Essays on Empirical Banking

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Chapter 1

Introduction

Regulation is an integral part of well-functioning financial systems (Barth et al. (2004)). In fact, the financial sector is regulated more than many other sectors because it is prone to market failures with disastrous consequences, such as bank failures and financial crises (Barth et al. (2008)). Also, the institutions and actors of the financial sector are highly interconnected both within countries and globally. Therefore, they are susceptible to the spread of market disturbances. As such, financial regulation has many important functions to fulfill, ranging from addressing contagion and aligning the incentives of unsophisticated users of financial products and sophisticated sellers to preventing negative social externalities emanating from costs of failure of a financial institution being in excess of private costs to the shareholders (Levine (2011)).

However, the consequences of a particular financial regulation may not always match with the intended policy objectives (Levine (2011)). For example, an important stabilizing attribute of financial regulation, deposit insurance, aims to protect uninsured depositors and reduce the risk of systemic bank runs (Dewatripont and Tirole (1994); Diamond and Dybvig (1983)). However, deposit insurance faces major challenges as it fails to restore depositor monitoring of banks and incentivizes banks to take excessive risk by weakening the link between banks' default risk and cost of funding (Kane (1989); Calomiris (1999); Ioannidou and de Dreu (2006)). A deposit insurance reform that fails to ensure depositor confidence could still lead to bank runs

(Martinez-Peria et al. (2001)). Another example comes from the strand of literature studying the effects of banking deregulation since the mid-1970s in the United States. Several studies have shown that interstate branching deregulation accelerated state-level economic growth (Jayaratne and Strahan (1996)) and reduced poverty (Beck et al. (2010)), while others pointed to deregulation episodes being associated with higher wage inequality (Jerzmanowski and Nabar (2013)).

This dissertation presents three chapters that explore the effects of selected financial reforms and regulations on financial intermediation as well as the real effects of bank failures in a highly deregulated banking environment. Chapter 2 concerns the effects of deposit insurance in banking systems where Islamic and conventional banks operate side by side. It looks at the depositors' deposit supply and disciplining behavior in six banking systems after the announcement of a split of deposit insurance administration between Islamic and conventional banks promoted in the aftermath of the 2008 financial crisis to restore depositor confidence. This regulatory intervention allows Islamic banks to be covered by a Sharia-compliant deposit insurance scheme. The chapter sheds light on how banking reforms shape depositors' financial decision making and sensitivity to financial risk. Chapter 3, unlike the other chapters, does not study the effects of a particular banking reform. It rather investigates the effects of bank failures following a period of high deregulation on wage inequality during the recent financial crisis in the United States. Finally, Chapter 4 studies how a regulatory shock (the repeal of Regulation Q in the United States after the crisis) to funding costs affects banks' branch networks and geographic diversification. It sheds light on how banking regulations put in place in the aftermath of the recent financial crisis have affected bank conduct and strategies. In what follows, I give an overview of each chapter.

Chapter 2 is titled Religious Incentives in the Deposit Market. It is solely my own work. In the aftermath of the recent global financial crisis, the Basel Committee on Banking Supervision and the International Association of Deposit Insurers urged Islamic banks to adopt a Sharia compliant deposit insurance scheme to align the Islamic

financial Institutions with their core business models, based on re (IADI, 2014). In effect, several jurisdictions where Islamic and conventional banks operate side by side (Bahrain, Jordan, Indonesia, Kuwait, Malaysia and Turkey) announced separation of their deposit insurance schemes to allow Islamic banks to have a Sharia administered deposit insurance fund. I exploit that these regulatory changes happened in a staggered way to explore the role of religion on depositors' decision making.

I observe that after the announcement of a separate Islamic deposit insurance scheme, deposit growth increases and deposit return decreases differentially for treated banks. These results are both statistically and economically significant. The announcement is associated with an approximate 2.8 percentage points increase in deposit growth and a 0.8 percentage points decrease in deposit return. These results suggest that the observed effects are supply-driven. Furthermore, these results are robust to standard error clustering adjusted for a small number of clusters and hold even after controlling for country dependent and time varying shocks, religiosity and political governance indicators. I attribute these findings to the fact that Islamic depositors rewarded their banks after the announcement of a Sharia-compliant scheme due to reduction of the risk of Sharia noncompliance.

I also find that although bank risk-taking did not significantly change after the separation announcements, Islamic depositors' reaction to risk changed. A conventional deposit insurance scheme covering both Islamic and conventional deposits may face difficulties in convincing depositors about the Sharia conformity of reimbursed funds in the event of bank insolvency (Sole (2007); Aysan et al. (2017)). By comparing the degree of disciplining against bank insolvency risk (measured by time-varying Z-score) before and after the separation announcements, I find that the depositor disciplining disappears after the announcement of an Islamic deposit insurance scheme.

Overall, my findings indicate that Islamic deposit insurance reforms promoted in the aftermath of the 2008 global financial crisis were effective in restoring depositor confidence (as evidenced by a supply-driven increase in Islamic banks' deposits) but hampered depositor disciplining against banks' financial risk-taking. Alternatively,

these results can be interpreted as ethical appeals, such as religiosity playing a role in depositors' financial decision making and determining depositors' sensitivity to bank risk. As such, to my knowledge, this is the first study that examines the effects of religious branding of financial reforms on non-pecuniary incentives in the financial markets.

Chapter 3 is titled Bank Failures and Wage Inequality. It is a joint work with Christopher Martin Hols from the Bonn Graduate School of Economics. The literature examining the roots and consequences of the correlations between financial distress and wage inequality has proliferated in the last decade. Beck et al. (2010) and Jerzmanowski and Nabar (2013) studied the correlation between financial deregulation and income and wage inequality. The former found that deregulation decreased poverty, whereas the latter showed that deregulation increased wage inequality. However, to date, there are no studies examining the association between bank failures and wage inequality. In Chapter 3, we attempt to fill this gap in the literature. In particular, we investigate the effects of bank failures on wage inequality.

It is important to answer this question because financial distress plays a critical role in shaping the gaps between the certain segments of the population (Demirgüç-Kunt and Levine (2009)). It can influence who can start and run a business and who cannot, who can afford education and who cannot, and essentially, who can realize one's economic desires and ambitions and who cannot. Furthermore, by affecting the allocation of capital, financial distress can change both the rate of economic growth and the demand for labor, with potentially big distributional implications (Townsend and Ueda (2006)).

In Chapter 3, we focus on investigating the effects of bank failures on wage inequality between skilled and unskilled workers. We exploit the geographical variation in bank failures across Public Use Micro-data Areas (PUMA) in the United States. To that end, we make use of American Community Survey data to elicit demographic and labor market characteristics of individuals in a repeated cross-section framework. This enables us to use several multi-way fixed effects that control for all time-varying

community, sector as well as community and sector differences that capture underlying economic conditions across each industry within a PUMA.

We run multiple Mincerian skill-wage regressions and show for the period of 2008-2010 that local bank failures explain 6% to 10% of the annual wage gap between skilled and unskilled workers. Simultaneous consideration of annual work hours, wages, employment status as well as labor force status of individuals suggests that the observed effects can be attributed to the labor demand. In particular, communities with bank failures (relative to unaffected communities) do not incur reductions in hourly wages for unskilled workers but experience a decline in labor demand through a reduction in annual working hours for unskilled workers, translating into declines in annual wages for unskilled workers more than for skilled workers.

In the next step, we identify a unique channel for the decline in labor demand. Specifically, we exploit the sector-level variation in the use of knowledge-dependent capital, which we determine by the ratio of intangible capital to overall capital. The underlying idea is that sectors that rely more on intangible (knowledge-dependent) capital are expected to have more elastic demand for unskilled workers. At the same time, these sectors are less able to pledge collateral as sectors relying on tangible capital. Hence, when credit shocks induced by bank failures hit the economy, relatively affected sectors (sectors with intangible capital) would shed unskilled labor more so than unaffected sectors. Our results suggest that the size of the effect bank failures on the skill premium when the ratio of intangibility to tangibility increases by one standard deviation from the mean is 4.1% to 8.1%.

These results open up a new channel, bank failures, for the widening of the skill premium. They also indicate a potential mechanism of how shocks in the financial markets can be transmitted to labor markets. To our knowledge, we are the first to show that knowledge-dependent capital matters for the transmission of such shocks with distributional consequences. Also, we are the first to study the correlation between bank failures followed by a period of financial deregulation and wage inequality.

Chapter 4 is titled The Net Interest Margin and the Branch Network. It is a joint work with Christopher Martin Hols from the Bonn Graduate School of Economics. It studies how banks' funding costs can affect geographical diversification of banks by exploring the regulatory shock to funding costs in the United States in the aftermath of the recent financial crisis.

There is extensive literature on how geographical bank branch network diversification affects banks' funding costs. Diamond (1984) and Boyd and Prescott (1986) suggest that geographic diversification can bring additional assets that are imperfectly correlated with existing assets and this can reduce bank risk and lower funding costs. Similarly, if banks spread to diverse geographic areas where the economies are imperfectly correlated with the bank's existing local economy, they will be able to use internal capital markets to respond effectively to local shocks on asset quality and liquidity (Houston et al. (1997); Houston et al. (1998); Gatev et al. (2009) and Cornett et al. (2011)). Other studies investigate how geographical bank branch network diversification can result in increased funding costs. For example, Brickley et al. (2003) and Berger et al. (2005) show that distance can make it difficult for bank headquarters to monitor subsidiaries, which can have a negative impact on efficiency, asset quality and funding costs.

However, the literature is largely mute on the causality going in the opposite direction, i.e., how funding shocks translated into changes in net interest margins affect geographical bank branch network diversification. In fact, a reduction in the net interest margins can "... reduce gross value of core deposits and given that branches have non-interest expenses, maintaining deposit relationships could become a negative present value business" (Claessens et al. (2017)). If an increase in the funding costs indeed affects banks' geographical branch network diversification, consequences other than for banks' profits can follow. For example, the literature on banks' interstate expansion in the United States suggests that it accelerates the economic growth and reduces the poverty rates of individual states (e.g. Jayaratne and Strahan (1996); Beck et al. (2010)).

Using the repeal of Regulation Q (i.e. the ban on interest payments on commercial checking accounts) as a natural experiment, we study the causal relationship between banks' funding costs and the number of branches they have. In the United States, interest payments were heavily regulated in the aftermath of the Great Depression. Interest payments on time deposit accounts and savings deposits were capped until 1986 and interest payments on demand deposits were abolished altogether until 2011. The partial repeal of Regulation Q in 2011 allowed banks to pay interest on demand deposits. We use this intervention as an exogenous variation in a natural experiment as it increased the interest expenses for banks that relied heavily on funding through demand deposits more than for banks that relied to a lesser extent on demand deposits. The reform led to a reduction in the net interest margin of affected banks of around 0.4 percentage points annually, which is nearly on par with the decline in the net interest margin between 2009 and 2015.

We document that the banks more reliant on demand deposits before Regulation Q was repealed decreased their number of branches substantially by around a third of a branch per bank. The overall effect is around 670 branches, which corresponds to 10% of affected banks' aggregated branch network. Extrapolating our results, we can explain a decline in the aggregated branch network of around 1600 branches, which corresponds to a quarter of the aggregated decline in the branch network since 2009. Interestingly, we find that the affected banks managed to reduce asset risk, which was to some extent achieved by withdrawing branches from poorer neighborhoods. Our results on banks' time-varying Z-scores suggest that banks might become safer after the reform. However, banks also decreased their capital ratios following the regulatory intervention, making an overall risk assessment inconclusive.

Chapter 2

Religious Incentives in the Deposit Market

2.1 Introduction

While the literature on finance pays great attention to the use of monetary incentives to shape investors' behavior, circumstances that investors face are also abound with examples in which they are attracted through non-monetary incentives and, in particular, through appeals of an ethical kind such as religious and moral norms. Instances of such appeals driving financial decision-making are ubiquitous. The value of ethically driven financial activities, including socially responsible investing, green finance, religiously-compliant investing and the like has grown by a factor of six over the last decade.[1] However, despite the recognition that ethical appeals affect financial choices, empirical evidence remains scarce.

In this chapter, I study the effects of religion on financial decision-making. To this end, I exploit a unique natural experiment that involves Islamic banks as the unit of analysis. Islamic banking is one of the fastest growing segments of the global financial market, with Islamic financial assets having more than doubled in size since 2006 and Islamic banks spreading across over 75 countries (World Bank (2014)). Islamic banks

[1] Ethical Futures (2016) presents the soaring trend in the ethical market from a historical perspective.

offer various products that conform to the Islamic law (Sharia) and highlight the ethical aspect of their business models. Their prevalence suggests that consumers may have religious considerations when making financial decisions.

In my natural experiment, I concentrate on six jurisdictions where Islamic and conventional banks operate side by side, and where regulators have announced administrative separation of deposit insurance funds between conventional and Islamic banks.[2] This separation allows Islamic depositors to be covered by a Sharia-compliant deposit insurance scheme, which was formerly not the case. For the majority of the countries, the reform only concerned administrative separation was not associated with any other regulatory changes.[3] Time variation in the announcement of separation of deposit insurance funds across countries enables me to run a staggered difference-in-differences (DID) model where units belong to both control and treatment groups at several points in time.[4] By comparing Islamic banks operating in different countries at different points in time, the experiment tests how depositors of Islamic banks react to additional religious compliance induced by a separate Islamic deposit insurance scheme.

Theoretically, it is not *ex-ante* clear how Islamic depositors would react to the religiosity of their banks induced by Sharia-compliant deposit insurance schemes. On the one hand, the theory of market discipline suggests that given that depositors are incentivized to monitor their banks, they engage in disciplining by rewarding or punishing their banks for their relative performance (Calomiris and Kahn (1991); Diamond and Rajan (2001)). Interestingly, the quasi-equity contract structure of Islamic deposits (*Mudarabah*) may ensure that Islamic depositors intensely monitor their banks (Errico and Farahbaksh (1998); El-Hawarey et al. (2004); Beck et al. (2013)).[5] Therefore, so

[2]Jurisdictions in which both conventional and Islamic banks operate are defined as dual banking systems. My experiment includes Bahrain, Indonesia, Jordan, Kuwait, Malaysia and Turkey as dual banking systems.

[3]Only for Bahrain, the separation announcement came alongside a change in the coverage of the deposit insurance scheme. However, I conduct a triple differences analysis and show that this does not contaminate my results.

[4]Staggered DID models are used extensively in financial intermediation literature. Beck et al. (2010) and Haselmann (2011) are two examples.

[5]*Mudarabah* is based on a profit-loss sharing mechanism (PLS). Under a PLS arrangement, borrow-

far as monitoring incentives are concerned, one can expect that a Sharia-compliant deposit insurance scheme would be rewarded by Islamic depositors.

However, the reaction of depositors to Sharia-compliant deposit insurance schemes may also depend on the level of religious devotion (Aysan et al. (2017)). The literature on social and behavioral finance has distinct views on this issue. On the one hand, religious people tend to be more risk-averse (Miller and Hoffmann (1995); Hillary and Hui (2009)). This might make them prefer an environment where the risk of Sharia non-compliance is low. On the other hand, Abedifar et al. (2013) argue that Islamic depositors may be more loyal. Similarly, Webley et al. (2001) show that investors with ethical concerns tend to be over-committed even in the case that their investment operations deviate from the ethical criteria. Thus, loyalty and over-commitment might numb Islamic depositors' sensitivity to Sharia compliance. Therefore, depending on the effect of religion on individuals (piety, peer and network effects, social belonging and shaming and the like), Islamic depositors may or may not react to a Sharia-compliant deposit insurance scheme.

Taking stock of these predictions, I estimate the *average treatment effect on the treated* of separation announcement on deposit growth and return in Islamic banks for the period 2007-2015. Considering deposit growth (quantity) and return (price) simultaneously enables me to observe whether the effects are demand- or supply-driven (Ioannidou and de Dreu (2006)). My baseline findings suggest that following separation announcements, deposit growth increases while deposit return declines for Islamic banks. In other words, depositors reward additional Sharia compliance induced by separation of deposit insurance funds in that they increase deposited amounts and accept lower returns on their deposits, suggesting that the observed effects are supply-driven.

To better understand the dynamics of separation announcement on the outcome variables, deposit growth and return, I apply several placebo treatments where I as-

ers share profits and losses with banks, which in turn, share profits and losses with depositors. In a way, depositors become residual claimants rather than creditors of banks. Such a business model makes Islamic banks prone to agency problems in the deposit market.

sign the treatment one, two and three years before and after the actual intervention took place. For placebo treatments preceding the actual treatment, I observe that differences in the outcome variables between treated and control banks are statistically zero. For placebo treatments after the actual treatment, however, I observe systematically significant differences in outcome variables between treated and control banks, which supports previous findings. This observation enables me to show evidence in favor of the satisfaction of "the parallel trend" assumption of DID setups. Such a placebo treatment also reiterates that separation announcement is the mostly reason for the observed supply-driven increases in deposits and other channels such as improvements in trust within Islamic banks are very unlikely, unless the timing of these alternative channels exactly correspond to my treatment years.

Besides, I concentrate on the timing of the separation announcements. I conduct a thorough desk review on whether deposit insurance separations in my sample countries happened as part of larger financial reforms that ultimately increased deposit supply. Here, anecdotal evidence suggests that in all the majority of countries in my sample, deposit insurance separation is promoted as a stand-alone regulatory program.[6] In addition, I test whether religiosity or the ability of a country to make laws potentially confounded the timing of the separation announcement. Controlling for these factors in my regressions, I verify the baseline findings that depositors of Islamic banks reward the additional Sharia-compliance induced by separation of deposit insurance funds.

In the next step, I study the channel through which the rewarding behavior of Islamic depositors occurs. Sóle (2007) argues that since the funds of a common deposit insurance scheme, which lacks Sharia compliance mechanisms, may be invested in interest-bearing assets, concerns are raised about its religious conformity. Thus, a conventional insurance scheme covering both Islamic and conventional deposits may face difficulties in convincing depositors about the reliability of reimbursed funds when

[6]Section 4 provides a discussion on the institutional framework leading to separation of deposit insurance schemes at the country level.

an Islamic bank failure occurs (cross-subsidization channel) (Aysan et al. (2017)). That is, Islamic depositors could be affected adversely by a conventional deposit insurance scheme only when their banks fail. By comparing the degree of disciplining against bank insolvency risk (measured by time-varying Z-score) before and after the treatment, I find that the former market discipline disappears after the announcement of an Islamic deposit insurance scheme. Therefore, I attribute this finding to the presence of a cross-subsidization channel.

As an alternative identification strategy, I utilize conventional banks as an additional control group to Islamic banks and employ a triple-differences methodology to control for time-varying heterogeneity across countries. The underlying idea is that conventional banks operating in the same countries would have similar characteristics to Islamic banks, but they would not be necessarily affected by additional Sharia compliance induced by separation of deposit insurance funds. Estimation results from triple-differences regression verify that the baseline findings, that Islamic depositors reward their banks and the channel of rewarding is cross-subsidization, still hold.

Improvements in religious compliance can potentially influence Islamic depositors' behavior in two ways: Piety or religious identity (Pepinsky (2010)). For pious individuals, separation of deposit insurance funds to allow Islamic depositors to be covered by Sharia-compliant schemes can be seen as morally "right". However, the choice of Sharia-compliant products could be part of an outward expression of religious identity and due to peer effects or even social shaming as documented in similar environments (DellaVigna et al. (2012); Perez-Truglia and Troiano (2016)). It can be expected that for pious individuals to act, the actual religious compliance matters, as opposed to the announcement of future religious compliance. Note that my analysis concentrates on the announcement of separation of deposit insurance funds rather than the event of actual separation due to data limitations. In a research design allowing one to observe the effects of both the announcements and the actual separations, one could potentially disentangle piety from religious identity. As I am not able to measure the effects of the actual separation announcements, the explanatory power

of my findings regarding the effects of piety on depositors is unclear. However, my findings can at least suggest that factors related to religious identity (peer and network effects, social belonging and shaming, etc.) may explain depositors' rewarding behavior more than piety or religious morality.

Overall, my findings indicate that improvements in religious compliance induced by separation of conventional and Islamic deposit insurance funds are morally persuasive from Islamic depositors' perspective. This suggests that ethical appeals such as religion affect financial decision-making, most likely through religious identity. This provides a suggestive rationale for the increasing trend in ethical financial activities. My findings also demonstrate the trade-off between the availability of funds and the presence of market discipline in designing deposit insurance schemes in banking sectors where religious sentiments play a role.

This chapter contributes to several strands of the literature. First, my work is related to the well-established literature on non-monetary incentives (Frey (1997); Akerlof and Kranton (2000); Gneezy (2005); Bénabou and Tirole (2003), (2006)). Non-pecuniary appeals, especially moral ones, are commonly used tools of persuasion and many companies' corporate social responsibility strategies are directed towards gaining reputation through business models incorporating charitable causes to attract consumers. I contribute to this literature by showing evidence from the dimensions of religious morality in a banking regulation setup and how this makes Islamic depositors reward their banks following the improvements in Sharia compliance.

Second, my work contributes to the literature studying deposit insurance and market discipline (Flannery (2001); Martinez-Peria et al. (2001); Demirgüç-Kunt and Huizinga (2004); Ioannidou and de Dreu (2006)). This literature finds that introduction of deposit insurance numbs the monitoring and disciplining incentives of depositors against bank risk, exacerbating moral hazard. I contribute to this literature by providing evidence on how depositors' disciplining works in Islamic banks where religious sentiments shape financial choices. There is also a body of evidence indicating that depositors monitor not only their banks but also the credibility of deposit

insurance schemes (Flannery (1998); Martinez-Peria et al. (2001)). I also complement this stock of evidence by finding that for Islamic banks, deposit insurance is credible when Sharia compliance is ensured.

Beyond shedding light on the role of non-pecuniary incentives in financial decision-making, this chapter also contributes to a broader literature on economic behavior and religion (Iannaccone (1998); Barro and McCleary (2006), Clingingsmith, Khwaja and Kremer (2009); Hilary and Hui (2009); Cantoni (2015); Campante and Yanagizawa-Drott (2015); Bénabou et al. (2015); Benjamin et al. (2016); He and Hu (2016)). Identifying the effects of religion on economic behavior is difficult using observational data for several reasons. First, it is hard to observe religiosity as exogenous treatment. Secondly, in the absence of counter-factual, second-order effects of religion on personal characteristics, such as risk aversion, commitment and loyalty, could confound the first-order effects of religion, such as religious identity or faith, on economic behavior. My work contributes to this literature by comparing Islamic banks with each other and establishing the causal link between religious compliance of financial institutions and commitment of depositors through an arguably exogenous event.

Finally, this chapter contributes to a small but growing body of literature on Islamic banking. This literature mainly studies the business models, efficiency, effectiveness and stability of Islamic banking (Abdull-Majid et al. (2010); Čihák and Hesse (2010); Khan (2010); Ongena and Sendeniz-Yuncu (2011); Beck et al. (2013); Aysan et al. (2017)). There is also a growing body of evidence that show the effects of religion on credit relationships in Islamic banking setups (Baele et al. (2014); Beck et al. (2017)). I contribute to this literature by showing evidence on the effects of religion in Islamic deposit markets.

The remainder of this chapter is structured as follows: Section 2 summarizes the fundamental characteristics of Islamic banking and Islamic deposit insurance schemes. Building on this institutional knowledge, Section 3 develops the main hypotheses of the chapter. Section 4 discusses the sample selection and specifies the empirical models for hypothesis testing. Section 5 presents the empirical results and

robustness tests. Section 6 summarizes the findings and concludes.

2.2 Background

2.2.1 Islamic Deposit Contracts

Islamic banking is a system of banking practices that are in line with the rules of Islam or Sharia, which are based on the Qur'an or the sayings of the prophet Mohammad. The key elements that differentiate Islamic banks from conventional banks are mainly the prohibition of gambling and the interest (*riba*) as well as the emphasis on risk sharing. Possessing these principles, Islamic deposit contracts (*Mudarabah*) are different to conventional deposits. An Islamic deposit contract is a partnership agreement between bank and depositor, with profits and losses of investment being shared in jointly agreed ratios (Aysan et al. (2017)). This special contract arrangement is manifested in the liability side of an Islamic banks' balance sheet as an investment account that does not bear a predetermined interest rate but rather delivers a share of profits. In fact, an Islamic deposit account owns neither debt-based nor equity-based compensations. Thus, potential agency problems are embedded in the contract structure of Islamic deposits. They arise from the separation of cash flow and control rights for investment account holders (Saifieddine (2009)). Even though depositors are not creditors but residual claimants of banks, they are not given the control rights under a Mudarabah arrangement (Aysan et al. (2017)).

2.2.2 Conventional Deposit Insurance from a Sharia Perspective

The role of conventional and Islamic deposit insurance schemes is largely similar, whereby a deposit insurance fund collects premiums from banks and protects the insured deposits. The major difference between Islamic and conventional deposit insurance schemes is that only permissible expenses are paid by the Islamic fund and the fund is invested in Islamic instruments, which are generally in the form of *Sukuk*

issued or guaranteed by the governments (Arshad (2011)). *Sukuk* refers to shares in the ownership of underlying real assets relating to investment activity. A *Sukuk* holder keeps a common share in the ownership of the assets associated with the investment. In effect, *Sukuk* holders are entitled to a share in the revenues earned through the underlying assets.

There are two types of Islamic deposit insurance practice around the world: Insurance of Islamic deposits under a conventional deposit insurance scheme and Insurance of Islamic deposits by a Sharia-compliant deposit insurance scheme. The evaluation of the former from the perspective of Sharia is done based on the following grounds: how a conventional deposit insurance scheme generates funds and how it utilizes those funds (IADI (2010)).

Firstly, moral purity of all transactions is at the center of Islamic finance. In that sense, Sharia-compliant funds should not be mixed (commingled) with those of non-Islamic funds (Sóle (2007)). Although it is not *ex-ante* clear if commingled funds are morally impure from a Sharia perspective, the practice of interest-bearing investment of collected funds in a conventional deposit insurance scheme raises concerns regarding Sharia conformity. Therefore, management of Islamic deposits under conventional deposit insurance schemes lacks a control mechanism to ensure the separation of funds.

Secondly, utilization of commingled funds in the case of an Islamic bank failure (cross-subsidization) is considered to be at odds with Sharia compliance. Thus, a conventional deposit insurance scheme covering both conventional and Islamic deposits may face difficulties in convincing depositors about the reliability of reimbursed funds in case of an Islamic bank failure (Aysan et al. (2017)). For these reasons, separation of deposit insurance funds may signal a radical departure from conventional banking towards Sharia-compliant banking.

2.3 Hypothesis Development

In the deposit market, Islamic banks' depositors are concerned with various risks. Apart from the financial risk, one particular risk characteristic endemic to Islamic banks is the Sharia risk, which can be defined as the operational risk of deviating from the rules of Islam (Ginena (2013)).

An announcement of separation of deposit insurance funds between conventional and Islamic banks to allow the latter to have a scheme under Sharia control improves religious compliance, or to put it differently, reduces Sharia noncompliance risk. I formulate two hypotheses on how Islamic depositors would react to the introduction of a Sharia-compliant deposit insurance scheme.

How would Islamic depositors react to the decline in Sharia risk due to a separation of deposit insurance funds?

Theoretically, depositors monitor their "investment" by evaluating the activities of banks to ensure that their investments are not in danger. The underlying theory of market discipline suggests that depositors would ask for higher returns and withdraw their funds in response to excessive risk-taking by banks. Banks, needing to maintain a steady flow of reasonably priced working capital, would then have incentives to decrease excessive risk-taking. The empirical literature also supports the theory that financial risk characteristics and deposit growth (deposit return) are negatively (positively) correlated (Martinez-Peria et al. (2001); Demirgüç-Kunt and Huizinga (2004)).

Recent corporate finance theories argue that corporate governance is increasingly based on entry-exit strategies (Aysan et al. (2017)). In the absence of control rights (as in Mudarabah contracts) dispersed blockholders may have the ability to govern firms (Edmans (2009); Admati and Pfleider (2009); Edmans and Manso (2011)). This indirect governance mechanism is based on disciplining through trading, i.e., an entry strategy upon positive information and an exit strategy upon negative information.

A governance and disciplining mechanism similar to an entry-exit strategy is especially relevant to Islamic banks. Based on the contract structure of Islamic deposits,

depositors are considered as "quasi-shareholders". Although deposits are not tradable, they are liquid. Thus, Islamic depositors can reward or punish their banks by increasing or decreasing the supply of deposits, respectively, depending on the level of risk taken.[7] Such a governance strategy can discipline banks, even though the individual depositors do not have direct control over the operation of the bank. Thus, the decline in Sharia risk resulting from a separate Islamic deposit insurance fund is expected to be related to deposit growth positively and return on deposits negatively. In other words, depositors may reward improvements in Sharia compliance induced by a separate deposit insurance fund by increasing their funds deposited and by asking for lower returns.

However, the degree of religious devotion may also play a crucial role in determining whether Islamic depositors would reward their banks for compliance with Sharia. The literature on behavioral finance has different views on this topic. On the one hand, religious agents tend to be more risk-averse than non-religious agents (Miller and Hoffman (1995); Hilary and Hui (2009)). This would imply that indeed low levels of Sharia risk are preferable for Islamic depositors. On the other hand, Abedifar et al. (2013) posit that Islamic depositors may have a strong sense of loyalty. Similarly, experimental economics literature shows that ethical investors tend to be over-committed to their investment even if it operates outside the ethical criteria (Webley et al. (2001)). These arguments on loyalty and over-commitment may numb the sensitivity of Islamic depositors to Sharia risk. In light of this argumentation, I present the first hypothesis of the chapter:

Hypothesis 1: Conditional on religious devotion, Islamic depositors reward their banks for the decline in Sharia risk. That is, they increase the amount deposited and ask for lower returns due to a Sharia-compliant deposit insurance scheme.

What happens to depositor disciplining against excessive (financial) risk-taking by banks after a separation of deposit insurance funds?

[7] In the case of punishing due to high bank risk, depositors can also switch to a more liquid investment.

The previous section presented that the administration of Islamic deposits under a conventional deposit insurance scheme is Sharia non-compliant as the commingled funds would be utilized to recover insolvent Islamic banks (cross-subsidization). Apart from concerns regarding impurity of funds, the negative effects of a Sharia non-compliant administration of a common deposit insurance scheme can only materialize when Islamic banks become insolvent. Conversely, a deposit insurance scheme with a Sharia-compliant administration would be free of cross-subsidization as the funds reimbursed to depositors in case of a bank failure would be Sharia-compliant funds. Hence, I present the next hypothesis of the chapter:

Hypothesis 2: Under a common deposit insurance scheme, Islamic depositors engage in market disciplining in response to the insolvency risk of their banks. That is, they withdraw their funds and ask for higher returns in response to an increase in bank insolvency risk under a common deposit insurance scheme. Moreover, this market discipline disappears with the introduction of a separate Islamic deposit insurance fund.

Hypotheses 1 and 2 are developed to address Sharia and insolvency risks within the context of Islamic deposits, respectively. However, these two types of risk can also evolve together. In particular, non-compliance with Sharia risk may eventually result in insolvency risk in Islamic banks. In fact, Swartz (2013) mentions depositor discipline for non-compliance of Sharia risk, which may cause operational risk and in turn insolvency risk. I specifically test the presence of this in a subsequent section and find that Sharia-compliant deposit insurance schemes do not cause higher or lower bank insolvency risk.

2.4 Empirical Method

2.4.1 Sample Selection and Data

The concept of deposit insurance is relatively new to the Islamic banking system. Until recently, the majority of jurisdictions either did not have an explicit deposit insurance scheme or the coverage of Islamic deposits was arranged under a conventional deposit insurance.

The idea of a separate and Sharia-compliant deposit insurance scheme first came into existence in the Turkish banking system. The "special finance houses" were allowed to have their own religious deposit insurance scheme as a separate arrangement operating side by side with the conventional deposit insurance fund in 2001. The announcement and the immediate introduction of this scheme were largely attributed to the bank run episode of "Ihlas Special Finance House" during the crisis of the Turkish banking system due to Sharia misconduct in 2001. However, the operation of this arrangement did not last long. In December 2005, upon enactment of the Banking Act No.541, special finance houses were given the status of Islamic banks and the Islamic deposit insurance fund was transferred to the Savings and Deposit Insurance Fund (SDIF), the country's conventional deposit Insurance system.

In Malaysia, the adoption of a deposit insurance scheme for the overall banking system was managed in 2005. Former implicit guarantees were replaced by the dual deposit insurance scheme. In other words, the administration of the Islamic and conventional deposits was separate since from the beginning. To this end, the principal contract structure of the Malaysian Islamic deposit insurance was decided to be *Kafalah bil 'Ujr* (guarantee with fee), in which Islamic banking institutions pay a fee in the form of annual premiums in return for a deposit insurance fund assuming the obligation of reimbursing insured depositors.

Bahrain was the first country to provide insurance to Islamic banks under the conventional deposit insurance system. This practice was abandoned in 2010. The

deposit insurance reform of 2010 aimed at replacing the current post-funded scheme with a pre-funded scheme. Moreover, the old scheme was revised and the administration of the Islamic deposit insurance fund was separated. The administration of the fund responsible for the Islamic banking system assumed a *Takaful* (mutual guarantee) principle.

For Kuwait, Indonesia and Jordan, the announcement of the separate deposit Islamic deposit insurance scheme can largely be attributed to external reasons. In the years after the global financial crisis, the Basel Committee of Banking Supervision (BCBS) and the International Association of Deposit Insurers (IADI) produced the "Core Principles for Effective Deposit Insurance Systems" in November 2014, emphasizing the need to establish "Islamic deposit insurance systems ... for the protection of Islamic deposits in accordance with Islamic principles and rules." Subsequently, Jordan, Indonesia and Kuwait made the announcements of creating a separate deposit insurance framework for Islamic deposits to ensure their Sharia-compliant administration (Abdelhady (2015); Central Bank of Kuwait (2016)).

Importantly, there is no anecdotal evidence that separation announcements were part of a bigger package of financial reforms across these jurisdictions. This ensures that another financial reform confounding the separation event is highly unlikely. It is worth mentioning that other jurisdictions such as Oman and Qatar also announced the establishment of a Sharia-compliant deposit insurance scheme in order to meet the "Core Principles" in 2015. However, so far, they lack an explicit deposit insurance scheme for their banking system, making the event of separation irrelevant for them.

Therefore, I concentrate on all dual banking systems with an explicit deposit insurance scheme covering Islamic banks. Data availability restricts my analysis to Bahrain, Jordan, Indonesia, Kuwait, Malaysia and Turkey. The time span of the analysis is determined by the data limitations and the fact that Kuwait implemented an explicit deposit insurance scheme in 2007. Therefore, I focus on the time period of 2007-2015.

I collect yearly balance sheet and income statement information from the Islamic banks of these jurisdictions by utilizing the annual reports either published on the

websites of the banks or by contacting them by telephone. Previous studies (Beck et al. (2013); Abedifar et al. (2013)), utilize the Bankscope dataset to study Islamic banking. However, it is not possible to obtain the information specifically on PLS accounts in Bankscope. Moreover, Bankscope provides limited information on the breakdown of aggregate deposit returns across deposit categories. As I specifically focus on deposit accounts' PLS feature, I hand-collect annual reports of banks, where I can observe information broken down based on different deposit categories. I do so by systematically reviewing banks' websites. Wherever the information is not available, I contact banks' officers by telephone or personal bilateral meetings.

Several countries in my sample have conventional banks with Islamic windows. I exclude those banks from my analysis as it is not possible to observe Islamic accounts separately in these banks' annual reports. Moreover, Indonesia has established regional Islamic banks, for which annual reports are not available.[8]

In brief, I end up with 47 Islamic banks from 6 dual banking systems. Despite the above-mentioned limitations, my sample covers at least 81% of all Islamic banks in terms of bank size in individual dual banking systems.[9]

2.4.2 Identification

To test the hypotheses presented in the previous section, I consider a setup where regulators announced splitting up their deposit insurance funds to allow Islamic banks to be covered by Sharia-compliant deposit insurance in 6 dual banking systems. Following Aysan et al. (2017) and Karas et al. (2013), I treat this event, happening at the country level, as exogenous to the individual depositors.

The time variation in the separation announcements across countries makes the identification strategy a staggered DID strategy. A similar research design has been used in several studies (Haselmann et al. (2010); Beck et al. (2010)). Focusing on

[8]According to Indonesia Financial Services Authority, regional Islamic banks comprise only 2.5% of total Islamic deposits (OJK (2017)), a negligible amount for my study.

[9]The total assets of the Islamic banking sector at the country level were obtained from Dubai Islamic Bank (2017).

multiple interventions takes care of many threats regarding the (external) validity of the results. This methodology can be illustrated as follows: Suppose there are two countries, A and B, implementing a separate Islamic deposit insurance scheme at times t = 1 and t = 2, respectively. If we consider t = 0 to be the starting period of our sample, then from t = 1 to t = 2, country B serves as the control group and after that serves as treatment group for subsequent time periods. Therefore, most countries belong to both treatment and control groups at several points in time. Moreover, this specification is robust to some countries (Malaysia) being treated before the sample period and some countries (Turkey) never being treated during the sample period.

Similar to most studies focusing on deposit insurance and depositor behavior (Ioannidou and de Dreu (2006); Karas et al. (2013); Aysan et al. (2017)), I analyze both the quantity and price equation in a reduced-form model. The combined information from both of these equations helps me show that observed effects are supply as opposed to demand-driven (e.g. regulatory discipline).

