pre-deregulation innovation trend				.23**			
				(.098)			
State-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,350	57,350	57,350	57,350	57,350	57,350	57,350
Pseudo-R2	.73	.73	.73	.73	.73	.73	.73

Note: 50 U.S. states, 37 industries, 1968-1998. We estimate a Poisson model in which the dependent variable is the number of innovators at the state-industry-year level. All regressions include the deregulation index, state, year and industry fixed effects, and the same set of state-year control variables as in Table 1.2. In column (1) we control for the innovation trend predicted by the initial technological specialization of the state (see the text for details). In column (2) we control for the initial share of state GDP in innovative industries times a time trend. In column (3) we control for the pre-deregulation innovation trend. In column (4) we control for the pre-sample period innovation trend. In columns (5) we redefine the deregulation index taking into account deregulation events occurring in 1976 or later only. In column (6) we replace the deregulation index by a post-deregulation trend defined as $\sum_{k=1}^{3}(t > T_k) \times (t - T_k)$, where k indexes the three types of deregulation and T_k is the deregulation year. In column (7) we include both the deregulation index and post-deregulation trend. Standard errors are clustered at the state level.

Table 1.11: Robustness tests

	Poisson model: Number of innovators						
	Local banking competition	Random deregulation dates	Interstate banking deregulation	Deregulation dummy	Non-parametric index	Excl. most innovative industries	Excl. most innovative states
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deregulation	.028	.0078	-.072**			-.085**	-.06*
	(.057)	(.043)	(.036)			(.042)	(.038)
Deregulation × Local banking market HHI	-.67*						
	(.41)						
Interstate banking			.013				
			(.021)				
Deregulation index>0				-.15**			
				(.068)			
Deregulation index=1					-.12*		
					(.069)		
Deregulation index=2					-.19**		
					(.08)		
Deregulation index=3					-.25**		
					(.1)		
State-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,350	57,350	57,350	57,350	57,350	51,150	49,321
Pseudo-R2	.71	.73	.73	.73	.73	.66	.65

Note: 50 U.S. states, 37 industries, 1968-1998. We estimate a Poisson model in which the dependent variable is the number of innovators at the state-industry-year level. All regressions include state, year and industry fixed effects, and the same set of state-year control variables as in Table 1.2. In column (1) we include the deregulation index and its interaction with the Herfindahl index of concentration in local banking markets. In column (2) we use a "placebo" deregulation index computed from random deregulation dates. In column (3) we include the deregulation index and a dummy variable equal to one if the state has started to implement interstate banking deregulation. In column (4) we use a dummy variable equals to one if the deregulation index is nonzero. In column (5) we use dummy variables for each value of the deregulation index. In column (6) we exclude "Other mechanical", "Material processing and handling", "Other chemical" and "Miscellaneous industries". In column (7) we exclude California, Massachusetts, New York, Ohio, Pennsylvania, New Jersey and Texas. Standard errors are clustered at the state level.

1.8 Appendix: Construction of variables

1.8.1 Relationship dependence

We use the National Survey of Small Business Finances (NSSBF) which is available on the Fed website. The first proxy of relationship dependence is the average distance from the main lender in the 1987 survey (variable r6481) by two-digit SIC industry. The second proxy is the growth rate of the average distance from the main lender between 1987 (variable r6481 in 1987 survey) and 1998 (variable idist1 in 1998 survey). The third proxy is the average length of relationship with the main lender. Note that length of relationship is mechanically correlated with firm age, since only an old firm can already have a long-standing relationship with its bank. Besides, we know from Table 1.4 that deregulation has a stronger effect on younger firms. Therefore, if we want to assess the effect of length of relationship, we need to filter out the age component from that variable. To do that, we regress log of length of relationship (variable r3311 in the 1987 survey) on log of firm age (1987 minus foundation year, variable r1008) at the firm-level: $\log(Length_i) = a + b.\log(Age_i) + \varepsilon_i$, and we compute the age-adjusted length of relationship as $\log(Length_i^{Adj}) = \log(Length_i) - \hat{b}.(\log(Age_i) - \overline{\log(Age)})$ where the upper bar denotes the sample average.

We cannot use directly these variables in our regressions because patent data use a different industry classification. To map SIC codes into patent technology classes, we merge the NBER Patents File with Compustat (the patent data include the GVKEY of public innovators) and compute, for each patent technology class i, the fraction $w_{i,j}$ of innovators in this technology class that belongs to SIC j. We then compute for each technology class the weighted average of the relationship dependence proxies across all SIC codes where the weights are the $w_{i,j}$'s.

1.8.2 External financial dependence and asset tangibility

We start from Compustat and keep all non-financial firms during the sample period 1968–1998. We compute firm-level external financial dependence as investment (capital expenditure (item #128) + R&D expenses (item #129) + acquisitions using cash (item #46)) minus ROA (item #13) divided by investment, and we take the mean across all firms and years at the three-digit SIC level. Asset tangibility is defined as property, plant and equipment (item #7) divided by total assets (item #6). We then use the same procedure described in Appendix 1.8.1 to map these variables into the patent technology classes that we use in our regressions.

1.8.3 Innovative industries

To define innovative industries, we merge the patents filed by public firms (whose GVKEY is provided in the NBER Patents File) with Compustat to obtain the SIC classification of the innovator. For each two-digit SIC industry and each year, we count the number of patents filed by public firms in this industry over the past five years. We then use correspondence table between the SIC classification and the BEA classification to obtain the number of patents by BEA industry, and we divide by the industry's value added obtained from the BEA data to obtain the innovation intensity at the BEA industry-year level. Therefore, our measure of industry innovation intensity is time-varying to account for the fact that the distribution of patents across industries has changed over time.

1.8.4 Appendix: Deregulation and M&As

In this appendix, we test whether deregulation affects M&A activity in innovative industries. As discussed in Section 1.5.3 of the paper, an alternative interpretation for our results is that, if being a target is the only exit route for innovative firms and the probability of being a takeover target increases with innovation, and if deregulation eases access to credit for entrepreneurs, then entrepreneurs may have fewer incentives to innovate as they will be able to grow internally. This alternative story would also predict that the number of takeovers in innovative industries decreases. We test this prediction using Compustat. We first restrict the sample to the 1968–1998 period and to firms in innovative industries, defined as industries in the top quartile of the R&D/Total assets ratio (i.e., with a ratio above 7.5%). Then, we compute for these firms the dollar amount of acquisitions (variable AQC) normalized by total assets. Because this variable is nonzero for only 20% of firm-year observations, we also construct an acquisition dummy equal to one when the amount of acquisitions is strictly positive. We regress these two variables on the deregulation index as well as on the same set of controls and fixed effects as in our main specification. Results are reported in Table B.1

The coefficient on the deregulation index is positive and significant (column (1)) even after controlling for standard determinants of acquisitions such as size, M/B, ROA, and age (column (2)). When we include firm fixed effects, the coefficient remains positive but becomes insignificant (column (3)). When we use the acquisition dummy instead of the continuous acquisition variable, we now obtain a positive effect of deregulation even when we include firm fixed effects (columns (4) to (6)). Overall, if anything, deregulation increases takeover activity. This result is therefore not consistent with the incentive explanation.

Table B.1: Public firms' acquisitions in innovative industries

	Amount of acquisitions/ Total assets			Dummy amount of acquisitions >0		
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation	.0022***	.0021***	.00089	.026***	.028***	.017**
	(.00044)	(.0004)	(.00062)	(.0088)	(.0077)	(.0084)
College graduates	.0068	.0063	.0062	.075*	.057	.048
	(.0077)	(.0074)	(.0098)	(.044)	(.046)	(.078)
PhD graduates	-.0062	-.0071	-.0014	-.038	-.046	.0054
	(.0045)	(.0042)	(.0064)	(.047)	(.052)	(.067)
R&D federal expenses	.0034***	.0031***	.0021	.03***	.027***	.033**
	(.00099)	(.001)	(.0014)	(.0096)	(.0086)	(.013)
VC funds	.00015	.0003	.00013	-.01	-.0068	-.008
	(.00058)	(.00061)	(.00087)	(.0068)	(.0068)	(.0095)
Log(Assets)		.0028***	.0023***		.059***	.061***
		(.00035)	(.00085)		(.0038)	(.0051)
Market to Book		.0012***	.0025***		.0071***	.015***
		(.00028)	(.00029)		(.0016)	(.0012)
ROA		.017***	-.0065***		.13***	-.014
		(.0015)	(.0023)		(.014)	(.018)
Age		-.00017**	.00071*		.0011	.0098***
		(.000081)	(.00035)		(.00074)	(.0028)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes
Observations	25,731	25,731	25,731	25,731	25,731	25,731
Adjusted-R2	.029	.048	.21	.04	.13	.29

Chapter 2

Unbanked Households: Evidence on Supply-Side Factors

Why Banks are (partially) responsible for the share of high unbanked households

Joint work with Claire Célérier (University of Zürich)

The fact that poor families often rely on informal means to manage their financial lives suggests that the formal sector is not meeting their needs.

National Poverty Center, 2008

2.1 Introduction

There is a large debate about the reasons why so many low-income households - 35 to 45% in the United States - are unbanked, i.e., they possess neither a checking nor a savings account. One question is whether being unbanked is driven by supply- or demand-side factors (see, for instance, Bertrand et al. (2004) or Barr and Blank (2008)). The "demand-side" view attributes the unbanked phenomenon to cultural determinants (the poor may distrust financial institutions or may not have a culture of saving) or to a lack of financial literacy. Alternatively, the "supply-side" view suggests that standard bank practices create hurdles for the poor. Minimum account balances, overdraft fees, a large distance between branches and the proliferation of formal steps to open an account result in costs that may be too high for poor households to manage (Washington (2006), Barr (2008)). Furthermore, bank financial services may not be tailored to low-income households. These two polar explanations have different policy implications. Whereas the demand-side view predicts interventions at the household level through financial literacy programs, for example, the supply-side view suggests that banking regulation, by giving banks incentives to change their behavior, may reduce the share of unbanked households.[1]

[1]We would like to thanks Giorgia Barboni, Martin Brown, Olivier Dessaint, Ralph de Haas, Augustin Landier, Eric Maurin, Thomas Piketty, Jerome Pouyet, Robert Seamans, Mickael Spira, Boris Vallee, Neeltje Van Horen, Ernesto Villanueva Lopez and various participants at the Banque de France Seminar, the Lunch Seminar of Paris School of Economics and the Swiss Conference on Financial Intermediation for their comments. Claire gratefully acknowledge financial support by the University Research Priority Program "Financial Market Regulation" of the University of Zurich.

This paper presents evidence that supply-side factors significantly drive the share of unbanked households among populations with an income below twice the poverty line ("low-income households"). Similar to Rice (2010), we exploit interstate branching deregulation in the U.S. after 1994 as an exogenous shock on bank competition. We combine this shock with micro data on households from the Survey of Income and Program Participation (SIPP) from 1993 to 2010 to identify low-income households with or without a bank account (Washington (2006)). The SIPP focuses on low-income American households. Coupled with its yearly frequency, these data are particularly well suited for our analysis.[2]

Our first set of empirical evidence shows that as bank competition intensifies, the share of unbanked households decreases. We find that interstate branching deregulation is associated with a significant drop in the rate of unbanked households among low-income populations. Figure 2 plots the change in the likelihood of holding a bank account in the years before and after deregulation relative to a control group of states that do not deregulate. We observe a significant increase in the share of banked households following deregulation. Our regressions confirm this result: the share of households with a bank account increases by 4 percentage points after a state fully deregulates.

Moreover, we show that the effect of intensified bank competition is stronger for populations that are *more likely to be rationed by banks.* First, we differentiate states along several measures of discrimination. We find that black households benefit more from interstate-branching deregulation than do non-black households only in states with a history of discrimination. Second, the effect of deregulation is higher at the bottom

[2]Although we are not running a "horse race" between the demand-side and the supply-side views, we focus on a clean supply shock and use a wide array of economic variables at the state and household levels to control for demand factors.

of the income distribution. On average, deregulating results in a 6% increase in the likelihood of holding a bank account for households below the poverty line, whereas it has no significant impact for households with incomes above twice the poverty line. Third, the magnitude of the effect is significantly larger for households living in rural areas, where competition is expected to be lower ex ante. Finally, we differentiate between households with lower and higher levels of education. We find that the effect of deregulation is stronger for more educated households. For these households, being unbanked is less likely to be driven by sophistication because they have relatively high financial literacy.

Finally, we show that having access to bank accounts improves wealth accumulation but does not translate into higher levels of indebtedness. We first find that deregulation increases the share of low-income households with interest-earning assets both in banks and in other financial institutions. Second, we show that owning a bank account improves access to credit without translating into a higher ratio of debt to income, which mitigates the fear that banking competition fosters "predatory lending".

Our results are robust to controlling for the demand-side factors identified in the literature. Banking deregulation, by decreasing unemployment among low-income households through growth (Jayaratne and Strahan (1996)) and easier firm access to credit (Black and Strahan (2001), Rice and Strahan (2010)), may increase *demand* for bank accounts. However, in all of our specifications, we control for a large number of household covariates that capture several dimensions of income, skills and labor status and for main state macroeconomic variables that capture the effect of deregulation on GDP growth or unemployment. We also find that the effect is not higher for households that are more likely to benefit from an increase in revenue due to deregulation.

Our paper contributes to the literature on the determinants of being unbanked. This literature has been scarce mainly as a result of the challenge of disentangling demand-side from supply-side factors (see Barr and Blank (2008) for a broad survey of the literature). Socio-economic characteristics are often noted as the most influential determinants of holding a bank account (Rhine et al. (2006), Barr (2005), Barr et al. (2011), Hogarth and O'Donnell (1999)). However, the effect of socio-economic characteristics may capture both unobserved supply-side or demand-side factors. On the demand side, Kearney et al. (2010) show that by offering a savings account with lottery-like features, banks can motivate the opening of savings accounts. The debate on the determinants of being unbanked also raises the question of the role played by the development of alternative financial services (see, for instance, Morgan and Strain (2008), Melzer (2011), Morse (2011), Morgan et al. (2012), Carrell and Jonathan (2013)).

More generally, our paper relates to the literature that shows the large and positive effect of access to banking accounts on savings rates (Ashraf et al. (2006), Prina (2013) and Schaner (2013)), on investment in preventative health (Dupas and Robinson (2013)) and on starting a business (Dupas and Robinson (2009)). Holding a bank account can also protect households from predatory lending.

Our paper also complements the literature on the impact of bank competition on household finance. This literature has focused on implications in terms of household debt, such as mortgages (Favara and Imbs (2010)) or credit cards (Dick and Lehnert (2010)), but not household savings. A more developed stream of literature has used interstate and intrastate deregulation in the U.S. to investigate the impact of bank competition on the financing of firms (Rice and Strahan (2010), Zarutskie (2006), Cetorelli and Strahan (2006)), economic growth (Jayaratne and Strahan (1996)) and economic volatility (Morgan et al. (2004)).

Finally, our paper adds to the literature that evaluates the effect of intensified competition on racial or gender discrimination. Increased competition has been found to reduce the black-white wage gap in the trucking industry (Peoples and Talley (2001)), in the economy overall (Levine et al. (2012)) and between genders (Black and Strahan (2001)). Our results are also in line with Chatterji and Seamans (2012), who shows that credit card deregulation expanded access to credit, particularly among blacks.[3]

The rest of the paper proceeds as follows. Section 2 provides theoretical explanations for the impact of intensified bank competition on the share of unbanked households. Section 3 describes the data and the empirical strategy. Section 4 presents the results. Section 5 runs various robustness checks. Section 6 concludes.

2.2 Background

2.2.1 Theoretical Discussion

How can bank competition increase the share of households with a bank account among low-income populations or minorities? Several forces are potentially at play.

First, concentration in banking could lead to an excessively low supply of bank accounts and high prices. Low-income households whose level of wealth is low are more likely to be harmed by this lack of competition. By this standard channel, intensified competition should drive prices closer to marginal cost, which should favor low-income

[3]However, as shown by Ouazad and Rancière (2013), the relaxation of credit standards can also lead to more black segregation by giving white households the opportunity to relocate in white neighborhoods Ouazad and Rancière (2013).

households.

Second, intense competition may give banks incentives to cope with the higher costs that low-income households generate. Low-income households are more likely to have low balances, to overdraw accounts, and may require more time from customer services, making them less profitable as consumers. In addition, although there is a fixed cost of opening a bank account, the small amount of these households' savings and loans reduces future expected revenues. When competition is lower, banks have fewer incentives to pay this fixed entry cost.

Third, although expertise in offering services to low-income populations and minorities may be costly, it may partially insulate banks from pure price competition. Therefore, when competition intensifies, banks have more incentives to invest in this expertise.[4] Moreover, intensified competition can lead to an increase in market size of the most efficient banks, which decreases the marginal cost of acquiring expertise and hence fosters bank specialization.

Fourth, increased bank competition may result from a decrease in entry barriers. When entry barriers decrease, banks that already have expertise in offering services to low-income populations or minorities can enter new markets in which such expertise is lacking. For example, bank expertise in offering bank accounts to black households may be low in states with a history of discrimination. The entry of specialized banks into these markets would induce a decrease in the share of unbanked households among black populations.

Beyond profit-maximizing reasons for why intensified bank competition may be asso-

[4] A similar mechanism is described in Boot and Thakor (2000). In their model, banks invest in acquiring expertise in the market for loans through relationship banking.

ciated with a decrease in the share of unbanked households, competition may also reduce "taste-based" discrimination toward minorities. In a seminal work published in 1957, Gary Becker argues that over the long run, competition drives discrimination out of the market-place. The application of Becker's model to the market for financial services can be described in relatively general terms: banks with a "taste for discrimination" will forego profits to indulge their desire to offer bank accounts to a specific type of depositor. For example, banks in states with a history of discrimination may offer less than the profit-maximizing number of bank services to black households or to households living in "black areas". This practice in the market for loans has been identified under the term "redlining" and was one of the main reasons for the adoption of the Community Reinvestment Act in 1977. Cohen-Cole (2011) finds some evidence that this practice has even persisted in recent years. Thus, in a perfectly competitive market, non-discriminating banks should gain a cost advantage and ultimately drive discriminating banks out of business, which should result in a decrease in the share of unbanked households among minorities.

2.2.2 Banking Deregulation

Restrictions on interstate banking and branching have their historical roots in the 1789 Constitution (Johnson and Rice (2008)).[5] Although the Constitution prevented states from issuing fiat money and taxing interstate commerce, it gave them the right to charter and regulate banks. Since then, states have used banks as a source of revenue by charging fees for granting charters, levying taxes and owning shares. These revenues

[5]Interstate banking refers to the control by bank holding companies of banks across state lines, whereas interstate branching means that a single bank may operate branches in more than one state without requiring separate capital and corporate structures for each state.

have given states incentives to restrict competition from out-of-state banks and to create local monopolies. In 1927, the McFadden Act implicitly prohibited interstate branching by commercial banks. In the following years, however, bank holding companies were created to circumvent the law and to acquire branches across states. In 1956, the Bank Holding Company Act ended this development, preventing banks from acquiring banks or branches outside their state unless the state of the targeted bank permitted such acquisitions. The first step toward interstate banking came in 1978 when Maine began to allow out-of-state bank holding companies to acquire banks on a reciprocal basis. Other states followed beginning in 1982, but interstate branching was still not allowed until 1994.

In 1994, the Interstate Banking and Branching Efficiency Act (IBBEA), also known as the Riegel-Neal Act, effectively permitted bank holding companies to enter other states and operate branches. However, it also allowed states to erect barriers to out-of-state entry with regard to four dimensions: (i) the minimum age of the targeted bank (5 years, 3 years or less), (ii) de-novo branching without an explicit agreement by state authorities, (iii) the acquisition of individual branches without acquiring the entire bank and (iv) a statewide deposit cap, that is, the total amount of statewide deposits controlled by a single bank or bank holding company. Following the passage of the IBBEA in 1997, states had the opportunity to modify each of these provisions, and many states did so. In fact, 43 states have relaxed the protection of their banking market since then.

Following Rice and Strahan (2010), we construct a deregulation index that ranges from 0 to 4 to capture each dimension of state-level branching restrictions: 0 for fully regulated and 4 for deregulated states. Therefore, an increase in the index value implies greater competition.[6]

Interstate branching deregulation has fostered the development of multi-state banking. As Figure 2.1 shows, not only has the total number of branches increased since 1994, but each local market has also experienced a strong penetration of "out-of-state" branches, which have challenged local incumbents. Analyzing the other dimension of IBBEA, the interstate *banking* deregulation, Dick (2006) finds that it has translated into a dramatic decrease in the number of regional dominant banks and a slight increase in the number of small banks, resulting in a strong appreciation of bank density.

INSERT FIGURE 2.1

2.3 Data and Empirical Strategy

2.3.1 Household Data

Data on households comes from the SIPP and covers the 1993-2010 period. The SIPP is a running panel that collects detailed information about income and demographics for 20,000 to 30,000 households over 2 to 3 years. Most importantly, the SIPP includes topical modules focusing on household asset allocation and the use of banking services. We exploit the data from these topical modules to create a dummy variable *BankAccount* that takes the value 1 if at least one member in the household has either a free checking or savings account, and 0 otherwise.

The large size of the sample allows us to focus on low-income households, i.e., those below 200% of the poverty threshold, which is key for our analysis because low-income

[6] We reverse Rice and Strahan (2010)'s index to facilitate the description of our results

households are more likely to be rationed by banks.[7] We work at the household rather than the individual level because households often pool resources; a bank account in one member's name can provide access to banking services to other members of the same household. We collapse each household observation at the year level. This leaves us with a total sample of 135,524 low-income households living in 45 states plus the District of Columbia over the 1993-2010 period.[8]

Finally, we exploit the very detailed information on socio-demographics that the SIPP provides to control for a large set of variables in our identification strategy. These controls include family type (size of the households, whether the household head is single and female, and whether the head is married), the socio-demographic characteristics of the head of household (age, race, three dummies for education: elementary, high school or college degree, employment status) and the household's economic characteristics (monthly income, dummy for receiving social security, dummy for transfer income).

Based on the SIPP data, we find that 36.3% of low-income households are unbanked in 1993. This rate increases up to more than 40% in 2002. We observe the same increasing trend in the Panel Study of Income Dynamics data (Table 2.12). One potential explanation would be the rapid development of alternative financial services over this period. The 2011 National Survey of Unbanked and Underbanked Households from the FDIC indicates that the proportion of unbanked households has also increased slightly during the recent financial crisis.[9]

[7]The poverty threshold is defined in the SIPP and varies with the number of adults and children in the household and, for some household types, the age of the household head.

[8]To ensure the confidentiality of the data, the SIPP aggregates five states in two groups. These states are: Maine and Vermont (first group) and North Dakota, South Dakota and Wyoming (second group), this explains why we do not have 50 +1 states. Unfortunately, there is a gap in the data between 2006 and 2008 because no topical module on asset allocation was administered during these years.

[9]http://www.fdic.gov/householdsurvey/

Table 2.1 shows the summary statistics for banked and unbanked households in our sample. On average, banked households are less likely to be black and to receive transfer income and are richer than unbanked households.

INSERT TABLE 2.1

2.3.2 Identification Strategy

The baseline model estimates the effect of deregulation on the probability of holding a bank account:

$$P(BankAccount_{ist}) = \alpha + \beta Deregulation_{st} + \theta X_{ist} + \lambda StateControl_{st} + \delta_t + \eta_s + \epsilon_{ist} \quad (2.1)$$

where $BankAccount_{ist}$ equals 1 if household i in state s holds a bank account at time t, $Deregulation_{st}$ is the deregulation index in state s at time t, X_{ist} is a vector of household characteristics, $StateControl_{st}$ are state characteristics and δ_t and η_s are year and state fixed effects, respectively. The controls at the state level come from data from the Bureau of Economic Analysis and include state-level GDP growth, unemployment and a log of the total population. Although our dependent variable is binary, the use of a non-linear model such as probit or logit is not suitable given the numerous fixed effects we are using. Therefore, following Angrist and Pischke (2008) we use a linear probability model.[10][11] Standard errors are clustered at the state level to account for serial correlation within states.

[10] In addition, Angrist and Pischke (2008) argue that once raw coefficients from non-linear estimators are converted to marginal effects, they offer little efficiency or precision gains over linear specifications. The other main advantage of linear probability models is that the coefficient can be interpreted directly in term of percentage points.

[11] Our results still hold in logit regressions

The parameter of interest is β, which measures the incremental effect of one step of deregulation out of four possible steps on the likelihood of holding a bank account. State fixed effects capture time-invariant determinants of access to banking services in each U.S. state, such as the size of the state, the initial structure of the local banking market and the level of education. Year fixed effects control for aggregate shocks and common trends in access to banking services. The identification of ? therefore relies on comparing the probability of a household holding a bank account in a state before and after deregulation relative to a control group of states that do not experience a change in regulation. All the other regressions rely on the same identification strategy.

Table 2.2 reports the estimated coefficients when we regress the *BankAccount* dummy on both the household and state-level control variables. The coefficients have the expected signs. Holding a diploma, whether it is from elementary school, high school or college, increases the likelihood of holding a bank account, whereas being poor decreases it. The coefficient on Black is -0.75, which implies that being black decreases the likelihood of holding a bank account by 16%. Given that we control for many socio-economic determinants, this result may suggest that black households suffer from discrimination. Finally, the coefficients of state-level controls are not significant, which may be explained by the fact that macroeconomic factors do not matter once we control for socio-economic variables at the household level. To save space and facilitate the reading of the results, the coefficients of the control variables are reported only in Table 2.2.

INSERT TABLE 2.2

One concern with our identification strategy is that we may capture the effect of the Community Reinvestment Act (CRA) on unbanked households rather than the effect of banking deregulation. The IBBEA stipulates that meeting the credit needs of communities, as defined by the CRA, is a condition for the operation of interstate branches.[12] However, the CRA's focus on access to credit rather than on access to basic bank accounts alleviates this concern. In addition, even if the CRA had an effect through the IBBEA, our results on the impact of banking deregulation would be even stronger than reported. Indeed, a bank that wants to operate interstate branches in a newly deregulated state must meet the requirements of the CRA *in its home state*. Therefore, the bank may increase the supply of bank accounts to low-income households in its home state (the control state) before entering the newly deregulated state (the treated state).

2.4 Results

2.4.1 Basic Model

We begin by investigating whether and to what extent banking deregulation affects the share of unbanked households.

Table 2.3 reports four versions of our baseline regression, which all indicate a large and positive impact of banking deregulation on the share of banked households. The first column does not include any control. The coefficient on *Deregulation index* is 0.012 and significant at the 1% level. That is, when a state fully deregulates, we observe an increase in the share of households with a bank account of 4.8 percentage points. The second column introduces household controls and the third column introduces time-

[12]The CRA was enacted in 1977 to fight the problem of "redlining" namely, the existence of discrimination in loans and access to banking services to individuals and businesses from low- and moderate-income neighborhoods (see, for instance, Barr (2005) for a review of the CRA and Agarwal et al. (2012) for a recent application on the effect of CRA on bank lending).

varying state controls. The coefficient on *Deregulation index* subsequently remains stable.

INSERT TABLE 2.3

Two concerns regarding endogeneity arise from our baseline model. First, the relationship between banking deregulation and the share of households holding a bank account may be subject to reverse causality. By studying the previous waves of deregulation in the 1970s and 1980s, Kroszner and Strahan (1999) show that the timing of deregulation is not random across states but related to interest group factors such as the prevalence of small banks and small firms. In our case, our identification would be compromised if for instance, the regulator responds to increasing demand for banking services. Second, unobserved factors such as changes in economic conditions could drive both deregulation and the demand for bank accounts.

We first address these endogeneity concerns with the large set of household and state level controls that we introduce in our specification in the second and third columns of Table 2.3. These controls aim to capture factors that foster the demand for banking services at the household level and the economic conditions that may drive deregulation. We observe that the coefficient of our deregulation index is even slightly reinforced.

Second, we analyze the dynamics of the share of banked households around deregulation. Figure 2.2 plots the change in the likelihood of holding a bank account in the years before and after a state deregulates (i.e., it relaxes at least two out of the four restrictions to out-of-state entry). The figure shows that the probability of holding a bank account is relatively high after deregulation and, most importantly, that there is no discernible pattern before the deregulation date. The fourth column of Table 2.3 confirms this result. We interact four dummy variables indicating four periods around the deregulation date with our deregulation index: more than 3 years before, less than 3 years before, 0 to 3 years after, and more than 3 years after. We observe that only the interaction terms with the dummies indicating years after deregulation have a positive and significant coefficient. Therefore, we observe no pre-deregulation trend, and the share of banked households increases only after deregulation takes place. These findings suggest that deregulation is not endogenous to the share of unbanked households but *causes* an increase in the share of banked households.

INSERT FIGURE 2.2

Finally, in section 5.1, we investigate the timing of deregulation following the method of Krozner and Strahan (1999) and find that deregulation does not seem to be driven by variables that also affect access to banking services. As such, interstate branching deregulation seems to provide a valid exogenous shock to the supply of bank accounts to low-income households.

2.4.2 Heterogeneous Treatment Effect

In this section, we investigate whether the effect of banking deregulation is higher for households that are *more likely to be rationed by banks.*

Table 2.4 examines the impact of banking deregulation among black households. We make the assumption that black households are more likely to be rationed by banks in states with a history of discrimination, because we know from the literature that norms and institutions have a long-term impact. Thus, following Chatterji and Seamans (2012), we build four discrimination dummies that indicate states with a history of discrimination. The first index, “slave state”, is equal to one if states allowed slavery before the civil war of

1861-1865. The second index, "banning interracial marriages", comes from Fryer (2007) and identifies states that still banned interracial marriage before 1967, the date when the US Supreme Court's 1967 decision in Loving v. Virginia repealed such anti-miscegenation laws. The third index, "fair housing law", is based on Collins (2004) and identifies states that did not curb discriminatory practices by sellers, renters, real estate agents, builders, and lenders until the federal Fair Housing Act of 1968. Finally, for the fourth index, "interracial marriage bias", we use the racial bias index reported in Levine et al. (2012), which measures the difference between actual and predicted interracial marriage rates in 1970 and classifies states as above or below the median for interracial marriage bias. Not surprisingly, the correlation between these four measures is fairly high and ranges from 40% to more than 90%.

Table 2.4 reports the result of the basic model after introducing the double interaction *Deregulation* $\times$*Black* in the first column, plus the triple interaction *Deregulation* $\times$*Black* $\times$ *Discrimination* for our four discrimination dummies in the final four columns. The coefficient of the double interaction *Deregulation* $\times$*Black* in the first column indicates whether the effect of deregulation is larger for black households than for non-black households. The coefficient of the triple interaction *Deregulation* $\times$*Black* $\times$ *Discrimination* in the other columns indicates whether the gap between black and non-black households reduces more in states with a history of discrimination.

INSERT TABLE 2.4

We find that the effect of deregulation on the share of banked households is larger among black households than among non-black households, but only in states with a history of discrimination. The first column of Table 2.4 shows no significant difference in the impact of deregulation between black and non-black households, because the coefficient of the *Deregulation* $\times$*Black* interaction is positive but not significant. However, the second, third, fourth and fifth columns suggest that the effect of deregulation is larger for black households in states with a history of discrimination. The coefficient of the triple interaction *Deregulation* $\times$*Black* $\times$ *Discrimination* is always positive and significant for our four discrimination dummies. Furthermore, the coefficient of *Deregulation Index*, which measures the effect of banking deregulation on non-black households, does not decrease and is still highly significant in all the specifications of the table. This result suggests that the large effect of deregulation on black households does not drive our main result alone and that the entire population of low-income households also benefits from the reform. Table 2.14 in the Appendix reports the results when we split our sample along our four measures of discrimination. We find again that the impact of deregulation is larger for black households in states with a history of discrimination.

Next, the first three columns of Table 2.5 present the impact of deregulation along income distribution and test whether the poorest households, which are more likely to be rationed by banks' standard practices (e.g., minimum account balance), are more impacted by deregulation. We split our sample into three groups: poor households (below the poverty line), low-income households (between one and two times the poverty line) and middle income households (between two and three times the poverty line). Figure **??** shows that the effect of deregulation is higher for poor households and that there is no effect for middle-income households. Table 2.5 confirms this result. Column 1 indicates that interstate branching deregulation has a particularly pronounced effect on the likelihood of holding a bank account among poor households. More specifically, each step in the deregulation index induces a 2% increase in the probability of holding a bank account among poor households (column (1)) against a 0.9% increase among low-income households (column (2)). By contrast, deregulation has no significant impact on middle-income households (column (3)), which seems logical because middle-income households are less likely to face hurdles or entry barriers to opening a bank account. The absence of a significant effect on middle-income households also confirms that our main result does not simply capture a general decreasing trend in the share of unbanked households in the deregulated states.

INSERT TABLE 2.5

Columns (4) and (5) in Table 2.5 focus on the heterogeneous impact of deregulation across geographical areas. We assume here that the effect of deregulation is higher in rural areas, where households are more likely to be rationed due to lower bank competition ex-ante. To test this hypothesis, we split our sample into "rural" (column (4)) and "urban" households (column (5)). We find that the coefficient of our deregulation index is twice as large for households living in rural areas. This result is consistent with the idea that since rural areas are more likely to be dominated by few local banks, they experience the strongest competitive shocks.

Finally, the last two columns in Table 2.5 investigate whether the impact of deregulation is larger for more educated household. Being unbanked is less likely to be driven by sophistication for these households because they have a higher level of financial literacy (Lusardi and Mitchell, 2011). To do so, we split our sample between households with low education in column (6) (none or only elementary) and households with at least a high school degree in column (7). We find that the effect of deregulation appears mostly for more educated households (column (6)).

2.4.3 Banking Deregulation, Asset Accumulation and Debt

This section investigates the impact of banking deregulation on households' debt and savings. If banking deregulation results in an increase in the likelihood of holding a bank account among low-income households, we could expect the latter not only to accumulate more interest-earning savings given the key role of transaction accounts in asset accumulation (Carney and Gale, 2001), but also to have easier access to debt financing.

Table 2.6 examines the detailed impact of banking deregulation on households' savings. The table shows estimates of the baseline model, where the dependent variables include the two components of our *BankAccount* dummy. *Checking*, in columns (1), (3) and (4), and *Savings*, in columns (2), (5) and (6), indicate whether the household holds a non-interest bearing checking account and a savings account, respectively. The positive and significant coefficients of the deregulation index in columns (1) and (2) show that deregulation significantly increases the likelihood of holding both a checking *and* a savings account to a similar degree. Banking deregulation may therefore foster savings accumulation on interest bearing accounts.

When splitting the sample between poor households and low-income households in columns (3) to (6), the coefficient of our deregulation index indicates that poor households are much more likely to open a checking account (column (3)) than a savings account (column (4)) following deregulation, whereas the opposite result is found for low-income households (column (5) and (6)). This finding is consistent with the intuition that households that are below the poverty line do not have sufficient income to accumulate savings and that savings accounts may better meet the needs of low-income households.

The final column of Table 2.6 reports estimates of our basic model on a dummy indicating whether the household has accumulated interest-earning assets in other financial institutions such as savings and loans, credit unions and mutual funds. The coefficient of our deregulation index is again positive and significant. Because we control for several income variables in our regression, as well as state macroeconomic conditions, this result implies that for an equal amount of income, low-income households are more likely to accumulate wealth when they have access to bank accounts, which confirms the considerable role of bank accounts in fostering asset accumulation.

INSERT TABLE 2.6

Next, Table 2.7 turns to the relationship between deregulation, bank accounts and households' access to debt and investigates whether the increased probability of holding a bank account following deregulation translates into increased access to debt. We begin by mitigating the risk of reverse causality in columns (1) to (3). It may be the case that intensified bank competition provides banks with incentives to increase the credit supply for low-income households and to subsequently offer them the opportunity to open bank accounts. Column (1) focuses on the subsample of banked households and estimates the baseline model in which where the dependent variable is a dummy indicating whether the household holds debt. The coefficient of the deregulation index is not significant and close to zero, which indicates that the credit supply does not appear to increase after deregulation for low-income households with a bank account. Columns (2) and (3) estimate the baseline model in which the dependent variable is our dummy *Bankaccount*, but the sample is split into households without debt (in column (2)) and households with debt (in column (3)). The positive and significant coefficient of our deregulation index in both columns (2) and (3) suggests that deregulation strongly increases access to bank accounts regardless of whether the household takes a loan.

Finally, the last column of Table 2.7 estimates our basic model, where the dependent variable is the debt to income ratio. The negative but not significant coefficient of the deregulation index indicates that deregulation has no impact on the debt-to-income ratio. This result mitigates the fear that deregulation increases the risk of over-indebtedness.

INSERT TABLE 2.7

2.5 Robustness

2.5.1 The Timing of Bank Deregulation

This section strengthen the robustness of our results to several potentially confounding influences resulting from the timing of deregulation. First, one might be concerned that the causal link between deregulation and the share of banked households is reversed. States may have more incentives to deregulate *when the share of banked households is low*. Following deregulation, the share of banked households would then mechanically increase. Another plausible explanation for our results is that states deregulate when their economies are doing well and therefore when the demand for bank accounts is high, because banks are less vulnerable to deregulation during these periods . This phenomenon would translate into a subsequent increase in the share of banked households after deregulation.

We test whether the share of banked households or the macro-economic conditions at the state-level drive the timing of deregulation with a Weibull proportional hazards model (Kroszner and Strahan (1999)).[13] The hazard rate function takes the following form:

$$h(t, X_t, \beta) = h_0(t) \exp[X_t' \beta], \tag{2.2}$$

where X_t is a vector of covariates; β is a vector of unknown parameters; and the baseline hazard rate, $h_0(t)$, is pt^{p-1} with shape parameter p. The parameters β and p are estimated with maximum likelihood. Because we consider four steps of deregulation (the amount of bank deposits, de novo branching, the acquisition of a single branch and the minimum age of a targeted bank), the covariates vector includes an indicator variable for each type of deregulation. We include all state-deregulation step pairs in the analysis . We keep state-deregulation step pairs even when the state has still not deregulated in 2010, in which case the duration is right-censored. We are left with 204 state-deregulation step pairs of which 172 are not censored (i.e., deregulation is observed during the sample period).[14] For each state-deregulation step pair we have one observation for each year up to and including the year of deregulation, which gives us a total of 1,773 observations.

First, to investigate whether the initial level of the share of banked households influences the timing of deregulation, we introduce three new variables: the share of unbanked households, the share of low-income unbanked households and the share of black unbanked households at the beginning of the period (1994). Second, to estimate the effect of macro-economic conditions on the deregulation date we include three broad state variables: the share of black people in the state population, the unemployment rate and

[13]See Hombert and Matray (2014) for a similar exercise in the context of innovation.

[14]Excluding the right-censored state-deregulation step pairs from the analysis yields similar results.

real GDP per capita. Third, we include the main variables that are used by Kroszner and Strahan (1999) and Rice and Strahan (2010): the share of small banks in the state, their relative capital ratio, the size of the insurance sector and the share of small firms in total employment of the state.[15] Finally, we include a proxy for political ideology with a dummy "Republican" that equals one if the majority of the voters chose the Republican candidate in the last presidential election.

Table 2.16 in the Appendix reports the results of the analysis. Reassuringly, the first three columns indicate that the different measures of the share of unbanked households have no significant impact on the timing of deregulation. The fourth column shows that among the macro-economic variables, only GDP per capita has a positive and significant coefficient, suggesting that richer states tend to deregulate earlier. The fifth column reports the coefficients of the Krozner-Strahan variables and shows that the factors that had an impact on the timing of intrastate deregulation in the 1970s and 1980s (Kroszner and Strahan (1999)) also affect interstate deregulation. For instance, a larger share of small banks delays deregulation,whereas a large insurance sector leads to earlier deregulation. However, contrary to the first waves of deregulation, the share of small firms appears to have no effect. Finally, column (6) shows the results when we include all of the variables and confirms that overall, the timing of deregulation does not seem to be related to the share of unbanked households, the share of black households, state unemployment or GDP per capita.

[15]Data for the share of small banks and their relative capital ratio comes from the Call Reports. The share of small banks is the fraction of total assets held by banks with assets below the state median, and the relative capital ratio is the difference in the capital-to-asset ratio of small banks that of large ones. The size of the insurance sector is defined as the ratio of value added from insurance to value added from insurance plus banking. The share of small firms is defined as the fraction of employees in firms with fewer than 20 employees. Data for value added come from the Bureau of Economic Analysis and data for employment by state-firm size come from the Bureau of Dynamic Statistics.

2.5.2 Ruling out Demand Factors

Although the timing of deregulation seems to be exogenous to the share of unbanked households, there may be concern that demand-side factors are driving our results.

One alternative explanation is that banking competition, by decreasing unemployment through growth (Jayaratne and Strahan (1996)) and providing easier firm access to credit (Black and Strahan (2001), Rice and Strahan (2010)), would in fact *foster the demand for bank accounts*.

However there are three facts that suggest that our result is not driven solely by demand effects. First, in all of our previous specifications, we control for a large number of covariates that capture demand effects. At the household level, we control for several dimensions of income, skills and labor status. At the state level, we control for the main state macroeconomic variables such as GDP growth or unemployment. However, to strengthen our specification, we include more detailed controls for unemployment. Table 2.8 reports the results. Columns (1) and (2) show estimates of our main specification after controlling for whether the head of the family is unemployed or whether one of the adults in the household is unemployed. In both cases, our result holds. Next, columns (3) and (4) include detailed controls for unemployment at the state level. In column (3), we replace *State Unemployment* with three variables: *Poor Unemployment*, the unemployment rate of households living below the poverty line; *low-income Unemployment*, the unemployment rate of households with an income between one and two times the poverty line; and *Unemployment Other Income*, the unemployment rate of households whose income is above twice the poverty line. In column (4), we decompose these three unemployment rates by race (black and non-black), resulting in six different unemployment rates.[16] In both cases, our result holds, which mitigates the concern that our effect is driven only by demand effects through a reduction in the unemployment rate.

Second, we consider the effect of deregulation depending on the likelihood of a household being unemployed. If, following banking deregulation, households are more likely to find jobs, and therefore to hold a bank account, households that are initially more at risk of being unemployed (but have jobs) should be more affected by deregulation. To test this prediction, we generate a predicted probability to be unemployed based on household characteristics and location. Columns (5) and (6) present the results of splitting the sample into households with a probability of being unemployed that is *below* the median and households with a probability of being unemployed that is *above* the median. The effect of deregulation is roughly the same; if anything, the point estimate of our deregulation index for households with a lower probability of being unemployed (column 5) is slightly higher.

INSERT TABLE 2.8

Finally, Rice and Strahan (2010), examining the same deregulation, shows that although the increase in competition resulted in a decrease in the cost of credit, it did not translate into an increase in the volumes of loans. This finding suggests that deregulation had a limited effect on demand from firms.

2.5.3 Evidence of Racial Discrimination across Income Groups

Given that the impact of banking deregulation on the poorest households is relatively large and given that black households are poorer on average, our results for racial discrim-

[16] To construct each unemployment rate we use the CPS (Current Population Survey). A detailed description of how we construct the variables is provided in the Appendix.

ination may only reflect an income distributional effect. However, there are two reasons why this should not be the case.

First, we find that banking competition has an impact on the racial gap in access to banking services *only* in states with a history of discrimination. This finding contradicts the view that we simply capture a reduction in the gap between poor and middle-income households.

Second, we show that deregulation has a larger impact on black households than on non-black households at each point of the income distribution. We split our initial sample into *very poor* (below half the poverty line), *poor* (between half the poverty line and the poverty line), *low-income* (between one and two times the poverty line) and *middle-income* households (between two and three times the poverty line). Columns (2) to (5) in Table 2.15 report the results of this decomposition and show that deregulation has an impact on the racial gap in each income group in states with a history of discrimination. In addition, we find no significant effect on the racial gap in the rest of the sample. These results suggest that banking competition reduces the gap between black and non-black households in states with a history of discrimination.

2.5.4 The Effect of Deregulation across Periods and States

In this section, we run a set of standard robustness checks.

First, we show that our result does not capture a general trend in the share of unbanked households in states that deregulate. To do so, we perform a placebo test and randomly change the date of each state deregulation in column (1) in Table 2.9. If the effect we are measuring simply results from a trend, by randomly changing the deregulation date we should still observe a positive and significant impact of deregulation. Column (1) in Table 2.9 shows that the coefficient of the deregulation index is no longer significant and that the point estimate equals 0. In column (2) we re-run our baseline regression and directly add *State* x *Trend* control variables, such that the effect of the reform is identified purely by a deviation from a trend that differs for each state. Column (2) indicates that such a variable does not affect our results.

We then run two other types of robustness checks. First, we check that our results are robust to the sample period. Column (3) starts the sample in 1997 (the date at which the IBBEA becomes effective), and column (4) ends it in 2006 (the date before our gap in the data). Second, we consider what happens when we use different control groups. Because our dependent variable is an index, the identification comes both from the comparison between states that never deregulate with states that deregulate and from the comparison between states that deregulate *more* than others (for instance the comparison between states that move from an index of 1 to 2 as opposed to a state that stays at 1). In column (5) we replace our index with a simple dummy variable that takes the value 1 if a state has adopted at least one of the four deregulations. By contrast, in column (6), we restrict our sample to states that have already deregulated at least once and use our index variable such that the identification comes purely from the increment of the index and the control group is always composed of states that have deregulated at least once. Reassuringly, our results hold in both cases.

Finally, in column 7, we restrict the sample to the largest 11 states (California, Florida, Georgia, Illinois, Michigan, Missouri, New York, North Carolina, Ohio, Pennsylvania and Texas) to ensure that our results are not driven only by small states. We find that our results still hold.

INSERT TABLE 2.9

2.6 Conclusion

In this paper, we investigate whether an intensified bank competition can have a positive impact on the share of banked households among low-income populations. We exploit interstate bank branching deregulation in the U.S. after 1994 as an exogenous shock. We find that the share of unbanked households decreases in the years following deregulation. This result is consistent with the hypothesis that supply-side factors contribute to the unbanked phenomenon.

By examining at the impact of bank competition on access to bank accounts across household types, we confirm the robustness of our results. We find that the effect of intensified bank competition is stronger for populations that are more likely to be restricted by banks. Hence, black households benefit more from deregulation than do non-black households in states with a history of discrimination. The effect of deregulation is also higher for households below the poverty threshold, that are more likely to face entry barriers, such as minimum account balances for opening a bank account.

We also find that this increase in the likelihood of holding a bank account improves savings for low-income households but not their leverage, which suggests that having access to the formal banking sector plays a role in asset accumulation for this population.

Finally, we rule out the alternative interpretation of our result that bank competition decreases the share of unbanked households by fostering demand for bank accounts. First, in all of our specifications, we control for a large set of covariates that capture demand effects at both the household and state levels. Second, we find that the effect is not higher for households that are more likely to benefit from an increase in revenue due to deregulation.

Our paper shows that an intensification of bank competition promotes access to banking services for low-income households. It suggests that changes in banking regulation could impact minorities access to financial services. Because households with no bank accounts turn to alternative financial services, this raises the question of how bank competition interacts with this sector. We leave this question for future research.

Bibliography

Agarwal, S., E. Benmelech, N. Bergman, and A. Seru (2012). Did the Community Reinvestment Act (CRA) Lead to Risky Lending ? *Working Paper*.

Angrist, J. D. and J.-S. Pischke (2008). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton university press.

Ashraf, N., D. Karlan, and W. Yin (2006). Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines. *The Quarterly Journal of Economics 121*(2), 635–672.

Barr, M., J. Dokko, and E. Feit (2011). Preferences for banking and payment services among low-and moderate-income households. *FEDS Working Paper*.

Barr, M. S. (2005). Credit Where it Counts : the Community Reinvestment Act and its Critics. *New York University Law Review 80*, 101–233.

Barr, M. S. and R. Blank (2008). Access to Financial Services, Savings, and Assets Among the Poor. *National Poverty Center Policy Brief 13*.

Black, S. E. and P. E. Strahan (2001). The Division of Spoils: Rent-Sharing and Discrimination in a Regulated Industry. *The American Economic Review 91*(4), 814–831.

Boot, A. W. and A. V. Thakor (2000). Can relationship banking survive competition? *The journal of Finance 55*(2), 679–713.

Carrell, S. and Z. Jonathan (2013). In Harm's Way? Payday Loan Access and Military Personnel Performance. *Working Paper*.

Cetorelli, N. and P. E. Strahan (2006). Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local U.S. Markets. *The Journal of Finance 61*(1), pp. 437–461.

Chatterji, A. K. and R. C. Seamans (2012). Entrepreneurial finance, credit cards, and race. *Journal of Financial Economics 106*(1), 182–195.

Cohen-Cole, E. (2011). Credit Card Redlining. *Review of Economics and Statistics 93*(2), 700–713.

Collins, W. J. (2004). The housing market impact of state-level anti-discrimination laws, 1960-1970. *Journal of Urban Economics 55*(3), 534.

Dick, A. A. (2006). Nationwide Branching and Its Impact on Market Structure, Quality, and Bank Performance. *The Journal of Business 79*(2), 567–592.

Dick, A. A. and A. Lehnert (2010). Personal Bankruptcy and Credit Market Competition. *Journal of Finance 65*(2), 655–686.

Dupas, P. and J. Robinson (2009, January). Savings constraints and microenterprise development: Evidence from a field experiment in kenya. (14693).

Dupas, P. and J. Robinson (2013). Why Don't the Poor Save More? Evidence from Health Savings Experiments. *American Economic Review 103*(4), 1138–71.

Favara, G. and J. Imbs (2010). Credit Supply and the Price of Housing. *Working Paper* (October).

Fryer, R. G. (2007). Guess Who's Been Coming to Dinner? Trends in Interracial Marriage over the 20th Century. *Journal of Economic Perspectives 21*(2), 71–90.

Hogarth, J. and K. O'Donnell (1999). Banking Relationships of Lower-Income Families and the Governmental Trend toward Electronic Payment. *Federal Reserve Bulletin 85*(7), 459–473.

Jayaratne, J. and P. E. Strahan (1996). The Finance-Growth Nexus: Evidence from Bank Branch Deregulation. *The Quarterly Journal of Economics 111*(3), 639–670.

Johnson, C. A. and T. Rice (2008). Assessing a Decade of Interstate Bank Branching. *Washington and Lee Law Review 65*, 73–127.

Kearney, M. S., P. Tufano, J. Guryan, and E. Hurst (2010). Making Savers Winners: An Overview of Prize-Linked Savings Products. *National Bureau of Economic Research Working Paper* (16433).

Kroszner, R. S. and P. E. Strahan (1999). What Drives Deregulation? Economics and Politics of the Relaxation of Bank Branching Restrictions. *The Quarterly Journal of Economics 114*, 1437–1467.

Levine, R., A. Levkov, and Y. Rubinstein (2012). Bank Deregulation and Racial Inequality in America. *Working Paper* (October).

Melzer, B. T. (2011). The Real Costs of Credit Access: Evidence from the Payday Lending Market. *The Quarterly Journal of Economics 126*(1), 517–555.

Morgan, D. P., B. Rime, and P. E. Strahan (2004). Bank Integration and State Business Cycles. *The Quarterly Journal of Economics 119*(4), 1555–1584.

Morgan, D. P. and M. R. Strain (2008). Payday Holiday: How Households Fare After Payday Credit Bans. *Working Paper, Federal Reserve Bank of New York*.

Morgan, D. P., M. R. Strain, and I. Seblani (2012). How Payday Credit Access Affects Overdrafts and Other Outcomes. *Journal of Money, Credit and Banking 44*(2-3), 519–531.

Morse, A. (2011). Payday Lenders: Heroes or Villains? *Journal of Financial Economics 102*(1), 28–44.

Ouazad, A. and R. Rancière (2013). Credit Standards and Segregation. *Working Paper*.

Peoples, J. J. and W. K. Talley (2001). Black-White Earnings Differentials: Privatization versus Deregulation. *American Economic Review 91*(2), 164–168.

Prina, S. (2013). Banking the Poor via Savings Accounts: Evidence from a Field Experiment. *Case Western Reserve, Working Paper*.

Rhine, S. L., W. H. Greene, and M. Toussaint-Comeau (2006). The Importance of Check-cashing Businesses to the Unbanked: Racial/Ethnic Differences. *Review of Economics and Statistics 88*(1), 146–157.

Rice, T. and P. E. Strahan (2010). Does Credit Competition Affect Small-Firm Finance? *Journal of Finance 65*(3), 861–889.

Schaner, S. (2013). The Persistent Power of Behavioral Change: Long-Run Impacts of Temporary Savings Subsidies for the Poor. Technical report, Working Paper.

Washington, E. (2006). The Impact of Banking and Fringe Banking Regulation on the Number of Unbanked Americans. *The Journal of Human Resources 41*(1), 106–137.

