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Three Essays on Banking and Other Financial Institutions

Xiaonan Ma

THREE ESSAYS ON BANKING AND OTHER FINANCIAL INSTITUTIONS

by

Xiaonan Ma

Bachelor of Science
Beijing Forestry University, 2010

Master of Philosophy
The University of Hong Kong, 2013

Doctor of Philosophy
The University of South Carolina, 2018

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Accepted by:

Allen N. Berger, Major Professor

Hugh H. Kim, Committee Member

John Hackney, Committee Member

Yongqiang Chu, Committee Member

Tracey L. Weldon, Interim Vice Provost and Dean of the Graduate School

Dedication

This dissertation is dedicated to my brilliant, loving, and supportive husband; to my cutie, silly, and angel baby Ethan; and to my always encouraging and ever faithful parents and brother. Without your love and support, I could never have accomplished so much.

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Table of Contents

List of Tables

List of Figures

Chapter 1.Payday Lending and the Opioid Epidemic [1]

Abstract

This paper shows that access to payday lending affects societal health outcomes such as opioid-related mortality. I find that states allowing payday lending experience 1.5 lives lost per 100,000 population every year, which amounts to one-third of its mean value. I interpret this result in line with the increased consumption of opioid pills after accessing to payday lending. Accordingly, the effects are more pronounced in areas with low socioeconomic status. Confounding events, time trends, or systematic differences between states allowing and prohibiting payday lending are unlikely to explain the association between payday lending access and opioid mortality. Overall, my findings suggest that household finance regulations can impact societal health.

Introduction

Drug overdose is a crucial public health concern and is one of the leading causes of injury-related death in the United States. National Center for Health Statistics (NCHS) shows that more than 841,000 people have died from drug overdoses in the U.S. since 1999 (Center for Disease Control and Prevention 2021). This figure exceeds the number of U.S. COVID-19 deaths of about 664,000 as of mid-September 2021, although the COVID-19 deaths were over a short interval. The overdoses surged nearly 30% during the COVID-19

[1] Ma, X. To be submitted to *Journal of Finance*.

pandemic in 2020 (The Wall Street Journal 2021). The mortality rates have also increased about four times since 1999 (Figure 1.1).The opioid overdose death rate in particular are quite concerning, and have been called "the biggest public health epidemic of a generation" (Davenport, Weaver, and Caverly 2019). As of 2018, two-thirds of drug overdose deaths involved an opioid (Wilson et al. 2020), such as prescription opioids, heroin, and synthetic opioids (e.g., Fentanyl). The Centers for Disease Control and Prevention (CDC) data suggests that 91 Americans die every day from an opioid overdose. In addition to the tragic loss of lives, the economic losses of the opioid crisis from many sources are also significant. The crisis is estimated to have caused at least a $631 billion economic burden from 2015 to 2018 in the United States (Davenport et al. 2019).

The Trump Administration and the U.S. Department of Health and Human Services (HHS) officially declared the opioid crisis a "public health emergency" (Center for Medicare & Medicaid Services 2020) in October 2017, drawing attention from both health practitioners and policymakers in combating the crisis. It is, therefore, crucial to understand the causes of the opioid epidemic. The literature largely focuses on the effects of health care practices, government regulation, and social factors on the opioid epidemic. In this paper, I instead focus on the impact of financial forces on the epidemic. In particular, I examine how access to payday lending affects opioid-related deaths.

Payday loans emerged in the mid-1990s, and the industry has grown dramatically. There were 14,348 payday loan storefronts in the United States as of 2017 (Federal Reserve Bank of St. Louis 2019). This figure about one fifth of the number of bank branches (78,196) in 2017 (Statista 2020), despite these storefronts being prohibited in 14 of the 50 states.

The rapid expansion of the payday loan industry potentially has both favorable and unfavorable financial consequences. On the one hand, the availability of payday loans increases household credit access. The increased access to payday loans may improve individuals' overall financial conditions (Karlan and Zinman 2010; Zinman 2010), alleviate financial distress (Morse 2011), absorb expenditure shocks (Wilson et al. 2010), and reduce borrowers' depression and mental stress (Karlan and Zinman 2010). Morgan, Strain, and Seblani (2012) find after states banned payday loans, households have more financial concerns, such as more bounced checks, complaints about lenders and debt collectors, and more bankruptcy protection filings. On the other hand, other studies finds that access to the high-interest rate payday loans does not alleviate economic hardship (Campbell, Martínez-Jerez, and Tufano 2012; Melzer 2011, 2018; Skiba and Tobacman 2008). Melzer (2011) also finds that payday loan access makes it more difficult for households to pay mortgage, rent, and utility bills, and causes them to delay needed healthcare.

Access to payday lending could also favorably or unfavorably affect deaths from the opioid epidemic. On the one hand, opioid drugs are often very costly, and payday loans could help people overcome financial difficulties and increase the accessibility of opioid drugs. As such, access to payday loans could exacerbate the opioid epidemic. Furthermore, the increased financial difficulties associated with payday lending as documented by Melzer (2011, 2018) could manifest themselves as more stress, which could also induce more drug overdoses. On the other hand, the increased credit accessibility could relieve financial constraints and reduced borrowers' stress, and therefore, reduce the reliance on opioids drugs. As such, payday lending could alleviate the opioid epidemic.

In this paper, I examine the net effects of credit access to payday loans on the opioid epidemic. I establish a causal link between credit access and opioid-related deaths using the variation in payday lending access generated by state statutes allowing or prohibiting payday lending. Decisions on payday lending law change may be correlated with consumers' behaviors or macroeconomic conditions in the state. However, Bhutta, Goldin, and Homonoff (2016) show that the macroeconomic trends (i.e. unemployment rates and income per capita) and consumers' behaviors (i.e. credit card use) are very similar between the law-changing states and always-restrictive states. Using a generalized difference-in-differences specification, I find that allowing payday lending statistically and economically significantly increases opioid-related deaths. Payday lending access increases opioid-related mortality rates by about 1.5 additional lives lost per 100,000 population every year, which amounts to one third of the mean. Using the state or country and year fixed effects, results are driven by within-state legal changes, rather the across-state differences, which could be driven by other effects, such as local culture. The results are also robust after controlling for the county- and state-level time-varying socioeconomic conditions.

To ensure that the results are not driven by the time trend of increasing opioid-related mortality rates in the states allowing payday lending or the systematic differences between states allowing and prohibiting payday lending, I examine the dynamic effect before or after the state allows payday lending. The results suggest that the significant positive effect on opioid-related mortality rates only appears after, but not before, the state allows payday lending, suggesting that the results are driven by increased access to payday lending.

The baseline results could also be driven by other confounding events that occur around the same time of the payday lending law changes. For example, states could decrease healthcare spending at the same time as allowing payday lending. In this case, the increase in opioid-related deaths could be driven by decreased healthcare spending. To mitigate this concern, I conduct a falsification test to examine whether deaths of other causes also increase after states allowing payday lending. I find none of the leading causes of death is associated with the increased payday lending access, suggesting the results are less likely to be driven by the systematic change in the healthcare system.

I then proceed to identify whether the effect of payday lending laws on opioid-related deaths proceeds through the channel of increased access to opioid drugs. Using a novel dataset of opioid pill distribution, I find that the increased access to payday lending also increases opioid pill distribution, suggesting that our baseline results are indeed driven by increased access to opioid drugs.

I also exploit cross-sectional heterogeneity in socioeconomic conditions. If the results are driven by payday lending, I should observe a weaker effect in areas with high socioeconomic status because the high socioeconomic group is less likely to use payday lending. I split the sample by state GDP per capita or median household income to examine the cross-sectional heterogeneity effects. I find that the positive effects are more pronounced among states with lower GDP per capita or low median household income. These cross-sectional test results further suggest that our results are driven by the increased access to payday lending.