Hypothesis 1 states that depositors would positively react to a Sharia-compliant deposit insurance scheme conditional on the level of religiosity. Unlike former studies that use conventional banks as a control group to Islamic banks (Aysan et al. (2017)), I argue that the comparison of treated Islamic banks with control Islamic banks takes care of the fact that level of religious devotion is to some extent controlled for. Moreover, to the extent that religiosity is time-invariant, it is controlled by bank or country fixed effects. Unfortunately, surveys that measures religiosity at the country level such as the World Values Survey provide only one wave for each country. This makes it impossible for me to observe how religiosity changes over time. However, an advantage of my setup is that it, unlike others that compare conventional and Islamic banks, compares Islamic banks with one another to difference out the potential effects

Table 2.1: Announcement of separate Islamic deposit insurance scheme

Year	2007	2008	2009	2010	2011	2012	2013	2014	2015
Country									
Bahrain	0	0	0	1	1	1	1	1	1
Indonesia	0	0	0	0	0	0	0	1	1
Jordan	0	0	0	0	0	0	0	1	1
Kuwait	0	0	0	0	0	0	0	0	1
Malaysia	1	1	1	1	1	1	1	1	1
Turkey	0	0	0	0	0	0	0	0	0

Notes: This table reports the coding of the treatment variable, *Treatment*, used in this analysis across six dual banking systems. Years before the announcement of separate Islamic deposit insurance are coded as zero (0), whereas years after the announcement are coded as one (1).

of religiosity. The model to test Hypothesis 1 is presented below:

$$DEPG_{ict} = \alpha_i + \alpha_t + \delta_1 Treatment_{ct} + \gamma_1 BankFundamentals_{ict-1} + \eta_1 MacroControls_{ct} + \epsilon_{ict} \tag{2.1}$$

$$DEPR_{ict} = \beta_i + \beta_t + \delta_2 Treatment_{ct} + \gamma_2 BankFundamentals_{ict-1} + \eta_2 MacroControls_{ct} + u_{ict} \tag{2.2}$$

where i is bank, c is country and t is year. The dependent variables are $DEPG$, the growth of price-level adjusted deposits, and $DEPR$, implicit return on deposits calculated by banks expenditures on deposits divided by total bank deposits. α and β factors are bank- and time-level fixed effects, respective of their indexes.[10] *Treatment* is a dummy variable taking the value one (1) for years after the announcement of a separate Islamic deposit insurance scheme and zero otherwise, as shown in Table 2.1. *BankFundamentals* and *MacroControls* refer to the control variables used in equations (2.1) and (2.2). *BankFundamentals* include standard bank characteristics such as size and also include publicly observable bank risk measures such as capital-asset ratio, loan quality, asset returns etc., which have been used in previous studies (Martinez-Peria et al. (2001); Demirgüç-Kunt and De Dreu (2004)) to show financial risk and depositor discipline. They are lagged one period to account for delayed publication

[10] α_i and β_i serve also as country dummies.

of annual reports by one year. Finally, *MacroControls* are the variables that are changing across countries and over the years such as GDP per capita growth, population growth, inflation and the Herfindahl-Hirschmann Index, which measures the concentration of the Islamic banking sector in a country, in a given year.

A positive estimate for δ_1 in equation (2.1) and a negative estimate for δ_2 in equation (2.2) would verify Hypothesis 1. Ensuring that signs of these coefficients simultaneously hold would imply that the effect is supply-driven.

Hypothesis 2 claims that since cross-subsidization is only relevant in case of a bank failure, Islamic depositors should discipline their banks for high insolvency risk in the absence of a Sharia-compliant deposit insurance. Moreover, disciplining behavior must vanish after the introduction of a Sharia-compliant deposit insurance scheme. To test this hypothesis, I reformulate equations (2.1) and (2.2), *and* estimate the following model:

$$\begin{aligned} DEPG_{ict} = \alpha_i + \alpha_t + \phi_1 Z - score_{ict} + \psi_1 Treatment_{ct} * Z - score_{ict} \\ + \gamma_1 BankFundamentals_{ict-1} + \eta_1 MacroControls_{ct} + v_{ict} \end{aligned} \tag{2.3}$$

$$\begin{aligned} DEPR_{ict} = \beta_i + \beta_t + \phi_2 Z - score_{ict} + \psi_2 Treatment_{ct} * Z - score_{ict} \\ + \gamma_2 BankFundamentals_{ict-1} + \eta_2 MacroControls_{ct} + \omega_{ict} \end{aligned} \tag{2.4}$$

I use a time-varying $Z - score$ measure following Lepetit and Strobel (2013). $Z - score$ is inversely related to an upper bound of the probability of insolvency $p(roa \leq -car)$. Hence, low values of $Z - score$ imply the tendency towards insolvency. In consequence, the examination of depositor disciplining in response to an increase in bank insolvency must be manifested in equations (2.3) and (2.4) in such a way that $Z - score$ must be positively correlated with $DEPG$ and must be negatively correlated with the $DEPR$. A positive estimate for ϕ_1 and a negative estimate for ϕ_2 would indicate the existence of disciplining against bank insolvency risk before the announcement of the separation of deposit insurance funds (i.e., when $Treatment_{c,t} = 0$). Instead, a negative estimate for ψ_1 and a positive estimate for ψ_2 would indicate that the

change in the degree of disciplining against bank insolvency risk depends positively on $Treatment_{c,t}$. (i.e., when $Treatment_{c,t} = 1$) (Ioannidou and de Dreu, 2006).[11]

2.4.3 Summary Statistics

Table 2.2 presents the summary statistics on the variables used in the analysis. It compares pre- and post-treatment periods separately. Interestingly, the comparison of pre- and post-event mean values of deposit growth ($DEPG$) and deposit return ($DEPR$) suggests an increase in the deposit supply after the announcement of a separate Islamic deposit insurance scheme in a "reduced-form" sense. Some bank characteristics such as return on assets and loan quality worsen after the announcement, while other bank characteristics stay roughly similar for both pre-event and post-event observations, based on Table 2.2. It is important to note that the effect of separation announcements on deposit growth and return on deposits are conditional on the bank characteristics. The underlying assumption here is that separation announcements do not, in and off themselves, have an impact on these covariates. Observing that there are potentially significant differences in bank characteristics between pre- and post-treatment periods may indeed suggest that a separation announcement affects some bank characteristics. In a following section, I specifically test for the presence of this and do not find evidence for bank characteristics being affected by a separation announcement.

[11] See appendix B for a more formal discussion.

Table 2.2: Summary statistics

	Description	Source	Mean-pre-treatment	Mean - post-treatment
Dependent Variables				
DEPG	Rate of growth of total deposits	Annual reports	0.09 (0.08)	0.11 (0.09)
DEPR	Expenses on deposits divided by total depsoits	Annual reports	0.05 (0.03)	0.03 (0.03)
Bank Fundamentals				
Return on Assets	Net income divided by total assets	Annual Reports	0.08 (0.07)	0.05 (0.03)
Loan Quality	Nonperforming loans divided by gross loans	Annual Reports	0.09 (0.16)	0.05 (0.11)
Liquidty	Liquid assets (cash, central bank debt, short term securities) divided by total assets	Annual Reports	0.24 (0.13)	0.25 (0.14)
Size	Natural logarithm of total assets	Annual Reports	21.2 (1.51)	21.3 (1.49)
Capital Asset Ratio	Equity divided by total assets	Annual Reports	0.18 (0.08)	0.15 (9.00)
Z-score	sum of capital asset ratio and return on assets divided by standard deviation of return on assets	Annual Reports	2.59 (2.44)	2.35 (2.11)
Macro-level Variables				
HHI	Herfindahl-Hirschmann Index: sum of squared deposit market share	Annual Reports	4247.08 (1247.34)	2358.92 (1289.05)
GDP per Capita Growth	Rate of growth of real GDP	World Bank	0.024 (0.03)	0.026 (0.023)
Inflation	Rate of Change of GDP deflator	World Bank	0.08 (0.06)	0.025 (0.06)
GOVEFF	Index ranging from -2.5 to 2.5, bigger values indicating more efficient government.	World Bank	0.0043 (0.283)	0.7714 (0.386)
Religiosity	Percentage of respondents stating they are religious	World Values Survey	0.745 (0.065)	0.642 (0.111)
Secularism	Categorical variable from 1 to 10, 10 being least secular	World Values Survey	5.386 (0.452)	5.335 (0.440)
Population Growth	Rate of growth of population	World Bank	0.0009 (0.0066)	0.0004 (0.003)

Notes: Standard deviations are in parentheses. This table shows the variable descriptions and the means of the variables used in this analysis across pre- and post-treatment periods.

2.5 Empirical Results

Table 2.3 presents the results from DID estimators. Columns (1), (3) and (5) exhibit the estimation results of equation (2.1), while columns (2), (4) and (6) refer to the estimation results of equation (2.2). All columns include bank and year fixed effects. Bank fundamentals comprise of *Size, Z-score, Capital Asset Ratio, Loan Quality, Return on Assets* and *Liquidity* and macro controls include *GDP per Capita Growth, Inflation, Population Growth and HHI*. Baseline estimates from columns (1) and (2) demonstrate the evidence of depositors of Islamic banks rewarding their banks upon the announcement of a separate Sharia-compliant deposit insurance scheme leading to a decline in Sharia risk. As mentioned in the previous section, the coefficients should be interpreted with caution. In the absence of a price equation (equation (2.2)), a decrease in Sharia risk due to the announcement of a separate Islamic deposit insurance fund is associated with, ceteris paribus, a 2.8 percentage points increase in deposit growth, on average. Similarly, in the absence of a quantity equation (equation (2.1)), the announcement of a separate Islamic deposit insurance, on average, decreases the implicit return on deposits by 0.8 percentage points. However, within a reduced-form framework, it is not possible to give an exact magnitude of the increase in deposit supply as a result of the treatment. Nevertheless, the signs and the significance of the coefficients demonstrate that there definitely exists a deposit supply increase after the announcement of a separate Islamic deposit insurance scheme, failing to reject Hypothesis 1 that Islamic bank depositors reward their banks due to an exogenous Sharia-compliant scheme they have to take part in.

Regressions of columns (1) and (2) do not include a factor controlling for time-varying and country-specific shocks. In order to control for all such shocks, a fixed effects factor that varies both in a country and year dimension ($\alpha_{c,t}$) is required. However, such a fixed effects factor would fully absorb the variation in my treatment variable. To be able to tackle this issue, I follow the methodology proposed by Bertrand and Mullainathan (2003) and Haselmann et al. (2010). They suggest including the

Table 2.3: Baseline regressions

	(1) DEPG	(2) DEPR	(3) DEPG	(4) DEPR	(5) DEPG	(6) DEPR	(7) DEPG	(8) DEPR
Treatment	0.0281***	-0.00804*	0.0259***	-0.00777**	0.0236**	-0.00731*	0.0192**	-0.00577
	(0.00645)	(0.00314)	(0.00512)	(0.00266)	(0.00655)	(0.00333)	(0.00680)	(0.00382)
$DEPG_{(-i)ct}$			0.0494*					
			(0.0219)					
$DEPR_{(-i)ct}$				0.0960				
				(0.111)				
$Before^3$					0.0110	-0.00548		
					(0.0113)	(0.00601)		
$Before^2$					-0.00836	-0.00274		
					(0.0100)	(0.00204)		
$Before^1$					-0.000890	-0.00234		
					(0.00737)	(0.00426)		
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Country FE	No	No	No	No	No	No	Yes	Yes
Bank Fundamentals	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.015	0.095	-	-	-	-	0.095	0.150
N	294	292	294	292	294	292	48	48
R^2	0.460	0.783	0.486	0.788	0.450	0.787	0.326	0.334

Notes: The sample period is 2007-2015. This table reports the estimation results from equations (2.1) and (2.2). The dependent variable in columns (1), (3), (5) and (7) is $DEPG$. The dependent variable in columns (2), (4), (6) and (8) is $DEPR$. *Treatment* refers to the announcement of separate Islamic deposit insurance scheme. Columns (1)-(6) include bank fixed effects and bank fundamentals. Bank fundamentals are $ReturnonAssets$, $LoanQuality$, $Liquidity$, $Size$, $CapitalAssetRatio$ and $Z-score$. All columns include time fixed effects and macro controls. Macro controls include HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ are respective mean values of $DEPG$ and $DEPR$ of each country and each year excluding each respective bank i itself. $Before^3$, $Before^2$, $Before^1$ are 3 years, 2 years and 1 year leads of the *Treatment* variable, respectively. Columns (7) and (8) show the estimations whose bank dimensions are collapsed. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. Wild bootstrapped p-values for the coefficient of *Treatment* variable is reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

mean value of the dependent variable of each country and each year excluding each respective bank i itself, denoted as $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ respectively for the quantity and the price equations. As presented in Table 2.3, columns (3) and (4), the inclusion of these variables leaves the results unchanged.

In columns (5) and (6), $Before^1$, $Before^2$ and $Before^3$ coefficients are placebo treatments that test if the outcome variables were significantly different between treatment and control banks 1 year, 2 years and 3 years before the announcement of a separate Islamic deposit insurance scheme. Estimation results suggest that, on average, there were no significant differences in outcome variables between treated and control banks. Thus, one can claim that in the absence of the treatment, treated and non-treated banks would behave similarly.

So far, the standard errors are clustered at the country level to account for the correlation of error terms in the time dimension for each country. However, a small number of clusters might lead to the inconsistent estimation of the standard errors. In difference-in-differences setups where the variable of interest is most of the time a binary variable, this problem is exacerbated (Bertrand and Mulliathan (2004)). Another potential problem concerning the inconsistent estimation of standard errors is the "Moulton" problem, referring to the group-level correlation of error terms when using individual-level dependent variables explained by group-level regressors (Moulton (1990)).

To address the latter problem, I collapse the bank dimension of my sample and estimate the country-level equations in columns (7) and (8). Although the quantity equation is robust to collapsing the bank dimension, the coefficient of the treatment variable in the price equation is no longer significant at the 10% level but at the 15% level. Despite a minor reduction in the significance in the price equation, the signs of the coefficients remain the same. For the former problem, I use the "Wild cluster bootstrap percentile-t" procedure proposed by Cameron et al. (2008). Inference with cluster-robust standard errors has a limitation that it assumes that the number of clusters goes to infinity asymptotically. This assumption is hard to satisfy

with small number of clusters. What Cameron et al. (2008) proposes is to bootstrap clusters of small sample corrected standard errors based on T-distribution. Through Monte-Carlo simulations, they find that the wild cluster bootstrap percentile-t procedure empirical rejection rates were very similar to theoretical values, even when the number of clusters were six (6). This procedure has recently started to be used in DID setups with a small number of clusters, especially in labor economics literature (Nandi (2015); Kalsi (2017)). To this end, Table 2.3 also presents the p-values of the coefficient estimates of the treatment variable for the baseline regressions.

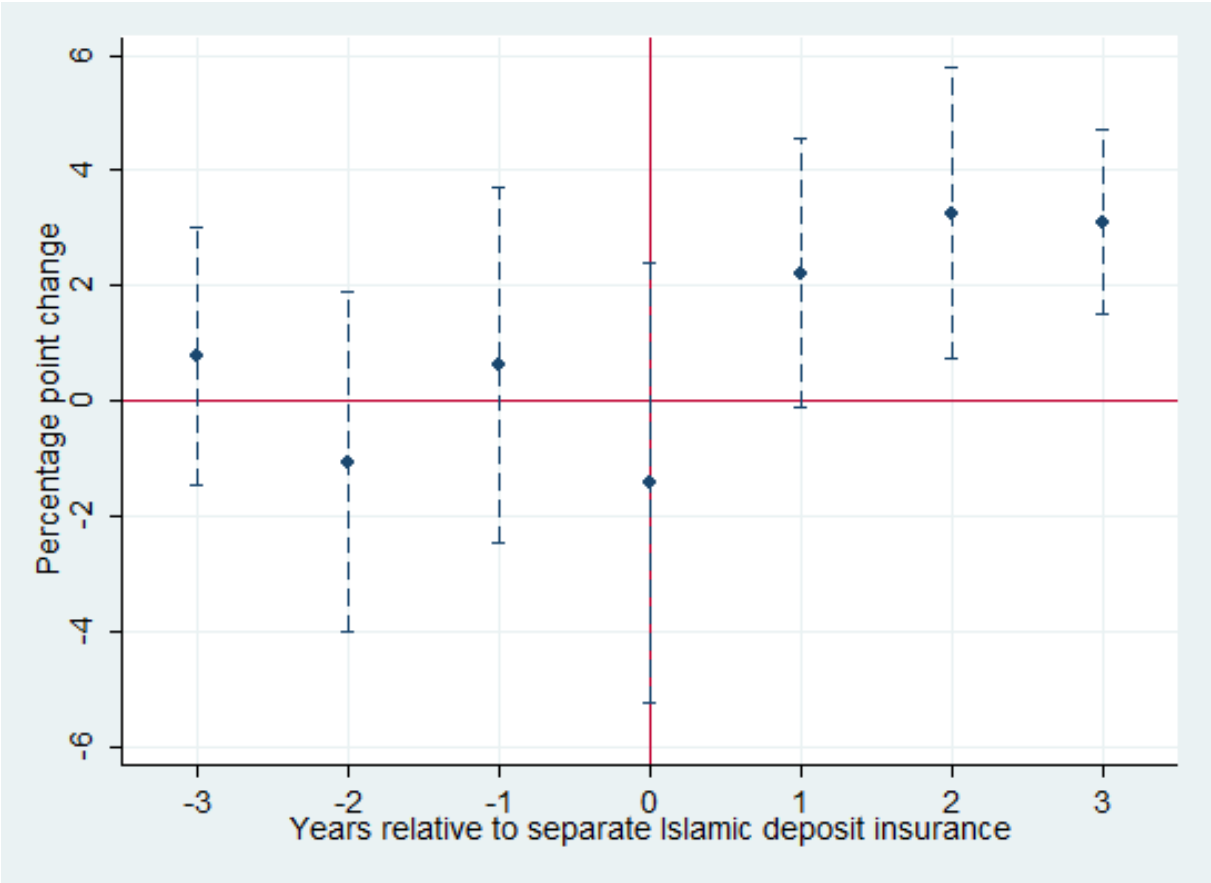
2.5.1 Dynamics of the Treatment

Figure 2.1 and Figure 2.2 present the dynamic effects of the treatment for equation (2.1) and (2.2), respectively. The y-axes of the figures show the percentage point difference in the respective outcome variables across treatment and control groups, while the x-axes show the years relative to the announcement of the separate Islamic deposit insurance. The dashed lines indicate the 95% confidence intervals adjusted for the state-level clustering for the respective point estimates. These figures illustrate two key points: Innovations in deposit growth and deposit return do not precede the announcement of a separate Islamic deposit insurance and the effect of the announcement materializes after one year. As shown by the confidence intervals, coefficients on the placebo treatments preceding the actual announcement year are insignificantly different from zero, whereas subsequent years after the actual announcement are associated with differential deposit growth increase and implicit return decrease for the treated banks as opposed to control banks, thus verifying the existence of Islamic depositors rewarding their banks due to a reduction in Sharia risk.

2.5.2 Timing of the Treatment

The factors determining the timing of deposit insurance adoption is rarely studied in the literature. Demirgüç-Kunt et al. (2008) point out several channels that, in

Figure 2.1: The dynamic impact of the treatment on Islamic deposit growth

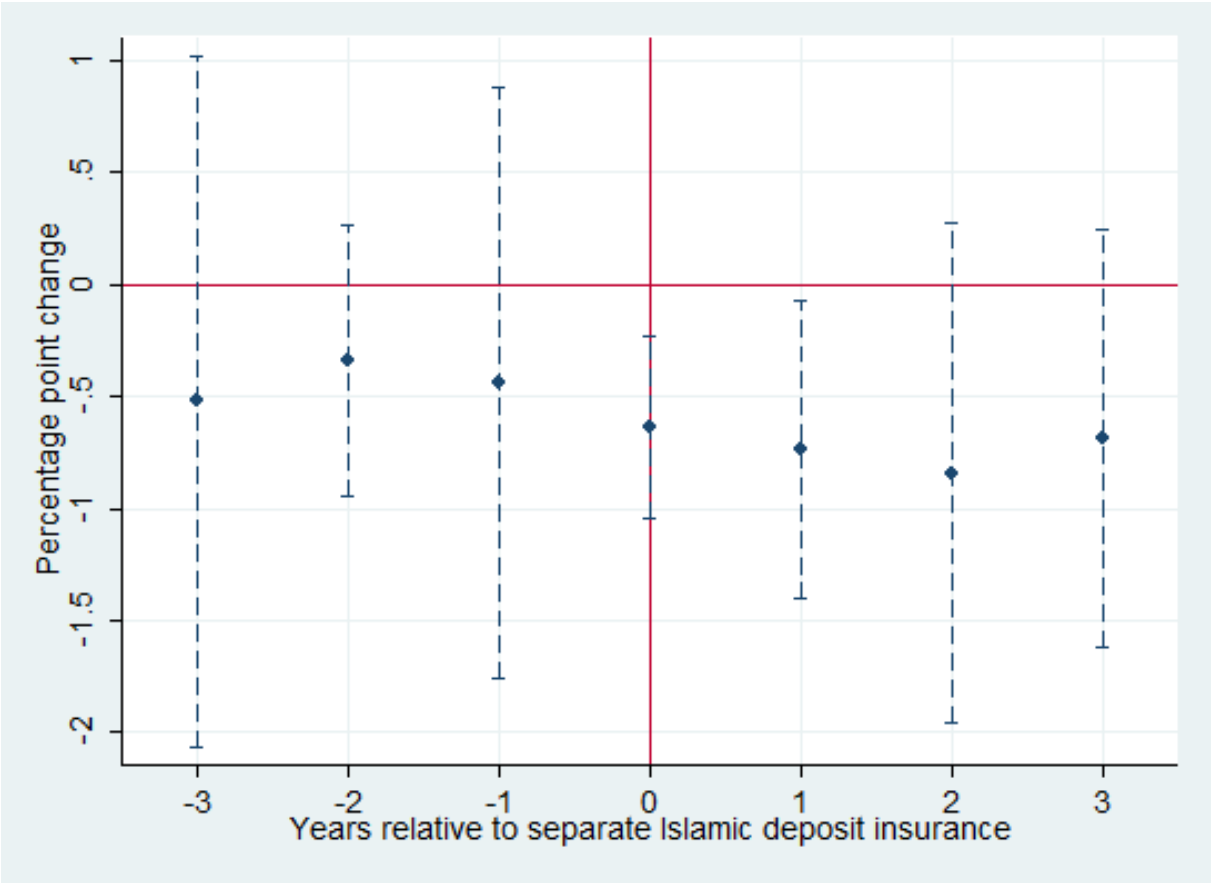


Notes: This figure plots the impact of the announcement of a separate Islamic deposit insurance scheme on the growth of deposits. I consider a 7-year window, spanning from 3 years before the announcement until 3 years after the announcement. The dashed lines represent 95% confidence intervals, adjusted for country-level clustering. Specifically, I report estimated coefficients from the following regression:

$$DEPG_{i,c,t} = \alpha_i + \alpha_t + \beta_{-3} Treatment_{c,t}^{-3} + ... + \beta Treatment_{c,t} + ... + \beta_{+3} Treatment_{c,t}^{+3} + \epsilon_{i,c,t}$$

The *Treatment*s equal zero, except as follows: $Treatment^{-j}$ equals one for countries in the j^{th} year before the announcement, while $Treatment^{+j}$ equals one for states in the j^{th} year after the announcement. α_i and α_t are bank (as well as country) and year fixed effects, respectively.

Figure 2.2: The dynamic impact of the treatment on Islamic deposit return



Notes: This figure plots the impact of the announcement of a separate Islamic deposit insurance scheme on the growth of deposits. I consider a 7-year window, spanning from 3 years before the announcement until 3 years after the announcement. The dashed lines represent 95% confidence intervals, adjusted for country-level clustering. Specifically, I report estimated coefficients from the following regression:

$$DEPR_{i,c,t} = \eta_i + \eta_t + \gamma_{-3} Treatment_{c,t}^{-3} + ... + \gamma Treatment_{c,t} + ... + \gamma_{+3} Treatment_{c,t}^{+3} + u_{i,c,t}$$

The *Treatment*s equal zero, except as follows: $Treatment^{-j}$ equals one for countries in the j^{th} year before the announcement, while $Treatment^{+j}$ equals one for states in the j^{th} year after the announcement. η_i and η_t are bank (as well as country) and year fixed effects, respectively.

theory, might affect the timing of the adoption of deposit insurance schemes. Two of those channels seem to be relevant to my setup. The first one is the efficiency of government; that is, how fast and effectively a government can implement public goods. In countries where different forms of financial safety net arrangements are provided by public entities and where participation in the safety net is mandatory by the agents in the economy, the financial safety net is ultimately a non-exclusive good. In that sense, there is a possibility that more efficient governments can implement financial safety nets such as deposit insurance earlier.

The second channel states that political interest can lead to the early adoption of deposit insurance (Laeven (2004); Calomiris and Jaremski (2016)). Political interest theory sees society comprising of different groups with divergent private interests and, therefore, with divergent views on how the deposit insurance scheme should be designed. The private interest group with the most political influence ensures the adoption of a form of deposit insurance with risk shifting mechanisms working in favor of itself, which may explain the observed heterogeneity among countries regarding the mechanisms that control the risk shifting in deposit insurance. When projected to my setup, in countries where the political influence of private interest groups that favor Sharia compliance is high, the implementation of a deposit insurance scheme which ensures the Sharia compliance might happen earlier.

Furthermore, the political interest channel might affect the adoption of a deposit insurance scheme with a particular risk shifting control depending on the efficiency of the government. That is, private interest groups might have enough political influence over the design of the deposit insurance scheme, however, as long as the governments are inefficient, implementation of such a deposit insurance system might incur delays. In short, more religious countries or countries with efficient governments might have the potential to adopt a separate Islamic deposit insurance scheme earlier. And if this were true, the timing of the treatment might not be random. Therefore, I may need to control for religiosity, government efficiency and their interaction in my regressions. As long as religiosity and government efficiency are time-invariant characteristics,

Table 2.4: Timing of the treatment

	(1) DEPG	(2) DEPG	(3) DEPR	(4) DEPR	(5) DEPG	(6) DEPG	(7) DEPR	(8) DEPR
Treatment	0.0288**	0.0281***	-0.00886**	-0.00824*	0.0269***	0.0262***	-0.00855***	-0.00794**
	(0.00717)	(0.00650)	(0.00238)	(0.00358)	(0.00623)	(0.00530)	(0.00187)	(0.00302)
GOVEFF	0.114	-0.0824	-0.252*	-0.00569	0.216	-0.00208	-0.223*	-0.00580
	(0.292)	(0.178)	(0.0984)	(0.0808)	(0.263)	(0.152)	(0.103)	(0.0613)
GOVEFF × Secularism	-0.0349		0.0510**		-0.0498		0.0449*	
	(0.0506)		(0.0169)		(0.0448)		(0.0187)	
GOVEFF × Religiosity		0.00432		0.0487		-0.0772		0.0408
		(0.236)		(0.124)		(0.203)		(0.101)
$DEPG_{(-i)ct}$					0.0507*	0.0500*		
					(0.0220)	(0.0221)		
$DEPR_{(-i)ct}$							0.0755	0.0950
							(0.117)	(0.115)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fundamentals	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.066	0.052	0.069	0.089	-	-	-	-
N	294	294	292	292	294	294	292	292
R^2	0.461	0.460	0.790	0.783	0.488	0.486	0.793	0.788

Notes: The sample period is 2007-2015. This table reports the estimation results from equations (2.1) and (2.2) altered by adding *GOVEFF*, *Secularism* and *Religiosity* variables defined in Table 3. $GOVEFF * Secularism$ and $GOVEFF * Religiosity$ denote the interaction terms. The dependent variable in columns (1), (2), (5) and (6) is *DEPG*. The dependent variable in columns (3), (4), (7) and (8) is *DEPR*. *Treatment* refers to the announcement of a separate Islamic deposit insurance scheme. All columns include bank fixed effects, bank fundamentals, year fixed effects and macro controls. Bank fundamentals are *ReturnonAssets*, *LoanQuality*, *Liquidity*, *Size*, *CapitalAssetRatio* and $Z - score$. Macro controls include *HHI*, *GDPperCapitaGrowth*, $Inflation$, *PopulationGrowth*. $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ are respective mean values of *DEPG* and *DEPR* of each country and each year excluding each respective bank i itself. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. Wild bootstrapped p-values for the coefficient of *Treatment* variable is reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

they would be captured by bank fixed effects.[12] Nevertheless, I try to proxy these variables and include them in the regressions to evaluate if the results found in Table 2.3 still hold.

In Table 2.4, *GOVEFF* variable refers to the Government Effectiveness Index from the World Bank, Worldwide Governance Indicators. It varies both by year and by country. This proxy captures the perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation and the credibility of the government's commitment to such policies. Furthermore, I use two time-invariant proxies for religious devotion from the latest waves of World Values Survey in each country: *Religiosity*, which is the percentage of the respondents who consider themselves religious in the survey, and *Secularism*, indicating how necessary the respondents consider the interpretation of regulations by religious authorities. Due to bank fixed effects, individual effects of *Religiosity* and *Secularism* cannot be estimated. However, the effects of interactions of these variables with *GOVEFF* can still be estimated. The results from Table 2.4 suggest that the baseline results are robust to the inclusion of control variables capturing religiosity and government efficiency and their interaction.

2.5.3 Commingling, Cross-Subsidization and Bank Risk-Taking

Hypothesis 2 argues that under a common deposit insurance scheme, Islamic depositors can be affected by commingled funds when their banks become insolvent. In other words, under a conventional deposit insurance system, Islamic funds would be invested in interest-bearing assets (commingling). In case of bank insolvency, Islamic depositors would be cross-subsidized by the commingled funds. A sufficient condition to test this hypothesis is to consider how Islamic depositors react to bank insolvency before and after the reform. If Hypothesis 2 is indeed correct, one must observe a decrease in the deposit supply in response to an increase in bank insolvency

[12]Note that bank fixed effects also serve as country fixed effects in this setup.

by depositors for pre-treatment observations. Also, the degree of disciplining against bank insolvency must decline for post-treatment observations.

Table 2.5 shows the estimation results of equations (2.3) and (2.4). Coefficient estimates of $Z-score$ demonstrate that before the treatment, Islamic depositors tend to withdraw their funds and ask for higher returns in response to an increase in the probability of bank insolvency (shown in columns (1) and (2), respectively). Interestingly, the coefficient estimates of the interaction term, $Treatment * Z-score$ in columns (1) and (2) indicate that there is a significant decline in the degree of disciplining depending on the status of the treatment. That is, the degree of disciplining in response to an increase in the probability of bank insolvency decreases significantly after the announcement of a separate Islamic deposit insurance scheme.

In short, the results from Table 2.5 verify the hypothesis that depositors of Islamic banks are concerned about being compensated by the commingled funds in case their banks become insolvent. After the treatment, however, as the possibility of commingling and cross-subsidization vanishes, depositors of Islamic banks also stop engaging in disciplining through reducing the deposit supply in response to an increase in bank insolvency risk.

The economic intuition behind the estimation results presented in Table 2.5 might not be straight forward. Ultimately, the table presents the results on the effect of the treatment depending on $Z-score$. To be able to interpret the estimated coefficients in a meaningful way, I concentrate on the changes in deposit growth and return along the distribution of $Z-score$. Moving from 75^{th} percentile (51.38) to 25^{th} percentile (17.14) of the $Z-score$ distribution, treatment variable increases deposit growth by 1.2 percentage points and decreases deposit return by 0.8 percentage points.

So far, I have not touched upon the issue of whether the treatment variable has an effect on risk-taking of individual banks. This issue is particularly relevant in my study for the following reason: If the new deposit insurance scheme provided an adequate regulatory discipline forcing Islamic banks to take on less financial risk, the evidence on depositors' rewarding their banks might simply be due to a decrease in

Table 2.5: Evidence on commingling and cross-subsidization

	(1) DEPG	(2) DEPR	(3) DEPG	(4) DEPR
Z-score	0.00101**	-0.000353**	0.000905**	-0.000319*
	(0.000306)	(0.000104)	(0.000298)	(0.000136)
Treatment × Z-score	-0.000317***	0.000162*	-0.000313***	0.000153*
	(0.0000552)	(0.0000764)	(0.0000431)	(0.0000615)
$DEPG_{(-i)ct}$			0.0493*	
			(0.0205)	
$DEPR_{(-i)ct}$				0.0751
				(0.114)
Bank FE	Yes	Yes	Yes	Yes
Bank Fundamentals	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.044	0.092	-	-
N	294	292	294	292
R^2	0.472	0.788	0.497	0.791

Notes: The sample period is 2007-2015. This table reports the estimation results from equations (2.3) and (2.4). The dependent variable in columns (1) and (3) is $DEPG$. The dependent variable in columns (2) and (4) is $DEPR$. *Treatment* refers to the announcement of a separate Islamic deposit insurance scheme. All columns include bank fixed effects, year fixed effects, bank fundamentals and macro controls. Bank fundamentals are $ReturnonAssets$, $LoanQuality$, $Liquidity$, $Size$ and $CapitalAssetRatio$. Macro controls include HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ are respective mean values of $DEPG$ and $DEPR$ of each country and each year excluding each respective bank i itself. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. Wild bootstrapped p-values for the coefficient of $Treatment * Z - score$ variable is reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 2.6: Sharia-compliant deposit insurance and bank risk-taking

	(1) Z-score	(2) Loan Quality	(3) CAR	(4) ROA	(5) Liquidity
Treatment	-4.699	0.00235	-0.0193	-0.00981	0.0709
	(4.107)	(0.0108)	(0.0101)	(0.0120)	(0.0453)
Bank FE	Yes	Yes	Yes	Yes	Yes
Bank Fundamentals	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.65	0.77	0.32	0.95	0.43
N	347	319	347	347	347
R^2	0.951	0.638	0.878	0.482	0.618

Notes: The sample period is 2007-2015. This table reports the estimation results from $y_{i,c,t} = \alpha_i + \alpha_t + \beta_1 Treatment_{c,t} + \beta_2 MacroControls_{c,t} + \epsilon_{i,c,t}$. The dependent variables are $Z-score$, $LoanQuality$, CAR (capital asset ratio), ROA (return on assets), $Liquidity$. *Treatment* refers to the announcement of a separate Islamic deposit insurance scheme. All columns include bank fixed effects, bank fundamentals, year fixed effects and macro controls. Bank fundamentals include *Size*. Macro controls include HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. Wild bootstrapped p-values for the coefficient of *Treatment* variable is reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

risk-taking. Although the reduced-form model takes care of the *direct* effects of regulatory discipline (deposit demand side), the *indirect* effects manifesting themselves through financial risk-taking cannot be taken care of. Especially in the price equation, this effect can show itself through a traditional risk-return relationship. Thus, whether such a scenario is present is an empirical concern, which I address in Table 2.6.

I use five dependent variables, which are largely accepted as factors capturing the publicly observable risk of banks (Martinez-Peria et al. (2001)). The coefficient of interest is still the treatment variable that measures if there was a significant change in terms of risk-taking differentially for treated banks after the announcement of a separate Islamic deposit insurance scheme. Given that all of these publicly observable risk variables could be affected by the treatment variable and confound the treatment, I exclude them as controls for one another in the regressions. Therefore, Table 2.6 presents the results without $BankFundamentals$ as controls.

As Table 2.6 suggests, none of the risk variables seem to be significantly changed after the treatment. Hence, the indirect effects of regulatory discipline affecting the supply of deposits through a reduction in banks' financial risk-taking seem not to be present.

2.5.4 Robustness Tests

This section presents several robustness tests. First, baseline regressions presented in Table 2.3 include units that are never treated (Turkey) and always treated (Malaysia). Although the staggered DID methodology is robust to the inclusion of such units, given the limited variation in my treatment, I may pick up a cross-country variation that does not change over time. In other words, the baseline findings might not be related to the change in deposit insurance if Malaysia always had high levels of Islamic deposit supply and Turkey had low levels of Islamic deposit supply. To show that my results are indeed driven by separation announcements of deposit insurance

Table 2.7: Baseline regressions without Malaysia and Turkey

	(1) DEPG	(2) DEPR	(3) DEPG	(4) DEPR
Treatment	0.0201***	-0.00904*	0.0198***	-0.00885**
	(0.00602)	(0.00377)	(0.00506)	(0.00306)
$DEPG_{(-i)ct}$			0.0445	
			(0.0297)	
$DEPR_{(-i)ct}$				0.0840
				(0.136)
Bank FE	Yes	Yes	Yes	Yes
Country FE	No	No	No	No
Bank Fundamentals	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.046	0.097	-	-
N	124	122	124	122
R^2	0.323	0.698	0.402	0.743

Notes: The sample period is 2007-2015. This table reports the estimation results from equations (2.1) and (2.2) with a sample excluding Malaysia and Turkey. The dependent variable in columns (1) and (3) is $DEPG$. The dependent variable in columns (2) and (4) is $DEPR$. *Treatment* refers to the announcement of a separate Islamic deposit insurance scheme. All columns include bank fixed effects, time fixed effects, bank fundamentals and macro controls. Bank fundamentals are $ReturnonAssets$, $LoanQuality$, $Liquidity$, $Size$, $CapitalAssetRatio$ and $Z-score$. Macro controls include HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ are respective mean values of $DEPG$ and $DEPR$ of each country and each year excluding each respective bank i itself. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. Wild bootstrapped p-values for the coefficient of *Treatment* variable is reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 2.8: Evidence on commingling and cross-subsidization with natural logarithm of Z-score

	(1) DEPG	(2) DEPR	(3) DEPG	(4) DEPR
ln(Z-score)	0.0308*	-0.0139*	0.0281*	-0.0129*
	(0.014504)	(0.006217)	(0.013547)	(0.006143)
Treatment × ln(Z-score)	-0.0079**	0.0023*	-0.0074**	0.0022*
	(0.00268)	(0.000993)	(0.002264)	(0.000801)
$DEPG_{(-i)ct}$			0.0479*	
			(0.0230)	
$DEPR_{(-i)ct}$				0.0773
				(0.113)
Bank FE	Yes	Yes	Yes	Yes
Bank Fundamentals	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.066	0.095	-	-
N	294	292	294	292
R^2	0.472	0.788	0.497	0.791

Notes: The sample period is 2007-2015. This table reports the estimation results from equations (2.3) and (2.4). The dependent variable in columns (1) and (3) is $DEPG$. The dependent variable in columns (2) and (4) is $DEPR$. *Treatment* refers to the announcement of a separate Islamic deposit insurance scheme. All columns include bank fixed effects, year fixed effects, bank fundamentals and macro controls. Bank fundamentals are $ReturnonAssets$, $LoanQuality$, $Liquidity$, $Size$ and $CapitalAssetRatio$. Macro controls include HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ are respective mean values of $DEPG$ and $DEPR$ of each country and each year excluding each respective bank i itself. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. Wild bootstrapped p-values for the coefficient of $Treatment * ln(Z-score)$ variable is reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

funds, I re-run baseline regressions with a sample that excludes Malaysia and Turkey. Taking these two countries out of my sample leads to a decline in the number of observations by 170. Table 2.7 shows the estimation results. It indicates that even with a sample that excludes never treated and always treated units, baseline results still hold. In other words, previous results are not driven by cross-country variation that is unrelated to the treatment variable.