Zarutskie, R. (2006). Evidence on the Effects of Bank Competition on Firm Borrowing and Investment. *Journal of Financial Economics 81*(3), 503 – 537.

2.7 Figures and Tables

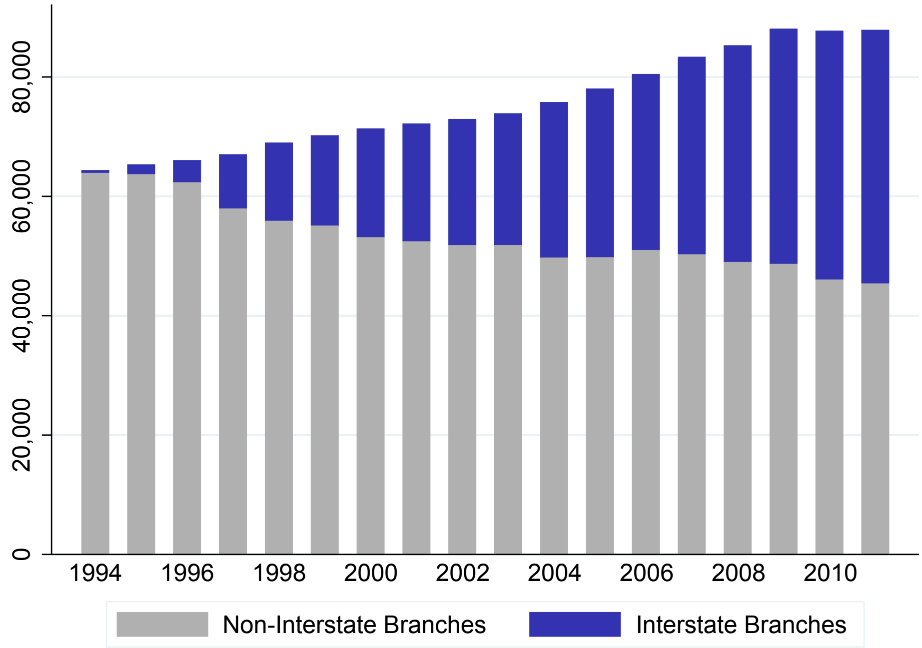


Figure 2.1: . Number of Branches Operated by FDIC-insured Commercial Banks

This figure shows the number of interstate and non interstate branches operating in the U.S. over the years. Data are form the FDIC.

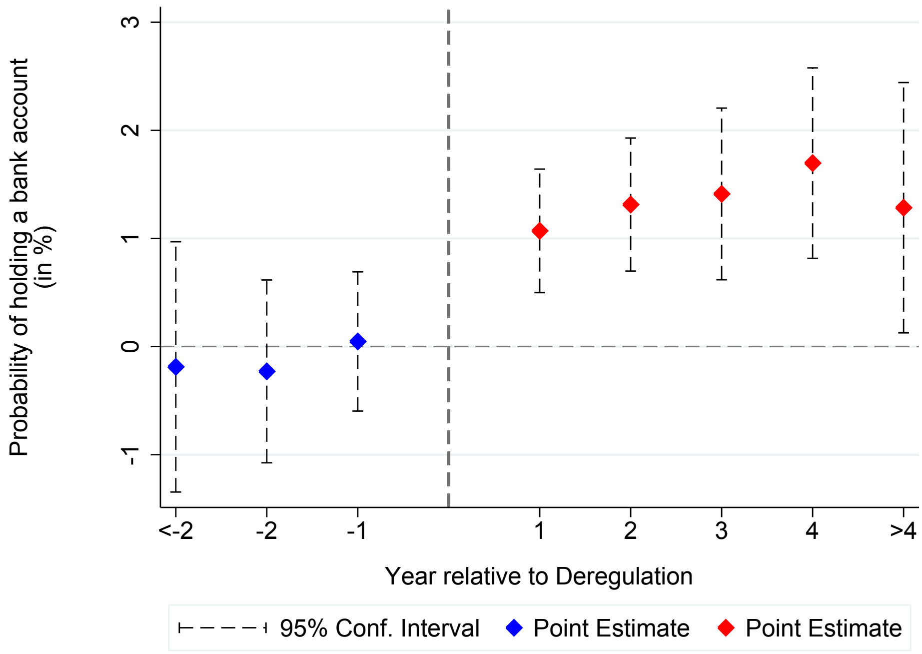


Figure 2.2: . The Impact of Banking Deregulation on the Share of Banked Households

This figure shows the relative change in odd ratios of holding a bank account around deregulation dates among low-income households , where deregulation is defined as a state removal of at least two interstate branching restriction. The specification is the same as equation (1) except that the deregulation index is replaced by dummy variables I(k) equal to one exactly k years after (or before if k is negative) interstate branching deregulation. The point estimates of the dummy variables I(k) and the 95% confidence intervals are plotted. Standard errors are clustered at the state-level.

Table 2.1: Summary Statistics

Sample	**Banked Households**	**Unbanked Households**	*Test*
Black (%)	13	30	***
Married Couple (%)	42	32	***
Single Female-Headed (%)	43	50	***
Household Size	2.5	2.7	***
Age (year)	53	48	***
Elementary Education (%)	22	38	***
High School Education (%)	35	36	***
College Education (%)	42	26	***
Monthly Household Income	1,403	1,297	***
Recepients of Social Security (%)	47	45	***
Recepients of Transfer Income (%)	25	34	***
Unemployed Head of Household (%)	7.7	9	***
Observations	*83,856*	*51,668*	-

This table contains summary statistics on banked and unbanked low-income household socio-demographic characteristics, SIPP (1993 - 2010). The first column displays the mean value of these characteristics for the sample of banked households, whereas the second column displays the mean value of these characteristics for the sample of unbanked households. The test column displays the level of statistical significance of a t-test between the mean values of the right column minus the left column. *, **, and *** represent statistical significance at the 10%, 5%, and 1% confidence levels, respectively.

Table 2.2: Standard Determinants of Banked Households

Dependent Variable	**=1 if the household holds a bank account**	
Household Controls		
Black	-0.162***	(0.010)
Married Couple	0.091***	(0.006)
Single Female-Headed	0.035***	(0.006)
Household Size	-0.020***	(0.002)
Age	0.004***	(0.000)
Elementary Education	0.091	(0.080)
High School Education	0.193**	(0.080)
College Education	0.308***	(0.082)
Monthly Household Income	0.00***	(0.00)
Income < Poverty Threshold	-0.059***	(0.005)
Receive Social Security	0.013*	(0.007)
Receive transfer income	-0.139***	(0.007)
Head unemployed	0.012**	(0.004)
State-Year Controls		
GDP Growth	-0.103	(0.129)
Population	-0.037	(0.132)
State Unemployment	-0.003	(0.006)
Year Fixed Effects	Yes	
State Fixed Effects	Yes	
Observations	135,524	

This table reports a linear probabiliy regression of household and state-year controls on access to bank accounts. The dependent variable equals 1 if the household has access to a checking or savings account (SIPP 1993 - 2010). The regression includes state and year fixed effects. Standard errors are clustered by state.

Table 2.3: The Impact of Bank Deregulation on the Share of Banked Households

Dependent Variable	**=1 if the household holds a bank account**			
	(1)	**(2)**	**(3)**	**(4)**
Deregulation Index	0.012*** (0.004)	0.012*** (0.004)	0.012*** (0.004)	
Deregulation (<t-3)				-0.012 (0.016)
Deregulation (t-3,t-1)				-0.012 (0.012)
Deregulation (t+1,t+3)				0.033*** (0.012)
Deregulation (>t+3)				0.037** (0.017)
Household Controls	-	Yes	Yes	Yes
State-Year Controls	-	-	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	135,524	135,524	135,524	135,524

This table reports linear probability regressions of the Interstate Branching Deregulation Index on access to bank accounts. The dependent variable equals 1 if the household holds a checking or a savings account (SIPP 1993 - 2010). The deregulation index ranges from 0 to 4, 0 is most, 4 is least restricted. Column (1) does not include any controls whereas columns (2), (3) and (4) include household controls, plus state-year controls in columns (4) and (5). All regressions include state and year fixed effects. In column (5) the deregulation index is split into four sub-periods: more than 3 years before deregulation, less than 3 years before deregulation, 0 to 3 years after deregulation, and more than 3 years after deregulation, where deregulation corresponds to the removal of at least two out of the four possible restrictions. Household and state-year controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income and state unemployment, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

Table 2.4: The Impact of Bank Deregulation on the Share of Banked Households: Evidence on Racial Discrimination

Dependent Variable	**=1 if the household holds a bank account**				
Discrimination Dummy	**-**	**Former Slave State**	**Antimiscegenation Law**	**No Fair Housing Law**	**Share of interacial marriage**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Deregulation Index	0.011***	0.014***	0.012***	0.014***	0.014***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
Index x Black	0.009	-0.003	0.003	-0.005	-0.001
	(0.006)	(0.009)	(0.008)	(0.007)	(0.006)
Index x Black x Discrimination		0.025***	0.020**	0.029***	0.024***
		(0.010)	(0.010)	(0.009)	(0.009)
Household Controls	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	135,524	135,282	134,066	135,524	135,524

This table reports linear probability regressions of the Interstate Branching Deregulation Index on access to bank accounts, its interaction with black and its interaction with a racial discrimination dummy. The dependent variable equals 1 if the household has access to a checking or savings account (SIPP 1993 - 2010). The deregulation index ranges from 0 to 4, 0 is most, 4 is least restricted. From column (2) to (5) four racial discrimination dummies are interacted first, with black, second, with black and the deregulation index: slaves state in the year immediately prior to Civil war (1 if yes, 0 if not), anti-miscegenation law not repealed until after the US Supreme Court's 1967 decision in *Loving v. Virginia* (1 if yes, 0 if no), no fair housing law until federally mandated by the Fair Housing Act of 1968 (1 if yes, 0 if no), racial bias index, as measured by the interracial marriage rate (1 if below median). All regressions include black*discrimination, index*deregulation, black*deregulation controls as well as state and year fixed effects. Household and state controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income, unemployed status and state unemployment rate, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

Table 2.5: Heterogenous Effect of Bank Deregulation across Household Types

Dependent Variable	**=1 if the household holds a bank account**						
	Income Group			**Residence**		**Education**	
Sample	**Poor**	**Low**	**Middle**	**Rural**	**Urban**	**High**	**Low**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deregulation Index	0.020***	0.009**	0.003	0.018***	0.010**	0.012***	0.007
	(0.005)	(0.004)	(0.003)	(0.007)	(0.005)	(0.005)	(0.006)
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,069	118,107	48,343	37,550	98,626	97,873	38,303

This table investigates the effect of banking deregulation on access to bank accounts across various types of households. In columns (1) to (3) we split the sample into three groups based on income level: "Poor" is below the poverty line, "Low" is between once and twice the poverty line and "Middle" is between two and three times the poverty line. Columns (4) and (5) split between households living in rural and urban areas. Columns (6) and (7) split the sample between low educated (less than high school) and highly educated (high school or higher) households. Household and state controls are the same as previously described. Standard errors are clustered by state.

Table 2.6: The Effect of Bank Deregulation on Asset Accumulation

Dependent variable	**=1 if the household holds**						
	Checking Account	**Savings Account**	**Checking Account**		**Savings Account**		**Savings in o. Institutions**
Sample	**All**	**All**	**Poor**	**Low Inc.**	**Poor**	**Low Inc.**	**All**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deregulation Index	0.007**	0.006***	0.014***	0.008*	0.004	0.009***	0.094***
	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.003)	(0.038)
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,524	135,524	38,620	46,115	52,250	83,274	135,340

This table reports the effect of the Interstate Branching Deregulation Index on asset accumulation. In columns (1) to (6) the dependent variable is dichotomous and takes the value 1 if the household owns a non interest bearing checking account (columns (1) and (3)-(4)), a savings account (columns (2) and (5)-(6)) and interest earning assets in financial institutions other than a bank in column (columns (7)). We also decompose the effect of owning a checking and savings account between income groups. “Poor” is below the poverty line and “Low” is between once and twice the poverty line. Household and state controls are the same as previously described. Standard errors are clustered by state.

Table 2.7: The Effect of Bank Deregulation on Debt

Dependent variable	**=1 if the household holds**			**Debt to Income Ratio**
	Debt	**Bank Account**		
Sample	**Banked**	**Debt-free**	**In debt**	**All**
	(1)	(2)	(3)	(4)
Deregulation Index	0.001	0.014***	0.007**	-0.310
	(0.002)	(0.005)	(0.003)	(0.366)
Household Controls	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	83,567	62,773	72,274	135,047

This table reports the effect of owning a bank account on access to debt. We estimate linear probability regressions in which dependent variables are dichotomous variables that take the value 1 if the household owns debt in columns (1), (4) and (5) or a bank account in columns (2) and (3) and the debt to income ratio in columns (6) and (7). The deregulation index ranges from 0 to 4, 0 is most, 4 is least restricted. All regressions include state and year fixed effects. Household and state controls are the same as previously described. Standard errors are clustered at the state level.

Table 2.8: Ruling Out Demand Factors

Dependent variable	=1 if the household holds a bank account					
Sample	**Total Sample**				Likely unemployed	Not likely unemployed
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation Index	0.012***	0.012***	0.012***	0.011***	0.013***	0.011***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Head Unemployed	0.012***		0.012***	0.013***		
	(0.004)		(0.004)	(0.004)		
Any Family Member Unemployed		0.017***				
		(0.004)				
Unemployment Poor			-0.086*			
			(0.051)			
Unemployment Low Income			-0.037			
			(0.095)			
Unemployment Other Income			-0.077			
			(0.313)			
Unemployment Poor Black				0.014		
				(0.013)		
Unemployment Low Income Black				0.037		
				(0.024)		
Unemployment Poor No Black				-0.148**		
				(0.059)		
Unemployment Low Income No Black				-0.098		
				(0.110)		
Unemployment Other Income Black				0.011		
				(0.063)		
Unemployment Other Income No Black				-0.036		
				(0.356)		
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,524	135,524	135,524	135,524	135,524	135,524

This table reports the effect of Interstate Branching Deregulation Index on the likelihood to have a bank account depending on various measure of the unemployment rate in the the state of location. In column (5) and (6), we estimate the probability to be unemployed based on household and state characteristics and split the sample between households with a probability below the median sample (column (5)) and above median sample (column (6)). Standard errors are clustered by state.

Table 2.9: Robustness Checks

Dependent Variable	**=1 if the household holds a bank account**						
	All		**Periods**		**States**		
Sample			**After 1997**	**Before 2005**	**All**	**Only Deregulated**	**Largest**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Placebo Index	-0.002 (0.004)						
Deregulation Index		0.011** (0.005)	0.015*** (0.005)	0.013*** (0.004)		0.011** (0.005)	0.014** (0.005)
Deregulation Dummy					0.020* (0.012)		
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year Trend	-	Yes	-	-	-	-	-
Observations	135,524	135,524	107,464	112,029	135,524	96,845	72,716

This table reports results from linear probability model regressions on access to bank accounts. Columns (1) and (2) and (5) include the whole sample. In columns (3) and (4) data are split into two sub-periods: (1997-2010) and (1993-2006). In column (6) the sample is restricted to states with at least one deregulation over the 1993-2010 period, and in column (7) to the largest 10 states. The dependent variable is 1 if the household holds a checking or a savings account (SIPP 1993 - 2010). The explanatory variable is the deregulation index in each column, except in column (1) and in column (5). In column (1) the dependant variable is a placebo index. In column (5) the dependant variable is a dummy with value 1 if the deregulation index is strictly higher than 0. All regressions include state and year fixed effects. Column (2) also includes state*trend effects. Household and state controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income, unemployed status and state unemployment, population (log) and unemployment rate. Standard errors are clustered by state.

2.8 Appendix

2.8.1 Tables

Table 2.10: State Interstate Branching Laws: 1994-2010

This table shows for every state the year in which the deregulation reforms came into effect and gives the deregulation index resulting from these changes. The index ranges from 0 to 4, 4 indicating maximum openness to out-of-state branching.

State	**Effective Year**	**No Minimum Age on target**	**Allows De Novo Branching**	**Allows Single Branch Acquisition**	**Deposit cap higher than 30%**	**Index**
Alabama	1997	0	0	0	1	1
Alaska	1994	0	0	1	1	2
Arizona	1996	0	0	0	1	1
Arizona	2001	0	0	1	1	2
Arkansas	1997	0	0	0	0	0
California	1995	0	0	0	1	1
Colorado	1997	0	0	0	0	0
Connecticut	1995	0	1	1	1	3
Delaware	1995	0	0	0	1	1
DC	1996	1	1	1	1	4
Florida	1997	0	0	0	1	1
Georgia	1997	0	0	0	1	1
Georgia	2002	0	0	0	1	1
Hawaii	1997	0	0	0	1	1
Hawaii	1997	1	1	1	1	4
Idaho	1995	0	0	0	1	1
Illinois	1997	0	0	0	1	1
Illinois	2004	1	1	1	1	4
Indiana	1997	1	1	1	1	4
Indiana	1998	0	1	1	1	3
Iowa	1996	0	0	0	0	0
Kansas	1995	0	0	0	0	0
Kentucky	1997	0	0	0	0	0
Kentucky	2000	1	0	0	0	1
Kentucky	2004	1	0	0	0	1
Louisiana	1997	0	0	0	1	1
Maine	1997	1	1	1	1	4
Maryland	1995	1	1	1	1	4
Massachussets	1996	1	0	0	1	2
Michigan	1995	1	1	1	1	4
Minnesota	1997	0	0	0	1	1
Mississipi	1997	0	0	0	0	0
Missouri	1995	0	0	0	0	0
Montana	2001	0	0	0	0	0

Table 2.11: State Interstate Branching Laws: 1994-2010 (End)

This table shows for every state the year in which the deregulation reforms came into effect and gives the deregulation index resulting from these changes. The index ranges from 0 to 4, 4 indicating maximum openness to out-of-state branching.

State	**Effective Year**	**No Minimum Age on target**	**Allows De Novo Branching**	**Allows Single Branch Acquisition**	**Deposit cap higher than 30%**	**Index**
Nebraska	1997	0	0	0	0	0
Nevada	1995	0	1	1	1	3
New Hampshire	1997	0	0	0	0	0
New Hampshire	2000	0	1	1	1	3
New Hampshire	2002	1	1	1	1	4
New Jersey	1996	1	0	1	1	3
New Mexico	1996	1	0	0	0	1
News York	1997	0	0	1	1	2
North Carolina	1995	1	1	1	1	4
North Dakota	1997	1	0	0	0	1
North Dakota	2003	1	1	1	0	3
Ohio	1997	1	1	1	1	4
Oklahoma	1997	0	0	0	0	0
Oklahoma	2000	1	1	1	0	3
Oregon	1997	0	0	0	0	1
Pennsylvania	1995	1	1	1	1	4
Rhode Island	1995	1	1	1	1	4
South Carolina	1996	0	0	0	1	1
South Dakota	1996	0	0	0	1	1
Tennessee	1997	0	0	0	1	1
Tennessee	1998	0	0	1	1	2
Tennessee	2001	0	1	1	1	3
Tennessee	2003	0	1	1	1	3
Texas	1995	0	1	1	0	2
Texas	1995	0	0	0	0	0
Texas	1999	1	1	1	0	3
Utah	1995	0	0	1	1	2
Utah	2001	0	1	1	1	3
Vermont	1996	0	0	1	1	2
Vermont	2001	0	1	1	1	3
Virginia	1995	1	1	1	1	4
Washington	1996	0	0	0	0	1
Washington	1996	0	1	1	1	3
West Virginia	1997	0	1	1	0	2
Wisconsin	1996	0	0	0	1	1
Wyoming	1997	0	0	0	1	1

Table 2.12: Percent of Unbanked Households by Data Source and Year

This table reports the share of unbanked households. Percentages are authors' calculations except for the 1994 Panel Study and Income Dynamics one, which is from Hogarth and O'Donnell (1999).

Year	**Survey of Income and Program Participation**	**Panel Study of Income Dynamics**	**SIPP (Low Income Households)**
1993	16.4		36.3
1994	16.5	22	35.5
1995	16.8		35.6
1996	17.8		36.8
1997	19.2		39.2
1998	19.1		38.6
1999	19.4	23.3	38.8
2000	19.5		38.5
2001	20.1	24.8	38.6
2002	21.4		40.6
2003	22.4	25.7	40.6
2004	19.0		38.6
2005	18.6	25.8	36.7
2009	19.0	26.9	36.7
2010	20.6		38.4

Table 2.13: The Impact of the Four Provisions on Interstate Branching Deregulation on the Share of Banked Households

This table reports Logit regressions of the Interstate Branching Deregulation Index on access to banking services. The dependent variable equals 1 if the household has access to a checking or saving account (SIPP 1993 - 2010). The deregulation index ranges from 0 to 4, 0 is most, 4 is least restricted. Column (1) does not include any controls whereas columns (2), (3) and (4) include Household controls, plus State-Year controls in columns (4) and (5). In column (5) the deregulation index is split into four sub-periods. All regressions include state and year fixed effects. Household and State-Year controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income, car or home ownership and state unemployment, population (log) and unemployment rate. Standard Errors are clustered by state.

	(1)	(2)	(3)	(4)
Deposit Cap	0.002			
	(0.013)			
De Novo Branching		0.031**		
		(0.014)		
Minimum Age			0.024*	
			(0.014)	
Single Branching				0.037***
				(0.009)
Household Controls	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	135,524	135,524	135,524	135,524

Table 2.14: The Impact of Bank Deregulation on the Share of Banked Households: Evidence on Racial Discrimination (2)

This table reports logit regressions of the Interstate Branching Deregulation Index and its interaction with black on access to banking services. The dependent variable equals 1 if the household has access to a checking or saving account (SIPP 1993 - 2010). The deregulation index ranges from 0 to 4, 0 is most, 4 is least restricted. For each set of regressions, the data are split into two mutually exclusive samples: slave state in the year immediately prior to the Civil War (yes or no), anti-miscegenation law not repealed until after the US Supreme Court's 1967 decision in *Loving v. Virginia* (yes or no), no fair housing law until federally mandated by the Fair Housing Act of 1968 (yes or no), racial bias rate, as measured by the interracial marriage rate (below or above median). All regressions include state and year fixed effects. Household and state controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income, unemployed status and state unemployment, population (log), GDP growth and a republican dummy. Standard Errors are clustered by state.

	Slave Territory		Antimisce -genation Law		No Fair Housing Law		Share of interacial marriage	
	No	Yes	No	Yes	No	Yes	>Median	<Median
Deregulation Index	0.031	0.049	0.041	0.042	0.015	0.067**	0.046*	0.060*
	(0.030)	(0.030)	(0.026)	(0.034)	(0.039)	(0.028)	(0.027)	(0.033)
Index x Black	-0.020	0.100***	0.004	0.101***	-0.032	0.110***	-0.020	0.109***
	(0.041)	(0.027)	(0.038)	(0.026)	(0.033)	(0.026)	(0.029)	(0.030)
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	65,472	69,810	76,261	57,805	66,439	69,085	77,960	57,564

Table 2.15: Racial Discrimination Across Income Groups

In this table, data are split into four exclusive samples of households based on their annual income: *very poor* (below half the poverty line), *poor* (between half and once the poverty line), *low income* (between once and twice the poverty line) and *middle income* households (between twice and three times the poverty line). In column (1) we estimate logit regressions of the Interstate Branching Deregulation Index on access to banking and its interaction with a *poor household* (including *very poor* households) dummy and with a *low income household* dummy. In column (2) to (5) we estimate logit regressions of the Interstate Branching Deregulation Index on access to banking and its interaction with black and a discrimination dummy (No Fair housing law) on each sample. The dependant variable is 1 if the household has access to a checking or saving account (SIPP 1993 - 2010). The deregulation index ranges from 0 to 4, 0 is most, 4 is least restricted. Regressions in columns (2) to (5) include black x discrimination, index x deregulation, black*deregulation controls. Household and State controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income, unemployed status and state unemployment, population (log) and unemployment rate. Standard Errors are clustered by state.

	(1) Poor+Low+Middle Income Households	(2) Very Poor Households	(3) Poor Households	(4) Low Income Households	(5) Middle Income Households
Deregulation Index	-0.012	0.090**	0.072*	0.069**	0.037
	(0.022)	(0.036)	(0.039)	(0.034)	(0.035)
Index x Poor	0.103***				
	(0.022)				
Index x Low Income	0.067***				
	(0.014)				
Index x Black		0.147**	0.142***	0.150***	0.135*
x Discrimination		(0.072)	(0.049)	(0.049)	(0.075)
Household Controls	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	183,693	17,989	34,261	83,274	48,169

Table 2.16: Addressing Endogeneity Concerns: What Drives Bank Deregulation?

The hazard model is Weibull, where the dependent variable is the log expected time to deregulation. All variables are measured for each state in each year. The share of unbanked households, low income unbanked households or black unbanked households are measured at the state level in 1994. The share of black people in the state population, unemployment rate and real GDP per capita is at the state-year level. Share of small banks is the percent of banking assets in the state held by banks below the median size of banks in each state in each year. Relative capital ratio of small banks is the capital to assets ratio of small banks minus that of large banks. Relative size of insurance relative to banking plus insurance in the state is measured as gross state product from insurance divided by gross state product from insurance plus banking. Republican is equal to one if the majority of the voters chose the Republican candidate in the latest presidential election.

	Duration Model for the Time until Deregulation					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of unbanked households	-1.983					-4.057
	(1.948)					(3.378)
Share of unbanked among the low income		-4.252				
		(2.777)				
Share of unbanked among the black			0.359			
			(3.456)			
Share of black households				-0.259		1.822
				(1.486)		(2.875)
Unemployment				0.118		0.247
				(0.121)		(0.190)
GDP per capita				0.032**		0.014
				(0.014)		(0.022)
Republican					-0.133	0.278
					(0.370)	(0.434)
Share of small banks					-19.37***	-17.513***
					(5.528)	(6.576)
Relative capital ratio of small banks					0.000	0.000
					(0.000)	(0.000)
Relative size of insurance					1.010***	1.113***
					(0.377)	(0.352)
Observations	1,773	1,773	1,773	1,773	1,773	1,773

2.8.2 Variables Definitions

State-level variables

State GDP Growth: annual growth rate in gross state product (GSP) using data obtained from the U.S. Bureau of Economic Analysis Region Tables.

Population: log of total state population from the U.S. Bureau of Economic Analysis Region Tables.

State Unemployment: the state unemployment rate, obtained from the U.S. Bureau of Labor Statistics.

CPS Data

To construct unemployment rate by racial and income categories, we use publicly-available microdata from IPUMS-CPS (Ruggles et al. 2010) for the years 1994 to 2010.

We select the sample as follow. We drop the population not in the labor force ($labforce$ = 13) and in military (*empstat*=13) and keep persons between the age of 16 and 64. We identify families below the poverty line if their total family income $ftotval$ is below the threshold given by the CPS (variable *cutoff*). Finally, we collapse the data at the state-year level using population weight $wtsupp$.

Chapter 3

The Local Innovation Spillovers of Listed Firms

How local innovation shocks diffuse in the economy

Innovation activities tend to be geographically concentrated and in particular, much more so than manufacturing employment (Audretsch and Feldman, 1996). The success of innovation clusters such as Silicon Valley is often explained by local networks of innovative firms helping diffuse knowledge across firms (e.g. Saxenian, 1994). It has motivated large investments by governments to promote such clusters (e.g. Lerner, 2011). Often, particular emphasis is put on developing ecosystems of large and small firms, such as the recent American "Regional Cluster Initiative" funded by the Economic Development and Small Business administrations (Katz and Muro, 2013).[1] Supporters of such policies often stress that knowledge produced by large firms will benefit neighboring smaller firms, as appears to be the case with Seattle's innovation cluster that began developing after Microsoft relocated its headquarters in the area.

The spatial concentration of innovative activities is expected to foster innovation because as for economic spillovers in general, agglomeration allows local firms to share inputs, workers and ideas more efficiently.[2] However, while strong evidence exists that agglomeration and innovation are correlated, causal identification remains elusive (Carlino and Kerr, 2014). Indeed, establishing a causal link is difficult because of the "reflection problem" (Manski, 1993) whereby innovation trends for all firms are driven by the same underlying factors. For instance, innovative firms may cluster in areas with attractive attributes, such as leading research universities, benign weather conditions, tax advantages, etc. In this case, common innovation trends would not result from positive spillovers but simply from local area characteristics.

[1] In a similar initiative, the French government has invested nearly $2 billion to create "competitiveness clusters" ("pôles de compétitivité").

[2] Surveys about the link between knowledge and agglomeration include Audretsch and Feldman (2004), Moretti (2004c), Feldman and Kogler (2010) and Carlino and Kerr (2014).

To disentangle local innovation spillovers from the effects of local conditions, I instrument listed firms' innovation in a given geographical area with a regulatory shock in a different state. The shock is caused by the staggered adoption by individual states of business combinations (BC) laws preventing acquirers from using the target's assets to pay down acquisition debt. The laws made it more difficult to complete hostile takeovers of listed firms incorporated in the adopting state. The lower takeover threat has been shown to have weakened external governance, allowing management to enjoy "the quiet life" or to "play it safe" (Bertrand and Mullainathan, 2003; Gormley and Matsa, 2014). This resulted in a decrease in innovation by listed firms, even in areas outside their state of incorporation (Atanassov, 2013).

BC laws provide an appealing instrument because they cause local areas to experience variations in innovation driven by out-of-state shocks and because they affect innovation by listed firms but not by private firms. My hypothesis is that local innovation spillovers happen mostly because changes in listed firms's innovation in a given area directly affect innovation by private firms in the same area. I limit the concerns that local private firms may be affected by other changes produced by BC laws by removing state-year unobserved heterogeneity and by controlling for important economic characteristics at the local area level. I also provide multiple cross-sectional tests consistent with the fact that local innovation spillovers happen because knowledge diffuses locally.

I study innovation by US firms over the period 1975-2000.[3] I use the NBER patent and inventor database containing information about patent inventors, including address and employer. Inventor addresses allow me to allocate innovations to different Com-

[3]The sample stops in 2000 to avoid truncation bias.

muting Zones (CZ), i.e. local geographic areas encompassing all metropolitan and non-metropolitan areas in the US (e.g. Tolbert and Size, 1996; Autor and Dorn, 2013). I consider that a firm is active in a CZ if it files patents in that CZ. The dataset covers both listed and private firms, a classification my identification strategy exploits. In the data, both sets of firms account for a similar fraction of patents filed: around 60% for listed firms and 40% for private firms. However, as one would expect, they differ in the geography of patenting. On average, listed firms file patents in 12 different CZs vs. 1.5 for private firms. Moreover, listed firms file less than 20% in their state of incorporation.

Local areas experience variations in innovation driven by out-of-state shocks at different points in time, depending on when a given state adopts a BC law, and the same shock affects different areas with different intensities, depending on how many listed firms active in an area are incorporated in the adopting state. This allows me to employ a difference-in-differences strategy that studies how innovation by private firms reacts to the change in innovation by neighboring listed firms as implied by the adoption (or not) of BC laws in their state of incorporation.

In the first part of the paper, I study how important local innovation spillovers are and how local they are. I find that listed firms generate positive and economically significant innovation spillovers onto private firms in the same local area. On average, a change in one patent to the stock of patents held by listed firms in a CZ leads to a similar change in 0.14 patent filed by private firms in the same CZ. This result is robust to the inclusion of controls for CZ-level innovation capacities and labor characteristics as well as to sample restrictions, such as excluding the most innovative cities or states.

I also find these innovation spillovers to be markedly local, i.e. they fade away quickly with distance. Indeed, innovation by listed firms in a given CZ have spillovers mostly for private firms in the same CZ. For private firms in other CZs within 100 miles, innovation spillovers are still positive, but small: the elasticity is divided by a factor of more than three. Beyond 100 miles, innovation spillovers are undistinguishable from zero.

Next, I turn to the mechanisms underlying local innovation spillovers. Agglomeration can foster innovation by reducing the costs of accessing inputs, workers and knowledge, knowledge diffusion being likely to be particularly important (Carlino and Kerr, 2014). In the second part of the paper, I study two main channels of knowledge diffusion (Audretsch and Feldman, 2004): learning across local firms and inventors moving from their employer to both existing and newly started local firms.

First, I find evidence of knowledge diffusion via learning across local firms. Indeed, I document higher local innovation spillovers onto firms that are technologically closer to listed firms innovating locally, i.e. that file patents in the same technological classes, or tend to cite patents filed by listed firms or by local firms. For each proxy of technological proximity, I find that a one standard deviation of this proxy amplifies local innovation spillovers by about half of the average effect.

Knowledge diffusion through learning across local firms is also likely to depend on the local supply of educated workers, whose ability to assimilate and apply new knowledge may be more important (e.g. Moretti, 2004b). For each CZ, I calculate the supply of college graduate workers at the beginning of the period. I find that CZs in the 75th percentile of the distribution of educated workers experience local innovation spillovers that are twice as large as those of CZs in the 25th percentile. I find similar results using instruments that exploit historical differences in the supply of colleges to predict the fraction of educated workers.

Second, I find evidence of knowledge diffusion via employees moving across local firms. In a first test, I exploit variations across states in the enforcement of non-compete clauses that limit worker mobility, I find that CZs in states that restrict non-compete clauses experience local innovation spillovers that are twice as large as those in states that do not.

I also study how variations in the stock of patents by listed firms affect the mobility of inventors from listed firms to both existing private firms and newly started spin-outs in the same area. I define a spin-out as a new firm employing, in the first year it files patents, inventors formerly employed by a listed firm active in the same area. For both existing and new firms, I observe more mobility when the stock of patents by listed firms increases.

In the third part of the paper, I investigate the two-way connection between local innovation spillovers and the availability of venture capital.

First, I examine whether local innovation spillovers attract capital to the area. To identify non local investors, I use the VentureXpert database, which reports for each venture capital (VC) fund covered its address and the location of all its investments. I find that when listed firms in a CZ innovate more, VC funds located outside that CZ increase the volume of their investments in that CZ. On average, a one standard deviation increase in the stock of patents by listed firms in a CZ increases non local VC investments per year by 11%. This is all the more remarkable given that non-local investments are rare in the VC industry (e.g. Chen et al. 2010).

Second, I examine whether conversely, exogenous fluctuations in local capital availability amplifies local innovation spillovers (e.g. by enabling local firms to finance innovations). To do so, I instrument the amount of VC capital available locally using variation in state pension funds. Because state pension funds invest disproportionably in local investment funds (e.g. private equity, venture capital funds), local investment funds raise capital more easily when local pension pools are larger (e.g. Bernstein et al. 2010; Hochberg and Rauh, 2012; Gonzalez-Uribe, 2014). I find that CZs in the 75th percentile in the distribution of VC financing experience local innovation spillovers that are twice as large as those in CZs in the 25th percentile.

Finally, I conduct robustness checks and, in particular, I assess the sensitivity of my results to two alternative channels: a product market competition channel, whereby changes in competition triggered by BC laws would affect innovation by private firms and, a demand channel whereby in response to BC laws, listed firms would generate a demand for technologies that increases innovation by private firms. The findings suggest that these alternative channels are unlikely to play a major role in explaining the comovements in the innovation activity of listed and of private firms.

Taken together, the paper's results show evidence that sizeable local innovation spillovers exist, that are at least partly driven by knowledge diffusion via learning across local firms as well as employee and inventor mobility across local firms. Furthermore, these spillovers attract capital to the area which in turn amplifies the spillovers. These findings point to possible policy implications. Indeed, if the clustering of innovation were mostly due to attractive local attributes (universities, etc.), local public policies aimed at fostering innovation clusters should focus on providing those. However, if innovation clusters stem from innovation spillovers, subsidies can be justified.[4] My findings also suggest that local

[4]The existence of spillovers constitutes a rationale for location-based policies but does not imply these

innovation spillovers can be amplified by policies promoting intrastate labor mobility (e.g. by restricting non compete clauses), improving the supply of skilled labor (e.g. via the construction of college institutions) and improving access to capital.

The paper proceeds as follows. Section 2 reviews the relevant literature and states my hypotheses. Section 3 presents the data. Section 4 describes the framework and discusses specification and measurement errors. Section 5 reports the empirical results. Section 6 concludes.

3.1 Literature Review and Hypothesis Development

3.1.1 Literature Review

My paper contributes to the literature studying how corporate investment is shaped by the economic environment and in particular by neighboring firms. This question has been studied for investment in general (Dougal, Parsons and Titman, 2013; Fresard and Foucault, 2014), as well as for firm creation (Doms, Lewis and Robb, 2010; Guiso, Pistaferri and Schivardi, 2014) and innovations in particular (e.g., Peri, 2005; Bloom, Schankerman and Van Reenen 2013). I add to this literature by providing a new method of studying innovation spillovers and by providing evidence for specific channels through which these local innovation spillovers can occur. I also use a finer measure of geographic proximity by using inventor addresses rather than firms' headquarters as the location of innovation.[5] Finally, I study the specific interactions between publicly listed and private

to improve aggregate welfare. See e.g. Glaeser and Gottlieb (2008) or Kline and Moretti (2014).

[5]Indeed, it is not clear that information regarding failed or successful innovative projects will be communicated by CEOs. Inventors appear more likely to spread knowledge locally, in particular by moving across firms. A similar approach is used in Lychagin et al. 2010

firms, which is a subject that has received little attention thus far.

More broadly, a recent stream of the corporate finance literature has recently begun to studied how performance and behavior are shaped by firms' economic and geographic proximities to other firms with respect to stock prices and sales (Cohen and Frazzini, 2008; Barrot and Sauvagnat 2014), leverage (Leary and Roberts, 2014), corruption (Parsons, Sulaeman and Titman, 2014) and/or bankruptcy (Bennmelech, Bergman, Milanez and Mukharlyamov, 2014).

In this burgeoning literature, two very different mechanisms are at play that explain the comovement of behaviors. The first mechanism is that managers either infer information from their peers or simply "mimick" these peers, and the second is that neighboring firms have a direct effect on their peers' inputs or cash-flows. My paper is about the second mechanism. My paper is about the second mechanism. I show that innovation by private firms is affected because a key input in their own innovation production functions varies: the local stock of external knowledge produced by listed firms.

Therefore, I also relate to the literature studying how the stock of external knowledge available in the surroundings of economic agents affects their productivity and their ability to innovate. The dominant approach in this literature is to regress productivity, wages (used as a proxy for productivity) or innovation on a proxy for the stock of knowledge available such as the stock of R&D (e.g., Bernstein and Nadiri, 1989; Griliches, 1992; Adams and Jaffe, 1996; Peri 2005 or Bloom et al., 2013 and references therein), the supply of college graduates (e.g., Rauch, 1993; Moretti, 2004a, 2004b), population density (e.g., Ciccone and Hall, 1996; Glaeser, 1999; Glaeser and Resseger, 2010) or firm density (Carlton, 1983; Rosenthal and Strange, 2003; Henderson, 2003; Greenstone, Hornbeck and Moretti, 2010; Guiso and Schivardi, 2011; Guiso et al. 2014).

Finally, my paper builds on the literature studying the importance of anti-takeover laws, by studying spillovers produced by changes in corporate governance. The literature typically estimates the evolution of firm outcomes when the firm is *directly* affected by a change in corporate governance and in particular, how this type of change can lead firms to take less risk.[6] One important feature of my paper is that I do not examine firms that are directly affected by antitakover laws at the firms *surrounding* them.

3.1.2 Hypothesis Development

A priori, the effect of innovation by listed firms on local private firms is unclear. Innovation by firms in the same area may be strategic substitutes for at least two reasons. If firms are rivals and an increase in innovation by a given firm improves its competitive position, it will reduce the marginal profitability of other firms regarding innovative investment, leading them to innovate less (Jones and Williams, 1998, 2000, Bloom et al. 2013).[7] In addition to this "business stealing effect", negative spillovers can also occur because of "labor stealing". Studying spillovers at the local labor market level implies that innovative firms compete for skilled labor. To innovate more, listed firms may try to poach inventors from private firms by offering them higher wages. If private firms are not able to retain skilled labor, innovation by listed firms will generate negative spillovers, which might occur for instance because labor has a quasi-fixed cost component and small

[6] Antitakeover laws have been associated with reductions in firm leverage (Garvey and Hanka, 1999), managerial's ownership (Cheng, Nagar and Rajan, 2004) and patenting (Atanassov, 2013). Such laws have lead to increases in cash holdings (Yun, 2009), bond values (Francis, Hasan and Waisman, 2010) and acquisitions of "cash-cow" firms (Gormley and Matsa, 2014). The previous literature has studied wages (Bertrand and Mullainathan, 2003) or ROA (Giroud and Mueller, 2010).

[7] Of course, if innovation between rivals are strategic complements, an increase in innovation by a firm will lead other firms to innovate more.

private firms are more credit constrained than listed firms (e.g., Benmelech, Bergman and Seru, 2011).

Innovations across neighboring firms can also be strategic complements because knowledge is non-exclusive and non-rival (Arrow, 1962). Once introduced, knowledge is subsequently available to other local firms, which increases their ability to innovate because they can use this outside pool of knowledge. Proximity will matter because knowledge can be vague, difficult to codify and - as such - transmitted mostly via social interaction. For example failed experiments may produce knowledge for people nearby but such knowledge is less likely to reverberate in far away areas.[8] Proximity can also increase the likelihood that other inventors hear about about new inventions before inventors located further away, giving them a time advantage. Using patent citations as a proxy for knowledge spillovers, Jaffe et al. (1993) and subsequent papers (e.g., Belenzon and Schankerman, 2013) find that patent citations are highly localized and decline quickly with distance, which implies that the surrounding stock of existing knowledge should generate spillovers on local firms by affecting their ability to innovate, but only at a limited geographical scale.

Finally, both the local stock of knowledge and innovation activity by local listed and private firms might be determined by an omitted variable. For instance proximity with universities is positively linked with firms' innovative activities (Audretsch and Feldman, 1996; Andersson et al., 2009; and Kantor and Whalley, 2014). Similarly, the quality of life provided by local amenities such as nice weather, the absence of violent crime, the presence of multiple goods (restaurants, arts, etc.) attract educated workers (Shapiro,

[8]The literature makes the distinction between *hard information* and what is often called *"tacit knowledge"*. See for instance Jacobs (1969) or the discussion in Glaeser et al. (1992) and Feldman and Kogel (2010).

2006; Glaeser et al. 2010) which conversely is likely to affect innovation by local firms.[9] Therefore, it is also quite possible that local innovation spillovers do not exist, or are of rather limited scope, and are overestimated in naive regressions that neglect endogeneity concerns. In my setting, it implies that the patenting growth of private firms should not react to an exogenous change in innovation by listed firms.

If on the contrary listed firms' innovation generate local innovation spillovers for private firms, we should observe the following:

H1: Innovation by listed firms affects the patenting growth of private firms located in the same area and this effect quickly declines with distance.

Merely offering evidence of the existence of local innovation spillovers does not inform us about why they happen. One important mechanism is likely to be that knowledge produced by listed firms, as proxy by their patents, spreads locally to private firms. Notwithstanding the challenge to identify knowledge diffusion,[10] the literature on agglomeration has suggested two main channels.

First, knowledge spreads locally because physical proximity increases the ability of workers to exchange ideas and learn about important incipient knowledge (e.g., Saxenian, 1994; Crescenzi, Nathan and Rodriguez-Pose, 2013). But spatial proximity is not sufficient in and of itself to ensure the transmission of knowledge. Individuals must overcome other dimensions of distance, both cognitive and social, to efficiently exchange knowledge. Therefore, the magnitude of spillovers should depend on both the degree of technological overlap between firms (Jaffe, 1986) and on workers' abilities to recog-

[9]In their survey about agglomeration, Glaeser and Gottlieb (2009) note that "no variable can better predict city growth over the past 50 years than January temperature".

[10]As noted by Krugman (1991) *"knowledge flows are invisible, they leave no paper trail by which they may be measured and tracked"*.

nize, assimilate and apply new knowledge, which may be higher among educated workers (e.g., Cohen and Levinthal, 1990; Moretti, 2004b). Although we cannot exactly follow knowledge diffusion in this case, this channel implies the following hypothesis:

H2: Local innovation spillovers between listed and private firms should increase with the degree of technological proximity, intensity of information flow and density of skilled workers in the area.

The second channel of knowledge diffusion locally involve inventors moving from their employer to both existing firms and newly started spin-outs in the same area. Indeed, knowledge is embedded in inventors. By switching employers locally, inventors create regional networks of collaboration through which knowledge can be transferred (Almeida and Kogut, 1999; Breschi and Lissoni, 2009; Singh and Agrawal, 2011).

A close variation is if inventors move from their employer to create a spin-out. In this case, new entrepreneurs build on knowledge learned when working for their previous employers. The increase in the knowledge stock will foster entrepreneurship through spin-outs because such vehicles may be the best means for an employee with a given endowment of new knowledge to capture the returns from that knowledge (Audretsch, 1995). According to Aggrawal, Cockburn, Galasso and Oettl (2014), this argument explains why regions with large labs are more innovative, i.e., because they are more likely to spawn spin-outs unrelated to lab-owning firm's overall business.

Thus, I propose my third hypothesis:

H3: Local innovation by listed firms should foster inventors moving locally from their employer to existing private firms or new spin-outs.

Finally, if listed firms' innovation generate innovation by private firms in the same area, we may expect venture capital funds from outside the area to invest more in areas where local innovation spillovers happen. Conversely, we should expect capital availability to determine the magnitude of local innovation spillovers. Indeed, if an increase in the amount of local patents filed by listed firms improves private firms' abilities to innovate, they may not be able to effectively do so if they are credit constrained. Thus, I posit my final hypothesis:

H4: Venture capital funds from outside the area should concentrate their investments in areas where local innovation spillovers are more important. Conversely, capital availability should amplify the magnitude of innovation spillovers.

If the foregoing propositions are true, it would suggest that capital mobility, can contribute to the gaps between cities by allocating resources to those cities where potential spillovers are higher.

3.2 Data

3.2.1 Innovation

I use patents filed with the US Patent and Trademark Office (USPTO), as compiled in the National Bureau of Economic Research (NBER) Patents File (Hall, Jaffe, and Trajtenberg 2000) to measure innovation. These data contain all patents granted in the US, including information about the patentee (including a unique identifier, institutional characteristics, nationality, and geographic localization) and about the patent (year of application, technology class, and number of citations received). An appealing feature of the NBER Patents File is that it covers the entire universe of patents filed in the US, including patents filed by young and private firms. I follow the procedure developed in Hall, Jaffe and Trajtenberg (2001) and Bessen (2009) to match patents through Compustat to identify patents filed by listed firms.

Both listed firms and private firms play an important role in innovation activity in the US. Throughout my sample period, the fraction of patents filed by listed firms is relatively stable at approximately 50%-60%. Therefore, a shock on the patenting activity of publicly listed firms can plausibly affect innovation by private firms.

I keep only those patents filed by US corporations in my sample and exclude patents filed by foreign firms, universities, and government agencies.[11] I date patents by the year in which the application was filed to avoid anomalies resulting from a lag between the application and the grant dates. I consider all patents filed between 1975 and 2000 (the first year and last year where the truncation bias is limited).

To obtain the location of the inventors at the county level, I use the Harvard Patent Database[12] which provides the latitude and longitude for each inventor associated with a patent. These coordinates can then be used to obtain the exact county in which a patent has been created.[13]

Patents have long been used as an indicator of innovative activity (Griliches 1990); this measure, however, contain two sources of "noise" that limit the perfect correspondence between patents and ideas. First the propensity to patent a new idea may vary across

[11] I exclude foreign firms because these firms frequently file patents with the USPTO to protect their innovations on US soil but actually seek financing and conduct their R&D in their home country (see Acharya and Subramanian 2009).

[12] The data are available at http://dvn.iq.harvard.edu/dvn/dv/patent

[13] I am extremely grateful to Juanita Gonzalez Uribe for sharing with me the mapping between latitude-longitude in the patent database and county geographical coordinates.

firms and cities, and some new ideas may not meet the criteria for patentability, or firms may rely on secrecy or other means to protect innovation. Despite these drawbacks, there is nevertheless a strong relationship between R&D and the number of patents in the cross-section of firms (R-squared is 0.9; see Griliches 1990). Second, patents can be heterogeneous in the number of ideas they contain (e.g., Jaffe and Trajtenberg, 2002). However, as noted by Peri (2005), this problem is largely attenuated in studies using patents aggregated at a geographical level (in contrast with firm-level studies) because differences in the quality of patents is likely to average out in the aggregate. The problem of potential differences in patenting is addressed using fixed effects.

3.2.2 Geographic Area: Commuting Zones

The geographical unit of observation I use to estimate the local effect of innovation by listed firms is the Commuting Zone (CZ). The CZ concept was developed by Tolbert and Sizer (1996), who used county level commuting data from the 1990 Census to create 741 clusters of counties that are characterized by strong commuting ties within CZs and weak commuting ties across CZs.[14]

I restrict my analysis to CZs in which I can observe patents during the 1975-2000 period, which results in a balanced panel of 685 distinct CZs, mapping 48 states in the US (the three missing states are Alaska, Hawaii and the District of Columbia).

CZs have three main advantages. First, they are based on economic geography rather than political boundaries and, as such, are a suitable candidate to estimate the scope of innovation spillovers. Second, they cover the entire United States (as opposed to

[14]CZs have been used in the recent literature, such as by Autor and Dorn (2013), Autor, Dorn and Hanson (2013); Adelino, Ma and Robinson (2014) and Chetty et al. (2014).

metropolitan statistical areas (MSAs), for instance, that do not contain rural areas, which represents two third of all counties in the US). Finally, using the cross-walk developed by Dorn (2009), CZs can be consistently constructed using the Census Public Use Microdata Areas (PUMAs).

Therefore, CZs represent an appealing geographic level because to estimate how a shock on a group of firms affects the behavior of non-affected neighboring firms, the geographical unit must satisfy the following three criteria. 1) it must be sufficiently small so that spillovers can plausibly occur because, the literature finds that spillovers occur on relatively small scales. 2) its geographical boundaries must be constant over time, which makes otherwise natural candidates such as MSAs unappealing. 3) it must offer sufficient information in terms of the characteristics of the labor market or the population, which makes counties somewhat unappealing because they cannot be identified in the Census Public Use Micro Areas (PUMAs) and as such, strongly limit the amount of information that researchers can construct.

Because I'm interested in geographical spillovers, I aggregate patents at the CZ level. However, this decision implies that I will capture innovation spillovers both within and between industries. This aggregation is actually attractive because I am studying innovation spillovers on the production of other new ideas, which can occur across sectors. For instance, Jaffe et al. (1993) report that up to 25% of citations occur *across* five broad technological fields. When looking at the 3-digit level (approximately 450 technological fields) approximately 40% of citations are across fields.[15] These high numbers suggest the existence of important inter-sector spillovers and justify aggregating all patents at

[15]These five broad technological fields include the following: 1) Drugs and Medical Technology; (2) Chemicals and Chemical Processes Excluding Drugs; (3) Electronics, Optics, and Nuclear Technologies; (4) Mechanical Arts; and (5) All Other.

the CZ level.[16]

3.2.3 Local Labor Markets Characteristics

I construct different characteristics at the CZ level using various datasources. The main source is the Census Integrated Public Use Micro Samples for the years 1970, 1980, 1990, and 2000 (Ruggles et al. 2010).[17] I apply the usual restrictions to compute labor market characteristics: individuals must be between 16 and 64 and be working in the year preceding the survey (*empstatd* between 10 and 12). Residents of institutional group quarters such as prisons and psychiatric institutions are dropped (*gqtype* different from 0) as well as unpaid family workers (*classwkrd*=29). All calculations are weighted by the Census sampling weight (*perwt*) multiplied by the weight from Dorn (2009). Population estimates on a yearly basis are from the Census.[18]

Data on venture capital activity and venture capital funds availability come from the VentureXpert database. I identify the CZ in which the fund is located and where it makes an investment using the zipcode information provided by Venture Xpert and by mapping the zipcode with its county. I then map counties with CZs using Dorn's (2009) correspondence table.

Finally, data regarding educational attainment, number of colleges and federal R&D expenses are from the National Science Foundation's CASPAR database.

Table 3.1 provides summary statistics for the main variables used in the paper.

[16] As noted by Henderson et al. (2005), one reason is that plain imitation cannot be patented (given that novelty is a prerequisite to be allowed to fill a patent). Therefore, there must be some technological distance for spillovers to occur.

[17] The Census samples for 1980, 1990, and 2000 include 5% of the US population, the 1970 Census and ACS sample include 1% of the population. The Census 1970 corresponds to the "Census Metro2".

[18] Appendix 3.7.1 details the construction of the variables.