Finally, I undertake several additional analyses to mitigate various concerns. First, to address the concern that the results may be driven by time-varying state-level factors, especially the payday law is lobbied by the payday lenders, I examine whether access to neighbor states' payday lending also affects the opioid-related mortality rates when the home state prohibits payday lending. I include a border county dummy to indicate a county locating in a not allowing state is bordering with a state that allows payday lending. I include the state by year fixed effects and state fixed effects to control for the time-varying factors at the state level. I found that access to the neighbor states' payday lending also has a significant positive effect on opioid-related mortality rates, suggesting the baseline results are less likely to be driven by the time-varying factors at the state level. Second, to address the concern that the state legislature changes around the same time of payday law change may lead to systematic changes (e.g., reducing funding available) to treat the opioid addiction problems, I exclude states that payday law changes happen at the same time of the change of control of state legislature. The concern is that the change in control of state legislature could drive both the changes in payday lending laws, but also could cause other changes in state laws, which could then affect opioid-related deaths. After excluding those cases, the results remain robust, suggesting that the baseline results are unlikely to be driven by changes in the state legislature.

This paper contributes to interdisciplinary studies on the opioid epidemic. Previous empirical studies have investigated the impact of the opioid epidemic on socioeconomic outcomes, such as labor market participation and firm values. Li and Zhu (2019) find that the opioid epidemic is associated with higher offering yield spreads for local municipal bond issuers. Jansen (2020) finds that the opioid epidemic leads to increased loan defaults. Local opioid prescription rates have a negative relationship with labor force participation (Aliprantis, Fee, and Schweitzer 2018; Krueger 2017). Ouimet, Simintzi, and Ye (2019) find that increased opioid prescriptions are associated with worse subsequent individual employment outcomes, and in turn, affect the firm growth. This paper instead examines how access to expensive credit affects the opioid epidemic from a finance perspective.

This paper also contributes to the literature on the consequences of expensive credit access literature. The previous literature has documented both favorable (Karlan and Zinman 2010; Morgan et al. 2012; Morse 2011; Wilson et al. 2010; Zinman 2010) and unfavorable (Campbell et al. 2012; Melzer 2011, 2018; Skiba and Tobacman 2008) effects of expensive credit access. This paper adds to the literature on the unfavorable effects of expensive credit access by documenting that payday lending access can even cause public health problems. The paper therefore may also provide new ideas on how to combat the opioid epidemic more effectively.

The remainder of the paper is organized as follows. Section 2 presents the data and summary statistics. Section 3 reports the main empirical results, dynamics, falsification tests, and cross-sectional heterogeneity effects of payday lending access on opioid-related mortality rates. Section 4 explores several additional analyses, and Section 5 concludes and offers policy implications.

Data and key variables

Payday lending

Payday loans are a type of alternative financial service and usually short-term, high cost, unsecured, generally have a small principal balance ($500 or less), and are typically due on the next payday. Borrowers do not need to provide the collateral or a particular credit score to get a payday loan. It emerged in the mid-1990s, and the industry has grown rapidly and dramatically, reaching 14,348 payday loan storefronts as of 2017 in the United

States (Federal Reserve Bank of St. Louis 2019). This alternative source of finance is prevalent in the United States, and as many as 12 million Americans use payday loans each year (Federal Reserve Bank of St. Louis 2019).

The regulation of payday lending differs significantly across states and over time, which provides the basis of the identification strategy of this paper. Table 1.1 summarizes payday law changes over time. Eight states, including Connecticut, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, Vermont, and West Virginia, prohibit payday lending from 1984 to 2018. The majority of states allow payday lending in the 1990s. Some states, including Arizona, Arkansas, District of Columbia, Georgia, New Mexico, and North Carolina, prohibit payday loans after allowing payday lending for a few years.

Opioid mortality

I obtain the opioid-related mortality data at the county level from 2002 to 2018 from the CDC WONDER database. Causes of death are based on the death certificates for U.S. residents. Deaths are classified using the International Classification of Diseases, 10th Revisions (ICD-10). I follow Li and Zhu (2019) to define opioid-related deaths. I use the drug overdose (or poisoning) mortality to proxy opioid-related deaths. In particular, the following ICD-10 categories, including X40-X44 (unintentional overdose), X60-X64 (suicide by self-poisoning), or Y10-Y14 (undetermined intent), are considered opioid-related deaths. The key dependent variable $OpioidDeathRate$ is the opioid-related mortality rate per 100,000 residents.

There are several limitations of the opioid-related mortality data. First, the ICD-10 codes, as mentioned earlier, include all drug overdose deaths, not only opioid-related deaths. However, two-thirds of them are opioid-related mortality (Wilson et al. 2020). Thus, it is reasonable to proxy opioid-related mortality using drug overdose mortality. Second, the data is suppressed if a county has fewer than ten deaths. To avoid any sample selection issue, I include these counties in the analysis and replace these suppressed counties with zero for the opioid-related death rates. The results, however, remain robust if I exclude those counties from the sample.

Other variables

To control for any time-varying local socioeconomic conditions, I include controls at both state- and county-level. Data is obtained from different sources, including the U.S. Census Bureau, or Federal Reserve Bank of St. Louis. In particular, I obtain the number of jobs offered in a county, county population, and state African American population from the U.S. Census Bureau. I use data of the state's median household income, GDP per capita, percentage of the jobless labor force, and the ratio of poverty population from the Federal Reserve Bank of St. Louis. The detailed definitions of the variables and data sources are described in Table 1.2.

Table 1.3 presents the summary statistics by whether states allow payday lending or not. The opioid-related mortality rates in states allowing payday lending are about 1.2 lower than that in states not allowing payday lending. Counties in allowing states have a slightly lower population and a lower number of jobs offering than counties in not allowing states. Allowing states and not allowing states have similar state-level characteristics, except that allowing states have a slightly lower proportion of the population in poverty or and a lower African American population.

Main results - Regression analysis

To identify the effect of payday lending access on opioid-related deaths, I employ a generalized difference-in-differences specification as follows,

$$OpioidDeathRate_{i,j,t} = \beta Allowed_{i,t} + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_i \left(or\ \alpha_j\right) + \alpha_t + \epsilon_{i,j,t}, \quad (1.1)$$

where $OpioidDeathRate_{i,j,t}$ is opioid-related mortality rates per 100,000 residents in state *i*, county *j*, and year *t*. The key independent variable of interest is *Allowed*, which equals one if a state allows payday lending in year *t*, and zero otherwise. I also include either state (α_i) or county (α_j) fixed effects to control for time-invariant local economic conditions. I include year fixed effect (α_t) to absorb the effect of macroeconomic conditions. With the state (county) and year fixed effects, the variation of *Allowed* only comes from states that change their payday lending laws. County-level controls ($X_{j,t}$) include the natural logarithm of *no. of jobs* and *population*. The state-level controls ($Z_{i,t}$) include the natural logarithm of *median household income* and *GDP per capita* and the percentage of *unemployed, poverty,* and *African American* population. I include these time-varying county- and state-level characteristics to control for the socioeconomic characteristics. I cluster the standard errors at the state level because the payday lending law varies at the state level.

Baseline specification and main results

Table 1.4 presents the baseline results of the effects of payday lending access on opioid-related mortality rates. In column (1), I include only state and year fixed effects but no control variables to mitigate the concern that some control variables could also be affected by payday lending access. The coefficient estimate on *Allowed* is positive and statistically significant. Payday lending access increases opioid-related mortality rates by

1.5. I then add county-level controls in column (2) to control for time-varying county-level socioeconomic conditions. The coefficient estimate on *Allowed* remains positive and statistically significant. Besides, the magnitude of the coefficient estimate is also similar to that in column (1), suggesting that state payday lending laws are not highly correlated with local socioeconomic conditions. In column (3), I further add state-level control variables, and the results remain robust.

In columns (4) – (6), I instead include county fixed effects. The coefficient estimates on *Allowed* are still positive and statistically significant at the 1% level. The magnitudes of the estimates are slightly lower than those in columns (1) – (3). The economic significance of the estimates is sizable. After allowing payday lending, the opioid-related mortality rates increased 1.5 more deaths per 100,000 residents every year after controlling for socioeconomic, macroeconomic, and local economic conditions.

Dynamics

One concern is that the results could be driven by systematic differences between states allowing and not allowing payday lending. In particular, states allowing payday lending may be suffering from worsening economic conditions. In this case, the states could be motivated by fiscal concerns to allow payday lending. The worsening economic conditions could also cause increases in opioid drug overdose and hence opioid-related deaths. However, if this is the case, we should observe the increases in opioid-related deaths even before the passing of the payday lending laws. To examine whether this is indeed the case, I examine the dynamics of the effect. In particular, I estimate the following specification:

$$OpioidDeathRate_{i,j,t} = \sum_{k=-5}^{5} \beta_k D(Allowed_{i,k}) + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_j + \alpha_t + \epsilon_{i,j,t}, (1.2)$$

where the dummy variable $D(Allowed_{i,k})$ equals one if state i is -k years before or k years after the state allows payday lending, and zero otherwise. If there is a time trend of opioid-related deaths even before the state allows payday lending, β_k will be positive for some $k<0$. On the other hand, if the effect is driven by states allowing payday lending, β_k will be close to zero for all $k<0$, but will be positive for some $k>0$.