Second, evidence on the channel of cross-subsidization depicted in Table 2.5 includes Z-scores as bank solvency measures as proposed by Lepetit and Strobel (2013). However, as noted in Leaven and Levine (2009), Z-score is a highly-skewed measure of bank solvency. Instead, they suggest using the natural logarithm of Z-score. Although subsequent studies questioned this suggestion (Lapteacru (2016)), for the sake of completeness, I repeat the analysis shown in Table 2.5 with the natural logarithm of Z-score. Table 2.8 presents the respective estimation results. It verifies the previous findings on the channel of cross-subsidization: though correcting the skewness in Z-score measure by taking its natural logarithm leads to a loss in statistical significance to a certain extent, all the findings presented in Table 2.5 still hold at the 10% significance level.

2.5.5 Alternative Identification: Triple-Differences Model

DID estimates assume that, conditional on control variables, there are no country factors confounding the treatment variable, varying in both country and time dimensions. As in all DID setups, the treatment variable is attributed to the event that the researcher thinks most important. However, the uncertainty of what is actually captured by the treatment variable is a significant concern due to potential unobserved contaminating events.[13]

This critical DID assumption can be relaxed by including another control group in

[13]For instance, in Bahrain, the announcement of a separate Islamic deposit insurance scheme was coupled with an announcement of an increase in deposit insurance coverage both for Islamic and conventional banks. Therefore, the observed effect of Islamic depositors rewarding their banks might not come from the separate deposit insurance announcement but from increased deposit insurance coverage. Many contaminating events alike might exist.

Table 2.9: Conventional banks only

	(1) DEPG	(2) DEPR	(3) DEPG	(4) DEPR
Treatment	-0.00441	0.00356	-0.00240	0.00186
	(0.00506)	(0.00257)	(0.00315)	(0.00101)
$DEPG_{(-i)ct}$			0.659***	
			(0.106)	
$DEPR_{(-i)ct}$				0.736***
				(0.0860)
Bank FE	Yes	Yes	Yes	Yes
Bank Fundamentals	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Macro Controls	Yes	Yes	Yes	Yes
N	990	993	990	993
R^2	0.469	0.801	0.502	0.827

Notes: The sample covers only conventional banks and the sample period is 2007-2015. The dependent variable in columns (1) and (3) is $DEPG$. The dependent variable in columns (2) and (4) is $DEPR$ *Treatment* refers to the announcement of separate Islamic deposit insurance scheme. All columns include bank fixed effects, time fixed effects and macro controls. Macro controls include HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. $DEPG_{(-i)ct}$ and $DEPR_{(-i)ct}$ are respective mean values of $DEPG$ and $DEPR$ of each country and each year excluding each respective bank i itself. Standard errors are shown in parentheses and are clustered by country. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 2.10: Pre-treatment variable means of Islamic banks and conventional banks

	Mean (Islamic)	**Mean (Conventional)**
Dependent Variables		
DEPG	0.09 (0.08)	0.10 (0.16)
DEPR	0.05 (0.03)	0.049 (0.023)
Bank Fundamentals		
Return on Assets	0.08 (0.07)	0.01 (0.04)
Loan Quality	0.09 (0.16)	0.05 (0.14)
Liquidty	0.24 (0.13)	0.26 (0.13)
Size	21.2 (1.51)	21.5 (1.55)
Capital Asset Ratio	0.18 (0.08)	0.13 (0.06)
Z-score	2.59 (2.44)	2.35 (2.11)

Notes: Standard deviations are given in parentheses. This table shows the variable means across Islamic and conventional banks before the announcement of a separate Islamic deposit insurance scheme. Group means of dependent variables are not (statistically) significantly different from each other.

each country which is not subject to our treatment of decline in Sharia risk as a result of a separate Islamic deposit insurance announcement. A recent implementation of this approach in financial intermediation literature comes from Körner (2017). The underlying idea is that a comparison between non-treated control units in the country where the treatment happened and the non-treated units in other countries are subtracted from the difference-in-differences estimate. Thus, this approach is called "triple-differences".

To this end, conventional banks in my sample countries can be used as an additional control group for my analysis. However, using conventional banks as another set of control group requires satisfying some conditions. Under the Neyman-Rubin framework, one crucial assumption to establish a causal relationship is the stable unit treatment value assumption (SUTVA), where treatment assignment to one group does not affect the potential outcome for the other group (Rubin (1977)). In my set up, although the announcement of a separate, Sharia-compliant deposit insurance scheme should not directly affect conventional banks' depositors, it can affect them indirectly. First, conventional depositors could be affected by the announcement due to a potential shrink in the size of the conventional deposit insurance scheme after separation. Second, depositors could switch between the two types of banks, which could very well occur in my setting. In fact, switching from conventional banks to Islamic banks after the separation announcement could result in overestimation in my triple-differences regression. Therefore, it is crucial to test whether treatment has some significant effect on the newly introduced control group of conventional banks.

Table 2.9 presents the results emanating from regressing the treatment variable on the outcome variables of the conventional banks. The insignificant coefficients indicate that the treatment variable does not affect the newly introduced control group of conventional banks. A triple-differences methodology could be applied from the perspective of SUTVA.

In the next step, I investigate how similar are the Islamic and conventional banks in outcome variables before the intervention took place. Table 2.10 shows the summary

Table 2.11: Triple-differences regressions

	(1) DEPG	(2) DEPR	(3) DEPG	(4) DEPR
Treatment × Islamic	0.0185*	-0.00967***		
	(0.00867)	(0.00153)		
Z-score × Islamic			0.000876***	-0.000268**
			(0.000167)	(0.000100)
Treatment × Z-score × Islamic			-0.000281***	0.000177***
			(0.0000563)	(0.0000348)
Bank FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Bank Type-Year FE	Yes	Yes	Yes	Yes
Bank Type Controls	Yes	Yes	Yes	Yes
Bank Fundamentals	Yes	Yes	Yes	Yes
Wild Bootstrap-t p-value	0.090	0.045	0.015	0.025
N	1145	1146	1145	1146
R^2	0.517	0.809	0.516	0.808

Notes: The sample period is 2007-2015. The dependent variable in columns (1) and (3) is $DEPG$. The dependent variable in columns (2) and (4) is $DEPR$. Columns (1) and (2) show the estimation results of equations (2.5) and (2.6), respectively. Columns (3) and (4) show the estimation results for triple-differences specification. *Treatment* refers to the announcement of a separate Islamic deposit insurance scheme. *Islamic* is a dummy variable taking the value one (1) for Islamic banks and zero (0) otherwise. All columns include bank fixed effects, country-year fixed effects, bank type-year fixed effects, bank fundamentals and bank type controls. Bank fundamentals are $ReturnonAssets$, $LoanQuality$, $Liquidity$, $Size$, $CapitalAssetRatio$ and $Z-score$. Bank type controls include the interaction of *Islamic* variable with following macro-level variables: HHI, $GDPperCapitaGrowth$, $Inflation$, $PopulationGrowth$. Standard errors are shown in parentheses and are clustered by country. Wherever shown, country-level clustered standard errors are wild bootstrapped. For columns (1) and (2), wild bootstrapped p-values for the coefficient of $Treatment * Islamic$ variable *and* for columns (3) and (4) wild bootstrapped p-values for the coefficient of $Treatment * Z-score * Islamic$ variable are reported. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

statistics of dependent and independent variables separately for conventional and Islamic banks in pre-treatment periods. Interestingly, there are no systematic differences between these two groups in terms of the dependent variables, which suggests that it is plausible to use conventional banks as the second control group. The difference between these two groups manifests itself in other bank characteristics, which I would ultimately include as control variables in my estimations. The corresponding reduced-form model turns into

$$DEPG_{isct} = \alpha_i + \alpha_{ct} + \alpha_{st} + \delta_1 Treatment_{ct} * Islamic_s + \gamma_1 BankFundamentals_{isct-1} + \eta_1 X_{sct} + \epsilon_{isct} \quad (2.5)$$

$$DEPR_{isct} = \beta_i + \beta_{ct} + \beta_{st} + \delta_2 Treatment_{ct} * Islamic_s + \gamma_2 BankFundamentals_{isct-1} + \eta_2 X_{sct} + u_{isct} \quad (2.6)$$

where s indexes banking sector (Islamic bank or conventional bank). α_i and β_i capture all time-invariant bank factors. Moreover, $\alpha_{c,t}$ captures all time-variant country-level factors common to both banking sectors. Finally, $\alpha_{s,t}$ and $\beta_{s,t}$ reflects time-varying banking sector factors that are constant across countries. $Treatment_{c,t} * Islamic_s$, this time, indicates Islamic banks after the announcement of a separate Islamic deposit insurance scheme. So as to take into account the possibility that Islamic banks and conventional banks can react differently to prevailing macroeconomic conditions, previous macro-level controls are multiplied by a dummy variable, $Islamic_s$ that takes the value one (1) for Islamic banks and zero (0) otherwise, which are denoted by $X_{s,c,t}$. Bank-level controls are included as before.

The results of this approach are presented in columns (1) and (2) of Table 2.11. Though the magnitude of $\hat{\delta_1}$ is smaller and $\hat{\delta_2}$ is bigger than the difference-in-difference estimates, the inclusion of conventional banks as an additional control group does not alter the conclusion that Islamic depositors reward their banks due to further Sharia compliance brought by an announcement of a separate Islamic deposit insurance scheme. In addition, columns (3) and (4) of Table 2.11 also verify the finding that

commingling and cross-subsidization channels are present.

2.6 Conclusion

This chapter attempts to improve our understanding of whether non-pecuniary appeals such as religion play a role in financial decision-making. To answer this question, I turn my attention to a natural experiment in which several countries announced a separation of deposit insurance funds between Islamic and conventional banks to allow the former to have a Sharia-compliant coverage scheme. Using hand-collected bank-level data and differences-in-differences as well as triple-differences methodologies, I find that depositors of Islamic banks reward their banks after the announcement of a separate Islamic deposit insurance fund in the sense that they increase the quantity deposited and ask for lower deposit returns. In a way, they reward their banks due to a decline in Sharia nonconformity risk, which was present when the administration of the funds attributable to Islamic deposits was under a conventional deposit insurance scheme.

I also find that former disciplining by Islamic banks' depositors against the risk of insolvency vanishes after the announcement of a separate Islamic deposit insurance scheme. I attribute these findings to the commingling and cross-subsidization channels. These results point to a trade-off between availability of funds (deposit supply) and the existence of market discipline in the design of deposit insurance schemes in banking systems where religious sentiments matter.

Overall, my findings expand several strands of the literature ranging from the role of non-monetary incentives in financial decision making to the effect of deposit insurance on depositor disciplining. The results indicate that Sharia compliance induced by Islamic deposit insurance reforms were morally persuasive from Islamic depositors' perspective. These results can also be interpreted as ethical appeals such as religiosity playing a role in depositors' financial decision making and determining depositors' sensitivity to bank risk. They provide a rationale for the recent increases in ethical in-

vesting around the world. To my knowledge, this is the first evidence in the literature that examines the effects of religious branding of financial reforms.

Appendices

A Reports on Separate Islamic Deposit Insurance Announcement

This section presents anecdotal evidence on the concept of deposit insurance from a religious perspective as well as the announcements of separate Islamic deposit insurance funds.

1. Reuters Financials, December 3, 2014: *Indonesia eyes Islamic repo rules, separate deposit insurance* "... Deposit Insurance Corporation or Lembaga Penjamin Simpanan (LPS) plans to create a separate scheme to guarantee Islamic bank deposits, according to Bisnis Indonesia newspaper... Lack of Islamic deposit insurance has been a longstanding problems in the industry, which is set to aggravate due to incoming requirements from Basel III regulatory standards... Analysts believe creating a separate fund to cover Islamic deposits would also improve customer perception of the industry."

2. Jordan Deposit Insurance Corporation, 2012: *Deposit Insurance for Islamic Banks* "The General Ifta' Department issued its Fatwa nr (13/2012) relevant to the permissibility of insuring deposits held with Islamic Banks. The Fatwa stipulates that: '..., the Council considers the legitimate permissibility of establishing the Fund for Islamic deposit insurance and the soundness of proposed amendments on the draft Law and its compliance with the respective Shari'ah Principles. And God knows best'."

3. Butterworths Journal of International Banking and Financial Law, February 2015: *Deposit Insurance frameworks for Islamic banks: design and policy considerations* (by Hdeel Abdelhady) "... Jordan was, as of November 2014, amending its law to es-

tablish an Islamic deposit insurance framework, to operate alongside its existing conventional system..."

4. The Jakarta Post, August 2, 2016: *Sharia compliance in deposit insurance* "... Under the deposit insurance system, a bank pays a premium to the deposit insurer and, if the bank is wound up, the deposit insurer reimburses the insured depositors. The acts of the deposit insurer in collecting the premium from the bank and reimbursing the insured depositors of the bank is deemed to be an interest-based transaction. Based on the above, deposit insurance does involve the exchange of money for money and the exchange occurs with different values and at different times... The interest element... exist in deposit insurance when the deposit insurer is involved in interest-based transactions or activities.

   This can happen when the deposit insurer protects deposits, invests the deposit insurance funds, lends to troubled banks or obtains external funds (when in deficit), as all these activities are based on interest. In addition to interest, uncertainty and gambling, Sharia also does not allow certain other elements to exist in a deposit insurance system. For instance, deposit insurance funds must not be used for the purchase of liquor and pork, or any activities prohibited under Sharia."

5. Central Bank of Bahrain, 2012: *Consumer Information: Deposits & Unrestricted Investment Accounts Protection Scheme* "The Central Bank of Bahrain has issued on the 13th January 2011, Resolution No. (34) for the year 2010 with respect to promulgating a Regulation "Protecting Deposits and Unrestricted Investment Accounts" in accordance with the provisions of Article 177 of the Central Bank of Bahrain and Financial Institutions Law No. (64) for the year 2006... In order to maintain a level playing field and to encourage a healthy competitive environment between Conventional and Islamic banks, the new scheme provides protection to unrestricted investment accounts in Islamic banks vis-à-vis the deposits in conventional banks. The new scheme requires the establishment of two

separate funds (Conventional fund and Islamic fund). ”

B Disciplining Based on Bank Insolvency in the Rubin Causal Framework

Let $y_{i,c,t}$ and $z_{i,c,t}$ respectively measure Islamic deposit supply and solvency of bank i, in country c at time t and $d_{c,t}$ be the announcement ofa separate Islamic deposit insurance. Consider the following empirical model of testing the degree of market discipline based on $z_{i,c,t}$ and $d_{c,t}$:

$$y_{i,c,t} = \beta_0 + \beta_1 z_{i,c,t} + \beta_2 z_{i,c,t} d_{c,t} + \epsilon_{i,c,t}, \tag{2.7}$$

where $\epsilon_{i,c,t}$ well-behaves. Then, before the announcement of a separate Islamic deposit insurance, the change in Islamic deposit supply due to a decline in bank solvency is given by:

$$\mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{high}, d_{c,t} = 0] - \mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{low}, d_{c,t} = 0] = \beta_1(z_{high} - z_{low}) \tag{2.8}$$

Thus, β_1 measures the degree of market discipline before the announcement of a separate Islamic deposit insurance. By the same token, after the announcement of a separate Islamic deposit insurance, the change in deposit supply due to a decline in bank solvency is given by:

$$\begin{aligned}\mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{high}, d_{c,t} = 1] - \mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{low}, d_{c,t} = 1] = \\ \beta_1(z_{high} - z_{low}) + \beta_2(z_{high} - z_{low})\end{aligned} \tag{2.9}$$

Thus, $\beta_1 + \beta_2$ measures the degree of market discipline after the announcement of a separate Islamic deposit insurance. Finally, the differential change in the deposit supply moving from before announcement to after announcement is given by:

$$\begin{aligned} &\mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{high}, d_{c,t} = 1] - \mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{low}, d_{c,t} = 1] \\ -[&\mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{high}, d_{c,t} = 0] - \mathbf{E}[y_{i,c,t}|z_{i,c,t} = z_{low}, d_{c,t} = 0]] \\ &= \beta_2(z_{high} - z_{low}) \end{aligned} \tag{2.10}$$

Thus, β_2 measures what happens to the degree of market discipline when we move from the pre-treatment to the post-treatment observations.

Chapter 3

Bank Failures and Wage Inequality*

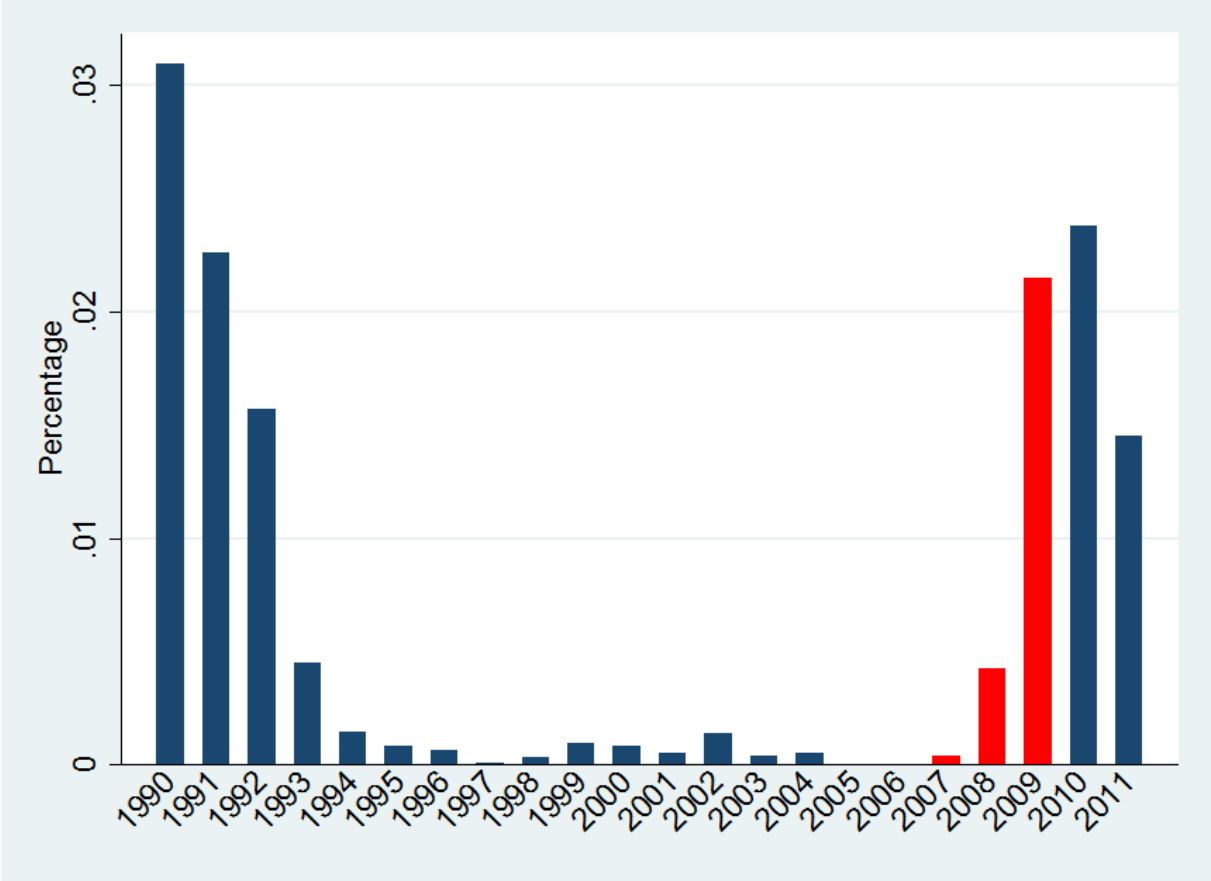
3.1 Introduction

A notable feature of the recent financial crisis has been the spike in local bank failures in the US, with the share of failing depository institutions approximating the levels seen towards the end of the savings and loan crisis in the 1990s (see Figure 3.1). Although there is a growing consensus that local bank failures lead to disruptions in credit provision, resulting in a decline in economic growth (Gilbert and Kochin (1989); Ashcraft (2005); Ziebarth (2013); Kandrac (2014)), it is still unclear whether the credit shocks induced by bank failures affect the whole population equally, or whether they disproportionately affect the rich or poor. This chapter attempts to fill this gap by analyzing the effect of local bank failures on the wage differential between skilled and unskilled workers; or, the skill premium.

There are both supply (worker) and demand (employer) side arguments explaining how bank failures may affect skilled and unskilled workers differentially. On the supply side, bank failures may amplify adverse economic shocks and unskilled workers, who may not have as much of savings compared to skilled workers, might be willing to supply more hours of work to smooth consumption. On the extensive margin of the labor supply, a similar effect may occur as a household strategy: Due to

*This chapter is a joint work with Christopher Martin Hols from the Bonn Graduate School of Economics.

Figure 3.1: Share of commercial bank failures



Notes: Data is obtained from Federal Deposit Insurance Corporation (FDIC), Failed Bank List. The figure shows the percentage of failed commercial banks as a share of the total commercial banks in the US for the time period 1990-2011. Years whose bars are indicated with red demonstrate the time frame considered in this chapter. We are not able to study the bank failure episodes after 2009 due to changing community borders, which is an essential ingredient for the identification strategy of this paper. This is explained in what follows.

unemployment of one spouse, the other, who would not have participated in the labor force otherwise, may enter into the labor force (Degirmenci and Ilkkaracan, 2013). To the extent that this occurs among unskilled individuals relatively more, it may exacerbate the wage differentials between skilled and unskilled individuals.

On the demand side, bank failures can influence both aggregate production and the allocation of credit, which may alter the demand for unskilled and skilled workers with concomitant ramifications on the wage differentials between skilled and unskilled workers (Townsend and Ueda (2006)). During episodes of lacking financing induced by bank failures, an average firm may have to scale down operations and reduce labor demand. In doing so, it may tend to protect more educated workers due to higher firing and future re-recruitment costs, leading to relatively lower demand for unskilled vis-à-vis skilled workers (Lopez and Oliviella (2014)). On the other hand, bank failures could trigger firm failures and drive down wages of either one of the groups that become unemployed due to failure of their employers.

Also, the effects of bank failures on wage inequality may not be homogeneous across all sectors of an economy. Heterogeneity may arise from the differential exposures of sectors to credit shocks. In particular, sectors may structurally differ in their dependence on bank credit and their technological ability to pledge collateral to alleviate the adverse effects of bank failures. For example, for sectors that rely on a type of capital that cannot be pledged as collateral, relationship lending can be an important financing mechanism and disruptions to credit relationships as a result of bank failures may affect these sectors more severely.[1] There may also be technological differences across sectors regarding the degree of complementarity/substitutability of skilled versus unskilled workers with the type of capital employed in production.

We argue that tangible capital increases the ability of a firm to alleviate the effects of negative credit shocks induced by local bank failures. Conversely, firms that use

[1]Boot (2000) demonstrates the importance of relationship lending in eliminating information asymmetries for borrowers with little collateral value. Kandrac (2014) documents that the negative effects of bank failures on the real economy was felt more severely for the US. counties where credit relationships were disrupted more during the 2008 financial crisis.

more intangible capital as opposed to tangible capital would be relatively more affected by bank failures. We also argue that intangible capital is knowledge-intensive and, therefore, relatively more complementary with skilled labor than unskilled labor. Hence, affected firms, which cannot pledge enough collateral, are the ones which are likely to demand less unskilled labor during episodes of bank failure.

Empirical testing of these hypotheses faces a significant endogeneity problem: Bank failures do not happen randomly.[2] Underlying economic conditions (demographics, corporate and household leverage, credit-worthiness of the community members) which vary over time may cause both the incidence of financial distress (Mian et al. (2013)) and widening inequality (Kumhof et al. (2015)). Similarly, regulatory institutions could "forbear" failing certain banks based on community-level economic trajectories that may coincide with factors affecting wage differentials between skilled and unskilled workers.[3]

We address these challenges by following a two-fold empirical methodology. First, we identify the location of failed bank branches in each Public Use Micro-data Area (PUMA) for the years 2007-2009.[4] We are not able to study the effects of bank failures after 2009 due to changing PUMA borders with the census of 2010. Following Ashcraft (2005) and Kandrac (2014), we classify PUMAs with at least one failed bank branch as affected areas. We then estimate a Mincerian skill-wage equation to test if the wage gap between skilled and unskilled workers is higher in affected PUMAs. Importantly, we enrich our regressions with several multi-way fixed effects that control for all time-varying community, sector as well as community and sector differences that capture underlying economic conditions across each industry within a PUMA. Conditional on all these time-varying and sector- as well as community-specific characteristics, we argue that failures of thrift and depository institutions are exogenous to individual workers. Hence, a multi-way fixed effects approach enables us to abstract from the

[2]See Demirgüç-Kunt and Levine (2009) for an overarching description of the endogenous relationship between financial distress and inequality.

[3]See Brown and Dinç (2011) for the determinants of regulatory forbearance.

[4]PUMAs are geographical units used by the US Census for providing statistical and demographic information. Section 5 offers an extensive description of PUMAs.

endogeneity concerns mentioned above.

Our findings indicate that local bank failures indeed widened the wage gap between skilled and unskilled workers during the 2007-2009 financial crisis. These findings are statistically and economically significant and suggest that local bank failures explain around 6% to 10% of the average annual wage gap between skilled and unskilled workers. We apply several tests to show that these results are not driven by other confounding factors documented for similar setups such as suppressed unskilled wages causing subprime defaults that, in turn, lead to bank failures (Duffie (2010); Mian and Sufi (2014)).

In the second step, we focus on whether the increase in the skill premium as a result of bank failures is demand- or supply-driven. To this end, we follow Popov and Rocholl (2016) and investigate the effect of bank failures not only on price (wages) but also on quantity (labor hours) measures. Simultaneous consideration of wages (price) and labor hours (quantity) in a reduced-form model enables us to conclude if the observed effects are demand- or supply-driven. In addition, we apply several tests to study the impact of local bank failures on the labor force participation, labor market attachment of individuals and the adjustment mechanism of labor. We conclude that the observed effects are indeed driven by a differential decline in unskilled labor demand, not by a reduction in unskilled employment but cuts in working hours of the unskilled.[5] Importantly, local bank failures not having an impact on unemployment suggests the following: A potential explanation where bank failures push firms into failures, making unskilled unemployed, and ultimately driving the wage gap, is unlikely because we can reject the hypothesis that bank failures lead to unemployment.

We further substantiate the demand channel by exploiting the variation of knowledge intensity across sectors. The idea is the following: If credit shocks induced by bank failures increased the skill premium, it should have done more so in sectors for which skilled workers are indispensable for production, or to put it differently, sec-

[5]We also find evidence that bank failures result in the differential decline in full-time unskilled employment. This is on a par with the adjustment of labor costs through cuts in working hours.

tors that are knowledge intensive. Following the definition of knowledge intensity by Claessens and Ueda (2008), we document that the effect of bank failures on the skill premium positively depends on the knowledge intensity of the sectors.[6] The economic significance of the results suggests that one standard deviation increase from the mean in the knowledge intensity results in 2.8% more bank failure-induced skill premium, which, evaluated at the average, translates into an annual increase of US $1,260.

In the next step, continuing with the sector approach, we identify a unique channel for the observed demand-driven wage inequality. We exploit sectoral heterogeneity across the usage of tangible and intangible capital to test if one observes higher wage inequality in more affected firms, which use more intangible capital and less tangible capital. In particular, we document that the effect of bank failures on wage inequality is differentially higher for sectors that rely more on intangible capital relative to tangible capital. We attribute this finding to the fact that the type of capital firms use matters for the transmission of local credit shocks to the labor market. These results are consistent with knowledge-dependent capital being financed through forgone earnings of unskilled workers when total financing capacity shrinks.[7]

The remainder of this chapter is organized as follows: Section 2 presents the literature review and the contribution of this study. Section 3 provides background information on the institutional setup. Section 4 explains the mechanism through which bank failures lead to increased wage inequality. Section 5 describes the data used. Section 6 presents the empirical methodology employed in the study and presents the baseline results. Section 7 establishes the channel through which bank failures widen the wage gap between skilled and unskilled workers. Section 8 presents the additional robustness tests and, finally, Section 9 concludes.

[6]The ratio of research and development (R&D) expenditures to sales is the measure proposed by Claessens and Ueda (2008).

[7]Even if the firms reliant on intangible capital (hence, more affected) might fail after their banks fail, laying off and ultimately driving down the wages of skilled workers, these can only lead to the underestimation of our results.

3.2 Literature Review

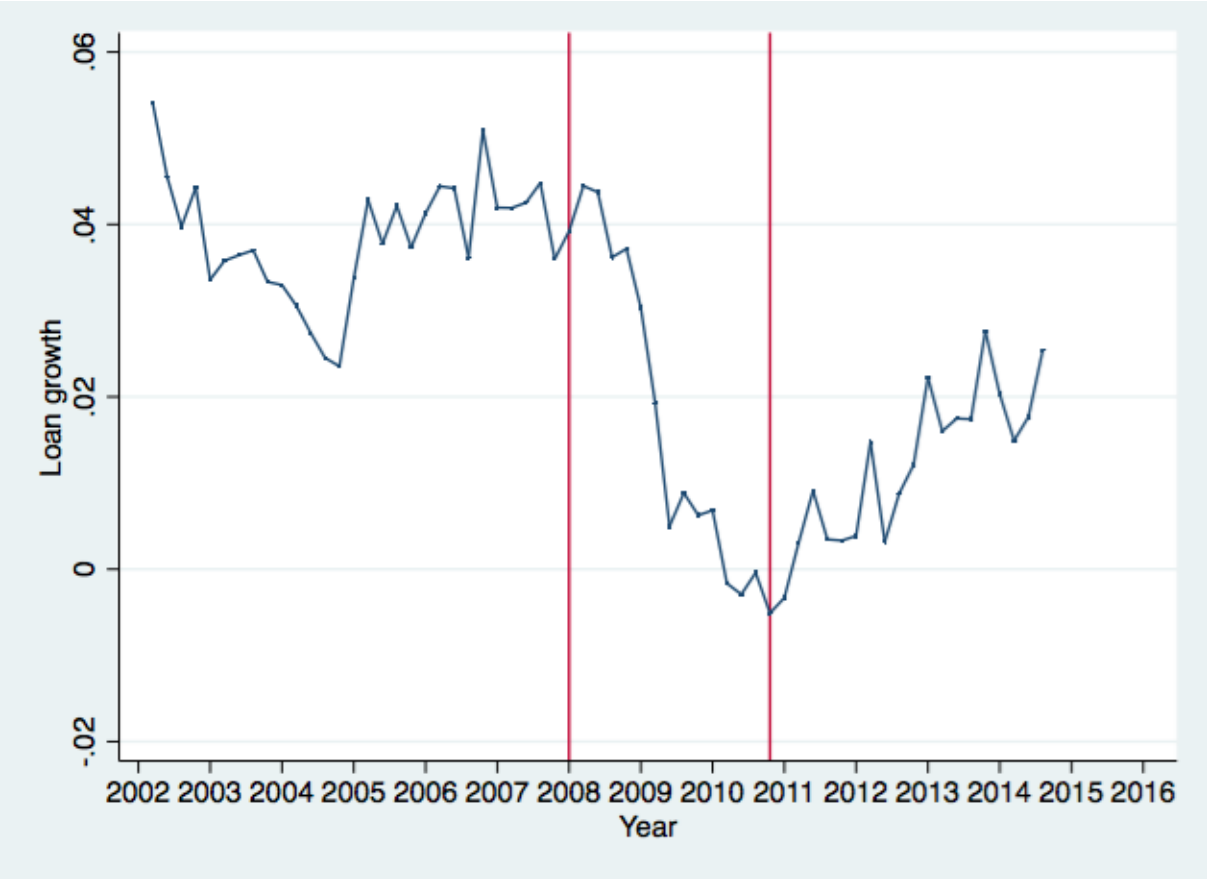
"The bank failure channel" as a term is brought up to summarize the mechanisms through which bank failures can influence the real economy (Ramirez and Shively (2012)). There are two main channels through which this can happen: (i) Wealth effect - a bank failure can result in the loss of wealth for uninsured depositors or other creditors. Friedman and Schwartz (1963) and Calomiris (1993) show that the adverse wealth effects induced by bank failures can be felt in the real economy through a reduction in consumer spending. (ii) Credit effect - a bank failure can result in disruptions in bank-firm credit relationships, in which case bank-dependent firms may face an increased cost of funds. Besides, a local bank failure can lead to credit crunches not only through a contraction in loan supply but also more apprehensive lending behavior by surviving banks in view of increasing uncertainty in the local economy (Bernanke (1983); Calomiris and Mason (2003)).

We rule out the possibility that the bank failures observed during the recent financial crisis created an adverse wealth effect for several reasons. First, the US Emergency Economic Stabilization Act, introduced in October 2008 increased the deposit insurance coverage from US $100,000 to 250,000 to protect the wealth of depositors and prevent bank runs. Secondly, Kandrac (2014), Granja (2017) and Vij (2018) argue that the resolution regime applied in 95% of the failed bank cases during the recent financial crisis ensured that the acquiring banks would assume 100% of deposits of the failed banks. As a result, the deposit franchise value of failed banks was directly transferred to acquiring banks. In addition to this, Kandrac (2014) and Vij (2018) document that it was the contraction in loan supply and disrupted credit relationships induced by bank failures that led to the observed decline in economic activity in the US during the recent financial crisis.

Besides, we document suggestive evidence on the apprehensive effects of bank failures on the lending behavior of surviving banks found by Bernanke (1983) and Calomiris and Mason (2003) for similar environments. Figure 3.2 plots the lending

behavior of surviving banks when their peers that operate in the same county fail. It confirms that the loan growth by the surviving peers drops dramatically, reaching even negative growth during the recent financial crisis. Therefore, we argue that bank

Figure 3.2: Lending behavior of banks whose peers failed



Notes: This figure shows the mean loan growth for banks whose peers, banks operating in the same counties, failed. Vertical lines indicate the sample period considered in this chapter. The data sources are the FDIC Summary of Deposits and the FDIC Call Reports.

failures observed during the recent financial crisis induced negative credit shocks. This is in line with studies that look at similar environments and claim the credit effects as the primary channel through which bank failures affected the real economy throughout US history (Gilbert and Kochin, 1989; Ashcraft, 2005; Ziebarth, 2013; Kandrac 2014; Vij, 2018). These studies conclude that negative credit shocks induced by bank failures result in a decline in economic activity. We contribute to this literature by focusing on the distributional effects of bank failures. In particular, we show that bank failures result in differential demands widening the wage gap between skilled

and unskilled workers.

This study also contributes to the newly emerging literature on how finance affects wage inequality. Beck et al. (2010) showed that branch deregulation decreases overall income as well as wage inequality, while Jerzmanowski and Nabar (2013) showed that the effect of branch deregulation reduces wage inequality. Larrain (2015) found that financial capital account opening by eastern European countries increased the wage gap between skilled and unskilled workers through the channels of capital-skill complementarity and skill-biased technical change. This study also contributes to this stock of evidence by introducing the channel of bank failures as a determinant of wage inequality.

There is an extensive literature setting out the empirical evidence on the rising skill premium and providing potential explanations (Katz and Murphy (1992); Acemoglu (2003); Acemoglu and Autor (2011)). Accelerating demand for skill due to skill-biased technological change (Katz and Murphy (1992)), changes in the organization of production such as changes in factory systems (Acemoglu (1999); Beaudry and Green (2003); Caroli and Van Reenen (2001)) and international trade (Acemoglu (2003); Burstein and Vogel (2017)) are provided as key explanations for the rising trend in the skill premium observed since the 1970s in the US. Although our study has a short-term view and focuses on explaining wage inequality during the recent financial crisis, it expands the literature on the skill premium by showing that bank distress has an amplifying effect on it.

Our work is very closely related to previous studies that focus on the impact of imperfections in the financial market on employment outcomes. The earlier evidence relied on indirect measures of financing constraints such as balance sheet size of the firms to pin down the effect of monetary policy on firm employment (Gertler and Gilchrist (1994); Sharpe (1994); Nickell and Nicolitsas (1999)). More recent evidence uses measures of external financial dependence introduced by Rajan and Zingales (1998) to show employment effects. Duygan-Bump et al. (2015) showed that during episodes of economic downturn, workers in small firms are more likely to become

unemployed provided that they work in industries with high external financial dependence. Benmelech et al. (2011) showed that shocks abroad matter for domestic labor market conditions: A sharp decline in prices of real estate in Japan increases unemployment in US metropolitan statistical areas with Japanese-affiliate banks. Greenstone et al. (2014) found that the decline in small business lending in the US translates into low levels of firm creation and higher unemployment. We expand this literature by analyzing the distributional effects of financial distress in labor markets.

There is also a growing stock of evidence studying the effects of financial distress on employment outcomes with bank-firm matched data. Chodorow-Reich (2014) exploited the event of Lehman bankruptcy and documented that firms with former credit relationships with unhealthy lenders reduced employment by more compared with healthier lenders' clients in the US. Similar evidence for Germany was documented by Popov and Rocholl (2016). They studied the impact of funding shocks on German savings banks during the US subprime mortgage crisis and found that firms with credit relationships with affected banks experienced a decline in labor demand. There are advantages and disadvantages of using bank-firm matched datasets. Although such datasets allow precision in the assignment of treatment, Minamihashi (2011) documented for the case of Japan that specific types of firm deal with unhealthy banks and observed real outcomes as bank distress can be explained 80% of the time by self-selection bias. Moreover, bank-firm matched datasets do not allow for a holistic analysis of bank distress such as of the apprehensive credit contraction by surviving banks mentioned above. Therefore, we compare similar communities with and without bank failure experience to study the overarching effects of negative credit shocks on wage inequality.