[INSERT TABLE 3.1 ABOUT HERE]

3.3 Identification Strategy

3.3.1 Empirical Specification

Since Griliches (1979), the standard approach to study innovation spillovers has been to add a measure of external knowledge stock available to the firm innovation production function. The justification for using the stock of knowledge and not only the flow of new knowledge is that because knowledge is non rival (Arrow, 1962): even past knowledge can increase firm ability to innovation currently.[19]

In my case, the innovative output of private firms located in CZ c, state s and year t denoted Y_{cst}, will depend on various time-varying characteristics at the CZ level, denoted X_{cst}, and on the stock of external knowledge accumulated from past and current innovation activities by listed firms that I proxy using the stock of patents by listed firms. Using a log-transformation of the innovation production function, the log level of innovative output in CZ c, state s and year t is equal to:

$$Log(Y_{cst}) = \alpha_c + \delta_t + \beta\ Log(StockListedPatents_{c(t-1)}) + LogX_{ct} + \gamma_{st} + \epsilon_{cst} \tag{3.1}$$

The stock of patents by listed firms filed in CZ c is constructed using the standard perpetual inventory method with a 15% depreciation rate.[20] The stock of listed patents in

[19]See e.g. Peri (2005), Griffith et al. (2006) or Bloom et al. (2013) for recent applications.

[20]0.15 is the value suggested by Hall, Jaffe and Trajtenberg (2005). I also use a depreciation of 0.1 as in Keller (2002) or Peri (2005) and find similar results.

year t is $Stock_t = (1-\eta)Stock_{t-1} + Listed\ Patents_t$ where $Listed\ Patents_t$ is the number of new patents filed by listed firms in year t and η=0.15.[21] α_c, and δ_t denote CZ and year fixed effects. CZ fixed effects capture time-invariant determinants of innovation in the different CZs, such as geographic characteristics or the presence of an important university. Year fixed effects control for aggregate shocks and common trends in innovation activity.[22] Finally, I add State $\times$ Year fixed effects denoted γ_{st} to remove any time varying shocks or state characteristics that might affect innovation by all firms, such as state business cycles or, time-varying state institutional or policy differences (e.g., marginal tax rate).

The parameter of interest is β, which measures the extent to which private firms react to the innovation activity of listed firms. Given the State $\times$ Year fixed effects, β captures only spillovers that occur within a state across CZs and does not include variations coming from CZs in different states. Because I aggregate innovation at the CZ level, I cluster standard errors at the CZ level. The main challenge when estimating this type of equation is that innovation activity of both private and listed firms in a given city is likely to be endogenous for two reasons. First, innovation by all firms in the area might be affected by unobserved shocks (e.g., a major technological breakthrough). Second, this specification is confronted with the "reflection problem" described in Manski (1993). Each firm adapts its investment and innovation strategy by reacting to the decisions of the other firms in its environment. As such the link between the two reflects the equilibrium reached by the endogenous answer of the various firms.

[21] I make the simplifying assumption that the initial stock of patents by listed firms is zero to reduce the number of assumptions I must make to initialize the stock.

[22] Such common shocks can be caused by changes in the legal and institutional environment at the federal level, such as the creation of the Court of Appeals for the Federal Circuit in 1982.

To correct for these problems, I predict variations in the supply of patents by listed firms that only come from characteristics unrelated to the area in which the firm is active and use this prediction as my instrument. Therefore, the fluctuations in the predicted measure of patents is uncorrelated with unobserved local conditions or private firm innovations. The shock on innovation by listed firms comes from the adoption of BC laws.

A challenge that arises when instrumenting innovation of a group of firms is that this instrument will likely affect other dimensions of firms' policies, violating the exclusion restriction. In the case of BC laws, we know for instance that they lead to higher wages (Bertrand and Mullanaithan, 2003) or lower profitability (Giroud and Mueller, 2010). Those changes may then generate variations in innovation by private firms. My identifying assumption is that variations in patents by private firms are mostly generated by variations in patents by listed firms and not by other listed firms' policies. As a way to validate this identification assumption, I provide multiple cross-sectional tests in the second part of the paper consistent with local innovation spillovers happening essentially because knowledge diffuses across firms in the same CZ. These results are harder to reconcile with other interpretations.

3.3.2 Exogenous Variations in Innovation by Listed Firms

Antitakeover Laws

In the 1980s and early 1990s, states adopted what are generally referred to as the "second generation" of antitakeover laws. The most stringent of these are called "business combination laws" (BC laws).[23]

[23] For a detailed history of first and second generation of antitakeover laws, see Kahan and Kamar, 2002; Bebchuck, Cohen and Ferrel, 2002 or Bertrand and Mullanaithan, 2003).

BC laws strongly limit the likelihood that a firm will be the target of a highly leveraged hostile takeover, by restricting a raider's ability to sell the assets of the acquired firm. Because these takeovers are frequently financed by means of the sale of certain of the target's assets, BC laws have effectively insulated managers from hostile takeovers. Therefore, their adoption can be considered as a valid source of variation in corporate governance. In particular, BC laws allow managers to follow preferences that are not necessary aligned with shareholders' best interests. Two types of these preferences would lead to a decline in innovation. First managers might exert less effort based on their intention to "enjoy the quiet life" (Bertrand and Mullanaithan, 2003). Second, risk-averse or career-concerned managers might undertake less risk than desired by a diversified shareholder and decide to "play it safe" (Gormley and Masta, 2014). Both types of behavior have been found to increase after BC laws were adopted.

One remaining problem is that the law may change or reflect the state's economic context. To deal with this, I exploit the geographic dispersion of innovation by listed firms. For instance, listed firms file only 20% of their patents in their state of incorporation. So I exclude from my analysis innovation by firms in their state of incorporation. For example, I consider a firm incorporated in Virginia but that files patents in Austin. When Virginia passes a BC law in 1988, the firm reduces its innovation in all areas, including Austin. I use this to study the impact on innovations by local private firms in Austin.

Building an instrument from BC law adoptions

To instrument variations in patents by listed firms at the CZ level, I adopt a two-step procedure. First, I estimate the expected number of patents generated by the BC laws and then I aggregate this estimate at the CZ-year level and use it as my instrument.[24]

Because the effect of BC law adoption on innovation is likely to be non-linear and to obtain a more precise estimate of its effect, I start my analysis at the listed firm-CZ-year level. It allows me to remove heterogeneity both across firms but also across firm-CZs.

After I drop all patents in CZs located in the state of incorporation of the firm, I predict fluctuations in patents by listed firms in the CZs in which the firm is active using the adoption of BC laws after I filter Firm × CZ and year fixed effects. I follow Atanassov (2013) and run a standard difference-in-differences strategy, in which I explain the number of patents that the listed firm i, files in CZ c, and year t as follows:

$$Log(1 + ListedPatents_{ict}) = \alpha_i \times \gamma_c + \delta_t + \beta\ BC_{it} + \epsilon_{it} \tag{3.2}$$

where BC_{it} is a dummy variable equals to one if firm i is incorporated in a state that has passed a BC law after year t. $\alpha_i \times \gamma_c$ denote Firm × CZ fixed effects and δ_t denote year fixed effects.

I then predict the value of patents using only βBC_{it} and aggregate it at the CZ-year level. The predicted number of patents in CZ c in year t is thus equal to: $\widehat{ListedPatents}_{ct}$ $= \exp\ [\sum_{i \in CZ} \beta BC_{ict}]$-1

Therefore, I obtain fluctuations in the number of patents by listed firms that only come from the adoption of BC laws and not from local economic activity or productivity shocks in the CZ. I then use the predicted value of patent flow $\widehat{ListedPatents}_{ct}$ to create a predicted value of the stock of patents by listed firms using the same perpetual inven-

[24]See Wooldridge (2002) for a discussion of this two step-procedure and Paravisini (2008) for example in banks' allocation of government funds or Boustan (2010) for example in the case of black migration across US cities.)

tory method that is described in section 3.3.1. From there, I can run a standard 2SLS regression using the predicted value $\widehat{StockListedPatents}_{ct}$ to instrument the actual stock of patents by listed firms $StockListedPatents_{ct}$ in a given CZ.

It should be noted that even if the adoption of a BC law constitutes a plausible source of *variation* in the number of patents filed by listed firms exogenous to local CZ attributes, one source of endogeneity remains. Indeed, the allocation of *where* a listed firm decides to conduct its research activity initially is not a random decision. For instance, assume that Austin-San Marcos (Texas) experiences a productivity shock. In that event, listed firms are more likely to conduct their research activity there, producing a spurious correlation between patents filed by listed firms and private firms. However, after the first year, the evolution of patents by listed firms will again only depend on the BC laws. Therefore, the threat to identification comes from the entry of new listed firms in my sample.

To address this problem, I use patents by listed firms that are present for the entire period of my analysis (25 years) and consider that they begin their innovative activities from the beginning in all the CZs in which they will patent at some time. As such, the only variation in patents by listed firms comes from adopting the BC law. One problem with this strategy is that it reduces the number of listed firms, as it leaves me with 1.491 firms. I also run a similar regression with the complete sample (16.914 firms) to generate a prediction based on this sample and find similar results, which suggests that the magnitude of the bias that entry could produce is very small. Nevertheless, I restrict the analysis to the balanced sample.

Table 3.2 shows the effect of adopting a BC law on listed firms' innovation for the balanced sample from 1975 to 2000. Adopting a BC law generates a decline in patenting between 0.04% to 0.06% depending on the specification, always highly significant at the 1% level and equivalent in magnitude to the estimation finds in Atanassov (2013). I also check in column (2) whether the results hold when I include Industry × Year fixed effects that absorbs time varying fluctuations at the industry level (such as technology or sale shocks). In column (3) I also add also CZ × Year fixed effects, which absorb any CZ-specific time-varying shocks that are shared by all firms in the same CZ, such as business activity or productivity shocks.[25] Finally, Column (4) excludes listed firms incorporated in Delaware and column (5) excludes patents filed in California. In all cases the results continues to be negative and strongly significant.

[INSERT TABLE 3.2 ABOUT HERE]

I also check that the results are not capturing a trend. To do so, I plot the evolution of patenting activity around the regulation date. In Figure 3.2, I estimate equation (3.2) but replace the adoption of the BC law with dummy variables for each year from 10 years before to 10 years after the regulation. Reassuringly, there is no trend before the event date, which is consistent with my identifying assumption that BC laws are not endogenous to innovation. Figure 3.2 also shows that the effect of regulation materializes only progressively after the event date which is expected because firms are likely to adjust progressively to new environments.

[INSERT FIGURE 3.2 ABOUT HERE]

Identifying the effects of innovation by listed firms on private firms exploits the fact that CZs will be more or less affected by the shock generated by the adoption of BC laws.

[25]For example assume that I have only two firms in a given CZ, the identification comes from the fact that one firm will be incorporated in New York where a B.C. law was adopted in 1985, whereas the other is incorporated in California (where such a law never adopted.

Therefore, Figure 3.1 maps the distribution of patents filed by listed firms before 1984 (the last year before the adoption of the first BC law) that will be affected at some point in time by BC laws.

Figure 3.1 shows that listed firms affected by BC laws represent an important part of all patents filed by listed firms throughout the US and is not limited to a specific area. This widespread distribution reduces the risks that my identification will only capture evolution that is specific to a limited number of geographic areas.[26]

[INSERT FIGURE 3.1 ABOUT HERE]

3.3.3 First-Stage Results

Using BC laws to predict innovation by listed firms in states outside their state of incorporation ensures that this prediction is not correlated with CZ characteristics where listed firms innovate. Figure 3.3 graphs the first stage relationship between the predicted stock of patents by listed firms and the actual stock, after I filter CZ and State $\times$ Year fixed effects and cluster at the state level. I find a strong positive relationship between the two measures, which confirms that the prediction provides a valid instrument. The coefficient for the first stage is 0.45 with a t-statistic of 16, which is well above the conventional threshold for a strong instrument.[27] I obtain similar results when I use changes in the stock of patents rather than the level.

[26]It should be noted that the identification actually does not come uniquely from the comparison between CZs with more or fewer listed firms affected by a BC law, but also from the *composition* of the different states of incorporation. Indeed, because of the difference-in-differences strategy employed in the first stage, firms affected by BC laws are in both the control and treated groups. Therefore, a CZ with a majority of firms incorporated in New York will be affected by the reform beginning in 1985 (the year in which the BC law was adopted in NY), whereas a CZ with a majority of firms incorporated in Massachusetts will only be affected after 1989.

[27]Because I use a predictive value for the stock of patents by listed firms, I correct the standard errors using 1,000 bootstrap replications over firms by randomly drawing with replacements firms in the sample of states in which they operate.

[INSERT FIGURE 3.3 ABOUT HERE]

3.4 Local Innovation Spillovers

3.4.1 Baseline Results

I begin by investigating the effect of the stock of patents filed by listed firms on the number of patents filed by private firms in a given CZ in the following year. The results are reported in Table 3.3. Column (1) shows the naive OLS. The elasticity of patents filed by private firms to the stock of patents filed by listed firms is 24%. Columns (2) to (5) report the estimated elasticity when I instrument the stock of patents filed by listed firms using the adoption of BC laws. In every case, the effect is strongly significant at the 1% level and with an elasticity between 21% and 18%. The fact that the IV estimate does not differ substantially from the OLS estimate suggests that the size of the bias is limited in this context.

Column (3) explores how the effect evolves with distance. I define *Stock Listed Patents Close* CZ_{ct} as the stock of patents by listed firms filed in the four closest neighbor surrounding the CZ c. I also calculate the stock of patents by listed firms filed in the *next* four closest neighbors labeled "$DistantCZ_{ct}$". I identify close neighbors and distant neighbors by calculating the geographical distance between each CZ using the average latitude and longitude of all zipcodes located in the CZ.[28] I find that the stock of patents by listed firms filed in close neighboring CZs has a small positive effect on innovation by private firms, but the effect become undistinguishable from zero for distant neighbors. This sharp

[28]E.g., similar strategies have been used for instance in Wilson (2009) for states and Dessaint and Matray (2014) for counties.

decrease with distance is consistent with other papers documenting that "knowledge does not travel well".[29] It also implies that analyses attempting to estimate spillovers in innovation at the state level are likely to underestimate their existence because they occur on a much smaller scale.

In columns (4) and (5), I follow Moretti (2004b) and add various controls at the CZ level that might influence my estimation. Column (4) adds "demographic" controls such as the share of African-Americans and women in the population, in addition to population density and the share of population living in an urban area, given the importance of cities in fostering innovation (e.g., Glaeser and Gottlieb, 2009 or Carlino and Kerr, 2014 for surveys). In column (5) I add economic and education controls. Education controls include the number of doctorates granted each year, the number of existing college institutions reported by the Integrated Postsecondary Education Data System (IPEDS) and the R&D conducted at local universities. Economic controls capture various economic and technologic dimensions: *Personal Income per Capita*, *Number of Firms* that I proxy using the number of establishments from the CBP, *Share of self employed* among the working population, *Industry specialization* defined as the local Hirschmann-Herfindahl Index for the 10 economic sectors available in the BEA.[30] *Technology specialisation* defined as the local Herfindahl of technology classes (thus in both cases, the greater this

[29]Given that the average distance for CZs in the neighborhood zone is approximately 100 miles and the distance for CZs in the remote neighboring zone is approximately 190 miles, my estimation is in the ballpark of that found by other papers. For instance Duranton and Overman (2002) find that geographic spillovers concentrate at a scale of approximately 30 miles, whereas Botazzi and Peri (2003) find that knowledge spillovers exists between 0 and 450 miles in their study of European regions. In the US, Lychagin et al. (2010) find that the effect disappears after around 300 miles and Belenzon and Schankerman (2013) find that knowledge spillovers decline at a distance of up to 150 miles. Similarly, studying the clustering of R&D labs, Carlino et al. 2012 find the scale of the clustering they observe is comparable to local labor markets, which are equivalent to CZs.

[30]Those sectors include the following: Agriculture, Mining, Construction, Manufacturing, Transportation, Wholesale trade, Retail trade, Finance, Services, Public Administration.

measure, the more highly specialized that a given CZ is); *Technology age* which is defined as the average age of technologies exploited in a CZ captures the fact that CZs working in newer, more fertile technologies may generate more patents. I also add the amount of venture capital investment made. Reassuringly, the coefficient for the stock of knowledge filed by listed firms is stable across the different specifications. Because several of those variables are likely to be directly influenced by the stock of patents filed by listed firms, I use only demographic controls in the rest of the paper because they are less likely to react immediately to innovation by listed firms and also control for the number of establishments.[31]

In term of economic magnitude, an elasticity of 0.2 implies that changing the stock of patents by listed firms by 1% changes similarly the number of patents filed by private by 0.2%. In term of within CZ standard deviation, I find that on average, a within CZ one standard deviation variation in the stock of patents by listed firms explains nearly 20% of the within CZ standard deviation of patents filed by private firms during the sample period. To have an estimation in term of patents, I have to multiply the elasticity by the ratio of the stock of patents filed by private firms over the stock of patents filed by listed firms. It implies that a variation in 1 patent filed by listed firms generates a similar variation in 0.14 patent filed by private firms.

Another possibility is to perform the following thought experiment. The average listed firm has a local stock of around 100 patents in a given CZ. If I relocate this activity to a new Commuting Zone, it will generate around 14 additional patents by private firms, which represents a 18% increase compared to the within CZ standard deviation of patents

[31] See for instance Gormley and Matsa (2013) for a discussion of the problem to include endogenous controls in a regression.

filed by private firms. This suggests a substantial effect that could explain why cities and states compete to attract R&D activities (Wilson, 2009).

[INSERT TABLE 3.3 ABOUT HERE]

Having established that innovation spillovers occur and are bounded spatially, I now explore how the local production of knowledge by listed firms proxy by their patents filed in a given CZ spreads across firms in the same area.

3.4.2 Knowledge Diffusion Channels

How and why does knowledge spread locally? What are the channels through which knowledge is transferred? In this section, I explore two channels through which knowledge diffuses from innovative listed firms to other private firms in the same area: learning across local firms and inventors moving across existing firms or founding or joining local spin-outs.

Effect Depending on Learning Opportunities

Technological proximity

If knowledge transmission plays an important role in the existence of local innovation spillovers, we should expect the magnitude of innovation spillovers to vary as a function of the technological proximity among listed firms and private firms in the same area.

I use two different proxies to capture the degree of technological proximity: the propensity of private firms to build on innovation produced by listed firms and the degree of technological overlap between listed firms and private firms.

I proxy the propensity of private firms to use innovation produced by listed firms and produced locally from patent citations. I define the propensity to use innovation produced by listed firms as follows. I examine all the citations *made by* patents filed by private firms in a given CZ. I then calculate the share of citations of listed firms' patents over the total number of citations made. Finally, I aggregate the foregoing data at the CZ-year level.

To proxy technological overlap, I use the measure of technological proximity introduced by Jaffe (1986). For each CZ, I calculate the number of patents granted to each firm by technological categories.[32] The share of patents granted to firm i located in CZ c in each technological class s (s=1, ...425) is then arranged in a vector T_{ic} = (T_{ic1},... T_{ic425}). The technological proximity in CZ c is defined as the uncentered correlation coefficient between the vectors of all firm i,j pairings, calculated as: $TECH\ CORR_c$ $=(T_{ic}T'_{jc})/[(T_{ic}T'_{ic})^{1/2}(T_{jc}T'_{jc})^{1/2}]$. The index ranges from 0 to 1, depending on the degree of technological overlap between firms. The closer this index is to 1 the more that firms located in CZ c overlap in technological classes.

One drawback of the Jaffe distance is that it considers proximity only within the same technology class. It is all the more so problematic that an important fraction of knowledge flows are inter-industries (around 40% of citations are across 3-digit technologies, Jaffe et al., 1993). I use the Mahalanobis Distance developed by Bloom et al. (2013) that allows to calculate a degree of technological proximity between different technology classes (see the Appendix C.2 of Bloom et al. (2013) for a detailed description of the metric).

The correlation between the technological proximity measured by the propensity to

[32] I use the disaggregated 3-digit (425 distinct) technological categories. Results are similar when I use the smaller division in 36 categories.

cite patents by listed firms and the two other proxies based on technological overlap across patent classes is quite low (between 20% and 30%), which suggests that I'm capturing different dimensions of technological proximity.[33] To obtain the marginal additional effect that each proxy create with respect to the mean effect of *Stock Listed Patents*, I demean all the proxies and interact them with the main variable *Stock Listed Patents*.

Column (1) of Table 3.4 reports the result when I interact the stock of patents held by listed firms with the propensity of private firms to cite listed firms' patents. Consistent with the intuition that spillovers should be more important when private firms rely more on technologies produced by listed firms, I find that the interaction term is positive and strongly significant. In terms of economic magnitude, increasing the fraction of citations of listed firms' patents by one standard deviation increases innovation spillovers by 11%. Columns (2) and (3) show a similar amplification when I interact the stock of patents filed by listed firms with the degree of technological proximity using the Jaffe distance and the Mahalanobis distance. Finally, columns (4) and (5) include in the regression two different measures of proximity (citations of listed firms and Jaffe distance or citation and Mahalanobis distance) and finds that each has a positive impact on spillovers. This result confirms that each measure captures a different dimension of learning opportunities that matters for local innovation spillovers.

[INSERT TABLE 3.4 ABOUT HERE]

Density of skilled workers

Marshall is among the first to notice that social interactions among workers create learning opportunities that enhance their productivity (1890). As he writes in his *Principles of*

[33]However the two measures of technological proximity based on the distance defined by Jaffe (1986) or Bloom et al. (2013) is very high.

Economics: "(...) so great are the advantages which people following the same skilled trade get from near neighborhood to one another. The mysteries of the trade become no mysteries; but are as it were in the air, and children learn many of them unconsciously".[34]

The challenge with this channel is that economists cannot directly observe communications, discussions or gossips among workers. Instead, I exploit the prediction that spillovers should be more important in areas in which workers can interact and learn more easily from one another. In particular, I expect two CZs facing the same shock on listed firms' innovation to react differently depending on the density of skilled workers. Indeed, to engage in fruitful knowledge exchanges, being geographically close is not sufficient. Workers must first have the capacity to understand and assimilate new knowledge and second to share social ties to a certain degree. In her study of knowledge sharing in the Silicon Valley, Saxenian (1999) notes: "The initial social connections often have a basis in shared educational experiences, technical backgrounds, (...)".

The different proxies for local workers' ability to learn from one another and the intensity of social interactions are built on the analysis of Moretti (2004b) Glaeser and Resseger (2010) and Belenzon and Schankerman (2013), among others. I use the supply of scientists and engineers (S&E) and college graduates in a given CZ at the beginning of my sample period.

To construct the two proxies, I use 1970 census data (1% Form 2 Metro Sample) and the methodology of Autor and Dorn (2013) to aggregate Census Public Micro Samples at the CZ level. Scientists and engineers are identified using the consistent *occupation* variable in 1990.[35]

[34]Or as Glaeser, Kallal, Scheinkman and Shleifer (1992) write more directly: "After all, intellectual breakthroughs must cross hallways and streets more easily than oceans and continents."

[35]For the Census in 1970, I use the crosswalk provided by Dorn (2009) that is kindly made available

Table 3.5 shows how the density of skilled workers in a CZ affect the magnitude of local innovation spillovers generated by listed firms. Consistent with the intuition that having a greater "brain density" fosters local innovation spillovers, I find that the stock of patents by listed firms has a greater effect when the supply of S&E is higher.[36] Column (2) shows a similar result when I proxy learning opportunities using the supply of college graduates.[37] The effect is economically sizable and implies that the last quartile of the college graduate distribution experiences spillovers that are twice as large as those experienced by CZs in the first quartile of the distribution.

[INSERT TABLE 3.5 ABOUT HERE]

The inherent limit of cross-sectional tests is that potentially, unobserved characteristics may be correlated with the variables used in the cross-section. For instance, CZs with a higher supply of college graduates might also differ in other dimensions such as investment opportunities which could also foster local innovation spillovers. Ideally, we would like to instrument every variable. Although I cannot (unfortunately) find different instruments for each variable, the literature on agglomeration economics has suggested two possible instruments for the share of college graduates.

The first instrument comes from Beaudry, Doms and Lewis (2010)[38] and uses the share of 15-19 year-olds enrolled in school in 1880, which proxies for the local availability of high schools at that time. To provide a valid instrument, this deep lagged variable must be uncorrelated with current local economy specialization and technology development,

on his webpage.

[36]Because all my proxies are time invariant, the simple term is absorbed by the CZ fixed effect.

[37]Similarly, Kantor and Whalley (2014) find that academic research spillovers are more important when the university is located in a county with a higher level of college-educated workers.

[38]I am deeply thankful to Ethan Lewis for answering my questions about his instrument.

which would not be the case if, school enrollment in 1880, for instance, was correlated with physical capital at that time and if capital has built up over time. In this case, capital accumulation would make the area more productive, violating the exclusion condition of the instrument. Beaudry et al. (2010) argues that capital and skill were more substitutes than complements prior to the twentieth century (Goldin and Katz, 2008). Therefore, the reasons why some areas had better high schools in 1880 were unlikely to be related to economic and technological development in 1880 and in the following periods.[39]

High school enrollment in 1880 is a good predictor of the share of educated workers a century later. The coefficient is equal to .12 with an F-test of 13. Column (3) of Table 3.5 reports the effect of increasing the share of the college-educated population on the magnitude of local innovation spillovers when I instrument *College Graduate* by *School Enrollment 1880*. Again, I find a positive effect, with a similar order of magnitude.

The second instrument comes from Moretti (2004a), who shows that the presence of college and universities created in the nineteenth century following the "land-grant movement" still strongly predicts cross-sectional variation in college share today.

Following two acts in 1862 and 1890, the federal government gave every state a grant to establish colleges, which resulted in the creation of 69 colleges and universities, with each state having at least one. Because this program was undertaken more than a century ago and was not dependent on natural resources,[40] land-grant institution is unlikely to be correlated with unobservable factors that affect innovation today.[41]

[39] See Beaudry et al. (2010) for a detail discussion of the instrument.

[40] See Moretti, 2004a and Nervis, 1962 for a detail history of the land grant movement. From today's perspective, Moretti argues that "the geographical location of land-grant colleges seems close to random."

[41] Doms and Lewis (2006) and Shapiro (2006) also use this instrument. Shapiro (2006) shows that there is no difference in the human capital distribution between land-grant and non land-grant cities from 1850 to 1880, i.e. before the land-grant movement. In addition, the correlation between land-grant establishments and college-educated workers arose strongly after 1940 when college attendance began to increase, a time when these institutions might have played a significant causal role.

Using the list of all land-grant institutions provided in the appendix of Nervis (1962), I create a dummy variable *Land-Grant* which is equal to one if the CZ contains at least one land-grant institution. I end up with 63 distinct CZs with at least one land-grant institution (in only six cases the CZ contains two land-grant institutions). When I regress the average share of college graduates over the sample period on the*Land-Grant* dummy, I obtain a strong effect both economically and statistically. The presence of a land-grant college in a CZ increases the share of college workers by 20%, with a t-stat over 9.4. It implies a F-test of 89, which is well above the 10 recommended by Stock and Yogo (2005) to have a strong instrument.

Column (4) shows the result when I instrument *College Graduate* by *Land-Grant* and confirms again that increasing the share of college graduates (in this case because the CZ has one land-grant institution) increases the innovation spillovers generated by listed firms.

Local Inventor Mobility and Spin-outs

The second channel through which knowledge can be transferred locally from one firm to another is by inventors moving across firms in the same area. Beginning with the case study of Saxenian (1994) who compares Silicon Valley with Route 128 (Boston), the ability for inventors to change jobs has been identified as a mechanism that fosters collaboration and learning. New workers can share ideas regarding how to organize research production, information about new technologies or about failed experiments that they experienced with previous employers. Adopting the Jaffe, Trajtenberg, and Henderson (1993) "paper trail" methodology to identify knowledge spillovers, Almeida and Kogut (1999) and Breschi and Lissoni (2009) provide evidence that knowledge is transferred locally by individuals who move from one organization to the other, but who do not relocate geographically.

I use two strategies to test this channel. First, I build on the literature studying the effects of "Non Compete Covenants Law". These laws restrict intrastate job mobility, because they specify a period during which employees cannot take a job with a competing company (typically within the same industry) located in the same state. By affecting mobility rate of employees, non-compete laws should affect the speed at which knowledge diffuses locally (Stuart and Sorenson, 2003; Garmaise, 2009; Marx et. al. 2007, 2010; Sorenson and Samila, 2011; Belenzon and Schankerman, 2013).

I create two measures of state-level differences in enforcing non-compete covenants. The first follows Stuart and Sorenson (2003) and is a dummy variable *Presence of Non-Compete Laws*: this variable equals one if the state enforces non-compete covenants.[42] The second follows Garmaise (2009) and is an index ranging from 0 to 7 and count the number of employer-friendly provisions: higher values indicate stronger enforceability of non-compete laws. Therefore, an increase in *Intensity of Non-Compete Law* implies greater difficulty for employees to move from one firm to another.

I then interact each variable with the stock of patents filed by listed firms. I expect that if knowledge is diffused by labor mobility, more stringent non-compete laws should limit local innovation spillovers.

Table 3.6 shows that the magnitude of spillovers is affected by non-compete laws. Column (1) reports the result when I use the dummy variable *Presence of Non-Compete*

[42] See Stuart and Sorenson (2003), Table 1 p.190)

Laws. Being in a state that enforces non-compete covenants reduces innovation spillovers by 0.8%, which is nearly half of the average effect. In columns (2) and (4), I exclude California from the sample because Fallick et al. (2006) show that cities in California are characterized by a higher rate of mobility of high-skilled workers (what they called "job-hopping") than cities in other states and are also more innovative. I find a slightly stronger effect. Column (3) shows the result when I use the degree of enforceability of non-compete laws and confirms that enforcement of non-compete covenants (an increase in the index) limits knowledge diffusion locally by reducing labor mobility, which ultimately reduces local innovation spillovers. The point estimate of the interaction term is equal to -0.04%, which implies that an increase in the enforcement of non-compete covenants strongly reduces local innovation spillovers. Taken together, these results suggest that states can have an important impact on the ability for local agglomerations to generate innovation spillovers by affecting the rate of labor mobility across local firms. These results confirm quantitatively the conjecture of Saxenian (1994) that the ability for employed inventors to change jobs may be an important determinant of a city's innovative capacities.

[INSERT TABLE 3.6 ABOUT HERE]

The second strategy to identify whether local innovation spillovers are the result of inventors moving across firms in the same area is to estimate directly whether variation in the stock of patents filed by listed firms affects the number of mobile inventors within a CZ. To perform this estimation, I use the unique inventor identifier provided by Lai, D'Amour, and Fleming (2009) that permits me to track inventors across firms and zipcodes.[43]

[43]Although patent data include the names of the inventors of every patent, they do not, however,

To measure inventor moving across local firms, I follow papers such as Marx et al. (2009) or Hombert and Matray (2014) and identify an inventor as changing employers when she files two successive patent applications that are assigned to different firms. Because I'm interested in innovation spillovers in a given CZ from listed firms to private firms, I define an inventor as moving if (i) the inventor's employer is different from the previous employer. (ii) the current employer is a private firm and the former employer is a listed firm, and (iii) the inventor was working in the same CZ.[44]

I construct the following three measures: *# Mobile Inventors from Listed Firms*$_{ct}$ is the number of inventors who work in a private firm at year t in CZ c, but who previously worked in a listed firm located in the same CZ. *Share of Mobile Inventors from Listed Firms*$_{ct}$ is the fraction of mobile inventors who worked in a listed firms located in the same CZ over the total of all mobile inventors who arrive in private firms in year t in CZ c, and *Share Inventors Previously in Listed Firms*$_{ct}$ is the share of all inventors working for private firms in year t in CZ c that formerly worked for a listed firm located in the same CZ.

I also explore a variation on the theme of inventor mobility: entrepreneurial spin-out.[45] In this case, inventors formerly employed by a listed firm may decide to leave their employer, to join a newly founded local spin-out in which they can exploit the

provide consistent listings of inventor names or unique inventor identifiers. To overcome this problem, Lai, D'Amour, and Fleming (2009) develop a disambiguation algorithm to create unique inventor identifiers. The data are available at http://dvn.iq.harvard.edu/dvn/dv/patent and the disambiguation algorithm is discussed in Lai, D'Amour, and Fleming (2009).

[44]When I observe a firm change, I do not know precisely when it occurred within the time interval between the two observations, which is however, unlikely to be a major problem because the average time between two consecutive observations is only 2.4 years. In the main analysis, I follow Marx, Strumsky, and Fleming (2009) and consider that the move occurs at the midpoint of the time window between the two observations. In unreported regressions, I obtain similar results when assuming that the move occurs at the earliest date or at the latest date.

[45]According to Audretsch and Feldman (2004), spin-out is one of the most important mechanisms through which knowledge is transmitted locally.

knowledge and experience they previously accrued (Audretsch, 1995; Gompers, Lerner and Scharfstein, 2005; Agrawal et al. 2014). I define a spin-out as follows. Using the unique firm identifier in the NBER patent data, I identify first all the new private firms that appear in the database. Then, I look at all the inventors who work for a new firm in the first year after it appears. If at least one of the inventors formerly worked for a listed firm in the same CZ previously, I consider the new firm to be a spin-out. I end up with 22,627 spin-outs, which represents 20% of the total of new firms I observe in the patent data (a total of 112,929 new firms).

Table 3.7 shows how innovation by listed firms in a given CZ can affect inventor mobility flow from listed firms to private firms in the same area. Column (1) finds that an increase in the stock of patents by listed firms generates a higher number of inventors who move from listed firms to private firms. Column (2) shows that this effect is not simply due to an increase in overall mobility, but that inventors formerly working for listed firms represent a higher fraction of mobile inventors who come to work at a new private firm. Finally, column (3) adopts a static view and checks whether this increased local flows of mobile inventors affect the composition of employment in private innovative firms. I find that inventors who formerly worked for a listed firm represent an increasing fraction of inventors employed by private firms. In terms of magnitude, doubling the stock of patents by listed firms increases the share of inventors employed by private firms who formerly worked for listed firms by 50%. Finally, column (4) shows that spin-out creation in the CZ increases with the local stock of patents by listed firms, which provides direct evidence that local innovation spillovers are produced in part because former employees join spin-outs created in the same area and benefit in this manner from the knowledge produced in their previous firm.

[INSERT TABLE 3.7 ABOUT HERE]

3.4.3 Local Innovation Spillovers and Venture Capital

In this section, I investigate how local innovation spillovers interact with investment by VC funds. If innovation by listed firms active in a CZ fosters innovation by local private firms, we should expect VC funds from outside the CZ to invest more in the local area where those innovation spillovers occur. Conversely, we should expect capital availability to affect the magnitude of local innovation spillovers. Indeed, if an increase in the amount of local stock of patents by listed firms improves private firms' ability to innovate, they may not be able to do so if they are credit constrained. Therefore, better capital availability should allow firms to seize new innovative investment opportunities and therefore increase the magnitude of local innovation spillovers.

Capital Inflows

To study whether VC funds from outside the CZ follow potential innovation spillovers, I investigate the behavior of VC funds using the VentureXpert database. The main advantage of using VentureXpert is that the database records both the geographic localization (zipcode) of the VC fund and the localization of the company in which the fund makes an investment, which allows me to identify precisely when and where investments are made and whether the investments come from a fund located in a different area.

I use two different proxies for the ability of CZs to attract out-of-town VC money: the number of investments made and the total value of all investments made in a given

CZ-year. Each variable is in log and calculated only for non-local VC funds.

Chen et al. (2010) have shown that the VC industry is highly clustered in three metropolitan areas (combined statistical areas or CSAs) in the US, that they call "venture capital centers": San Francisco/San Jose, Boston, and New York. Because of this feature, I estimate the different models on the entire sample and then I exclude the 16 CZs that belong to these three centers.

Column (1) of Table 3.8 shows the result for the number of different investments and reports that the stock of patents filed by listed firms in a given CZ in the previous years increase the likelihood that this CZ attracts investment from VC funds located in other CZs. Column (3) show similar results when I use the total money invested in a CZ-year. Columns (2) and (4) report that the effects are similar when I exclude "VC centers" from the sample.

This result is notable because non-local investments are rare in the VC industry (Chen et al. 2010). Indeed, VC firms must to interact frequently with companies in which they invest, to either monitor or coach the management team (e.g., Lerner, 1995).

[INSERT TABLE 3.8 ABOUT HERE]

We now have clear evidence that VC activity has a *causal* effect in fostering entrepreneurial companies (Kortun and Lerner, 2000; Samila and Sorenson, 2011) and does not simply "chase deals" (Gompers and Lerner, 1998). However, my results show that VC investments are indeed partially driven by demand and that more funds are driven into areas that become more innovative. If capital endogenously follow local innovation spillovers, a related question is whether *exogenous* fluctuations in capital availability amplify spillovers.

3.4.4 Effect Depending on Capital Availability

In this section, I investigate whether venture capital availability influences the magnitude of spillovers, something that has received little attention in the literature thus far.

To do so, I interact the variable *Stock Listed Patents* with the total amount of investments made by VC funds. The hypothesis behind these tests is the following: if innovations by listed firms generate new innovation opportunities for private firms, the effect should be more important when these private firms can more easily finance new innovative investments.

Because the previous section showed that capital flows endogenously react to the stock of patents by listed firms, I must be able to generate exogenous variations in the local availability of capital. I build on the literature showing that public pension funds display a "home-bias" and are more likely to invest the asset under their management in local private equity funds and venture capital funds (Hochberg and Rauh, 2012). As a result, fluctuations in public pension assets in the home-state of VC funds will affect the ability of domestic VC funds to raise capital, which will generate variations in the amount of money they can invest (e.g., Bernstein et al. 2010; Gonzalez Uribe, 2014).

I obtain data for local and state public pensions from the State and Local Government Public-Employee Retirement Systems annual survey conducted by the Census Bureau and available since 1970.[46] I compute the amount of asset holdings of the state pension fund for every year and use it as the instrument for the total amount of VC investments made at the state level.[47]

[46]Data from 1993 forward may be directly downloaded from https://www.census.gov/govs/retire/. Historical data are available upon request.

[47]Ideally, I would like to be able to use *within* state variations in VC capital availability, but I use VC investment at the state level because the instrument is at the state level.

Table 3.9 reports the results for the different proxies. In column (1), I use the volume of investment made by VC funds in log in a given state-year and find that greater levels of VC investment increase the magnitude of local innovation spillovers. The interaction term is positive and statistically significant at the 1% level. However, because VC investments are likely to be endogenous with the stock of patents by listed firms, I instrument VC investments by the amount of local and state public pension funds in column (2). The first stage produces an F-test of 30, which rules out the risk of having a weak instrument. The IV estimate yields similar results and shows that exogenous variations in the amount of VC capital available amplify local innovation spillovers. The magnitude of the amplification is important because, as moving from the 25th percentile to the 75th percentile increases the elasticity by more than 0.4, which is twice the size of the average effect. Column (3) reproduces the analysis when I again exclude those CZs belonging to a "VC center" and shows a similar effect.

[INSERT TABLE 3.9 ABOUT HERE]

Overall, these results demonstrate that capital relocates to areas in which local innovation spillovers occur and that in return, capital availability amplifies the magnitude of these local innovation spillovers, which thus suggests that capital mobility can contribute to increase differences between geographic entities rather than narrow such differences.

3.5 Robustness

3.5.1 Alternative Stories

There are three possible alternative explanations for my results. First, changes in the production of innovation by listed firms may affect competitive pressure on local private firms. Second, listed firms can be consumers of local private firms. Third, the adoption of BC laws affect innovation by private firms via the M&A market.

In this section, I discuss the consistencies of these three explanations with my results and provide new results to rule out those alternative theories.

The first possible concern is that listed firms compete locally with private firms. In this case, as shown by Aghion et al. (2005) model, an increase in local innovation activity by listed firms might force private firms to innovate in response, which seems unlikely to be the case in my setting for several reasons. First, CZs are small geographical areas and we can expect innovative firms to compete industry-wide, at the state level at the very least and most likely at the national and international levels. Nevertheless, I find a limited effect for innovative activities of local listed firms even in neighboring CZs (column (3) of Table 3.3). Second, as noted in section 3.2.2, to capture between-industry spillovers, I aggregate innovation activity at the CZ level, reducing the likelihood that all firms compete in the same product markets. Third, it is unclear why the inverted-U shape theory of Aghion et al. (2005) would imply that innovation by listed firms would have a greater effect when the density of college graduated or engineers is higher or when non-compete covenants are not enforceable. Similarly, the framework of Aghion et al. (2005) has no predictions regarding spin-out formation or inventor mobility. It seems also unlikely that out-of-town VC funds would invest more in the CZ if competition was higher for local private firms, because their expected profits would be lower.

Finally, I perform two additional tests. In column (1) of Table 3.10, I follow Agrawal et al. (2014) and add to my baseline regression the Herfindahl Index of patents across firms in each CZ, as well as the square term of HHI as a proxy for local competition. In column (2), I use the classification of Mian and Sufi (2014) and restrict my estimation to firms in tradable industries. I expect those firms to compete on a broader scale than the CZ and therefore to be less sensitive to local competition. In column (3), I exclude from my sample the biggest listed firms from each CZ (specifically I calculate for each CZ-year the fraction of patents filled by each listed firm and exclude the top 10%) because I expect those firms to generate the largest competitive pressure on their local environment. In all cases, I find similar results.

[INSERT TABLE 3.10 ABOUT HERE]

The second competing explanation is that rather than being competitors, listed firms and private firms are "allies" instead. For instance, it is possible that listed firms generate a demand for technologies that increases innovation by private firms. However, as with the previous alternative explanation, it is unclear why we should find an effect on inventor mobility, why the effect should nearly disappear when we look at neighboring CZs or why the effect should be bigger when the share of college-educated workers is higher or when non-compete covenants are not enforceable.

If this explanation was the main reason for my result, it is also difficult to understand why the effect is the same when I look at innovation in tradable industries (because those industries are more likely not to be dependent on local suppliers). In column (4) of Table 3.10, I also restrict my estimate to listed firms that are active in at least five different CZs.[48] I expect firms active in multiple CZs not to generate similar demands in each CZ. Therefore, if the demand channel was the main explanation, I should find a smaller effect. However, my result is not affected when I focus on those firms.

The third possible explanation is that the adoption of BC laws affect innovation by private firms is via the M&A market. One possibility is that entrepreneurs innovate in order to sell their startup to a large corporation. If the adoption of BC laws reduces listed firms' takeover demand, it might reduce potential targets' incentives to innovate (e.g. Phillips and Zhdanov, 2013). We know from Gormley and Matsa (2014) that the adoption of BC laws increases listed firms takeover activities. However, the acquires are concentrated among listed firms with greater risk of distress and which target "cash cows" and in diversified segments. As such, the effect on the M&A market for innovative firms is unlikely to be affected. In addition, it is unclear why in this case the effect of innovation spillover would be so local or why it would be affected the presence of by non compete laws. In addition in columns (5) and (6), I estimate whether innovation by listed firms in a given CZ affect the likelihood to observe the acquisition of a private firm (column (5)) or a private high-tech firm (column (6)) in the same CZ. I identify the localisation of an acquired private firm using SDC Platinium. Similarly, I consider a private firm as "high-tech" if SDC indicates that the firm operates in an high-tech industry. In both cases, I find no effect.

[48] I find similar results when I look at firms in at least two or ten CZs.

3.5.2 Additional Robustness Checks

In Table 3.11, I explore the robustness of my main result. In column (1), I follow Kerr and Lincoln (2010) and add a specific technological trend at the CZ level to my main specification. Differences in sectoral growth rates or changing propensities to seek patents might affect my findings if for instance, the CZs in which patents by listed firms increase more are simultaneously initially more specialized in a growing sector. I thus include a measure of expected CZ-level patenting based on pre-period technological specialisation and national patenting trends. To predict patenting growth based on initial specialization, I calculate the initial innovation specialization using the 37 different "technological subcategory" (variable *subcat* in the NBER Patent database) and interact this specialization with the aggregate patenting growth of each in each of the 37 categories. I interact the variable with a time trend and add it as a control. In columns (2) to (4) I exclude various CZs / firms. In columns (2) and (3) I exclude various CZs to ensure that my estimate does not reflect the specificities of certain cities (and in particular the most innovative ones). In column (2), I exclude all the CZs that belong to one of the five main high-tech clusters identified by Belenzon and Schankerman (2013): Austin, Boston, Raleigh-Durham, San Diego, and Silicon Valley (namely San Francisco-Oakland-San Jose). In column (3), I directly exclude all the CZs within California and Massachusetts which are the two most innovative states. In both cases, the estimates are similar to the initial result. Finally, column (4) excludes patents by listed firms that are incorporated in Delaware and column (5) exclude patents that are filed in CZs located in the state in which the listed firm has its headquarter. Again, my results remain unaffected.

[INSERT TABLE 3.11 ABOUT HERE]

3.6 Conclusion

Using a novel strategy to generate local shocks on the innovation activities of listed firms, I provide evidence for the existence of complementarities between the innovation of listed firms and private firms. Those complementarities explain why a shock on the innovation production of some firms can transmit to the rest of the local economy, although other firms are not directly hit by the shock.

I then explain these complementarities by local information transmission locally and identify different channels through which this transmission may occur. In particular, the ease with which workers can exchange ideas and learn from one another, the possibility for workers to move from one firm to another and to create their own firms are all channels through which knowledge is transmitted within the local area. Those results also suggest that state policies can play an important role in affecting the magnitude of local innovation spillovers by shaping the ability for local markets to absorb new knowledge and affecting labor mobility.

Finally, I find that local innovation spillovers generated by listed firms induce venture capital funds from outside the area to invest more into areas where local innovation spillovers happen. I also find that variations in the amount of capital available amplifies the magnitude of innovation spillovers. This last result suggests that finance could be an important factor to explain the important disparities between cities in terms of economic specialization, entrepreneurship, growth, etc. If capital follows innovation and in return magnifies economic spillovers, small differences between areas can become rapidly amplified

Assessing exactly and to what extent capital flow is responsible for how agglomerations are formed, sustained and strengthened offers interesting avenues for future researches.

References

Adams, J. and , A. Jaffe (1996). “Bounding the Effects of R&D: An Investigation Using Matched Establishment-Firm Data”. *RAND Journal of Economics*, 27, 700-721.

Adelino, M., S. Ma, and D. Robinson (2014). “Firm Age, Investment Opportunities, and Job Creation”. *NBER Working Paper*.

Almeida, P., and B. Kogut (1999). “Localization of Knowledge and the Mobility of Engineers in Regional Networks”. *Review of Economics and Statistics*, 88(4), 774-779.

Aghion, P., N. Bloom, R. Blundell, R. Griffith, and P. Howitt (2005). “Competition and innovation: An inverted-u relationship”. *Quarterly Journal of Economics*, 120(2), 701-728.

Agrawal, A. K., Cockburn, I. M., Galasso, A., and A. Oettl (2014). “Why Are Some Regions More Innovative than Others? The Role of Small Firms in the Presence of Large Labs”. *Journal of Urban Economics*, 81 (1), 149-65.

Andersson, R., Quigley, J., and Wilhelmsson, M. (2009). “Higher Education, Localization and Innovation: Evidence from Investment in Higher Education”. *Journal of Urban Economics*, 66 (1), 2-15

Arrow, K. (1962). “Economic Welfare and the Allocation of Resources for Invention”. In: Nelson, R. (Ed.), *The Rate and Direction of Inventive Activity*. Princeton Univ. Press, Princeton, NJ.

Atanassov, J. (2013). “Do Hostile Takeovers Stifle Innovation? Evidence from Anti-takeover Legislation and Corporate Patenting”. *Journal of Finance*, 68(3).

Audretsch, D. (1995). *Innovation and Industry Evolution*. MIT Press, Cambridge, MA

Audretsch, D., Feldman, M. (1996). “R&D Spillovers and the Geography of Innovation and Production”. *American Economic Review*, 86, 630-640.

Audretsch, D., and Feldman, M. (2004). “Knowledge spillovers and the geography of innovation”. in J.V. Henderson and J.F. Thisse (eds.), *Handbook of Regional and Urban Economics*, Vol.4, Amsterdam: Elsevier,North-Holland, 2119-2171

Autor, B. D. H., Dorn, D., and Hanson, G. H. (2013). “The China Syndrome: Local Labor Market Effects of Import Competition in the United States”. *American Economic Review*, 103(6), 2121-68.

Autor, D. H., and Dorn, D. (2013). “The Growth of Low-Skill Service Jobs and the Polarization of the US Labor Market”. *American Economic Review*, 103(5), 1553-97.

Barrot J.N. and Sauvagnat J. (2014). "Input Specificity and the Propagation of Idiosyncratic Shocks in Production Networks". *Working Paper.*

Beaudry, P., Doms, M., and Lewis, E. (2010). "Should the Personal Computer Be Considered a Technological Revolution? Evidence from U.S. Metropolitan Areas". *Journal of Political Economy*, 118(5), 988-1036.

Belenzon, S., and Schankerman, M. (2013). "Spreading the word: Geography, policy, and knowledge spillovers". *Review of Economics and Statistics.*

Benmelech, E., Bergman, N., Milanez, A., and Mukharlyamov, V. (2014). "The Agglomeration of Bankruptcy". *Working Paper.*

Benmelech, E., Bergman, N. K., and Seru, A. (2011). "Financing Labor". *NBER Working Paper*

Bernstein,J. and Nadiri M. (1989). "Research and Development and Intra-Industry Spillovers: An Empirical Application of Dynamic Duality". *Review of Economic Studies*, 56, 249-269.

Bernstein, S., Lerner, J., Sorensen, M., and Stromberg, P. (2010). "Private Equity and Industry Performance". *NBER Working Paper* (15632).

Bertrand, M. and S. Mullainathan (2003). "Enjoying the quiet life? Corporate governance and managerial preferences". *Journal of Political Economy*, 111, 1043-1075.

Bloom, N., Schankerman, M., and Van Reenen, J. (2013). "Identifying Technology Spillovers and Product Market Rivalry". *Econometrica*, 81(4), 1347-1393.

Bottazzi, L., and Peri, G. (2003). "Innovation and spillovers in regions: Evidence from European patent data". *European Economic Review*, 47(4), 687-710.

Boustan L. (2010) "Was Postwar Suburbanization 'White Flight'? Evidence from the Black Migration". *Quarterly Journal of Economics.*

Breschi, S., and Lissoni, F. (2009). "Mobility of skilled workers and co-invention networks: an anatomy of localized knowledge flows". *Journal of Economic Geography*, 9(4), 439-468.

Butler H. (1988). "Corporate-Specific Anti-Takeover Statutes and the Market for Corporate Charters". *Wisconsin Law Review*, 365.

Camey W. (1998). "The Production of Corporate Law". *South California Law Review*, 71.

Carlino, G., S. Chatterjee, and R. Hunt. (2007). "Urban Density and the Rate of Invention". *Journal of Urban Economics*, 61,389-419.

Carlino, G., and Kerr, W. (2014). "Agglomeration and Innovation". *NBER Working Paper*, (20367).

Chen, H., Gompers, P., Kovner, A., and Lerner, J. (2010). "Buy Local? The Geography of Successful and Unsuccessful Venture Capital Expansion". *Journal of Urban Economics*, 67(1).

Chetty, R., Hendren, N., Kline, P., and Saez, E. (2014). "Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States". *NBER Working Paper*, 19843.

Ciccone, A. and Hall R. (1996) "Productivity and the Density of Economic Activity". *American Economic Review*, 86, 54-70.

Cohen, L., and Frazzini, A. (2008). "Economic Links and Predictable Returns". *Journal of Finance*, 63(4), 1977-2011.

Cohen, W.M., Levinthal, D.A. (1990). "Absorptive capacity: a new perspective on innovation and learning". *Administrative Sciences Quarterly*, 35, 128-152.

Da Rin M., T. Hellmann, and M. Puri (2013). "A Survey of Venture Capital Research". in George Constantinides, Milton Harris, and RenÃ© Stulz (eds.) *Handbook of the Economics of Finance*, vol 2, Amsterdam, North Holland.

Dessaint O. and Matray A. (2014). "Do Managers Overreact to Salient Risks? Evidence from Hurricane Strikes". *Working Paper.*

Dougal, C., Parsons, C., and Titman, S. (2012). "Urban vibrancy and corporate growth". *Working Paper.*

Dorn, David. 2009. "Essays on Inequality, Spatial Interaction, and the Demand for Skills". *Dissertation University of St. Gallen no.3613.*

Duranton, G. (2007). "Urban evolutions: the fast, the slow, and the still". *American Economic Review*, 97, 197-221.

Duranton, G. and Puga D.. (2004). "Micro-Foundations of Urban Agglomeration Economies". in J.V. Henderson and J.F. Thisse (eds.), *Handbook of Regional and Urban Economics*, Vol. 4, Amsterdam: Elsevier,North-Holland, 2063-2155.