I plot the coefficient estimates of β_k and their 95% confidence intervals in Figure 1.2. The $\beta_k's$ are all close to zero for $k<0$. In contrast, the $\beta_k's$ become positive and statistically significant for $k>0$. The results suggest that the significant positive effects of payday lending access on opioid-related deaths are less likely to be driven by the pre-existing time trend or systematic differences, rather the results are likely to be driven by states allowing payday lending.

Falsification tests

Next, I proceed to address the concern that the results may be driven by confounding events occurring around the same time as the payday lending law changes. For example, if the law change is driven by states' incentives to increase revenue when facing a deteriorating economy, which may cause a reduction in funding available to the healthcare system. And in turn, the lack of funding of the health care system may drive up the opioid-related mortality rates. However, if that is the case, I should observe a similar effect on other causes of death. On the other hand, if the results are indeed driven by payday lending access, I should expect no effect on other causes of death. I conduct falsification tests by examining the effects on the leading causes of death in the U.S., such as deaths from accidents, heart disease, respiratory disease, chronic obstructive lung disease, pneumonia and influenza, Alzheimer's disease, stroke, and diabetes with the following model specification:

$$OtherCauseDeathRate_{i,j,t} = \beta_d Allowed_{i,t} + \delta_d' X_{j,t} + \theta_d' Z_{i,t} + \alpha_j + \alpha_t + \epsilon_{i,j,t}, \quad (1.3)$$

where controls and fixed effects are the same as the baseline analysis.

Table 1.5 presents the results of the falsification tests. None of these leading causes of death shows a statistically significant. The results suggest that the positive effect of payday lending on opioid-related deaths is not attributable to confounding events that drives both opioid-related mortality and payday lending. Payday lending access only affects opioid-related deaths, which relies on more financial resource access.

Cross-sectional heterogeneity

In this subsection, I examined the cross-sectional heterogeneity effects of payday lending on opioid-related mortality in terms of socioeconomic status. If the effect on opioid-related mortality is driven by payday lending, I expect a weaker effect in the areas with high socioeconomic status (SES) because high SES groups are less likely to use payday lending. I hence split the sample by the indicators of SES, such as the state GDP per capita or median household income to examine the heterogeneity effects.

Table 1.6 shows the results of the effects of payday lending on opioid-related deaths by GDP per capita. High groups include states whose GDP per capita falls the top tercile in year *t* and Low group otherwise. In columns (1) and (2), I include the state and year fixed effects. The coefficient estimate on *Allowed* is positive and statistically significant for the Low GDP group, and negative for the High GDP group. The coefficient estimates on the *Allowed* are significantly different between the Low and High groups. In columns (3) and (4), I instead include the county and year fixed effects. The results are consistent with those in columns (1) and (2). The effects of payday lending access on opioid-related mortality are more pronounced in states with low GDP.

Table 1.7 presents the effect of payday lending on opioid-related deaths by median household income. Similarly, I define the High median household income group with those states whose median household income falls the top tercile in year *t* and Low group otherwise. Columns (1) and (2) include the state and year fixed effects. The results are consistent with using state GDP per capita. The coefficient estimate on *Allowed* is positive and statistically significant for the Low median household income group, and negative for the High median household income group. The effects are significantly different between the two groups. Columns (3) and (4) instead include the county and year fixed effects. The results are consistent with columns (1) and (2).

Overall, the heterogeneity analyses suggest the effects of payday lending are more pronounced among the low SES group, which is more likely to use payday lending, therefore, suggesting that the baseline results are driven by payday lending.

Additional analyses and robustness check

To mitigate various concerns, in this subsection, I explore some additional analyses and robustness checks.

Neighboring states' payday lending access

To mitigate the concern that the results are driven by any time-varying factors at the state level, e.g., unobserved socioeconomic factors or the payday lending laws are lobbied by the payday lenders in that state, I explore the effect among the states not allowing payday lending, but their neighbor states allowing payday lending. Specifically, I examine this effect by focusing on the border counties that their home states prohibit payday lending, but their neighbor states allow payday lending. In particular, I follow the Melzer (2011) approach and estimate the following regression specification,

$$OpioidDeathRate_{i,j,t} = \gamma Border_{j,t} + \delta_b' X_{j,t} + \alpha_{it} + \epsilon_{i,j,t}, \qquad (1.4)$$

where $Border_{j,t}$ equals one if county j is in a not allowing state but adjacent to an allowing state, and zero otherwise. I include the state by year fixed effects to control for the home states' time-varying economic conditions. Within this model, the identifying variation of *Border* only comes from the neighbor states' payday lending access, but not payday lending laws in their own states.

Table 1.8 presents the effects of neighbor states' payday lending on opioid-related mortality. The coefficient estimate on *Border* is positive and statistically significant at the 5% level. The magnitude is smaller than the estimates in the baseline analysis, but the magnitude is also sizable. Access to payday lending from neighbor states increases opioid-related mortality rates by almost one more death per 100,000 population every year. The results suggest that the effect of payday lending access on opioid-related mortality is less likely to be driven by time-varying factors at the state level.

State legislature

One concern is that the changes in payday law may coincide with changes in the state legislature. For example, Republicans may be more likely to allow payday lending, but at the same time, they may also reduce the funding available to treat the opioid epidemic. To address this concern, I collect information on the partisan composition of the state legislature from the National Conference of State Legislature (NCSL). There are four types of partisan composition, including both legislative chambers have Republican majorities, both legislative chambers have Democratic majorities, neither party has majorities in both legislative chambers (split), and a non-partisan unicameral legislature (Nebraska). Six states change the state legislature at the same time as the change in payday lending law. Georgia, Nevada, and Oklahoma change from Democrat to split. Arizona, Colorado, and Michigan change from Republican to split. To mitigate the concern, I exclude these states from the baseline analysis.

Table 1.9 shows the effects of payday lending on opioid-related mortality rates after excluding states changing the state legislature. I include state and year fixed effects in column (1) and county and year fixed effects in column (2). Both coefficient estimates on *Allowed* are positive and statistically significant at the 1% level. The magnitudes are very similar to the baseline analysis. The results suggest that the significant positive effect of payday lending on opioid-related deaths is less likely to be driven by the state legislature changes, instead driven by states allowing payday lending.

The effects on opioid pill prescriptions

I argue that payday lending access increases opioid-related mortality, which implies that access to payday lending should increase access to opioid drugs. To examine whether this is indeed the case, I focus on the county-level opioid pill prescriptions as an alternative dependent variable. The Washington Post provides the Drug Enforcement Administration (DEA) pain pill database to promote a deeper understanding of the opioid crisis. The data includes all transactions of oxycodone and hydrocodone pills shipped from manufacturers to pharmacies in the United States between 2006 to 2012. It does not include all types of opioid pills because other types of opioids are of low quantities and rarely prescribed in recent years. The data does not include pills that do not go directly to the patients; instead, they go to other places such as the black market. Using this data, I estimate the following regression specification:

$$OpioidPills_{i,j,t} = \beta Allowed_{i,t} + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_i \left(or\ \alpha_j\right) + \alpha_t + \epsilon_{i,j,t}, \qquad (1.5)$$

where the controls and fixed effects are the same as the baseline analysis.

Table 1.10 shows the effects of payday lending on opioid pill prescriptions. In column (1), I regress payday lending access on the total number of opioid pills shipped to pharmacies in a county each year scaled by the number of transactions with state and year fixed effects. The coefficient estimate on *Allowed* is positive and statistically significant. The results suggest that payday lending access increases opioid pill prescriptions. In column (2), I include the county and year fixed effects instead, and the results remain similar, with a slightly lower magnitude. In columns (3) and (4), I scale the total number of opioid pills with the county population. The results remain consistent and robust. The results suggest that increased access to opioid drugs could be one of the possible channels through which the increased payday lending access could affect opioid-related mortality.