3.3 Resolution of Failed Banks

Vij (2018) stated that bank insolvencies are administered under the FDIC through a non-judicial process. The financial health of the commercial banks is screened contin-

uously by their regulators at either state or federal level and the case of resolving a bank is launched when regulatory monitoring yields the result that a bank is highly undercapitalized, meaning its equity to assets ratio is less than 2%. If the regulator and the FDIC agree on closing the bank, the FDIC enters as the receiver. There is no judicial option available for the failed bank itself or its creditors to challenge the receivership by the FDIC (Ragalevsky and Ricardi (2009)).

Overall the FDIC has two primary roles in its capacities regarding managing issues on bank failure. First, it provides deposit insurance services to covered banks. Secondly, it is in charge of settling affairs in bank failure cases. By doing so, the FDIC is mandated to choose the resolution scheme that is least costly for itself. To this end, it initially undertakes procedures to value the assets and liabilities of failed banks. It then evaluates which option is least costly from the perspective of the deposit insurance fund (FDIC (2014)).

During the recent financial crisis, the primary resolution mechanism adopted by the FDIC for an overwhelming majority of bank failures was the P&A transaction. Under such an arrangement, the failed banks are auctioned to healthy banks through a process very similar to a first price sealed bid auction. The winning bank comes into agreement with the FDIC on whether the assets would be sold to another bank, and if so, at what price and whether the FDIC would share the potential future losses as a result of maintaining troubled assets of a failed bank, i.e., loss-sharing agreement. Kandrac (2014) stated that during the recent financial crisis, loss-sharing contracts were commonly applied to preserve the credit relationships between the borrowers and the local bank branch.

One may argue that due to the presence of an acquirer, the adverse credit effects of bank failures should be negligible. However, Vij (2018) showed that the business lending portfolio of an acquiring bank significantly shrinks after the acquisition process. Moreover, Granja et al. (2017) showed that although the P&A mechanism meant that there was almost always an acquiring bank for failed banks, there was a suboptimal allocation of failed banks to potential acquirers, leading to a disruption of existing

credit relationships. Therefore, it is credible that bank failure episodes observed during the recent financial crisis led to negative credit shocks.

3.4 Hypothesis Development

Previous literature shows that during episodes of financial distress when firms have to cut down production and input costs, they may have skill-specific labor demand (Lopez and Oliviella (2014)). In particular, firms may tend to protect more educated workers due to higher firing and future re-recruitment costs, leading to relatively lower demand for unskilled versus skilled workers. Besides, employment protection legislation (EPL) could also create skill-specific unemployment risks (Bennett (2016)), which may favor skilled vis-à-vis unskilled workers, especially during times of reduced financing. This leads us to form our first hypothesis:

Hypothesis 1: *Local bank failures will increase wage inequality between skilled and unskilled workers (the skill premium) by, in relative terms, inducing firms to demand more skilled vis-à-vis unskilled labor, widening the wage gap between the two types of workers.*

It should be noted that Hypothesis 1 does not necessarily state that firms will demand, in absolute terms, more skilled and less unskilled labor as a result of bank failures. It instead argues that although there can be a decline in the labor demand overall, reduction in the demand for unskilled labor will be relatively more severe than that of skilled labor, which ultimately widens the wage gap between skilled and unskilled workers.

Moreover, observed effects of banks failures on wage inequality may be heterogeneous across sectors of the economy. First, it can be argued that how firms would be affected by bank failure-induced credit shocks is very much dependent on how much firms rely on external finance as opposed to internal finance. Rajan and Zingales (1998) showed, in their seminal contribution, that for technological reasons, sectors are heterogeneous regarding their need for external finance. It is therefore plausible to claim that, holding everything else constant, industries that rely more on external

finance are likely to be more affected by bank failures. Thus, to the extent that externally dependent sectors demand more skilled labor vis-à-vis unskilled labor, bank failures may widen the wage gap between skilled and unskilled workers.

Second, the heterogeneity in firms' exposure to credit shocks induced by bank failures can also stem from the heterogeneity in firms' ability to pledge collateral. It has been extensively shown that during financial distress episodes, determinants such as collateral constraints and debt overhang tighten the financial constraints faced by firms. Put differently, in financial distress situations, in which information asymmetries are exacerbated, a firm's ability to be able to pledge collateral could be vital for accessing scarce credit. Moreover, firms which, for technological reasons, are less capable of pledging collateral can rely more on relationship lending. Establishing and maintaining relationships with borrowers is a crucial way to alleviate information asymmetries by banks. Learning about borrowers allows banks to eliminate negative NPV projects over time and overcome adverse selection (Boot (2000); Ongena and Smith (2001); Degryse and Ongena (2001), (2005); Degryse et al. (2018)). Therefore, if bank failures lead to disruptions in credit relationships, it is expected that firms that cannot easily pledge collateral would be affected more severely by this. Overall, if firms that are less able to pledge collateral happen to be the firms that demand more skilled as opposed to unskilled labor, financial distress situations can indeed increase the wage inequality between skilled and unskilled workers.

The following question then arises: What is the link between a firm's exposure to credit shocks and its relative demand for skilled versus unskilled labor?

The type of labor a firm demands is very much dependent on the type of capital a firm employs. At the same time, the type of capital a firm uses can also determine how much a firm would be exposed to negative credit shocks (disruptions in credit relationships, credit crunches, apprehensive lending by surviving peers etc.) from local bank failures. Intangible capital, which is arguably more knowledge-dependent than tangible capital, complements skilled labor more than unskilled labor (Hall (2000) (2001)). Firms that have relatively more intangible capital naturally have more elastic

demand for unskilled labor compared to firms that operate with more tangible capital. However, as outlined by previous literature (Almeida and Campello (2008); Rampini and Viswanathan (2010)), tangible capital has a comparative advantage in terms of serving as collateral vis-à-vis intangible capital. Assets that are more tangible sustain more external financing because such assets mitigate contractibility problems: Especially during episodes of financial distress such as bank failures, in which information asymmetries and contractibility problems are exacerbated, possession of tangible capital that can be pledged as collateral is arguably vital to alleviate the negative effects of credit contractions.

Hence, during episodes of bank failures, firms that rely relatively more on intangible capital would be more affected by credit contraction. These are the firms that demand more skilled labor as opposed to unskilled labor. In a way, the affected firms, which cannot pledge enough collateral, are the ones which are likely to demand less unskilled labor in episodes of bank failures. In other words, when bank failures hit the local economy and lead firms to decrease input costs, the most affected sectors are the ones that rely more on skilled labor for sustaining production and that can give up employing unskilled labor relatively easily.

It should also be noted that the extent to which firms would be affected by bank failures does not only depend on the share of tangible capital in the overall capital. It also depends on to what extent firms are dependent on external finance. In fact, for firms that rely on external finance to a lesser degree or not at all, the effects of bank failures would be very indirect and minimal. Therefore, while testing the effect of intangibility on the gap between skilled and unskilled workers, we need to control for external dependence so that the sectors can be comparable to one another in terms their reliance on the external finance [8]. This leads us to form our second hypothesis:

Hypothesis 2: *Holding dependence on external finance constant, the effects of bank failures on the skill premium will be higher in sectors that rely more on intangible capital vis-à-vis*

[8] Although we control for sectors' dependence on external finance, in one specification, we also show the effect of interaction of intangibility and external dependence

tangible capital.

It is important to note that the hypotheses we describe in this section need not be the only answers to why and how bank failures may affect wage inequality. For example, one potential explanation for how the effects of bank distress have been felt in labor markets could be skill-biased technological change as documented for similar environments by Jerzmanowski and Nabar (2013) and Larrain (2015). Due to data limitations, we are unable to study the long-term effects or any other potential channel that may link bank failures with wage inequality. What we can do, however, is to control for all factors that are changing at community, sector and time levels through multi-way fixed effects to control for the unobserved confounders, which is explained in detail in the next section.

3.5 Data

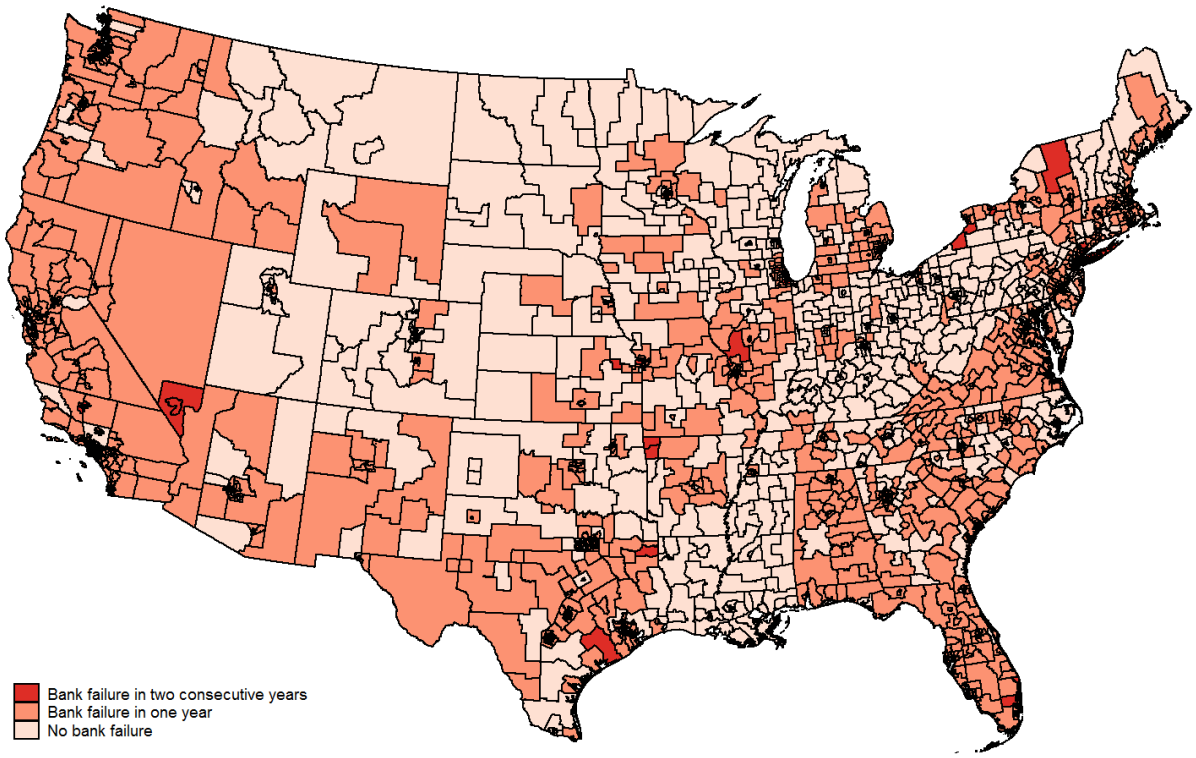
We follow Kandrac (2014) and take each PUMA as a separate unit of analysis. PUMAs are geographic divisions created by the US Census with the aim of providing statistical and demographic information. The reason for focusing on PUMA classification as opposed to more granular geographical units such as zip code or census tract is that PUMAs are the most granular geographic designation in the American Community Survey, from which we obtain American workforce information. In total, there were 2,071 PUMAs, as of the 2000 Census. PUMAs comprises at least 100,000 people and are always contained in a state.

We proceed by (1) identifying PUMAs affected by the failure of a financial institution in a given year, and (2) measuring the subsequent wage gap between skilled and unskilled workers. Even though the outreach of a bank branch may go beyond PUMA borders, we only focus on the effects in the PUMAs in which failed banks operated (Ashcraft (2005); Kandrac (2014)). The literature on relationship banking suggests that physical distance to a bank branch is a crucial factor in establishing bank-customer relationships (Whitehead (1982); Hannan (1991); Laderman (2008); Kandrac (2014)).

Moreover, to observe a PUMA-level effect, it is not essential that the market for an individual branch is limited to a single PUMA, but it is sufficient that a branch is mostly involved in the PUMA in which it operates. In either case, any potential adverse credit shocks induced by the failure of a bank would mainly be felt in areas geographically closest to a bank. Even though the impact would be felt more in the former case, it would nevertheless result in underestimating the actual effect.

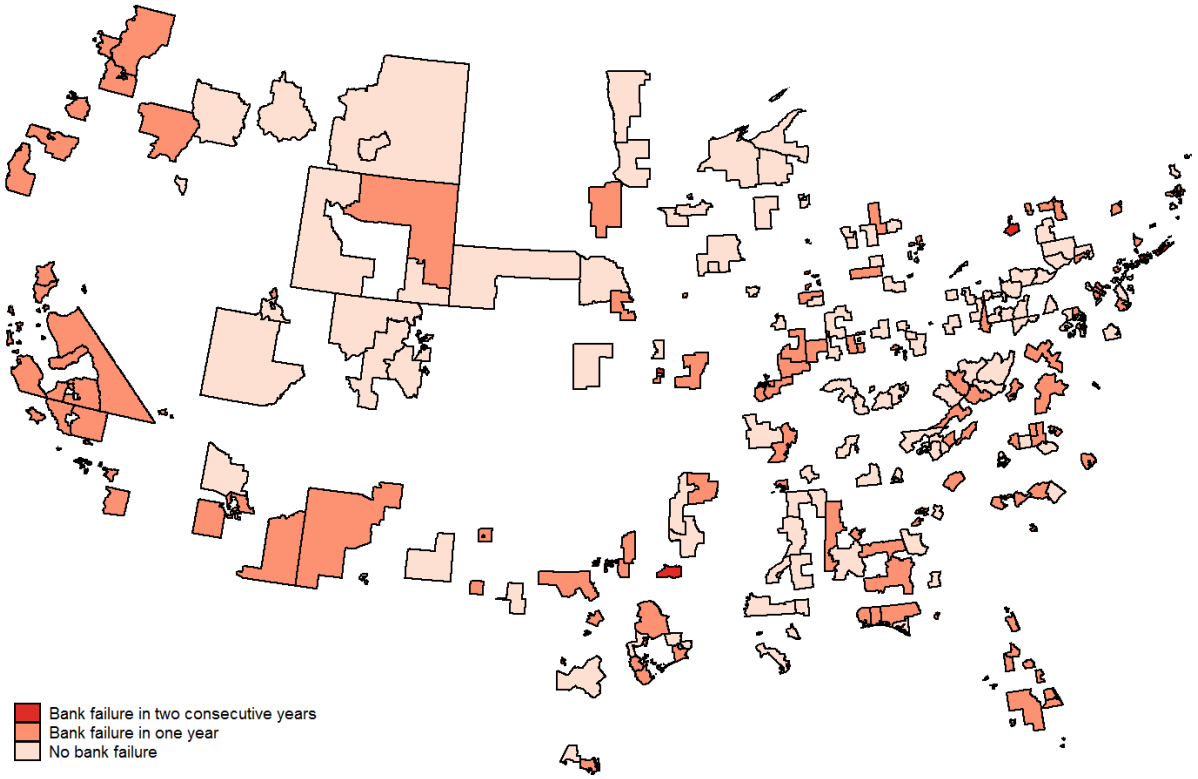
To begin with, we pinpoint the geographical location of each bank branch nationwide for 2007-2009. The FDIC's Summary of Deposits contains latitude and longitude information of each bank branch. This makes it possible for us to pinpoint the exact location of each bank branch. We then draw 2071 polygons on a physical US Map by using information on PUMA borders' coordinates as stated in Census 2000 from the Missouri Census Data Center. By applying a method that matches points to polygons, we can assign a PUMA code for each bank branch. Combining this information with the List of the FDIC's Failed Banks, we obtain the number of failed banks in each PUMA for each year. Figure 3.3 displays a heat map of the bank failure experiences of each PUMA in the continental US. Next, we gather worker-level information for each community from the American Community Survey (ACS). It is a current survey conducted by the US Census Bureau. It regularly collects information on par with the prior decennial census such as demographic, workforce, socio-economic and housing characteristics. The data collected by the ACS is mainly used to measure state-level statistics, allocate funding and track demographic characteristics of US states. Nearly 295,000 individuals/households respond the survey every month, which makes it the largest household survey of the US Census Bureau. It is crucial to note that the survey is intentionally done on a large number of people to ensure that it is always representative at PUMA- and state-levels as well as nationwide.

Figure 3.3: Bank failures by PUMA



Notes: Geo-code data of PUMA borders are obtained from the Missouri Census Data Center. Data on bank failure episodes are obtained from the FDIC's list on bank failures, which is merged with the FDIC Summary of Deposits. This figure highlights the bank failure episodes in PUMAs, with borders as of Census 2000. PUMA borders changed substantially with Census 2010.

Figure 3.4: Subsample: Bank failures by PUMA



Notes: Geo-code data of PUMA borders are obtained from the Missouri Census Data Center. Data on bank failure episodes are obtained from the FDIC's list of bank failures, which is merged with the FDIC Summary of Deposits. This figure highlights the bank failure episodes in 414 randomly sampled PUMAs, with borders as of Census 2000. PUMA borders changed substantially with Census 2010.

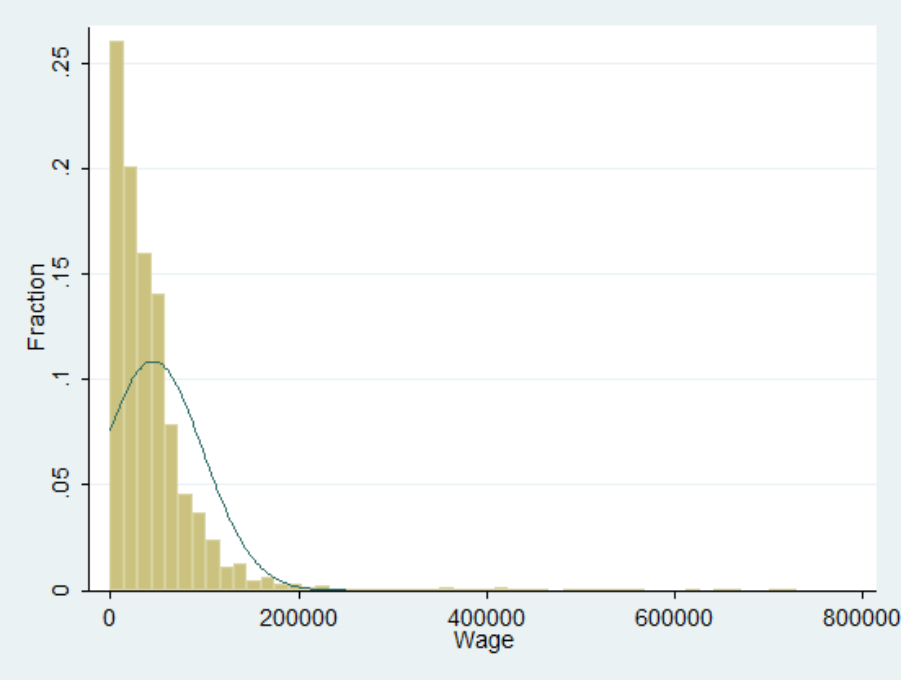
Table 3.1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Wage	45025.164	53641.319	4.348	729834	1143681
Hours	1759.665	812.025	7.5	5049	1222378
Labor Force	0.621	0.485	0	1	1883442
FAILED	0.212	0.409	0	1	1880454
EDUC	0.538	0.499	0	1	1883442
EDUC*FAILED	0.119	0.324	0	1	1880454
Level of education	2.698	1.190	0	5	1883442
Employed	0.590	0.492	0	1	1883442
Layoff	0.020	0.140	0	1	1883442
Fulltime	0.613	0.487	0	1	1222378
Available	0.228	0.659	0	1	1883442
Searching	0.058	0.234	0	1	1883442
Experience	5.935	4.531	1	15	1454709
Experience^2	55.748	67.882	1	225	1454709
Foreign Born	0.125	0.331	0	1	1883442
Race	1.845	1.677	1	7	1883442
Female	0.519	0.5	0	1	1883442
EDUC*Experience	2.772	4.066	0	15	1454709
EDUC*Experience^2	24.219	50.768	0	225	1454709
Age	47.583	18.923	16	95	1883442
Age^2	2622.218	1911.461	256	9025	1883442
Married	0.545	0.498	0	1	1883442
Married*Child	0.231	0.421	0	1	1814114
Female*Child	0.201	0.401	0	1	1814114

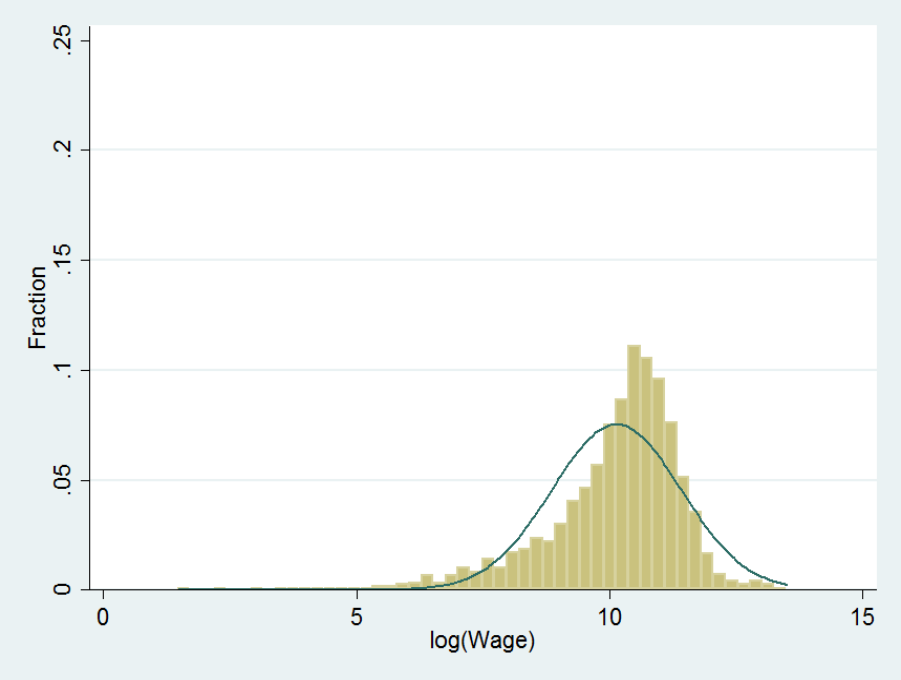
Notes: The exact definitions of all variables are given in the text and Table 3.2. Note that EDUC is a dummy variable whereas Level of education is a categorical variable.

Figure 3.5: Fractional histogram of wages versus log(wages)

(a) Annual wages



(b) Logarithm of annual wages



Notes: Data are obtained from the American Community Survey for the period 2008-2010. Panel (a) depicts the fractional histogram of annual wages whereas Panel (b) depicts the factional histogram of annual natural logarithm of wages. In order to make the panels comparable to one another, heights of the bars are scaled so that the sum of heights equals 1 in both panels.

We obtain information on yearly gross wages, the annual number of hours worked, the industry of a worker, schooling as well as demographic characteristics (age, sex, marital status and the like) from the ACS for the period 2008-2010. Although bank failure episodes were experienced even after 2010 (see Figure 3.1), it is not possible to extend the analysis to a broader time horizon because the PUMA borders significantly changed following the Census 2010 classification in 2011. Also, due to the immense size of the ACS (around 12 million observations for the period 2008-2010) and lack of computing power, it is not feasible to conduct the analysis with the entire dataset. Thus, we carry out a random sampling of 414 PUMAs (equivalent to taking a random sample of around 20% of all PUMAs) and analyze the effect of bank failures on labor market outcomes on this subgroup of PUMAs.[9] Figure 3.4 shows the PUMAs we are left with after random sampling. This subsample gives a fair picture of the bank failure episodes and includes PUMAs from all regions. Importantly, the observations remaining in the working-sample are still representative of their respective PUMAs.

We follow Jerzmanowski and Nabar (2013) and only focus on workers of working age and in or out of the labor force. That is, individuals who are below the age of 15 and above the age of 65 are not included in the sample. Table 3.1 outlines the summary statistics of the key dependent and independent variables (covariates) used in the analysis. The primary outcome variable of the study is annual wages. Table 3.1 shows that this variable has a mean value of US $45,025. Previous literature such as Lydall (1959) and Lillard and Willis (1978) mentioned the skewed distribution of annual earnings in setups with wage-schooling models. This observation is also eminent in our sample (see Figure 3.5).

Panel (a) in Figure 3.5 demonstrates that the histogram of yearly wages is right-skewed to a significant extent. To correct for this, and also in line with the theory of deriving skill-wage equations (Mincer (1974)), we focus on the natural logarithm of wages throughout the analysis. The fractional histogram of the annual wages after

[9]We also run baseline regressions with the entire sample and confirm the findings generated through the subsample.

taking the natural logarithm is depicted in Figure 3.5, Panel (b). It is indeed evident that the natural logarithm of wages ensures penalization of outliers and pulls together the distribution.[10]

Importantly, we use annual wages as opposed to hourly wages in our regression following Jermanowski and Nabar (2013) and Larrain (2015). It is a well-established phenomenon that hourly wages cannot be adjusted downwards (wage rigidity). In other words, the downward responsiveness of wages to economic shocks is almost zero (Götte et al. (2007)). For our setup, this may imply that there is no change in the average hourly wage gap between skilled and unskilled individuals following a bank failure. Therefore, focusing on annual wages gives a complete picture of the skill premium characteristics. To illustrate this, for an individual who switches from full-time to part-time work due to a decline in demand for her labor, the hourly wage may still stay the same, but her annual earnings undergo a dramatic decrease. The literature on how firms adjust labor costs supports this example by pointing out that even though wages are rigid, this may not necessarily imply a rigid labor cost structure for firms as the cost adjustment can also be through working hours (Babecký et al. (2012)).

Another outcome variable used throughout the study is annual hours worked, which we constructed using the information on the number of weeks as well as weekly working hours provided in the ACS. In particular, the ACS offers six intervals within which the weekly hours worked lie. We take the average of each interval and multiply this by the number of weeks worked to build a proxy for the otherwise unknown number of working hours in a given year. Table 3.1 shows that this variable has a mean value of 1759 hours. We also use binary variables of labor force participation, employment, job-search, availability for work and being laid off as dependent variables to understand the labor adjustment mechanisms better. Table 3.1 shows the

[10]The wage distribution shown in Figure 3.5 refers to the nation-wide wage distribution for our sample period. It assumes that one dollar is worth the same everywhere. Although this may not necessarily hold, we are do not have a PUMA-level price measure to account for real wages. In addition, our presentation is in line with Mincer (1974) and other studies with skill-wage equation.

shares for these categories in our sample.

Our "treatment" variable, $FAILED$, is a binary variable with a value of one (1) for PUMAs in which a failed bank branch is located and zero (0) elsewhere. As Table 3.1 indicates, around 21% of observations are associated with a PUMA in which at least one bank failure occurred during the period 2007-2009. We follow Jerzmanowski and Nabar (2013) and focus on a binary classification of skill. In particular, we classify individuals with a college degree or above as skilled and unskilled otherwise and encode this relationship as $EDUC$. Table 3.1 indicates that nearly half of the individuals are skilled, following this classification. The identification strategy of this study lies in measuring the average wage gap between skilled and unskilled workers in PUMAs with or without a failed bank experience, which is depicted as the interaction term, $EDUC * FAILED$. Around 12% of the workers are skilled and located in PUMAs with bank failures.

Our regressions are also enriched by several covariates in the form of continuous, categorical and dummy variables, as shown in Table 3.1. In particular, we have standard Mincerian skill-wage control variables such as experience and experience squared (measuring the decreasing returns to experience) as well as additional control variables such as age, age squared, race, sex, marital status, maternity or paternity status, being born in a foreign country, and interactions of these variables. Definitions of the control variables as well as other variables used throughout this chapter are provided in Table **??**.

Table 3.2: Other variables

Name	Type	Description
Level of education	Categorical	Individual-level integer scores from one to five, five indicating the highest-level education
Employed	Dummy	Indicator variable with value one if an individual is employed
Layoff	Dummy	Indicator variable with value one if an individual is laid-off
Fulltime	Dummy	Indicator variable with value one if an individual works full-time
Available	Dummy	Indicator variable with value one if an individual is available for work
Searching	Dummy	Indicator variable with value one if an individual is looking for work
Experience	Categorical	Individual-level integer scores from one to fifteen, one indicating the highest-level experience
Experience^2	Categorical	Squared value of experience variable as explained above
Foreign born	Dummy	Indicator variable with value one if an individual was born abroad (not in the US).
Race	Categorical	Individual-level integer scores from one to seven, each score being attributed to a different race
Female	Dummy	Indicator variable with value one for female individuals in the sample and zero otherwise
E*Experience:	Categorical	Interaction of experience scores and $EDUC$
E*Experience^2	Categorical	Interaction of squared experience scores and $EDUC$
Age	Absolute number	Age of a selected individual in the sample
Age^2	Absolute number	squared value of Age variable.
Married	Dummy	Indicator variable with value one for married individuals in the sample and zero otherwise
Married*Child	Dummy	Indicator variable with value one for married with children individuals in the sample and zero otherwise
Female*Child	Dummy	Indicator variable with value one for female individuals with children and zero otherwise

Notes: This table presents the type and description of the covariates used in the analysis.

3.6 Method and Results

3.6.1 Baseline Results

Following Jerzmanowski and Nabar (2013), we first estimate a standard Mincerian earnings equation of the following form:

$$ln(w)_{ipt} = \alpha_p + \alpha_t + \beta_1 EDUC_{ipt} + \beta_2 FAILED_{pt-1} + \beta_3 EDUC_{ipt} * FAILED_{pt-1} + \delta X_{ipt} + \epsilon_{ipt} \quad (3.1)$$

where, i stands for individual, p stands for PUMA and t stands for the year. $FAILED$ takes the value one (1) for the year that comes after the bank failure and zero (0) otherwise. X is the vector of covariates, including experience and experience squared of the Mincerian skill-wage model. Following Ashcraft (2005) and Kandrac (2014), we include $FAILED$ with a one-year lag to lessen the concerns regarding reverse causality. $EDUC$ is a dummy variable that takes the value one (1) for individuals with a high school diploma or with a lower degree of education and zero (0) for the ones who have some college (or more advanced) education.

PUMAs can be significantly different from each other. Based on the discussion in Section 1, there may be PUMA-level time-varying omitted factors that may lead to overestimation. To address this concern, we enrich the model above by including several multi-way fixed effects:

$$ln(w)_{ipt} = \alpha_{pt} + \beta_1 EDUC_{ipt} + \beta_2 FAILED_{pt-1} + \beta_3 EDUC_{ipt} * FAILED_{pt-1} + \delta X_{ipt} + \epsilon_{ipt} \quad (3.2)$$

$$ln(w)_{ispkt} = \alpha_{spk} + \alpha_{pkt} + \beta_1 EDUC_{ispkt} + \beta_2 FAILED_{pt-1} + \beta_3 EDUC_{ispkt} * FAILED_{pt-1} + \delta X_{isptk} + \epsilon_{ispkt} \quad (3.3)$$

$$ln(w)_{ispkt} = \alpha_{spkt} + \beta_1 EDUC_{ispkt} + \beta_3 EDUC_{ispkt} * FAILED_{pt-1} + \delta X_{ispkt} + \epsilon_{ispkt} \quad (3.4)$$

where s stands for the sector (based on three-digit SIC classification) and k stands for age cohort. In equations (3.2) and (3.4) we are unable to estimate the individual effect of $FAILED$ on $ln(w)$ since the multi-way fixed effects capture the entire variation that is changing at least at PUMA-year level. Sector fixed effects control for any potential wage difference attributable to the differences in sectors. Following the seminal contribution of Polachek (2008), we also include fixed effects for different age cohorts (for each age observed we include a dummy variable in the regressions). Our dataset is a repeated cross-section with a panel dimension at the PUMA-level. That is, although we can observe the same PUMAs over time, we are unable to track individuals over time. Tracking individuals would have been useful to observe the characteristics of individuals that may create wage differentials. However, by including age-cohort fixed effects, we are at least able to follow the same age cohort over the years within a PUMA, and, hence, can abstract from the potentially confounding effects that an age group could have on wages.

In Table 3.3, we present the estimation results arising from these equations. Columns (1) and (3) show that although average annual wages decline more in PUMAs with bank failure, the average wage gap between skilled and unskilled workers widens, i. e., the skill premium is significantly higher in PUMAs that saw bank failures in the previous year. In columns (2) and (4), we repeat the same exercise by controlling for all characteristics that change in PUMA and year by including PUMA-Year and PUMA-Ind-Age-Year fixed effects. Although we are unable to estimate the average effect of bank failures on the overall decline in wages (as PUMA-year fixed effects capture the entire variation in these dimensions), we are still able to estimate the interaction term. Overall, the estimation results in columns (2) and (4) verify the findings in columns (1) and (3). Therefore, the findings presented in Table 3.3, columns (1) to (4) suggest a strong positive relationship between bank failures and the wage gap between skilled and unskilled workers, with the direction of impact going from bank failures to the skill premium. In other words, bank failures result in widening wage inequality between skilled and unskilled workers. Moreover, these findings are also

Table 3.3: Baseline results: The effects of bank failures on the skill premium

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)	(5) log(Wage)	(6) log(Wage)	(7) log(Wage)	(8) log(Wage)
FAILED	-0.0351***		-0.0428***		-0.0425***		-0.0420***	
	(0.00935)		(0.0108)		(0.00750)		(0.0107)	
EDUC	0.789***	0.789***	0.396***	0.407***	0.568***	0.568***	0.393***	0.403***
	(0.0134)	(0.0134)	(0.0108)	(0.0149)	(0.00831)	(0.00831)	(0.0109)	(0.0150)
EDUC × FAILED	0.0454***	0.0489***	0.0426***	0.0466***	0.0566***	0.0594***	0.0415***	0.0456***
	(0.0117)	(0.0121)	(0.0115)	(0.0172)	(0.00807)	(0.00830)	(0.0114)	(0.0172)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	No	No	No	Yes	Yes	Yes	Yes
PUMA FE	Yes	No	No	No	Yes	No	No	No
Year FE	Yes	No	No	No	Yes	No	No	No
PUMA -Year FE	No	Yes	No	No	No	Yes	No	No
PUMA -Ind-Age FE	No	No	Yes	No	No	No	Yes	No
Ind-Age-Year FE	No	No	Yes	No	No	No	Yes	No
PUMA -Ind-Age-Year FE	No	No	No	Yes	No	No	No	Yes
Observations	911368	911368	654517	571112	911368	911368	654517	571112
R^2	0.168	0.170	0.818	0.885	0.375	0.376	0.819	0.885

Notes: The sample period is 2008-2010. The dependent variable is the natural logarithm of annual wages. Columns (1) and (5), (2) and (6), (3) and (7), (4) and (8) show the estimation results of equations (3.1), (3.2), (3.3) and (3.4) respectively. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. In line with equations (3.1), (3.2), (3.3) and (3.4), each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMA. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

economically significant: Bank failures are associated with roughly a 5% increase in the skill premium in the most conservative specification.[11] Considering all specifications, it is also evident that bank failures account for 6% to 10% of the skill premium observed in our data.

Regression estimations in Table 3.3 columns (1) to (4), use several control variables which are used as standard controls in estimating the Mincerian earnings functions. Following Jerzmanowski and Nabar (2013), we include the following additional control variables, Age, $Age-squared$, $married$, $married-child$, $female-child$, to ensure that the results are not driven by age, marriage, maternity (paternity) status and any interaction of the latter two.[12] We display these results with additional control variables in columns (5) to (8) of Table 3.3 and confirm the robustness of the previous findings to these additions.

We also investigate the effect of bank failures on the wages of skilled workers. That is, we consider a linear combination test where we fix $EDUC = 1$ and investigate what happens to average wages moving from $FAILED = 1$ to $FAILED = 0$. This is equivalent to testing whether the sum of the estimated coefficients of $FAILED$ and $EDUC * FAILED$ equals zero. Note that this test can only be applied to columns (1), (3), (5) and (7) because the coefficient of $FAILED$ can only be estimated in these specifications. Performing this hypothesis test, we find that in none of the specifications the effect of bank failures on skilled wages is significant (p values ranging between 0.174 and 0.986 across columns).

3.6.2 Bank Failures versus Local Recessions

Our fixed effects varying in PUMA and year dimensions help account for a potential confounding effect of local recessions. However, so far it has not been clear whether the estimated coefficient of EDUC $\times$ FAILED would remain significant if we included

[11] According to Halvoren and Palmquist (1980), the effect of dummy variables in semi-logarithmic equations is $(exp(\beta_3) - 1)$. Kennedy (1981) proposed a variance correction for this interpretation, which has a negligible impact here.

[12] Whenever we include age-cohort fixed effects, we are unable to estimate the individual impact of Age and $Age-squared$.

a variable capturing local recessions interacted with EDUC in our regressions.

As the time dimension of our study is quite short, it is not plausible to include measures of recessions based on the concept of the business cycle. Another limitation is that there is no GDP data to measure recessions at the PUMA level. Therefore, we use two alternative proxies for recessions. First, we receive quarterly state-level economic growth data from the Kansas FED and create a recession dummy variable taking the value one (1) for PUMAs whose states were in a recession, characterized by negative economic growth, for at least one quarter (encoded as REC). Second, from our ACS data set, we calculate the growth of the mean household income for each PUMA, which would arguably serve as a proxy for the local average economic situation (encoded as GROWTH). We interact both of these measures with EDUC and include them in our regressions to test whether the effect of bank failures would still hold.

Table 3.4 shows that the baseline results hold even when we interact the proxies of recessions and economic growth with EDUC.

3.6.3 Testing for Reverse Causality

In the baseline results, we document that bank failures of year $t-1$ lead to wage inequality in year t. The reason for lagging the bank failures variable by one year is to reduce the concerns regarding the reverse causality channel that either declining wages or more wage inequality lead to bank failures in the communities. Taking a one-year lag of the treatment variable ensures that there is time consistency in the direction of the effect observed, however, it cannot take care of the reverse causality concerns attributable to structural reasons.