Doms M. and Lewis E. (2006). "Labor Supply and Personal Computer Adoption." *Working Paper.*

Ellison, G., and E. Glaeser. (1999). "The Geographic Concentration of Industry: Does Natural Advantage Explain Agglomeration?" *American Economic Review*, 89(2), 311-16.

Ellison, G., Glaeser, E., and Kerr, W. (2010). "What Causes Industry Agglomeration?

Evidence from Coagglomeration Patterns". *American Economic Review*, 100(3), pp. 1195-1213.

Fallick, B., Fleischman, C. A., and Rebitzer, J. B. (2006). "Job-Hopping in Silicon Valley: Some Evidence concerning the Microfoundations of a High-Technology Cluster". *Review of Economics and Statistics*, 88(3), 472-481.

Feldman, M., Kogler, D. (2010). "Stylized facts in the geography of innovation." In: Hall, B., Rosenberg, N. (Eds.) *Handbook of the Economics of Innovation*, Vol.1. Elsevier, Oxford, 381-410.

Fresard L. and Foucault T. (2014) "Learning from Peers' Stock Prices and Corporate Investment". *Journal of Financial Economics*, 111, 554-577.

Garmaise, M. J. (2009). "Ties that Truly Bind: Noncompetition Agreements, Executive Compensation, and Firm Investment". *Journal of Law, Economics, and Organization*, 27(2), 376-425.

Giroud, X., and Mueller H. (2010). "Does corporate governance matter in competitive industries?". *Journal of Financial Economics*, 95, 312-331.

Giroud, X., and Mueller H. (2011). "Corporate Governance, Product Market Competition, and Equity Prices". *Journal of Finance*, 66(2), 563-600.

Glaeser, E. (1999). "Learning in Cities". *Journal of Urban Economics*, 46,254-277.

Glaeser, E., and Gottlieb, J. (2009). "The Wealth of Cities: Agglomeration Economies and Spatial Equilibrium in the United States". *NBER Working Paper*, 14806.

Glaeser, E., Kallal, H., Scheinkman, J., Shleifer, A. (1992). "Growth of cities". *Journal of Political Economy*, 100, 1126-1152.

Glaeser, E., Kerr, W., and Ponzetto, G. (2010). "Clusters of entrepreneurship". *Journal of Urban Economics*, 67(1), 150-168.

Glaeser, E. and Mare. D. (2001). "Cities and Skills". *Journal of Labor Economics*, 19, 316-342.

Glaeser, E. and Resseger, M. G. (2010). "The Complementarity Between Cities and Skills". *Journal of Regional Science*, 50(1), 221-244.

Gompers, P. A., Lerner, J. (1998). "What Drives Venture Capital Fundraising?". *Brookings Papers on Economic Activity.* Microeconomics, 149-204.

Gonzalez Uribe J. (2014) "Venture Capital and the Diffusion of Knowledge". *Working Paper.*

Gonzalez Uribe J. and Tompsett (2014) “Patents and Bridges”. *Working Paper.*

Gormley, T., and Matsa, D. (2014). “Playing it Safe? Managerial Preferences, Risk, and Agency Conflicts”. *Working Paper.*

Greenstone G., R. Hornbeck and E. Moretti (2010) “Identifying Agglomeration Spillovers: Evidence from Winners and Losers of Large Plant Openings”. *Journal of Political Economy*, 118(3).

Griffith, R., Harrison, R., and Van. Reenen, J. (2006). “How Special Is the Special Relationship? Using the Impact of U.S. Rand D Spillovers on U.K. Firms as a Test of Technology Sourcing”. *American Economic Review*, 96(5), 1859-1875.

Griliches, Z. (1979). “Issues in assessing the contribution of Rand D to productivity growth”. *Bell Journal of Economics*, 10, 92-116.

Griliches, Z. (1992). “The Search for R&D Spillovers”. *Scandinavian Journal of Economics*, 94, 29-47.

Guiso, L., and Schivardi, F. (2011). “What Determines Enrepreneurial Clusters?”. *Journal of the European Economic Association*, 9(1), 61-86.

Guiso, L., Pistaferri, L., and Schivardi, F. (2014). “Learning Entrepreneurship from other Entrepreneurs?”. *Working Paper.*

Hall, B.,Jaffe A., and Trajtenberg M. (2001). “The NBER Patent Citation Data File: Lessons, Insights and Methodological Tools”. *NBER Working Paper*, 8498.

Henderson, J.V. (2003). ”Marshall’s scale economies”. *Journal of Urban Economics*, 53, 1-28.

Henderson, R., Jaffe, A., and Trajtenberg M. (2005) “Patent citations and the geography of knowledge spillovers: a reassessment: comment”. *American Economic Review*, 95, 461-464.

Hochberg Y. and J. Rauh (2012) “Local Overweighting and Underperformance: Evidence from Limited Partner Private Equity Investments”. *Review of Financial Studies*, forthcoming

Hombert J. and A. Matray (2014) “The Real Effects of Lending Relationships: Evidence from Innovative Firms and Inventor Mobility”. *Working Paper.*

Jaffe A. (1986) “Technological Opportunity and Spillovers of R&D: Evidence From Firms’ Patents, Profits and Market Value”. *American Economic Review*, 76, 984-1001.

Jaffe, A.B., Trajtenberg, M., and Henderson, R. (1993). “Geographic Localization of Knowl- edge Spillovers as Evidenced by Patent Citations”. *Quarterly Journal of Eco-*

nomics, 108, 577-598.

Jaffe, A. and Trajtenberg M. (2002). *Patents, Citations and Innovations: A Window on the Knowledge Economy*, Cambridge, MA: MIT Press.

Jacobs, J. (1969). *The Economy of Cities.* Random House, New York.

Jones C. and Williams J. (1998). "Measuring the Social Rate of Return to R&D". *Quarterly Journal of Economics*, 113(4), 1119-1135.

Jones C. and Williams J. (2000). "Too Much of a Good Thing? The Economics of Investment in R&D". *Journal of Economic Growth*, 5 (1), 65-85.

Kahan, M., and Kamar, E. (2002). "The Myth of State Competition in Corporate Law". *Stanford Law Review*, 55(3), 679-749.

Kantor, S., Whalley, A. (2014). "Knowledge Spillovers from Research Universities: Evidence from Endowment Value Shocks". *Review of Economics and Statistics.* Forthcoming

Karpoff, J. M., and Malatesta, P. H., (1989). "The Wealth Effects of Second-Generation State Takeover Legislation". *Journal of Financial Economics*, 25, 291-322.

Keller,W. (2002) "Geographic Localization of International Technology Diffusion". *American Economic Review*, 92, 120-142.

Kerr, W. (2010). "Breakthrough inventions and migrating clusters of innovation". *Journal of Urban Economics*, 67 (1), 46-60.

Kline P. and Moretti E. (2013). "People, Places and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs". *Annual Review of Economics*, forthcoming.

Kortum, S., and Lerner J. (2000). "Assessing the Contribution of Venture Capital to Innovation". *RAND Journal of Economics*, 31, 674-692.

Krugman, P. (1991). *Geography and Trade.* MIT Press, Cambridge, MA.

Lai, R., A. D'Amour, and L. Fleming (2009). "The Careers and Co-Authorship Networks of U.S. Patent-Holders, Since 1975". *Harvard Institute for Quantitative Social Science.*

Leary T. and Roberts M. (2014). "Do Peer Firms Affect Corporate Capital Structure?". *Journal of Finance*, 2014, 69, 139-178.

Lerner, J. (1995). "Venture Capitalists and the Oversight of Private Firms". *Journal of Finance*, 50, 301-18.

Lerner, J. (2009). *Boulevard of Broken Dreams: Why Public Efforts to Boost Entrepreneurship and Venture Capital Have Failed- and What to Do About It.* Princeton:

Princeton University Press,

Lucas, R (1988). "On the Mechanics of Economic Development". *Journal of Monetary Economics*, 22, 3-42.

Lychagin, S., Pinkse, J., Slade, M. E., and Reenen, J. Van. (2010). "Spillovers in Space: Does Geography Matter?". *NBER Working Paper*, (16188).

Manski, C. (1993). "Identification of Endogenous Social Effects: The Reflection Problem". *Review of Economic Studies*, 60(3), 531-542.

Marshall, A.. (1890). *Principles of Economics*, London: Macmillan.

Marx, M., D. Strumsky, and L. Fleming (2009). "Mobility, Skills, and the Michigan Non-Compete Experiment". *Management Science*, 55, 875-889.

Mian, A., and A. Sufi, (2014). "What explains high unemployment? The aggregate demand channel". *Working Paper*.

Moretti, E.. (2004a). "Estimating the Social Return to Higher Education: Evidence from Longitudinal and Repeated Cross-Sectional Data". *Journal of Econometrics*, 121, 175-212.

Moretti, E., (2004b). "Workers' Education, Spillovers and Productivity: evidence from Plant- Level Production Functions". *American Economic Review*, 94, 656-690.

Moretti, E., (2004c). "Human Capital Externalities in Cities". *Handbook of Regional and Urban Economics*, North Holland-Elsevier, 2004.

Moretti, E. (2011). "Local Labor Markets". *Handbook of Labor Economics*, 4(11).

Nervis, A., (1962). *The State Universities and Democracy.* University of Illinois Press, Champaign, IL.

Paravisini, D. (2008). "Local Bank Financial Constraints and Firm Access to External Finance". *Journal of Finance*, 63(5), 2161-2193.

Peri, G. (2005). "Determinants of Knowledge Flows and their Effect on Innovation". *Review of Economics and Statistics*, 87, 308-322.

Parsons, C. A., Sulaeman, J., and Titman, S. (2014). "Peer Effects and Corporate Corruption". *Working Paper.*

Phillips G., and A. Zhdanov, 2013, "R&D and the Incentives from Merger and Acquisition Activity," *Review of Financial Studies*, 26 34-78.

Rauch, J. (1993). "Productivity Gains from Geographic Concentration of Human Capital: Evidence from the Cities". *Journal of Urban Economics*, 34, 380-400.

Romano, R. (1987). "The Political Economy of Takeover Statutes. Virginia Law Review". 73(1), 111.

Romer, P. (1986). "Increasing Returns and Long- Run Growth". *Journal of Political Economy*, 94(5), pp. 1002-37.

Rosenthal, S.S., Strange, W.C. (2003). "Geography, Industrial Organization, and Agglomeration". *Review of Economics and Statistics*, 85 (2), 377-393.

Rosenthal, S. and Strange W. (2004). "Evidence on the Nature and Sources of Agglomeration Economies", in J.V. Henderson and J.F. Thisse (eds.), *Handbook of Regional and Urban Economics*,Vol.4, Amsterdam: Elsevier/North-Holland, 2119-2171.

Ruggles, S., T.Alexander, K. Genadek, R. Goeken, Matthew B. Schroeder, and M. Sobek. (2010) Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]. Minneapolis: University of Minnesota.

Samila, S., and Sorenson, O. (2011). "Venture Capital, Entrepreneurship, and Economic Growth". *Review of Economics and Statistics*, 93(1), 338-349.

Seru, A. (2014). "Does Firm Boundaries Matter?: Evidence from Conglomerates and R&D Activity". *Journal of Financial Economics*, 111(2), 381-405.

Shapiro, J. (2006). "Smart Cities: Quality of Life, Productivity, and the Growth Effects of Human Capital". *Review of Economics and Statistics*, 88(2), 324-335.

Shleifer, A., and Vishny, R. W. (1997). "A Survey of Corporate Governance". *Journal of Finance*, 52(2), 737-783.

Singh, J., Agrawal, A. (2011). "Recruiting for ideas: how firms exploit the prior inventions of new hires". *Management Science*, 57 (1), 129-50.

Sorenson, O., and Samila, S. (2011). "Non-compete Covenants?: Incentives to Innovate or Impediments to Growth". *Management Science*, 57(3), 425-438.

Stock, J. H., and Yogo, M. (2005). "Testing for Weak Instruments in Linear IV Regression". Ch. 5 in J.H. Stock and D.W.K. Andrews (eds), *Identification and Inference for Econometric Models: Essays in Honor of Thomas J. Rothenber*, Cambridge University Press.

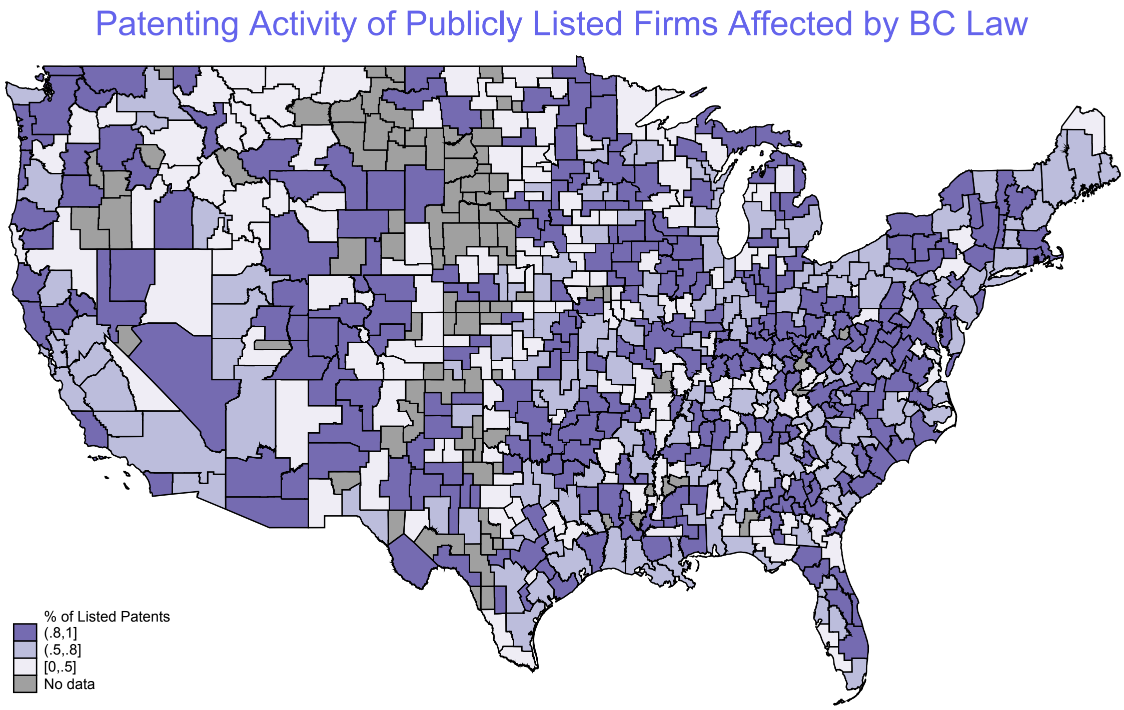
Stuart, T. E., and Sorenson, O. (2003). "Liquidity Events and the Geographic Distribution of Entrepreneurial Activity". *Administrative Science Quarterly*, 48(2), 175-201.

Tolbert, C., and Sizer M. (1996). "U.S. Commuting Zones and Labor Market Areas: A 1990 Update". *Economic Research Service Staff Paper 9614*.

Wilson, D. (2009). "Beggar thy Neighbor? The In-State, Out-of-State and Aggregate

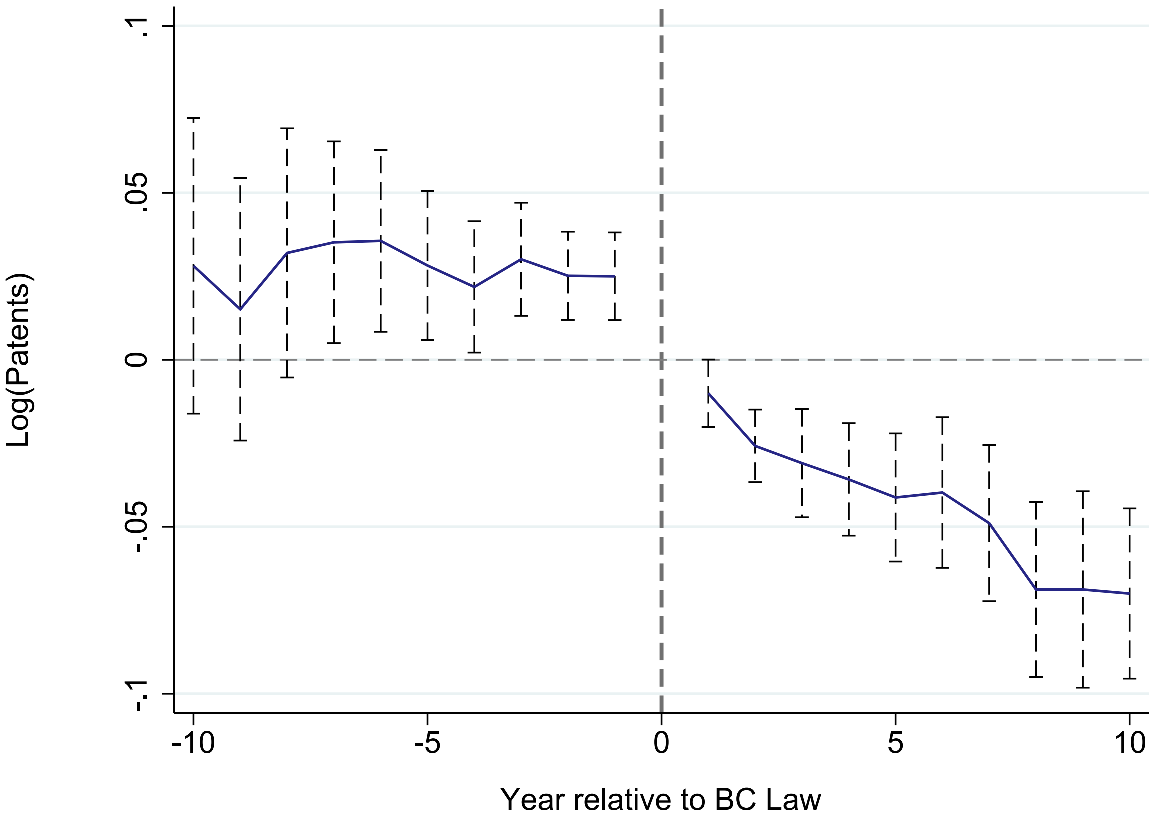
Effects of R&D Tax Credits". *Review of Economics and Statistics*, 91, 431-436.

Figure 3.1: . Fraction of Patents by listed firms Affected by BC Laws



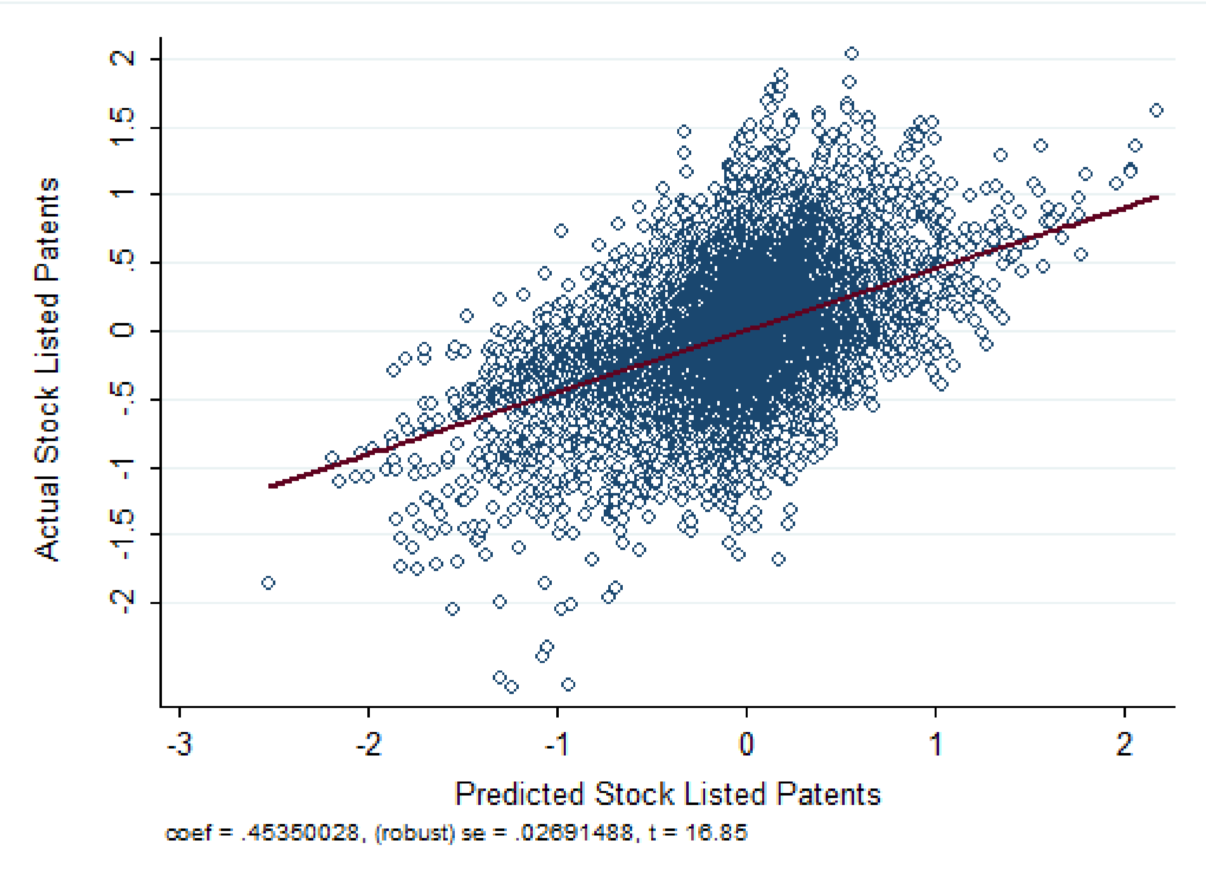
This map shows the geographic dispersion of publicly listed firms that will be affected by the adoption of BC laws. I calculate the fraction of patents filed by listed firms affected over the total of patents filed by listed firms for each Commuting Zone.

Figure 3.2: . Effect of BC Laws on Patenting by Publicly Listed Firms



The figure shows the evolution of innovation around regulation dates. The specification is the same as equation (3.2) except that the dummy for the adoption of business combination law is replaced by a collection of variables $I(k)$, where $I(k)$ is a dummy equal to one exactly k years after (or before if k is negative) the state implements the regulation. The solid line plots the point estimates for $k = -10, \ldots, 10$, using the regulation year $k = 0$ as the reference year. The dashed lines plot the 95% confidence interval.

Figure 3.3: . First-Stage Regression: Relationship Between Actual and Predicted Stock of Patents by Listed Firms: 1975-2000



The figure represents the partial correlation between the actual and the predicted stock of listed patents, after CZ and States × Year fixed effects have been removed. Each point in the scatter diagram represents a CZ-Year's residuals of actual and predicted stock of listed patents after fixed effects have been removed.

Table 3.1: . Summary Statistics

This table provides summary statistics for the main variables used in the paper. Statistics have been computed at the CZ-Year level. Variables are described in section 3.2

	Mean	Std. Dev.	p(25)	p(50)	p(75)
Patents Private Firms	45	175	1	5	19
Patents Listed Firms	64	269	0	3	17
Stock Listed Patents	293	1,212	2.5	12	78
Population Density	.41	.62	.1	.21	.42
Firm Density	.93	1.5	.24	.45	.92
Share Urban	.52	.21	.37	.52	.68
Share Black	.09	.12	.01	.04	.12
Share Women	.51	.011	.51	.51	.52
Share College Educated	.39	.094	.32	.39	.46
Share S&E	.017	.0091	.01	.01	.02
Fraction Citation Listed Firms	.3	.086	.26	.3	.35
Fraction Citation Local Firms	.033	.035	.01	.02	.05
Mobile Inventors from Listed Firms	5	16	0	0	2
Share Inventors Previously in Listed Firms	.066	.11	0	0	.11
Spin outs	3.9	15	0	0	2
# Non Local VC Investments	6.3	49	0	0	0

Table 3.2: . Effect of BC Laws on Patenting by Publicly Listed Firms

Dependent variable is the log of patents filed by Compustat firms in a given year and Commuting Zone (CZ) for the sample of firms present from 1975 to 2000. All regressions include Firm × CZ and Year and fixed effects (FE). Column (2) adds Industry × Year FE. Column(3) adds CZ × Year FE. Column (4) excludes all firms incorporated in Delaware. Column (5) excludes all innovation activity in California. Standard errors are clustered by CZ.

Sample	All	All	All	Exc. Delaware	Exc. California
	(1)	(2)	(3)	(4)	(5)
Post BC	-0.04***	-0.06***	-0.05***	-0.06***	-0.05***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Observations	183168	183168	183168	87630	169525
Lab (Firm x CZ) FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	-	-	-	-
CZ x Year FE	-	-	Yes	Yes	Yes
Industry x Year FE	-	Yes	Yes	Yes	Yes

Table 3.3: . Effect of Innovation by Listed Firms on Innovation by Private Firms

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. All regressions include CZ, Year and State x Year fixed effects. Column(1) is the stock of the actual number of patents filed by listed firms in a given CZ. Column(2) instruments the stock of patents by listed firms using the adoption of BC laws. Column (3) adds the stock of knowledge in *Close CZs* CZs defined as the 4 closest CZs and *Distant CZs* defined as the next 4 closest. Columns (4) and (5) add various controls at the CZ-Year level. Standard errors are in parentheses and clustered at the CZ level.

Estimation	OLS (1)	IV (2)	IV (3)	IV (4)	IV (5)
Stock Listed Patents	0.24*** (0.02)	0.21*** (0.04)	0.20*** (0.04)	0.18*** (0.04)	0.17*** (0.04)
Stock Listed Patents_Close CZs			0.06** (0.03)		
Stock Listed Patents_Distant CZs			-0.01 (0.06)		
Observations	17125	17125	17125	17125	17125
CZ Demographic	-	-	-	Yes	Yes
CZ Economic	-	-	-	-	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Table 3.4: . Innovation Spillovers Depending on Technology Proximity

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. In all regressions, *Stock Listed Patents* is instrumented. Each column interacts *Stock Listed Patents* with a proxy of proximity. Column (1) uses the fraction of citations of patents by listed firms made by private firms. Column (2) uses the degree of overlap in technological classes based on the procedure developed by Jaffe (1986). Column (3) uses the degree of proximity across technological classes based on the Mahalanobis distance defined by Bloom et al. (2013). Column (4) uses proxies in columns (1) and (2). Column (5) uses proxies in columns (1) and (3). All regressions include CZ, Year and State x Year fixed effects. Standard errors are clustered at the CZ level.

	(1)	(2)	(3)	(4)	(5)
Stock Listed Patents	0.17***	0.17***	0.19***	0.16***	0.18***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Stock Patent Public ×	1.92***			1.61***	1.63***
Tech. Prox. (Citation Listed Firms)	(0.37)			(0.37)	(0.34)
Stock Patent Public ×		0.61***		0.50***	
Tech. Prox. (Jaffe Distance)		(0.10)		(0.11)	
Stock Patent Public ×			0.04***		0.03***
Tech. Prox. (Mahalanobis Distance)			(0.01)		(0.01)
Observations	17125	17125	17125	17125	17125
CZ Controls	Yes	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Table 3.5: . Innovation Spillovers Depending on Skilled Worker Supply

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. In all regressions, *Stock Listed Patents* is instrumented. All regressions include CZ, Year and State x Year fixed effects. Columns (1) reports the effect when the stock of patents by listed firms is interacted with the supply of scientists and engineers (S&E) in a given CZ-year. Column (2) uses the supply of college graduates in a given CZ-year. Columns (3) and (4) instrument the supply of college graduate. In column (3), the instrument is the share of 15-19-year-olds enrolled in school in the past year in 1880, constructed from 1880 US Census-10% (Ruggles and al. 2010). The F-test is 13. In column (4), the instrument is a dummy equal to one if the CZ contained a college created via the "Land Grant Movement" in 1862 and 1890 (Nervis, 1962). The first stage F-test is 48. Standard errors are in parentheses and clustered at the CZ level.

	(1)	(2)	(3)	(4)
Stock Listed Patents	0.18***	0.18***	0.20***	0.17***
	(0.04)	(0.04)	(0.04)	(0.04)
Stock Listed Patents × S&E Supply	0.05***			
	(0.01)			
Stock Listed Patents × College Graduate		0.91***		
		(0.16)		
Stock Listed Patents × College Graduate (IV 1)			0.89**	
			(0.38)	
Stock Listed Patents × College Graduate (IV 2)				1.12***
				(0.36)
Observations	17125	17125	17125	17125
CZ Controls	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
F-test(Enrollement 1880)	-	-	13	-
F-test(Land Grant)	-	-	-	48

Table 3.6: . Innovation Spillovers Depending on Non-Compete Laws

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. *Stock Listed Patents* is instrumented in all regressions. All regressions include CZ, Year and State x Year fixed effects. Column (1) reports the effect when the stock of patents filed by listed firms is interacted with a dummy indicating whether the CZ is in a state that enforce non-compete covenants (Stuart and Sorenson, 2003). Column (2) excludes California. Column (3) uses the degree of enforceability of non-compete laws as an interaction term reported in Garmaise (2009). Standard errors are in parentheses and clustered at the CZ level.

Sample	All (1)	Exc. California (2)	All (3)	Exc. California (4)
Stock Listed Patents	0.21*** (0.03)	0.21*** (0.03)	0.32*** (0.07)	0.34*** (0.07)
Stock Listed Patents × Presence of Non-Compete Law	-0.08* (0.04)	-0.09** (0.04)		
Stock Listed Patents × Intensity of Non-Compete Law			-0.04** (0.01)	-0.04*** (0.01)
Observations	17125	16675	17125	16675
CZ Controls	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes

Table 3.7: . Effect on Inventor Mobility from Listed Firms to Private Firms

685 CZs, 1975-2000. This table shows the mobility of inventors to private firms within the same CZ. *Stock Listed Patents* is instrumented in all regressions. Column (1) examines the number of inventors who move from listed firms to private firms. Column (2) reports the fraction of mobile inventors who come from listed firms over the total of mobile inventors to private firms. Column (3) uses the fraction of inventors currently employed by private firms who formerly worked for a listed firm in the same CZ. Column (4) reports the number of spin-outs (defined as new private firms employing, in the first year they file patents, inventors formerly employed by a listed firm in the same CZ). All regressions include CZ, Year and State x Year fixed effects. Standard errors are in parentheses and clustered at the CZ level.

Dependent variable	# Mobile Inventors from Listed Firms	Share Mobile Inventors from Listed Firms	Share Inventors Previously in Listed Firms	# Spin-outs
	(1)	(2)	(3)	(4)
Stock Listed Patents	0.06***	0.05***	0.02***	0.09***
	(0.02)	(0.01)	(0.00)	(0.02)
Observations	17125	17125	17125	17125
CZ Controls	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes

Table 3.8: . Capital Inflow: Investments by Non-Local VC Funds

685 CZs, 1975-2000. *Stock Listed Patents* is instrumented in all regressions. In columns (1) and (2), the dependent variable is the number of VC investments made by non local VCs. Columns (3) examine the total amount invested by non-local VC funds. Column (2), (4) exclude from the sample CZs considered as VC centers (Chen et al. 2010). All dependent variables are in log. All regressions include CZ, Year and State x Year fixed effects. Standard errors are in parentheses and clustered at the CZ level.

Dependent variable	**# Investments**		**Total Value**	
	(1)	(2)	(3)	(4)
Stock Listed Patents	0.045** (0.022)	0.045** (0.021)	0.227** (0.097)	0.218** (0.097)
Observations	17125	16775	17125	16775
CZ Controls	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Sample	All	Exc. VC centers	All	Exc. VC centers

Table 3.9: . Innovation Spillovers Depending on Fund Availability

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. *Stock Listed Patents* is instrumented in all regressions. Column (1) reports the effect when the stock of patents filed by listed firms is interacted with the amount of VC investments made in the state (in log and demean to restore main effects). Column (2) instruments the amount of VC investments using the value of assets held by local and state pension funds. In the first stage, the coefficient on this variable is 0.30 with an F-statistic of 30. Column (3) excludes from the sample CZs considered as VC centers (Chen et al. 2010). Standard errors are in parentheses and clustered at the CZ level.

	(1)	(2)	(3)
Stock Listed Patent	0.16***	0.17***	0.17***
	(0.02)	(0.02)	(0.09)
Stock Listed Patent × VC	0.02***		
	(0.00)		
Stock Listed Patent × VC (IV)		0.05***	0.05***
		(0.01)	(0.01)
Observations	17125	17125	16775
CZ Controls	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
F-test(State Pension)	-	30	30
Sample	All	All	Exc. VC centers

Table 3.10: . Alternative Stories

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. *Stock Listed Patents* is instrumented in all regressions. All regressions include CZ, Year and State x Year fixed effects. In column (1) I add the Herfindahl Index and its square term of firms in a given CZ. Column (2) restricts the sample to tradable firms (using Mian and Sufi (2014) classification). Column (3) excludes listed firms that are in the last decile of patents filed in a given CZ-year. Column (4) is restricted to listed firms active in at least 5 CZs. Columns (5) and (6) use as a dependent variable a dummy equal to one if at least one private firm (column (5)) or one private and innovative firm (column (6)) has been observed in a CZ-year cell. Data on M&A come from SDC Platinium. Standard errors are in parentheses and clustered at the CZ level.

Dependent variable	# Patents Private Firms				Any M&A Private Firms	Any M&A Innovative Private Firms
Sample	**All**	**Tradable**	**Exc. Biggest Firms**	**At least 5 CZs**	**All**	**All**
	(1)	(2)	(3)	(4)	(5)	(6)
Stock Listed Patents	0.18***	0.21***	0.17***	0.17***	0.02	-0.01
	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	17125	17125	17125	17125	17125	17125
CZ Controls	Yes	Yes	Yes	Yes	Yes	Yes
CZ HHI	Yes	-	-	-	-	-
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.11: . Effect of Innovation by Listed Firms on Innovation by Private Firms: Robustness

685 CZs, 1975-2000. Dependent variable is the log of patents filed by private firms. In all regressions, *Stock Listed Patents* is instrumented. All regressions include CZ, Year and State x Year fixed effects. Column (1) includes a measure of expected CZ-level patenting based on its initial specialisation times a time trend. Column (2) excludes CZs belonging to one of the following "Tech Pole": Austin-San Marcos (TX) Boston-Worcester-Lawrence-Lowell-Brockt (MA), Raleigh-Durham-Chapel Hill (NC) or San Francisco-Oakland-San Jose (CA). In column (3) I exclude California and Massachusetts. Column(4) excludes listed firms whose state of incorporation is in Delaware. Column (5) uses only public patents in CZs different from the state of headquarter. Standard errors are in parentheses and clustered at the CZ level.

Sample	All (1)	Exc. TechPole (2)	Exc. TechStates (3)	Exc. Delaware (4)	Exc. State HQ (5)
Stock Listed Patent	0.16*** (0.02)	0.16*** (0.02)	0.18*** (0.02)	0.17*** (0.02)	0.17*** (0.02)
Observations	17125	16875	16550	17125	17125
Techno Trend	Yes	-	-	-	-
Czone Controls	Yes	Yes	Yes	Yes	Yes
Czone FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

3.7 Appendix

3.7.1 Construction of variables

Education Variables:

All Data for the education variables are available from WebCASPAR (https://ncsesdata.nsf.gov/webcaspar/)

Number of College Institutions: Data comes from IPEDS Enrollment Survey (Years available: 1967-2012). I obtain the list of enrolled students by institutions using the "Fall Enrollment (NSF popuplation of institutions)" survey. Institutions are located by zipcodes. I then map the zipcodes with county identifiers and then counties with Commuting Zone using the crosswalk from David Autor Website.

Number of Earned Doctorates: Data comes from NSF Data sources "NSF Survey of Earned Doctorates/Doctorate Records File" (Years available: 1966-2012). I use the "Number of Doctorate Recipients by Doctorate Institution". Institutions are identified by their zipcodes. I map zipcode with counties and counties with CZ.

R&D conducted by Universities: Data comes from NSF Data sources "NSF Survey of Research and Development Expenditures at Universities and Colleges/Higher Education Research and Development Survey" (Years available: 1972-2012).

Commuting Zone Characteristics:

Population and population characteristics come from Census "Population Estimates" (http://www.census.gov/popest/data/historical/)

Urbanisation: Share of population living in an urban area. Data comes from Census. Available from NHGIS https://www.nhgis.org/

Density: Total population scaled by area in square miles (variable $v27$) from Census of Population and Housing, 1990 (ICSPR 21983).
(http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/21983)

Share Black: Share of population who is black. Data comes from Census "Population Estimates". Data are collected at the county level and aggregated at the CZ level.

Share Women: Fraction of women over total population. Data comes from Census "Population Estimates".

Industry Specialisation: Data comes from BEA Local Area Personal Income. Industries are measured using total employment per sector. The list of sectors is the following: Agriculture (linecode 70), Forestry (linecode 100), Mining (linecode 200), Construction (linecode 300), Manufacturing (linecode 400), Transport (linecode 500), Wholetrade (linecode 610), Retail (linecode 620), FIRE (linecode 700), Services (linecode 800), Government (linecode 900).

Share Self-Employed: defined as total self-employed (linecode 260) divided by total population (linecode 100). Data comes from BEA Local Area Personal Income. Table "Personal income, per capita personal income, population".

Technology Age: for each technology class (*nclass*) year, I calculate the median age of innovative firm (defined as the number of years since first appearance in the database). I then take the average for each CZ-year cell.

3.7.2 List of Scientists and Engineers: Census 1990 occupation

Engineers correspond to the following occupations: Aerospace engineers (44) Metallurgical and material engineers (45), Petroleum, mining and geological engineers (47) Chemical engineers (48), Civil engineers (53), Electrical engineers (55), Industrial engineers (56), Mechanical engineers (57), Engineers and other professionals, n.e.c (59).

Scientists correspond to the following occupations: Computer systems analysts and computer scientists (64), Operations and systems researchers and analysts (65), Actuaries (66), Mathematicians and statisticians (68), Physicists and astronomists (69), Chemists (73), Atmospheric and space scientists (74), Geologists (75), Physical scientists, n.e.c. (76), Agricultural and food scientists (77), Biological scientists (78), Foresters and conservation scientists (79), Medical scientists (83).

Table G.1: . Business Combination Laws Adopted by State and Year

This table reports the states that adopted a business combination law along with the year in which the law was adopted. To identify when BC laws were adopted in each state, I use the dates for 30 states that adopted laws between 1985 and 1991, as reported in Bertrand and Mullainathan (2003) and augment their list using Pinnell (2000)-Oregon in 1991, and Iowa and Texas in 1997.

Arizona (1987)	Nevada (1991)
Connecticut (1989)	New Jersey (1986)
Delaware (1988)	New York (1985)
Georgia (1988)	Oklahoma (1991)
Idaho (1988)	Ohio (1990)
Illinois (1989)	Oregon (1991)
Indiana (1986)	Pennsylvania (1989)
Iowa (1997)	Rhode Island (1990)
Kansas (1989)	South Carolina (1988)
Kentucky (1987)	South Dakota (1990)
Maine (1988)	Tennessee (1988)
Maryland (1989)	Texas (1997)
Massachusetts (1989)	Virginia (1988)
Michigan (1989)	Washington (1987)
Minnesota (1987)	Wisconsin (1987)
Missouri (1986)	Wyoming (1989)
Nebraska (1988)	

Table G.2: . Effect of Innovation by Listed Firms on Innovation by Private Firms

685 CZs, 1975-2000. The dependent variable is the log of patents filed by private firms. All regressions include CZ, Year and State x Year fixed effects. Column(1) is the stock of the actual number of patents filed by listed firms in a given CZ. Column(2) instruments the stock of patents by listed firms using the adoption of BC laws. Column (3) adds the stock of knowledge in *Close CZs* CZs defined as the 4 closest CZs and *Distant CZs* defined as the next 4 closest. Columns (4) and (5) add various controls at the CZ-Year level. Standard errors are in parentheses and clustered at the CZ level.

Estimation	OLS (1)	IV (2)	IV (3)	IV (4)	IV (5)
Stock Listed Patents	0.24*** (0.02)	0.21*** (0.04)	0.20*** (0.04)	0.18*** (0.04)	0.17*** (0.04)
Stock Listed Patents_Close CZs			0.06** (0.03)		
Stock Listed Patents_Distant CZs			-0.01 (0.06)		
Population				5.57*** (0.58)	1.95** (0.83)
Urban				0.57*** (0.22)	0.50** (0.22)
Share Black				-1.32 (1.40)	-1.74 (1.26)
Share Female				4.23* (2.38)	3.09 (2.34)
College Institutions					0.08 (0.08)
Doctors					0.07*** (0.02)
R&D Universities					0.02*** (0.01)
Nb Establishments					1.54*** (0.37)
Personal Income					0.67*** (0.16)
Share of Self Employed					-0.01 (0.08)
Industry Specialisation					0.07 (0.14)
Technology Specialisation					0.05 (0.04)
Technology Age					-0.04** (0.01)
VC Investment					-0.00 (0.00)
Observations	17125	17125	17125	17125	17125
CZ Demographic	-	-	-	Yes	Yes
CZ Economic	-	-	-	-	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Chapter 4

Do managers overreact to salient risks?

Evidence from hurricane strikes

Joint work with Olivier Dessaint
(University of Toronto - Rotman School of Management)

It is a common experience that the subjective probability of traffic accidents rises temporarily when one sees a car overturned by the side of the road.

A. Tversky and D. Kahneman (1974)

4.1 Introduction

In this paper, we provide empirical evidence that managers exhibit biases when assessing risk. Specifically, we show that managers systematically respond to near-miss liquidity shocks by *temporarily* increasing the amount of corporate cash holdings. Such a reaction cannot be explained by the standard Bayesian theory of judgment under uncertainty because the liquidity shock stems from a hurricane landfall whose distribution is stationary (Elsner and Bossak, 2001; Pielke et al., 2008). Instead, this reaction is consistent with salience theories of choice (Tversky and Kahneman, 1973, 1974; Bordalo, Gennaioli and Shleifer, 2012a, 2012b, 2013) that predict that the temporary salience of a disaster leads managers to reevaluate their representation of risk and put excessive weight on its probability.[1]

Most corporate policy decisions are made under uncertainty and require managers to estimate risk. Standard corporate finance models assume that managers do so by estimating probabilities through a pure statistical approach. Under this assumption,

[1]We would like to especially thank FranÃ§ois Derrien and David Thesmar for their constant guidance and support. We also thank Luc Behagel, Thierry Foucault, Laurent FrÃ©sard, Nicola Gennaioli, Dong Lou, Daniel Metzger, Fabrice Riva, Andrei Schleifer, Michael Spira, seminar participants at Baruch College, CBS, ESSEC, HEC Paris, HKUST, IESE, Imperial College, INSEAD, Maryland University, Paris School of Economics, University of Miami, University of North Carolina, University of Toronto and the Wharton School of Business, as well as conference participants at the Rothschild Caesarea 10th Annual Conference in Herzliya, the 2013 European Finance Annual Conference in Cambridge, and the 2014 Frontiers of Finance conference for their comments and suggestions. This paper was previously circulated under the title: "Do firm managers properly assess risks? Evidence from US firm proximity to hurricane strikes". All remaining errors are ours.

beliefs about risky outcomes are based on all available information and are formed regardless of context-specific factors. In practice, however, assessing risk is complicated and time-consuming. Because individuals have limited cognitive resources, psychologists argue that they may rely on heuristics, i.e., mental shortcuts that simplify the task of assessing probabilities (Tversky and Kahneman, 1973 and 1974) by focusing on "what first comes to mind" (Gennaioli and Shleifer, 2010). Under this alternative manner of assessing risk, all information is not given equal importance, which may lead people to make mistakes in their estimation that can have important consequences. In this paper, we ask whether managers also use such heuristic rules and investigate whether this practice affects corporate policies.

We focus on the "availability heuristic" rule. Tversky and Kahneman (1973 and 1974) show that people have a tendency to infer the frequency of an event from its availability, namely the ease with which concrete examples of a situation in which this event occurred come to mind. As the quote above suggests, the drawback of such a heuristic rule is that availability may also be affected by the salience of the event. For many reasons (e.g., a dramatic outcome or high levels of media coverage), certain events have unusual characteristics that stand in stark contrast with the rest of the environment. Because such events are more salient, they come to mind more easily. People using the availability heuristic will then overestimate the probability that these events will occur again. As shown by Bordalo, Gennaioli and Shleifer (2012b), such people behave as "local thinkers" who use only partial (i.e., salient) information to estimate probabilities. They overweight possible outcomes whose features draw their attention while neglecting others and thereby make incorrect inferences about the true probability of an event.

If corporate managers also use the availability heuristic, salient risk situations should lead them to overreact and make inappropriate decisions in terms of risk management. Specifically, we hypothesize that managers then overestimate the probability that the risk will materialize again and take excessive precautionary measures against it. Testing this hypothesis empirically gives rise to two major difficulties. First, the risk perceived by the manager cannot be directly observed. To address this problem, we focus on how managers estimate the risk of liquidity shock at the firm level and use the variations in corporate cash holdings to measure how their perception of this risk changes. Given the overwhelming evidence that corporate cash holdings are primarily used as a buffer against the risk of liquidity shortage, variations in cash holdings should provide a good indication of the changes in liquidity risk that are perceived by firm decision makers.[2]

Second, testing this hypothesis also requires the identification of a salient event whose occurrence does not convey any new information about the real distribution of its probability. For instance, the bankruptcy of Lehman Brothers in 2008 was a salient event that might have led bankers to reevaluate their *subjective* estimation of their risk exposure. However, this event is also likely to have affected the *objective* distribution of their risks.[3] It is therefore impossible to disentangle the part of their reactions caused by the increase in *subjective* risks from that caused by the increase in *objective* risks.

We address this problem here by using hurricanes as the source of liquidity shocks. Hurricanes are risks that are well suited for our purpose for three reasons. First, hurricane

[2] Froot et al. (1993) and Holstrom and Tirole (1998, 2000) provide a theoretical basis for predicting that cash will be used in imperfect financial markets as an insurance mechanism against the risk of liquidity shock. Empirically, several papers document a positive correlation among various possible sources of cash shortfall in the future and the current amount of cash holdings; these studies thus confirm that precautionary motives are central to accumulating cash reserves (e.g., Kim et al., 1998; Harford, 1999; Opler et al., 1999; Almeida et al., 2004; Bates et al., 2009, Acharya et al., 2012).

[3] See Shleifer and Vishny (2011) for an analysis of how Lehman Brothers bankruptcy affected banks' balance sheets and increased the risk of fires sales.

frequency is stationary (Elsner and Bossak, 2001; Pielke et al., 2008); thus, the occurrence of hurricane does not convey any information about the probability of a similar event occurring again in the future. Second, their occurrence is a salient event that is exogenous to firm or manager characteristics and represents a credible source of liquidity shock. Finally, hurricane events permit a difference-in-differences identification strategy because their salience is likely to decline as the distance from the disaster zone increases. This feature allows us to estimate the causal effect of risk saliency on the perceived risk by comparing how a treatment group of firms located in the neighborhood of the disaster zone and a control group of distant firms adjust their cash holdings after a disaster.

We find that managers respond to the sudden salience of liquidity risk caused by the proximity of a hurricane by increasing the amount of their firm cash holdings, although there is nothing to indicate that this risk is now bigger than it was. On average, during the 12-month period following the hurricane, firms located in the neighborhood area increase their cash holdings by 0.84 percentage points of total assets relative to firms farther away. We also find that this cash increase is temporary. The amount of cash increases sharply during the first three quarters following the disaster and then progressively returns to pre-hurricane levels over the next four quarters. Thus, as time passes, salience decreases, people forget the event, and the bias vanishes. This bias increases when managers are likely to be less sophisticated (i.e., managers of firms without previous experience of hurricane strikes in their neighborhood area, managers of small firms, and managers of young firms) and decreases when they have good reasons to care less about liquidity risks because their firms are not financially constrained.

We also find that this bias is costly for shareholders. First, we find that managers institute higher earnings retention to increase cash holdings. Second, using the methodology of Faulkender and Wang (2006), we find that the market value of cash decreases when firms are subject to this bias. The additional cash accrued in the balance sheet does not lead to a positive change in market capitalization, which suggests that it would most likely have been better employed otherwise.

We then discuss alternative non-behavioral explanations to our findings, such as the possibility of changes in risk, risk learning, and regional spillover. First, cash holdings could increase if the real probability of being hit by a hurricane increases or if managers ignore the risk and learn of its existence only when the hurricane occurs. However, both of these explanations would imply a permanent increase in cash holdings, which we do not find. Second, cash might increase temporarily because of regional externalities. For instance, the hurricane may temporarily create new business opportunities for firms in the neighborhood area. These firms would then make more profits and hold more cash. However, this type of spillover effect would imply a positive change in operating performance (sales, income), which we do not find. The hurricane might also locally increase business uncertainty for firms in the neighborhood area. These firms may then postpone investment and accumulate cash. However, this additional uncertainty should generate greater variance in revenues or increased volatility in stock returns, which we also do not find. To further alleviate the concern that these effects (or any other form of regional spillover effect) are driving our results, we perform two additional tests. First, we focus on all vulnerable firms (and not necessarily firms in the neighborhood of the affected region). Those firms may be far away from the disaster zone (e.g. firms located in the East coast when a hurricane hits Louisiana). Second, we focus on US firms exposed to earthquake risk and examine how they react to violent earthquakes that occur outside the US. In both situations, the distance to the disaster zone makes the possibility of regional spillover irrelevant. Nevertheless, our primary finding still holds. In both cases, cash holdings increase after the disaster.

Finally, we verify that holding more cash protects firm revenues better in the case of a hurricane. Indeed, if managers respond to the salience of hurricane risk by increasing cash holdings, then we would expect that holding cash helps to reduce firm losses when this risk materializes. We test this prediction and examine how firms located in disaster areas perform in terms of revenue after the disaster depending on the level of their cash holdings before the hurricane. We find that firms that hold more cash perform better and recover much faster than other firms. This finding explains why managers are willing to increase cash holdings when they perceive that the risk of a hurricane strike is higher.

Our paper shows that managers are prone to use the availability heuristic to assess risk, which affects firm value by reducing the value of cash. As such, this study contributes first to the literature on behavioral corporate finance. Baker and Wurgler (2012) organize this literature around two sets of contributions: "irrational investors" and "irrational managers". Our paper is related to the "irrational managers" strand of the literature, which primarily focuses on how overconfidence and optimism can affect both investment and financing decisions (Malmendier and Tate, 2005; Hirshleifer, Low and Teoh, 2012; Landier and Thesmar, 2009). More recently, this literature has begun to study the effects of bounded rationality (Brav et al., 2005), such as reference point thinking (Baker, Pan and Wurgler, 2012; Baker and Xuan, 2011; Loughram and Ritter, 2002; Ljungqvist and Wilhelm, 2005; Dougal et al., 2011).

Next, our results are related to the growing literature that focuses on the effects of individual traits and past experiences on investors' decisions (Malmendier and Nagel, 2011; Malmendier and Nagel, 2013; Kaustia and KnÃ$\frac{1}{4}$pfer, 2008; Choi et al., 2009; Greenwood and Nagel, 2009). Because saliency is experienced-based, our paper complements this literature and shows that irrelevant contextual factors also influence firm decision makers.[4]

Finally and more generally, our paper contributes to the vast literature on the effects of behavioral biases "in the field". *A priori*, managers may act rationally because they are neither unsophisticated agents nor students in a lab with no real economic environment. Therefore, as noted by Levitt and List (2007), we should expect managers not to be affected by behavioral biases. Whether they rely on the availability heuristic to make financial decisions is thus an open question and to the best of our knowledge, this paper is the first to empirically show that managers use the availability heuristic to assess risk and the first to study its effects.

The rest of the paper is organized as follows. Section 2 briefly summarizes what is known about hurricane risk. Section 3 proposes hypotheses based on the availability heuristic phenomenon and reviews the related scientific and anecdotal evidence. Section 4 presents our empirical design. Section 5 provides evidence about whether managers overreact to salient risks. Section 6 investigates whether this reaction is costly. Section 7 discusses the possibility of alternative non-behavioral explanations. Section 8 examines the effects of cash holdings on post-hurricane performance. Section 9 concludes.

[4] Another strand of research examines how salience affects investors' attention. This literature shows that investors pay more attention to salient information (Hirshleifer, Hou, Teoh and Zhang, 2004; Barber and Odean, 2008; Hirshleifer, Lim, and Teoh, 2009 and 2011), which affects stock prices (Ho and Michaely, 1988; Klibanoff, Lamont, and Wizman, 1998; Huberman and Regev, 2001) and creates incentives for strategic information disclosure (Hirshleifer and Teoh, 2003).