Natural logarithm transformation of the crude mortality rates

The distribution of crude opioid-related mortality rates is highly skewed in our data. To mitigate any model fitness concern, I transform the crude mortality rates into a natural logarithm and examine the effect.

Table 1.11 presents the effects of payday lending on opioid-related mortality in the natural logarithm. In column (1), I include the state and year fixed effects and the same set of controls as the baseline analysis. The coefficient estimate on *Allowed* remains positive and statistically significant. Payday lending access increases opioid-related mortality by almost 11%. In column (2), I instead include county and year fixed effects. The coefficient estimate is still positive and statistically significant. The magnitude of the estimate on *Allowed* is slightly lower than that in column (1).

The effects of payday lending on each category of opioid-related mortality

To further pin down the effects of payday lending on opioid-related mortality, I then examine the effects on each subcategory of opioid-related mortality. In this analysis, I focus on the deaths from unintentional drug overdose (ICD-10 code of X40-44), suicide by drug self-poisoning (ICD-10 code of X60-64), and undetermined intention (ICD-10 code of Y10-14). These are typical causes of drug overdose death. There are some data limitations in estimating these effects. The opioid-related mortality, the key dependent variable in the baseline analysis, is the death from all these three subcategories. There are many counties suppressed because of fewer than ten deaths. When breaking it down to these finer categories, there are more counties marked as suppressed in the CDC WONDER database because of confidentiality concerns. Consistent with the baseline analysis, I again replace these suppressed counties with zero to keep them in the analysis.

Table 1.12 presents the effects of payday lending access on each subcategory of opioid-related mortality. The dependent variable in columns (1) and (2) is the unintentional overdose mortality rates. In column (1), I include the state and year fixed effects and the same set of controls as the main analysis. The coefficient estimate on *Allowed* is positive and statistically significant at 5% level. Payday lending access increases the unintentional overdose by about 1.2. In column (2), I instead include county and year fixed effects. The coefficient estimate is still positive and statistically significant. The magnitude of the estimate on *Allowed* is slightly lower than that in column (1). In columns (3) and (4), the cause of death is suicide by drug self-poisoning. I include the same set of controls and fixed effects as in columns (1) and (2). The coefficient estimate on *Allowed* is insignificant with a very small magnitude. In columns (5) and (6), I focused on the undetermined intent cause of deaths. The coefficient estimate on *Allowed* is positive and statistically significant at the 10% level. Overall, the results suggest that payday lending access increases opioid-related mortality mainly through increasing mortality from unintentional or undetermined intent drug overdoses.

Removing the suppressed counties

One of the data limitations is that opioid-related mortality rates are suppressed when a county has fewer than ten deaths because of confidentiality concerns. To overcome this data limitation, I replace those counties with zero mortality rates in the baseline analysis. To ensure that the baseline results are not driven by this imputation, I exclude those suppressed counties as a robustness check.

Table 1.13 shows the results of this robustness check. Column (1) includes state and year fixed effects. The coefficient estimate on *Allowed* is still positive and statistically significant. The magnitude is much larger than that in the baseline analysis. Payday lending access increases opioid-related mortality by about 3.3, suggesting an even stronger effect. Column (2) includes the county and year fixed effects, and the results remain robust.

Including homicide by drug poisoning

The literature typically includes the homicide by drug poisoning deaths in defining the drug overdose death. I exclude this category in the baseline analysis because it is involuntary. To ensure that this exclusion does not drive the results, I include this category in opioid-related mortality as a robustness check.

Table 1.14 presents the results. Column (1) includes state and year fixed effects, and column (2) includes county and year fixed effects. The coefficient estimates on *Allowed* are still positive and statistically significant. The magnitude is very similar to those in the main analysis.

Conclusion

The opioid crisis is drawing more and more concerns from both health practitioners and policymakers to combat. It is very crucial to understand the cause of the crisis from multiple facets. Access to opioid drugs is often very costly. It is important to examine the effects from the finance perspective. The dramatic increase in the payday loans industry provides access to expensive credits. Using the variation in payday lending access generated by state statutes allowing or prohibiting payday lending, I establish a causal link between payday lending access and opioid-related mortality. I find the increased access to payday lending significantly increases opioid-related mortality rates. The magnitude is sizable that allowing payday lending increases opioid-related mortality rates by about 1.5 lives lost per 100,000 population every year. This increase is statistically and economically significant, amounting to about one third of the mean. The effect is less likely to be driven by pre-existing time trends and systematic differences between the allowing and not allowing states or other confounding events. The effect is more pronounced in states with lower socioeconomic status. The results suggest that access to expensive credits affects the opioid epidemic. This finding provides new insights into combating the crisis more efficiently from the finance perspective.

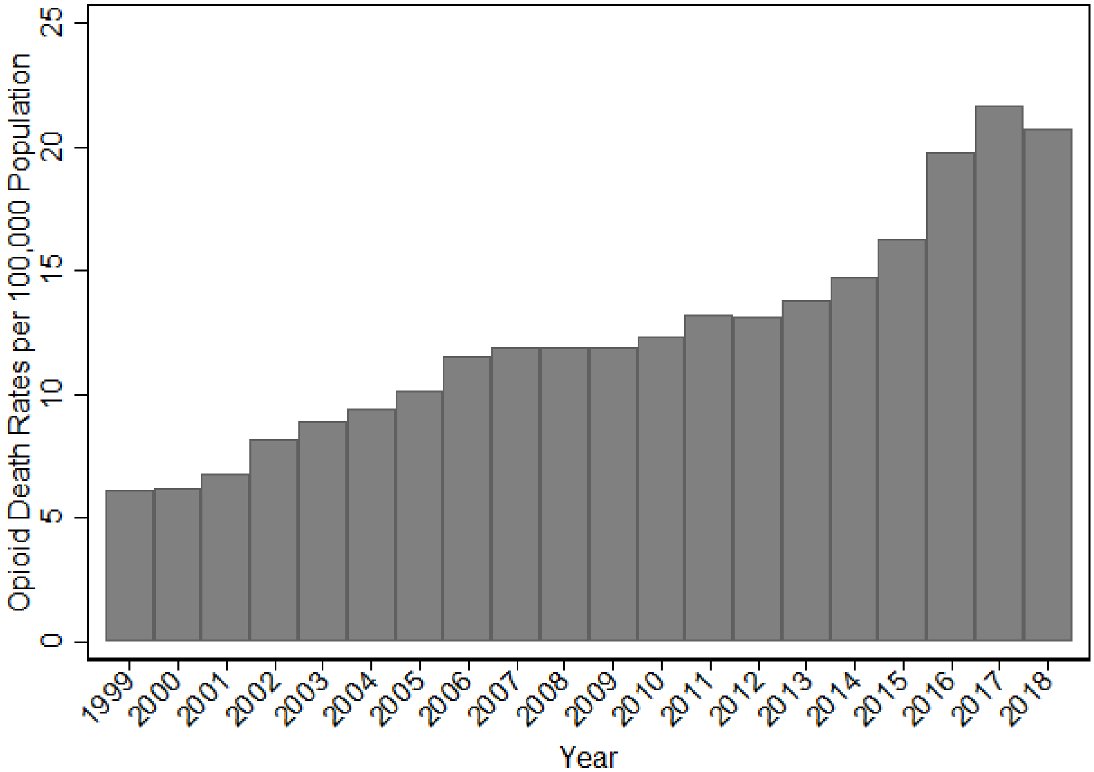

Figure 1.1 Opioid-related mortality over time

This figure presents the opioid-related mortality over time. The mortality rate is in the 100,000 population. Data source: https://www.cdc.gov/nchs/data/databriefs/db356_tables-508.pdf#1.