In particular, during the recent financial crisis, it is well documented that former subprime borrowing on mortgage credit against home equity led to a deterioration of household wealth and subsequent mortgage defaults with the sharp decline in house prices (Mian and Sufi (2011)). It has also been documented that counties that expe-

Table 3.4: Recessions versus bank failures

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)
FAILED	-0.0421***	-0.0421***		
	(0.0109)	(0.0106)		
EDUC	0.389***	0.393***	0.391***	0.399***
	(0.0136)	(0.0111)	(0.0197)	(0.0151)
EDUC × FAILED	0.0389**	0.0407***	0.0368*	0.0420*
	(0.0122)	(0.0115)	(0.0179)	(0.0170)
REC	0.00629			
	(0.00115)			
EDUC × REC	0.00718		0.0223	
	(0.0123)		(0.0172)	
GROWTH		0.0202		
		(0.0647)		
EDUC × GROWTH		0.0292		0.128
		(0.0726)		(0.104)
Controls	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes
Puma-Ind-Age FE	Yes	Yes	No	No
Ind-Age-Year FE	Yes	Yes	No	No
Puma-Ind-Age-Year FE	No	No	Yes	Yes
N	654517	654517	571112	571112
R^2	0.819	0.819	0.885	0.885

Notes: The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC isa dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

rienced the hardest deterioration in household balance sheets (comprising mostly of housing equity) incurred the highest employment in non-tradable sectors through either suppression of consumer demand or decline in collateral value (Mian and Sufi (2014)). Translated into our setup, one can argue that relatively more suppressed unskilled wages lead to subprime defaults, which then lead to bank failures in communities that experienced sharper declines in house prices. Although PUMA-year fixed effects would control the elevated effect of subprime defaults at the community level, we still provide an additional test.

To this end, we take stock of the findings of Martin (2011) who argues that states that experienced the severest subprime foreclosures (California, Connecticut, Florida, Nevada and New Jersey) in 2008 were also the ones whose labor market conditions were affected most intensely during the recent financial crisis. Therefore, we argue that removing these states from our sample would, at least to a certain degree, tackle the reverse causality concerns. In Table 3.5, we present the results with and without these "core states". The first four columns of Table 3.5 show the results for core states (California, Connecticut, Florida, Nevada and New Jersey) and the last four columns show the results from a sample without these core states. It is indeed evident that across the most conservative specifications (columns (4) and (8)), the coefficient sizes are higher for core states. However, the baseline results presented in Table 3.3 still hold, suggesting that the impact of reverse causality is negligible here. As the classification of core states seems arbitrary, in Table 3.6, we also exclude the next five states (Illinois, Maryland, Michigan, Ohio and Rhode Island) that experienced high subprime foreclosures. The estimation findings indicate that the baseline results still hold even after this adjustment. Therefore, we argue that a potential reverse causality from suppressed unskilled wages leading to subprime defaults that ultimately result in bank failures has a negligible impact here. It is plausible to argue that the direction of effect goes from bank failures to wage inequality.

Table 3.5: A test for reverse causality: Results with and without the core states

	Core states				Without core states			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(Wage)	log(Wage)	log(Wage)	log(Wage)	log(Wage)	log(Wage)	log(Wage)	log(Wage)
FAILED	-0.00439		-0.123***		-0.0448***		-0.0334**	
	(0.00937)		(0.0319)		(0.00873)		(0.0122)	
EDUC	0.611***	0.611***	0.376***	0.396***	0.560***	0.559***	0.399***	0.405***
	(0.0197)	(0.0198)	(0.0269)	(0.0355)	(0.00891)	(0.00890)	(0.0122)	(0.0167)
EDUC × FAILED	0.0364***	0.0393***	0.0422*	0.0708*	0.0602***	0.0626***	0.0394**	0.0384*
	(0.0100)	(0.0105)	(0.0192)	(0.0335)	(0.0101)	(0.0103)	(0.0137)	(0.0175)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	Yes	No	No	No	Yes	No	No	No
Year FE	Yes	No	No	No	Yes	No	No	No
PUMA -Year FE	No	Yes	No	No	No	Yes	No	No
PUMA -Ind-Age FE	No	No	Yes	No	No	No	Yes	No
Ind-Age-Year FE	No	No	Yes	No	No	No	Yes	No
PUMA -Ind-Age-Year FE	No	No	No	Yes	No	No	No	Yes
N	187286	187286	132284	116774	724082	724082	519660	454338
R^2	0.346	0.347	0.827	0.884	0.382	0.383	0.821	0.886

Notes: The core states include California, Connecticut, Florida, Nevada and New Jersey. The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

Table 3.6: A test for reverse causality: Exclusion of additional core states

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)
FAILED	-0.0419***		-0.0332*	
	(0.00957)		(0.0131)	
EDUC	0.556***	0.556***	0.399***	0.409***
	(0.00995)	(0.00994)	(0.0138)	(0.0194)
EDUC × FAILED	0.0590***	0.0615***	0.0404**	0.0480*
	(0.0114)	(0.0117)	(0.0145)	(0.0219)
Controls	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes
Puma FE	No	No	No	No
Year FE	No	No	No	No
Puma-Year FE	No	No	No	No
Puma-Ind-Age FE	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No
Puma-Ind-Age-Year FE	No	Yes	No	Yes
N	587129	587129	421704	368818
R^2	0.380	0.381	0.819	0.883

Notes: Regressions are run excluding core states: California, Connecticut, Florida, Nevada, New Jersey, Illinois, Maryland, Michigan, Ohio and Rhode Island. The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC isa dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

3.6.4 Supply versus Demand

Although previous results confirm that bank failures are associated with higher wage inequality, they are inconclusive as to whether this relationship is associated with demand-side or supply-side explanations. That is, it is still unclear if the observed effect is driven by a negative credit shock making employees (firms) demand relatively less for unskilled workers or if unskilled individuals increase the supply of labor during times of financial distress, driving down their average wages. However, the Mincerian earnings equation above does not exclude the fact that the effect could be supply-driven. In what follows, we investigate whether demand- or supply-side explanations are more credible.

Binary Labor Supply Decision and Labor Market Attachment

In this section, we investigate the binary labor supply decision of individuals, i.e. the decision of workers to supply labor or not. This binary decision is congruent with being in or out of the labor force. The results in Table 3.3 indicate that bank failures are associated with an increase in the wage gap between skilled and unskilled workers. One could presume that bank failures could affect the skill premium if it changed the relative supplies of skilled and unskilled labor. In particular, if the labor force participation of unskilled workers increased relative to the labor force participation of skilled workers after bank failures, the increase in the relative supply of unskilled workers could drive down their wages relative to the wages of skilled workers.

In Table 3.7, we study the impact of bank failures on labor force participation for skilled versus unskilled workers. Namely, we run the previous set of regressions with a dependent variable of labor force participation status, which is a binary variable taking one (1) for individuals who are in the labor force and zero (0) otherwise. This is equivalent to testing if bank failures had an impact on the labor force participation decision of a given individual in a linear probability framework. The insignificance of the coefficients of $FAILED$ as well as $EDUC * FAILED$ indicates bank failures neither

Table 3.7: The effects of bank failures on labor force participation

	(1) Labor Force	(2) Labor Force	(3) Labor Force	(4) Labor Force
FAILED	0.000886		0.000449	
	(0.00334)		(0.00331)	
EDUC	0.0440***	0.0493***	0.0464***	0.0518***
	(0.00332)	(0.00473)	(0.00329)	(0.00471)
EDUC × FAILED	-0.00383	-0.00253	-0.00330	-0.00191
	(0.00354)	(0.00490)	(0.00351)	(0.00489)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	No	Yes	Yes
PUMA FE	No	No	No	No
Year FE	No	No	No	No
PUMA-Year FE	No	No	No	No
PUMA-Ind-Age FE	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No
PUMA-Ind-Age-Year	No	Yes	No	Yes
Observations	802483	698436	802483	698436
R^2	0.695	0.803	0.697	0.804

Notes: The sample period is 2008-2010. The dependent variable is Labor Force, which is a dummy variable taking the value of one (1) for individuals in the labor force and zero (0) otherwise. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 3.8: The effects of bank failures on labor market attachment

	(1) Available	(2) Available	(3) Searching	(4) Searching
FAILED	-0.00179		0.00808**	
	(0.00550)		(0.00291)	
EDUC	-0.0368***	-0.0300***	-0.0122***	-0.00974**
	(0.00544)	(0.00737)	(0.00266)	(0.00341)
EDUC × FAILED	0.000472	0.00590	-0.00596*	-0.00358
	(0.00548)	(0.00783)	(0.00302)	(0.00429)
Controls	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes
PUMA FE	No	No	No	No
Year FE	No	No	No	No
PUMA-Year FE	No	No	No	No
PUMA-Ind-Age FE	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No
PUMA-Ind-Age-Year	No	Yes	No	Yes
N	802483	698436	802483	698436
R^2	0.648	0.772	0.640	0.760

Notes: The sample period is 2008-2010. Available is an indicator variable with a value of one (1) if an individual is available for taking a job and zero (0) otherwise. Searching is an indicator variable taking a value of one (1) if an individual is currently looking for work. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

change local labor force participation, nor do they increase the probability of unskilled workers supplying labor in the sense of deciding on participating in the labor market than skilled workers.[13]

To better understand the labor supply decision and to test whether bank failures led to any particular change in the labor market attachment of workers, we also run other linear probability models with dependent binary variables of "available for work" and "searching job". We are after testing whether bank failures led to differential reservation wages for skilled and unskilled individuals, affecting subsequent wages for skilled versus unskilled workers in the labor market. We present the findings arising from these regressions in Table 3.8.

Table 3.8 shows that there is no evidence of bank failures resulting in differential reservation wages which might have made certain types of worker more attached to the labor markets, ultimately driving down the wages for those types of worker. These results strongly indicate that the extensive margin of the labor supply of skilled vs. unskilled workers was not affected by bank failures.

Hours of Work

Next, we focus on the labor hours to test the intensive margin of the labor supply. In particular, we study whether there is a differential change in annual hours worked by skilled versus unskilled workers. To this end, we run the following regressions,

[13]Though not presented here, we also run the analogous logit regressions and confirm these findings.

which are analogous to previous baseline equations:

$$ln(h)_{ipt} = \alpha_{pt} + \gamma_1 EDUC_{ipt} + \gamma_2 FAILED_{pt-1} + \gamma_3 EDUC_{ipt} * FAILED_{pt-1} + \delta X_{ipt} + \epsilon_{ipt} \quad (3.5)$$

$$ln(h)_{ispkt} = \alpha_{spk} + \alpha_{pkt} + \gamma_1 EDUC_{ispkt} + \gamma_2 FAILED_{pt-1} + \gamma_3 EDUC_{ispkt} * FAILED_{pt-1} + \delta X_{isptk} + \epsilon_{ispkt} \quad (3.6)$$

$$ln(h)_{ispkt} = \alpha_{spkt} + \gamma_1 EDUC_{ispkt} + \gamma_3 EDUC_{ispkt} * FAILED_{pt-1} + \delta X_{ispkt} + \epsilon_{ispkt} \quad (3.7)$$

which is the analog of the wage equations presented formerly. In a reduced-form model, in which the wage equation is a "price" equation and the hours equation is a "quantity" equation, one can investigate whether there is a significant supply or demand effect. For this purpose, we expect both β_3 (from equations 3.1-3.4) and γ_3 to be negative. Observing that both of these coefficients are negative and significant, one can conclude that there is a significant demand-driven effect (Popov and Rocholl (2016)). The idea behind this is that in a simple supply-demand framework, a simultaneous decline in the price and the quantity can only be attributed to a downward shift in the demand. Although, this reduced-form model do not exclude the fact that there could still be a supply effect, showing negative and significant estimates for the coefficients of interest would at least ensure that the demand-side explanations are credible. Table 3.9 columns (1) to (4) show the baseline results, while columns (5) to (8) indicate the results with additional controls (being on par with Table 3.3, respectively.) Table 3.9 indicates that not just the skill premium but also the differences in annual working hours between skilled and unskilled workers are bigger after bank failures. Since both the estimated β_3 and γ_3 are positive and significant in all specifications in Table 3.3 and 3.9, we conclude that there is a credible demand-driven increase in skill premium after bank failures.

To better grasp the adjustment mechanism of labor demand, we consider if the

Table 3.9: The effects of bank failures on work hours

	(1) log(Hours)	(2) log(Hours)	(3) log(Hours)	(4) log(Hours)	(5) log(Hours)	(6) log(Hours)	(7) log(Hours)	(8) log(Hours)
FAILED	-0.0143**		-0.0201**		-0.0179***		-0.0198**	
	(0.00578)		(0.00864)		(0.00529)		(0.00865)	
EDUC	0.360***	0.360***	0.128***	0.130***	0.236***	0.236***	0.128***	0.129***
	(0.00845)	(0.00849)	(0.00793)	(0.0117)	(0.00548)	(0.00551)	(0.00798)	(0.0118)
EDUC × FAILED	0.0147**	0.0146**	0.0143*	0.0265**	0.0196***	0.0193***	0.0139*	0.0263**
	(0.00688)	(0.00708)	(0.00834)	(0.0121)	(0.00516)	(0.00534)	(0.00833)	(0.0122)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	No	No	No	Yes	Yes	Yes	Yes
PUMA FE	Yes	No	No	No	Yes	No	No	No
Year FE	Yes	No	No	No	Yes	No	No	No
PUMA -Year FE	No	Yes	No	No	No	Yes	No	No
PUMA -Ind-Age FE	No	No	Yes	No	No	No	Yes	No
Ind-Age-Year FE	No	No	Yes	No	No	No	Yes	No
PUMA -Ind-Age-Year FE	No	No	No	Yes	No	No	No	Yes
Observations	911368	911368	654517	571112	911368	911368	654517	571112
R^2	0.168	0.170	0.818	0.885	0.375	0.376	0.819	0.885

Notes: The sample period is 2008-2010. The dependent variable is the natural logarithm of annual work hours. Columns (1) and (5), (2) and (6), (3) and (7), (4) and (8) show the estimation results of equations (3.7), (3.8), (3.9) and (3.10) respectively. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively.

Table 3.10: Labor adjustment due to bank failures

	(1) Employed	(2) Employed	(3) Layoff	(4) Layoff	(5) Fulltime	(6) Fulltime
FAILED	-0.00855*		-0.000812		-0.0122*	
	(0.00387)		(0.00181)		(0.00480)	
EDUC	0.0522***	0.0598***	-0.00653***	-0.00677**	0.118***	0.124***
	(0.00381)	(0.00516)	(0.00177)	(0.00244)	(0.00487)	(0.00676)
EDUC × FAILED	0.00346	0.00491	0.00143	0.00278	0.00888*	0.0141*
	(0.00386)	(0.00555)	(0.00192)	(0.00280)	(0.00491)	(0.00743)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	No	No	No	No	No	No
Year FE	No	No	No	No	No	No
PUMA-Year FE	No	No	No	No	No	No
PUMA-Ind-Age FE	Yes	No	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No	Yes	No
PUMA-Ind-Age-Year	No	Yes	No	Yes	No	Yes
N	802483	698436	802483	698436	703841	613923
R^2	0.709	0.811	0.561	0.701	0.749	0.841

Notes: The sample period is 2008-2010. The dependent variables in all columns are indicator variables. Employed takes the value of one (1) if an individual is employed and zero (0) otherwise. Layoff takes the value of one (1) if an individual is on layoff and zero (0) otherwise. Full-time takes the value of one (1) if an individual works full-time and zero (0) otherwise. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

bank failures were associated with differential employment, the probability of being laid-off and being employed full- versus part-time for skilled and unskilled individuals. Table 3.10 presents the results of linear probability models of the following binary dependent variables: Being employed, being on layoff and working full-time.

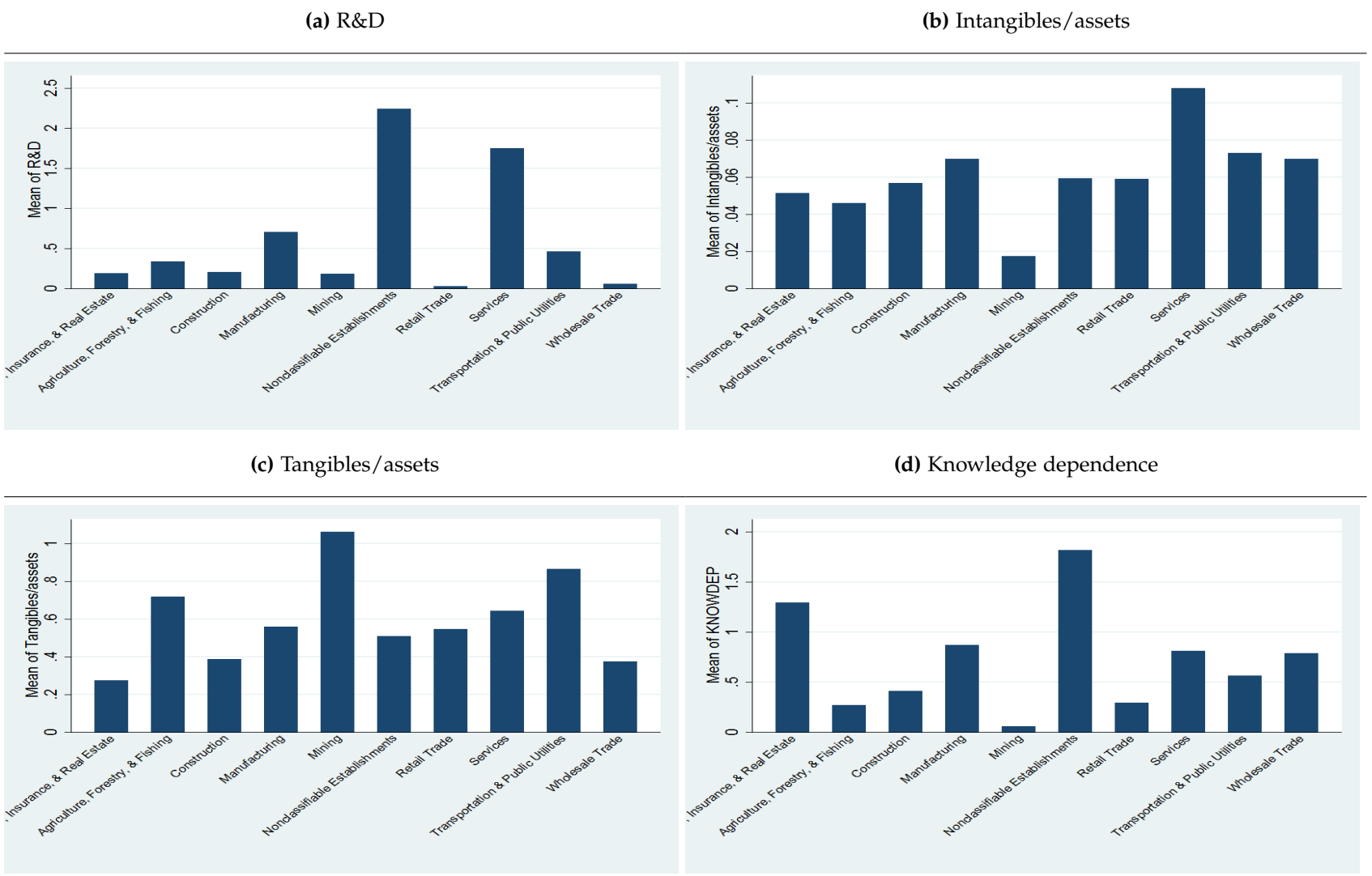
Table 3.10 indicates that the probability of being employed or being laid-off does not change differentially for skilled versus unskilled workers after bank failures. What changes differentially for skilled and unskilled workers, however, is the probability of being employed full-time. Columns (5) and (6) indicate that there is a differential decline in the probability of being employed full-time for unskilled workers. This finding is consistent with a differential decline in labor hours for unskilled workers and suggests that labor costs are adjusted through employing unskilled labor part-time, leading to a decline in annual wage earnings of unskilled workers.[14]

3.6.5 More on the Demand Channel: Knowledge Intensity

The previous sections show robust evidence of a demand-driven increase in the skill premium. To further substantiate this finding, we exploit the variation in knowledge intensity across sectors. The underlying idea is that sectors which are, for technological reasons, more dependent on knowledge-intensive capital are expected to have relatively more inelastic labor demand for skilled labor and more elastic demand for unskilled labor (Claessens and Ueda (2008)). Assuming that knowledge-intensive capital and skilled labor are somewhat complementary and knowledge-intensive firms have an elastic demand for unskilled labor, it can be expected that firms that depend more on knowledge-intensive capital will have a lower demand for unskilled labor during times of financial distress. Therefore, the effect of negative credit shocks on the wage gap between skilled and unskilled workers is expected to be higher in more knowledge-intensive sectors.

[14]This is in line with the discussion on the rigidity of downward adjustment in hourly wages and, hence, employers cutting labor hours to adjust labor costs during episodes of financial distress.

Figure 3.6: Industry characteristics by high level ISIC classification



Notes: Data is obtained from Compustat for the period 1987-2005.

Following Claessens and Ueda (2008), we calculate knowledge intensity as the ratio of R&D expenditures to sales, which we encode as $R\&D$ in the tables. To ensure that the knowledge intensity measure is not itself affected by bank failures, we take the average of this ratio for 1987-2005 and run the analysis with a time-invariant sector-specific knowledge intensity for two-digit Standard Industry Classification (SIC) classification. Although we use 2-digit classification in our analysis, we nevertheless present mean R&D ratio at 1-digit level sector classification in panel (a) of Figure 3.6. As expected, service, transportation and non-classifiable sectors have higher $R\&D$ ratios than sectors such as mining and agriculture.

To test if the effect of bank failures on the skill premium is differentially higher in relatively more knowledge-intensive sectors, we interact our variable of interest, $EDUC * FAILED$ with $R\&D$. That is, this triple interaction term captures the effect of bank failures on wage premium depending on knowledge intensity. The estimation results of this triple interaction model are shown in Table 3.11.

The positive and significant estimated coefficient of this triple interaction term indicates that there is a differentially higher skill premium across sectors, which increases as knowledge intensity increases. Regarding coefficient interpretations of the triple interaction terms, ceteris paribus, one standard deviation increase from the mean in R&D (an increase by 1.97) increases skill premium, on average, by around 2.8%, 5.2%, 2.8% and 5.3% based on estimates in columns 1,2,3 and 4 of Table 3.11, respectively.

Table 3.11: Knowledge intensity

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)
FAILED	-0.0616***		-0.0610***	
	(0.0154)		(0.0155)	
EDUC	0.464***	0.472***	0.461***	0.468***
	(0.0148)	(0.0203)	(0.0150)	(0.0205)
EDUC × FAILED	-0.0246	-0.0611**	-0.0253	-0.0632**
	(0.0176)	(0.0265)	(0.0175)	(0.0265)
EDUC × FAILED × R&D	0.0141***	0.0264***	0.0141***	0.0267***
	(0.00188)	(0.00383)	(0.00188)	(0.00382)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	No	Yes	Yes
PUMA FE	No	No	No	No
Year FE	No	No	No	No
PUMA-Year FE	No	No	No	No
PUMA-Ind-Age FE	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No
PUMA-Ind-Age-Year	No	Yes	No	Yes
Observations	431235	379054	431235	379054
R^2	0.821	0.886	0.821	0.886

Notes: The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. R&D is sector-level time-invariant variable. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

3.7 Transmission to the Labor Market

The results presented so far confirm Hypothesis 1 that bank failures lead to a reduction in labor demand. In this section, we try to understand the channel through which the effects of bank failures are transmitted to the labor market, ultimately widening the wage gap between skilled and unskilled workers. In doing so, we turn our attention to testing Hypothesis 2.

As discussed previously, the type of capital employed by the sectors of the economy can play an essential role in transmitting the credit market shocks to labor markets. In episodes of bank failures, sectors that rely relatively more on intangible capital as opposed to tangible capital would be more affected by credit contraction (Almeida and Campello (2008)). These are also the firms that demand more skilled labor as opposed to unskilled labor as we plausibly assume that intangible capital and skilled labor are complementary inputs (Hall (2000), (2001)). In a way, the affected firms, which cannot pledge enough collateral, are the ones which are likely to demand less unskilled labor during episodes of bank failure. In other words, when bank failures hit the local economy and lead firms that cannot substitute to other forms of financing to decrease input costs, the most affected sectors are the ones that rely more on skilled labor for sustaining production and that can give up employing unskilled labor relatively easily.

As in the previous section, we focus on time-invariant sector-variant measures of capital tangibility and intangibility to test this hypothesis. Following Claessens and Ueda (2008), we calculate the ratio of intangibles to tangibles as the ratio of intangible assets as given in Compustat to net property plant equipment (or tangible assets). As before, to ensure that the knowledge dependence measure is not itself affected by bank failures, we take the average of this ratio for 1987-2005 and conduct the analysis with a time-invariant sector-specific knowledge intensity for two-digit Standard Industry Classification (SIC) classification. We encode this variable as $KNOWDEP$ throughout the analysis. Although we use 2-digit classification in our study, we nev-

ertheless present the mean of this ratio for the 1-digit level sector classification in panel (d) of Figure 3.6. Panels (b) and (c) also present the means of the ratios of intangible assets to total assets and net property plant equipment to total assets.

To test whether the effects of bank failures on the skill premium will be differentially exacerbated in sectors that rely more on intangible capital vis-à-vis tangible capital, we interact our variable of interest, $EDUC * FAILED$ with $KNOWDEP$. That is, this triple interaction term captures the effect of bank failures on wage premium depending on knowledge intensity. The estimation results of this triple interaction model are presented in Table 3.12.

The positive and significant estimated coefficient of this triple interaction term in all columns of Table 3.12 verifies Hypothesis 2: The effects of bank failures on the skill premium depend on the use of intangible capital relative to tangible capital in a sector. Note that our most reliable estimates are presented in columns 3 and 4 where we include industry-year fixed effects, which also control for sector-level external dependence differentials across industries constant as required by Hypothesis 2.

Regarding coefficient interpretations of the triple interaction terms, ceteris paribus, one standard deviation increase in knowledge dependence (an increase by 0.705) increases skill premium, on average, by around 4.1%, 8.0%, 4.1% and 8.1% based on estimates in columns 1,2,3 and 4 of Table 3.12, respectively.

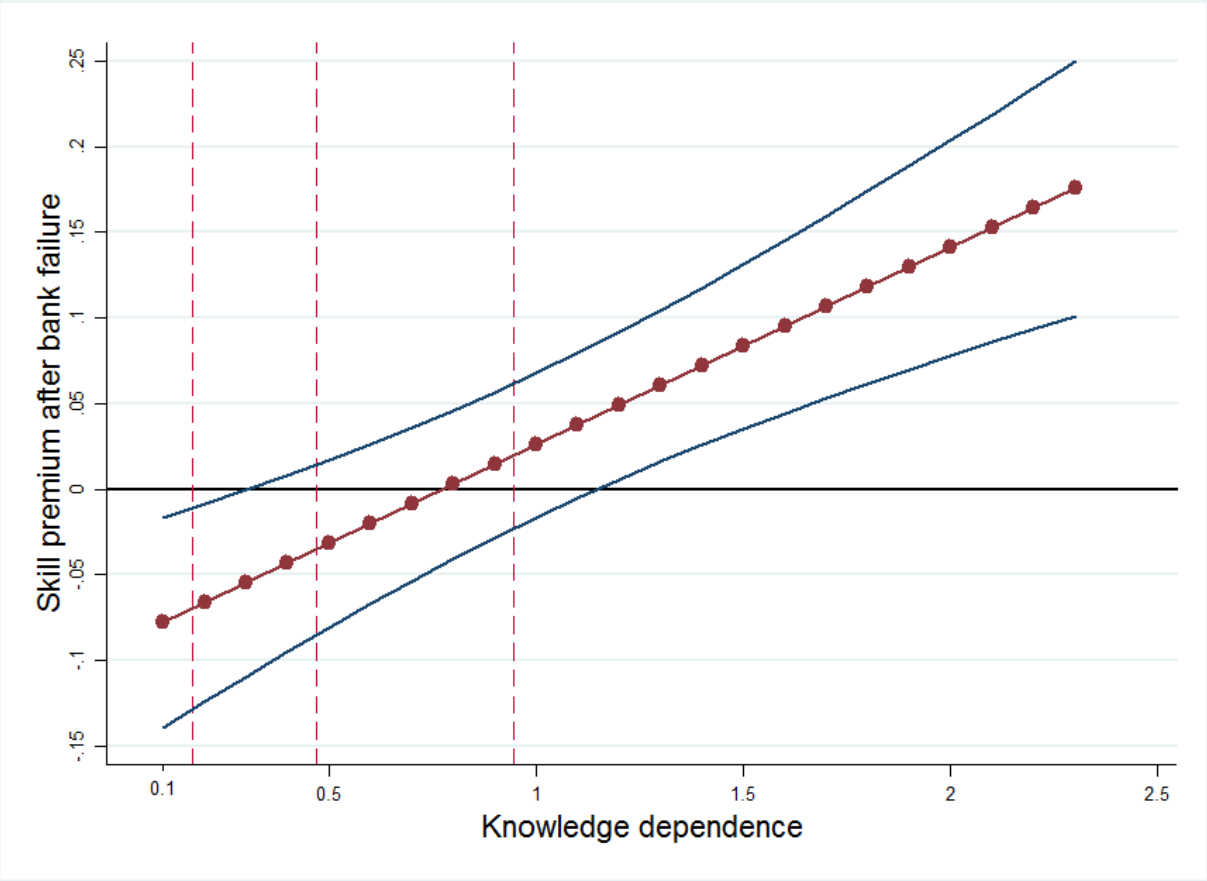
To further unveil the heterogeneity across sectors, we plot the marginal effect of bank failures on the skill premium, depending on $KNOWDEP$ in Figure 3.7. 95% confidence bands from PUMA-level clustering of standard errors are shown with blue. The dashed vertical lines indicated with red show 25, 50 and 75 percentiles of knowledge dependence. The figure highlights how the relative use of intangible to tangible assets may create heterogeneity in transmitting credit market shocks to the labor market. At the very low levels of intangibility/tangibility (i.e., unaffected, less skilled labor-dependent) (25^{th} percentile of $KNOWDEP$), the skill premium even declines after bank failures. Only at the high levels of intangibility/tangibility (i.e., affected, more skilled labor-dependent) (75^{th} percentile of $KNOWDEP$), we observe

Table 3.12: Sectoral heterogeneity: Knowledge dependence

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)
FAILED	-0.0460***		-0.0452***	
	(0.0142)		(0.0143)	
EDUC	0.469***	0.476***	0.466***	0.472***
	(0.0134)	(0.0185)	(0.0135)	(0.0186)
EDUC × FAILED	-0.0276	-0.0865***	-0.0287	-0.0896***
	(0.0192)	(0.0330)	(0.0193)	(0.0329)
EDUC × FAILED × KNOWDEP	0.0578***	0.113***	0.0578***	0.115***
	(0.0121)	(0.0244)	(0.0121)	(0.0244)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	No	Yes	Yes
PUMA FE	No	No	No	No
Year FE	No	No	No	No
PUMA-Year FE	No	No	No	No
PUMA-Ind-Age FE	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No
PUMA-Ind-Age-Year	No	Yes	No	Yes
Observations	481413	420886	481413	420886
R^2	0.817	0.883	0.817	0.884

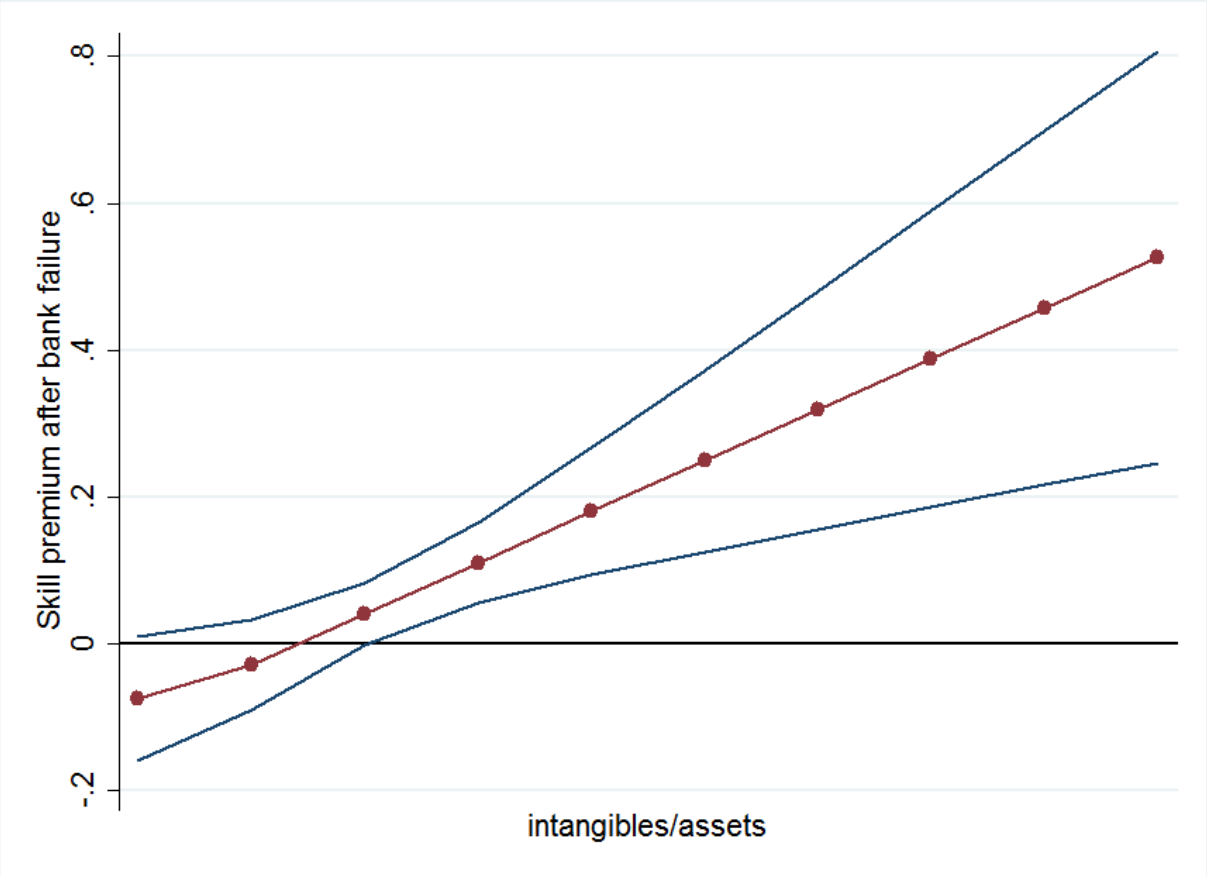
Notes: The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. KNOWDEP is a sector-level time-invariant variable. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

Figure 3.7: Marginal effects of sectoral knowledge dependence on the skill premium



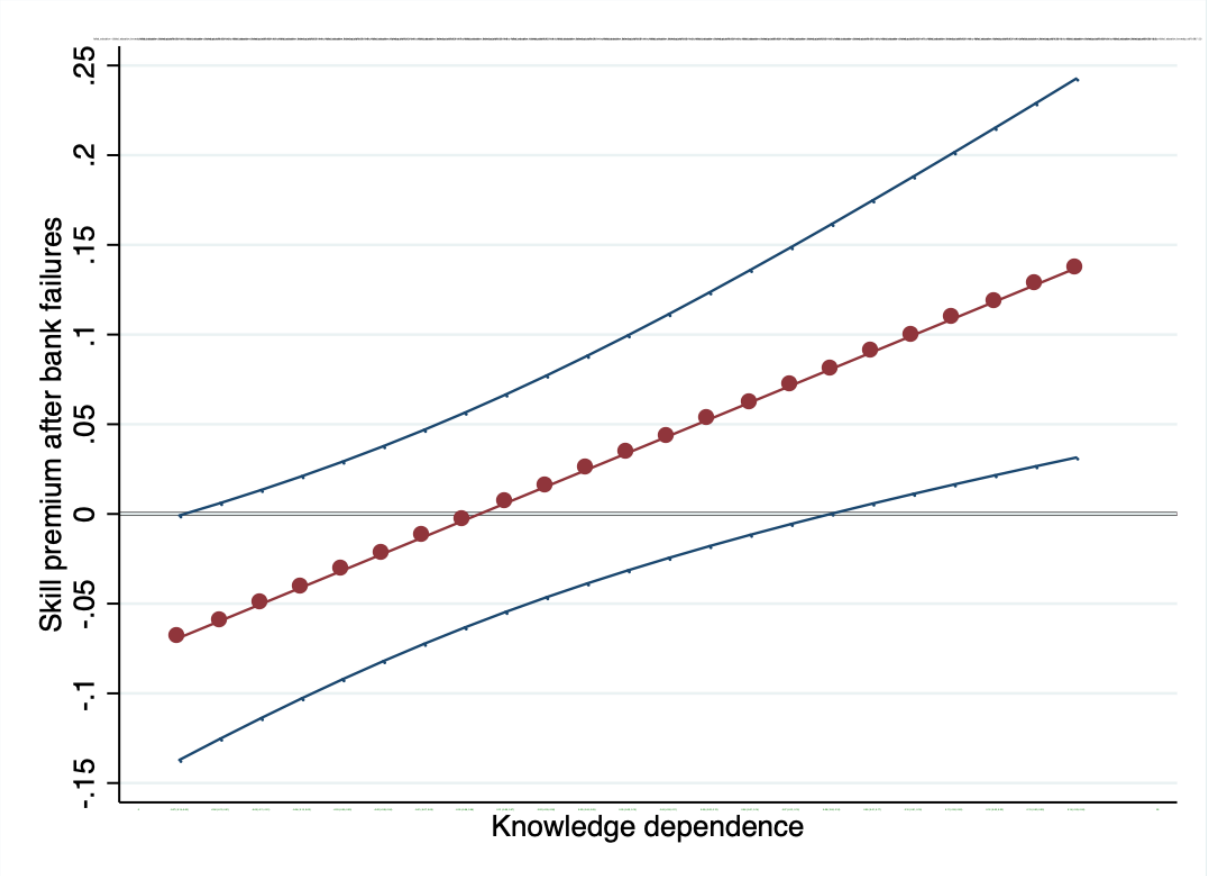
Notes: The figure refers to specification (4) of Table 6 and shows the marginal effects of sectoral knowledge dependence on bank-failure-induced skill premium. 95% confidence bands from PUMA-level clustering of standard errors are shown with blue. The dashed vertical lines indicated with red show 25, 50 and 75 percentiles of knowledge dependence.