4.2 Hurricane activity on the US mainland

Hurricanes are tropical cyclones that form in the waters of the Atlantic and eastern Pacific oceans with winds that exceed 32 m per second (approximately 72 miles per hour). In this section, we briefly summarize what is known about the risk of hurricanes in the US and why it is justified to use such a risk for our experiment. We highlight that hurricane risk can randomly affect an extensive number of firms throughout the US territory, is impossible to predict accurately, has not changed over time and should remain unchanged in the coming decades in terms of both volume (frequency) and value (normalized economic cost).

4.2.1 Event location

Hurricanes can randomly affect a large fraction of the US territory. Coastal regions from Texas to Maine are the main areas at risk. An extensive inland area can also be affected, either by floods resulting from the heavy rainfalls accompanying hurricanes or by the high winds produced by the hurricane as it moves across land. In the SHELDUS database (the main database for natural disasters in the US), 1,341 distinct counties (approximately 44% of the total counties in the US) are reported to have been affected at least once by a major hurricane. Figures 1 through 4 show on a map examples of disaster areas for hurricanes Fran, Floyd, Allison, and Katrina.

INSERT FIGURES 1 TO 4 AROUND HERE

4.2.2 Event frequency

Hurricanes are regular events in the US. Since 1850, an average of 2 hurricanes strike the US mainland every year.

INSERT FIGURES 5 AROUND HERE

Figure 5 suggests no particular increasing or decreasing trend in this frequency. This absence of a trend is supported by the climatology literature (e.g. Elsner and Bossak, 2001; Pielke, 2005; Landsea, 2005; Emanuel, 2005; Landsea, 2007, Pielke et al, 2008; Blake et al., 2011). In the US, Elsner and Bossak (2001) find that the distribution of hurricane strikes have been stationary since early industrial times for all hurricanes and major hurricanes as well as for regional activity.[5] Regarding possible future changes in storm frequencies, Pielke et al. (2008) conclude in their survey that given "*the state of current understanding (...) we should expect hurricane frequencies (...) to have a great deal of year-to-year and decade-to-decade variation as has been observed over the past decades and longer.*"[6]

4.2.3 Event cost

The total cost of hurricane strikes in terms of economic damages is now much larger than it was at the beginning of the past century (Blake, Landsea and Gibney, 2011). However, after normalizing hurricane-related damage for inflation, coastal population and wealth,

[5] "*The distributions of hurricanes during each [time] subinterval are indistinguishable, indicating a stationary record of hurricanes since early industrial times. Stationarity is found for all hurricanes and major hurricanes as well as for regional activity*" (p. 4349)

[6] In section 7, we discuss how possible change in the frequency of hurricane strikes in the US could affect the interpretation of our results. Further analyses on the likelihood of hurricane disaster at the county level are also documented in section 7. In particular, we show that the proximity of a hurricane disaster reveals no information about future hurricane likelihood in a given county.

no trend of increasing damage appears in the data. For instance, Pielke et al. (2008) find that had the great 1926 Miami hurricane occurred in 2005, it would have been almost twice as costly as Hurricane Katrina; thus, they stress that "*Hurricane Katrina is not outside the range of normalized estimates for past storms.*" Overall, their results indicate that the normalized economic cost of hurricane events has not changed over time, consistent with the absence of trends in hurricane frequency and intensity observed over the last century.

4.2.4 Event anticipation

Global tropical storm activity partly depends on climatic conditions that are predictable on seasonal time scales. However, the exact time, location and intensity of future hurricane strikes are *"largely determined by weather patterns in place as the hurricane approaches, which are only predictable when the storm is within several days of making landfall*".[7] Therefore, hurricane disasters in the US mainland are uncertain events that are very difficult to anticipate. Such events *"can occur whether the season is active or relatively quiet*", and in many instances come as a surprise to the local population.[8]

4.3 The psychological mechanisms for probability evaluation and risk assessment

4.3.1 The availability heuristic

Because assessing the likelihood of uncertain events is a complex and time-consuming task, people naturally tend to use their own experiences for developing simple mental rules

[7] See National Oceanic and Atmospheric Administration (NOAA) website

[8] See NOAA website.

to rapidly adjust their beliefs and adapt to their environment. Tversky and Kahneman (1973, 1974) describe such heuristic rules and show that, although useful in general, they sometimes lead people to make mistakes. One such rule is the "availability heuristic", which derives from the common experience that "frequent events are much easier to recall or imagine than infrequent ones." Therefore, when judging the probability of an event, most people assess how easy it is to imagine an example of a situation in which this event actually occurred. For example, people may assess the probability of a traffic accident by recalling examples of such occurrences among their acquaintances.

Tversky and Kahneman (1973, 1974) show that the use of this rule is problematic because availability may also be affected by factors that are not related to actual frequency. In particular, they argue that factors such as familiarity with the event, the salience of the event, the time proximity of the event and/or the preoccupation for the event's outcome can affect its availability and generate a discrepancy between subjective probability and actual likelihood. The availability of a car accident, for instance, will be higher when the person involved in the accident is famous (familiarity), if the accident was observed in real time (salience), if the accident occurred recently (time proximity), or if the physical pain caused by the injuries resulting from traffic accidents has been recently "vividly portrayed" (preoccupation with the outcome). In all these cases described above, the subjective probability of a car accident will then be temporarily higher than its actual likelihood.

4.3.2 The availability heuristic

The availability heuristic theory is consistent with anecdotal and scientific evidence. In a series of studies by Lichtenstein et al. (1978), people were asked to estimate the frequency of several dozen causes of death in the United States. The results from this study show that salient causes that killed many people during a single occurrence were overestimated, whereas less salient causes were systematically underestimated. In a survey conducted to understand how people insure themselves against natural hazards, Kunreuther et al. (1978) observe a strong increase in the number of people willing to buy insurance at a premium immediately after an earthquake. Conversely, people were found to be reluctant to buy such insurance even at a subsidized rate in the absence of a recent major earthquake. Johnson et al. (1993) also find that people are willing to pay more than two times the amount for the same insurance product in situations in which the risk is salient compared to situations in which it is not, confirming that saliency increases perceived risk.[9]

To account for such empirical findings, Bordalo, Gennaioli, and Shleifer (2012b, 2013b) develop a theoretical framework of choice under risk in which salient attributes grab individuals' attention. In their model, individuals do not equally consider the full set of possible states of the world when it comes to assessing risk. They neglect non-salient states, and over-emphasize the salient ones. Because the salience of a state depends on contextual factors, individuals then make context-dependent risk estimations. When a good state is salient, they over-estimate the likelihood of a positive outcome and take too

[9]Other similar results can be found in the housing literature, in which changes in housing prices can be used to infer changes in perceived risk. This literature shows that the occurrence of a salient event (e.g., floods, earthquakes, nuclear accidents, etc.) systematically results in a decrease in property prices that is larger than the value of the insurance premium (see, for instance, MacDonald et al., 1990; Bin et al., 2004, 2008; Kousky, 2010)

much risk. When a bad state is salient, they over-estimate the probability of a negative outcome and are excessively risk averse. In both cases, individuals overreact to salient risks.[10]

4.3.3 Implications and hypothesis development

In this paper, we focus on decision makers in firms. We ask whether they rely on the availability heuristic to assess risk and examine whether they overreact to salient risks (hereinafter, the *availability heuristic* hypothesis). Firm decision makers are neither uninformed, unsophisticated agents (such as home owners or property insurance retail buyers), nor are they undergraduate students in an experiment conducted outside of a real economic environment.[11] Whether managers will make incorrect financial decisions in the real world because of the availability heuristic therefore largely remains an open question.

One challenge is that we cannot directly observe the risk perceived by firm managers. To address this difficulty, we assume that changes in risk perception can be inferred from variations in corporate cash holdings. There is indeed strong theoretical and empirical evidence in the corporate finance literature that the main driver of policies regarding cash holdings is risk management. Froot et al. (1993) and Holstrom and Tirole (1998, 2000) provide a theoretical basis for predicting that cash will be used as an insurance mechanism against the risk of a liquidity shock in imperfect financial markets because firms have limited access to external financing. In this context, cash holdings offer a

[10]Other models based on the mechanism of salience include Bordalo, Gennaioli and Shleifer (2012a, 2013a), Gabaix (2011), Gennaioli and Shleifer (2010), KÃ¶szegi and Szeidl (2013), and Schwartzstein (2009). These models share the common assumption that individuals do not consider the whole set of available information before making a decision and neglect part of it. Significant judgment errors then occur when the neglected data are relevant for decision making.

[11]Levitt and List (2007) discuss the limitations of lab experiments and explain why economic agents may evolve toward more rational behaviors when placed in a familiar environment.

buffer against any risk of cash shortage that would prevent firms from financing positive Net Present Value (NPV) projects. Consistent with this argument, several empirical papers document a positive correlation among various possible sources of cash shortfalls for future and current levels of cash holdings (Kim et al., 1998; Harford, 1999; Opler et al., 1999; Almeida et al., 2004; Bates et al., 2009; Ramirez and Altay, 2011; Acharya et al., 2012). Surveys of CFOs also confirm this link. For instance, Lins et al. (2010) find that a sizeable majority of CFOs indicate that they use cash holdings for general insurance purposes.

If managers rely on the availability heuristic to assess the risk of an event that would trigger a cash shortage, cash holdings should then vary in response to the salience of this event. Under the *availability heuristic* hypothesis, we thus argue that corporate cash holdings will increase in those situations in which the risk of cash shortage becomes more salient.

Because firms are not identical to one another, the effect of event saliency on corporate cash holdings may vary in the cross section of the population. A primary source of heterogeneity is the level of managerial sophistication; sophisticated agents are expected to be less affected by behavioral biases. Therefore, changes in cash holdings for firms with sophisticated managers should be less sensitive to event saliency. Another source of heterogeneity is the level of financial constraints. Managers of less financially constrained firms should be less concerned about potential liquidity shocks. Therefore, changes in cash holdings for unconstrained firms should be less sensitive to event saliency. Another source of heterogeneity consists of firms' vulnerability to hurricane disasters. Indeed, not all industries are similarly affected by hurricane events. Certain industries may suffer higher losses, perhaps because they are more difficult to insure or because they are more dependent on the local economy. Changes in cash holdings should be more sensitive to event saliency for firms that operate in such vulnerable industries.

4.4 Empirical design

4.4.1 Identification strategy

In this paper, we use both the occurrence of hurricanes and the proximity of the firm to the disaster area to identify situations in which the risk of liquidity shocks becomes salient. Our motivation for the use of hurricanes relies on the following arguments. First, hurricanes can trigger liquidity shocks because of the heavy damage they can inflict.[12] Although firms might buy insurance to cover this risk, direct insurance is unlikely to cover all type of indirect losses. In addition, Froot (2001) shows that hurricane insurance is overpriced.[13] Thus, firms should prefer to self-insure by accumulating cash reserves instead of directly insuring this liquidity risk. Second, the occurrence of hurricanes is a salient event because hurricanes draw people's attention and leave their marks on observers' minds. Third, this saliency effect is likely to vary with the proximity of the landfall. Indeed, we expect the event to be salient for managers whose family members and friends are directly affected by the disaster, which is likely to occur for firms located

[12] Cash shortages can come in many ways, including reinvestment needs caused by the partial destruction of operating assets (headquarters, plants, equipment, etc.), a drop in earnings because of a drop in local demand, or new investment financing needs caused by unexpected growth opportunities (reconstruction opportunities, acquisition of a local competitor, etc.).

[13] Froot (2001) shows that hurricane insurance is in short supply because of the market power enjoyed by the small number of catastrophe reinsurers. As a result, insurance premiums are much higher than the value of expected losses. Garmaise and Moskowitz (2009) provide evidence that such inefficiencies in the hurricane insurance market lead to partial coverage of this risk at the firm level, which hurts bank financing and firm investment.

in the disaster area and the environs nearby (referred to herein as the neighborhood) but not for more distant firms. The hurricane event should also receive more attention in situations in which firms are at risk, which again is more likely to occur when firms are located in the neighborhood of the disaster area. Fourth, the occurrence of a hurricane makes hurricane risk salient but does not imply a change in the risk itself. The distribution of hurricanes is stationary; therefore, there is no reason to believe that the real risk of hurricane landfall changes after its occurrence. Finally, hurricanes are exogenous events that can randomly affect a large number of firms. A firm's distance from hurricane landfalls thus offers an ideal natural experiment framework to test for the presence of a causal link between event saliency and managers' risk perception through changes in corporate cash holdings.

4.4.2 Data

We obtain the names, dates and locations of the main hurricane landfalls in the US from the SHELDUS (Spatial Hazard and Loss Database for the United States) database at the University of South Carolina. This database provides the location for each disaster at the county level for all major hurricanes since the early 1960s. In SHELDUS, a county is reported as an affected county whenever the hurricane event and the subsequent rainfalls cause monetary or human losses. To ensure that the event is sufficiently salient, we focus on hurricanes with total direct damages (adjusted for CPI) above five billion dollars. We also restrict the list to hurricanes that occurred after 1985 because there are no financial data available from Compustat Quarterly before that date. This selection procedure leaves us with 15 hurricanes between 1989 and 2008.[14]

We obtain detailed information about their characteristics (start date, end date, date of landfall, direct number of deaths, total damage, and category) from the tropical storm reports available in the archive section of the National Hurricane Center website and from the 2011 National Oceanic and Atmospheric Administration (NOAA) Technical Memorandum. Table 1 presents summary statistics for these 15 hurricanes.

[INSERT TABLE 1 AROUND HERE]

We obtain financial data and information about firm headquarters location from Compustat's North America Fundamentals Quarterly database.[15] We use headquarters rather than plants or clients' location to identify the location of the firm because our objective is to study managers' risk perception, which requires knowing where the decision makers are. Quarterly data rather than annual data are used to identify changes in cash holdings in firms near hurricane landfalls with the highest possible precision.[16]

We restrict our sample to non-financial and non-utility firms whose headquarters are located in the US over the 1987-2011 period. If the county location of a firm's headquarters is missing or if the fiscal year-end month is not a calendar quarter-end month (i.e., March, June, September or December), the firm is removed from the sample. This selection procedure leaves us with a firm-quarter panel dataset of 11,948 firms and 411,490 observations. In Panel A of Table 2, we present summary statistics for the main firm-level variables we use. All variables are winsorized at the first and 99th percentile

[14] We obtain the same results when using all hurricanes from the SHELDUS database. Our results also remain unchanged when we remove the largest hurricanes (e.g. Katrina).

[15] One possible concern with location data is that Compustat only reports the current county of firms' headquarters. However, Pirinsky and Wang (2006) show that in the period 1992-1997, less than 3% of firms in Compustat changed their headquarter locations.

[16] We obtain the same results with annual financial data (See Internet appendix).

and are defined in Appendix 1.

[INSERT TABLE 2 AROUND HERE]

4.4.3 Assignment to treatment and control groups

We measure the degree of salience of each hurricane event according to the distance between the firm's headquarters and the landfall area. For this purpose, we define three different geographic perimeters that correspond to various distances from the landfall area: the *disaster zone*, the *neighborhood* area, and the *rest of the US mainland*. The *disaster zone* includes all counties affected by the hurricane according to the SHELDUS database. The *neighborhood* area is obtained through a matching procedure between affected counties and non-affected counties according to geographical distance. Under this procedure, we first assign a latitude and longitude to each county using the average latitude and average longitude of all the cities located in the county. For each affected county, we next compute the distance in miles to every non-affected county using the Haversine formula.[17] We then match with replacement each affected county with its five nearest neighbors among the non-affected counties.[18] This procedure leaves us with a set of matched counties that constitute our neighborhood area and a set of non-matched counties that form the rest of the US mainland area. Figures 1 to 4 present the results of this identification procedure on a map for hurricanes Fran, Floyd, Allison and Katrina.

[INSERT FIGURES 1 TO 4 AROUND HERE]

[17]The Harversine formula gives the distance between two points on a sphere from their longitudes and latitudes.

[18]We find that on average, a county has approximately five adjacent counties. Our results remain the same when we use three or four rather than five nearest non-affected counties.

Firms located in the *neighborhood* area (represented by the light blue zone on the map) are assigned to the treatment group because the hurricane landfall should be a salient event for the managers of such firms. Given their proximity to the disaster zone, the hurricane is indeed a near-miss event, meaning that they could have been affected by the hurricane but were not by chance. For that reason, we expect the event to raise firm managers' attention. Firms located in the *rest of the US mainland* (the blank zone on the map) are assigned to the control group. Given their distance from the landfall area, the hurricane should not be a salient event for the managers of these firms. Some of these managers may even completely ignore the event if they are located in an area in which the risk of a hurricane strike is not of concern. Firms located in the *disaster zone* (the dark blue zone on the map) are separated in our analysis because of the direct effects of the hurricane on their cash levels. Given their location, these firms are affected by the disaster. The event is not only obviously salient for their managers but is also a potential source of direct cash outflow (e.g., replacement costs of destroyed operating assets) or cash inflow (e.g., receipt of the proceeds of insurance claims). The variation of cash holdings surrounding the hurricane event is thus more likely to reflect the direct effects of the disaster rather than the change in managerial perceived risk. In practice, we do not remove these firms from our sample.[19] Instead, we control to ensure that the variation of cash holdings that we observe when these firms are affected by the hurricane does not influence our results. Panel B of Table 2 presents summary statistics for each group of firms.

[19]In fact, we cannot exclude these firms because these firms can also be in the neighborhood of another hurricane at another point in time. Because we are considering various hurricane strikes over time, it is possible that the same firm may be in each of the three groups defined in our experiment (*disaster zone*, *neighborhood*, and *the rest of the US mainland*).

[INSERT TABLE 2 AROUND HERE]

The statistics are mean values computed one quarter before a hurricane's occurrence. The last column shows the t-statistic from a two-sample test for equality of means across treated and control firms. Treatment firms and control firms appear to be similar along various dimensions, including the amount of cash holdings.

4.4.4 Methodology

We examine the effect of the hurricane saliency on managers' risk perception through changes in the levels of corporate cash holdings using a difference-in-differences estimation. The basic regression we estimate is

$$Cash_{ict} = \alpha_i + \delta_t + \gamma\ X_{itc} + \beta\ Neighbor_{tc} + \epsilon_{itc} \tag{4.1}$$

where i indexes firm, t indexes time, c indexes county location, $Cash_{ict}$ is the amount of cash as a percentage of total assets at the end of the quarter, α_i are firm fixed effects, δ_t are time fixed effects, X_{itc} are control variables, $Neighbor_{tc}$ is a dummy variable that equals one if the county location of the firm is in the neighborhood of an area hit by a hurricane over the last 12 months and zero if not, and ϵ_{itc} is the error term that we cluster at the county level to account for potential serial correlations (Bertrand, Duflo and Mullainathan, 2004).[20]

Firm fixed effects control for time invariant differences among firms (which include fixed differences between treatment and control firms). Time (year-quarter) fixed effects

[20]Allowing for correlated error terms at the state level or firm level leads to similar inferences in the statistical significance of regression coefficients.

control for differences between time periods, such as aggregate shocks and common trends.

The other variables, X_{itc}, systematically include a dummy variable $DisasterZone_{ct}$ to capture the effect of the hurricane strike when the firm is located in the disaster zone. This $DisasterZone_{ct}$ variable enables the comparison of firms in the neighborhood area with firms farther away (the rest of the US mainland) by isolating the changes in cash holdings observed when firms are located in the disaster zone from the rest of our estimation.[21] Our estimate of the effect of hurricane landfall proximity is β, which is our main coefficient of interest. It measures the change in the level of cash holdings after a hurricane event for firms in the neighborhood of the disaster area relative to a control group of more distant firms.

4.5 Do managers overreact to salient risks?

4.5.1 Main results

We examine the effect of the event availability on the risk perceived by firm managers through differences in corporate cash holdings after a hurricane landfall. Tables 3 and 4 present our main results.

[INSERT TABLE 3 AROUND HERE]

Table 3 reports the effects of being in the neighborhood of a disaster area in the 12 months after a hurricane. Column 1 shows that, on average, firms located in the neighborhood of a disaster zone increase their cash holdings (as % of total assets) by

[21]When firms are located in the disaster area, changes in cash holdings are likely to be caused by the direct effects of the hurricane.

0.84 percentage points during the four quarters following the hurricane event. This effect represents an average increase in cash holdings of 16 million dollars in absolute terms and accounts for 8% of the within-firm standard deviation of cash holdings.

We investigate the robustness of this effect in the rest of Table 3. First, our results may capture within-year seasonality. Because hurricane activity is seasonal, firms in the neighborhood area might anticipate the possibility of hurricane strikes and hold more cash at the end of the third quarter of the year. We control for this possibility by using firm-calendar quarter fixed effects (i.e. four quarter fixed effects for each firm) rather than firm fixed effects. Second, our result might be driven by industry-specific shocks. Thus, we use year-quarter-SIC3 fixed effects rather than year-quarter fixed effects to remove any time varying unobserved heterogeneity across industries (Gormley and Matsa, 2013). Column 3 shows that the inclusion of these two high-dimension fixed effects does not alter our estimation.[22] In fact, the magnitude of the effect of hurricane proximity on cash holdings remains exactly the same. In column 3, we show that this effect is robust to the inclusion of firm-specific controls: age, size and market-to-book. Because such controls might be endogenous to the proximity of a hurricane disaster, we do not include them in our basic specification.[23] Similar to Bertrand and Mullanaithan (2003), we prefer to verify that our findings are not modified by their inclusion.[24] Overall, the effect is extremely robust to the different specifications, and the magnitude of the coefficient is always the same. Consistent with the *availability heuristic* hypothesis, managers respond

[22]See Guimaraes and Portugal (2010) for a simple procedure to estimate models with two high-dimension fixed-effects.

[23]See Roberts and Whited (2012) for a discussion about the effect of including covariates as controls when they are potentially affected by the treatment.

[24]Similarly, this result does not change when other control variables frequently associated in the literature with the level of cash holdings are added, such as capital structure, working capital requirements, capital expenditures, or R&D expenses.

to the sudden salience of danger by increasing their firm cash holdings, although there is no indication that the danger is bigger now than it was.

[INSERT TABLE 4 AROUND HERE]

In Table 4, we examine how the effect of hurricane proximity on cash holdings changes over time. Specifically, we study the difference in the level of cash holdings between treated and control firms at different points in time before and after hurricane landfall. To do so, we replace the $Neighbor$ variable with a set of dummy variables, $Neighbor^q(i)$, that captures the effect of the saliency of the event at the end of every quarter surrounding the hurricane. For each quarter i $(-i)$ after (before) the hurricane, we create a variable, $Neighbor^q + i$, that is equal to one if the county location of the firm headquarters at the end of the quarter was in the neighborhood of an area hit by a hurricane during quarter q0 and zero otherwise. The regression coefficient estimated for this dummy variable then measures the difference-in-differences in the level of cash holdings i $(-i)$ quarters after (before) the disaster. We undertake the same procedure for the $DisasterZone$ variable. This approach allows us to identify when the effect starts and how long it lasts. Column 1 of Table 4 shows that no statistically significant change in cash holdings appears before the hurricane event for firms located in the neighborhood area. However, consistent with a causal interpretation of our result, we do find that the amount of cash begins to increase following the occurrence of the hurricane.[25] This effect increases during the subsequent three quarters, and the increases in cash holdings reach their maximum during $q+2$ and $q+3$, which is when the following annual hurricane season begins and becomes active. On

[25]The positive and statistically significant effect for $Neighbor^q0$ does not contradict our interpretation. Indeed, $q0$ is the first balance sheet published after the event and therefore shows the change in cash that occurs in reaction to the hurricane.

average, hurricanes from our sample occur by mid-September. The next annual hurricane season starts around mid-June (i.e. after $q+2$ and before $q+3$). The coefficient for the $Neighbor^q+2$ and $Neighbor^q+3$ variables show that, on average, firms located in the neighborhood area respond to the saliency of the disaster by increasing their cash levels by 1.15 and 1.13 percentage points of their total assets (approximately 20 million dollars and approximately 11% of the within-firm standard deviation of *cash*) at the end of the second and third quarters after the hurricane, respectively. The level of cash holdings then begins to decrease, and the effect progressively vanishes over the next three quarters. The coefficient for the $Neighbor^q+8$ variable shows that the average difference in cash holdings between firms in the neighborhood area and control firms is not statistically different from zero two years after the hurricane landfall.

This drop in the amount of cash holdings is consistent with our behavioral interpretation. As time goes by, memories fade, the salience of the event decreases, and the subjective probability of risk retreats to its initial value. Managers then reduce the level of corporate cash holdings.

[INSERT FIGURE 6 AROUND HERE]

We plot the result of this analysis in a graph in which we also display the evolution of the difference in corporate cash holdings between firms located in the disaster zone and control firms. This graph is presented in Figure 6. While firms in the neighborhood area experience a temporary increase in cash holdings, firms hit by the hurricane display a symmetric decrease. This "reversed mirror" trend is notable for two reasons. First, it confirms that the occurrence of a hurricane can trigger a liquidity shock, as firms hit by a hurricane experience a significant drop of 0.6 percentage points in their cash holdings.

Second, it offers an indication of the magnitude of the increase in cash observed when firms are located in the neighborhood area. Indeed, the graph demonstrates that the additional amount of cash accrued in the balance sheet (+1.1 percentage points of total assets), presumably for insurance purposes against the risk of cash shortages after a hurricane strike, exceeds the actual loss of cash (-0.6 percentage points) that firms experience when this risk materializes. Thus, even if the increase in cash holdings observed for firms in the neighborhood area was justified, the magnitude of this increase would be excessive compared to the real loss of cash at risk.

However, we do recognize that the loss of cash (-0.6%) we observe here may not correspond to the real economic cost of the hurricane. We address this issue in Section 7 when we examine market reaction at the time of landfall. We find that the present value of losses caused by the disaster represents 1.03% of the total assets of the firm, on average, which remains lower than the increase in cash observed in firms located in the neighborhood area (+1.1%). This last result is useful to determine whether managers overreact to the salience of hurricane risk, or if alternatively they properly take hurricane risk into account only when a disaster occurs and neglect this risk in normal times. Here, we cannot (and do not) rule out the possibility of risk neglect in normal times. However, we can rule out the possibility that managers correctly adjust cash holdings when a disaster occurs. Indeed, the magnitude of the increase in cash compared to the value of losses suggests that managers overshoot and increase cash holdings too much, which is more consistent with an overreaction-based explanation.

4.5.2 Cross sectional variation in managers' responses

Because firms have different characteristics, they may not respond in the same way to the salience of hurricane risk. We first investigate whether this response changes with the degree of sophistication of firm decision makers. Our primary proxy for sophistication is the experience of a firm's managers in terms of hurricane proximity. Indeed, we expect managers to learn from past experiences and to be less sensitive to danger saliency if they have previously been "fooled". In practice, we count the number of instances in which a firm has been located in the neighborhood area during previous hurricane events. We then split our sample into three categories of sophistication (low, medium, and high). Firms are assigned to the low (medium or high) sophistication category if their headquarters were never (once or more than once, respectively) located in the neighborhood area during a prior hurricane event.

To complement this analysis, we also use two more indirect proxies for sophistication: firm size and age. We use firm's size because we expect large firms to be run by sophisticated CEOs and CFOs (e.g. Krueger Landier and Thesmar, 2011). We use the age of the firm because various studies in the behavioral literature show that young age is more associated with behavioral biases (Greenwood and Nagel, 2009; or Malmendier and Nagel, 2011). Each period, we split our sample into terciles of firm size and terciles of firm age, and we assign firms to the high, medium, or low sophistication category if they belong to the high, medium, or low tercile of the distribution, respectively.

For each criterion (experience, size, and age), we define three dummy variables corresponding to each sophistication category (e.g., *Low Sophistication*, *Medium Sophistication*, *High Sophistication*). We then interact each dummy variable with the $Neighbor$

variable to investigate how the response to the salience of hurricane risk varies with the degree of managerial sophistication.

[INSERT TABLE 5 AROUND HERE]

Columns 1 to 3 of Table 5 indicate that a low degree of sophistication systematically leads to a strong increase in the amount of cash holdings. Conversely, we find no statistically significant change in cash holdings for firms whose managers are likely to be more sophisticated. In all three cases, an F-test indicates that the difference between the two coefficients (high vs. low) is statistically significant at the 1% or 5% level. Overall, the results of table 5 are consistent with our availability heuristic hypothesis, which predicts that sophisticated managers should react less to salient risks. The effect of managers' experience on how neighboring firms react to the event (column 1) is also notable because it mitigates the concern that our main finding is driven by possible regional spillover effects between the disaster area and the neighborhood area. As further discussed in section 7, corporate cash holdings may increase temporarily in the neighborhood area because of possible connections between the neighboring firms and the local economy shocked by the natural disaster. However, this explanation implies that a temporary increase in cash should *consistently* be observed after each hurricane event, which is not what column 1 suggests. Indeed, column 1 indicates that as managers accumulate experience because the same event repeats, this temporary increase in cash holdings tends to be weaker.

In the Internet Appendix, we further investigate how this response varies in the cross section of firm population. First we find that managers of firms located in the neighborhood area have a stronger response to the salience of liquidity risk when their firms are more financially constrained. Second, we show that firms in the neighborhood area also respond more strongly when their firm is more vulnerable to a hurricane disaster. Specifically, the amount of corporate cash holdings increases more when a firm operates in an industry that suffers higher losses in the case of hurricane disaster, when firms operations mainly rely on intangible assets that are more difficult to insure, and when firms are less diversified geographically.

The last set of results indicates that our effect is concentrated on neighboring firms for which hurricane risk is very relevant. By contrast, firms that are less vulnerable to this specific liquidity risk react less even though their managers are exposed to the same traumatic event. This finding casts doubt on the possibility of a general fear-based reaction. Indeed, the hurricane disaster could modify managers' preferences and temporarily increase their risk aversion (Guiso, Sapienza and Zingales, 2013). However, in this fear-based story, all managers exposed to the same traumatic situation should react in the similar way, which is not what we find.

4.5.3 Robustness and validity check

Our main source of concern is the slight heterogeneity between treated firms and control firms. Although these firms are fairly comparable along various dimensions, Table 2 indicates that some differences exist in terms of age and dividends. To ensure that our results are not driven by this heterogeneity, we combine our difference-in-differences approach with a matching approach. We match on SIC3 industry, size, age, market-to-book, financial leverage, working capital requirements, capital expenditures, and dividends. The results of this analysis as well as a detailed description of our matching procedure are presented in the Internet Appendix. Overall, this analysis leads to the same conclusion as the one obtained with the simple difference-in-differences approach: firms located in the neighborhood area temporarily increase their level of cash holdings after the hurricane.

To ensure that this result is both valid and robust, we also conduct a series of additional tests that are described and reported in the Internet Appendix. In particular, we run a placebo test in which we randomly change the dates of hurricanes to ensure that our results are driven by hurricane landfalls. We also re-run our main regression in many different ways to verify that our effect is robust to alternative specifications. Finally, we verify that our effect is not driven by the manner in which we scale corporate cash holdings. Thus, we re-run the main regression using firm size (total assets) as the dependent variable and find nothing.

4.6 Is managers' reaction costly?

Because the liquidity risk remains unchanged, managers' decisions to temporarily increase cash holdings after a hurricane event are likely to be suboptimal in terms of resource allocation. In this section, we examine whether this temporary increase in cash is costly for shareholders. We begin by analyzing the counterparts to this cash increase. Next, we study whether this response to risk saliency negatively impacts firm value by reducing the value of cash.

4.6.1 Source of cash

The cash increase observed after the hurricane landfall may come from a variety of sources: an increase in revenues (*Sales Growth* variable) and operating profits (*EBIT Margin* variable), a drop in net working capital requirements (*NWC* variable), a drop in investments

(*Net investment* variable), a decrease in repurchases (*Repurchases variable*), a reduction of dividends (*Dividend* variable), or an increase in new financing (debt or equity) (*New financing* variable). Because total assets include the amount of cash holdings, we do not normalize these items by total assets and instead use the amount of sales (unless the literature suggests another more relevant normalization method). Next, we replicate our difference-in-differences analysis and apply our basic specification to each item separately.[26] The results of this analysis are reported in Table 6.

[INSERT TABLE 6 AROUND HERE]

We begin by examining whether hurricanes affect operating activity. Column 1 shows that, on average, the occurrence of a hurricane has no significant effect on revenues for firms located in the neighborhood area of the disaster zone. While sales growth decreases by 2.4 percentage points relative to the control group for firms hit by the hurricane, we find no evidence that the relative sales growth for neighborhood firms is affected by the proximity of the disaster. Column 2 confirms that neighborhood firms are truly unaffected in terms of operating activity. Unlike firms in the disaster zone, firms located in the neighborhood area suffer no significant decrease in operating margin (the coefficient on the $Neighbor$ variable is not statistically different from zero).

In the rest of Table 6, we examine other possible channels through which the change in cash holdings may occur. We find no evidence that the proximity of the hurricane modifies either the investment activity (columns 3 and 4) or the financing activity (column 7). All coefficients have the expected sign and go in the direction of an increase in cash, but none is statistically significant. We also find no evidence that neighborhood

[26]We include firm-quarter fixed effects rather than firm fixed effects in the specification to adjust for within-year seasonality. Using firm fixed effects leads to the same results.

firms reduce the amount of repurchases after the hurricane (column 5). The sign of the coefficient is negative, but again, it is not statistically significant. However, we find that the proximity of the disaster changes payout policies. Indeed, column 6 indicates that firms in the neighborhood area tend to pay lower dividends and retain more earnings after the hurricane (the coefficient on the Neighbor variable is negative and statistically significant at the 5% level). The economic magnitude of the coefficient is low compared to the increase in cash. One possible interpretation is that managers also marginally adjust all sources of cash inflow. This would explain why all other coefficients have the right sign but turn out insignificant.

In columns 8, 9 and 10, we further investigate whether hurricanes affect the payout policy or the financing policy. We use a linear probability model to assess whether hurricane landfalls affect the likelihood of stock repurchases, dividend payment, and new financing issues. In column 8, we find that the likelihood of a stock repurchase is lower in the case of hurricane proximity. Similarly, column 9 indicates a decrease in the probability of dividend payment. However, we find no change in the probability of new security issues in column 10.

Overall, these results suggest that, when located in the neighborhood area of a disaster zone, firm managers increase earnings retention and probably, also marginally adjust all other sources of cash inflow.

4.6.2 Value of cash

We next investigate whether this change in cash holdings is an efficient decision or a source of value destruction for shareholders. If it is an efficient decision, the increase in cash holdings should translate into a similar increase in value for firm shareholders. If by contrast, cash would have been better employed otherwise, the additional cash accrued in the balance sheet should be discounted and will not result in a similar increase in terms of market capitalization. In our tests, we follow the literature on the value of cash (Faulkender and Wang, 2006; Dittmar and Mahrt-Smith, 2007; Denis and Sibilkov, 2010). We examine how a change in cash holdings leads to a change in market valuation for firms in the neighborhood relative to control firms over different time periods surrounding the hurricane event. We estimate the additional market value that results from a change in a firm's cash position by regressing the abnormal stock return of the firm on its change in cash holdings and various control variables. The coefficient for the change in cash holdings is then interpreted as a measure of the value of a marginal dollar of cash. Next, we interact this coefficient with a dummy variable, $Neighbor^{q}0$, that is equal to 1 if the firm is in the neighborhood area at time $q0$. This allows us to assess whether being in the neighborhood area of a hurricane marginally deteriorates or improves the value of a marginal dollar of cash. The abnormal return we use is the stock return in excess of the Fama and French (1993) size and book-to-market portfolio return. All control variables are those used in the cash value literature. We exclude from our analysis those observations that correspond to firms located in the disaster zone and to stocks that are not sufficiently liquid.[27]

[INSERT TABLE 7 AROUND HERE]

[27] Stocks not sufficiently liquid are defined as stocks with more than 50% of zero daily returns during the time window considered in the analysis (see Lesmond et al. (1999) for a discussion about the relationship between illiquidity and zero returns).

In columns 1 and 2 of Table 7, we estimate the value of cash during two time periods that end before the occurrence of the hurricane. We find that being located in the neighborhood area at time $q0$ does not change the value of cash before the occurrence of the hurricane. This result is reassuring as cash variations for these firms (Neighborhood area) are not yet statistically different from those of other firms in the rest of the US mainland. However, when the time window begins to capture the hurricane event, the same analysis shows that the value of cash decreases for firms that are in the neighborhood area. In column 3, for instance, the interaction term between $Neighbor^{q}0$ and *Change in cash* is negative and statistically significant. This result indicates that over a 6-month period surrounding the hurricane landfall, the value of a marginal dollar of cash decreases on average by 22 cents when the firm is located in the neighborhood area compared to an average value of 88 cents otherwise. In columns 4 and 5, we use larger time windows around the event, and we obtain similar results. Unsurprisingly, the effect disappears when the time window becomes too large (column 6) because firms located in the neighborhood area increase their level of cash holdings only temporarily.

Overall, these results suggest that the managerial decision to increase the amount of corporate cash holdings temporarily after hurricanes negatively impacts firm value by reducing the value of cash.

4.7 Are there any other alternative explanations?

In this section, we discuss alternative explanations to our results, namely, the possibility of "regional spillover", "change in risk", and/or "risk learning". We first examine and test the implications of each alternative interpretation. Next, we propose and perform another experiment based on earthquake risk whose design alleviates the concern that such alternative explanations are driving our findings.

4.7.1 The possibility of "regional spillover"

First, cash might increase temporarily because of geographical externalities. Indeed, firms located in the neighborhood area could be indirectly affected by the hurricane. Such indirect effects may then explain why the amount of cash holdings temporarily increases. We review the main possible regional spillover effects and test whether they are likely to drive our results.

Higher business and / or investment opportunities

A first spillover effect might arise if the hurricane creates new business or investment opportunities for firms in the neighborhood area. In this case, neighborhood firms may temporarily hold more cash because they make more profits or because they plan to invest in the disaster zone. Under this possible interpretation of our results, firms located in the neighborhood area should thus perform better and invest more after the disaster. However, none of our findings in Table 6 are consistent with such predictions. Indeed, we find no evidence that the proximity of the hurricane positively impacts either growth in terms of revenue or operating income. In addition, we do not find that neighborhood firms invest more after the hurricane. In the Internet Appendix, we further investigate how the hurricane affects the growth of sales for neighborhood firms relative to the control group at every quarter surrounding the disaster. The graph in Figure 7 illustrates the main outcome of this analysis.

[INSERT FIGURE 7 AROUND HERE]

This graph shows that growth in revenues for neighborhood firms does not increase significantly relative to the control group after the hurricane. Therefore, and unlike firms located in the disaster zone, firms located in the neighborhood area are on average truly unaffected. This conclusion is also supported by the analysis of the market reaction at the time of the hurricane landfall.

[INSERT TABLE 8 AROUND HERE]

In Table 8, we report the results of a simple event study analysis. For each group of firms (disaster area, neighborhood area, and the rest of the US mainland), we estimate the average Cumulated Abnormal Return (CAR) of the stock price over the hurricane event period. The methodology used to perform this event study is described in the Internet Appendix. Unsurprisingly, we find a negative abnormal return for firms located in the disaster zone. However, we find no significant reaction for firms located in the neighborhood area, which suggests that investors perceive that there are no benefits (new business and/or investment opportunities) from the proximity of the natural disaster.[28]

Higher business uncertainty

A second form of spillover effect might arise if the hurricane creates locally higher business uncertainty. In this case, managers may decide to stop and/or postpone their investment projects. Neighborhood firms would then temporarily hold more cash. However, this explanation would imply a negative reaction at the announcement of the hurricane, which

[28] We also note that at the time of the event study, the change in cash holdings is not yet observable by market participants. Thus, finding no market reaction here is not inconsistent with the decrease in the value of cash observed in Table 7.

we do not find. We also do not find that firms in the neighborhood area reduce their investments in Table 6 (Column 4). We also explicitly test whether the proximity of the hurricane creates higher uncertainty.

We begin by examining whether the proximity of the hurricane affects the volatility of firm revenues.

[INSERT TABLE 9 AROUND HERE]

We use two different approaches to conduct this examination. In Panel A of Table 9, we estimate revenue volatility at the firm level using the standard deviation of sales growth in a time series. We estimate the standard deviation of the growth in revenues before and after the hurricane for each firm over a four-quarter period.[29] We then test whether this standard deviation is higher for firms in the neighborhood are after the hurricane. In panel B of Table 9, we estimate revenue volatility at the county level using the standard deviation of sales growth in cross section. We estimate the standard deviation of the growth in revenues across all firms from the same county at every quarter surrounding the hurricane event. We then test whether this standard deviation at the county level is affected by the hurricane. Under both approaches, we find that the proximity of the hurricane strike does not significantly affect the variance in revenues.

[INSERT TABLE 10 AROUND HERE]

Our analysis of stock return volatility in Table 10 also provides evidence that the hurricane does not create higher uncertainty for firms in the neighborhood area. In Panel A, we follow a methodology proposed by Kalay and Loewenstein (1985) and use an F-test to assess whether a hurricane event affects stock return variances. We find that an

[29]Estimating the standard deviation over a longer time window leads to the same results.

F-test cannot reject at the 5% level the null hypothesis that the pre-hurricane and post-hurricane stock return variances are equal for the majority of firms in the neighborhood area (64.8%). We next compute stock return volatility at each quarter and test in Panel B whether this volatility changes for firms in the neighborhood area using our baseline specification; we again find that the proximity of the hurricane does not affect stock return volatility. Overall, these results suggest that investors do not perceive higher uncertainty after the hurricane.

Higher financing constraints

Other regional spillover effects include the possibility that the hurricane hurts the lending capacity of banks. If bank customers withdraw their deposits after the hurricane, banks located in the disaster zone and/or the neighborhood area may no longer be able to effectively finance the local economy. Firms in the neighborhood might anticipate that banks will be constrained after the shock and may decide to hold more cash as a precaution. Under this explanation, the amount of new credits at the bank level should decrease after the hurricane. We test this prediction in the Internet Appendix and find the opposite result. In fact, the amount of new commercial and industrial loans increases after the hurricane event for banks located in the disaster zone and for banks located in the neighborhood area relative to other banks. This result casts doubts on the possibility that the hurricane damages the entire local bank lending capacity. It is also consistent with our findings in Table 6 that the proximity of the hurricane does not negatively affect the probability of issuing new financing (Column 10).

A similar alternative story could be that the hurricane hurts local insurance companies and generates insurance rationing (Froot and O'Connell (1999), Froot (2001)). Neighboring companies may react to increased insurance costs by reducing their level of insurance and by increasing their level of cash instead. After some time, insurance premia return to normal levels. Firms then insure again and decrease their cash holdings accordingly. However, at least two of our findings are difficult to reconcile with this explanation. First, cash holdings increases over a one-year period whereas Froot and O'Connell (1999) show that prices for insurance tend to rise over a 3-year period. Second, under the insurance-based explanation, the increase in cash holdings should be concentrated on firms that depend on insurance companies to insure their business. By contrast, firms that are more likely to self-insure should react less. Our result from the internet appendix does not support this prediction. In fact, firms with a lot of intangible assets that cannot be directly insured react more.

Other forms of regional spillover effects

Because a variety of other forms of regional spillover effects might affect our results, we conduct another series of tests in which we focus on firms operating outside of the disaster zone and outside of the neighborhood area. To the extent that these firms are less dependent on the local economy, any increase in corporate cash holdings should be less likely to be driven by a regional spillover effect. The results of these tests are reported in Table 11.

[INSERT TABLE 11 AROUND HERE]

In the first column, we re-run our main test and focus on firms that do not have significant business connections with other firms potentially affected by the hurricane event. Using the Compustat Customer Segment database, we identify 287 neighborhood firms from our sample that have their main customer and/or provider in the disaster area. Column 1 indicates that excluding those firms from our sample does not change our main result: neighborhood firms increase the amount of their corporate cash holdings after a disaster.

In the second column, we examine the effect of the disaster on "the neighbors of neighbors". We define two groups of neighbors according to geographical distance. Specifically, we create a fourth category of firms that correspond to firms located in the neighborhood of the disaster zone but not in its close neighborhood (hereafter, a "Remote Neighbor"). To identify these firms, we match with replacement each affected county with its ten nearest neighbors among the non-affected counties. Firms are then assigned to the Remote Neighbor group if their headquarters are located in the ten nearest non-affected counties but not in the five closest. For each firm identified as a "Remote Neighbor", we calculate the distance between its headquarters and the headquarters of the closest affected firm. On average, we find that firms from our Remote Neighbor group are 80 miles away from the disaster zone. Despite the distance, the regression in Column 2 indicates that these firms also respond to the occurrence of the hurricane by increasing the amount of cash holdings.

In the third column, we focus on all vulnerable firms (excluding firms in the neighborhood of the affected region). Those firms may be far away from the disaster zone (e.g. firms located in the East coast when a hurricane hits Louisiana). We define a firm as sensitive to the risk of hurricane strike if it has been strongly affected once by a hurricane during the sample period. We create a dummy variable Vulnerable that is equal to one if (i) the firm is identified as sensitive to the risk of hurricane disaster, (ii) the firm is neither in the disaster area nor in the neighborhood area, and (iii) the hurricane made landfall over the past twelve months. We obtain a group of 614 "vulnerable firms", whose average distance from the disaster zone is 444 miles. Despite such a distance, the regression in Column 3 indicates that the managers of these firms increase cash holdings after the hurricane.

Overall, these results suggest that while some regional spillover effects may possibly affect firms in the neighborhood area, these effects cannot be the key explanation of our primary finding.

4.7.2 The possibility of a "change in risk"

Cash holdings might also increase if the real probability of being struck by a hurricane increases. However, this explanation would imply a permanent increase in cash, which we do not find in our results. To be consistent with a "change in risk" interpretation, the increase in risk must be temporary.

Such a temporary increase in risk might occur if hurricane strikes cluster in certain geographic areas during a one-year or two-year period. In this case, being a neighbor could indicate that the probability of being hit by a hurricane in the coming year is now higher than it used to be. We are not aware of any evidence of such a clustering phenomenon in the climate literature (see section 2). Nevertheless, we assess this possibility by testing whether the probability of being hit by a hurricane depends on the geographical location of past hurricane strikes. We use a linear probability model to test whether being in the neighborhood of an area hit by a hurricane affects the probability of being hit by a hurricane in the future. The dependent variable is a dummy equal to 1 if the county is hit by a hurricane. The main explanatory variable is a dummy equal to 1 if a hurricane event occurred over the past 12 months and if the county was in the neighborhood of the disaster zone. The results of this test are reported in table 12.

[INSERT TABLE 12 AROUND HERE]

In Column 1, the regression coefficient for the variable Neighbor is not statistically different from zero, which indicates that when a hurricane makes landfall in a given county, the event reveals no information about future disaster likelihood in the neighboring counties.[30]

4.7.3 The possibility of "risk learning"

Finally, cash holdings might increase if managers ignore or underestimate the risk before the occurrence of the hurricane and learn the true probability of a disaster after the hurricane's landfall. However, this explanation would again imply a permanent increase in cash, which we do not find.

It is also difficult to reconcile such a risk-learning hypothesis with our results regarding the value of cash. If managers learn the true probability of suffering a liquidity shock and increase their cash holdings accordingly, investors should value this decision positively and should not discount the additional cash in the balance sheet.

[30]Column 2 shows the same result when taking into account all hurricanes from the SHELDUS database (and not only the 15 biggest).

4.7.4 Reaction to extreme earthquakes outside the US

To further alleviate the concern that our results are driven by a non-behavioral explanation, we perform one final experiment based on earthquake risk rather than hurricane risk. We test the validity of the *availability heuristic* hypothesis by looking at US firms whose headquarters are located in urban communities in which earthquakes are frequently felt. We then focus on the announcement of extremely violent (and therefore salient) earthquakes outside the US and examine whether these firms respond to such announcements by changing the amount of their cash holdings. Finding an increase in cash holdings would then be consistent with the *availability heuristic* hypothesis while allowing us to rule out other possible explanations. Indeed, it would neither be consistent with the *change in risk hypothesis* nor with the *risk-learning* hypothesis because the occurrence of an earthquake outside the US (for instance, in Pakistan) provides no information about the likelihood of experiencing an earthquake in US territory.[31] It would also not be consistent with the geographical spillover hypothesis because of the distance to the disaster area.

We obtain information about the level of intensity felt by zip code address for each earthquake from the *"Did you feel it?"* surveys performed under the Earthquake Hazard Program by the USGS. For each zip code, we compute the average earthquake intensity felt over the past 20 years. We assign the average earthquake intensity felt to each firm in Compustat using the zip code from the headquarters' address. We then focus on firms within the top 10% of the average intensity felt distribution and assign them to a seismic zone group (treatment group). All other firms are assigned to a non-seismic zone group

[31] In addition, this test focuses on US firms whose managers frequently feel earthquakes. Thus, they cannot ignore this risk. This also casts doubts on the possibility of a learning reaction.

(control group). Next, we focus on the strongest earthquakes that have occurred outside the US in the past 30 years according to descriptions of magnitude, total deaths, and total damage. We obtain all this information from the Significant Earthquake Database.[32]

These selection criteria lead to the list of major non-US earthquakes described in the Internet Appendix. We then estimate the average change in cash holdings for the seismic zone group around the announcement of the earthquake outside the US using exactly the same matching methodology as the one previously used and described above for hurricanes. The results of this analysis are depicted in the graph of Figure 8.[33]

[INSERT FIGURE 8 AROUND HERE]

Figure 8 shows qualitatively the same pattern as that previously observed. Firm managers located in seismic areas respond to the sudden salience of earthquake risk by temporarily increasing the level of cash holdings compared to firms located outside a seismic zone. This analysis confirms that firm managers are subject to the availability bias while rejecting other non-behavioral explanations.

4.8 The effects of cash holdings on post-hurricane performance

If managers respond to the salience of hurricane risk by increasing corporate cash holdings, and if this reaction is motivated by seeking insurance against such risk, then we should expect cash holdings to protect firm revenues and reduce losses when this risk

[32] National Geophysical Data Center/World Data Center (NGDC/WDC) Significant Earthquake Database, Boulder, CO, USA. (Available at http://www.ngdc.noaa.gov/nndc/struts)

[33] More details about our methodology and the detailed results are provided in the Internet Appendix.

materializes. We run this falsification test in this section. We focus on firms affected by a hurricane event and examine how the level of cash holdings before the disaster affects firm performance in terms of sales growth after the disaster.

To perform this test, we again use a difference-in-differences methodology. We use an approach identical to that used to estimate the effect of a hurricane on cash holdings except that (i) firms in the treatment group are firms whose headquarters are located in the disaster area, (ii) firms assigned to the control group are all other firms, and (iii) the outcome variable we are interested in is growth in revenues. We estimate how firms that are directly affected by the hurricane perform in terms of sales growth relative to the control group after the disaster conditional on their level of cash holdings (low, medium or high) before the hurricane. The graph depicted in Figure 9 illustrates the main outcome of this analysis.[34]

[INSERT FIGURE 9 AROUND HERE]

This graph compares three categories of firms defined according to the level of their cash holdings before the hurricane (high, medium, or low) and shows how each category performs in terms of sales growth relative to the control group over time. All categories of firms appear to be negatively affected by the hurricane during the first two quarters following the hurricane event. On average, sales growth is approximately 9% lower for treated firms than for control firms during the second quarter following the disaster, and the economic magnitude of this revenue loss is similar across the three categories of firms. However, performance in terms of sales growth in subsequent quarters is different. Firms in the high cash tercile before the disaster rapidly catch up with firms in the control group

[34] More details about our methodology and the detailed results are provided in the Internet Appendix.

in terms of sales growth. These high cash firms even temporarily outperform control firms and recover their loss of revenues within the year following the shock. By contrast, it takes approximately two years for firms in the low cash tercile to catch up with firms in the control group in terms of sales growth, and these low cash firms never recover their losses.

Overall, these results confirm that holding cash contributes to insuring against the effects of hurricane risk. They are consistent with our primary finding and help to explain why managers may be willing to increase the amount of corporate cash holdings when they perceive that the risk of a hurricane strike is higher.

4.9 Conclusion

In their seminal paper, Tversky and Kahneman (1973, 1974) observe that people have a tendency to develop heuristic rules to reduce the complex task of estimating probabilities. They show that, although useful in general, relying on these rules can also produce mistakes. This paper provides direct evidence that firm managers rely on one such rule to assess risk: the availability heuristic. Using cash holdings as a proxy for risk management, we find that managers located in the neighborhood area of a hurricane landfall temporarily perceive more risk after the event even though the real risk remains unchanged. We show that this mistake, which is caused by the temporary salience of the danger, is costly and inefficient. It leads to reduce shareholders compensation and destroys firm value by reducing the value of cash. Over our sample period and across all firms, the total amount of cash temporarily immobilized because of this assessment bias is almost 65 billion dollars. Given the large and increasing diversity of risks that must be assessed every day by firm managers, our results suggest that the total real economic cost of this bias is likely to be considerable.