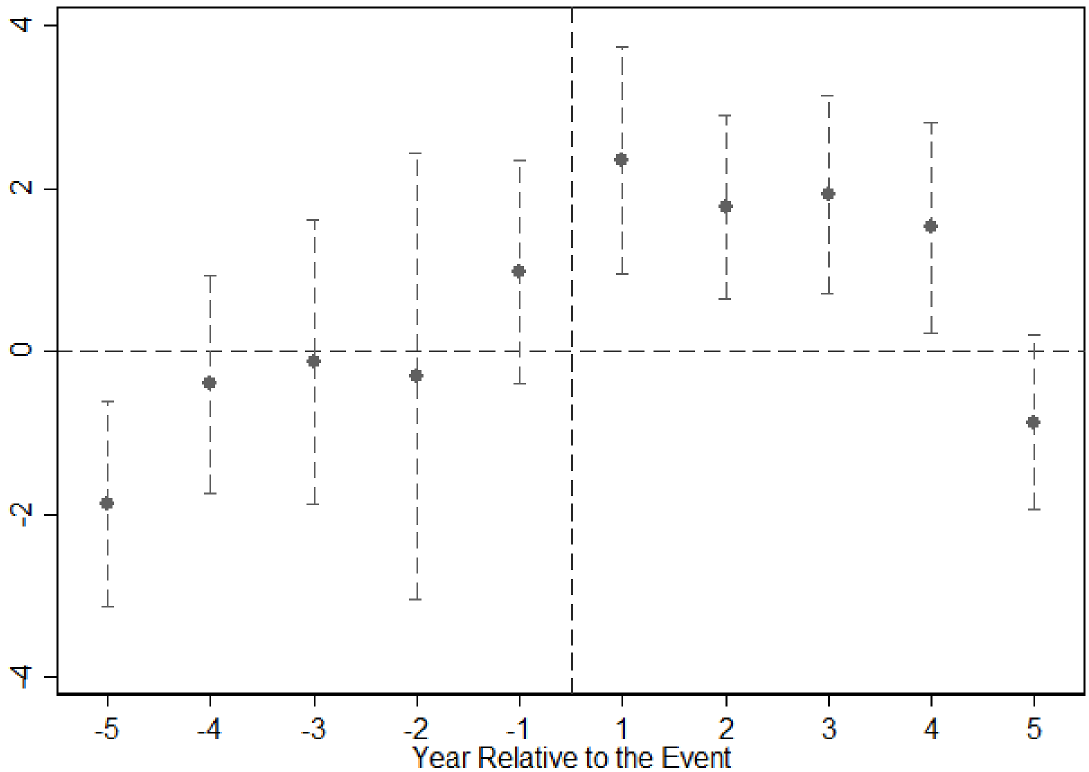

Figure 1.2 Dynamic effects

The figure presents the coefficient estimates of β_k and their confidence intervals from the following regression specification:

$$OpioidDeathRate_{i,j,t} = \sum_{k=-5}^{5} \beta_k D(Allowed_{i,k}) + \delta' X_{j,t} + \theta' Z_{i,t} + \alpha_j + \alpha_t + \epsilon_{i,j,t},$$

where the $D(Allowed_{i,k})$ equals to one if state *i* is -*k* years before or *k* years after the state allows payday lending, and zero otherwise. $X_{j,t}$ are the county-level controls. $Z_{i,t}$ are the state-level controls. α_j and α_t are the county and year fixed effects, respectively. The controls and fixed effects are the same as those in Table 1.4 column (6).

Table 1.1 The payday lending law changes over time

State Name	Year Allowed	Year Prohibit	As of Now
Alabama	1994		Legal
Alaska	2004		Legal
Arizona	2000	2010	Prohibited
Arkansas	1999	2009	Prohibited
California	1997		Legal
Colorado	2000		Legal
Connecticut	--		Prohibited
Delaware	1987		Legal
District of Columbia	1998	2007	Prohibited
Florida	1990		Legal
Georgia	2001	2004	Prohibited
Hawaii	1999		Legal
Idaho	2001		Legal
Illinois	2000		Legal
Indiana	1990		Legal
Iowa	1998		Legal
Kansas	1991		Legal
Kentucky	2009		Legal
Louisiana	1990		Legal
Maine	2000		Legal
Maryland	--		Prohibited
Massachusetts	--		Prohibited
Michigan	2005		Legal
Minnesota	1995		Legal
Mississippi	1998		Legal
Missouri	2002		Legal
Montana	1999		Legal
Nebraska	1993		Legal
Nevada	1984		Legal
New Hampshire	2003		Legal
New Jersey	--		Prohibited
New Mexico	1916	2018	Prohibited
New York	--		Prohibited
North Carolina	1997	2001	Prohibited
North Dakota	2001		Legal
Ohio	1995		Legal
Oklahoma	2003		Legal
Oregon	1998		Legal
Pennsylvania	--		Prohibited
Rhode Island	2001		Legal
South Carolina	1998		Legal
South Dakota	1990		Legal
Tennessee	1990		Legal

Texas	2001	Legal
Utah	1999	Legal
Vermont	--	Prohibited
Virginia	2002	Legal
Washington	1995	Legal
West Virginia	--	Prohibited
Wisconsin	2010	Legal
Wyoming	1996	Legal

Table 1.2 Variable definition

Variables	Definition
Dependent Variable	
Opioid Death Rate	Crude opioid-related death rates per 100,000 residents at county and year level. Opioid-related deaths are defined based on either the ICD-10 underlying cause-of-death codes X40–44 (unintentional), X60–64 (suicide), or Y10–Y14 (undetermined intent). (CDC WONDER database)
Opioid Pills Prescriptions	Total number of opioid pills prescribed and shipped to a county scaled by the number of transactions or population. Only shipments of oxycodone and hydrocodone pills are available in the dataset. Other opioid pills are not included in the dataset because of low quantities. (The Washington Post)
Payday Lending Access	
Allowed	Dummy variable equals one if the state allows payday lending in a year, and zero otherwise.
County-Level Controls	
Ln(No. of Jobs)	Natural logarithm of the number of jobs offering +1 in a county. (U.S Census Bureau)
Ln(Population)	Natural logarithm of total population +1 in a county. (U.S Census Bureau)
State-Level Controls	
Ln(Median Income)	Natural logarithm of median household income+1. (Federal Reserve Bank of St Louis)
Ln(GDP Per Capita)	Natural logarithm of state GDP per capita+1. (Federal Reserve Bank of St Louis)
Unemployed	Percentage of the labor force that is jobless. (Federal Reserve Bank of St Louis)
Poverty	The ratio of the number of people in each age group whose income falls below the poverty line. (Federal Reserve Bank of St Louis)
African American	The proportion of the African American population in a State. (U.S Census Bureau)

Table 1.3 Summary statistics

	Allowing States						Not Allowing States					
	N	**Mean**	**SD**	**p25**	**p50**	**p75**	**N**	**Mean**	**SD**	**p25**	**p50**	**p75**
Dependent Variable												
Crude Opioid Death Rate	36496	4.160	9.702	0.000	0.000	0.000	16986	5.364	11.136	0.000	0.000	8.200
County-Level												
Ln(No. of Jobs)	36496	9.275	2.011	8.440	9.287	10.233	16986	9.597	1.926	8.526	9.549	10.669
Ln(Population)	36496	10.148	1.506	9.266	10.072	10.957	16986	10.439	1.549	9.431	10.335	11.397
State-Level												
Ln(Median Income)	36496	10.777	0.492	10.662	10.787	10.929	16986	10.798	0.173	10.677	10.785	10.918
Ln(GDP Per Capita)	36496	10.707	0.206	10.569	10.690	10.847	16986	10.760	0.213	10.653	10.769	10.902
Unemployed	36496	5.764	2.324	4.200	5.300	6.800	16986	5.902	1.788	4.700	5.400	6.900
Poverty	36496	0.130	0.032	0.106	0.126	0.150	16986	0.143	0.029	0.125	0.145	0.167
African American	36496	0.121	0.096	0.047	0.093	0.163	16986	0.156	0.087	0.082	0.129	0.222

Table 1.4 The effects of payday lending access on opioid-related mortality rates

This table presents coefficient estimates from regressions of opioid-related mortality rates on payday lending access (*Allowed)* and controls from 2002 to 2018. The dependent variables are the crude opioid-related mortality rates throughout all the regression specifications. Columns (1) and (4) do not include any control variables. Columns (2) and (5) include country-level control variables. Columns (3) and (6) additionally include state-level control variables. All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Allowed	1.468***	1.498***	1.479***	1.468***	1.468***	1.453***
	(3.16)	(3.21)	(2.97)	(3.16)	(3.17)	(2.93)
Ln(No. of Jobs)		0.325**	0.325**		-0.108	-0.085
		(2.58)	(2.58)		(-0.52)	(-0.42)
Ln(Population)		2.485***	2.485***		0.390	0.380
		(6.84)	(6.82)		(1.00)	(0.99)
Ln(Median Income)			0.303***			0.307***
			(3.42)			(3.49)
Ln(GDP Per Capita)			1.829			1.593
			(0.40)			(0.35)
Unemployed			0.047			0.047
			(0.42)			(0.42)
Poverty			20.874			20.699
			(1.55)			(1.57)
African American			-19.594			-17.798
			(-0.29)			(-0.26)
Constants	3.558***	-24.959***	-48.327	3.558***	0.575	-20.626
	(11.23)	(-7.51)	(-0.88)	(11.23)	(0.13)	(-0.39)
Adj R^2	0.170	0.294	0.295	0.552	0.552	0.553
N	53482	53482	53482	53482	53482	53482
State Fixed Effects	Yes	Yes	Yes	No	No	No
County Fixed Effects	No	No	No	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.5 The results of falsification tests