Figure 3.8: Marginal effects of intangibles/assets on the skill premium in sectors more prone to credit shocks



Notes: The figure refers to a specification with quadruple interaction of $EDUC * FAILED$, *intangibles/assets* and *tangibles/assets* with PUMA-industry-age-year fixed effects. It shows the marginal effects of *intangibles/assets* on bank-failure-induced skill premium when moving from a sector at 75^{th} percentile of *tangibles/assets* (sectors unaffected by bank failures) to 25^{th} percentile of *tangibles/assets* (sectors affected by bank failures). 95% confidence bands from PUMA-level clustering of standard errors are shown with blue.

Figure 3.9: Marginal effects of intangibles/assets on the skill premium in sectors more prone to credit shocks



Notes: The figure refers to a specification with quadruple interaction of $EDUC * FAILED$, $KNOWDEP$ and external dependence with PUMA-industry-age-year fixed effects. It shows the marginal effects of $KNOWDEP$ on bank-failure-induced skill premium when moving from a sector at 25^{th} percentile of external dependence (sectors unaffected by bank failures) to 75^{th} percentile of external dependence (sectors affected by bank failures). 95% confidence bands from PUMA-level clustering of standard errors are shown with blue.

an increase in the skill premium after bank failures.

Although at the high and low levels of intangibility/tangibility, the interpretation of knowledge dependence is clear, one is unable to interpret the cases of what would happen if both intangibility and tangibility were high or low. To provide a robustness check that in principle also capture these effects we would need to present an empirical model that includes the interaction of $EDUC * FAILED$, $intangibles/assets$, $tangibles/assets$, in which we end up with a quadruple interaction term, which makes coefficient interpretations highly complicated. Nevertheless, we still run the following empirical model with quadruple interaction with PUMA-industry-age-year fixed effects. Figure 3.8 shows the plot of the marginal effect of $intangibles/assets$, when we move from a sector at 75^{th} percentile of $tangibles/assets$ (sectors unaffected by bank failures) to 25^{th} percentile of $tangibles/assets$ (sectors affected by bank failures). The plot indicates that when sectors are more prone to credit shocks, bank failures widen the wage gap more in the sectors whose share of intangible capital is higher as opposed to sectors with lower shares of intangible capital. This confirms the finding presented in Figure 3.7.

Finally, we consider external dependence. So far, we have controlled the effect of external finance via our fixed effects. The reason for this was that sectors could differ in their dependence on external sources. Some sectors may not get affected by bank failures if their external dependence (e.g. dependence to bank credit) is low even if they have high intangibility. Therefore, by controlling for external finance, we have "corrected" for potentially confounding differences across sectors' dependence on external financial resources. However, we also present results with a quadruple interaction of $EDUC * FAILED$, $KNOWDEP$ and external dependence, as defined by Rajan and Zingales (1998). External dependence is a time-invariant sector-variant measure defined as the difference between capital expenditures and cash flows divided by capital expenditures. It captures the amount of desired finance that cannot be generated by cash flows internally. We calculate external finance from Compustat for 1987-2005 for all the sectors included previously.

The analog graph is shown in Figure 3.9. It presents the plot of the marginal effect of *KNOWDEP* when we move from a sector at 25^{th} percentile of external dependence (i.e. a sector relatively unaffected by bank failures) to 75^{th} percentile of external dependence (sectors affected by bank failures). The plot indicates that when sectors are affected by bank failures more, we observe a higher skill premium in the sectors which are more knowledge dependent. In fact, for sectors with low levels of knowledge dependence, skill premium after bank failures is insignificant and only for the sectors that heavily on knowledge dependent capital, skill premium increases significantly.

3.8 Additional Robustness Tests

3.8.1 Heckmann Correction

So far, our wage regressions have been based on a sample of individuals for whom we can observe wages. If the person is working at the time of the survey, then the wage we observe would be her labor market wage. However, for people out of the workforce, we cannot observe a wage. To make sure that an unobserved factor related to "propensity to work" does not confound our findings on the skill premium, we run a two-step Heckmann selection model. In the first stage, we consider our previous covariates plus a variable that can affect the propensity to work but might not necessarily directly affect labor market earnings. To this end, we consider the presence of elderly (+65 years) people in the household, which is a factor that may affect labor market participation of individuals due to caring responsibilities, however, should not necessarily affect the wages of those who already participate in the labor market. [15] The first and second stage estimations for our baseline wage regression and the regression with *KNOWDEP* as an interaction term are provided in Table 3.13.

In the first stage regressions, the significance of the coefficients for the variable

[15]Note that marital status is a common factor included in the first stage of Heckmann correction models for wage regressions. As we already control for marriage status in our baseline regressions, we proceed with the presence of the elderly in the household. See Polachek (2008) for details.

Table 3.13: Heckmann correction for selection bias

	Baseline		Knowledge dependence	
	(1)	(2)	(3)	(4)
	First stage	Second stage	First stage	Second stage
EDUC	0.12010***	0.320754***	0.104441***	0.369497***
	(0.016706)	(0.010746)	(0.019708)	(0.013290)
EDUC×FAILED	-0.039588**	0.033296**	-0.005299	-0.099415***
	(0.020610)	(0.01411)	(0.039268)	(0.027227)
EDUC×FAILED×KNOWDEP			-0.028431	0.114876***
			(0.026869)	(0.021207)
Senior	-0.262436***		-0.211127***	
	(0.0176911)		(0.020123)	
Controls	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes
PUMA-Ind-Age-Year FE	Yes	Yes	Yes	Yes
Inverse Mills Ratio (λ)	-0.18322***		-0.188093***	
	(0.004051)		(0.004267)	
N	1105094	1105094	911368	911368
Consored N	193726	193726	138838	138838

Notes: The sample period is 2008-2010. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. Level of education is a categorical variable based on years of schooling. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. KNOWDEP is a sector-level time-invariant variables. Each column includes multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Married, Married*Child, Female*Child. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

capturing the presence of elderly in the household, encoded as *Senior*, shows the relevance of the variable. As inverse mills ratio's are significant, we conclude that there is indeed a selection bias based on propensity to work in our regressions. However, correcting for the selection (shown in the columns presenting the second stage results) suggest that the effect of bank failures on the skill premium is still significant (column (2)) and this effect increases as sectors' knowledge dependence increase (column (4)).

3.8.2 Bank Failures and Skill Composition

Under the Neyman-Rubin framework, one crucial assumption to establish a causal relationship is the stable unit treatment value assumption (SUTVA), where treatment assignment to one group does not affect the potential outcome for the other group (Rubin (1977)). Following the Neyman-Rubin framework, we would like to give more credibility to the magnitudes of our coefficients and test whether the results we presented so far suffer from not satisfying SUTVA.

In our setting, if bank failures induce a change in skill composition and distribution in communities with no experience of bank failures, we may violate SUTVA. One can possibly think of a scenario of mobility of skilled workers towards unaffected PUMAs, driving down skilled wages and, hence, decreasing wage disparity relative to affected PUMAs. Although PUMA-year fixed effects fix all factors changing in community and year dimensions within a community (intra-community), it cannot take into account the inter-community effects. Similarly, there could be switching of skills between different sectors and occupation types.

One test of showing that SUTVA is met in several dimensions of our data (PUMA, sector and occupation types), looks at whether bank failures change the skill composition and distribution. In particular, we concentrate on specifications with within estimation in which skill composition and distribution are regressed on bank failures. Finding significant asymmetries in skill composition and distribution within PUMAs, sectors and occupation types after bank failures would lead us to conclude that our

Table 3.14: Skill composition and distribution within PUMAs and sectors

	Within PUMA		Within sector					
			Skill composition			Skill distribution		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	EDUC	Level of education	EDUC	EDUC	EDUC	Level of education	Level of education	Level of education
FAILED	-0.000469	-0.00125	-0.000515	0.000175	0.0000308	-0.00000771	-0.00165	-0.00421
	(0.000920)	(0.00294)	(0.000991)	(0.00107)	(0.00181)	(0.00317)	(0.00328)	(0.00520)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	Yes	Yes	Yes	No	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	No	Yes	Yes	No	No
Ind FE	No	No	Yes	No	No	Yes	No	No
PUMA-Ind FE	No	No	No	Yes	No	No	Yes	No
Ind-Year FE	No	No	No	Yes	No	No	Yes	No
PUMA-Ind-Age FE	No	No	No	No	Yes	No	No	Yes
Ind-Age-Year FE	No	No	No	Yes	Yes	No	No	Yes
N	1451995	1451995	1105094	1093746	802483	1105094	1093746	802483
R^2	0.841	0.641	0.828	0.846	0.937	0.653	0.692	0.877

Notes: The sample period is 2008-2010. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. Level of education is categorical variable based on years of schooling. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. KNOWDEP and R&D are sector-level time-invariant variables. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

results are contaminated by not satisfying SUTVA. Table 3.14 presents the estimation results for within PUMA and within sector regressions. In Table 3.14 the first two columns refer to within PUMA estimates and the remaining six columns refer to within sector fixed effects. *EDUC* is, as defined before, to be skilled or not (corresponding to skill composition) and "Level of education" is finer ordered categories of schooling such as elementary, high school, college and so on (corresponding to skill distribution). The estimation results clearly show that neither the skill composition nor the distribution changes within sectors and PUMAs. In other words, there is no significant switching of one specific type of worker from one PUMA or sector to the other. This suggests that our baseline results can unlikely be explained by not satisfying SUTVA across PUMA and sector dimensions.

We also present the same analysis in Table 3.15 for occupation types. The Amer-

Table 3.15: Skill composition and distribution within occupation types

	Skill composition			Skill distribution		
	(1)	(2)	(3)	(4)	(5)	(6)
	EDUC	EDUC	EDUC	Level of education	Level of education	Level of education
FAILED	-0.000666	-0.000317	-0.000160	-0.000738	-0.000221	-0.000757
	(0.000998)	(0.00110)	(0.00234)	(0.00284)	(0.00319)	(0.00589)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra controls	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	Yes	No	Yes	Yes	No	No
Year FE	Yes	No	Yes	Yes	No	No
Occp FE	Yes	No	No	Yes	No	No
PUMA-Occp FE	No	Yes	No	No	Yes	No
Occp-Year FE	No	Yes	No	No	Yes	No
PUMA-Occp-Age FE	No	No	Yes	No	No	Yes
Occp-Age-Year FE	No	Yes	Yes	No	No	Yes
N	1105094	1077346	729649	1105094	1077346	729649
R^2	0.833	0.858	0.957	0.718	0.766	0.937

Notes: The sample period is 2008-2010. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. Level of education is a categorical variable based on years of schooling. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. KNOWDEP and R&D are sector-level time-invariant variables. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

ican Community Survey provides information on occupation types consisting of 539 specific occupational categories arranged into 23 high-level occupational groups. This classification was created on the basis of the Standard Occupational Classification (SOC) Manual: 2010, published by the Executive Office of the President, Office of Management and Budget (OMB) (US Census Bureau (2017)). Table 3.15 displays the results of within occupation type regressions.

Table 3.15 reiterates the findings that there is no evidence for significant asymmetries in skill composition and occupation as a result of bank failures. Overall, Table 3.14 and Table 3.15 indicate that our baseline results can unlikely be explained by not satisfying SUTVA across PUMA, sector and occupation types.

3.8.3 Migration, Occupation Types, Stable Sample, Winsorizing data and Two-Way Standard Error Clustering

Another way of showing that our results are robust to individuals switching between communities is to study the role of migration between PUMAs. Section 5 argues that to detect a PUMA-level effect, it is not a requirement that the banking market for an individual branch is confined to a single PUMA, but only that a bank is most heavily engaged with the community in which it operates. In either case, the incidence of any potential negative credit shocks as a result of bank failure would fall most heavily on the area nearest the bank (though the disruption would be stronger in the former case, leading us to at least estimating a lower bound of the actual effect). Here the underlying assumption is that individuals are stable within a PUMA. However, migration between PUMAs could lead to overestimation of the results if, for an endogenous reason, highly skilled individuals systematically migrate to PUMAs that experience bank failure.

Although our data neither shows a systematic migration pattern nor does it show a mass migration to the extent that it could contaminate results, we still provide a robustness test against this concern. Our dataset allows us to track individuals' location

Table 3.16: Key results with a sample of non-movers

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)	(5) log(Wage)	(6) log(Wage)	(7) log(Wage)	(8) log(Wage)
FAILED	-0.0300**		-0.0291**		-0.0335**		-0.0329**	
	(0.0118)		(0.0118)		(0.0150)		(0.0151)	
EDUC	0.397***	0.409***	0.395***	0.405***	0.474***	0.485***	0.473***	0.483***
	(0.0115)	(0.0152)	(0.0116)	(0.0153)	(0.0140)	(0.0189)	(0.0141)	(0.0189)
EDUC × FAILED	0.0330***	0.0354*	0.0317**	0.0344*	-0.0509**	-0.115***	-0.0520**	-0.117***
	(0.0125)	(0.0181)	(0.0125)	(0.0181)	(0.0203)	(0.0337)	(0.0204)	(0.0337)
EDUC × FAILED × KNOWDEP					0.0639***	0.123***	0.0637***	0.123***
					(0.0127)	(0.0251)	(0.0127)	(0.0250)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	No	Yes	Yes	No	No	Yes	Yes
PUMA FE	No	No	No	No	No	No	No	No
Year FE	No	No	No	No	No	No	No	No
PUMA-Year FE	No	No	No	No	No	No	No	No
PUMA-Ind-Age FE	Yes	No	Yes	No	Yes	No	Yes	No
Ind-Age-Year FE	Yes	No	Yes	No	Yes	No	Yes	No
PUMA-Ind-Age-Year	No	Yes	No	Yes	No	Yes	No	Yes
Observations	573163	501266	573163	501266	421466	369545	421466	369545
R^2	0.831	0.894	0.831	0.894	0.829	0.893	0.829	0.893

Notes: The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

for the last 12 months. In particular, we run the regressions only with individuals who have been living in the same PUMA for at least 12 months. Results of these estimations are shown in Table 3.16. They confirm our results in the sense that even only with a sample of non-movers, we can show a statistically and economically significant effect of bank failures on the wage gap between skilled and unskilled workers as well as heterogeneity of the effect of bank failures on the skill premium depending on *KNOWDEP*.

In addition to migration, one more potential problem could be present. Namely, the link between skills and wages may not be strong in certain occupation types. That is, despite requiring little education certain job types may provide high earnings. To the extent that low skilled people with high earnings leave the labor market whereas low skilled people with low earnings stay, we may still observe an increase in the skill premium which is independent of bank failures. So far, we have used several multi-way fixed effects including a component for sector fixed effects, which can imperfectly control for the differences across occupation types to the extent that workers in the same sector are working in similar types of occupation. However, this is a rather unrealistic assumption. To control for differences in occupation types, we repeat our baseline regressions by replacing industry components of multi-way fixed effects with occupation types. Table 3.17 presents the baselines results provided with occupation types fixed effects. Controlling for all factors that may change across occupation types, time and communities, our results still suggest that bank failures lead to a widening wage gap between skilled and unskilled workers.

We also test the robustness of our results to changing sample sizes. So far, the number of observations has varied across different specifications significantly. There is a big drop in sample size when we make specifications more conservative each time by including multi-way fixed effects. In the most conservative specification in which we focus on the variation across wages within an age cohort, in a sector, in a PUMA at a given point in time, we sometimes lack sufficient observations to ensure the convergence of the estimates to population parameters. To ensure that different samples

Table 3.17: Key results with occupation fixed effects

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)
FAILED	-0.0339*		-0.0344*	
	(0.0140)		(0.0140)	
EDUC	0.146***	0.163***	0.145***	0.161***
	(0.0138)	(0.0206)	(0.0138)	(0.0205)
EDUC × FAILED	0.0347*	0.0593*	0.0348*	0.0592*
	(0.0142)	(0.0233)	(0.0142)	(0.0233)
Controls	Yes	Yes	Yes	Yes
Extra controls	No	No	Yes	Yes
Puma FE	No	No	No	No
Year FE	No	No	No	No
Puma-Year FE	No	No	No	No
Puma-Occp-Age FE	Yes	No	Yes	No
Occp-Age-Year FE	Yes	No	Yes	No
Puma-Occp-Age-Year FE	No	Yes	No	Yes
N	592444	523621	592444	523621
R^2	0.885	0.934	0.885	0.934

Notes: The sample period is 2008-2010. The dependent variable in all columns is the natural logarithm of annual wages. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. KNOWDEP and R&D are sector-level time-invariant variables. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

Table 3.18: Stable sample, including winsorizing data and two-way standard error clustering

	(1) log(Wage)	(2) log(Wage)	(3) log(Wage)	(4) log(Wage)	(5) log(Hours)	(6) log(Hours)	(7) log(Hours)	(8) log(Hours)
FAILED	-0.0734***		-0.0323**		-0.0232**		-0.00718	
	(0.0125)		(0.0159)		(0.00942)		(0.0133)	
EDUC	0.579***	0.578***	0.408***	0.403***	0.243***	0.243***	0.142***	0.137***
	(0.00975)	(0.00975)	(0.0132)	(0.0150)	(0.00674)	(0.00674)	(0.0105)	(0.0121)
EDUC × FAILED	0.0976***	0.0964***	0.0505***	0.0456***	0.0383***	0.0362***	0.0256**	0.0287**
	(0.0139)	(0.0144)	(0.0154)	(0.0172)	(0.00978)	(0.0101)	(0.0118)	(0.0124)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	Yes	No	No	No	Yes	No	No	No
Year FE	Yes	No	No	No	Yes	No	No	No
PUMA -Year FE	No	Yes	No	No	No	Yes	No	No
PUMA -Ind-Age FE	No	No	Yes	No	No	No	Yes	No
Ind-Age-Year FE	No	No	Yes	No	No	No	Yes	No
PUMA -Ind-Age-Year FE	No	No	No	Yes	No	No	No	Yes
Observations	571112	571112	571112	571112	571112	571112	571112	571112
R^2	0.397	0.399	0.861	0.885	0.256	0.258	0.815	0.847
[Winsorizing at 1%]								
EDUC × FAILED	0.0967***	0.0958***	0.0512***	0.0463***	0.0379***	0.0360***	0.0248**	0.0288**
	(0.0133)	(0.0138)	(0.0150)	(0.0167)	(0.00942)	(0.00971)	(0.0113)	(0.0121)
[Two-way standard error clustering: PUMA and sector]								
EDUC × FAILED	(0.0184)	(0.0184)	(0.0089)	(0.0118)	(0.00997)	(0.0105)	(0.0071)	(0.0120)

Notes: The sample period is 2008-2010. The dependent variable in columns (1) to (4) is the natural logarithm of annual wages, whereas the dependent variable in columns (5) to (8) is the natural logarithm of annual working hours. Columns (1) and (3), (2) and (4), (5) and (7), (6) and (8) show the estimation results of equations (3.3), (3.4), (9) and (10), respectively. EDUC is a dummy variable taking the value of one (1) for individuals with an undergraduate degree or above and zero (0) otherwise. FAILED is a dummy variable taking the value of one (1) for individuals residing in PUMAs that experience at least one bank failure during the sample period and zero (0) otherwise. Each column includes one- or multi-way fixed effects. Controls include the following variables: Experience, Experience^2 Foreign born, Race, Female, EDUC*Experience and EDUC*Experience^2. Extra controls include the following variables: Age, Age^2, Married, Married*Child, Female*Child. Whenever Age fixed effects are included, Age and Age^2 are dropped from the regressions. Standard errors are shown in parentheses and are clustered by PUMAs. *, **, *** indicate significance at the 10%, 5% and 1% levels.

used throughout the tables do not drive results, we conduct the entire analysis with a stable sample. The key results with a stable sample are presented in Table 3.18. They verify the findings demonstrated so far that the observed increases in the skill premium after bank failures are demand-driven and they are not driven by differential samples and sample sizes.

In addition, we consider winsorizing our data. Given that extremely low values of yearly wages and working hours are rather implausible but present in our dataset, Table 3.18 also repeats the results with a data where dependent variables are winsorized at 1% level. It shows that the magnitude of the coefficients and the significance levels do not change much after winsorizing.

Finally, the results we have presented so far are based on standard error clustering at the PUMA level. However, this may not be sufficient if wage and hours of work are correlated beyond PUMAs, e.g. within sectors. To debunk this concern, we repeat our baseline analysis with two-way standard error clustering at the PUMA and sector level. Table 3.18 presents the standard errors clustered at both PUMA and sector levels. The findings suggest that our previous results' statistical significance of previous results remain valid.

3.9 Conclusion

Bank failures affect both economic growth and income inequality. While economists have thoroughly studied the effects of bank failures on economic growth, the potentially enormous impact of such an event on inequality has been underappreciated. The three volumes of the Handbook of Income Distribution, for example, do not mention any possible connections between wage inequality and bank failures.

In this chapter, we provide robust evidence that bank failures increase the skill premium in a big sample of Americans from US PUMAs for the 2007-2009 recession, which is associated with a large number of bank failures.

We conduct a two-fold empirical methodology. First, we identify the location of failed bank branches in each PUMA and classifying PUMAs with at least one failed bank branch as affected areas. We then estimate a Mincerian skill-wage equation to test if the wage gap between skilled and unskilled workers are higher in affected PUMAs. We estimate various fixed effects specifications, in many of which we control for time-varying PUMA-specific characteristics to control for all static and dynamic regional economic conditions.

We find that bank failures lead to an increase in the wage gap between skilled and unskilled workers by around 5% and the observed effect can credibly be attributed to demand-side explanations. We also show that the impact of bank failures on wage inequality is differentially exacerbated for sectors that are knowledge-intensive, which further substantiates the observed demand effects.

We also show, for the first time in the literature, that the type of capital employed in sectors of an economy is an essential channel through which the effects of bank failures are transmitted to local labor markets. In particular, we show that the impact of bank failures on the skill premium is differentially exacerbated for sectors that rely more on intangible capital relative to tangible capital. We attribute this finding to the fact that the type of capital firms use matters for the transmission of local credit shocks to the labor market. These results are consistent with knowledge-dependent capital

being financed through forgone earnings of unskilled workers when total financing capacity shrinks.

The findings of this study open up a new channel, bank failures, as an accelerator of the skill premium. It also explains one of the potential mechanisms that outlines how shocks in the financial markets can be transmitted to the labor markets. To our knowledge, we are the first to show that knowledge-dependent capital matters for the transmission of such shocks. The sectoral heterogeneity we exploit can also easily be translated into other setups where the interaction of financial and labor markets is analyzed.

Our findings can be extended in several directions. First, it would be interesting to conduct this analysis with a richer dataset in which the researcher can observe the matched bank-firm and employee to be able to gain a deeper understanding of the relationship between the firm and bank characteristics and the evolution of wages under financial distress. Given the importance of lending to small and medium-sized enterprises, one could test whether the observed effects of bank failures on the skill premium are different for small and large businesses. Secondly, a theoretical framework could be built to improve our understanding of the exact mechanisms that play a role beyond the one that we present here.

Chapter 4

The Net Interest Margin and the Branch Network*

4.1 Introduction

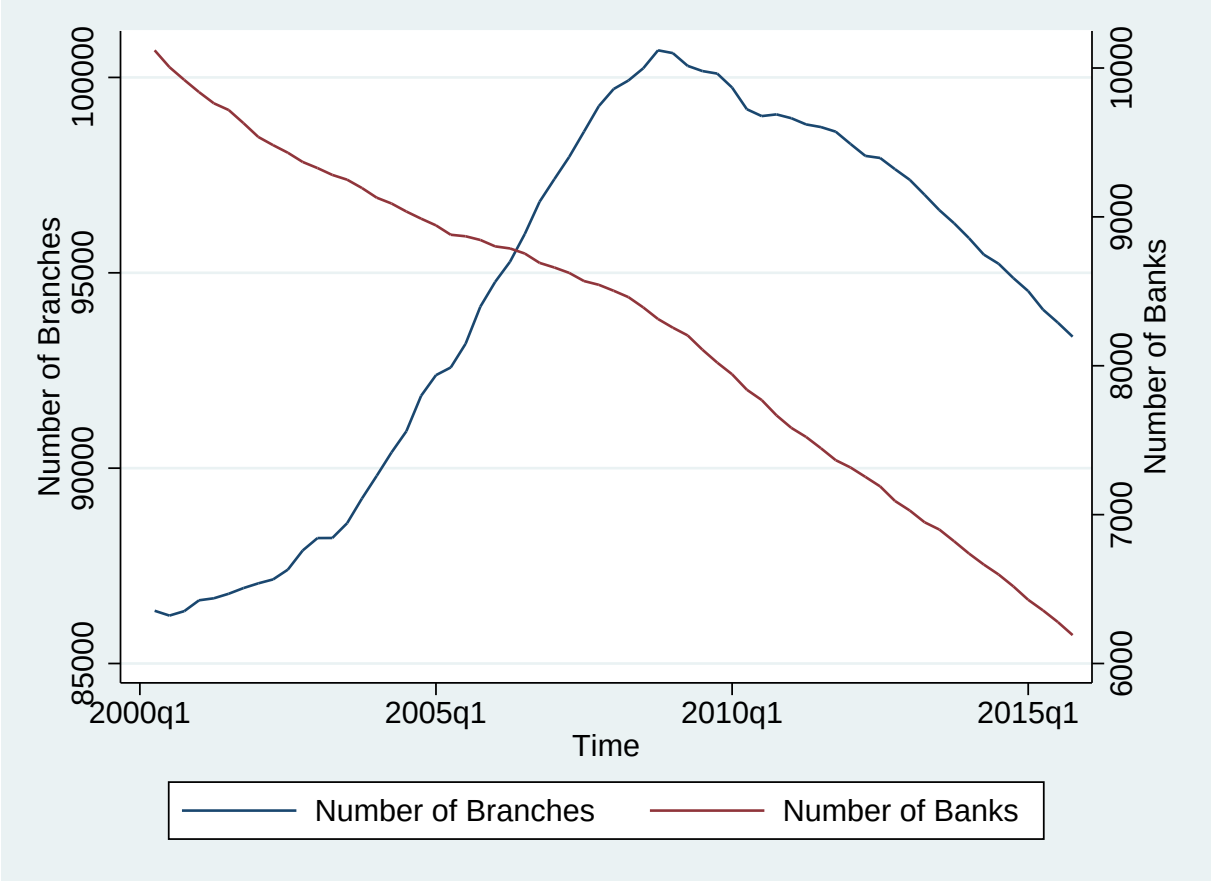
The bank branch network is of crucial importance for the flow of credit from savers to borrowers. The seminal contribution of Jayaratne and Strahan (1996) showed that bank branch deregulation in the United States benefits economic development through more efficient lending practices. Furthermore, the deregulation benefits disproportionately poorer households and minorities (Beck et al. (2007); Beck et al. (2010); Levine et al. (2014)). Recent research (Gilje et al. (2016); Berrospide et al. (2016); Gilje (2017); Córtes and Strahan (2017)) demonstrated that the bank branch network is still at the core of distribution of funds, particularly for areas with a large market share of small banks.

In the United States, the number of banks has been falling persistently in recent years. Furthermore, the slowdown accelerated in the aftermath of the financial crisis. Out of 10,170 banks that existed in 2000, 1700 had disappeared by 2009 and a further 2000 had disappeared by 2016.[1] In contrast, the number of branches, which stood at

*This chapter is a joint work with Christopher Martin Hols from the Bonn Graduate School of Economics.

[1]Bank failures account for 573 of the banks that disappeared. The vast majority of these failures

Figure 4.1: Number of banks and bank branches in the United States



Notes: This figure indicates the number of banks (right axis) and the number of bank branches (left axis) in the United States between 2000 and 2016. Data is taken from the FDIC call reports and aggregated by the authors.

100,695 in the last quarter of 2008, decreased to 93,366 in the last quarter of 2015.[2]

There is extensive literature on how geographical bank branch network diversification affects banks' funding costs. Diamond (1984) and Boyd and Prescott (1986) suggest that geographic diversification can bring additional assets that are imperfectly correlated with existing assets and this can reduce bank risk and lower funding costs. Similarly, if banks spread to diverse geographic areas where the economies are imperfectly correlated with the bank's existing local economy, they will be able to use internal capital markets to respond effectively to local shocks on asset quality and liquidity (Houston et al. (1997); Houston et al. (1998); Gatev et al. (2009) and Cornett et al. (2011)). Other studies investigate how geographical bank branch network diversification can result in increased funding costs. For example, Brickley et al. (2003) and Berger et al. (2005) show that distance can make it difficult for bank headquarters to monitor subsidiaries, which can have a negative impact on efficiency, asset quality and funding costs.

However, the literature is largely mute on the causality going in the opposite direction, i.e., how funding shocks translated into changes in net interest margins affect geographical bank branch network diversification. In fact, a reduction in the net interest margins can "... reduce gross value of core deposits, and given that branches have non-interest expenses, maintaining deposit relationships could become a negative present value business" (Claessens et al. (2017)). If an increase in the funding costs indeed affects banks' geographical branch network diversification, consequences other than for banks' profits can follow. For example, the literature on banks' interstate expansion in the United States suggests that it accelerates the economic growth and reduces the poverty rates of individual states (e.g. Jayaratne and Strahan (1996); Beck et al. (2010)).

In this chapter, we study the effect of a reduction in the net interest margin induced by higher funding costs on the branch network using a natural experiment. We iden-

took place during the financial crisis.

[2]The evolution of the number of banks insured by the FDIC and the number of branches of deposit insured banks can be found in Figure 4.1.

tify the exogenous reduction through the repeal of Regulation Q in the United States, i.e. the lift of the ban of interest payments on demand deposits in 2011. The abandonment provides a natural experiment for our setting as it increased funding costs for banks that finance their activities largely through demand deposits more strongly than for banks that finance themselves to a lesser extent through demand deposits. As banks are not forced to pay interest on deposits, our results provide additional insights into the effect of deposit market competition on bank branch networks.

To compensate for the funding costs (induced by the repeal of Regulation Q), banks might try to expand to new markets or expand market power by creating new branches in existing markets to decrease funding cost.[3] However, additional branches come at a cost, which might surpass the benefit of market power. Furthermore, some banking regulations like the Community Reinvestment Act (CRA) are tied to banks having a branch in a certain area. Therefore, banks might be willing to shrink the branch network to reduce costs and further reduce exposure to clients in poorer areas, who are protected by these kinds of regulation. In this study, we will test whether banks increase or decrease their branch networks after a funding shock. Furthermore, we will analyze how banks' soundness is affected by this change in the branch network and whether differences in the income of potential clients are associated with the change in bank risk.

Following the Great Depression, the market for deposits in the United States was tightly regulated. The Banking Act of 1933 made it illegal to pay interest on demand deposits and ceilings were imposed on the interest that could be paid on time and savings deposits. The purpose of this reform was to strengthen the soundness of banks and to stop excessive competition. Furthermore, banks would save interest expenditures, which would make it easier to pay the cost of deposit insurance.[4] In the 1960s, Benston (1964) and Cox (1967) argued that the introduction of interest ceilings was unjustified in the first place since banks that paid higher interest on deposits were

[3]Drechsler et al. (2017) suggested that banks pay lower funding costs in their deposits if their market power is greater.

[4]See Preston (1933).

not riskier than other banks before the Great Depression.

Over the years, Regulation Q, which is the chapter of the financial regulations of the United States that dealt with the interest ceilings, was modified frequently. Until the 1980s, the interest ceilings on time and savings deposits were gradually increased and in the 1980s, these interest ceilings were finally abandoned. However, the ban on interest payments on demand deposits was kept in place until the recent financial crisis. In response to the crisis, the Dodd-Frank Act was passed, which lifted the ban on demand deposits for the first time in over 70 years. In the aftermath of the reform, the market for demand deposits expanded rapidly from US $568.1 billion in the second quarter of 2011 to US $1,024.4 billion in the fourth quarter of 2013.[5]

Our results suggest that banks that relied more heavily on demand deposits before the deregulation took place experienced an increase in their interest expenses and a reduction in their net interest margin. This was compensated by a gradual reduction in their branch network by around 10% of affected banks' total branch network.[6] This indicates that the funding shock caused by the new regulation induced a reduction in the branch network. As the aggregate number of branches falls, it is unlikely that other banks will fill the gap and open new branches where affected banks closed down branches. Furthermore, banks' assets become safer and hold less capital, which is in line with the evidence that geographical diversification does not translate into lower risk for banks (Demsetz and Strahan (1997); Acharya et al. (2006); Berger et al. (2010)). In addition, the offices they build are located in richer neighborhoods and they give credit in areas that have a higher per capita income, while the individual income of the applicants does not increase. This indicates that banks react with a geographical concentration around safer markets.

Our findings contribute to several strands of the literature. First, we add to the literature on the effect of interest rate ceilings. The interest rate ceilings adopted

[5]Figure 4.3 presents the development of the number of demand deposits in the United States.

[6]We also investigate the presence of other cost-cutting measures by banks. We find that bank premises as well as the number of employees are differentially lower for the affected banks after the repeal of Regulation Q. These findings further strengthen our results related to branch closures.

after the Great Depression have been frequently analyzed over the past decades. Benston (1964) and Cox (1967) were the first to analyze whether the banks that paid higher interest on deposits engaged in hazardous business practices before the Great Depression and both rejected this hypothesis. Mingo (1978) studied the flexibility that financial institutions had under Regulation Q to rely on interest-bearing or non-interest-bearing liabilities and found that there is a negative relationship between bank risk and interest payments; that is banks become safer if they have a higher share of interest expenses to total expenses. Taggart (1978) suggested that pricing controls make it possible for banks to extract monopoly rents by analyzing savings banks from Massachusetts. James (1982) and James (1983) analyzed the effect of changes in the interest cap on savings deposits and found that banks' stock market value decreases when the caps are increased suggesting that banks gained rents from the caps. The interest ceilings had macroeconomic consequences. Mertens (2008) showed that deposit rate ceilings are in part responsible for the volatility of output and inflation and that the lift of deposit caps could be partially responsible for the great moderation. Koch (2015) studied the role of the interest ceilings in the United States on bank lending and found that whenever interest ceilings were binding lending by banks contracted sharply.

Second, we contribute to the literature on the real effects of bank branch networks in a developed economy. Gilje et al. (2016) showed that the branch network of banks still plays an important role in the transmission of liquidity shocks to the real economy. Using the recent shell gas boom caused by hydraulic fracturing (fracking), they showed that banks only expanded their lending in areas where they had branches before the shell gas boom started, as these were the only areas where they have an informational advantage. Gilje (2017) analyzed the effect of the branch network on lending markets. He found that local lending markets benefit from the internal capital market of banks that are connected through branches. Córtes and Strahan (2017) used property damage to show that banks redirect funds to other branches in the aftermath of natural disasters. This shows how important the branch network is in

order to absorb shocks. Work by Benston (1965), Evanoff (1988) and Berger et al. (1997) mostly studied the efficiency of bank branch networks. Benston (1965) raised the question whether unit or branch banking is more efficient and found that banks with more branches have a higher operational cost arising from higher overhead expenses. Evanoff (1988) analyzed the effect of bank branches on the accessibility of banking services. He found that accessibility is improved when the bank branch network is expanded. Berger et al. (1997) found that banks operate with too many branches and that reducing branches would minimize costs.

Third, we contribute to the literature on the effect of distance in banking. Petersen and Rajan (2002) suggested that the distance between lenders and borrowers is an important determinant of the interest rate firms have to pay on their loans and whether or not a loan is approved by the lender. Degryse and Ongena (2005) observed that banks engage in spatial price discrimination; the distance from the borrower to the lender decreases the interest rate while the distance between the borrower and a competitor bank increases the interest rate. Butler (2008) observed that proximity between borrowers and financial intermediaries plays an important role in the bond market. His results suggest that local investment banks are able to offer lower fees and are able to place bonds with lower yields. The effect is particularly strong for firms without credit rating. Agarwal and Hauswald (2010) observed that physical distance is important for the lenders ability to gain private information about the borrower. This leads to a trade-off between the higher availability of credit for firms nearby at the cost of higher interest rates. In contrast, Knyazeva and Knyazeva (2012) found that distance between borrowers and lenders increases the lending spread. However, they used syndicated loans of large companies, which are less affected by spatial price discrimination and rent extraction of the lenders. Bellucci et al. (2013) presented results that are in contrast to the results of Degryse and Ongena (2005). They suggested that the distance between lenders and borrowers increases the interest rate on loans. Herpfer et al. (2017) were the first to analyze how exogenous changes in the distance between lenders and borrowers affect the interest rate and availability of credit. They

observed that a lower distance increases the interest rate in existing bank-borrower relationships but also increases the probability of initiating a new relationship between a lender and a borrower. We contribute to this literature as we show that banks transform their branch network strategically to be close to potentially safer borrowers after a shock to their profitability.

Finally, we add further evidence on the interaction of finance and inequality. A seminal contribution by Beck et al. (2007) showed that financial development benefits the poor over-proportionally. The lowest quintile in the wealth distribution is responsible for more than 60 % of the impact of financial development. Beck et al. (2010) found that the income distribution of states which started the financial deregulation between the 1970s and 1990s earlier reduced income inequality, and financial deregulation especially benefited people in the lower part of the income distribution. Levine et al. (2014) showed that banking deregulation in the same period especially improved the labor market opportunities of black workers by improving bank efficiency, lowering entry barriers for non-financial firms and the competition for labor. Using two new datasets on income inequality, Tan and Law (2012) studied nonlinear dynamics between financial development and inequality. They found that financial deepening reduces income inequality at first. Following Greenwood and Jovanovic (1990) they tested if the relationship is reversed U-shaped. However, they observed that the U-shape is not inverted as Greenwood and Jovanovic (1990) proposed. So, financial development leads to a wider income distribution when financial development increases above a certain threshold. Larrain (2015), using sectoral data, showed that opening capital accounts increases income inequality because capital and highly skilled labor are complements and capital inflows boost the income of highly-skilled workers through this channel. Reilly et al. (2016) studied the effect of financial deregulation on high school graduation. They found that financial deregulation increases high school graduation rates, but this effect is heterogeneous; white individuals were significantly affected, while non-whites were not. We add further evidence to these results as we show that the deregulation of interest payment on deposits reduces the

number of branches and the availability of credit differentially more in poorer neighborhoods.