References

Acharya, V., S. A. Davydenko, and I. A. Strebulaev, (2012), "Cash Holdings and Credit Risk". *Review of Financial Studies* 25, 3572-3609.

Almeida, H., M. Campello, and M. S. Weisbach, (2004), "The Cash Flow Sensitivity of Cash". *Journal of Finance* 59,1777-804.

Baker, M., and J. Wurgler, (2012), "Behavioral Corporate Finance: A Current Survey". *Handbook of the Economics of Finance*, Vol. 2, edited by George M. Constantinides, Milton Harris, and Rene M. Stulz. Handbooks in Economics. New York, 2012.

Baker, M., and Y. Xuan, (2011), "Under New Management: Equity Issues and the Attribution of Past Returns". *Working Paper.*

Baker, M., X. Pan, and J. Wurgler, (2012), "The effect of reference point prices on mergers and acquisitions". *Journal of Financial Economics* 106, 49-71.

Barber, B. M., and T. Odean, (2008), "All That Glitters: The Effect of Attention and News on the Buying Behavior of Individual and Institutional Investors". *Review of Financial Studies* 21, 785-818.

Bates, T. W., K. M. Kahle, and R. M. Stulz, (2009), "Why Do U.S. Firms Hold So Much More Cash than They Used To?". *Journal of Finance* 64, 1985-2021.

Bertrand, M., E. Dulfo, and S. Mullainathan, (2004) "How Much Should We Trust Differences-in-Differences Estimates?". *Quarterly Journal of Economics*, 119, 249-75.

Betton, S., B. E. Eckbo, K. S. Thorburn, (2008), "Corporate Takeovers", ". *Handbook of Corporate Finance: Empirical Corporate Finance*, Vol. 2, Chapter 15, 291-430.

Bin, O., and Stephen Polasky, (2004), "Effects of Flood Hazards on Property Values: Evidence before and after Hurricane Floyd". *Land Economics* 80. 490-500.

Blake, E. S., C. W. Landsea, and E. J. Gibney, (2011), "The deadliest, costliest, and most intense United States tropical cyclones from 1851 to 2010 (and other frequently requested hurricane facts)". *NOAA Technical Memorandum NWS NHC-6.*

Bordalo, P., N. Gennaioli, and A. Shleifer, (2012a). "Salience in Experimental Tests of the Endowment Effect.". *American Economic Review* 102, 47-52.

Bordalo, P., N. Gennaioli, and A. Shleifer, (2012b). "Salience Theory of Choice Under Risk". *Quarterly Journal of Economics* 127, 1243-85.

Bordalo, P., N. Gennaioli, and A. Shleifer, (2013), "Salience and Consumer Choice". *Journal of Political Economy* (Forthcoming).

Brav, Alon P., J.R. Graham, C. Harvey and R. Michaely, (2005), "Payout Policy in the 21st Century". *Journal of Financial Economics* 77, 483-527.

Choi, J. J., D. Laibson, B.C. Madrian and A. Metrick, (2009), "Reinforcement Learning and Savings Behavior". *Journal of Finance* 64, 2515-2534.

DellaVigna, S. (2009). "Psychology and Economics: Evidence from the Field". *Journal of Economic Literature*, 47(2), 315-372.

Denis, D. J., and V. Sibilkov, (2010), "Financial Constraints, Investment, and the Value of Cash Holdings". *Review of Financial Studies* 23, 247-269.

Dittmar, A., and J. Mahrt-Smith, (2007), "Corporate Governance and the Value of Cash Holdings". *Journal of Financial Economics* 83,599-634.

Dougal C., J. Engelberg, C. A. Parsons and E. D. Van Wesep, (2011). "Anchoring and the Cost Capital". *Working Paper.*

Elsner, J. B., and B. H. Bossak, (2001), "Bayesian Analysis of U.S. Hurricane Climate". *Journal of Climate* 14, 4341-4350

Emanuel K. (2005), "Emanuel replies". *Nature* 438, E13.

Fama, E., French, K., (1993), "Common risk factors in the returns on stocks and bonds". *Journal of Financial* Economics 33, 3-56.

Faulkender, M., and R. Wang, (2006), "Corporate Financial Policy and the Value of Cash". *Journal of Finance* 61, 1957-1990.

Fresard L. and P. Valta, (2012), "Competitive Pressures and Corporate Policies". *Working Paper.*

Froot, K. A., (2001), "The market for catastrophe risk: A clinical examination". *Journal of Financial Economics* 60, 529-571.

Froot, K. A., and D. S. Scharfstein, and J. C. Stein, (1993), "Risk Management: Coordinating Corporate Investment and Financing Policies". *Journal of Finance* 48, 1629-1658.

Garmaise, M. J. and Moskowitz, T. J. (2009), "Catastrophic Risk and Credit Markets". *Journal of Finance*, 64(2), 657-707.

Gennaioli, N., and A. Shleifer, (2010), "What Comes to Mind". *Quarterly Journal of Economics* 125, 1399-1433.

GuimarÃ£es, P., and P. Portugal, (2010), "A simple feasible procedure to fit models with high-dimensional fixed effects". *Stata Journal* 10:628-49.

Guiso, L., Sapienza, P., and L. Zingales, (2013), "Time Varying Risk Aversion". *Working*

Paper.

Greenwood, R., and S. Nagel, (2009), "Inexperienced investors and bubbles". *Journal of Financial Economics* 93, 239-258.

Harford, J., (1999), "Corporate Cash Reserves and Acquisitions". *Journal of Finance* 54,1969-97.

Heckman, J. J., H. Ichimura, and P. Todd, (1998), " Matching As An Econometric Evaluation Estimator". *Review of Economic Studies* 65, 261-294.

Hirshleifer, D. A., K. Hou, S. H. Teoh, and Y. Zhang, (2004), "Do Investors Overvalue Firms with Bloated Balance Sheets?". *Journal of Accounting and Economics* 38, 297-331.

Hirshleifer, D. A., and S. H. Teoh, (2003), "Limited Attention, Information Disclosure, and Financial Reporting". *Journal of Accounting and Economics* 36, 337-386.

Hirshleifer, D. A., S. Lim and S. H. Teoh, (2009), "Driven to Distraction: Extraneous Events and Underreaction to Earnings News". *Journal of Finance* 63, 2287-2323.

Hirshleifer, D. A., S. Lim and S. H. Teoh, (2011), "Limited Investor Attention and Stock Market Misreactions to Accounting Information". *Review of Asset Pricing Studies* 1, 35-73.

Hirshleifer, D. A., S. H. Teoh, S. H., and A. Low, (2012), "Are Overconfident CEOs Better Innovators?". *Journal of Finance* 67, 1457-1498.

Ho, Thomas S. Y., and Roni Michaely, (1988), "Information Quality and Market Efficiency". *The Journal of Financial and Quantitative Analysis* 23, 53-70.

Holmstrom B. and J. Tirole, (2000), "Liquidity and Risk Management". *Journal of Money, Credit and Banking* 32, 295-319.

Holmstrom B., and J. Tirole, (1998), "Private and Public Supply of Liquidity". *Journal of Political Economy* 106, 1-40.

Huberman, G, and T. Regev, (2001), "Contagious speculation and a cure for cancer: A nonevent that made stock prices soar". *The Journal of Finance* 66, 387-396.

Johnson, E. J., J. Hershey, J. Meszaros, and H. Kunreuther, (1993), "Framing, probability distortions, and insurance decisions". *Journal of Risk and Uncertainty* 7, 35-51.

Kalay, A., and U. Loewenstein, (1985), "Predictable events and excess returns. The Case of Dividend Announcements". *Journal of Financial Economics* 14,429-449.

Kaustia, M., and S. KnÃ$\frac{1}{4}$pfer, (2008), “Do Investors Overweight Personal Experience? Evidence from IPO Subscriptions”. *Journal of Finance* 63, 2679-2702.

Khokhar, R., (2012), "Window Dressing in Corporate Cash Holdings: Evidence from Quarterly Financial Statements". *Working Paper.*

Kim, C.-S., D. Mauer, and A. Sherman, (1998), "The Determinants of Corporate Liquidity: Theory and Evidence". *Journal of Financial and Quantitative Analysis* 33, 335-59.

Klibanoff, P., O. Lamont, and TA Wizman, (1998), "Investor Reaction to Salient News in Closed-End Country Funds". *The Journal of Finance* 51, 673-700.

Kousky, C., (2010), "Learning from Extreme Events: Risk Perceptions after the Flood". *Land Economics* 86, 395-422.

Krueger, P., A. Landier, and D. Thesmar, (2011), "The WACC Fallacy: The Real Effects of Using a Unique Discount Rate". *Working Paper*

Kunreuther, H. C., ed. (1978). *Disaster Insurance Protection: Public Policy Lessons.* New York: John Wiley and Sons.

Landier, A., and D. Thesmar, (2009), "Financial Contracting with Optimistic Entrepreneurs". *Review of Financial Studies* 22, 117-150.

Landsea, C. W., (2005), "Hurricanes and global warming". *Nature* 438, E11

Landsea, C. W., B. A. Harper, K. Hoarau, and J. A. Knaff (2005). "Can We Detect Trends in Extreme Tropical Cyclones?". *Science*, 313, 452-454.

Lesmond, D., J. Ogden, and C. Trzcinka, (1999), "A New Estimate of Transaction Costs". *Review of Financial Studies* 12, 1113-1141.

Levitt, S. D., and J. A. List, (2007), "What Do Laboratory Experiments Measuring Social Preferences Reveal About the Real World?". *Journal of Economic Perspectives* 21, 153-174.

Lichtenstein, S., and P. Slovic, (1978), "Judged frequency of lethal events". *Journal of Experimental Psychology: Human Learning and Memory* 4, 551-578.

Lins, K. V., H. Servaes, and P. Tufano, (2010), "What drives corporate liquidity? An international survey of cash holdings and lines of credit". *Journal of Financial Economics* 98, 160-176.

Ljungqvist, A. and W. Wilhelm, (2005), "Does Prospect Theory Explain IPO Market Behavior?". *Journal of Finance* 60, 1759-1790.

Loughran T. and J. Ritter, (2002), "Why don't Issuers get Upset about Leaving Money on the Table in IPOs?". *Review of Financial Studies* 15, 413-443.

MacDonald, D. N, H. L. White, P. M. Taube, and W. L. Huth, (1990), "Flood Hazard Pricing and Insurance Premium Differentials: Evidence from the Housing Market". *Journal of Risk and Insurance* 5, 654-63.

Malmendier, U., and G. Tate, (2005), "CEO Overconfidence and Corporate Investment". *Journal of Finance* 60, 2661-2700.

Malmendier, U., and S. Nagel, (2011), "Depression babies: do macroeconomic experiences affect risk taking?". *Quarterly Journal of Economics* 126, 373-416.

Malmendier, U., and S. Nagel (2011), "Learning from Inflation Experiences". *Working Paper.*

Opler, T., L. Pinkowitz, R. M. Stulz, and R. Williamson, (1999), "The Determinants and Implications of Corporate Cash Holdings". *Journal of Financial Economics* 52, 3-46.

Pielke, R. A., (2005), "Are there trends in hurricane destruction?". *Nature* 438, E11

Pielke, R. A., Jr., C. Landsea, M. Mayfield, J. Laver, and R. Pasch, (2008), "Hurricanes and global warming". *American Meteorological Society*, 1571-1575.

Pielke, R. A., Jr., J. Gratz, C. W. Landsea, D. Collins, M. A. Saunders, and R. Musulin, (2008), "Normalized Hurricane Damage in the United States:1900-2005". *Natural Hazards Review* 9, 29-42.

Ramirez, A., and N. Altay, (2011), "Risk and the Multinational Corporation Revisited: The Case of Natural Disasters and Corporate Cash Holdings". *Working Paper.*

Roberts, M. R., and T. M. Whited, (2012), "Endogeneity in Empirical Corporate Finance". *Simon School Working Paper* No. FR 11-29.

Savickas, R., (2003), "Event-induced Volatility and Tests For Abnormal Performance". *The Journal of Financial Research* 106, 165-178.

Shleifer, A., and R. Vishny, (2011), "Fire Sales in Finance and Macroeconomics". *Journal of Economic Perspectives* 25, 29-48.

Todd, P., (1999), "A Practical Guide to Implementing Matching Estimators". *Mimeo*, University of Pennsylvania.

Tversky, A., and Kahneman, D, (1973), "Availability: A heuristic for judging frequency and probability". *Cognitive Psychology* 4, 207-232.

Tversky, A., and Kahneman, D, (1974), "Judgment under Uncertainty: Heuristics and Biases". *Science* 185, 1124-31.

4.10 Figures and Tables

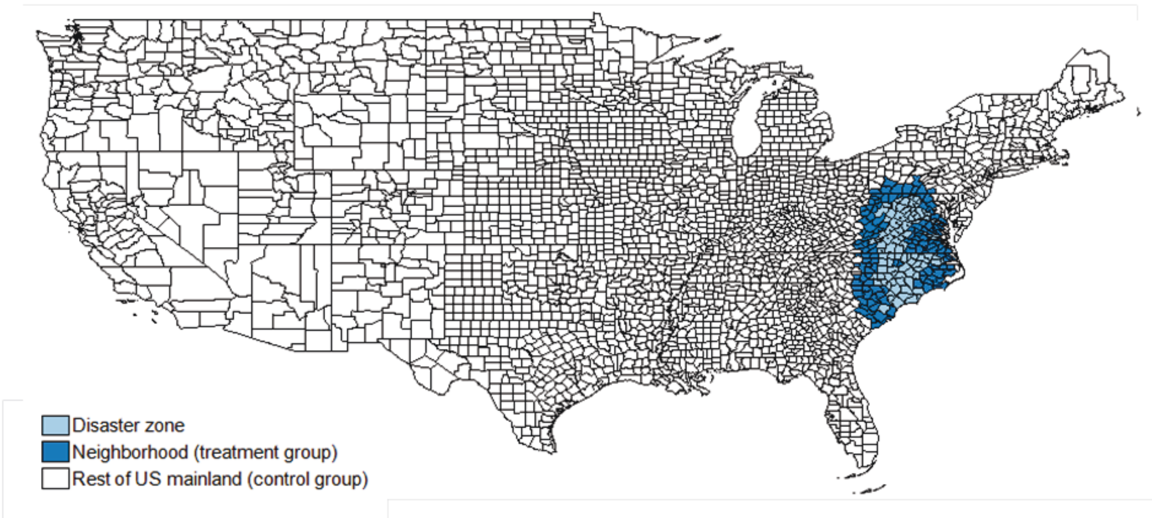


Figure 4.1: . Identification of Neighbors: Illustration for Hurricane Fran (1996)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Fran in 1996. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

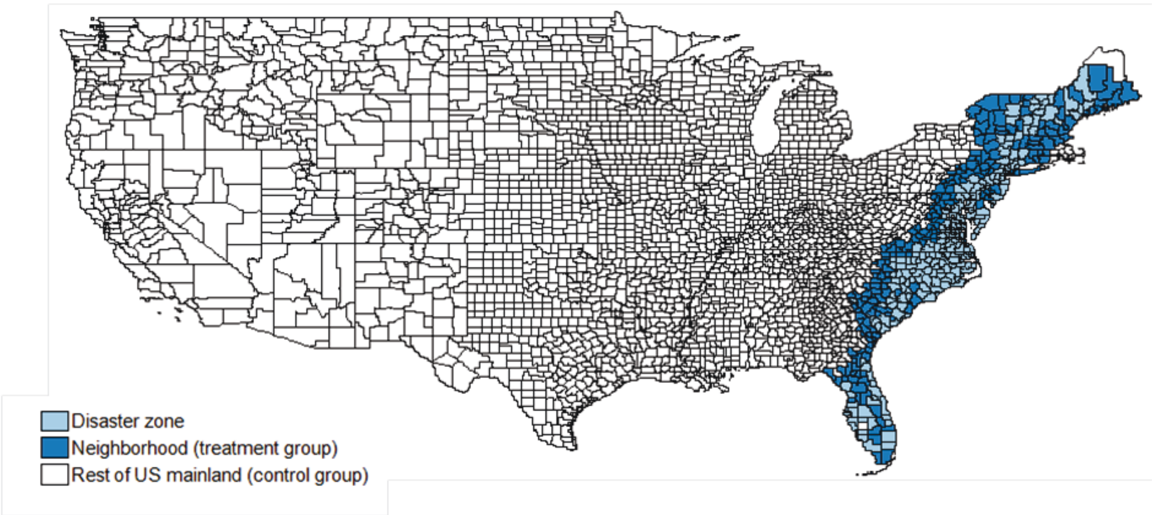


Figure 4.2: . Identification of Neighbors: Identification of Neighbors: Illustration for Hurricane Floyd (1999)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Floyd in 1999. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

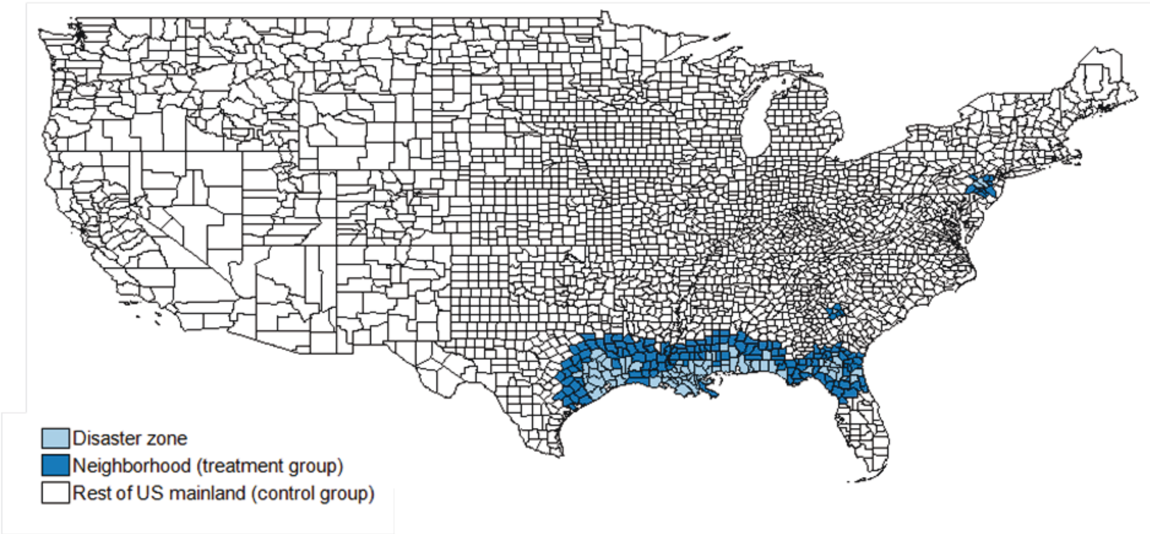


Figure 4.3: . Identification of Neighbors: Illustration for Hurricane Allison (2001)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Allison in 2001. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

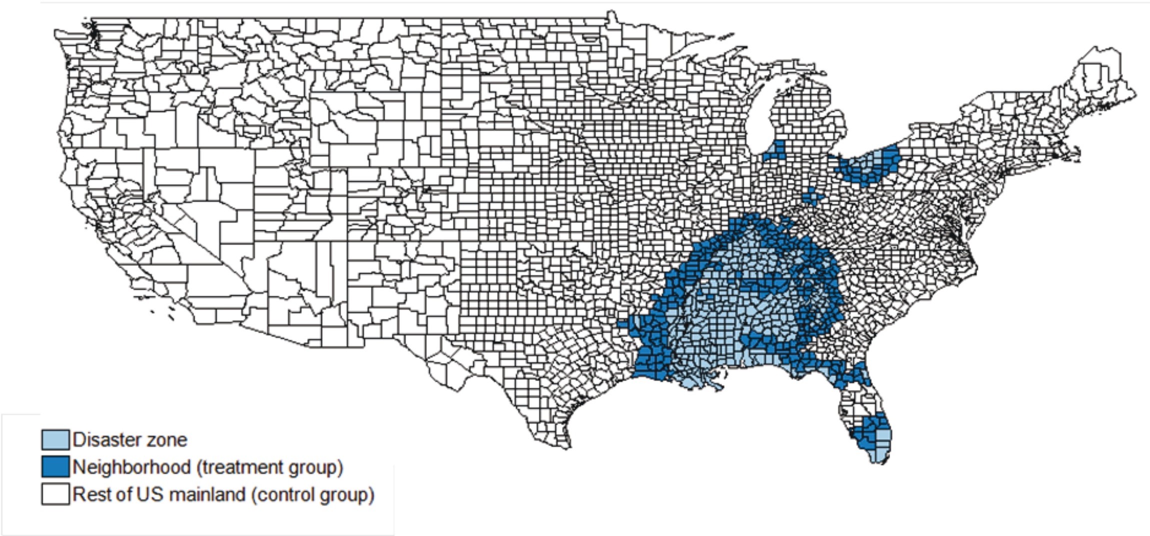


Figure 4.4: . Identification of Neighbors: Identification of Neighbors: Illustration for Hurricane Katrina (2005)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Katrina in 2005. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

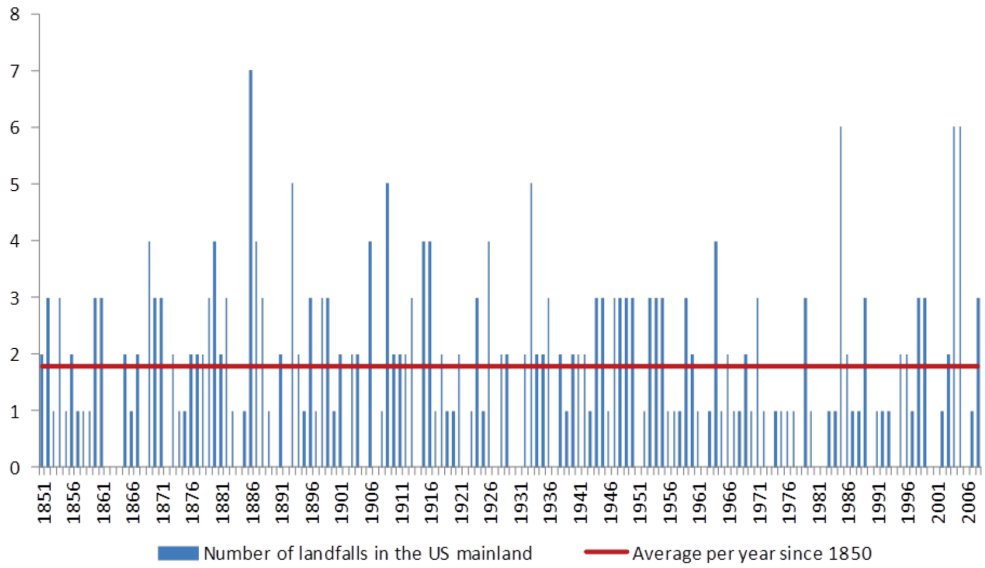


Figure 4.5: . Annual Number of Hurricanes since 1850

This graph presents the total annual number of hurricanes with landfall in the US mainland since 1850. The source of the information is the NOAA Technical Memorandum (2011)

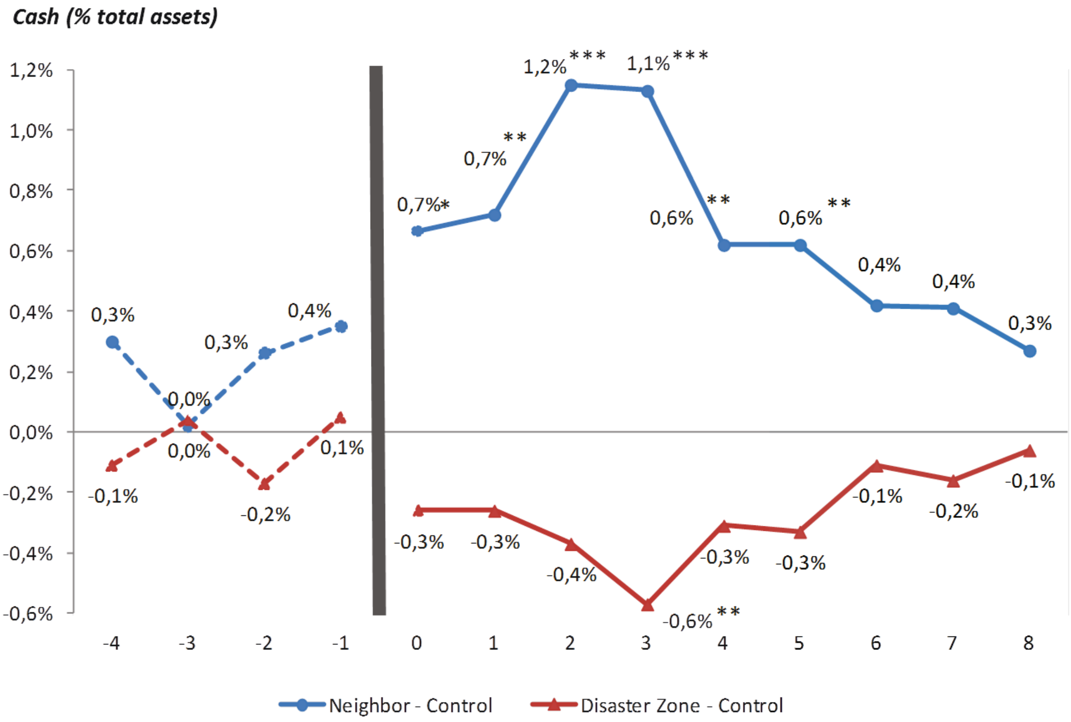


Figure 4.6: . Effects of Hurricane Proximity on Corporate Cash holdings

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters surrounding the hurricane event (quarter $q0$). The blue line plots the difference-in-differences in the level of corporate cash holdings for firms located in the neighborhood area. The red line plots the difference-in-differences in the level of corporate cash holdings for firms located in the disaster zone. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table 4. ***, **, and * denote significance at the 1%, 5% and 10% levels.

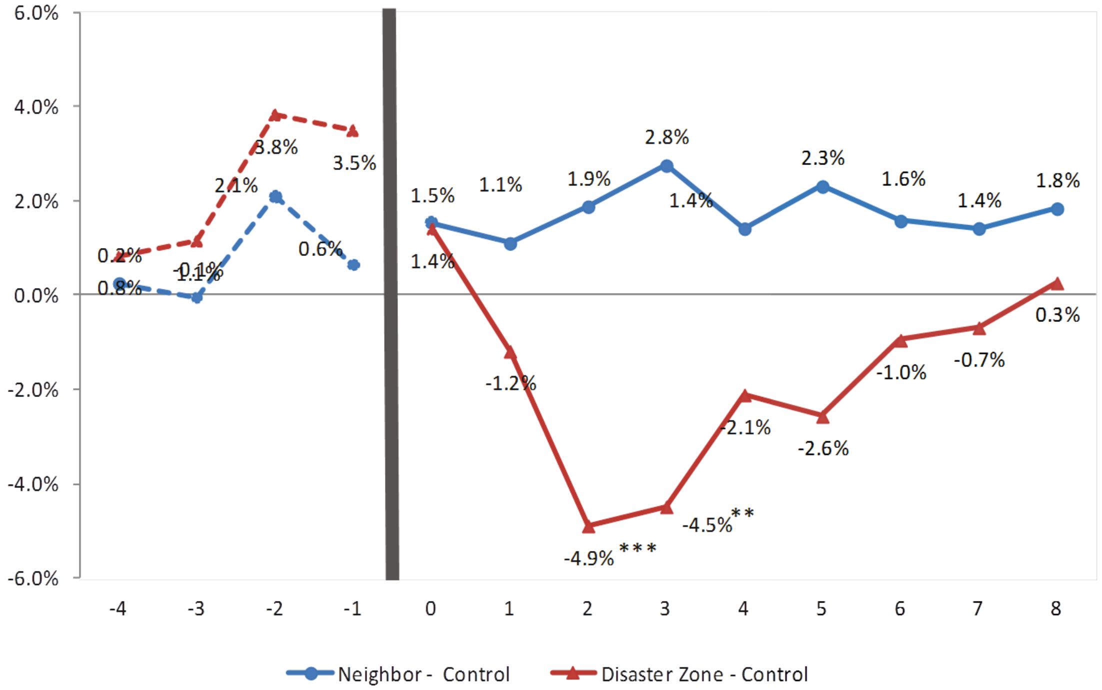


Figure 4.7: . Effects of Hurricane Proximity on Sales Growth

This graph presents difference-in-differences in sales growth at different quarters surrounding the hurricane event (quarter $q0$). The growth in sales is the growth in total revenues relative to the same quarter of the previous year. The blue line plots the difference-in-differences in sales growth for firms located in the neighborhood area. The red line plots the difference-in-differences in sales growth for firms located in the disaster zone. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table D reported in Internet Appendix. ***, **, and * denote significance at the 1%, 5% and 10% levels.

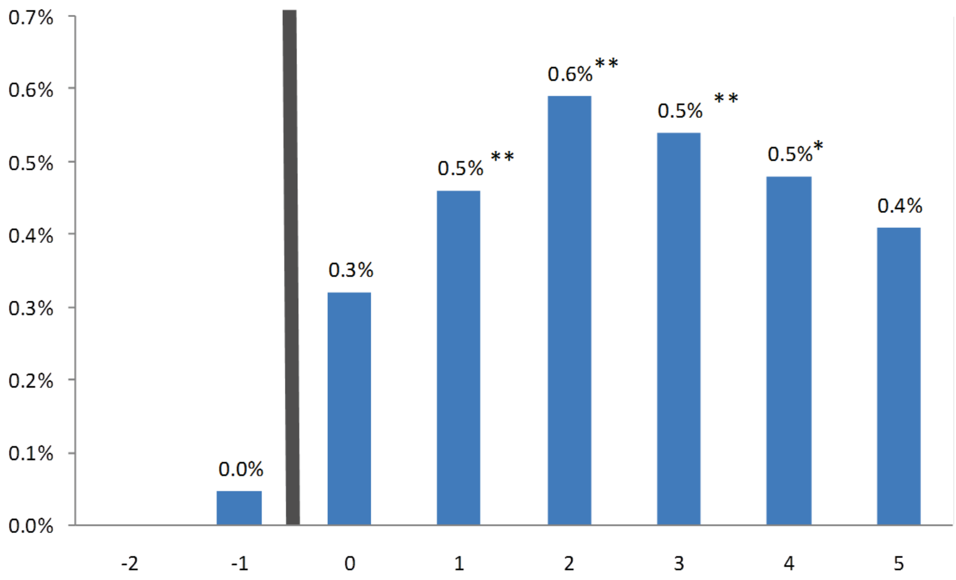


Figure 4.8: . Effects of Earthquakes outside the US on Corporate Cash holdings of US Firms

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters surrounding the announcement of a violent earthquake outside the US (quarter $q0$) for a sample of US firms located in a seismic area. This sample comprises 1,191 treated firms whose headquarters are located in a urban community where an earthquake is frequently felt according to the U.S. Geological surveys ("Seismic zone firms"). For each treated firm, the counterfactual outcome is the weighted average of the change in the level of cash holdings relative to q-2 over all control firms with the same SIC 3 code ("Matched firm"). The weighting is achieved through a kernel function so that the closer control firms in terms of Mahalanobis distance to the treated firm receive greater weight. The Mahalanobis distance is computed at quarter $q-2$ (ie. three months before the earthquake occurrence) along four dimensions: size, age, market-to-book, and financial leverage. ***, **, and * denote significance at the 1%, 5% and 10% levels.

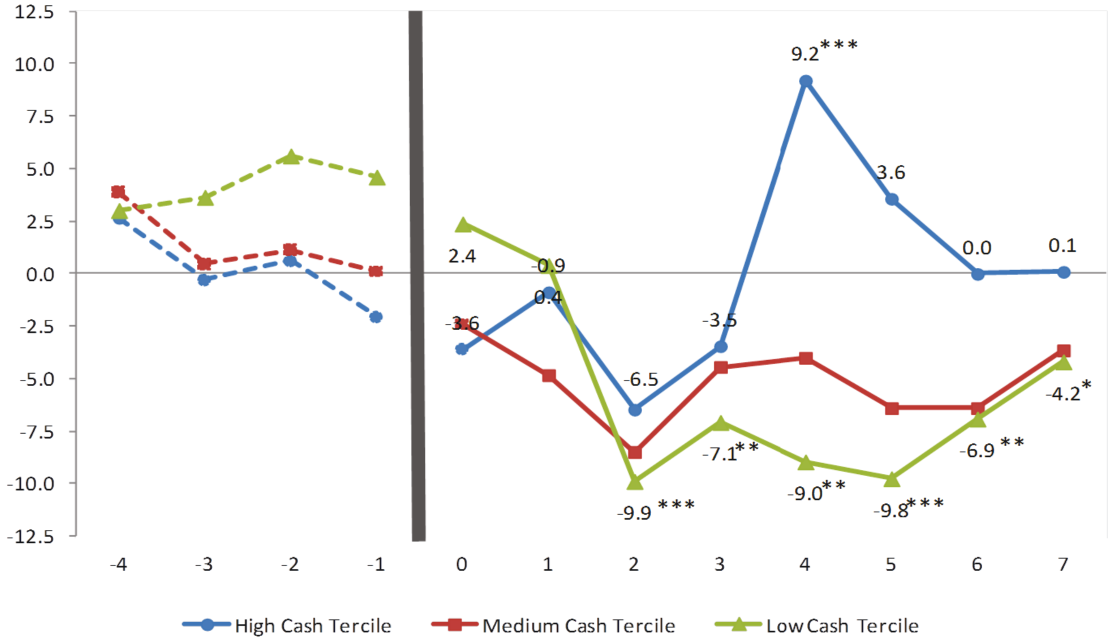


Figure 4.9: . Effects of Cash Holdings on Revenues of Firms Located in the Disaster Area

This graph presents difference-in-differences in sales growth between firms located inside and outside the disaster area at different quarters surrounding the hurricane event (quarter $q0$) conditional on the level of corporate cash holdings before the occurrence of the disaster. The growth in sales is the growth in total revenues of the firm relative to the same quarter of the previous year. The blue (respectively, red, green) line plots the difference-in-differences in sales growth for the sub-sample of firms with a level of cash holdings in the top (respectively, middle, bottom) tercile of the distribution at the end of the quarter before the occurrence of the hurricane. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table I reported in Internet Appendix. ***, **, and * denote significance at the 1%, 5% and 10% levels.

Chapter 1

Do managers overreact to salient risks?

Evidence from hurricane strikes

Joint work with Olivier Dessaint

(University of Toronto - Rotman School of Management)

It is a common experience that the subjective probability of traffic accidents rises temporarily when one sees a car overturned by the side of the road.

A. Tversky and D. Kahneman (1974)

1.1 Introduction

In this paper, we provide empirical evidence that managers exhibit biases when assessing risk. Specifically, we show that managers systematically respond to near-miss liquidity shocks by *temporarily* increasing the amount of corporate cash holdings. Such a reaction cannot be explained by the standard Bayesian theory of judgment under uncertainty because the liquidity shock stems from a hurricane landfall whose distribution is stationary (Elsner and Bossak, 2001; Pielke et al., 2008). Instead, this reaction is consistent with salience theories of choice (Tversky and Kahneman, 1973, 1974; Bordalo, Gennaioli and Shleifer, 2012a, 2012b, 2013) that predict that the temporary salience of a disaster leads managers to reevaluate their representation of risk and put excessive weight on its probability.[1]

Most corporate policy decisions are made under uncertainty and require managers to estimate risk. Standard corporate finance models assume that managers do so by estimating probabilities through a pure statistical approach. Under this assumption,

[1] We would like to especially thank FranÃ§ois Derrien and David Thesmar for their constant guidance and support. We also thank Luc Behagel, Thierry Foucault, Laurent FrÃ©sard, Nicola Gennaioli, Dong Lou, Daniel Metzger, Fabrice Riva, Andrei Schleifer, Michael Spira, seminar participants at Baruch College, CBS, ESSEC, HEC Paris, HKUST, IESE, Imperial College, INSEAD, Maryland University, Paris School of Economics, University of Miami, University of North Carolina, University of Toronto and the Wharton School of Business, as well as conference participants at the Rothschild Caesarea 10th Annual Conference in Herzliya, the 2013 European Finance Annual Conference in Cambridge, and the 2014 Frontiers of Finance conference for their comments and suggestions. This paper was previously circulated under the title: "Do firm managers properly assess risks? Evidence from US firm proximity to hurricane strikes". All remaining errors are ours.

beliefs about risky outcomes are based on all available information and are formed regardless of context-specific factors. In practice, however, assessing risk is complicated and time-consuming. Because individuals have limited cognitive resources, psychologists argue that they may rely on heuristics, i.e., mental shortcuts that simplify the task of assessing probabilities (Tversky and Kahneman, 1973 and 1974) by focusing on "what first comes to mind" (Gennaioli and Shleifer, 2010). Under this alternative manner of assessing risk, all information is not given equal importance, which may lead people to make mistakes in their estimation that can have important consequences. In this paper, we ask whether managers also use such heuristic rules and investigate whether this practice affects corporate policies.

We focus on the "availability heuristic" rule. Tversky and Kahneman (1973 and 1974) show that people have a tendency to infer the frequency of an event from its availability, namely the ease with which concrete examples of a situation in which this event occurred come to mind. As the quote above suggests, the drawback of such a heuristic rule is that availability may also be affected by the salience of the event. For many reasons (e.g., a dramatic outcome or high levels of media coverage), certain events have unusual characteristics that stand in stark contrast with the rest of the environment. Because such events are more salient, they come to mind more easily. People using the availability heuristic will then overestimate the probability that these events will occur again. As shown by Bordalo, Gennaioli and Shleifer (2012b), such people behave as "local thinkers" who use only partial (i.e., salient) information to estimate probabilities. They overweight possible outcomes whose features draw their attention while neglecting others and thereby make incorrect inferences about the true probability of an event.

If corporate managers also use the availability heuristic, salient risk situations should lead them to overreact and make inappropriate decisions in terms of risk management. Specifically, we hypothesize that managers then overestimate the probability that the risk will materialize again and take excessive precautionary measures against it. Testing this hypothesis empirically gives rise to two major difficulties. First, the risk perceived by the manager cannot be directly observed. To address this problem, we focus on how managers estimate the risk of liquidity shock at the firm level and use the variations in corporate cash holdings to measure how their perception of this risk changes. Given the overwhelming evidence that corporate cash holdings are primarily used as a buffer against the risk of liquidity shortage, variations in cash holdings should provide a good indication of the changes in liquidity risk that are perceived by firm decision makers.[2]

Second, testing this hypothesis also requires the identification of a salient event whose occurrence does not convey any new information about the real distribution of its probability. For instance, the bankruptcy of Lehman Brothers in 2008 was a salient event that might have led bankers to reevaluate their *subjective* estimation of their risk exposure. However, this event is also likely to have affected the *objective* distribution of their risks.[3] It is therefore impossible to disentangle the part of their reactions caused by the increase in *subjective* risks from that caused by the increase in *objective* risks.

We address this problem here by using hurricanes as the source of liquidity shocks. Hurricanes are risks that are well suited for our purpose for three reasons. First, hurricane

[2] Froot et al. (1993) and Holstrom and Tirole (1998, 2000) provide a theoretical basis for predicting that cash will be used in imperfect financial markets as an insurance mechanism against the risk of liquidity shock. Empirically, several papers document a positive correlation among various possible sources of cash shortfall in the future and the current amount of cash holdings; these studies thus confirm that precautionary motives are central to accumulating cash reserves (e.g., Kim et al., 1998; Harford, 1999; Opler et al., 1999; Almeida et al., 2004; Bates et al., 2009, Acharya et al., 2012).

[3] See Shleifer and Vishny (2011) for an analysis of how Lehman Brothers bankruptcy affected banks' balance sheets and increased the risk of fires sales.

frequency is stationary (Elsner and Bossak, 2001; Pielke et al., 2008); thus, the occurrence of hurricane does not convey any information about the probability of a similar event occurring again in the future. Second, their occurrence is a salient event that is exogenous to firm or manager characteristics and represents a credible source of liquidity shock. Finally, hurricane events permit a difference-in-differences identification strategy because their salience is likely to decline as the distance from the disaster zone increases. This feature allows us to estimate the causal effect of risk saliency on the perceived risk by comparing how a treatment group of firms located in the neighborhood of the disaster zone and a control group of distant firms adjust their cash holdings after a disaster.

We find that managers respond to the sudden salience of liquidity risk caused by the proximity of a hurricane by increasing the amount of their firm cash holdings, although there is nothing to indicate that this risk is now bigger than it was. On average, during the 12-month period following the hurricane, firms located in the neighborhood area increase their cash holdings by 0.84 percentage points of total assets relative to firms farther away. We also find that this cash increase is temporary. The amount of cash increases sharply during the first three quarters following the disaster and then progressively returns to pre-hurricane levels over the next four quarters. Thus, as time passes, salience decreases, people forget the event, and the bias vanishes. This bias increases when managers are likely to be less sophisticated (i.e., managers of firms without previous experience of hurricane strikes in their neighborhood area, managers of small firms, and managers of young firms) and decreases when they have good reasons to care less about liquidity risks because their firms are not financially constrained.

We also find that this bias is costly for shareholders. First, we find that managers institute higher earnings retention to increase cash holdings. Second, using the methodology of Faulkender and Wang (2006), we find that the market value of cash decreases when firms are subject to this bias. The additional cash accrued in the balance sheet does not lead to a positive change in market capitalization, which suggests that it would most likely have been better employed otherwise.

We then discuss alternative non-behavioral explanations to our findings, such as the possibility of changes in risk, risk learning, and regional spillover. First, cash holdings could increase if the real probability of being hit by a hurricane increases or if managers ignore the risk and learn of its existence only when the hurricane occurs. However, both of these explanations would imply a permanent increase in cash holdings, which we do not find. Second, cash might increase temporarily because of regional externalities. For instance, the hurricane may temporarily create new business opportunities for firms in the neighborhood area. These firms would then make more profits and hold more cash. However, this type of spillover effect would imply a positive change in operating performance (sales, income), which we do not find. The hurricane might also locally increase business uncertainty for firms in the neighborhood area. These firms may then postpone investment and accumulate cash. However, this additional uncertainty should generate greater variance in revenues or increased volatility in stock returns, which we also do not find. To further alleviate the concern that these effects (or any other form of regional spillover effect) are driving our results, we perform two additional tests. First, we focus on all vulnerable firms (and not necessarily firms in the neighborhood of the affected region). Those firms may be far away from the disaster zone (e.g. firms located in the East coast when a hurricane hits Louisiana). Second, we focus on US firms exposed to earthquake risk and examine how they react to violent earthquakes that occur outside the US. In both situations, the distance to the disaster zone makes the possibility of regional spillover irrelevant. Nevertheless, our primary finding still holds. In both cases, cash holdings increase after the disaster.

Finally, we verify that holding more cash protects firm revenues better in the case of a hurricane. Indeed, if managers respond to the salience of hurricane risk by increasing cash holdings, then we would expect that holding cash helps to reduce firm losses when this risk materializes. We test this prediction and examine how firms located in disaster areas perform in terms of revenue after the disaster depending on the level of their cash holdings before the hurricane. We find that firms that hold more cash perform better and recover much faster than other firms. This finding explains why managers are willing to increase cash holdings when they perceive that the risk of a hurricane strike is higher.

Our paper shows that managers are prone to use the availability heuristic to assess risk, which affects firm value by reducing the value of cash. As such, this study contributes first to the literature on behavioral corporate finance. Baker and Wurgler (2012) organize this literature around two sets of contributions: "irrational investors" and "irrational managers". Our paper is related to the "irrational managers" strand of the literature, which primarily focuses on how overconfidence and optimism can affect both investment and financing decisions (Malmendier and Tate, 2005; Hirshleifer, Low and Teoh, 2012; Landier and Thesmar, 2009). More recently, this literature has begun to study the effects of bounded rationality (Brav et al., 2005), such as reference point thinking (Baker, Pan and Wurgler, 2012; Baker and Xuan, 2011; Loughram and Ritter, 2002; Ljungqvist and Wilhelm, 2005; Dougal et al., 2011).

Next, our results are related to the growing literature that focuses on the effects of individual traits and past experiences on investors' decisions (Malmendier and Nagel, 2011; Malmendier and Nagel, 2013; Kaustia and KnÃ$\frac{1}{4}$pfer, 2008; Choi et al., 2009; Greenwood and Nagel, 2009). Because saliency is experienced-based, our paper complements this literature and shows that irrelevant contextual factors also influence firm decision makers.[4]

Finally and more generally, our paper contributes to the vast literature on the effects of behavioral biases "in the field". *A priori*, managers may act rationally because they are neither unsophisticated agents nor students in a lab with no real economic environment. Therefore, as noted by Levitt and List (2007), we should expect managers not to be affected by behavioral biases. Whether they rely on the availability heuristic to make financial decisions is thus an open question and to the best of our knowledge, this paper is the first to empirically show that managers use the availability heuristic to assess risk and the first to study its effects.

The rest of the paper is organized as follows. Section 2 briefly summarizes what is known about hurricane risk. Section 3 proposes hypotheses based on the availability heuristic phenomenon and reviews the related scientific and anecdotal evidence. Section 4 presents our empirical design. Section 5 provides evidence about whether managers overreact to salient risks. Section 6 investigates whether this reaction is costly. Section 7 discusses the possibility of alternative non-behavioral explanations. Section 8 examines the effects of cash holdings on post-hurricane performance. Section 9 concludes.

[4] Another strand of research examines how salience affects investors' attention. This literature shows that investors pay more attention to salient information (Hirshleifer, Hou, Teoh and Zhang, 2004; Barber and Odean, 2008; Hirshleifer, Lim, and Teoh, 2009 and 2011), which affects stock prices (Ho and Michaely, 1988; Klibanoff, Lamont, and Wizman, 1998; Huberman and Regev, 2001) and creates incentives for strategic information disclosure (Hirshleifer and Teoh, 2003).

1.2 Hurricane activity on the US mainland

Hurricanes are tropical cyclones that form in the waters of the Atlantic and eastern Pacific oceans with winds that exceed 32 m per second (approximately 72 miles per hour). In this section, we briefly summarize what is known about the risk of hurricanes in the US and why it is justified to use such a risk for our experiment. We highlight that hurricane risk can randomly affect an extensive number of firms throughout the US territory, is impossible to predict accurately, has not changed over time and should remain unchanged in the coming decades in terms of both volume (frequency) and value (normalized economic cost).

1.2.1 Event location

Hurricanes can randomly affect a large fraction of the US territory. Coastal regions from Texas to Maine are the main areas at risk. An extensive inland area can also be affected, either by floods resulting from the heavy rainfalls accompanying hurricanes or by the high winds produced by the hurricane as it moves across land. In the SHELDUS database (the main database for natural disasters in the US), 1,341 distinct counties (approximately 44% of the total counties in the US) are reported to have been affected at least once by a major hurricane. Figures 1 through 4 show on a map examples of disaster areas for hurricanes Fran, Floyd, Allison, and Katrina.

INSERT FIGURES 1 TO 4 AROUND HERE

1.2.2 Event frequency

Hurricanes are regular events in the US. Since 1850, an average of 2 hurricanes strike the US mainland every year.

INSERT FIGURES 5 AROUND HERE

Figure 5 suggests no particular increasing or decreasing trend in this frequency. This absence of a trend is supported by the climatology literature (e.g. Elsner and Bossak, 2001; Pielke, 2005; Landsea, 2005; Emanuel, 2005; Landsea, 2007, Pielke et al, 2008; Blake et al., 2011). In the US, Elsner and Bossak (2001) find that the distribution of hurricane strikes have been stationary since early industrial times for all hurricanes and major hurricanes as well as for regional activity.[5] Regarding possible future changes in storm frequencies, Pielke et al. (2008) conclude in their survey that given "*the state of current understanding (...) we should expect hurricane frequencies (...) to have a great deal of year-to-year and decade-to-decade variation as has been observed over the past decades and longer.*"[6]

1.2.3 Event cost

The total cost of hurricane strikes in terms of economic damages is now much larger than it was at the beginning of the past century (Blake, Landsea and Gibney, 2011). However, after normalizing hurricane-related damage for inflation, coastal population and wealth,

[5] "*The distributions of hurricanes during each [time] subinterval are indistinguishable, indicating a stationary record of hurricanes since early industrial times. Stationarity is found for all hurricanes and major hurricanes as well as for regional activity*" (p. 4349)

[6] In section 7, we discuss how possible change in the frequency of hurricane strikes in the US could affect the interpretation of our results. Further analyses on the likelihood of hurricane disaster at the county level are also documented in section 7. In particular, we show that the proximity of a hurricane disaster reveals no information about future hurricane likelihood in a given county.

no trend of increasing damage appears in the data. For instance, Pielke et al. (2008) find that had the great 1926 Miami hurricane occurred in 2005, it would have been almost twice as costly as Hurricane Katrina; thus, they stress that "*Hurricane Katrina is not outside the range of normalized estimates for past storms.*" Overall, their results indicate that the normalized economic cost of hurricane events has not changed over time, consistent with the absence of trends in hurricane frequency and intensity observed over the last century.

1.2.4 Event anticipation

Global tropical storm activity partly depends on climatic conditions that are predictable on seasonal time scales. However, the exact time, location and intensity of future hurricane strikes are *"largely determined by weather patterns in place as the hurricane approaches, which are only predictable when the storm is within several days of making landfall*".[7] Therefore, hurricane disasters in the US mainland are uncertain events that are very difficult to anticipate. Such events *"can occur whether the season is active or relatively quiet*", and in many instances come as a surprise to the local population.[8]

1.3 The psychological mechanisms for probability evaluation and risk assessment

1.3.1 The availability heuristic

Because assessing the likelihood of uncertain events is a complex and time-consuming task, people naturally tend to use their own experiences for developing simple mental rules

[7] See National Oceanic and Atmospheric Administration (NOAA) website

[8] See NOAA website.

to rapidly adjust their beliefs and adapt to their environment. Tversky and Kahneman (1973, 1974) describe such heuristic rules and show that, although useful in general, they sometimes lead people to make mistakes. One such rule is the "availability heuristic", which derives from the common experience that "frequent events are much easier to recall or imagine than infrequent ones." Therefore, when judging the probability of an event, most people assess how easy it is to imagine an example of a situation in which this event actually occurred. For example, people may assess the probability of a traffic accident by recalling examples of such occurrences among their acquaintances.

Tversky and Kahneman (1973, 1974) show that the use of this rule is problematic because availability may also be affected by factors that are not related to actual frequency. In particular, they argue that factors such as familiarity with the event, the salience of the event, the time proximity of the event and/or the preoccupation for the event's outcome can affect its availability and generate a discrepancy between subjective probability and actual likelihood. The availability of a car accident, for instance, will be higher when the person involved in the accident is famous (familiarity), if the accident was observed in real time (salience), if the accident occurred recently (time proximity), or if the physical pain caused by the injuries resulting from traffic accidents has been recently "vividly portrayed" (preoccupation with the outcome). In all these cases described above, the subjective probability of a car accident will then be temporarily higher than its actual likelihood.

1.3.2 The availability heuristic

The availability heuristic theory is consistent with anecdotal and scientific evidence. In a series of studies by Lichtenstein et al. (1978), people were asked to estimate the frequency of several dozen causes of death in the United States. The results from this study show that salient causes that killed many people during a single occurrence were overestimated, whereas less salient causes were systematically underestimated. In a survey conducted to understand how people insure themselves against natural hazards, Kunreuther et al. (1978) observe a strong increase in the number of people willing to buy insurance at a premium immediately after an earthquake. Conversely, people were found to be reluctant to buy such insurance even at a subsidized rate in the absence of a recent major earthquake. Johnson et al. (1993) also find that people are willing to pay more than two times the amount for the same insurance product in situations in which the risk is salient compared to situations in which it is not, confirming that saliency increases perceived risk.[9]

To account for such empirical findings, Bordalo, Gennaioli, and Shleifer (2012b, 2013b) develop a theoretical framework of choice under risk in which salient attributes grab individuals' attention. In their model, individuals do not equally consider the full set of possible states of the world when it comes to assessing risk. They neglect non-salient states, and over-emphasize the salient ones. Because the salience of a state depends on contextual factors, individuals then make context-dependent risk estimations. When a good state is salient, they over-estimate the likelihood of a positive outcome and take too

[9]Other similar results can be found in the housing literature, in which changes in housing prices can be used to infer changes in perceived risk. This literature shows that the occurrence of a salient event (e.g., floods, earthquakes, nuclear accidents, etc.) systematically results in a decrease in property prices that is larger than the value of the insurance premium (see, for instance, MacDonald et al., 1990; Bin et al., 2004, 2008; Kousky, 2010)

much risk. When a bad state is salient, they over-estimate the probability of a negative outcome and are excessively risk averse. In both cases, individuals overreact to salient risks.[10]

1.3.3 Implications and hypothesis development

In this paper, we focus on decision makers in firms. We ask whether they rely on the availability heuristic to assess risk and examine whether they overreact to salient risks (hereinafter, the *availability heuristic* hypothesis). Firm decision makers are neither uninformed, unsophisticated agents (such as home owners or property insurance retail buyers), nor are they undergraduate students in an experiment conducted outside of a real economic environment.[11] Whether managers will make incorrect financial decisions in the real world because of the availability heuristic therefore largely remains an open question.