This table presents coefficient estimates from regressions of the leading causes of mortality rates in the United States on payday lending access (*Allowed)* and controls from 2002 to 2018. The dependent variables are crude accidents mortality rates in column (1), heart disease mortality rates in column (2), respiratory disease mortality rates in column (3), chronic obstructive lung disease (Lung) mortality rates in column (4), Pneumonia and Influenza (Flu) mortality rates in column (5), Alzheimer's disease mortality rates in column (6), stroke mortality rates in column (7), and diabetes mortality rates in column (8). Controls are the same as those specified in Table 1.4 columns (3) and (6). All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
	Accidents	**Heart**	**Respiratory**	**Lung**	**Flu**	**Alzheimer's**	**Stroke**	**Diabetes**
Allowed	0.230	2.590	3.493	2.175	0.287	-0.144	1.065	0.046
	(0.98)	(0.73)	(1.24)	(1.19)	(1.13)	(-0.25)	(1.25)	(0.10)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.575	0.720	0.673	0.664	0.569	0.595	0.645	0.637
N	53482	53482	53482	53482	53482	53482	53482	53482
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.6 The effects by GDP per capita

This table presents coefficient estimates from regressions of opioid-related mortality rates on payday lending access (*Allowed)* and controls by state's GDP per capita from 2002 to 2018. The dependent variables are the crude opioid-related mortality rates throughout all the regression specifications. High group includes states whose GDP per capita is in the top terciles in year *t* and Low group otherwise. Columns (1) and (2) include both state and year fixed effect, and columns (3) and (4) include both county and year fixed effects. Controls are the same as those specified in Table 1.4 columns (3) and (6). All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1) **Low**	**(2)** **High**	**(3)** **Low**	**(4)** **High**
Allowed	2.200***	-0.381	2.178***	-0.374
	(5.39)	(-0.85)	(6.13)	(-0.88)
County controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes
Adj R^2	0.292	0.377	0.542	0.611
N	37385	16097	37385	15959
Fixed Effects	State and Year	State and Year	County and Year	County and Year
Chi^2 test	Column (1) vs. (2)		Column (3) vs. (4)	
Chi^2-statistics	21.19		25.21	
Prob> Chi^2	<0.001		<0.001	

Table 1.7 The effects by median household income

This table presents coefficient estimates from regressions of opioid-related mortality rates on payday lending access (*Allowed)* and controls by state's median household income from 2002 to 2018. The dependent variables are the crude opioid-related mortality rates throughout all the regression specifications. The high group includes states whose median household income is in the top terciles in year *t* and Low group otherwise. Columns (1) and (2) include both state and year fixed effect, and columns (3) and (4) include both county and year fixed effects. Controls are the same as those specified in Table 1.4 columns (3) and (6). All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	**(2)**	**(3)**	**(4)**
	Low	**High**	**Low**	**High**
Allowed	2.055***	-0.419	2.019***	-0.436
	(4.82)	(-0.90)	(5.37)	(-0.94)
County controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes
Adj R^2	0.277	0.381	0.539	0.600
N	36309	17173	36309	16883
Fixed Effects	State and Year	State and Year	County and Year	County and Year
Chi^2 test	Column (1) vs. (2)		Column (3) vs. (4)	
Chi^2-statistics	14.24		15.93	
Prob> Chi^2	<0.001		<0.001	

Table 1.8 Neighbor states' payday lending

This table presents coefficient estimates from the following regression from 2002 to 2018.

$$OpioidDeathRate_{i,j,t} = \gamma Border_{j,t} + \delta_b' X_{j,t} + \alpha_{it} + \epsilon_{i,j,t},$$

where $Border_{j,t}$ equals one if county *j* is in a not allowing state but adjacent to an allowing state in year *t*, and zero otherwise. α_{it} is the state by year fixed effects, and α_i is the state fixed effects. For the dependent variables, we consider the crude opioid-related mortality rates. All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the county level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Variables	**(1)**
Border	0.908**
	(2.01)
Ln(Employment)	0.329***
	(3.50)
Ln(Population)	2.491***
	(16.20)
Constants	-24.109***
	(-23.09)
Adj R^2	0.347
N	53482
Fixed Effects	State*Year

Table 1.9 State legislature

This table presents coefficient estimates from regressions of opioid-related mortality rates on payday lending access (*Allowed)* and controls from 2002 to 2018 by excluding states that change the state legislature. The excluding states are Arizona, Colorado, Michigan, Georgia, Nevada, and Oklahoma. The dependent variables are the crude opioid-related mortality rates throughout all the regression specifications. Controls are the same as those specified in Table 1.4 columns (3) and (6). All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Variables	**(1)**	**(2)**
Allowed	1.579***	1.570***
	(2.77)	(2.79)
County controls	Yes	Yes
State controls	Yes	Yes
Adj R^2	0.290	0.552
N	46427	46427
Fixed Effects	State and Year	County and Year

Table 1.10 Opioid pill prescriptions

This table presents coefficient estimates from regressions of opioid pill distribution on payday lending access (*Allowed)* from 2006 to 2012. For the dependent variables, we consider the oxycodone and hydrocodone pills distributed to a county every year. The dependent variable in columns (1) and (2) is the total opioid pills divided by the number of transactions shipped to a county in year *t*. The dependent variable in columns (3) and (4) is the total opioid pills divided by the county population in year *t*. Controls are the same as those specified in Table 1.4 columns (3) and (6). All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	**(2)**	**(3)**	**(4)**
Dep. Var.	***Total pills/ transactions***	***Total pills/ transactions***	***Total pills/ population***	***Total pills/ population***
Allowed	0.068***	0.062**	0.026*	0.026*
	(2.86)	(2.48)	(1.71)	(1.84)
County controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes
Adj R^2	0.273	0.718	0.266	0.891
N	18161	18148	18161	18148
Fixed Effects	State and Year	County and Year	State and Year	County and Year

Table 1.11 Natural logarithm of the opioid-related mortality rates

This table presents coefficient estimates from regressions of opioid-related mortality rates in natural logarithm on payday lending access (*Allowed)* and controls from 2002 to 2018. The dependent variables are the crude opioid-related mortality rates in the natural logarithm throughout all the regression specifications. All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Variables	**(1)**	**(2)**
Allowed	0.108***	0.104**
	(2.69)	(2.58)
County controls	Yes	Yes
State controls	Yes	Yes
Adj R^2	0.457	0.702
N	53482	53482
Fixed Effects	State and Year	County and Year

Table 1.12 Subcategory of opioid-related mortality

This table presents coefficient estimates from regressions of each category of opioid-related mortality rates on payday lending access (*Allowed)* and controls from 2002 to 2018. The dependent variables in columns (1) and (2) are mortality rates from unintentional overdose (ICD-10 category: X40-44); columns (3) and (4) are mortality rates from suicide by drug self-poisoning (ICD-10 category: X60-64); and columns (5) and (6) are mortality rates from undetermined intent (ICD-10 category: Y10-14). Columns (1), (3), and (5) include state and year fixed effects. Columns (2), (4), and (6) include county and year fixed effects. All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Dep. Var.	**(1) Unintentional overdose**	**(2) Unintentional overdose**	**(3) Suicide by drug self-poisoning**	**(4) Suicide by drug self-poisoning**	**(5) Undetermined intent**	**(6) Undetermined intent**
Allowed	1.170**	1.152**	-0.001	-0.002	0.082*	0.081*
	(2.46)	(2.43)	(-0.09)	(-0.19)	(1.73)	(1.72)
County controls	Yes	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.260	0.503	0.175	0.561	0.215	0.537
N	53482	53482	53482	53482	53482	53482
Fixed Effects	State and Year	County and Year	State and Year	County and Year	State and Year	County and Year