The remainder of this chapter is organized as follows. Section 2 describes the institutional framework and the data. Section 3 presents the results at the bank level, the branch level and the mortgage credit level, and Section 4 concludes.

4.2 Institutional Framework and Data

4.2.1 Relationship between Funding Cost and Bank Branches

Our primary object of interest is the effect of a funding shock on the bank branch network. To elaborate on this, one more general issue has to be clarified: Why do banks open branches and take retail deposits in the first place? Banks could finance themselves using capital markets paying the short-term interest rate and use these funds to lend to their customers. However, evidence from Drechsler et al. (2017) indicates that banks have market power in the deposit market and are therefore able to fund themselves more cheaply than using short-term bonds. Following the literature on the impact of distance in bank lending, we assume that at least part of this market power arises from the existing branch network. However, sustaining an elaborate network of bank branches is costly. Even in the absence of interest payments, which was the case as long as Regulation Q was in place, banks will compete for depositors but can only do so using non-financial measures. One possibility would be to offer cheaper service, i.e. lower fees while the alternative is to build more branches to be close to customers. Once interest payments on deposits are permitted, this increases the costs of funding, especially for banks that had a lot of demand deposits in their balance sheets, assuming that they want to sustain the same deposit base, while the operational costs remain unchanged.[7] Even if banks attract more depositors after they

[7]Banks had the choice of paying interest but were not forced to. However, banks might obviously face problems attracting new depositors and keeping the old depositors if they decide not to pay any interest.

are allowed to pay interest on deposits and lend more to customers, their margin will decline as the interest rates on earning assets remain constant or even decline due to the higher supply of credit.

In the spirit of Drechsler et al. (2017), we consider a simple model of bank funding to study the reaction to the abandonment of Regulation Q. Banks can invest one dollar today and gain income y tomorrow. We fix asset returns as well as bank size as we want to highlight the effect on the funding side of the bank. This dollar has to be raised entirely through deposits as the bank does not own any equity.[8] There are two ways of raising deposits: (1) paying an interest rate equal to the short-term interest rate r, or (2) opening branches at the proportional cost $c(\gamma)$. The cost is increasing in γ, which is the share of deposits raised through branches. $\beta(\gamma)$ is the interest paid on deposits and it decreases in γ. As customers have to travel less far to the branch if it is closer to their location, they might be willing to accept lower deposit rates (Degryse and Ongena (2005)). The problem of the bank is therefore:

$$\min_{\gamma} \; cost = \gamma(c(\gamma) + \beta(\gamma)) + (1-\gamma)r \;\; , \;\; \gamma \in [0,1]$$

If we assume that $c(0) = 0$ and $r > 0$, then there exists an interior solution in which banks use both deposit funding as well as market funding.

Let us now consider the two cases before and after the lifting of the deposit rate ceiling. Once the deposits ceiling is in place, the interest paid on deposits is $\beta(\gamma) = 0$. Therefore, it is optimal for the bank to build sufficient branches such that the marginal cost of the branch network is equal to wholesale funding and hence:

$$\gamma c'(\gamma) + c(\gamma) = r$$

After the ceiling is lifted, the banks' problem changes. If the bank has higher

[8]The income prospects, as well as the capital structure of the bank, are irrelevant for our argument as there is no risk in this model. Therefore, we abstract from them as the bank is entirely deposit-funded and asset returns are fixed and independent from the bank's funding structure.

market power (higher γ), its interest payments on deposits will decrease.[9] Therefore, the cost-minimizing number of branches is given by

$$\gamma(c'(\gamma) + \beta'(\gamma)) + c(\gamma) + \beta(\gamma) = r$$

with $\beta(\gamma) > 0$ and $\beta'(\gamma) < 0$. This leads to the conclusion that depending on whether $\gamma\beta'(\gamma) + \beta(\gamma) \lesseqgtr 0$, banks will build more or fewer branches after deregulation and interest rates depend on market power. If the interest rate paid on deposits is close to the market rate or the sensitivity to market power is low, the branch network will decrease in size, while banks will expand their branch networks if interest rates are highly sensitive to market power.

4.2.2 Data

Our analysis makes use of three data sources. First, we use bank balance sheet information for all depository institutions in the United States, which we obtain from the Statistics on Depository Institutions (SDI), provided by the Federal Deposit Insurance Corporation (FDIC), for 8 quarters before (2009Q3) and after (2013Q3) the reform took place.[10] This gives us a sample of more than 7,000 banks over 16 quarters. A list of all variables used in the analysis can be found in Table 4.1. Following Kashyap et al. (2002), we attempt to minimize the number of observations excluded from our sample to avoid a potential sample selection bias. Therefore, we do not account for bank mergers in our sample. Although bank mergers were common for our sample period, dropping banks that engage in merger activities does not alter any of our principal results, which we document in a following section in this chapter.

The summary statistics for the entire time span can be found in Table 4.2, the summary statistics after winsorizing are presented in Table 4.3 and the summary statistics for the second quarter of 2011 (one quarter before Regulation Q was in place)

[9] In this context, higher market power arises from the reduced distance to the customer.

[10] As all banks that offer insured deposits are part of the sample, we also include banks with new business models like internet banks. However, they account for only a very small share of our banks

Table 4.1: Variables definitions

Variable	Definition
Number of Branches	The number of physical domestic branches located in the United States a bank operates with.
Number of Branches per $ bil. of Deposits	The number of physical domestic branches located in the United States a bank operates with rescaled by the amount of deposits.
Share of Demand Deposits	The share of a bank's liabilities financed by demand deposits.
Share of Deposits	The share of a bank's liabilities financed by total deposits.
Number of Employees	The number of employees in FTE (Full Time Equivalent).
Number of employees per $ bil. of Deposits	The number of employees in FTE (Full Time Equivalent) rescaled by the amount of deposits.
Bank Premises	Real estate and equipment owned by the bank and used for its operations as a share of total assets.
Interest Expenses	The ratio of interest expenses to total deposits.
Net Interest Margin	The ratio of net interest income to total assets.
Nonperforming Assets	The ratio of nonperforming assets to total assets. Nonperforming assets are assets whose payment is more than 90 days overdue and real estate owned by the bank not used for operations, i.e. real estate from mortgage delinquencies.
Risk-weighted Assets	The ratio of risk-weighted assets to total assets.
Capital Asset Ratio	The ratio of total equity to total assets.
Average of Branch Median Income	The average over the median income of the zip-code where the branch is located.
All loans - Areas	The average income of the areas in which loan applicants from the HMDA database resident in.
Bank Size	The log size of a bank's total assets.
Profitability	A bank's return on assets.
Liquidity	A bank's ratio of securities to total assets.
Share of agricultural Loans	The ratio of loans financing agricultural production and loans secured by farm land to total assets.
Share of C&I Loans	The ratio of commercial and industrial loans and loans secured by non-farm non-residential owner-occupied properties to total assets.
Share of mortgage Loans	The ratio of loans secured by single family and multi-family home to total assets.
Share of consumer Loans	The share of consumer loans to total assets.
Z-score	The sum of a bank's return on assets and capital ratio divided by standard deviation of return on assets.

Table 4.2: Summary statistics - bank level - complete interval - not winsorized

	Obs.	Mean	Median	S.D.	Min	Max
Number of Branches	113146	13.09	3.00	138.17	0.00	6728.00
Number of Branches per $ bil. of Deposits	113122	62.27	26.03	3091.22	0.00	1.00e+06
Share of Demand Deposits	113011	11.37	10.25	8.13	0.00	97.64
Share of Deposits	113146	82.99	85.03	9.58	0.00	115.19
Number of Employees	113011	276.75	38.00	4624.24	0.00	231333.00
Number of Employees per $ bil. of Deposits	113011	272.96	247.00	440.73	0.00	77294.69
Bank Premises	113011	1.78	1.48	1.45	0.00	28.37
Interest Expenses	112987	3.60	0.65	227.17	-0.00	35630.20
Net Interest Margin	113011	2.18	2.11	2.58	-1.61	759.90
Non-performing Assets	113011	2.68	1.52	3.56	0.00	49.07
Risk-weighted Assets	113011	65.33	66.58	13.93	0.00	199.67
Capital Asset Ratio	113011	11.37	10.25	6.63	-214.95	100.00
Average of Branch Median Income	107580	10.81	10.78	0.30	9.20	12.25
All Loans - Areas	38206	11.06	11.06	0.18	9.97	11.61
Bank Size	113146	1211.25	1196.89	134.88	421.95	2139.01
Profitability	113011	0.24	0.33	15.52	-5084.11	202.89
Liquidity	113146	21.64	18.81	15.93	-0.02	99.51
Share of Agricultural Loans	105728	8.14	2.37	12.01	0.00	85.93
Share of C&I Loans	113146	24.05	22.31	14.79	0.00	96.15
Share of Mortgage Loans	113146	21.39	18.12	15.38	0.00	100.93
Share of Consumer Loans	113011	3.69	2.23	6.40	0.00	105.69
Observations	113146					

Notes: This table presents the summary statistics of the variables used in the analysis. Ratios are stated in percentage points, and Bank Size is the logarithm of the bank size multiplied by 100. Number of branches and Number of branches per bil. $ of deposits are not rescaled.

Table 4.3: Summary statistics - bank level - complete interval

	Obs.	Mean	Median	S.D.	Min	Max
Number of Branches	113146	7.15	3.00	13.85	1.00	102.00
Number of Branches per $ bil. of Deposits	113122	31.96	26.03	27.82	2.14	254.32
Share of Demand Deposits	113011	11.37	10.25	8.13	0.00	97.64
Share of Deposits	113146	82.99	85.03	9.58	0.00	115.19
Number of Employees	113011	109.52	38.00	280.37	4.00	2309.00
Number of Employees per $ bil. of Deposits	113011	261.40	247.00	119.89	71.63	1080.14
Bank Premises	113011	1.76	1.48	1.34	0.02	6.56
Interest Expenses	112987	0.84	0.65	0.70	0.07	5.81
Net Interest Margin	113011	2.16	2.11	1.10	0.49	5.37
Non-performing Assets	113011	2.46	1.52	2.65	0.00	10.23
Risk-Weighted Assets	113011	65.35	66.58	13.56	26.07	96.34
Capital Asset Ratio	113011	11.20	10.25	4.43	5.13	42.24
Average of Branch Median Income	107580	10.81	10.78	0.30	9.20	12.25
All Loans - Areas	38206	11.06	11.06	0.18	9.97	11.61
Bank Size	113146	1211.25	1196.89	134.88	421.95	2139.01
Profitability	113011	0.24	0.33	15.52	-5084.11	202.89
Liquidity	113146	21.64	18.81	15.93	-0.02	99.51
Share of Agricultural Loans	105728	8.14	2.37	12.01	0.00	85.93
Share of C&I Loans	113146	24.05	22.31	14.79	0.00	96.15
Share of Mortgage Loans	113146	21.39	18.12	15.38	0.00	100.93
Share of Consumer Loans	113011	3.69	2.23	6.40	0.00	105.69
Observations	113146					

Notes: This table presents the summary statistics of the variables used in the analysis. Ratios are stated in percentage points, and Bank Size is the logarithm of the bank size multiplied by 100. Number of branches and Number of branches per bil. $ of deposits are not rescaled.

Table 4.4: Summary statistics - bank level - second quarter 2011

	Obs.	Mean	Median	S.D.	Min	Max
Number of Branches	7522	7.14	3.00	13.84	1.00	102.00
Number of Branches per $ bil. of Deposits	7520	32.00	26.09	27.48	2.14	254.32
Share of Demand Deposits	7513	11.16	10.11	7.86	0.00	87.41
Share of Deposits	7522	83.20	85.25	9.49	0.00	100.85
Number of Employees	7513	109.60	37.00	281.29	4.00	2309.00
Number of Employees per $ bil. of Deposits	7513	262.75	249.48	119.35	71.63	1080.14
Bank Premises	7513	1.76	1.49	1.34	0.02	6.56
Interest Expenses	7511	0.59	0.56	0.33	0.07	5.81
Net Interest Margin	7513	1.74	1.73	0.41	0.49	5.37
Non-performing Assets	7513	2.60	1.62	2.74	0.00	10.23
Risk-Weighted Assets	7513	64.87	66.00	13.25	26.07	96.34
Capital Asset Ratio	7513	11.25	10.29	4.44	5.13	42.24
Average of Branch Median Income	7469	10.81	10.78	0.30	9.20	12.25
All Loans - Areas	2518	11.06	11.06	0.18	10.04	11.60
Bank Size	7522	1210.37	1195.26	134.57	451.09	2130.61
Profitability	7513	0.27	0.34	2.00	-131.87	80.90
Liquidity	7522	22.36	19.68	16.01	0.00	99.28
Share of Agricultural Loans	6805	8.35	2.70	11.98	0.00	81.53
Share of C&I Loans	7522	24.18	22.53	14.81	0.00	94.89
Share of Mortgage Loans	7522	21.27	18.13	15.26	0.00	97.76
Share of Consumer Loans	7513	3.67	2.22	6.50	0.00	100.45

Notes: This table presents the summary statistics of the variables used in the analysis in the quarter before the reform took place. Ratios are stated in percentage points, and Bank Size is the logarithm of the bank size multiplied by 100. Number of branches and Number of branches per bil. $ of deposits are not rescaled.

Table 4.5: Summary statistics - normalized differences without matching

	Mean		Normalized	Observations	
	Untreated	Treated	Difference	Untreated	Treated
Number of Branches	8.20	3.99	-0.24	5,635.00	1,887.00
Number of Branches per $ bil. of Deposits	30.07	37.76	0.19	5,633.00	1,887.00
Share of Demand Deposits	7.74	21.41	0.85	5,635.00	1,878.00
Share of Deposits	82.33	85.79	0.28	5,635.00	1,887.00
Number of Employees	128.61	52.54	-0.22	5,635.00	1,878.00
Number of Employees per $ bil. of Deposits	249.26	303.25	0.31	5,635.00	1,878.00
Bank Premises	1.75	1.78	0.02	5,635.00	1,878.00
Interest Expenses	0.65	0.40	-0.53	5,633.00	1,878.00
Net Interest Margin	1.72	1.80	0.13	5,635.00	1,878.00
Non-performing Assets	2.76	2.10	-0.18	5,635.00	1,878.00
Risk-weighted Assets	65.68	62.45	-0.17	5,635.00	1,878.00
Capital Asset Ratio	11.31	11.06	-0.04	5,635.00	1,878.00
Average of Branch Median Income	10.82	10.78	-0.10	5,598.00	1,871.00
All Loans - Areas	11.07	11.03	-0.14	2,009.00	509.00
Bank Size	1,228.62	1,155.86	-0.39	5,635.00	1,887.00
Profitability	0.27	0.30	0.01	5,635.00	1,878.00
Liquidity	22.06	23.25	0.05	5,635.00	1,887.00
Share of Agricultural Loans	8.19	8.80	0.04	4,954.00	1,851.00
Share of C&I Loans	24.18	24.20	0.00	5,635.00	1,887.00
Share of Mortgage Loans	23.14	15.69	-0.36	5,635.00	1,887.00
Share of Consumer Loans	3.52	4.14	0.08	5,635.00	1,878.00

Notes: This table presents the mean and normalized differences of our treatment and control group of the variables used in the analysis in the quarter before the reform took place. Ratios are stated in percentage points, and Bank Size is the logarithm of the bank size multiplied by 100. Number of branches and Number of branches per bil. $ of deposits are not rescaled.

Table 4.6: Summary statistics - normalized differences with matching

	Mean		Normalized	Observations	
	Untreated	Treated	Difference	Untreated	Treated
Number of Branches	4.38	4.63	0.02	1,209.00	1,238.00
Number of Branches per $ bil. of Deposits	33.28	36.30	0.08	1,209.00	1,238.00
Share of Demand Deposits	9.03	20.97	0.82	1,209.00	1,230.00
Share of Deposits	83.77	85.61	0.17	1,209.00	1,238.00
Number of Employees	59.07	62.02	0.01	1,209.00	1,230.00
Number of Employees per $ bil. of Deposits	260.38	296.96	0.22	1,209.00	1,230.00
Bank Premises	1.75	1.78	0.02	1,209.00	1,230.00
Interest Expenses	0.62	0.42	-0.51	1,209.00	1,230.00
Net Interest Margin	1.73	1.78	0.09	1,209.00	1,230.00
Non-performing Assets	2.73	2.29	-0.11	1,209.00	1,230.00
Risk-Weighted Assets	65.19	63.41	-0.10	1,209.00	1,230.00
Capital Asset Ratio	11.25	10.91	-0.06	1,209.00	1,230.00
Average of Branch Median Income	10.79	10.79	0.00	1,203.00	1,227.00
All Loans - Areas	11.06	11.04	-0.09	372.00	367.00
Bank Size	1,172.82	1,171.78	-0.01	1,209.00	1,238.00
Profitability	0.24	0.28	0.03	1,209.00	1,230.00
Liquidity	22.89	22.26	-0.03	1,209.00	1,238.00
Share of Agricultural Loans	9.60	8.50	-0.06	1,120.00	1,209.00
Share of C&I Loans	24.83	24.97	0.01	1,209.00	1,238.00
Share of Mortgage Loans	19.23	16.71	-0.14	1,209.00	1,238.00
Share of Consumer Loans	3.38	3.77	0.07	1,209.00	1,230.00

Notes: This table presents the mean and normalized differences of our matched treatment and control group of the variables used in the analysis in the quarter before the reform took place. Ratios are stated in percentage points, and Bank Size is the logarithm of the bank size multiplied by 100. Number of branches and Number of branches per bil. $ of deposits are not rescaled.

Table 4.7: Summary statistics - *continued*

	Level	Observations	Mean	S.D.	Min	Max
log Mean Income	Branch	106,228	11.16	0.38	9.15	12.65
Opened after Regulation Q	Branch	107,695	0.09	0.29	0	1
HUB Log Median Income	Mortgage	5,973,88	11.08	0.23	9.67	11.6
Applicant Income	Mortgage	6,306,67	4.24	0.74	0	9.21
Denied	Mortgage	5,452,182	0.19	0.40	0	1
Minority Status	Mortgage	5,457,268	0.17	0,38	0	1
Purchased Loan	Mortgage	6,454,147	0.45	0,50	0	1

Notes: This table presents the log mean income of all branches' zip-codes and the number of branches that were opened after Regulation Q had taken place. It also presents the area's log median income of all loan applications, the log of applicant's income, whether or not the loan application was denied, the minority status and whether the loan was purchased or not.

can be found in Table 4.4. Our main dependent variable is the number of branches per $ billion of deposits. Additionally, we look at the number of branches of a bank. The mean number of branches per $ billion of deposits is 32.0 and the median is 26.09, while the mean number of branches is 7.14 and the median is 3. Our treatment variable is the interaction of a reform dummy that is zero (0) before the third quarter of 2011 and one (1) afterwards, and a dummy that is one (1) if the bank's share of demand deposits to total assets banks have on their balance sheet in the second quarter of 2011 is in the upper quartile.[11] The median share of demand deposits to total assets is 11% and banks are considered treated if their share is above 14.9%, which corresponds to the third quarter of the demand deposits to total assets ratio. The treated banks fund themselves with around 20% of demand deposits on average, while the demand deposit share of the banks that are considered untreated is 8%.

Banks might compensate the increase in the interest rate on demand deposits by reducing interest rates on other deposits and, therefore, they might not experience a funding shock and the net interest margin remains unchanged. For this reason, we analyze the ratio of net interest income to total assets and the ratio of interest expenses to deposits. The net interest margin should decrease, while the interest expenses should increase more for banks with a large share of demand deposits in

[11] As a robustness check, we interact the reform dummy with the continuous share of demand deposits to total assets.

the aftermath of the reform. Mean net interest margin is 2.16% and mean interest expenses are 0.58%.

To analyze if banks reduced their risk after the reform, we consider two measures. First, we employ banks' nonperforming assets to total asset ratio. If banks changed the composition of their branch network in the aftermath of the reform towards areas with better borrowers, we would expect to see a decline in the ratio of non-performing assets to total assets. The mean share of non-performing assets is 2.6%. In addition, we consider the ratio of risk-weighted assets to total assets as an additional risk measure which should capture the riskiness of the bank as well. The mean share of risk-weighted assets over all banks is 65%. Additionally, we want to test if the treated banks reduced their capital in order to save costs. Therefore, we consider the capital asset ratio. The mean capital asset ratio is 11.2%. As non-performing loans are a backward-looking measure of bank risk and risk-weighted assets are subject to manipulation, we consider a third risk measure which is the Z-score. As it is necessary to calculate the standard deviation of earning to calculate the Z-score, we can only test for differences in the Z-score by collapsing the data before and after the reform.

All variables were winsorized at the 1% level. In some specifications we employ additional bank level controls such as the profitability of banks measured by the ratio of net income to total assets (Profitability), the size of the bank measured by the logarithm of banks total assets (Bank Size) and the bank's liquidity of the balance sheet measured by the ratio of securities to total assets (Liquidity).[12] All results are presented with or without controls.

Following Imbens and Wooldridge (2009), we test whether the normalized difference between our variables is small enough to employ standard regression approaches. The normalized differences in the second quarter of 2011 are reported in Table 4.5. We observe that the standardized differences are lower than the rule of

[12]In this context, liquidity is supposed to capture the ability of the bank to restructure its balance sheet in the short-term. A large share of securities on the balance sheet (in contrast to loans) makes the bank more flexible if it observes a higher loan demand as the securities can be sold on the market and the liquidity can be used for new loan origination. It should not be associated with regulatory liquidity measures like the liquidity coverage ratio.

thumb of ± 0.25 for most of our variables. However, banks have a substantial difference in their size, their share of demand deposits and their interest expenses. While it is obvious that banks that rely on a large share of demand deposits have lower interest expenses, as interest payment was prohibited on these kinds of deposit, the fact that banks in our control group are generally larger and have a lower share of deposit funding might be problematic in general. To avoid contamination of our results, we construct a new control group by matching banks that are treated to banks that are in the same state and have only one log difference in size. The normalized differences for the matched subsample can be found in Table 4.6. For this matched sample, the normalized differences are below the 0.25 cutoff for all variables apart from interest expenses and the share of funding achieved through demand deposits. Therefore, we are comparing banks of comparable size, with similar asset structures and comparable funding strategies.

In the next step, we exploit information on each branch a bank has using the FDIC Summary of Deposits. It provides a yearly panel of all branches of all depository institutions in the United States including the amount of deposits held in that branch, the establishing date, the acquisition date and most importantly by branch's location. Using this data, we can analyze where treated banks build or acquire new branches. The summary statistics can be found in Table 4.7. The main variable of interest is the median income in the branch zip-code location in the year 2010.[13] Furthermore, we only consider branches of type 11 (Full Service Bricks and Mortar Office) and type 12 (Full Service Retail Office) because we want to exclude cyber offices and offices that have limited service, which might be much cheaper to run but do not provide the same benefits as full-service branches do.[14] In the next step, we calculate the average of all branches zip-codes' median incomes for every bank in a year. If banks reduce branch presence, especially in poorer neighborhoods, or create branches in

[13] Zip-code-level income is only available in Census years. However, as we are interested in whether banks move to richer neighborhoods, it should not bias our results that income in a zip-code remains constant over time.

[14] Cyber offices and offices that have limited service comprise around 2% of the overall branches in our sample.

richer neighborhoods, we would expect that average median income of all branches to increase. We have around 106,228 different branches in the sample from which around 9.3% were established after the abandonment of Regulation Q.

Finally, we consider the mortgage loans originated by each bank. To do so, we make use of the Home Mortgage Disclosure Act (HMDA) database. The HMDA provides data on all loan applications, whether they were accepted or denied, applicants' income, loan size, minority status and location for mortgages. Using this data, we can analyze whether banks that are more affected by the abandonment of Regulation Q give more loans in better neighborhoods than before and even to richer households. The summary statistics can be found in Table 4.7. The main dependent variable is HUD-income which is the yearly median household income in the county of residence and the applicants' income.[15] We collapse the data at the bank and year level. Furthermore, we only consider observations if banks have at least 20 loan applications. In a further step, we only include loans that were not used for refinancing and loans that were granted. Excluding loans only made for refinancing, our sample has around 6.4 million observations, out of which around 19% of the applications were denied, 17% of the loan applications were made by applicants belonging to a minority and around 44,5% of the loans were securitized.

4.2.3 Deposit Legislation

The banking regulation in the United States acknowledges three types of deposits (Table 4.8), namely demand deposits, savings deposits and time deposits. The practical difference between the former and the two latter is that demand deposits are callable on demand while the bank has the right to wait until it pays out the funds invested in savings and time deposits.[16] Therefore, demand deposits were traditionally used for checking accounts, while time and savings deposits were used to invest and gain interest. In the 1970s and 1980s, two innovations took place that were able to substitute

[15] Zip-code information is not available for the HMDA data.

[16] The minimum time a bank had to demand such that the deposit was not considered a demand deposit was 7 days.

demand deposits: (1) money market funds emerged, which offered a higher interest rate than Regulation Q permitted and (2) NOW (Negotiable Order of Withdrawal) accounts, which are deposit accounts that pay interest and an unlimited amount of checks may be written upon and thereby circumvent the ban on interest payment on demand deposits.[17] However, banks had the right to take 7 days until they transfer the payments and these accounts could only be used by consumers and not by companies. This forced companies to continue using demand deposits for their cash management.

[17]See IMF (2010) for the explanation, why money market funds circumvented Regulation Q. See Gilbert et al. (1986) on the staggered introduction of NOW accounts and how they circumvent Regulation Q.

Table 4.8: Deposit types

Deposit Type	Description	Account Type	Interest	Callable
Demand Deposits	A deposit that is payable on demand, or a deposit issued with an original maturity or required notice period of less than seven days, or a deposit representing funds for which the depository institution does not reserve the right to require at least seven days' written notice of an intended withdrawal	Checking accounts; Certified, cashier's, teller's and officer's checks; Traveler's checks and money orders; Checks or drafts drawn by, or on behalf of, a non-United States office of a depository institution on an account maintained at any of the institution's United States offices; Letters of credit sold for cash; Withheld taxes, withheld insurance and other withheld funds; Time deposits that have matured	Forbidden until the abandonment of Regulation Q in the second Quarter of 2011	less than 7 days
Time Deposits	A deposit that the depositor does not have a right and is not permitted to make withdrawals from within six days after the date of deposit unless the deposit is subject to an early withdrawal penalty of at least seven days' simple interest on amounts withdrawn within the first six days after deposit	Certificate of deposit	Allowed	At least 7 days
Savings Deposits	A deposit or account with respect to which the depositor is not required by the deposit contract but may at any time be required by the depository institution to give written notice of an intended withdrawal not less than seven days before withdrawal is made, and that is not payable on a specified date or at the expiration of a specified time after the date of deposit. The term savings deposit includes a regular share account at a credit union and a regular account at a savings and loan association.	Passbook savings account; statement savings account; money market deposit account (MMDA); NOW accounts	Allowed	At least 7 days

Notes: This table presents the different kind of deposits that exist under the US regulation. Information on the regulation is taken from Title 12: Banks and Banking, Part 204-Reserve Requirements of Depository Institutions (Regulation D) §204.2 Definitions.

The Glass-Steagall Act of 1933 eliminated interest on demand deposits and limited interest payments on other classes of deposits. The main reasons to do so was to prevent banks from taking excessive risk. Unfair competition in the deposit market was perceived as a reason for banks to engage in investing in hazardous securities. However, other motives played a role as well. The elimination of interest was viewed as a tool to save banks a portion of the costs they had to bear for the newly introduced deposit insurance.[18]

Over the years, interest rate ceilings on time and savings deposits increased and were finally abandoned completely through the Depository Institutions Deregulation and Monetary Control Act of 1980 by 1986. However, the ban on interest payments on demand deposits was kept in place. It remained forbidden until the Dodd-Frank Wall Street Reform and Consumer Protection Act was passed that allowed but not forced banks to pay interest on demand deposits. A similar regulation was proposed in 2009 under the Business Checking Fairness Act. However, it was turned down by Congress. While the Dodd-Frank Act was debated in parliament, the abandonment was not yet part of the legislation. It was added at the end of the legislative process without further hearing of Congress. The federal authorities sought comments on the new rule until April 6, 2011, announced the change on July 14, 2011 and the reform was enacted on July 21, 2011. Banks were concerned about the effects of the reform. Several comments, which were made public, state concerns about the stability and earnings of banks, as well as potentially disastrous consequences for rural areas.[19] Companies were the direct beneficiary of the reform as cash management became much easier for them as they were not allowed to use NOW accounts to circumvent Regulation Q.

As the legislation for the reform was already decided in 2010 but only implemented in 2011, one might question if this law change constitutes a funding shock. The abandonment of Regulation Q was decided jointly with other financial reforms in

[18]For a more detailed discussion on the motivation of the Banking Act of 1933, see Preston (1933).

[19]The FDIC received 8 comments and many of those requested to keep Regulation Q. The Fed received 55 comments. Again, the fast majority opposed the repeal.

the Dodd-Frank Act. However, the regulatory implementation was not that straightforward. The Dodd-Frank Act should have implemented the Volcker Rule as well, which has not become part of the banking regulation until today.[20] This example should illustrate that, even though being politically decided, it might take a long time for a regulation to be enacted.

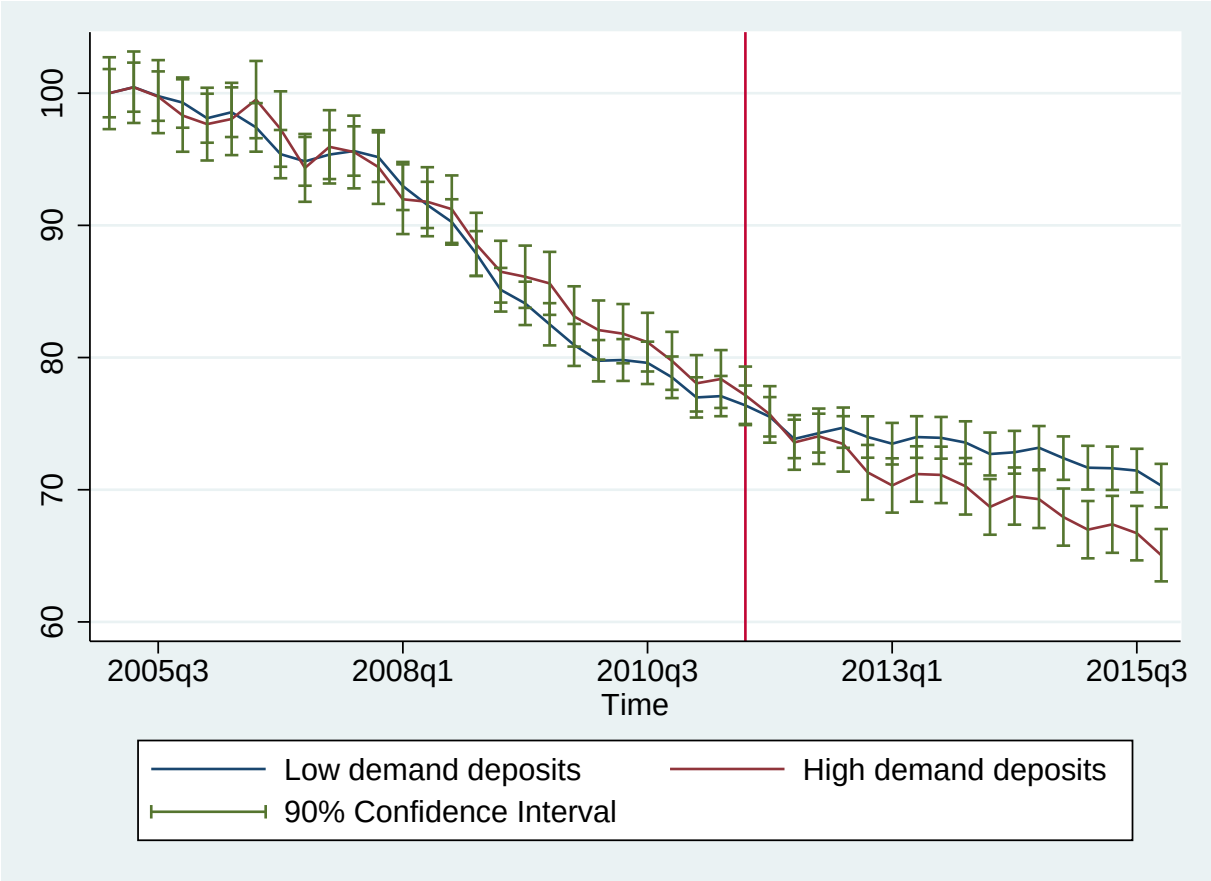
4.2.4 Anticipation Effects and Parallel Trends Test

The underlying assumption of the diff-in-diffs method is that in the absence of the treatment, treated and control groups would behave similarly. Although it is not possible to observe what would have happened had the treatment not taken place, we can still investigate if the outcome variables of the treatment and the control groups behave similarly before the intervention take place (the so called "parallel trends test"). To this end, we plot the evolution of the number of branches across treated and control banks to check for the existence of parallel trends. Figure 4.2 plots the mean number of branches per $ billion of deposits for banks in the upper quartile of the distribution of demand deposits to total assets against the mean number of branches of the remaining banks and their 90% confidence intervals. The red line indicates the third quarter of 2011. Both lines are parallel before the reform and start diverging slowly after the reform was passed, becoming significant some years after. No anticipation effect of the reform is apparent as the divergence starts after the intervention took place and the trends of both subgroups are parallel before the reform was enacted. Note that the two lines start to diverge three quarters after the regulatory intervention took place and further divergence happens with a slow pace thereafter. We attribute this observation to a potential inertia in closing down some of the physical branches, which can very well take some time.

As anticipation does not seem to play a role and trends between treatment and control group are parallel before the reform was enacted, we arguably measure the

[20]The Volcker Rule bans proprietary trading by commercial banks. Even through implemented on July 21, 2015, the rule is still not effective today due to extensions granted by the FED.

Figure 4.2: Number of branches per $ billion of deposits for treated and untreated banks over time



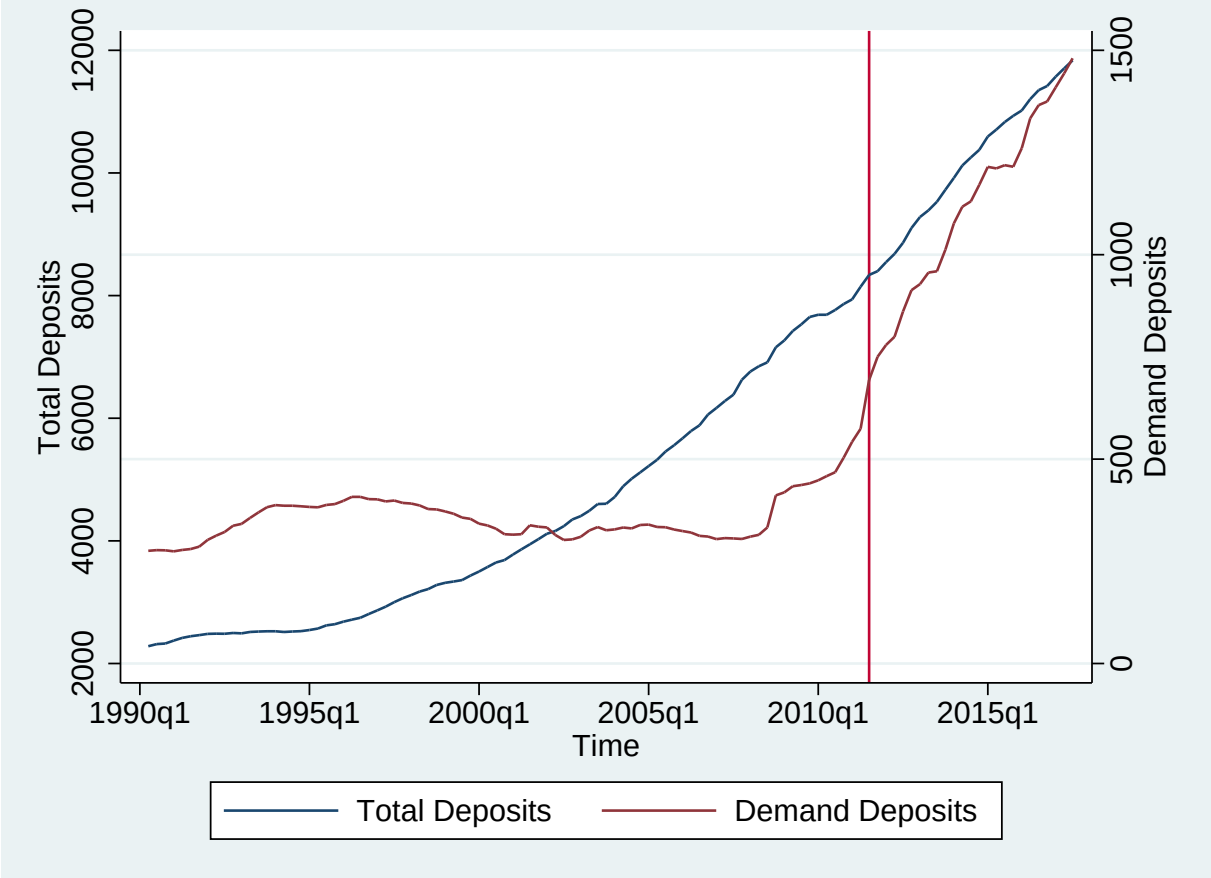
Notes: This figure plots the number of branches per $ billion of deposits for banks with a large (above 14.9% of total assets) share of demand deposits against the number of branches per $ billion of banks with a low share of demand deposits and the 90% confidence interval. Data is taken from the FDIC call reports and aggregated by the authors.

causal effect of the shock to the net interest margin induced by the abandonment of Regulation Q on the branch structure of the affected banks. Another potential explanation for our results could be that banks change their branch networks because the demand for demand deposits declined before the reform took place. To counter these concerns, Figure 4.3 plots the total amount of total deposits and demand deposits in the United States over the last 30 years. It can be seen that the amount of demand deposits did not decline. On the contrary it started to increase several quarters before the repeal of Regulation Q and grew (in relative terms) faster than total deposits after the regulatory change. However, an increase in the supply of deposits should lead to an increase in branches and not a decrease and therefore, downward bias our results. This gives us further confidence that the change in the branch network is driven by the reform and not by external demand factors.[21]

The implementation of the different parts of the Dodd-Frank act took different amounts of time. Therefore, there are no important contaminating regulatory events in the third quarter of 2011 that affect banks in terms of of demand deposits. Other possible confounding regulatory events include: On 6th of July 2011, the Federal Reserve announced the issuance of new rules on disclosure of credit score requirements. On 14th of June, the FED adopted a final rule regarding a floor for the risk-based capital requirements applicable to the largest, internationally active banking organizations. None of these events should affect the deposits funding or specifically banks with a high share of demand deposits. The only regulatory change associated with demand deposits in the period of interest was the unlimited deposit insurance coverage for non-interest-bearing transaction accounts. This change was enacted at the beginning of 2011 and ended at the beginning of 2013. However, the accounts that were eligible for the unlimited deposit insurance coverage must not bare any interest. As we are interested in the change in the interest rate of demand deposit accounts after the abandonment of Regulation Q, this change should not bias our results.