One challenge is that we cannot directly observe the risk perceived by firm managers. To address this difficulty, we assume that changes in risk perception can be inferred from variations in corporate cash holdings. There is indeed strong theoretical and empirical evidence in the corporate finance literature that the main driver of policies regarding cash holdings is risk management. Froot et al. (1993) and Holstrom and Tirole (1998, 2000) provide a theoretical basis for predicting that cash will be used as an insurance mechanism against the risk of a liquidity shock in imperfect financial markets because firms have limited access to external financing. In this context, cash holdings offer a

[10]Other models based on the mechanism of salience include Bordalo, Gennaioli and Shleifer (2012a, 2013a), Gabaix (2011), Gennaioli and Shleifer (2010), KÃ¶szegi and Szeidl (2013), and Schwartzstein (2009). These models share the common assumption that individuals do not consider the whole set of available information before making a decision and neglect part of it. Significant judgment errors then occur when the neglected data are relevant for decision making.

[11]Levitt and List (2007) discuss the limitations of lab experiments and explain why economic agents may evolve toward more rational behaviors when placed in a familiar environment.

buffer against any risk of cash shortage that would prevent firms from financing positive Net Present Value (NPV) projects. Consistent with this argument, several empirical papers document a positive correlation among various possible sources of cash shortfalls for future and current levels of cash holdings (Kim et al., 1998; Harford, 1999; Opler et al., 1999; Almeida et al., 2004; Bates et al., 2009; Ramirez and Altay, 2011; Acharya et al., 2012). Surveys of CFOs also confirm this link. For instance, Lins et al. (2010) find that a sizeable majority of CFOs indicate that they use cash holdings for general insurance purposes.

If managers rely on the availability heuristic to assess the risk of an event that would trigger a cash shortage, cash holdings should then vary in response to the salience of this event. Under the *availability heuristic* hypothesis, we thus argue that corporate cash holdings will increase in those situations in which the risk of cash shortage becomes more salient.

Because firms are not identical to one another, the effect of event saliency on corporate cash holdings may vary in the cross section of the population. A primary source of heterogeneity is the level of managerial sophistication; sophisticated agents are expected to be less affected by behavioral biases. Therefore, changes in cash holdings for firms with sophisticated managers should be less sensitive to event saliency. Another source of heterogeneity is the level of financial constraints. Managers of less financially constrained firms should be less concerned about potential liquidity shocks. Therefore, changes in cash holdings for unconstrained firms should be less sensitive to event saliency. Another source of heterogeneity consists of firms' vulnerability to hurricane disasters. Indeed, not all industries are similarly affected by hurricane events. Certain industries may suffer higher losses, perhaps because they are more difficult to insure or because they are more dependent on the local economy. Changes in cash holdings should be more sensitive to event saliency for firms that operate in such vulnerable industries.

1.4 Empirical design

1.4.1 Identification strategy

In this paper, we use both the occurrence of hurricanes and the proximity of the firm to the disaster area to identify situations in which the risk of liquidity shocks becomes salient. Our motivation for the use of hurricanes relies on the following arguments. First, hurricanes can trigger liquidity shocks because of the heavy damage they can inflict.[12] Although firms might buy insurance to cover this risk, direct insurance is unlikely to cover all type of indirect losses. In addition, Froot (2001) shows that hurricane insurance is overpriced.[13] Thus, firms should prefer to self-insure by accumulating cash reserves instead of directly insuring this liquidity risk. Second, the occurrence of hurricanes is a salient event because hurricanes draw people's attention and leave their marks on observers' minds. Third, this saliency effect is likely to vary with the proximity of the landfall. Indeed, we expect the event to be salient for managers whose family members and friends are directly affected by the disaster, which is likely to occur for firms located

[12] Cash shortages can come in many ways, including reinvestment needs caused by the partial destruction of operating assets (headquarters, plants, equipment, etc.), a drop in earnings because of a drop in local demand, or new investment financing needs caused by unexpected growth opportunities (reconstruction opportunities, acquisition of a local competitor, etc.).

[13] Froot (2001) shows that hurricane insurance is in short supply because of the market power enjoyed by the small number of catastrophe reinsurers. As a result, insurance premiums are much higher than the value of expected losses. Garmaise and Moskowitz (2009) provide evidence that such inefficiencies in the hurricane insurance market lead to partial coverage of this risk at the firm level, which hurts bank financing and firm investment.

in the disaster area and the environs nearby (referred to herein as the neighborhood) but not for more distant firms. The hurricane event should also receive more attention in situations in which firms are at risk, which again is more likely to occur when firms are located in the neighborhood of the disaster area. Fourth, the occurrence of a hurricane makes hurricane risk salient but does not imply a change in the risk itself. The distribution of hurricanes is stationary; therefore, there is no reason to believe that the real risk of hurricane landfall changes after its occurrence. Finally, hurricanes are exogenous events that can randomly affect a large number of firms. A firm's distance from hurricane landfalls thus offers an ideal natural experiment framework to test for the presence of a causal link between event saliency and managers' risk perception through changes in corporate cash holdings.

1.4.2 Data

We obtain the names, dates and locations of the main hurricane landfalls in the US from the SHELDUS (Spatial Hazard and Loss Database for the United States) database at the University of South Carolina. This database provides the location for each disaster at the county level for all major hurricanes since the early 1960s. In SHELDUS, a county is reported as an affected county whenever the hurricane event and the subsequent rainfalls cause monetary or human losses. To ensure that the event is sufficiently salient, we focus on hurricanes with total direct damages (adjusted for CPI) above five billion dollars. We also restrict the list to hurricanes that occurred after 1985 because there are no financial data available from Compustat Quarterly before that date. This selection procedure leaves us with 15 hurricanes between 1989 and 2008.[14]

We obtain detailed information about their characteristics (start date, end date, date of landfall, direct number of deaths, total damage, and category) from the tropical storm reports available in the archive section of the National Hurricane Center website and from the 2011 National Oceanic and Atmospheric Administration (NOAA) Technical Memorandum. Table 1 presents summary statistics for these 15 hurricanes.

[INSERT TABLE 1 AROUND HERE]

We obtain financial data and information about firm headquarters location from Compustat's North America Fundamentals Quarterly database.[15] We use headquarters rather than plants or clients' location to identify the location of the firm because our objective is to study managers' risk perception, which requires knowing where the decision makers are. Quarterly data rather than annual data are used to identify changes in cash holdings in firms near hurricane landfalls with the highest possible precision.[16]

We restrict our sample to non-financial and non-utility firms whose headquarters are located in the US over the 1987-2011 period. If the county location of a firm's headquarters is missing or if the fiscal year-end month is not a calendar quarter-end month (i.e., March, June, September or December), the firm is removed from the sample. This selection procedure leaves us with a firm-quarter panel dataset of 11,948 firms and 411,490 observations. In Panel A of Table 2, we present summary statistics for the main firm-level variables we use. All variables are winsorized at the first and 99th percentile

[14] We obtain the same results when using all hurricanes from the SHELDUS database. Our results also remain unchanged when we remove the largest hurricanes (e.g. Katrina).

[15] One possible concern with location data is that Compustat only reports the current county of firms' headquarters. However, Pirinsky and Wang (2006) show that in the period 1992-1997, less than 3% of firms in Compustat changed their headquarter locations.

[16] We obtain the same results with annual financial data (See Internet appendix).

and are defined in Appendix 1.

[INSERT TABLE 2 AROUND HERE]

1.4.3 Assignment to treatment and control groups

We measure the degree of salience of each hurricane event according to the distance between the firm's headquarters and the landfall area. For this purpose, we define three different geographic perimeters that correspond to various distances from the landfall area: the *disaster zone*, the *neighborhood* area, and the *rest of the US mainland*. The *disaster zone* includes all counties affected by the hurricane according to the SHELDUS database. The *neighborhood* area is obtained through a matching procedure between affected counties and non-affected counties according to geographical distance. Under this procedure, we first assign a latitude and longitude to each county using the average latitude and average longitude of all the cities located in the county. For each affected county, we next compute the distance in miles to every non-affected county using the Haversine formula.[17] We then match with replacement each affected county with its five nearest neighbors among the non-affected counties.[18] This procedure leaves us with a set of matched counties that constitute our neighborhood area and a set of non-matched counties that form the rest of the US mainland area. Figures 1 to 4 present the results of this identification procedure on a map for hurricanes Fran, Floyd, Allison and Katrina.

[INSERT FIGURES 1 TO 4 AROUND HERE]

[17] The Harversine formula gives the distance between two points on a sphere from their longitudes and latitudes.

[18] We find that on average, a county has approximately five adjacent counties. Our results remain the same when we use three or four rather than five nearest non-affected counties.

Firms located in the *neighborhood* area (represented by the light blue zone on the map) are assigned to the treatment group because the hurricane landfall should be a salient event for the managers of such firms. Given their proximity to the disaster zone, the hurricane is indeed a near-miss event, meaning that they could have been affected by the hurricane but were not by chance. For that reason, we expect the event to raise firm managers' attention. Firms located in the *rest of the US mainland* (the blank zone on the map) are assigned to the control group. Given their distance from the landfall area, the hurricane should not be a salient event for the managers of these firms. Some of these managers may even completely ignore the event if they are located in an area in which the risk of a hurricane strike is not of concern. Firms located in the *disaster zone* (the dark blue zone on the map) are separated in our analysis because of the direct effects of the hurricane on their cash levels. Given their location, these firms are affected by the disaster. The event is not only obviously salient for their managers but is also a potential source of direct cash outflow (e.g., replacement costs of destroyed operating assets) or cash inflow (e.g., receipt of the proceeds of insurance claims). The variation of cash holdings surrounding the hurricane event is thus more likely to reflect the direct effects of the disaster rather than the change in managerial perceived risk. In practice, we do not remove these firms from our sample.[19] Instead, we control to ensure that the variation of cash holdings that we observe when these firms are affected by the hurricane does not influence our results. Panel B of Table 2 presents summary statistics for each group of firms.

[19]In fact, we cannot exclude these firms because these firms can also be in the neighborhood of another hurricane at another point in time. Because we are considering various hurricane strikes over time, it is possible that the same firm may be in each of the three groups defined in our experiment (*disaster zone*, *neighborhood*, and *the rest of the US mainland*).

[INSERT TABLE 2 AROUND HERE]

The statistics are mean values computed one quarter before a hurricane's occurrence. The last column shows the t-statistic from a two-sample test for equality of means across treated and control firms. Treatment firms and control firms appear to be similar along various dimensions, including the amount of cash holdings.

1.4.4 Methodology

We examine the effect of the hurricane saliency on managers' risk perception through changes in the levels of corporate cash holdings using a difference-in-differences estimation. The basic regression we estimate is

$$Cash_{ict} = \alpha_i + \delta_t + \gamma\ X_{itc} + \beta\ Neighbor_{tc} + \epsilon_{itc} \qquad (1.1)$$

where i indexes firm, t indexes time, c indexes county location, $Cash_{ict}$ is the amount of cash as a percentage of total assets at the end of the quarter, α_i are firm fixed effects, δ_t are time fixed effects, X_{itc} are control variables, $Neighbor_{tc}$ is a dummy variable that equals one if the county location of the firm is in the neighborhood of an area hit by a hurricane over the last 12 months and zero if not, and ϵ_{itc} is the error term that we cluster at the county level to account for potential serial correlations (Bertrand, Duflo and Mullainathan, 2004).[20]

Firm fixed effects control for time invariant differences among firms (which include fixed differences between treatment and control firms). Time (year-quarter) fixed effects

[20] Allowing for correlated error terms at the state level or firm level leads to similar inferences in the statistical significance of regression coefficients.

control for differences between time periods, such as aggregate shocks and common trends.

The other variables, X_{itc}, systematically include a dummy variable $DisasterZone_{ct}$ to capture the effect of the hurricane strike when the firm is located in the disaster zone. This $DisasterZone_{ct}$ variable enables the comparison of firms in the neighborhood area with firms farther away (the rest of the US mainland) by isolating the changes in cash holdings observed when firms are located in the disaster zone from the rest of our estimation.[21] Our estimate of the effect of hurricane landfall proximity is β, which is our main coefficient of interest. It measures the change in the level of cash holdings after a hurricane event for firms in the neighborhood of the disaster area relative to a control group of more distant firms.

1.5 Do managers overreact to salient risks?

1.5.1 Main results

We examine the effect of the event availability on the risk perceived by firm managers through differences in corporate cash holdings after a hurricane landfall. Tables 3 and 4 present our main results.

[INSERT TABLE 3 AROUND HERE]

Table 3 reports the effects of being in the neighborhood of a disaster area in the 12 months after a hurricane. Column 1 shows that, on average, firms located in the neighborhood of a disaster zone increase their cash holdings (as % of total assets) by

[21] When firms are located in the disaster area, changes in cash holdings are likely to be caused by the direct effects of the hurricane.

0.84 percentage points during the four quarters following the hurricane event. This effect represents an average increase in cash holdings of 16 million dollars in absolute terms and accounts for 8% of the within-firm standard deviation of cash holdings.

We investigate the robustness of this effect in the rest of Table 3. First, our results may capture within-year seasonality. Because hurricane activity is seasonal, firms in the neighborhood area might anticipate the possibility of hurricane strikes and hold more cash at the end of the third quarter of the year. We control for this possibility by using firm-calendar quarter fixed effects (i.e. four quarter fixed effects for each firm) rather than firm fixed effects. Second, our result might be driven by industry-specific shocks. Thus, we use year-quarter-SIC3 fixed effects rather than year-quarter fixed effects to remove any time varying unobserved heterogeneity across industries (Gormley and Matsa, 2013). Column 3 shows that the inclusion of these two high-dimension fixed effects does not alter our estimation.[22] In fact, the magnitude of the effect of hurricane proximity on cash holdings remains exactly the same. In column 3, we show that this effect is robust to the inclusion of firm-specific controls: age, size and market-to-book. Because such controls might be endogenous to the proximity of a hurricane disaster, we do not include them in our basic specification.[23] Similar to Bertrand and Mullanaithan (2003), we prefer to verify that our findings are not modified by their inclusion.[24] Overall, the effect is extremely robust to the different specifications, and the magnitude of the coefficient is always the same. Consistent with the *availability heuristic* hypothesis, managers respond

[22]See Guimaraes and Portugal (2010) for a simple procedure to estimate models with two high-dimension fixed-effects.

[23]See Roberts and Whited (2012) for a discussion about the effect of including covariates as controls when they are potentially affected by the treatment.

[24]Similarly, this result does not change when other control variables frequently associated in the literature with the level of cash holdings are added, such as capital structure, working capital requirements, capital expenditures, or R&D expenses.

to the sudden salience of danger by increasing their firm cash holdings, although there is no indication that the danger is bigger now than it was.

[INSERT TABLE 4 AROUND HERE]

In Table 4, we examine how the effect of hurricane proximity on cash holdings changes over time. Specifically, we study the difference in the level of cash holdings between treated and control firms at different points in time before and after hurricane landfall. To do so, we replace the $Neighbor$ variable with a set of dummy variables, $Neighbor^q(i)$, that captures the effect of the saliency of the event at the end of every quarter surrounding the hurricane. For each quarter i $(-i)$ after (before) the hurricane, we create a variable, $Neighbor^q + i$, that is equal to one if the county location of the firm headquarters at the end of the quarter was in the neighborhood of an area hit by a hurricane during quarter q0 and zero otherwise. The regression coefficient estimated for this dummy variable then measures the difference-in-differences in the level of cash holdings i $(-i)$ quarters after (before) the disaster. We undertake the same procedure for the $DisasterZone$ variable. This approach allows us to identify when the effect starts and how long it lasts. Column 1 of Table 4 shows that no statistically significant change in cash holdings appears before the hurricane event for firms located in the neighborhood area. However, consistent with a causal interpretation of our result, we do find that the amount of cash begins to increase following the occurrence of the hurricane.[25] This effect increases during the subsequent three quarters, and the increases in cash holdings reach their maximum during $q+2$ and $q+3$, which is when the following annual hurricane season begins and becomes active. On

[25]The positive and statistically significant effect for $Neighbor^q0$ does not contradict our interpretation. Indeed, $q0$ is the first balance sheet published after the event and therefore shows the change in cash that occurs in reaction to the hurricane.

average, hurricanes from our sample occur by mid-September. The next annual hurricane season starts around mid-June (i.e. after $q+2$ and before $q+3$). The coefficient for the $Neighbor^q+2$ and $Neighbor^q+3$ variables show that, on average, firms located in the neighborhood area respond to the saliency of the disaster by increasing their cash levels by 1.15 and 1.13 percentage points of their total assets (approximately 20 million dollars and approximately 11% of the within-firm standard deviation of *cash*) at the end of the second and third quarters after the hurricane, respectively. The level of cash holdings then begins to decrease, and the effect progressively vanishes over the next three quarters. The coefficient for the $Neighbor^q+8$ variable shows that the average difference in cash holdings between firms in the neighborhood area and control firms is not statistically different from zero two years after the hurricane landfall.

This drop in the amount of cash holdings is consistent with our behavioral interpretation. As time goes by, memories fade, the salience of the event decreases, and the subjective probability of risk retreats to its initial value. Managers then reduce the level of corporate cash holdings.

[INSERT FIGURE 6 AROUND HERE]

We plot the result of this analysis in a graph in which we also display the evolution of the difference in corporate cash holdings between firms located in the disaster zone and control firms. This graph is presented in Figure 6. While firms in the neighborhood area experience a temporary increase in cash holdings, firms hit by the hurricane display a symmetric decrease. This "reversed mirror" trend is notable for two reasons. First, it confirms that the occurrence of a hurricane can trigger a liquidity shock, as firms hit by a hurricane experience a significant drop of 0.6 percentage points in their cash holdings.

Second, it offers an indication of the magnitude of the increase in cash observed when firms are located in the neighborhood area. Indeed, the graph demonstrates that the additional amount of cash accrued in the balance sheet (+1.1 percentage points of total assets), presumably for insurance purposes against the risk of cash shortages after a hurricane strike, exceeds the actual loss of cash (-0.6 percentage points) that firms experience when this risk materializes. Thus, even if the increase in cash holdings observed for firms in the neighborhood area was justified, the magnitude of this increase would be excessive compared to the real loss of cash at risk.

However, we do recognize that the loss of cash (-0.6%) we observe here may not correspond to the real economic cost of the hurricane. We address this issue in Section 7 when we examine market reaction at the time of landfall. We find that the present value of losses caused by the disaster represents 1.03% of the total assets of the firm, on average, which remains lower than the increase in cash observed in firms located in the neighborhood area (+1.1%). This last result is useful to determine whether managers overreact to the salience of hurricane risk, or if alternatively they properly take hurricane risk into account only when a disaster occurs and neglect this risk in normal times. Here, we cannot (and do not) rule out the possibility of risk neglect in normal times. However, we can rule out the possibility that managers correctly adjust cash holdings when a disaster occurs. Indeed, the magnitude of the increase in cash compared to the value of losses suggests that managers overshoot and increase cash holdings too much, which is more consistent with an overreaction-based explanation.

1.5.2 Cross sectional variation in managers' responses

Because firms have different characteristics, they may not respond in the same way to the salience of hurricane risk. We first investigate whether this response changes with the degree of sophistication of firm decision makers. Our primary proxy for sophistication is the experience of a firm's managers in terms of hurricane proximity. Indeed, we expect managers to learn from past experiences and to be less sensitive to danger saliency if they have previously been "fooled". In practice, we count the number of instances in which a firm has been located in the neighborhood area during previous hurricane events. We then split our sample into three categories of sophistication (low, medium, and high). Firms are assigned to the low (medium or high) sophistication category if their headquarters were never (once or more than once, respectively) located in the neighborhood area during a prior hurricane event.

To complement this analysis, we also use two more indirect proxies for sophistication: firm size and age. We use firm's size because we expect large firms to be run by sophisticated CEOs and CFOs (e.g. Krueger Landier and Thesmar, 2011). We use the age of the firm because various studies in the behavioral literature show that young age is more associated with behavioral biases (Greenwood and Nagel, 2009; or Malmendier and Nagel, 2011). Each period, we split our sample into terciles of firm size and terciles of firm age, and we assign firms to the high, medium, or low sophistication category if they belong to the high, medium, or low tercile of the distribution, respectively.

For each criterion (experience, size, and age), we define three dummy variables corresponding to each sophistication category (e.g., *Low Sophistication*, *Medium Sophistication*, *High Sophistication*). We then interact each dummy variable with the *Neighbor* variable to investigate how the response to the salience of hurricane risk varies with the degree of managerial sophistication.

[INSERT TABLE 5 AROUND HERE]

Columns 1 to 3 of Table 5 indicate that a low degree of sophistication systematically leads to a strong increase in the amount of cash holdings. Conversely, we find no statistically significant change in cash holdings for firms whose managers are likely to be more sophisticated. In all three cases, an F-test indicates that the difference between the two coefficients (high vs. low) is statistically significant at the 1% or 5% level. Overall, the results of table 5 are consistent with our availability heuristic hypothesis, which predicts that sophisticated managers should react less to salient risks. The effect of managers' experience on how neighboring firms react to the event (column 1) is also notable because it mitigates the concern that our main finding is driven by possible regional spillover effects between the disaster area and the neighborhood area. As further discussed in section 7, corporate cash holdings may increase temporarily in the neighborhood area because of possible connections between the neighboring firms and the local economy shocked by the natural disaster. However, this explanation implies that a temporary increase in cash should *consistently* be observed after each hurricane event, which is not what column 1 suggests. Indeed, column 1 indicates that as managers accumulate experience because the same event repeats, this temporary increase in cash holdings tends to be weaker.

In the Internet Appendix, we further investigate how this response varies in the cross section of firm population. First we find that managers of firms located in the neighborhood area have a stronger response to the salience of liquidity risk when their firms are more financially constrained. Second, we show that firms in the neighborhood area also respond more strongly when their firm is more vulnerable to a hurricane disaster. Specifically, the amount of corporate cash holdings increases more when a firm operates in an industry that suffers higher losses in the case of hurricane disaster, when firms operations mainly rely on intangible assets that are more difficult to insure, and when firms are less diversified geographically.

The last set of results indicates that our effect is concentrated on neighboring firms for which hurricane risk is very relevant. By contrast, firms that are less vulnerable to this specific liquidity risk react less even though their managers are exposed to the same traumatic event. This finding casts doubt on the possibility of a general fear-based reaction. Indeed, the hurricane disaster could modify managers' preferences and temporarily increase their risk aversion (Guiso, Sapienza and Zingales, 2013). However, in this fear-based story, all managers exposed to the same traumatic situation should react in the similar way, which is not what we find.

1.5.3 Robustness and validity check

Our main source of concern is the slight heterogeneity between treated firms and control firms. Although these firms are fairly comparable along various dimensions, Table 2 indicates that some differences exist in terms of age and dividends. To ensure that our results are not driven by this heterogeneity, we combine our difference-in-differences approach with a matching approach. We match on SIC3 industry, size, age, market-to-book, financial leverage, working capital requirements, capital expenditures, and dividends. The results of this analysis as well as a detailed description of our matching procedure are presented in the Internet Appendix. Overall, this analysis leads to the same conclusion as the one obtained with the simple difference-in-differences approach: firms located in the neighborhood area temporarily increase their level of cash holdings after the hurricane.

To ensure that this result is both valid and robust, we also conduct a series of additional tests that are described and reported in the Internet Appendix. In particular, we run a placebo test in which we randomly change the dates of hurricanes to ensure that our results are driven by hurricane landfalls. We also re-run our main regression in many different ways to verify that our effect is robust to alternative specifications. Finally, we verify that our effect is not driven by the manner in which we scale corporate cash holdings. Thus, we re-run the main regression using firm size (total assets) as the dependent variable and find nothing.

1.6 Is managers' reaction costly?

Because the liquidity risk remains unchanged, managers' decisions to temporarily increase cash holdings after a hurricane event are likely to be suboptimal in terms of resource allocation. In this section, we examine whether this temporary increase in cash is costly for shareholders. We begin by analyzing the counterparts to this cash increase. Next, we study whether this response to risk saliency negatively impacts firm value by reducing the value of cash.

1.6.1 Source of cash

The cash increase observed after the hurricane landfall may come from a variety of sources: an increase in revenues (*Sales Growth* variable) and operating profits (*EBIT Margin* variable), a drop in net working capital requirements (*NWC* variable), a drop in investments

(*Net investment* variable), a decrease in repurchases (*Repurchases variable*), a reduction of dividends (*Dividend* variable), or an increase in new financing (debt or equity) (*New financing* variable). Because total assets include the amount of cash holdings, we do not normalize these items by total assets and instead use the amount of sales (unless the literature suggests another more relevant normalization method). Next, we replicate our difference-in-differences analysis and apply our basic specification to each item separately.[26] The results of this analysis are reported in Table 6.

[INSERT TABLE 6 AROUND HERE]

We begin by examining whether hurricanes affect operating activity. Column 1 shows that, on average, the occurrence of a hurricane has no significant effect on revenues for firms located in the neighborhood area of the disaster zone. While sales growth decreases by 2.4 percentage points relative to the control group for firms hit by the hurricane, we find no evidence that the relative sales growth for neighborhood firms is affected by the proximity of the disaster. Column 2 confirms that neighborhood firms are truly unaffected in terms of operating activity. Unlike firms in the disaster zone, firms located in the neighborhood area suffer no significant decrease in operating margin (the coefficient on the $Neighbor$ variable is not statistically different from zero).

In the rest of Table 6, we examine other possible channels through which the change in cash holdings may occur. We find no evidence that the proximity of the hurricane modifies either the investment activity (columns 3 and 4) or the financing activity (column 7). All coefficients have the expected sign and go in the direction of an increase in cash, but none is statistically significant. We also find no evidence that neighborhood

[26] We include firm-quarter fixed effects rather than firm fixed effects in the specification to adjust for within-year seasonality. Using firm fixed effects leads to the same results.

firms reduce the amount of repurchases after the hurricane (column 5). The sign of the coefficient is negative, but again, it is not statistically significant. However, we find that the proximity of the disaster changes payout policies. Indeed, column 6 indicates that firms in the neighborhood area tend to pay lower dividends and retain more earnings after the hurricane (the coefficient on the Neighbor variable is negative and statistically significant at the 5% level). The economic magnitude of the coefficient is low compared to the increase in cash. One possible interpretation is that managers also marginally adjust all sources of cash inflow. This would explain why all other coefficients have the right sign but turn out insignificant.

In columns 8, 9 and 10, we further investigate whether hurricanes affect the payout policy or the financing policy. We use a linear probability model to assess whether hurricane landfalls affect the likelihood of stock repurchases, dividend payment, and new financing issues. In column 8, we find that the likelihood of a stock repurchase is lower in the case of hurricane proximity. Similarly, column 9 indicates a decrease in the probability of dividend payment. However, we find no change in the probability of new security issues in column 10.

Overall, these results suggest that, when located in the neighborhood area of a disaster zone, firm managers increase earnings retention and probably, also marginally adjust all other sources of cash inflow.

1.6.2 Value of cash

We next investigate whether this change in cash holdings is an efficient decision or a source of value destruction for shareholders. If it is an efficient decision, the increase in cash holdings should translate into a similar increase in value for firm shareholders. If by contrast, cash would have been better employed otherwise, the additional cash accrued in the balance sheet should be discounted and will not result in a similar increase in terms of market capitalization. In our tests, we follow the literature on the value of cash (Faulkender and Wang, 2006; Dittmar and Mahrt-Smith, 2007; Denis and Sibilkov, 2010). We examine how a change in cash holdings leads to a change in market valuation for firms in the neighborhood relative to control firms over different time periods surrounding the hurricane event. We estimate the additional market value that results from a change in a firm's cash position by regressing the abnormal stock return of the firm on its change in cash holdings and various control variables. The coefficient for the change in cash holdings is then interpreted as a measure of the value of a marginal dollar of cash. Next, we interact this coefficient with a dummy variable, $Neighbor^{q}0$, that is equal to 1 if the firm is in the neighborhood area at time $q0$. This allows us to assess whether being in the neighborhood area of a hurricane marginally deteriorates or improves the value of a marginal dollar of cash. The abnormal return we use is the stock return in excess of the Fama and French (1993) size and book-to-market portfolio return. All control variables are those used in the cash value literature. We exclude from our analysis those observations that correspond to firms located in the disaster zone and to stocks that are not sufficiently liquid.[27]

[INSERT TABLE 7 AROUND HERE]

[27] Stocks not sufficiently liquid are defined as stocks with more than 50% of zero daily returns during the time window considered in the analysis (see Lesmond et al. (1999) for a discussion about the relationship between illiquidity and zero returns).

In columns 1 and 2 of Table 7, we estimate the value of cash during two time periods that end before the occurrence of the hurricane. We find that being located in the neighborhood area at time $q0$ does not change the value of cash before the occurrence of the hurricane. This result is reassuring as cash variations for these firms (Neighborhood area) are not yet statistically different from those of other firms in the rest of the US mainland. However, when the time window begins to capture the hurricane event, the same analysis shows that the value of cash decreases for firms that are in the neighborhood area. In column 3, for instance, the interaction term between $Neighbor^{q}0$ and *Change in cash* is negative and statistically significant. This result indicates that over a 6-month period surrounding the hurricane landfall, the value of a marginal dollar of cash decreases on average by 22 cents when the firm is located in the neighborhood area compared to an average value of 88 cents otherwise. In columns 4 and 5, we use larger time windows around the event, and we obtain similar results. Unsurprisingly, the effect disappears when the time window becomes too large (column 6) because firms located in the neighborhood area increase their level of cash holdings only temporarily.

Overall, these results suggest that the managerial decision to increase the amount of corporate cash holdings temporarily after hurricanes negatively impacts firm value by reducing the value of cash.

1.7 Are there any other alternative explanations?

In this section, we discuss alternative explanations to our results, namely, the possibility of "regional spillover", "change in risk", and/or "risk learning". We first examine and test the implications of each alternative interpretation. Next, we propose and perform another experiment based on earthquake risk whose design alleviates the concern that such alternative explanations are driving our findings.

1.7.1 The possibility of "regional spillover"

First, cash might increase temporarily because of geographical externalities. Indeed, firms located in the neighborhood area could be indirectly affected by the hurricane. Such indirect effects may then explain why the amount of cash holdings temporarily increases. We review the main possible regional spillover effects and test whether they are likely to drive our results.

Higher business and / or investment opportunities

A first spillover effect might arise if the hurricane creates new business or investment opportunities for firms in the neighborhood area. In this case, neighborhood firms may temporarily hold more cash because they make more profits or because they plan to invest in the disaster zone. Under this possible interpretation of our results, firms located in the neighborhood area should thus perform better and invest more after the disaster. However, none of our findings in Table 6 are consistent with such predictions. Indeed, we find no evidence that the proximity of the hurricane positively impacts either growth in terms of revenue or operating income. In addition, we do not find that neighborhood firms invest more after the hurricane. In the Internet Appendix, we further investigate how the hurricane affects the growth of sales for neighborhood firms relative to the control group at every quarter surrounding the disaster. The graph in Figure 7 illustrates the main outcome of this analysis.

[INSERT FIGURE 7 AROUND HERE]

This graph shows that growth in revenues for neighborhood firms does not increase significantly relative to the control group after the hurricane. Therefore, and unlike firms located in the disaster zone, firms located in the neighborhood area are on average truly unaffected. This conclusion is also supported by the analysis of the market reaction at the time of the hurricane landfall.

[INSERT TABLE 8 AROUND HERE]

In Table 8, we report the results of a simple event study analysis. For each group of firms (disaster area, neighborhood area, and the rest of the US mainland), we estimate the average Cumulated Abnormal Return (CAR) of the stock price over the hurricane event period. The methodology used to perform this event study is described in the Internet Appendix. Unsurprisingly, we find a negative abnormal return for firms located in the disaster zone. However, we find no significant reaction for firms located in the neighborhood area, which suggests that investors perceive that there are no benefits (new business and/or investment opportunities) from the proximity of the natural disaster.[28]

Higher business uncertainty

A second form of spillover effect might arise if the hurricane creates locally higher business uncertainty. In this case, managers may decide to stop and/or postpone their investment projects. Neighborhood firms would then temporarily hold more cash. However, this explanation would imply a negative reaction at the announcement of the hurricane, which

[28] We also note that at the time of the event study, the change in cash holdings is not yet observable by market participants. Thus, finding no market reaction here is not inconsistent with the decrease in the value of cash observed in Table 7.

we do not find. We also do not find that firms in the neighborhood area reduce their investments in Table 6 (Column 4). We also explicitly test whether the proximity of the hurricane creates higher uncertainty.

We begin by examining whether the proximity of the hurricane affects the volatility of firm revenues.

[INSERT TABLE 9 AROUND HERE]

We use two different approaches to conduct this examination. In Panel A of Table 9, we estimate revenue volatility at the firm level using the standard deviation of sales growth in a time series. We estimate the standard deviation of the growth in revenues before and after the hurricane for each firm over a four-quarter period.[29] We then test whether this standard deviation is higher for firms in the neighborhood are after the hurricane. In panel B of Table 9, we estimate revenue volatility at the county level using the standard deviation of sales growth in cross section. We estimate the standard deviation of the growth in revenues across all firms from the same county at every quarter surrounding the hurricane event. We then test whether this standard deviation at the county level is affected by the hurricane. Under both approaches, we find that the proximity of the hurricane strike does not significantly affect the variance in revenues.

[INSERT TABLE 10 AROUND HERE]

Our analysis of stock return volatility in Table 10 also provides evidence that the hurricane does not create higher uncertainty for firms in the neighborhood area. In Panel A, we follow a methodology proposed by Kalay and Loewenstein (1985) and use an F-test to assess whether a hurricane event affects stock return variances. We find that an

[29] Estimating the standard deviation over a longer time window leads to the same results.

F-test cannot reject at the 5% level the null hypothesis that the pre-hurricane and post-hurricane stock return variances are equal for the majority of firms in the neighborhood area (64.8%). We next compute stock return volatility at each quarter and test in Panel B whether this volatility changes for firms in the neighborhood area using our baseline specification; we again find that the proximity of the hurricane does not affect stock return volatility. Overall, these results suggest that investors do not perceive higher uncertainty after the hurricane.

Higher financing constraints

Other regional spillover effects include the possibility that the hurricane hurts the lending capacity of banks. If bank customers withdraw their deposits after the hurricane, banks located in the disaster zone and/or the neighborhood area may no longer be able to effectively finance the local economy. Firms in the neighborhood might anticipate that banks will be constrained after the shock and may decide to hold more cash as a precaution. Under this explanation, the amount of new credits at the bank level should decrease after the hurricane. We test this prediction in the Internet Appendix and find the opposite result. In fact, the amount of new commercial and industrial loans increases after the hurricane event for banks located in the disaster zone and for banks located in the neighborhood area relative to other banks. This result casts doubts on the possibility that the hurricane damages the entire local bank lending capacity. It is also consistent with our findings in Table 6 that the proximity of the hurricane does not negatively affect the probability of issuing new financing (Column 10).

A similar alternative story could be that the hurricane hurts local insurance companies and generates insurance rationing (Froot and O'Connell (1999), Froot (2001)). Neighboring companies may react to increased insurance costs by reducing their level of insurance and by increasing their level of cash instead. After some time, insurance premia return to normal levels. Firms then insure again and decrease their cash holdings accordingly. However, at least two of our findings are difficult to reconcile with this explanation. First, cash holdings increases over a one-year period whereas Froot and O'Connell (1999) show that prices for insurance tend to rise over a 3-year period. Second, under the insurance-based explanation, the increase in cash holdings should be concentrated on firms that depend on insurance companies to insure their business. By contrast, firms that are more likely to self-insure should react less. Our result from the internet appendix does not support this prediction. In fact, firms with a lot of intangible assets that cannot be directly insured react more.

Other forms of regional spillover effects

Because a variety of other forms of regional spillover effects might affect our results, we conduct another series of tests in which we focus on firms operating outside of the disaster zone and outside of the neighborhood area. To the extent that these firms are less dependent on the local economy, any increase in corporate cash holdings should be less likely to be driven by a regional spillover effect. The results of these tests are reported in Table 11.

[INSERT TABLE 11 AROUND HERE]

In the first column, we re-run our main test and focus on firms that do not have significant business connections with other firms potentially affected by the hurricane event. Using the Compustat Customer Segment database, we identify 287 neighborhood firms from our sample that have their main customer and/or provider in the disaster area. Column 1 indicates that excluding those firms from our sample does not change our main result: neighborhood firms increase the amount of their corporate cash holdings after a disaster.

In the second column, we examine the effect of the disaster on "the neighbors of neighbors". We define two groups of neighbors according to geographical distance. Specifically, we create a fourth category of firms that correspond to firms located in the neighborhood of the disaster zone but not in its close neighborhood (hereafter, a "Remote Neighbor"). To identify these firms, we match with replacement each affected county with its ten nearest neighbors among the non-affected counties. Firms are then assigned to the Remote Neighbor group if their headquarters are located in the ten nearest non-affected counties but not in the five closest. For each firm identified as a "Remote Neighbor", we calculate the distance between its headquarters and the headquarters of the closest affected firm. On average, we find that firms from our Remote Neighbor group are 80 miles away from the disaster zone. Despite the distance, the regression in Column 2 indicates that these firms also respond to the occurrence of the hurricane by increasing the amount of cash holdings.

In the third column, we focus on all vulnerable firms (excluding firms in the neighborhood of the affected region). Those firms may be far away from the disaster zone (e.g. firms located in the East coast when a hurricane hits Louisiana). We define a firm as sensitive to the risk of hurricane strike if it has been strongly affected once by a hurricane during the sample period. We create a dummy variable Vulnerable that is equal to one if (i) the firm is identified as sensitive to the risk of hurricane disaster, (ii) the firm is neither in the disaster area nor in the neighborhood area, and (iii) the hurricane made landfall over the past twelve months. We obtain a group of 614 “vulnerable firms”, whose average distance from the disaster zone is 444 miles. Despite such a distance, the regression in Column 3 indicates that the managers of these firms increase cash holdings after the hurricane.

Overall, these results suggest that while some regional spillover effects may possibly affect firms in the neighborhood area, these effects cannot be the key explanation of our primary finding.

1.7.2 The possibility of a “change in risk”

Cash holdings might also increase if the real probability of being struck by a hurricane increases. However, this explanation would imply a permanent increase in cash, which we do not find in our results. To be consistent with a “change in risk” interpretation, the increase in risk must be temporary.

Such a temporary increase in risk might occur if hurricane strikes cluster in certain geographic areas during a one-year or two-year period. In this case, being a neighbor could indicate that the probability of being hit by a hurricane in the coming year is now higher than it used to be. We are not aware of any evidence of such a clustering phenomenon in the climate literature (see section 2). Nevertheless, we assess this possibility by testing whether the probability of being hit by a hurricane depends on the geographical location of past hurricane strikes. We use a linear probability model to test whether being in the neighborhood of an area hit by a hurricane affects the probability of being hit by a hurricane in the future. The dependent variable is a dummy equal to 1 if the county is hit by a hurricane. The main explanatory variable is a dummy equal to 1 if a hurricane event occurred over the past 12 months and if the county was in the neighborhood of the disaster zone. The results of this test are reported in table 12.

[INSERT TABLE 12 AROUND HERE]

In Column 1, the regression coefficient for the variable Neighbor is not statistically different from zero, which indicates that when a hurricane makes landfall in a given county, the event reveals no information about future disaster likelihood in the neighboring counties.[30]

1.7.3 The possibility of "risk learning"

Finally, cash holdings might increase if managers ignore or underestimate the risk before the occurrence of the hurricane and learn the true probability of a disaster after the hurricane's landfall. However, this explanation would again imply a permanent increase in cash, which we do not find.

It is also difficult to reconcile such a risk-learning hypothesis with our results regarding the value of cash. If managers learn the true probability of suffering a liquidity shock and increase their cash holdings accordingly, investors should value this decision positively and should not discount the additional cash in the balance sheet.

[30] Column 2 shows the same result when taking into account all hurricanes from the SHELDUS database (and not only the 15 biggest).

1.7.4 Reaction to extreme earthquakes outside the US

To further alleviate the concern that our results are driven by a non-behavioral explanation, we perform one final experiment based on earthquake risk rather than hurricane risk. We test the validity of the *availability heuristic* hypothesis by looking at US firms whose headquarters are located in urban communities in which earthquakes are frequently felt. We then focus on the announcement of extremely violent (and therefore salient) earthquakes outside the US and examine whether these firms respond to such announcements by changing the amount of their cash holdings. Finding an increase in cash holdings would then be consistent with the *availability heuristic* hypothesis while allowing us to rule out other possible explanations. Indeed, it would neither be consistent with the *change in risk hypothesis* nor with the *risk-learning* hypothesis because the occurrence of an earthquake outside the US (for instance, in Pakistan) provides no information about the likelihood of experiencing an earthquake in US territory.[31] It would also not be consistent with the geographical spillover hypothesis because of the distance to the disaster area.

We obtain information about the level of intensity felt by zip code address for each earthquake from the *"Did you feel it?"* surveys performed under the Earthquake Hazard Program by the USGS. For each zip code, we compute the average earthquake intensity felt over the past 20 years. We assign the average earthquake intensity felt to each firm in Compustat using the zip code from the headquarters' address. We then focus on firms within the top 10% of the average intensity felt distribution and assign them to a seismic zone group (treatment group). All other firms are assigned to a non-seismic zone group

[31] In addition, this test focuses on US firms whose managers frequently feel earthquakes. Thus, they cannot ignore this risk. This also casts doubts on the possibility of a learning reaction.

(control group). Next, we focus on the strongest earthquakes that have occurred outside the US in the past 30 years according to descriptions of magnitude, total deaths, and total damage. We obtain all this information from the Significant Earthquake Database.[32]

These selection criteria lead to the list of major non-US earthquakes described in the Internet Appendix. We then estimate the average change in cash holdings for the seismic zone group around the announcement of the earthquake outside the US using exactly the same matching methodology as the one previously used and described above for hurricanes. The results of this analysis are depicted in the graph of Figure 8.[33]

[INSERT FIGURE 8 AROUND HERE]

Figure 8 shows qualitatively the same pattern as that previously observed. Firm managers located in seismic areas respond to the sudden salience of earthquake risk by temporarily increasing the level of cash holdings compared to firms located outside a seismic zone. This analysis confirms that firm managers are subject to the availability bias while rejecting other non-behavioral explanations.

1.8 The effects of cash holdings on post-hurricane performance

If managers respond to the salience of hurricane risk by increasing corporate cash holdings, and if this reaction is motivated by seeking insurance against such risk, then we should expect cash holdings to protect firm revenues and reduce losses when this risk

[32] National Geophysical Data Center/World Data Center (NGDC/WDC) Significant Earthquake Database, Boulder, CO, USA. (Available at http://www.ngdc.noaa.gov/nndc/struts)

[33] More details about our methodology and the detailed results are provided in the Internet Appendix.

materializes. We run this falsification test in this section. We focus on firms affected by a hurricane event and examine how the level of cash holdings before the disaster affects firm performance in terms of sales growth after the disaster.

To perform this test, we again use a difference-in-differences methodology. We use an approach identical to that used to estimate the effect of a hurricane on cash holdings except that (i) firms in the treatment group are firms whose headquarters are located in the disaster area, (ii) firms assigned to the control group are all other firms, and (iii) the outcome variable we are interested in is growth in revenues. We estimate how firms that are directly affected by the hurricane perform in terms of sales growth relative to the control group after the disaster conditional on their level of cash holdings (low, medium or high) before the hurricane. The graph depicted in Figure 9 illustrates the main outcome of this analysis.[34]

[INSERT FIGURE 9 AROUND HERE]

This graph compares three categories of firms defined according to the level of their cash holdings before the hurricane (high, medium, or low) and shows how each category performs in terms of sales growth relative to the control group over time. All categories of firms appear to be negatively affected by the hurricane during the first two quarters following the hurricane event. On average, sales growth is approximately 9% lower for treated firms than for control firms during the second quarter following the disaster, and the economic magnitude of this revenue loss is similar across the three categories of firms. However, performance in terms of sales growth in subsequent quarters is different. Firms in the high cash tercile before the disaster rapidly catch up with firms in the control group

[34] More details about our methodology and the detailed results are provided in the Internet Appendix.

in terms of sales growth. These high cash firms even temporarily outperform control firms and recover their loss of revenues within the year following the shock. By contrast, it takes approximately two years for firms in the low cash tercile to catch up with firms in the control group in terms of sales growth, and these low cash firms never recover their losses.

Overall, these results confirm that holding cash contributes to insuring against the effects of hurricane risk. They are consistent with our primary finding and help to explain why managers may be willing to increase the amount of corporate cash holdings when they perceive that the risk of a hurricane strike is higher.

1.9 Conclusion

In their seminal paper, Tversky and Kahneman (1973, 1974) observe that people have a tendency to develop heuristic rules to reduce the complex task of estimating probabilities. They show that, although useful in general, relying on these rules can also produce mistakes. This paper provides direct evidence that firm managers rely on one such rule to assess risk: the availability heuristic. Using cash holdings as a proxy for risk management, we find that managers located in the neighborhood area of a hurricane landfall temporarily perceive more risk after the event even though the real risk remains unchanged. We show that this mistake, which is caused by the temporary salience of the danger, is costly and inefficient. It leads to reduce shareholders compensation and destroys firm value by reducing the value of cash. Over our sample period and across all firms, the total amount of cash temporarily immobilized because of this assessment bias is almost 65 billion dollars. Given the large and increasing diversity of risks that must be assessed every day by firm managers, our results suggest that the total real economic cost of this bias is likely to be considerable.

References

Acharya, V., S. A. Davydenko, and I. A. Strebulaev, (2012), "Cash Holdings and Credit Risk". *Review of Financial Studies* 25, 3572-3609.

Almeida, H., M. Campello, and M. S. Weisbach, (2004), "The Cash Flow Sensitivity of Cash". *Journal of Finance* 59,1777-804.

Baker, M., and J. Wurgler, (2012), "Behavioral Corporate Finance: A Current Survey". *Handbook of the Economics of Finance*, Vol. 2, edited by George M. Constantinides, Milton Harris, and Rene M. Stulz. Handbooks in Economics. New York, 2012.

Baker, M., and Y. Xuan, (2011), "Under New Management: Equity Issues and the Attribution of Past Returns". *Working Paper*.

Baker, M., X. Pan, and J. Wurgler, (2012), "The effect of reference point prices on mergers and acquisitions". *Journal of Financial Economics* 106, 49-71.

Barber, B. M., and T. Odean, (2008), "All That Glitters: The Effect of Attention and News on the Buying Behavior of Individual and Institutional Investors". *Review of Financial Studies* 21, 785-818.

Bates, T. W., K. M. Kahle, and R. M. Stulz, (2009), "Why Do U.S. Firms Hold So Much More Cash than They Used To?". *Journal of Finance* 64, 1985-2021.

Bertrand, M., E. Dulfo, and S. Mullainathan, (2004) "How Much Should We Trust Differences-in-Differences Estimates?". *Quarterly Journal of Economics*, 119, 249-75.

Betton, S., B. E. Eckbo, K. S. Thorburn, (2008), "Corporate Takeovers", ". *Handbook of Corporate Finance: Empirical Corporate Finance*, Vol. 2, Chapter 15, 291-430.

Bin, O., and Stephen Polasky, (2004), "Effects of Flood Hazards on Property Values: Evidence before and after Hurricane Floyd". *Land Economics* 80. 490-500.

Blake, E. S., C. W. Landsea, and E. J. Gibney, (2011), "The deadliest, costliest, and most intense United States tropical cyclones from 1851 to 2010 (and other frequently requested hurricane facts)". *NOAA Technical Memorandum NWS NHC-6*.

Bordalo, P., N. Gennaioli, and A. Shleifer, (2012a). "Salience in Experimental Tests of the Endowment Effect.". *American Economic Review* 102, 47-52.

Bordalo, P., N. Gennaioli, and A. Shleifer, (2012b). "Salience Theory of Choice Under Risk". *Quarterly Journal of Economics* 127, 1243-85.

Bordalo, P., N. Gennaioli, and A. Shleifer, (2013), "Salience and Consumer Choice". *Journal of Political Economy* (Forthcoming).

Brav, Alon P., J.R. Graham, C. Harvey and R. Michaely, (2005), "Payout Policy in the 21st Century". *Journal of Financial Economics* 77, 483-527.

Choi, J. J., D. Laibson, B.C. Madrian and A. Metrick, (2009), "Reinforcement Learning and Savings Behavior". *Journal of Finance* 64, 2515-2534.

DellaVigna, S. (2009). "Psychology and Economics: Evidence from the Field". *Journal of Economic Literature*, 47(2), 315-372.

Denis, D. J., and V. Sibilkov, (2010), "Financial Constraints, Investment, and the Value of Cash Holdings". *Review of Financial Studies* 23, 247-269.

Dittmar, A., and J. Mahrt-Smith, (2007), "Corporate Governance and the Value of Cash Holdings". *Journal of Financial Economics* 83,599-634.

Dougal C., J. Engelberg, C. A. Parsons and E. D. Van Wesep, (2011). "Anchoring and the Cost Capital". *Working Paper.*

Elsner, J. B., and B. H. Bossak, (2001), "Bayesian Analysis of U.S. Hurricane Climate". *Journal of Climate* 14, 4341-4350

Emanuel K. (2005), "Emanuel replies". *Nature* 438, E13.

Fama, E., French, K., (1993), "Common risk factors in the returns on stocks and bonds". *Journal of Financial* Economics 33, 3-56.

Faulkender, M., and R. Wang, (2006), "Corporate Financial Policy and the Value of Cash". *Journal of Finance* 61, 1957-1990.

Fresard L. and P. Valta, (2012), "Competitive Pressures and Corporate Policies". *Working Paper.*

Froot, K. A., (2001), "The market for catastrophe risk: A clinical examination". *Journal of Financial Economics* 60, 529-571.

Froot, K. A., and D. S. Scharfstein, and J. C. Stein, (1993), "Risk Management: Coordinating Corporate Investment and Financing Policies". *Journal of Finance* 48, 1629-1658.

Garmaise, M. J. and Moskowitz, T. J. (2009), "Catastrophic Risk and Credit Markets". *Journal of Finance*, 64(2), 657-707.

Gennaioli, N., and A. Shleifer, (2010), "What Comes to Mind". *Quarterly Journal of Economics* 125, 1399-1433.

GuimarÃ£es, P., and P. Portugal, (2010), "A simple feasible procedure to fit models with high-dimensional fixed effects". *Stata Journal* 10:628-49.

Guiso, L., Sapienza, P., and L. Zingales, (2013), "Time Varying Risk Aversion". *Working*

Paper.

Greenwood, R., and S. Nagel, (2009), "Inexperienced investors and bubbles". *Journal of Financial Economics* 93, 239-258.

Harford, J., (1999), "Corporate Cash Reserves and Acquisitions". *Journal of Finance* 54,1969-97.

Heckman, J. J., H. Ichimura, and P. Todd, (1998), " Matching As An Econometric Evaluation Estimator". *Review of Economic Studies* 65, 261-294.

Hirshleifer, D. A., K. Hou, S. H. Teoh, and Y. Zhang, (2004), "Do Investors Overvalue Firms with Bloated Balance Sheets?". *Journal of Accounting and Economics* 38, 297-331.

Hirshleifer, D. A., and S. H. Teoh, (2003), "Limited Attention, Information Disclosure, and Financial Reporting". *Journal of Accounting and Economics* 36, 337-386.

Hirshleifer, D. A., S. Lim and S. H. Teoh, (2009), "Driven to Distraction: Extraneous Events and Underreaction to Earnings News". *Journal of Finance* 63, 2287-2323.

Hirshleifer, D. A., S. Lim and S. H. Teoh, (2011), "Limited Investor Attention and Stock Market Misreactions to Accounting Information". *Review of Asset Pricing Studies* 1, 35-73.

Hirshleifer, D. A., S. H. Teoh, S. H., and A. Low, (2012), "Are Overconfident CEOs Better Innovators?". *Journal of Finance* 67, 1457-1498.