Table 1.13 Excluding the suppressed counties

This table presents coefficient estimates from regressions of opioid-related mortality rates on payday lending access (*Allowed)* and controls from 2002 to 2018. The dependent variables are the crude opioid-related mortality rates throughout all the regression specifications. All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Variables	**(1)**	**(2)**
Allowed	3.296***	2.657***
	(3.25)	(3.28)
County controls	Yes	Yes
State controls	Yes	Yes
Adj R^2	0.449	0.732
N	12707	12551
Fixed Effects	State and Year	County and Year

Table 1.14 Including homicide by drug poisoning

This table presents coefficient estimates from regressions of opioid-related mortality rates on payday lending access (*Allowed)* and controls from 2002 to 2018. The dependent variables are the crude opioid-related mortality rates including the homicide by drug poisoning (ICD-10 code of X85) throughout all the regression specifications. All variables are defined in Table 1.2. *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Variables	**(1)**	**(2)**
Allowed	1.479***	1.453***
	(2.97)	(2.93)
County controls	Yes	Yes
State controls	Yes	Yes
Adj R^2	0.295	0.553
N	53482	53482
Fixed Effects	State and Year	County and Year

Chapter 2. Managerial Sentiment and Corporate Liquidity Hoarding: Evidence from the Special Case of Banking [2]

Abstract

We analyze how managerial sentiment embedded in accounting statements affects corporate liquidity hoarding. We choose banks as our empirical setting due to superior detailed accounting data, a comprehensive research-based liquidity hoarding measure, and avoidance of interindustry differences in liquidity needs. We derive managerial sentiment from negative and positive tones in annual reports (10-K) language. We find more negative managerial sentiment results in more liquidity hoarding, consistent with our hypothesis. Further analysis confirms that findings incorporate bank volition rather than being driven only by customers. We also address endogeneity using exogenous weather conditions as instruments. We finally derive potential policy implications.

Introduction

The sentiment of economic agents is a powerful force in both the real economy and financial system. Managerial sentiment, broadly defined as a belief about future business conditions that is not justified by present information, crucially impacts corporate decisions. Almost a century ago, Keynes (1936) argued that corporate investment and other key economic decisions are greatly influenced by “animal spirits.” More recent literature

[2] Ma, X., A. N. Berger, and H. H. Kim. To be submitted to *Contemporary Accounting Research*.

finds that managerial sentiment plays critical roles in capital allocations (e.g., Ben-David, Graham, and Harvey 2013; Graham, Harvey, and Puri 2015) and financial accounting practices, such as earnings and management disclosures (Bergman and Roychowdhury 2008; Chen, Wu, and Zhang 2020; Brown, Christensen, Elliott, and Mergenthaler 2012). Managerial sentiment also affects stock price sensitivity to earnings disclosures (Mian and Sankaraguruswamy 2012) and banks' decisions on loan loss provisions and charge-offs (Hribar, Melessa, Small, and Wilde 2017). The sentiment of managers is also found to influence the accuracy of analysts' forecasts (Hribar and McInnis 2012, Walther and Willis 2013), firms' commitments to corporate social responsibility (Naughton, Wang, and Yeung 2019), and peer firms' investments (Beatty, Liao, and Yu 2013).

In this paper, we investigate whether managerial sentiment significantly affects corporate liquidity management. The management of liquidity shortfalls is crucial to a firm's ability to fund investments, take advantage of unexpected business opportunities, and avoid "fire sale" jettisons of valuable assets in the event of financial distress. It is also important to avoid hoarding liquidity at unnecessarily high levels that may also result in underinvestment, unexploited opportunities, and low returns on assets. Corporations entrust the key job of balancing these concerns and managing liquidity to their CFOs (e.g., Graham and Harvey 2001). Since at least the early 2000s, U.S. firms have increased cash holdings by large magnitudes, raising the possibility of liquidity hoarding to a serious degree of concern (e.g., Bates, Kahle, and Stulz 2009).

We hypothesize that more pessimistic managers hoard liquidity beyond what is warranted by firm fundamentals and market conditions. This hypothesis of sentiment-driven liquidity hoarding is based on studies in the psychology and behavioral finance literature (e.g., Rick and Loewenstein 2008; Lerner, Li, Valdesolo, and Kassam 2015). This managerial sentiment effect complements the existing literature's focus on the role of financial constraints in determining corporate liquidity (see Almeida, Campello, Cunha, and Weisbach 2014 for a literature review).

To the best of our knowledge, we are the first to investigate the link between managerial sentiment and corporate liquidity hoarding. Although it is not possible to know with any certainty why this important research topic remains underdeveloped, we point to three major challenges that may help explain this lack of prior research. These are 1) difficulties in gathering comparable data on all the sources and uses of liquid funds, 2) lack of a research-grounded measure into which to combine such data, and 3) interindustry differences in liquidity needs that are challenging to either incorporate into a liquidity hoarding measure or find adequate controls in an econometric setting. We offer a potential solution to these challenges. Specifically, we show how studying the effects of managerial sentiment on liquidity hoarding by commercial banking corporations solves these problems, provides some additional advantages, and yields empirical evidence that strongly supports our hypothesis.

The first challenge of finding comparable data on all the sources and uses of liquid funds is difficult to meet with standard publicly available financial statements. The scope of modern liquidity management is broad and stretches well beyond simple cash management. It covers all of the sources and uses of liquid funds on the asset and liability sides of the balance sheet as well as off the balance sheet. For example, the firm must manage, among other things, cash and other liquid assets, short-term debt and other liquid liabilities, and loan commitments, letters of credit, and other sources of liquid funds off the balance sheet. Publicly available financial statements of corporations may be inadequate to the task of capturing all of the key liquidity sources and uses of funds needed to compile accurate liquidity measures (e.g., Emery and Cogger 1982; Almeida et al. 2014).

Second, even if all relevant detailed asset, liability, and off-balance sheet information items were gathered, they must be combined into a meaningful measure of corporate liquidity hoarding that is backed by theoretical and empirical research. It is challenging to find well-accepted research that provides guidance on aggregating these data into a single firmly-grounded measure of corporate liquidity hoarding.

Third, firms in different industries may have very different liquidity needs. The literature provides many sources of these varying needs across industries, such as cash flow volatility (Opler, Pinkowitz, Stulz, and Williamson 1999), R&D expenditures (Bates, Kahle, and Stulz 2009), borrowing capacities (Acharya, Almeida, and Campello 2007), and M&A activities (Harford 1999). Many of these differences are difficult, if not impossible, to either incorporate into a comparable liquidity hoarding measure or to control for adequately in an econometric setting.

Studying the commercial banking industry overcomes these three challenges and provides an excellent setting to address whether managerial sentiment affects corporate liquidity hoarding. Starting with the data challenge, the public disclosures of the financial information of U.S. banks are unparalleled. U.S. banks file quarterly Reports of Condition and Income called “Call Reports” with their regulators. These reports provide granular information on virtually all bank asset, liability, and off-balance activity categories in sufficient detail that allows differentiation in the liquidity of otherwise similar items, such as transactions deposits and time deposits. We focus on publicly traded banking corporations as our experimental setting with rich data.

Turning to the second challenge of research-driven combining of the accounting information into a meaningful measure of liquidity hoarding, the banking literature has a rich history of theoretical and empirical research on which to draw. Banking theory includes contributions on the role of cash and liquid assets, lending, deposits, and off-balance sheet loan commitments to bank liquidity (e.g., Kashyap, Rajan, and Stein 2002; Diamond and Rajan 2011; Acharya and Skeie 2011; Acharya and Merrouche 2013). Empirical banking studies use these items in various combinations to measure bank liquidity holding (e.g., Cornett, McNutt, Strahan, and Tehranian 2011; Acharya and Mora 2015; Bordo, Duca, and Koch 2016; Gissler, Oldfather, and Ruffino 2016). We incorporate a recently designed and publicly available measure of bank liquidity hoarding by Berger, Guedhami, Kim, and Li (forthcoming) that embodies all of these components from the individual asset, liability, and off-balance sheet categories on the Call Reports. We describe this comprehensive liquidity hoarding measure in detail in Section II.