[21] In a following section, we conduct a placebo test by assigning the treatment before the actual regulatory change took place and observe no statistically significant differences across treated and control banks.

Figure 4.3: Demand deposits in the United States



Notes: This figure shows the total amount in billions of dollars of demand and total deposits in the United States from 1959q1 till 2016q3. The red line indicates the abandonment of Regulation Q. Data is taken from the Federal Reserve Bank of St. Louis.

We consider banks that are in the upper quartile of the distribution of demand deposits to total assets as treated. Obviously, every bank might be affected by the abandonment of Regulation Q and they might reshuffle their deposit portfolios and strategies. However, banks that relied to a large extent on demand deposit funding while Regulation Q was still in place, experienced a much larger exogenous increase in their funding cost than banks that only used a small share of demand deposits for funding their activities. Therefore, if a funding shock leads to changes in the branch structure of banks, we would assume that it is particularly strong for banks that relied largely on demand deposits.

As a sensitivity check, we also construct a set of matched banks. Even though the parallel trends assumption is satisfied, the treated banks in our sample might be not comparable to the remaining banks in the sample, i.e. they are substantially smaller. To counter such concerns, we construct a set of matched banks in the following way: For every treated bank in our sample, we look for a bank that is located in the same state and the difference between their log total assets is below one.

A final concern might arise from the fact that banks might be different not only in their funding but also in their asset choice. Despite controlling for state-time and bank-specific unobserved characteristics with our fixed effects setting, our results could be driven by the market trend in the corresponding markets rather than the different funding approaches. To counter these concerns, we consider the share of loans granted in the following areas: Commercial and Industrial (C&I), Agricultural, Mortgage and Consumer. We observe that the banks do not differ in these categories to a large extent. Only the share of mortgage loans exceeds the 0.25 cutoff. However, in our matched subsample the differences disappear. This strengthens our confidence that our results are driven by the liability side of the banks and not an unobserved effect affecting their asset holdings.

4.3 Results

This section describes the results. We start with the results at the bank level, then we turn to the branch level and finally the results of the mortgage credit level are presented.

4.3.1 Bank-Level Results

First, we present our baseline results concerning the number of branches. We estimated the model:

$$Branches_{b,t} = \alpha_b + \alpha_{c,t} + \beta \times Dem_{2011Q2,b} \times D_t + \gamma \times X_{b,t} + \epsilon_{b,t} \tag{4.1}$$

where b is for bank and t is for quarter. $Branches_{b,t}$ is the number of physical branches a bank operates in a given year, as suggested by Table 4.1. $Dem_{2011Q2,b}$ is a dummy that is one (1) if the share of demand deposits in the second quarter of 2011 (one quarter before the reform was enacted) is above 14.9% and D_t is a dummy that is one (1) from the third quarter of 2011 onwards. These banks are most affected by the increase in funding cost after the deregulation and therefore should react more sharply than banks with fewer demand deposits. α_b and $\alpha_{c,t}$ are bank and state-quarter fixed effects and $X_{b,t}$ are additional bank level controls. The headquarter state-quarter fixed effects should control for changes in the demand for banking services the bank faces in their local markets. We present every regression model with and without the additional bank controls.

Before turning to the effect on the branch network, we will analyze the magnitude of the funding shock. Table 4.9 presents the results. We observe that banks which relied more heavily on demand deposits before the reform took place experienced an increase in their interest expenses by around 0.1 percentage points, which is around half the difference between treated and untreated banks before the reform took place. Furthermore, the net interest margin of the treated banks is depressed by the same

Table 4.9: Interest expenses and net interest margin

	(1)	(2)	(3)	(4)
	Interest Expenses		Net Interest Margin	
Treatment	0.106***	0.100***	-0.0897***	-0.0912***
	(19.93)	(19.53)	(-12.04)	(-12.45)
Bank Size		0.00272***		0.000405
		(11.28)		(1.59)
Profitability		-0.00136		0.000841
		(-0.67)		(0.75)
Liquidity		0.000217		-0.00613***
		(0.44)		(-10.72)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	112874	112874	112898	112898
Within R-squared	0.00769	0.0296	0.00359	0.0115

Notes: This table reports bank quarter regressions of the interest expenses and the net interest margin on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. *t* statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

magnitude, falling around 0.1 percentage points. The baseline result in Table 4.10 column 1 shows that banks relying more on demand deposits in their funding reduce the number of branches per $ billion of deposits when they are allowed to pay interest on these deposits. This result is robust when bank controls (Bank Size, Profitability and Liquidity) are included, while the coefficient decreases in size. The economic magnitude of the results is between one (1) and one and a third (1.3) branches per $ billion of deposits. Considering that the total amount of deposits treated banks hold is around US $570 billion, this corresponds to a decline in the aggregated number of branches of around 570 branches. Considering that the treated banks have around 7,000 branches in total, this is a reduction of around 8%.

Furthermore, in the specification of column 3 we look at the number of branches without scaling by the amount of deposits. In the aftermath of the abandonment of

Table 4.10: Branches

	(1)	(2)	(3)	(4)
	Branches per $ bil. of Deposits		Number of Branches	
Treatment	-1.344***	-1.035***	-0.219***	-0.304***
	(-5.68)	(-4.43)	(-4.10)	(-5.39)
Bank Size		-0.143***		0.0388***
		(-7.81)		(7.64)
Profitability		-0.0312		-0.0300***
		(-0.24)		(-5.72)
Liquidity		-0.0317*		-0.00803**
		(-1.80)		(-2.41)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	113009	112874	113033	112898
Within R-squared	0.00147	0.0740	0.000731	0.102

Notes: This table reports bank quarter regressions of the number of branches per $ billion of deposits and the number of branches per bank on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Regulation Q, banks with a large share of demand deposits to total assets operate with significantly fewer branches. Controlling for additionally bank controls (column 4) increases the size of the coefficient. As we have over 7,000 banks in our sample and treatment as designed in such a way that a quarter of all banks is treated, the aggregated decline in the number of branches is 670 branches. This corresponds to around 0.5% of the total number of branches in the United States and the average yearly branch growth. Treated banks have around 7,000 branches in total. Therefore, they reduce the size of their branch network by around 10%.

The shock to the net interest margin that we identify is around 0.1 percentage points per quarter. The aggregate net interest margin has fallen around 0.3 percentage points annually since the financial crisis. The coefficient we measure indicates that the decline in the net interest margin can explain a decline in the aggregate number of branches of around 1600, which is roughly equal to a quarter of the total decrease in the aggregated branch network in the aftermath of the financial crisis.

Banks could concentrate their branches and build bigger but fewer branches, while the financial service provided is unchanged. To rule this out, we consider the number of employees and the number of employees per $ billion of deposits. The results can be found in Table 4.11. We observe that the number of employees is falling drastically. Treated banks reduce their number of full-time equivalent employees (FTE) by around 5-7 FTE in the aftermath of the reform. Treated banks have 134,015 FTE in the first quarter before the reform takes place. Therefore, they reduce their total employment by around 12,000 FTE, which corresponds to around 10% of their total employment. Furthermore, banks reduce their number of employees per $ billion of deposits by around 14. Taking into account that the banks which relied heavily on the amount of demand deposits, have a total amount of deposits corresponding to US $570 billion, this corresponds to a reduction in employment of 8,000 FTE, which corresponds to 6% of their total employment.

In addition, banks might reduce bank premises and capital to reduce costs. We observe in Table 4.12 that bank premises fall by around 0.05 percentage points, which

Table 4.11: Employees

	(1)	(2)	(3)	(4)
	Number of Employees		Employees per $ bil. of Deposits	
Treatment	-5.270***	-6.816***	-14.42***	-13.31***
	(-4.74)	(-5.75)	(-12.98)	(-12.26)
Bank Size		0.694***		-0.521***
		(7.56)		(-8.58)
Profitability		-0.510***		0.569***
		(-4.32)		(5.10)
Liquidity		-0.129		-0.431***
		(-1.42)		(-4.32)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	112898	112898	112898	112898
Within R-squared	0.00107	0.0825	0.00833	0.0601

Notes: This table reports bank quarter regressions of the number of employees per bank and the number of employees per $ billion of deposits on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.12: Bank premises and equity

	(1)	(2)	(3)	(4)
	Bank Premises		Capital Asset Ratio	
Treatment	-0.0582***	-0.0535***	-0.349***	-0.280***
	(-4.56)	(-4.22)	(-6.95)	(-5.92)
Bank Size		-0.00248***		-0.0314***
		(-6.11)		(-10.39)
Profitability		0.0000244		-0.00204
		(0.01)		(-0.19)
Liquidity		-0.00526***		0.00569
		(-8.15)		(1.56)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	112898	112898	112898	112898
Within R-squared	0.00152	0.0198	0.00226	0.0854

Notes: This table reports bank quarter regressions of the ratio of bank premises to total assets and the capital to assets ratio on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.13: Asset risk

	(1)	(2)	(3)	(4)
	Nonperforming Assets		Risk-weighted Assets	
Treatment	-0.0601*	-0.0669**	-1.028***	-1.079***
	(-1.81)	(-2.03)	(-6.20)	(-6.94)
Bank Size		0.00186**		0.0117**
		(2.44)		(2.44)
Profitability		-0.0125		0.0343***
		(-1.16)		(3.19)
Liquidity		-0.0165***		-0.232***
		(-10.53)		(-17.19)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	112898	112898	112898	112898
Within R-squared	0.000168	0.00767	0.00231	0.0568

Notes: This table reports bank quarter regressions of non-performing assets to total assets ratio and share of risk-weighted assets to total assets on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

corresponds to roughly 25% of total bank premises. Furthermore, banks reduce their capital ratio. Banks that were in the upper quartile of the distribution of demand deposits to total assets reduce their capital ratio by around 0.3 percentage points relative to their counterparts. As we use a difference-in-differences methodology, this results cannot be driven by factors like changes in the capital regulations as these effect all banks equally.

So far, our results suggest that the reform, which leads to a funding shock exogenously depressing the net interest margin, leads to a weakening of the capital balance and a reduction in the geographical diversification of banks. In the next step, we want to analyze whether this reduction in the branch network reduced banks soundness. Geographical expansion is associated with better diversification opportunities and therefore a bank's health should decrease after the branch network is cut (Diamond (1984)). To analyze if banks' assets became riskier after the reform, we perform the same analysis as before using banks' assets' soundness measures as dependent variables. Table 4.13 presents the results. Banks reduce the amount of non-performing assets in their balance sheet by around 0.06 percentage points (columns 1 and 2).

As a robustness check, we follow Delis and Kouretas (2011) and consider the ratio of risk-weighted assets to total assets. This measure is more universal than the non-performing loans and measures risks in all asset classes not only the loan portfolio. However, it is also easier for banks to manipulate this measure to save capital. Nevertheless, we observe that after the reform banks reduce the risk weight of their assets by around 1 percentage point (columns 3 and 4). This further indicates that treated banks' assets became safer in the aftermath of the reform. Note that an overall assessment of whether treated banks took less risk is inconclusive as Table 4.12 points to higher leverage ratios after the reform.

4.3.2 Robustness Tests

This section presents several tests to check the robustness of the results presented earlier. We start with presenting the results with standard errors clustered at the state level. Then, we turn to an analysis where we only consider banks whose branches are confined to the states in which bank headquarters lie. Finally, we present the results with a sample excluding mergers and acquisitions.

The results we have presented so far are based on standard error clustering at the bank level. However, this may not be sufficient if bank behavior is correlated across banks, e.g. within states. To weed out this concern, we repeat our baseline analysis with two-way standard error clustering at the bank and headquarter state level. Table 4.14 presents the results of this exercise. The findings suggest that our previous results' statistical significance hold even after a standard error clustering at the bank and the state headquarter level.

In the next step, we turn our attention to a sample where we only consider banks whose branches are confined to a single state. Baseline results presented in the previous section are conditional on headquarter state and quarter dummy variables. One reason for including such dummy variables in our analysis is to control for any demand factor varying in the community and time dimensions. However, for banks whose branches are located in more than one state, it is unclear to what extent these dummy variables capture the demand side of the story. In order to test how our previous findings change when we control for all community- and time-varying demand factors, we consider a subsample where we only focus on banks whose branches are confined to a single state. The results of this exercise are presented in Table 4.15. They indicate that our baseline findings hold for a subsample with which we can control for all time-varying state level demand shocks.

Finally, we consider a sample where we exclude banks that engage in mergers during our sample period. Previously, we discussed that we followed Kashyap et al. (2002) in an attempt to minimize the number of observations excluded from our

sample to avoid a potential sample selection bias. Therefore, we did not account for bank mergers in our sample. However, mergers may be a major way to reduce the branch network but without necessarily reducing market power. That is, banks that engage in mergers might not necessarily be affected by the increase in funding costs after the repeal of Regulation Q due to high market power. At a first glance, one can interpret that the existence of bank mergers leads to underestimation of the effects we observed so far. However, the issue could be more severe. The competition effects due to mergers could mean that banks that did not engage in mergers incurred relatively higher funding costs and, as a result, branch closures due to merger banks operating in the same market having higher market power. Under such a scenario, bank mergers could confound our treatment variable. To weed out these concerns, we repeat our baseline analysis with a sample that excludes all merger activities for the period 2009-2013. Table 4.16 presents the results of this exercise. It shows that our baseline results remain valid for a subsample that exclude merger activities. Therefore, we can claim that bank mergers do not contaminate our results.

Table 4.14: Two-way clustering at the bank and state level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Net Interest Margin		Branches per $ bil. of Deposits		Risk-weighted Assets		Nonperforming Assets	
Treatment	-0.0897***	-0.0912***	-1.344***	-1.035***	-1.028***	-1.079***	-0.0601*	-0.0669*
	(-11.34)	(-10.47)	(-4.83)	(-2.86)	(-5.93)	(-6.68)	(-1.70)	(-1.71)
Bank Size		0.000405***		-0.143***		0.0117		0.00186
		(6.36)		(-7.50)		(0.82)		(0.45)
Profitability		0.000841***		-0.0312		0.0343*		-0.0125
		(3.78)		(-0.86)		(1.91)		(-0.67)
Liquidity		-0.00613***		-0.0317		-0.232***		-0.0165***
		(-9.06)		(-0.82)		(-16.15)		(-6.46)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	112898	112898	113009	112874	112898	112898	112898	112898
Within R-squared	0.00359	0.0115	0.00147	0.0740	0.00231	0.00767	0.000168	0.00767

Notes: This table reports bank quarter regressions of the net interest margin, the number of branches per $ billion of deposits, risk-weighted assets and nonperforming loans on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank and state level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.15: Banks that are active in a single state

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Net Interest Margin		Branches per $ bil. of Deposits		Risk-weighted Assets		Nonperforming Assets	
Treatment	-0.0850***	-0.0775***	-1.401***	-1.098***	-0.782***	-0.812***	-0.0942**	-0.0986***
	(-10.37)	(-10.14)	(-7.45)	(-6.26)	(-4.16)	(-4.65)	(-2.39)	(-2.59)
Bank Size		-0.00323***		-15.01***		-0.0220***		-0.318**
		(-9.70)		(-13.09)		(-2.95)		(-2.28)
Profitability		0.0852***		-11.33		0.235***		-25.39***
		(14.84)		(-1.21)		(4.87)		(-15.54)
Liquidity		-0.00667***		-2.929***		-0.321***		-1.448***
		(-11.69)		(-2.63)		(-28.04)		(-6.78)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67975	67975	67975	67975	67975	67975	67975	67975
Within R-squared	0.0047	0.0709	0.0054	0.1428	0.0017	0.1016	0.0005	0.0394

Notes: This table reports bank quarter regressions of the net interest margin, the number of branches per $ billion of deposits, risk-weighted assets and nonperforming loans on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. The sample is confined to banks that are only active in one state. Standard errors are clustered at the bank level. *t* statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.16: Without mergers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Net Interest Margin		Branches per $ bil. of Deposits		Risk-weighted Assets		Nonperforming Assets	
Treatment	-0.0889***	-0.0772***	-1.373***	-1.010***	-0.860***	-0.831***	-0.0763*	-0.0727*
	(-10.71)	(-9.97)	(-6.87)	(-5.42)	(-4.51)	(-4.65)	(-1.88)	(-1.86)
Bank Size		-0.00429***		-16.98***		-0.0284***		-0.765***
		(-11.22)		(-9.82)		(-3.70)		(-6.31)
Profitability		0.0590***		-13.06		0.0981		-21.39***
		(6.00)		(-1.34)		(1.13)		(-8.47)
Liquidity		-0.00605***		-2.457*		-0.301***		-1.599***
		(-9.15)		(-1.88)		(-19.28)		(-7.28)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67174	67174	67166	67166	67174	67174	67174	67174
Within R-squared	0.0051	0.0657	0.0045	0.1410	0.0020	0.0957	0.0003	0.0419

Notes: This table reports bank quarter regressions of the net interest margin, the number of branches per $ billion of deposits, risk-weighted assets and nonperforming loans on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. The sample excludes banks that engage in mergers for the period 2009-2013. Standard errors are clustered at the bank level. *t* statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

4.3.3 Matched Subsample

Regression analysis only delivers unbiased results if there is no fundamental difference in the treatment and control group prior to treatment after controlling for sufficient covariates. Above, we test the parallel trend assumption and reveal that it holds true. To further strengthen the claim, we look at a sample of similar banks. Imbens and Wooldridge (2009) suggested considering the normalized differences of the variables used in the analysis. Obviously, our treated banks have lower interest expenses and a higher share of demand deposits to total assets. Furthermore, the normalized difference in size is also bigger than the rule of thumb of ± 0.25. To counter possible selection concerns, we match every treated bank with a bank in the same state, the same specialization and similar size (max difference 1 of log assets).[22] The analysis is then repeated using the matched subsample.

Tables 4.17 and 4.18 present the result of this exercise. After the matching procedure, we are left with around 36,000 observations compared with the 112,000 before. From the 1887 treated banks, we can find a possible match in around two-thirds of cases. We end up with a little more than 1200 banks for both our treatment and control group. Column 1 of Table 12 shows the results for the net interest margin. The coefficient of interest (treatment) is highly significant and the coefficient stays significant (column 2) once we control for additional bank controls. In the next step, we check whether the number of branches per $ billion of deposits decreases (columns 3 and 4). We observe that the results are similar to the coefficients in the baseline model. The magnitude of the effect remains at around one branch per $ billion of deposits.

Furthermore, asset risk (Table 4.18 columns 1 and 2) and non-performing loans (Table 4.18 columns 3 and 4) show the same behavior as in the complete sample. The two measures of bank risk decrease by around 1 percentage point and 0.1 percentage points respectively in the aftermath of the reform. This indicates that banks' assets become less risky even when decreasing their geographical diversification.

[22] If more than one match is possible, we take the best match regarding bank size.

Table 4.17: Matched branches and net interest margin

	(1)	(2)	(3)	(4)
	Net Interest Margin		Branches per $ bil. of Deposits	
Treatment	-0.0813***	-0.0806***	-1.028***	-0.701**
	(-7.56)	(-7.93)	(-3.42)	(-2.41)
Bank Size		0.00105***		-0.114***
		(2.75)		(-8.03)
Profitability		0.0426***		0.0495
		(8.83)		(0.19)
Liquidity		-0.00709***		-0.0633***
		(-7.61)		(-3.12)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	35985	35985	36103	35983
Within R-squared	0.00443	0.0288	0.00177	0.0616

Notes: This table reports bank quarter regressions of the net interest margin and the number of branches per $ billion of deposits on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.18: Matched asset risk

	(1)	(2)	(3)	(4)
	Risk-weighted Assets		Nonperforming Assets	
Treatment	-1.039***	-0.996***	-0.152***	-0.152***
	(-4.32)	(-4.56)	(-3.17)	(-3.25)
Bank Size		0.0218***		0.000173
		(2.83)		(0.14)
Profitability		0.254***		-0.192***
		(3.38)		(-6.50)
Liquidity		-0.257***		-0.0124***
		(-12.52)		(-5.11)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	35985	35985	35985	35985
Within R-squared	0.00318	0.0710	0.00161	0.0278

Notes: This table reports bank quarter regressions of the asset risk measured by the ratio of risk weighted assets to total assets and amount of nonperforming loans to total assets on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

4.3.4 Further Analysis

Our parallel trends test indicates that we measure the causal effect of the abandonment of Regulation Q on the size of banks' branch network. To strengthen this claim further, we perform a placebo test with respect to the reform. Our approach is the following: We move all events back two years, i.e. the new event date is the third quarter of 2009 and the period analyzed is between the second quarter of 2007 and the second quarter of 2011. Table 4.19 presents the results. We observe that the placebo coefficient is insignificant for the specifications with and without controls. In addition, we repeat this exercise for the matched subsample. The previous results are confirmed. For both specifications, with and without additional bank controls, the coefficient remains insignificant.

Bertrand et al. (2004) suggested collapsing the data into a pre- and post-reform period to deal with serial correlation. Obviously, serial correlation might be an issue in our setting as the bank branch network rarely changes over time. Following their suggestion, we collapse the data into a pre- and a post-reform period and run the same set of regressions as in Table 4.10. The results can be found in Table 4.20 and confirm our previous findings. In fact, the estimated coefficients are substantially bigger (two branches per bank or 2 branches per $ billion of deposits). Our analysis including bank and time fixed effects leads to R-squared values above 93%. To exclude potential over-fitting, we run our main regression without fixed effects. The results can be found in Table 4.21 and barely change. The coefficients get larger (columns 1 & 2). In addition, the results remain robust if the matched subsample is considered (columns 3 & 4).

Our reform might have diverse effects on different kinds of banks. In particular, larger banks might be less affected by the decrease in the net interest margin as they can, on the one hand, compensate with non-interest income and, on the other hand, make a larger use of loans compared to securities and therefore profit more from their branch network. We test this prediction in Table 4.22. Our results suggest

Table 4.19: Placebo test

	(1)	(2)	(3)	(4)
	Branches per $ bil. of Deposits		Branches per $ bil. of Deposits - Matched	
placebo	-0.130	-0.397	0.841	0.640
	(-0.37)	(-1.21)	(1.62)	(1.29)
Bank Size		-0.0649***		-0.0475*
		(-4.83)		(-1.82)
Profitability		-0.617***		-1.147***
		(-3.04)		(-2.78)
Liquidity		-0.0667**		-0.103
		(-2.07)		(-1.64)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	112386	112251	35908	35788
Within R-squared	0.00000597	0.0191	0.000369	0.0204

Notes: This table reports bank quarter regressions of the number of branches per $ billion of deposits on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) 8 quarters before the reform took place. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.20: Collapsed data

	(1)	(2)	(3)	(4)
	Number of Branches		Branches per $ bil. of Deposits	
Treatment	-2.074***	-1.277***	-2.053***	-4.002***
	(-10.71)	(-7.49)	(-2.88)	(-5.91)
Bank Size		0.0608***		-0.147***
		(17.50)		(-15.80)
Profitability		-0.161**		-0.723*
		(-2.29)		(-1.81)
Liquidity		-0.0136*		0.0358
		(-1.89)		(1.12)
State-Time Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	13734	13716	13730	13712
Within R-squared	0.0125	0.219	0.00118	0.130

Notes: This table reports bank regressions of the number of branches and number of branches per $ billion of deposits on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform took place. Data is collapsed before and after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.21: No fixed-effects

	(1)	(2)	(3)	(4)
	Branches per $ bil. of Deposits			
Treatment	-1.825***	-1.327***	-1.373***	-0.872**
	(-7.09)	(-5.46)	(-3.84)	(-2.57)
Regulation Q	-1.882***	-1.531***	-2.070***	-1.742***
	(-11.99)	(-6.69)	(-8.14)	(-4.34)
Treated	8.052***	1.372*	3.297***	2.951***
	(11.01)	(1.88)	(3.08)	(3.11)
Bank Size		-0.0892***		-0.117***
		(-25.32)		(-17.09)
Profitability		0.769		2.462**
		(1.60)		(2.27)
Liquidity		0.00755		-0.0966**
		(0.27)		(-2.58)
Constant	30.87***	139.8***	34.17***	172.1***
	(86.75)	(32.16)	(44.26)	(20.78)
Observations	113122	112987	36118	35998
Adjusted R^2	0.015	0.194	0.005	0.224

Notes: This table reports bank quarter regressions of the number of branches per $ billion of deposits on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Regulation Q is a dummy that is one (1) after the second quarter of 2011. Treated is a dummy that is one (1) if the bank was in the upper quartile of the demand deposit to total asset ratio in the second quarter of 2011. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

Table 4.22: Heterogeneity of the results

	(1)	(2)	(3)	(4)
	Number of Branches per $ bil. of Deposits			
Treatment	-1.486***	-1.284***	-1.701***	-1.592***
	(-6.03)	(-5.41)	(-4.25)	(-4.11)
Treatment × size_05	1.564***	2.884***		
	(4.26)	(6.48)		
Treatment × agricultural			-0.982**	-0.662
			(-2.04)	(-1.43)
Treatment × commercial			1.334***	1.599***
			(3.21)	(4.01)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	113009	112874	113009	112874
Within R-squared	0.00170	0.0747	0.00258	0.0751

Notes: This table reports bank quarter regressions of the number of branches per $billion of deposits on a dummy that is own (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform took place. Size_05 is a dummy that is one (1) if a bank is in the upper quartile of the size distribution in the second quarter of 2011, agricultural is a dummy that is one (1) if the bank has an agricultural specialization, commercial is a dummy that is one (1) if the bank has a specialization in commercial loans. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

that banks that are in the upper quartile of the size distribution prior to the reform, are unaffected by the increase in interest expenses, while the banks that are smaller are largely affected. This indicates that larger banks were less affected by the reform. Banks with different specializations might react differently to the decrease in their net interest margin. The FDIC groups banks hierarchically into 9 different categories depending on their asset portfolio. Most of the banks fall into three main categories: Agricultural Specialization (25%), Commercial Lending Specialization (45%) and Other (22%). Banks specialized in mortgage lending account for only 3% of our treated banks. Banks that are specialized in commercial lending might benefit to a

Table 4.23: Branches - continuous

	(1)	(2)	(3)	(4)
	Number of Branches		Branches per $ bil. of Deposits	
Treatment continuous	-0.0125***	-0.0185***	-0.0655***	-0.0454***
	(-3.93)	(-5.28)	(-3.97)	(-2.75)
Bank Size		0.0389***		-0.143***
		(7.65)		(-7.79)
Profitability		-0.0300***		-0.0313
		(-5.69)		(-0.24)
Liquidity		-0.00832**		-0.0323*
		(-2.49)		(-1.82)
State-Quarter Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	112898	112898	112874	112874
Within R-squared	0.000979	0.102	0.00143	0.0738

Notes: This table reports bank quarter regressions of the number of branches per bank and the number of branches per $ billion of deposits on the ratio of demand deposits to total assets interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank level. *t* statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

larger extent from a large branch network as distance to their clients shortens, which facilitates monitoring and information flow. These issues might be less pronounced for banks specialized in agricultural loans, where risks are, to a large extent, global price changes and the weather and banks with other specializations which invest a large share of their assets in the financial market. Our results confirm this view. Banks which are specialized in commercial lending do not shrink their branch network. The effect on other banks is both statistically and economically significant. Furthermore, the results indicate that banks specialized in agricultural lending might be affected more strongly by the decrease in the net interest margin. In our previous analysis, we split the sample based on the fixed cut-off of 14.9% demand deposits to total assets. Now we will relax this assumption and interact the reform dummy with the demand deposit to total asset ratio to prove that our results are not driven by our specific construction of the treatment and control groups. Table 4.23 presents the re-

Table 4.24: Bank soundness - Z-score

	(1)	(2)	(3)	(4)
	Z-score			
Treatment	0.0380**	0.0502***	0.0479*	0.0659**
	(2.05)	(2.70)	(1.73)	(2.43)
Bank Size		0.000731***		-0.000103
		(4.13)		(-0.29)
Profitability		0.0166**		0.169***
		(2.46)		(6.30)
Liquidity		0.00369***		0.00418***
		(5.30)		(3.42)
State-Time Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	13600	13600	4434	4434
Within R-squared	0.000587	0.0167	0.00138	0.0562

Notes: This table reports bank regressions of the Z-score on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform took place. Data is collapsed before and after the reform. Standard errors are clustered at the bank level. t statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

sults. Our previous results are confirmed. Banks that rely more heavily on demand deposits in their funding reduce the number of branches (columns 1 and 2) and the number of branches per $ billion of deposits significantly.

So far, we can prove that banks reduced their asset risk in the aftermath of the reform. However, as banks reduce their capital, at the same time, the overall effect on bank risk remains uncertain. To be able to at least analyze how bank soundness in terms of the probability of default changed with the treatment intervention, we employ the Z-score. It is defined as the sum of the mean capital ratio plus the mean return on assets (ROA) divided by the standard deviation of the ROA. To calculate the standard deviation of the ROA, we collapse the data into a pre and post period in which we calculate the mean of the capital ratio, the mean ROA and the standard deviation of the ROA. As the Z-score is heavily skewed, we take the logarithm of the Z-score as the dependent variable. The results are shown in Table 4.24. We observe that the log Z-score, which is inversely related to the probability of default, increases in the aftermath of the reform by between 3.8% and 5.0% (columns 1 and 2). Furthermore, the results remain robust if the matched subsample is considered and the magnitude increases to between 4.8% and 6.6% (columns 3 and 4). These results indicate that although an overall risk assessment is inconclusive, the treatment intervention improves bank soundness in terms of the probability of default.

4.3.5 Branch-Level Results

In the next step, we analyze whether the reduction in asset risk is associated with expanding into richer or retreating from poorer neighborhoods. Neighborhoods populated by households with a higher income might be safer markets as poorer customers are riskier as they tend to have more volatile earnings (Gottschalk and Moffitt (1994)). As the summary of deposits data supplies us with information for every branch a bank has, and even more importantly its location, we are able to aggregate the zip-code-level income of all branches up to a bank level measure. To achieve that,

Table 4.25: Branch-level income

	(1)	(2)	(3)	(4)
		Log Median Income		
Treatment	0.00325**	0.00302**	0.00679***	0.00645***
	(2.11)	(1.96)	(2.82)	(2.66)
Bank Size		0.000000536		0.00000101
		(1.02)		(0.93)
Liquidity		-0.000000593		-0.00000215
		(-0.61)		(-1.18)
Profitability		0.00000616***		0.0000157
		(18.40)		(0.95)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Observations	35595	35551	11660	11621
Within R-squared	0.000351	0.00576	0.00192	0.00451

Notes: This table reports regressions on the bank's average zip-code-level median income over all branches on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) if the quarter is after the second quarter of 2011. Standard errors are clustered at the bank level. *t* statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

we match the branch data to the corresponding zip-code-level median family income obtained from the US 2010 census. As zip-code-level income is only available as per census frequency (every five years), the income of a given zip-code remains constant in our sample period. Therefore, our measure only changes if the bank opens a new branch or closes an existing branch. The findings of this exercise can be found in Table 4.25. In the first instance, we run our standard model from the previous section with the dependent variable, average log median income. Column 1 presents the result. It states that banks change their branch networks towards more branches located in richer areas after the reform. The results hold true if bank controls are included. The magnitude of the effect is a 0.3 percentage points increase in median family income. To rule out selection bias in our result, we employ the same analysis with our matched data set. The results can be found in columns 3 and 4. Our previous results still hold

but the magnitude of the coefficients increases to around 0.6% in both cases. These results indicate that banks close branches in neighborhoods with a lower income or open branches in areas with a higher income, potentially harming access to finance for poorer households.

4.3.6 Loan-Level Results

Our results at the branch level suggest that affected banks' geographical diversification tend to favor higher income neighborhoods. In the next step, we would like to investigate whether this had an implication for credit provision for relatively less prosperous neighborhoods. To test this, we analyze data from the Home Mortgage Disclosure Act (HMDA). The HMDA provides data on all mortgage loan applications from households in a given area, the area's median income, the applicant's income and a link to the FDIC identifier of the bank that reported the application. We collapse this data at the bank-year level to obtain the mean HUD (Department of Housing and Urban Development) income and mean income of the applicants. Furthermore, we observe which loans were accepted and which were used for refinancing. The results are presented in Table 4.26.

Column 1 reports the change in median income after the reform for all applications. In line with our previous results, we observe that banks that had a high share of demand deposits before the reform took place receive applications from borrowers that live in 0.6% richer areas. Furthermore, we observe that there is no significant improvement in applicants' income (column 2). In addition, we exclude loans that were made for refinancing purposes. These loans might just be rolled over by the same bank and therefore downward bias our results. However, we observe that the results remain unchanged. Median family income of the area where the loan was provided increases for treated banks, while the individual applicant's income remains unchanged. Finally, we consider only loans that were granted. The observed coefficients stay robust even though we consider only loans that are granted.

These results indicate that banks tried to mitigate poorer neighborhoods after the reform as their newly issued loans are granted more to customers in richer neighborhoods than before. However, they do not achieve a significantly better client portfolio (at least in the sense of household income) as the applicants remain unchanged. This behavior might be favorable for banks as houses in richer neighborhoods represent better collateral.

Table 4.26: HUD-median income and loan applicant's income

	(1) area_income_all	(2) income_all	(3) area_income_noref	(4) income_noref	(5) area_income_acc	(6) income_acc
Treatment	0.00678***	0.00856	0.00705***	0.00998	0.00641**	0.00635
	(2.91)	(1.01)	(2.81)	(0.96)	(2.58)	(0.55)
Bank Size	0.00000141***	0.000000719	0.00000142***	0.000000647	0.00000142***	0.000000945
	(2.75)	(0.40)	(2.66)	(0.36)	(2.68)	(0.54)
Liquidity	-0.00000111	-0.00000187	-0.00000105	-0.000000987	-0.00000103	0.00000285
	(-0.92)	(-0.36)	(-0.83)	(-0.16)	(-0.80)	(0.43)
Profitability	-0.0000224**	0.0000609*	-0.0000168	0.0000833*	-0.0000136	0.0000935**
	(-2.03)	(1.68)	(-1.36)	(1.92)	(-1.10)	(2.01)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9127	9134	9125	9133	9119	9128
Within R-squared	0.00877	0.00107	0.00709	0.000992	0.00611	0.000953

Notes: This table reports regressions on the income of the ares where loan applicants live on a dummy that is one (1) if the share of demand deposits to total assets prior to the abandonment of Regulation Q was in the upper quartile of the distribution interacted with the time dummy that is one (1) after the reform. Standard errors are clustered at the bank and county level for the first two columns. *t* statistics are reported in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% respectively.

4.4 Conclusion

The branch network is at the core of the transmission of funds and the absorption of shocks in the financial system. In this study, we analyze the effects of an exogenous decrease in the net interest margin, caused by the increases in funding cost following the abandonment of Regulation Q, on the banking sector, the bank branch structure and the mortgage market. Using data on all depository banking institutions in the United States, we show that banks that relied more on financing through demand deposits before the abandonment of Regulation Q, reduced the size of their branch networks in response to the funding shock caused by the reform. Overall, the abandonment of Regulation Q led to around 700 additional branch closures, which corresponds to the average yearly branch growth in the United States and 10% of the affected banks' branch networks. Considering our results, the decline in the net interest margin in the United States since the end of the financial crisis can account for the entire decline in the number of branches in the United States. Furthermore, treated banks have been able to reduce non-performing loans, risk-weighted assets and capital ratios. It appears that banks' assets have become safer. However an overall risk assessment is inconclusive as the regulatory change induces higher leverage ratios for treated banks. In terms of bank soundness, Z-score indicates that treated banks' probability of default decreases following the abandonment of Regulation Q.

We also show that banks achieved safer assets by a reduction of financial services offered in less prosperous areas. Banks created branches in areas populated by 0.6 percentage points richer households in the aftermath of the reform. Furthermore, the area income of their loan applicants as well as granted loans increased by around 0.6 percentage points. Hence, the positive effect on banks' asset quality was achieved by reducing the availability of financial services and in particular mortgage credit to poorer households. On the other hand, companies have benefitted from the reform as they are granted the opportunity to store their cash and earn interest on it. As the repeal of Regulation Q cleared a market friction, the reduction in bank branches

might mean the return to a more efficient equilibrium.

These results contribute to the emerging literature about the importance of the bank branch network. We are able to demonstrate that the bank branch network is sensitive to regulation and funding conditions and that this has real consequences for the financial inclusion of households. Further research is needed to clarify the effect of the bank branch network on income, employment and credit access for households.

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