Ho, Thomas S. Y., and Roni Michaely, (1988), "Information Quality and Market Efficiency". *The Journal of Financial and Quantitative Analysis* 23, 53-70.

Holmstrom B. and J. Tirole, (2000), "Liquidity and Risk Management". *Journal of Money, Credit and Banking* 32, 295-319.

Holmstrom B., and J. Tirole, (1998), "Private and Public Supply of Liquidity". *Journal of Political Economy* 106, 1-40.

Huberman, G, and T. Regev, (2001), "Contagious speculation and a cure for cancer: A nonevent that made stock prices soar". *The Journal of Finance* 66, 387-396.

Johnson, E. J., J. Hershey, J. Meszaros, and H. Kunreuther, (1993), "Framing, probability distortions, and insurance decisions". *Journal of Risk and Uncertainty* 7, 35-51.

Kalay, A., and U. Loewenstein, (1985), "Predictable events and excess returns. The Case of Dividend Announcements". *Journal of Financial Economics* 14,429-449.

Kaustia, M., and S. KnÃ$\frac{1}{4}$pfer, (2008), “Do Investors Overweight Personal Experience? Evidence from IPO Subscriptions”. *Journal of Finance* 63, 2679-2702.

Khokhar, R., (2012), "Window Dressing in Corporate Cash Holdings: Evidence from Quarterly Financial Statements". *Working Paper.*

Kim, C.-S., D. Mauer, and A. Sherman, (1998), "The Determinants of Corporate Liquidity: Theory and Evidence". *Journal of Financial and Quantitative Analysis* 33, 335-59.

Klibanoff, P., O. Lamont, and TA Wizman, (1998), "Investor Reaction to Salient News in Closed-End Country Funds". *The Journal of Finance* 51, 673-700.

Kousky, C., (2010), "Learning from Extreme Events: Risk Perceptions after the Flood". *Land Economics* 86, 395-422.

Krueger, P., A. Landier, and D. Thesmar, (2011), "The WACC Fallacy: The Real Effects of Using a Unique Discount Rate". *Working Paper*

Kunreuther, H. C., ed. (1978). *Disaster Insurance Protection: Public Policy Lessons.* New York: John Wiley and Sons.

Landier, A., and D. Thesmar, (2009), "Financial Contracting with Optimistic Entrepreneurs". *Review of Financial Studies* 22, 117-150.

Landsea, C. W., (2005), "Hurricanes and global warming". *Nature* 438, E11

Landsea, C. W., B. A. Harper, K. Hoarau, and J. A. Knaff (2005). "Can We Detect Trends in Extreme Tropical Cyclones?". *Science*, 313, 452-454.

Lesmond, D., J. Ogden, and C. Trzcinka, (1999), "A New Estimate of Transaction Costs". *Review of Financial Studies* 12, 1113-1141.

Levitt, S. D., and J. A. List, (2007), "What Do Laboratory Experiments Measuring Social Preferences Reveal About the Real World?". *Journal of Economic Perspectives* 21, 153-174.

Lichtenstein, S., and P. Slovic, (1978), "Judged frequency of lethal events". *Journal of Experimental Psychology: Human Learning and Memory* 4, 551-578.

Lins, K. V., H. Servaes, and P. Tufano, (2010), "What drives corporate liquidity? An international survey of cash holdings and lines of credit". *Journal of Financial Economics* 98, 160-176.

Ljungqvist, A. and W. Wilhelm, (2005), "Does Prospect Theory Explain IPO Market Behavior?". *Journal of Finance* 60, 1759-1790.

Loughran T. and J. Ritter, (2002), "Why don't Issuers get Upset about Leaving Money on the Table in IPOs?". *Review of Financial Studies* 15, 413-443.

MacDonald, D. N, H. L. White, P. M. Taube, and W. L. Huth, (1990), "Flood Hazard Pricing and Insurance Premium Differentials: Evidence from the Housing Market". *Journal of Risk and Insurance* 5, 654-63.

Malmendier, U., and G. Tate, (2005), "CEO Overconfidence and Corporate Investment". *Journal of Finance* 60, 2661-2700.

Malmendier, U., and S. Nagel, (2011), "Depression babies: do macroeconomic experiences affect risk taking?". *Quarterly Journal of Economics* 126, 373-416.

Malmendier, U., and S. Nagel (2011), "Learning from Inflation Experiences". *Working Paper.*

Opler, T., L. Pinkowitz, R. M. Stulz, and R. Williamson, (1999), "The Determinants and Implications of Corporate Cash Holdings". *Journal of Financial Economics* 52, 3-46.

Pielke, R. A., (2005), "Are there trends in hurricane destruction?". *Nature* 438, E11

Pielke, R. A., Jr., C. Landsea, M. Mayfield, J. Laver, and R. Pasch, (2008), "Hurricanes and global warming". *American Meteorological Society*, 1571-1575.

Pielke, R. A., Jr., J. Gratz, C. W. Landsea, D. Collins, M. A. Saunders, and R. Musulin, (2008), "Normalized Hurricane Damage in the United States:1900-2005". *Natural Hazards Review* 9, 29-42.

Ramirez, A., and N. Altay, (2011), "Risk and the Multinational Corporation Revisited: The Case of Natural Disasters and Corporate Cash Holdings". *Working Paper.*

Roberts, M. R., and T. M. Whited, (2012), "Endogeneity in Empirical Corporate Finance". *Simon School Working Paper* No. FR 11-29.

Savickas, R., (2003), "Event-induced Volatility and Tests For Abnormal Performance". *The Journal of Financial Research* 106, 165-178.

Shleifer, A., and R. Vishny, (2011), "Fire Sales in Finance and Macroeconomics". *Journal of Economic Perspectives* 25, 29-48.

Todd, P., (1999), "A Practical Guide to Implementing Matching Estimators". *Mimeo*, University of Pennsylvania.

Tversky, A., and Kahneman, D, (1973), "Availability: A heuristic for judging frequency and probability". *Cognitive Psychology* 4, 207-232.

Tversky, A., and Kahneman, D, (1974), "Judgment under Uncertainty: Heuristics and Biases". *Science* 185, 1124-31.

1.10 Figures and Tables

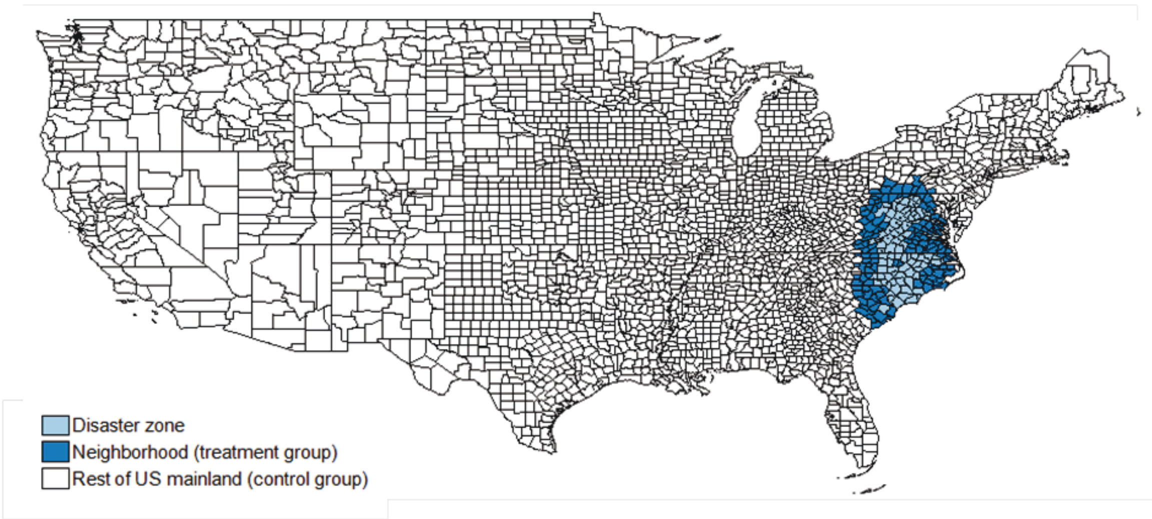


Figure 1.1: . Identification of Neighbors: Illustration for Hurricane Fran (1996)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Fran in 1996. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

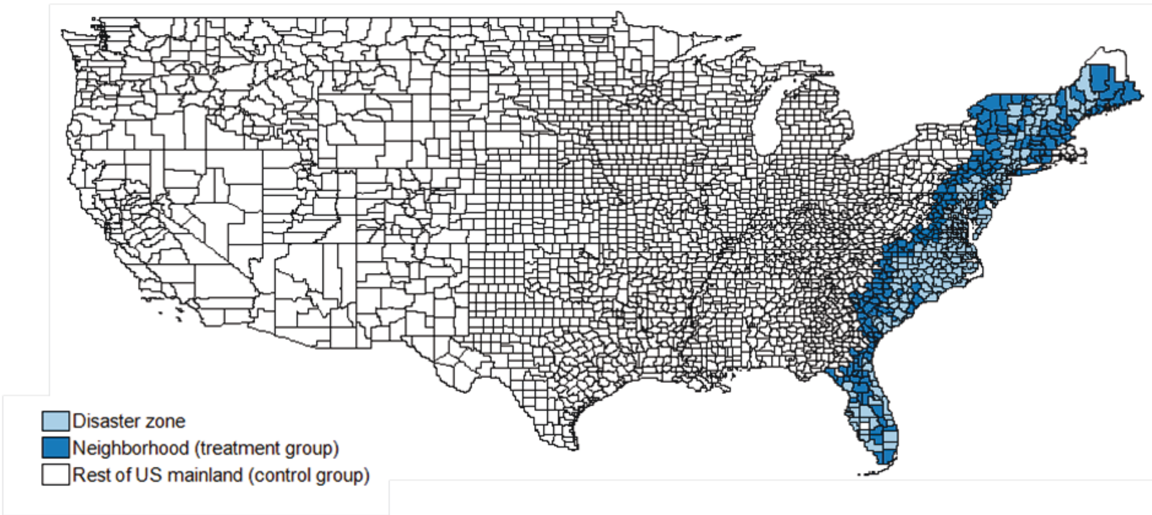


Figure 1.2: . Identification of Neighbors: Identification of Neighbors: Illustration for Hurricane Floyd (1999)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Floyd in 1999. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

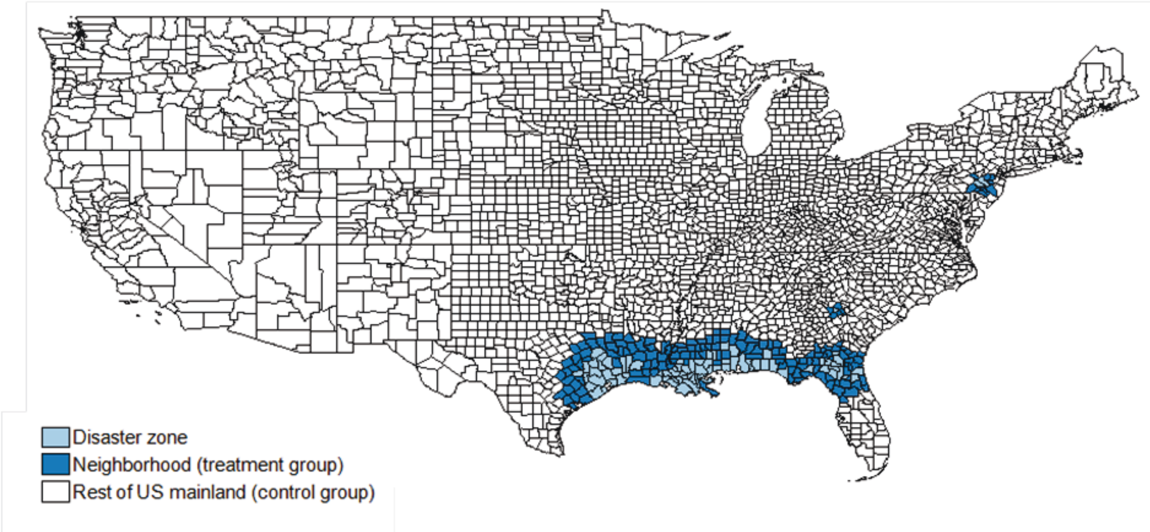


Figure 1.3: . Identification of Neighbors: Illustration for Hurricane Allison (2001)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Allison in 2001. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

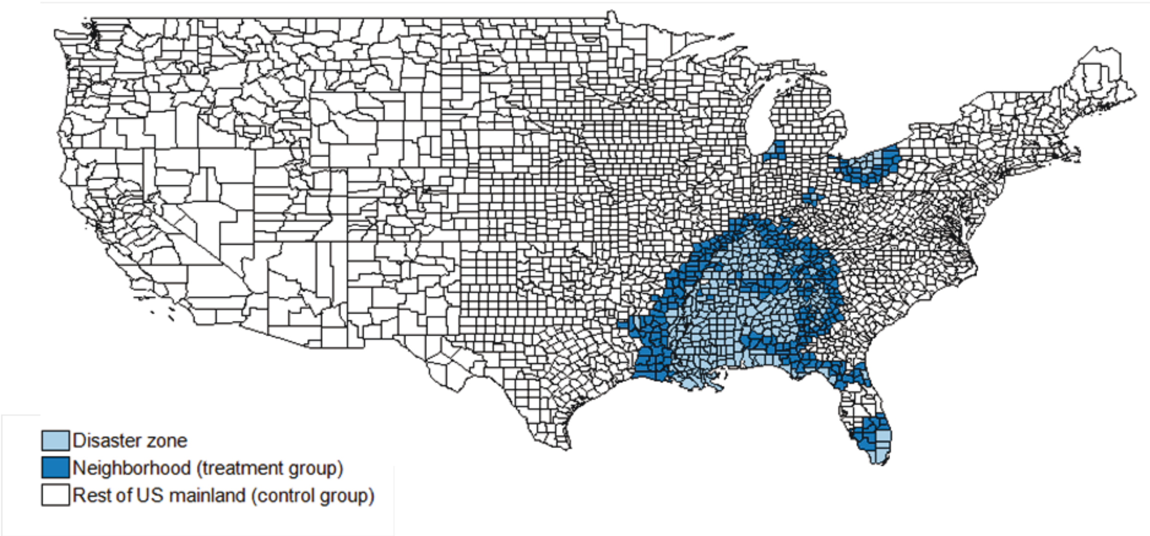


Figure 1.4: . Identification of Neighbors: Identification of Neighbors: Illustration for Hurricane Katrina (2005)

This map presents the result of the matching procedure performed to identify the degree of proximity of each county to the area affected by hurricane Katrina in 2005. Each county inside the disaster area is matched with replacement with the five nearest counties outside the disaster area according to geographical distance. The geographical distance is computed using the average latitude and longitude of all the urban communities of the county. Firms located in the Neighborhood (dark blue counties on the map) are assigned to treatment group. Firms located in the rest of the US mainland (White counties on the map) are assigned to control group. Firms located in the disaster zone (light blue counties on the map) are not considered in the analysis.

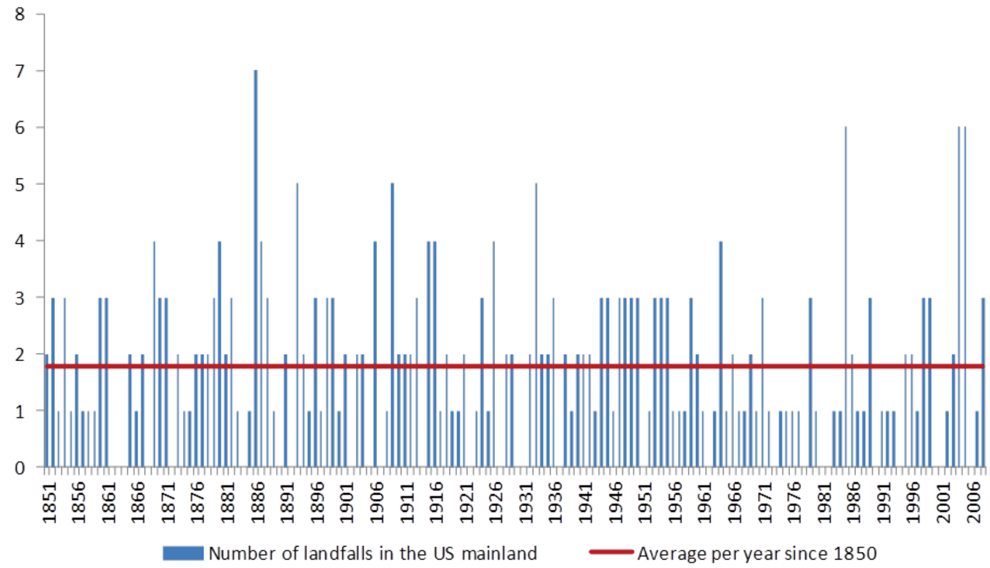


Figure 1.5: . Annual Number of Hurricanes since 1850

This graph presents the total annual number of hurricanes with landfall in the US mainland since 1850. The source of the information is the NOAA Technical Memorandum (2011)

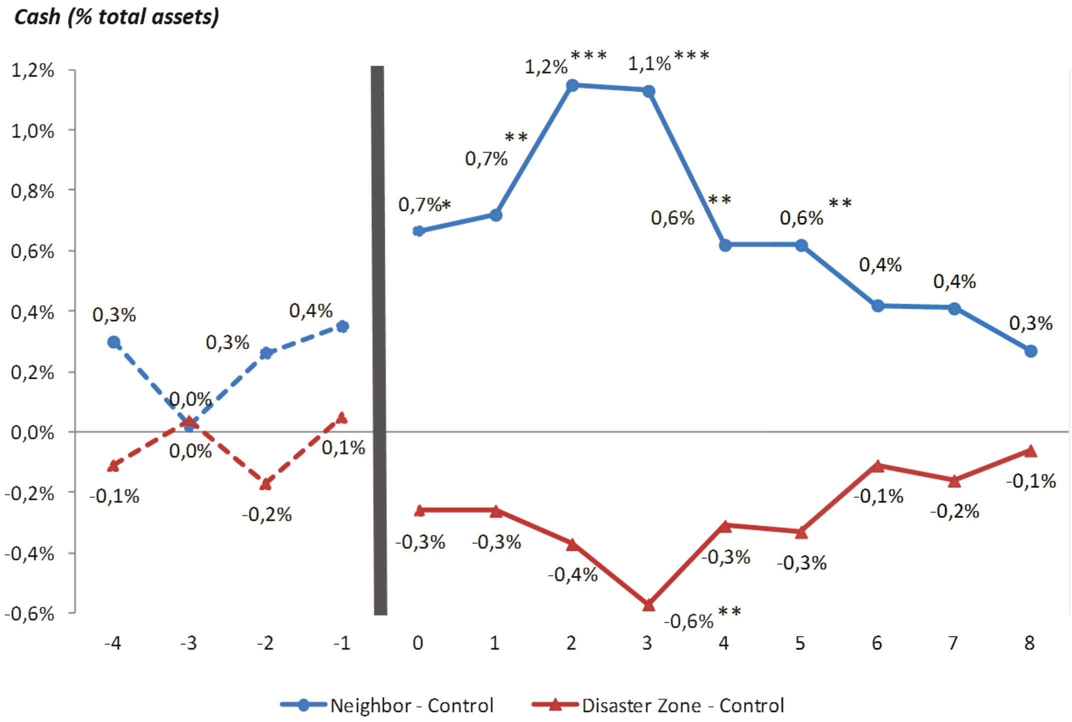


Figure 1.6: . Effects of Hurricane Proximity on Corporate Cash holdings

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters surrounding the hurricane event (quarter $q0$). The blue line plots the difference-in-differences in the level of corporate cash holdings for firms located in the neighborhood area. The red line plots the difference-in-differences in the level of corporate cash holdings for firms located in the disaster zone. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table 4. ***, **, and * denote significance at the 1%, 5% and 10% levels.

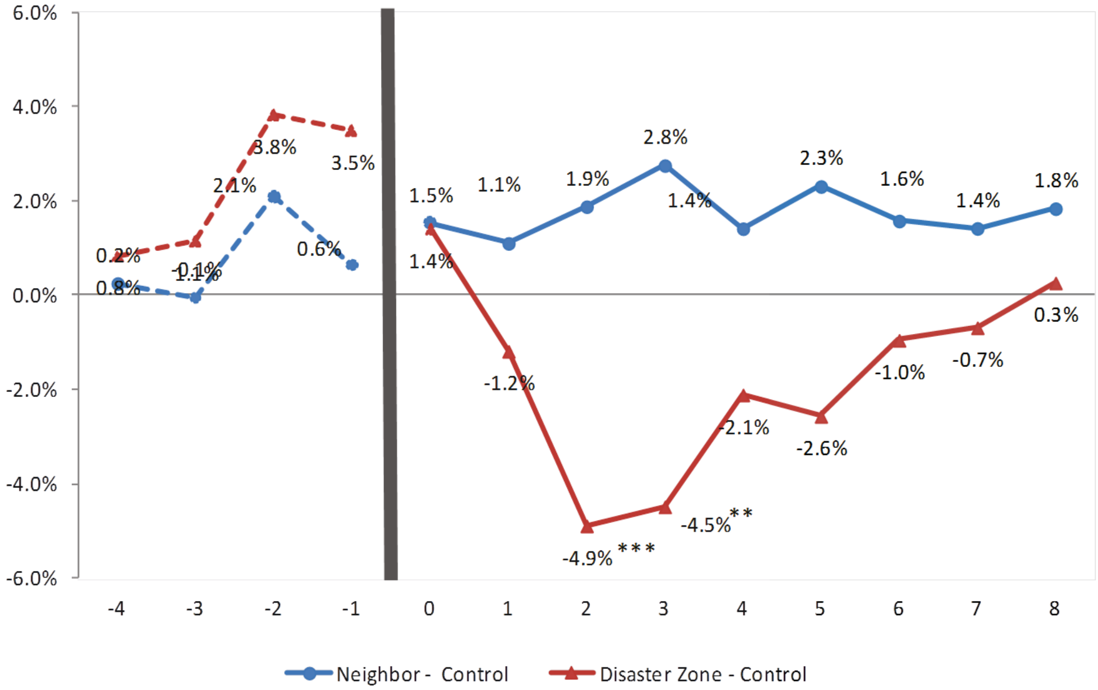


Figure 1.7: . Effects of Hurricane Proximity on Sales Growth

This graph presents difference-in-differences in sales growth at different quarters surrounding the hurricane event (quarter $q0$). The growth in sales is the growth in total revenues relative to the same quarter of the previous year. The blue line plots the difference-in-differences in sales growth for firms located in the neighborhood area. The red line plots the difference-in-differences in sales growth for firms located in the disaster zone. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table D reported in Internet Appendix. ***, **, and * denote significance at the 1%, 5% and 10% levels.

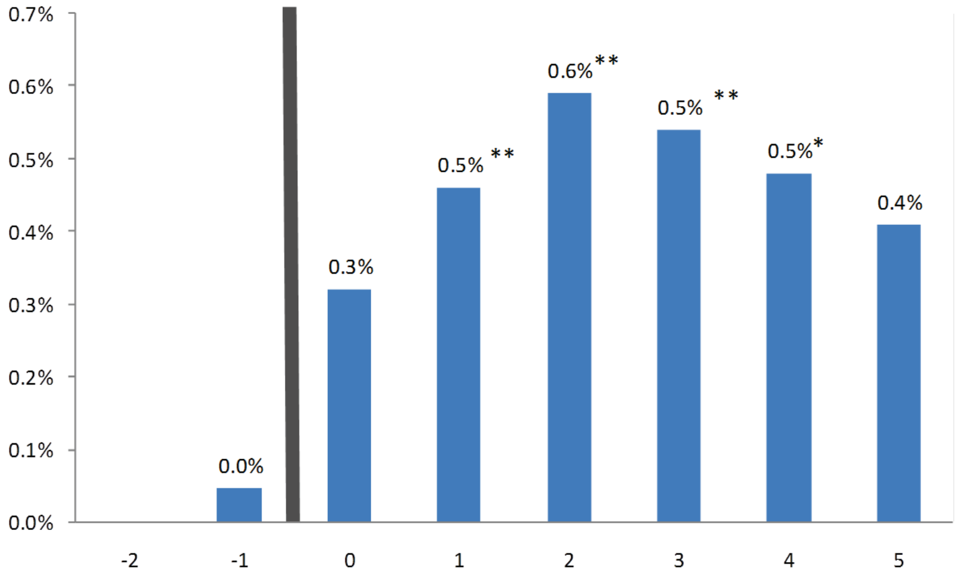


Figure 1.8: . Effects of Earthquakes outside the US on Corporate Cash holdings of US Firms

This graph presents difference-in-differences in the level of corporate cash holdings at different quarters surrounding the announcement of a violent earthquake outside the US (quarter $q0$) for a sample of US firms located in a seismic area. This sample comprises 1,191 treated firms whose headquarters are located in a urban community where an earthquake is frequently felt according to the U.S. Geological surveys (”Seismic zone firms”). For each treated firm, the counterfactual outcome is the weighted average of the change in the level of cash holdings relative to q-2 over all control firms with the same SIC 3 code (”Matched firm”). The weighting is achieved through a kernel function so that the closer control firms in terms of Mahalanobis distance to the treated firm receive greater weight. The Mahalanobis distance is computed at quarter $q-2$ (ie. three months before the earthquake occurrence) along four dimensions: size, age, market-to-book, and financial leverage. ***, **, and * denote significance at the 1%, 5% and 10% levels.

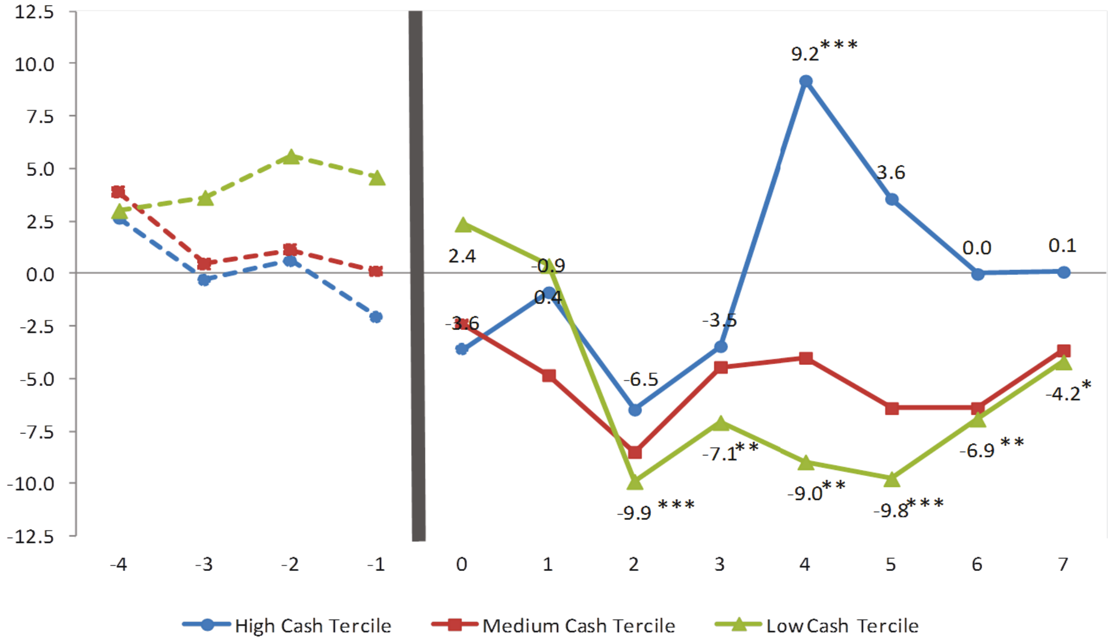


Figure 1.9: . Effects of Cash Holdings on Revenues of Firms Located in the Disaster Area

This graph presents difference-in-differences in sales growth between firms located inside and outside the disaster area at different quarters surrounding the hurricane event (quarter $q0$) conditional on the level of corporate cash holdings before the occurrence of the disaster. The growth in sales is the growth in total revenues of the firm relative to the same quarter of the previous year. The blue (respectively, red, green) line plots the difference-in-differences in sales growth for the sub-sample of firms with a level of cash holdings in the top (respectively, middle, bottom) tercile of the distribution at the end of the quarter before the occurrence of the hurricane. All difference-in-differences estimates use firms in the *Rest of the US Mainland* zone as the control group. These estimates are obtained using the specification of Table I reported in Internet Appendix. ***, **, and * denote significance at the 1%, 5% and 10% levels.

Résumé français

Cette thèse est composée de quatre essais qui s'articulent autour de trois principaux thèmes : la finance d'entreprise et l'innovation, les effets réels de la concurrence bancaire et enfin la finance d'entreprise comportementale.

Chapitre 1. Les effets réels d'un choc sur les relations bancaires sur les entreprises innovantes et l'allocation des talents dans l'économie

Dans ce papier avec Johan Hombert (HEC Paris), nous explorons les conséquences pour le financement des entreprises innovantes et l'allocation des talents d'un choc négatif sur les relations bancaires. Nous identifions le nombre d'entreprises innovantes présentes dans chaque état américain en utilisant la base de données des brevets du NBER. Cette base contient un identifiant unique pour l'entreprise déposant un brevet, l'état où l'entreprise est active, ainsi que les inventeurs qu'elle emploie et la location de ces derniers.

Notre intuition dans ce papier est que les relations bancaires, c'est-à-dire le fait qu'un banquier et un entrepreneur interagissent régulièrement, permettent au banquier d'acquérir de l'information « soft », i.e. non codifiée, précisément grâce aux multiples interactions personnelles que le banquier et l'entrepreneur vont avoir. Ce type d'interaction doit favoriser l'innovation qui est *a priori* un type d'investissement intensif en information soft, par opposition à de l'information codifiable qui peut être facilement transmise au sein de la hiérarchie de l'établissement bancaire (Stein, 2002).

Afin de générer un choc sur les relations bancaires, nous utilisons la dérégulation bancaire entre états qui s'est produite aux Etats-Unis entre le début des années 1970 et 1994. Durant cette période, les états ont progressivement dérégulé leur marché bancaire local à des années différentes. Cette dérégulation a eu deux conséquences importantes pour le type d'information que les prêteurs et les débiteurs peuvent échanger. D'une part, cette dérégulation a conduit à une hausse de la concurrence entre les banques en supprimant les restrictions qui s'appliquaient initialement à l'expansion des banques au sein de l'état (e.g. Black et Strahan, 2002). D'autre part, en supprimant les contraintes sur les opérations de fusions et acquisition entre les banques de l'état, cette dérégulation a conduit également au développement de plus grandes banques, plus hiérarchiques, au sein desquelles l'information soft n'est pas capable de circuler (Stein, 2002).

Cela conduit les banques à interagir de façon plus impersonnelles avec leurs emprunteurs potentiels. La mise en place de la dérégulation bancaire par les états à des années différentes nous permet d'analyser l'effet de la réforme avec une stratégie de « différence-en-différences », où nous comparons l'évolution du nombre d'entreprises innovantes avant et après l'adoption de la dérégulation relativement à un groupe « contrôle » d'états qui ne mettent pas en place la dérégulation.

Nous trouvons que le nombre d'entreprises innovantes décline après la mise en place de la dérégulation bancaire. En moyenne, le nombre d'entreprises innovantes dans les états dérégulés est 20% plus faible dix ans après la dérégulation.

Afin de confirmer que notre effet provient bien d'un choc de crédit négatif pour les entreprises dépendantes des relations bancaires, nous conduisons une série de tests en cross-section. Construisant trois proxies classiques dans la littérature afin de classifier les industries en fonction de l'importance des relations bancaires pour les entreprises dans ces industries, nous trouvons que l'effet négatif de la dérégulation est beaucoup plus fort pour les entreprises opérantes dans les industries fortement dépendantes des relations bancaires. Nous trouvons également que l'effet est plus prononcé pour les entreprises opérantes dans des industries ayant des besoins de financement externes plus importants (Rajan et Zingales, 1998) ou ayant moins d'actifs tangibles qui puissent servir de garantir pour un prêt (Almeida et Campello, 2007).

Nous construisons également des proxies de dépendance des relations bancaires plus fins au niveau des entreprises Puisque la théorie prédit que les entreprises plus « opaques » (difficile à évaluer) sont plus dépendantes des relations bancaires (e.g. Berger et al. 2005), nous construisons trois proxies d'opacité au niveau de l'entreprise. Tout d'abord, nous trouvons que l'effet négatif de la dérégulation est concentré sur les jeunes entreprises. Ensuite, en contrôlant pour l'âge de l'entreprise, nous trouvons que l'effet est plus fort pour les entreprises étant dans des industries plus jeunes, c'est-à-dire des industries où l'évaluation des projets est plus complexes pour les banquiers puisqu'ils ne disposent que de peu de projets ayant réussis et permettant une comparaison. Enfin, en utilisant les brevets déposés par le passé, nous trouvons que l'effet est plus important pour les entreprises ayant déposées moins de brevets jusqu'à présent et donc moins à même de fournir de l'information tangible sur la qualité de l'entreprise.

La seconde partie du papier est consacrée à étudier les conséquences de ce choc négatif de crédit sur le marché du travail des inventeurs. Tout d'abord, nous trouvons que les petites entreprises (plus affectées par le choc négatif de crédit) modifient la composition des inventeurs qu'elles embauchent et préfèrent des inventeurs expérimentés à de jeunes inventeurs, consistant avec l'hypothèse que ces entreprises compensent la perte d'information « soft » en employant des inventeurs ayant déjà démontré par le passé leur efficacité.

Ensuite, nous étudions l'effet de ce choc de crédit négatif sur la mobilité des inventeurs entre les entreprises (au sein du même état) et entre états. Nous trouvons que les jeunes inventeurs prometteurs sont plus enclins à quitter les petites entreprises et à rejoindre des entreprises plus grosses au sein de l'état. Nous trouvons également que ces inventeurs sont plus enclins à quitter les états qui ont déréglementé, encore plus quand ils étaient initialement employés dans une petite entreprise. Ces résultats indiquent que cette réforme bancaire n'a pas seulement affecté le niveau de l'innovation dans les états, mais également la distribution des inventeurs entre états et entre entreprises.

Chapitre 2. Le rôle des facteurs d'offre dans le phénomène de non-bancarisation des ménages

Ce papier, écrit en collaboration avec Claire Célerier (Université de Zurich) cherche à comprendre certains déterminants du très fort taux de non bancarisation des ménages à bas revenus aux Etats-Unis, c'est-à-dire des ménages vivant avec moins de deux fois le seuil de pauvreté. En effet, le taux de non bancarisation de cette population oscille entre 35% et 45%. Ce taux très élevé peut s'expliquer par deux facteurs essentiels : les facteurs de demande et les facteurs d'offre. Les partisans du rôle de la demande expliquent le phénomène de non bancarisation par des déterminants culturels (un manque de confiance dans le système bancaire, une absence de culture de l'épargne) ou un faible niveau d'éducation financière. A l'inverse, les tenants du rôle des facteurs d'offre considèrent que les ménages à faible revenu souhaitent avoir un compte bancaire mais ne peuvent tout simplement pas à cause des pratiques des banques : exigence d'un minimum d'argent sur le compte courant, faible couverture du territoire par les agences bancaires, multiplications de procédures complexes ou frais de dépassement importants sont autant de pratiques qui peuvent maintenir les ménages à faible revenu dans une situation de non-bancarisation (e.g. Washington, 2006 ou Barr, 2008).

Notre papier montre que les facteurs d'offre jouent un rôle important dans l'accès aux services bancaire de base. Afin d'établir un effet causal, nous utilisons la seconde vague de dérégulation bancaire que les états ont progressivement mis en place entre 1994 et 2005. Cette dérégulation progressive nous permet d'analyser la probabilité pour un ménage à bas revenu d'avoir un compte en banque si ce dernier habite dans un état qui vient de déréglementer, relativement à un état n'ayant pas déréglementé.

La déréglementation consiste pour les états à supprimer différentes barrières à l'entrée pour les agences bancaires d'autres états, permettant ainsi par exemple à une banque dans l'état de New York d'ouvrir facilement une agence dans le Nevada, conduisant ainsi à une hausse de la concurrence dans cet état.

Notre principal résultat établit que la hausse de la concurrence bancaire conduit à un accroissement de la probabilité pour un ménage à bas revenu de détenir un compte en banque, relativement à un ménage similaire étant dans un état n'ayant pas déréglementé. Les données sur les ménages dont nous disposons nous permettent de contrôler pour un très grand nombre de caractéristiques économiques (revenu, montant des différents transferts sociaux, situation de l'emploi) et sociales (âge du chef de ménage, taille du ménage, nombre d'enfants de moins de 18 ans, etc.). Une étude détaillée de la dynamique de l'effet montre qu'avant l'adoption de la déréglementation, la probabilité entre les ménages de différents états de détenir un compte en banque est absolument similaire. Ce n'est qu'une fois la déréglementation adoptée que nous observons une hausse de la probabilité pour un ménage à faible revenu de détenir un compte bancaire. L'effet économique est important. Un état passant d'une situation de complète réglementation à une situation de complète déréglementation (cf. Rice et Strahan, 2010) augmente la probabilité des ménages à faible revenu vivant dans cet état de détenir un compte bancaire de 4 point de pourcentage. Etant donnée un taux de non bancarisation moyen entre 35% et 40%, cela représente une baisse relative du taux de non bancarisé de plus de 10%.

Nous montrons également que les ménages à faible revenu qui sont ex ante plus enclins à être rationné par les banques bénéficient plus de la hausse de la concurrence bancaire. Cela est notamment le cas des ménages afro-américains vivant dans des états ayant montré par le passé une certaine « préférence pour la discrimination » (Becker, 1995). Consistant avec l'hypothèse beckerienne que cet effet amplifié de la concurrence ne doit avoir lieu que si certains agents ont une préférence pour la discrimination, nous trouvons que l'effet de la hausse de la concurrence n'est pas différent entre ménage à faible revenu blanc et afro-américain vivant dans des états ayant historiquement peu (ou moins) de préférence pour la discrimination. Nous constatons également que les ménages plus pauvres (ceux vivants en dessous du seuil de pauvreté) bénéficient encore plus de la hausse de la concurrence bancaire, tout comme les ménages vivant dans des endroits ruraux où la concurrence bancaire est initialement plus faible. Enfin, nous trouvons que l'effet est plus important pour les ménages à faible revenu plus éduqués (c'est-à-dire étant diplômé du lycée). Ce dernier résultat nous parait particulièrement important, car ces ménages sont *a priori* moins enclins à ne pas être bancarisés en raison d'une trop faible éducation financière.

La troisième partie du papier s'intéresse aux effets sur l'accumulation de la richesse de cette baisse de la non bancarisation. Nous trouvons que les ménages ayant un revenu entre un et deux fois le seuil de pauvreté sont plus enclins à ouvrir un compte épargne rémunéré et à détenir de l'épargne sous d'autres formes (investissement dans des fonds d'investissement, *etc*.). Nous trouvons également que cela favorise l'accès au crédit au bancaire, sans que cela ne se traduise pas un plus fort d'endettement pour ces ménages. Ce dernier point suggère que la hausse de la concurrence bancaire durant cette période n'a pas conduit à une augmentation des « prêts prédateurs » sur cette population et indique donc que la non bancarisation est bien un frein à l'accumulation de richesse pour les ménages à faible revenu.

Nous concluons l'étude par de multiples tests permettant de rejeter l'hypothèse selon laquelle notre effet de la baisse de la non bancarisation ne serait qu'un « effet collatéral » d'une hausse de la richesse dans l'état produit par la déréglementation. Autrement dit, le fait que nous ne capturerions pas un effet d'offre mais un effet de demande induis, où parce que les habitants sont plus riches (par exemple parce que la déréglementation bancaire favorise la croissance) les ménages à faible revenu sont soudainement plus enclins à ouvrir un compte en banque. Nous montrons tout d'abord que notre effet n'est absolument pas affecté par le fait de contrôler pour de très nombreux proxies du revenu du ménage et par la situation économique de l'état : croissance économique, niveau de chômage par différents catégories de population (pauvre, à faible revenu, à revenu moyen, pauvre et africain américain, pauvre et blanc, *etc.*). Nous montrons également que l'effet est absolument similaire lorsque nous séparons l'échantillon de la population entre ménages ayant une plus ou moins forte probabilité d'être au chômage. Or si notre effet reflétait simplement un effet de demande, nous devrions trouver que les ménages ayant plus de chance d'être au chômage bénéficient plus de la déréglementation bancaire.

Chapitre trois. Les effets de spillovers d'innovation locaux des entreprises cotées.

Ce chapitre cherche à établir l'exigence des spillovers d'innovation, c'est-à-dire le fait que l'innovation d'une entreprise dans une zone géographique donnée va favoriser l'innovation des autres entreprises localisées dans la même zone géographique. Ce phénomène est important car il est une des justifications essentielles des investissements publics élevés en termes de subvention à la R&D ou bien afin de crée des « pôles de compétitivité ».

Malgré les dizaines de milliards investis par les gouvernements de par le monde, l'existence même de ces spillovers d'innovation est loin d'être évident en raison du « problème de réflexion » décrit par Manski (1993). En effet, des entreprises situées dans une même zone géographique sont par définition exposée à des chocs similaires, comme par exemple des chocs de productivité, des chocs de taxes locales ou bien la recherche produite par des universités locales. Si une université locale produit soudainement de bonnes recherches, cela augmentera l'innovation des entreprises environnantes, sans que ces entreprises ne produisent entre elles des spillovers. L'évolution commune de l'innovation ne sera que le « reflet » de ces facteurs locaux. Afin de séparer les spillovers d'innovation de ces facteurs locaux non observables, j'utilise les données de brevets aux Etats-Unis qui me permettent de savoir exactement dans quelle localité l'innovation a été produite, par quel inventeur et pour quelle entreprise.

J'exploite la grande dispersion géographique de l'activité d'innovation des entreprises cotées pour utiliser des chocs sur l'activité d'innovation des entreprises cotées qui ne provient pas de l'endroit où l'entreprise conduit son activité d'innovation. Cela permet ensuite d'étudier si ces variations de l'innovation des entreprises cotées (produites par un choc extérieur) actives dans une zone géographique donnée affectent l'innovation des entreprises non cotées situées au même endroit. Autrement dit, est ce que des variations exogènes de l'innovation des entreprises cotées produits des spillovers sur les entreprises non côtées situées dans la même zone géographique ?

Afin d'obtenir des variations exogènes sur l'innovation des entreprises cotées, j'utilise l'adoption par différents états, à différents moments de législation limitant les risques d'opération de fusion et acquisition hostiles. En limitant la possibilité pour un « raider » de faire une opération d'achat hostile, ces lois ont conduit à une baisse de la qualité de la gouvernance extérieure et permis notamment aux managers des entreprises cotées de « profiter d'une vie tranquille « (« enjoy the quiet life », Bertrand et Mullainathan, 2003), ce qui a notamment conduit à une diminution de leur activité d'innovation. L'adoption de cette loi me permet d'instrumenter l'innovation des entreprises cotées et d'étudier comment les entreprises non cotées (qui ne sont pas concernées directement par la loi) réagissent.

Mon premier résultat conclut à l'existence de spillovers d'innovation. Plus spécifiquement, ces spillovers sont positifs, large économiquement et très locaux. Ainsi, une variation de 1% de l'innovation par les entreprises cotées conduit à une variation similaire de 0.2% pour les entreprises non cotées situées dans la même zone géographique. En explorant la dispersion géographique de ces spillovers, je trouve que ceux-ci sont concentrés sur un périmètre compris entre 40 miles et 100 miles et qu'ils déclinent rapidement avec la distance. Au-delà de 100 milles, l'innovation conduite par les entreprises cotées ne produit plus aucun spillover.

La seconde partie du papier se consacre à l'analyse du principal canal par le lequel les spillovers d'innovation peuvent avoir lieu, à savoir le fait que la connaissance se diffuse localement entre entreprises. La littérature en innovation suggère deux canaux principaux par lesquels la connaissance peut se transmettre. Tout d'abord les entreprises locales peuvent apprendre les unes des autres via l'interaction de leurs employés. Ensuite les employés, en changeant d'entreprises mais en restant dans la même zone géographique vont agir comme vecteur de diffusion de connaissance.

Pour tester si les entreprises locales apprennent les unes les autres, j'exploite deux tests en cross-section. En effet, ce canal suppose que les effets de spillovers d'innovation devraient être plus forts lorsque la possibilité d'apprendre est plus importante. Autrement dit, la magnitude des spillovers devrait varier avec le degré de proximité technologique des entreprises locales et la densité des travailleurs éduqués. Ce dernier test repose sur l'argument avancé dans la littérature de l'économie urbaine selon lequel la capacité des travailleurs à échanger, assimiler et appliquer de nouvelles connaissances devrait être plus grande. Dans les deux cas, je trouve qu'une plus grande proximité technologique et qu'une densité de travailleurs éduqués amplifient bien la magnitude des spillovers d'innovation.

Afin de tester le deuxième canal de transmission de la connaissance, à savoir la mobilité des employés, j'utilise deux stratégies. La première consiste à exploiter la base de données de la Harvard Business School sur l'ensemble des inventeurs déposant un brevet aux Etats-Unis. Cette base contient un identifiant unique pour chaque inventeur, ce qui permet de les suivre aussi bien dans le temps qu'entre entreprise ou localité. Cela me permet de montrer que les variations d'innovation par les entreprises cotées dans une zone géographique conduisent à des variations similaires du nombre d'inventeurs allant des entreprises cotées vers les entreprises non cotées dans la même zone géographique.

La seconde stratégie exploite des variations dans le taux de mobilité des employés, avec l'intuition qu'un taux de mobilité plus élevé implique que la connaissance se diffuse plus rapidement. Les variations de taux de mobilité proviennent de l'existence de clauses de non concurrence qui limite la mobilité des employés au sein d'un état et du fait que les états américains différent dans l'encadrement de ces clauses. Consistant avec l'hypothèse que la mobilité favorise la diffusion de l'information ce qui accroit les spillovers d'innovation, je trouve que les états qui restreignent fortement la possibilité pour l'employeur de mettre en place ces clauses connaissent des spillovers d'innovation plus forts.

La troisième partie du papier porte sur l'analyse du lien entre spillovers d'innovation et investissement par les fonds de venture capital. Je trouve que ce lien va dans les deux sens. D'une part, l'existence de spillovers d'innovation incite les fonds de venture capital localisés en dehors de la région à investir là où es spillovers d'innovation sont plus important. D'autre part, un accès plus facile au financement par les fonds de venture capital amplifie la magnitude des spillovers d'innovation, en donnant aux entreprises non cotées innovantes les moyens de financer leurs projets d'innovation. Parce que le montant d'investissements disponibles par les fonds de venture capital a toute chance d'être endogène, j'instrumente ce montant en utilisant la taille du fond de pension des fonctionnaire de l'état dans lequel est situé le fond de venture capital. En effet, plusieurs papiers ont montré que les états tendent à investir un montant disproportionné de leur argent dans des fonds d'investissements locaux (e.g. Hochberg et Rauh, 2012). L'utilisation de cet instrument confirme qu'une hausse des montants d'investissements des fonds de venture capital amplifient les spillovers d'innovation.

Chapitre 4. Les managers surréagissent-ils aux risques saillants ? Une preuve par les ouragans

Ce travail réalisé en collaboration avec Olivier Dessaint, professeur Toronto Rotman Business School montre que même des agents économiques sophistiqués comme les managers professionnels d'entreprises cotées peuvent être victime du « biais de disponibilité » identifié par Tversky et Kahneman (1974). Pour ces auteurs, l'estimation de l'occurrence d'un risque dépend de la facilité avec laquelle nous sommes capables de penser à un évènement similaire. Dès lors, l'expérience personnelle et la saillance de l'évènement vont affecter l'estimation du risque. Ainsi, si l'évènement est très saillant (par exemple parce que l'agent a été confronté à un cas similaire récemment), il aura tendance à surestimer son occurrence futur.

Afin de tester ce biais, nous utilisons les ouragans aux Etats-Unis comme une source de variation sur la saillance du risque d'un choc de liquidité pour les entreprises. Les ouragans ont trois caractéristiques importantes. Tout d'abord, ils représentent un risque stationnaire, ce qui implique pour les agents ne devraient inférer aucune nouvelle information sur la probabilité d'être affecté dans le futur s'ils ont été à proximité d'un ouragan dans le passé. Ensuite, ils constituent un évènement à la fois très localisé et saillant. Enfin, ils créent un choc de liquidité pour l'entreprise affectée, ce qui implique que les managers dont l'estimation du risque augmente devraient augmenter le niveau de cash de leur entreprise (e.g. Holmstrom et Tirole, 1998). Nous pouvons donc utiliser les variations de détention de cash comme un proxy de l'estimation du risque d'un choc de liquidité par les managers.

Nous considérons qu'une entreprise connait une hausse de la saillance du risque si son siège social se trouve dans la zone périphérique d'un ouragan mais n'a pas été directement affecté par l'ouragan. En comparant ces entreprises avec celles plus éloignées, nous trouvons que les entreprises voisines de l'ouragan augmentent de façon temporaire leur détention de cash pendant une période d'environ deux ans, avec un pic après un an. Au niveau le plus élevé, les entreprises dans la zone voisine ont augmenté leur niveau de cash de 1% de leur actif (10% de l'écart-type intra entreprise) relativement aux entreprises qui sont plus éloignées du choc. Puis, à mesure que le temps passe, la saillance de l'évènement décroit et le niveau de cash retombe à son niveau d'avant l'ouragan. Nous trouvons par ailleurs que cette réaction est amplifiée lorsque les managers ont plus de chance d'être moins sophistiqués (par exemple les managers de petites entreprises, d'entreprises jeunes ou n'ayant jamais par le passé été à proximité d'un ouragan) ou bien lorsqu'elles sont plus exposées au risque de choc de liquidité car elles sont plus contraintes financièrement.

Nous analysons ensuite le coût de cette décision pour les actionnaires et trouvons que le choix d'augmenter le cash est coûteux pour ces derniers. En utilisant la méthode développée par Faulkender et Wang (2006), nous trouvons que la valeur de marché du cash détenu par ces entreprises diminuent, suggérant que les actionnaires considèrent que ces liquidités auraient été mieux employées autrement.

La fin du papier est consacrée à discuter les principales explications « rationnelles » de notre résultat : la possibilité d'un changement du risque sous-jacent, l'apprentissage de la distribution du risque par les managers et enfin des spillovers régionaux. Les deux premières explications n'apparaissent pas consistante avec une hausse temporaire du cash, car elles impliquent que le niveau optimal de détention de cash a augmenté de façon permanente. La hausse temporaire du cash pourrait en revanche s'expliquer par des spillovers régionaux. Si les entreprises sont fortement dépendantes des marchés locaux, même si elles n'ont pas été directement affectées, le fait que les zones voisines l'aient été pourrait soit créer de nouvelles opportunités d'investissement (si leurs concurrents directs ont été touchés) soit au contraire ralentir leur vente. Hors, nous ne trouvons absolument aucun effet ni sur l'investissement, ni sur la croissance des ventes, ce qui montre bien que ces entreprises augmentent leur niveau de cash alors que la situation est pour elles « business as usual ».

Abstract

Abstract This dissertation is made of four distinct chapters. The first chapter with Johan Hombert shows that when lending relationships are hurt, it reduces the number of innovative firms and foster inventor mobility who move out of geographical areas where lending relationships are hurt. The second chapter presents a work with Claire Celerier and shows that supply-side factors account for a large part of the unbanked household phenomenon in the US. The third chapter studies spillovers of innovation and show that when some firms innovate less other firms in the same city innovate less in response and this effect declines sharply with distance. The fourth chapter with Olivier Dessaint presents evidence that managers systematically respond to near-miss liquidity shocks by temporarily increasing the amount of corporate cash holdings.

Keywords: knowledge spillovers, patents, corporate governance, banking deregulation, household finance, behavioral finance

Résumé

Cette thèse est constituée de quatre articles. Le premier article avec Johan Hombert montre que lorsque les relations bancaires sont affectées, cela réduit le nombre d'entreprises innovantes et conduit également à une hausse de la mobilité géographiques des inventeurs, qui quittent les états oú les relations bancaires sont dégradées. Le second article est un travail avec Claire Célerier mettant en avant le rôle de l'offre dans le phénomène de non bancarisation des populations pauvres aux Etats Unis. Le troisième article étudie les externalités d'innovation et montre que lorsque certaines entreprises innovent moins les autres entreprises locales innovent moins en réponse. Cet effet décroit rapidement avec la distance. Le quatrième article, en collaboration avec Olivier Dessaint, montre que les mangers répondent systématiquement à des chocs de liquidité proches d'eux en augmentant temporairement leur trésorier.

Mots-Clefs: externalités de connaissance, brevets, gouvernance d'entreprise, dérégulation bancaire, finance des ménages, finance comportementale