Finally, focusing on the commercial banking industry clearly mitigates the issue of interindustry differences in liquidity needs. Publicly traded banking organizations are relatively homogeneous in their operations, and we can readily control for any remaining key differences influencing their liquidity sources and usages.

We point to two additional desirable features of our dataset. First, banks are important sources of liquidity to other firms that help fuel the real economy and keep the financial system functioning. Thus, understanding whether managerial sentiment has important effects on their liquidity hoarding is an important topic on its own (Acharya, Almeida, Ippolito, and Perez-Orive 2014; Holmstrom and Tirole 1998). Second, the 10-K financial accounting disclosures that these publicly traded banking corporations file with the SEC provide a valuable source for our measure of managerial sentiment. As discussed in Section II, our measure of managerial sentiment is based on the negative and positive tones of the words in these disclosures following the current practice in the literature (e.g., Loughran and McDonald 2011; Jiang, Lee, Martin, and Zhou 2019). The sentiment embedded in 10-Ks is likely to reflect top corporate executives' team-level emotions as they are required to carefully review these documents by the Sarbanes-Oxley Act of 2002.

By way of preview, we find statistically and economically significant evidence supporting our hypothesis that banks with more negative manager sentiment hoard additional liquidity above the level warranted by the banks' fundamentals. Our results hold after controlling for key characteristics of banks and other sentiment measures for investors, consumers, and corporate managers of non-financial firms. Our findings are more pronounced for highly capitalized banks and during and especially after the Global Financial Crisis. The result is particularly strong for banking organizations with investment banking operations. We also provide evidence that the negative sentiment leads banks to hoard liquidity more than a benchmark level rationalized by their fundamentals and market conditions.

We are keenly aware of and address potential identification challenges. Observed bank liquidity hoarding could be driven in part by customer demands for and supplies of liquidity as well as bank actions. We deal with this concern by analyzing the determinants of interest rate spreads on bank loans and credit lines using DealScan data, as well as spreads on deposits using RateWatch data. Loans, credit lines, and deposits are key elements of asset-side, off-balance sheet-side, and liability-side liquidity hoarding, respectively. While we cannot precisely separate bank supplies and demands of liquidity from those of customers, the directions of the spread movements effectively rule out the possibility that customer supplies and demands entirely explain our main results.

We also deal with potential endogeneity concerns based on our model specification and an instrumental variable (IV) application. To substantiate our hypothesis, we need to address the question of whether management sentiment captures behavioral bias or fundamental information. The model specification includes a rich set of controls for liquidity demand and supply to minimize the incidence of omitted variables issues. We also lag our sentiment measure and the controls to mitigate possible reverse causality problems. Importantly, the IV application uses exogenous local weather conditions in the vicinity of bank headquarters to instrument for negative bank management sentiment. Weather is exogenously determined and is found to affect human sentiment (e.g., Lerner et al., 2015). To find the best instruments from a large number of weather conditions, we implement the least absolute shrinkage and selection operator (LASSO) of Belloni, Chernozhukov, and Hansen (2011). A potential concern is that these weather conditions may also affect the sentiment of bank customers. Because our sample includes only publicly-traded banking organizations that often have broad geographical footprints, local weather conditions in the vicinity of bank headquarters are less likely to affect their customers' demands and supplies for banking services. In a robustness check, we verify that our results hold in a subsample of only banks that operate in multiple states.

In our empirical analysis, we include annual sentiment data from all 837 publicly-traded U.S. banks and bank holding companies (BHCs) from 1993 to 2016, a total of 7,770 unique annual 10-K files. We calculate liquidity hoarding for 57,841 quarterly bank observations from 1993:Q4 to 2016:Q4 for our regression analysis. Our analyses for the impact of bank sentiment on loan and credit line pricing are based on over 12,000 individual term loans and over 36,000 revolving lines of credit from the DealScan database, as well as information on the corporate borrowers using Compustat. Our deposit spread analysis employs almost 400,000 observations from the RateWatch database. The LASSO instrumental variable technique is based on combinations of 2,090 instruments created from 144 different weather conditions in the vicinity of bank headquarters.

This paper contributes to three specific research literatures. The first is the broad literature on the role of sentiment in the accounting and finance literature. There is extensive research on the impact of investor sentiment on asset prices (e.g., Baker and Wurgler 2006). As mentioned above, the literature has also studied earnings forecasts of managers, accrual estimates, and stock price sensitivity to good earnings disclosures. Studies also find that investor sentiment affects analyst forecast accuracy, firms' commitment to corporate social responsibility, and peers' investments. Our paper contributes to the literature by providing additional analysis on the role of managerial sentiment on corporate liquidity hoarding, focusing on the banking industry.

Second, we contribute to a growing literature on understanding informational content embedded in banks' textual disclosures. Many accounting and finance studies focus on banks' timely loan loss provisions to investigate the role of banks' transparency (Beatty and Liao 2011; Bushman and Williams 2015). Other studies (e.g., Hanley and Hoberg 2019; Correa, Garud, Londono, and Mislang 2021) turn attention to building predictive measures of early warning signs for future financial crises. Our paper explores the role of bank management sentiment embedded in the disclosure documents. By doing so, we also shed additional light on the informativeness of banks' disclosure documents as actively researched in the accounting literature (see Beatty and Liao 2014 for a literature review).

Finally, we add to the findings on corporate liquidity hoarding. Most liquidity hoarding studies document the impact of observable risk characteristics of firms and banks such as financial constraints (e.g., Almeida et al. 2014; Cornett et al. 2011; Acharya and Mora 2015) or uncertain environment (e.g., Caballero and Krishnamurthy 2008; Allen, Carletti, and Gale 2009) on their excessive demand for liquidity. We add the impact of managerial sentiment on corporate liquidity hoarding for the first time.

Our findings using banking data may or may not apply in whole to the general corporate setting, but we argue that our results constitute the first step towards an understanding of the effects of managerial sentiment on corporate liquidity hoarding. The publicly traded banks in our application are corporate entities, and the managers of these organizations are flesh-and-blood human beings subject to sentiment just as are the managers of other corporations. Although banks may be distinct from nonfinancial firms in *how* they manage their liquidity, we do not see any reason why the *direction* of the effects we find and the support of our hypothesis should not be applicable to the general corporate setting. Our statistically and economically significant and robust empirical findings that negative managerial sentiment increases liquidity hoarding for this key industry arguably provide useful insights on the general role of corporate managers' sentiment on liquidity hoarding.

Data and key variables

Bank sentiment measure derived from textual analysis of annual reports

We construct our bank sentiment measure based on the textual tone of the most recent annual reports (Form 10-K) of publicly traded banks and BHCs from 1993:Q4 to 2016:Q4. Annual reports of public companies provide comprehensive overviews of their business and financial conditions assessed by core management officials. Almost all publicly traded banking organizations are BHCs that own commercial banks, so the sentiment is measured at the BHC level for these organizations and at the bank level for independent traded banks. We refer to it as bank management sentiment in all cases for expositional convenience.

Using the PERMCO – RSSD identifier link provided by the Federal Reserve Bank of New York, we merge the Call Report information with the CRSP and COMPUSTAT datasets. For publicly traded BHCs, we use the RSSD9364 identifier, and for independent public banks, we employ the RSSD9001 identifier. We obtain 7,770 unique 10-K files reported by 837 publicly-traded banks. We derive their tone using Loughran and McDonald (2011)'s dictionary of positive and negative words. We use the fraction of negative words minus positive words relative to total words in the 10-Ks as our measure of bank sentiment:

$$Negative\ bank\ sentiment = \frac{(Negative\ words - Positive\ words)}{Total\ words} \quad (2.1)$$

An ancillary finding that negative and positive words have approximately equal effects on bank liquidity hoarding (see Table 2.13) helps justify using the proportional difference in equation (2.1).

Two other studies of the effects of bank management sentiment employ dummy variables for option-based bank CEO optimism. Bui, Chen, Lin, and Lin (2017) and Huang, Chen, and Chen (2018) investigate if a bank CEO postpones exercising stock options that are more than 100% in the money at least twice during their tenure, and classify the CEO as optimistic from the time of the first delay. This measure originates in the corporate finance literature (e.g., Malmendier and Tate 2005; Campbell, Gallmeyer, Johnson, Rutherford, and Stanley 2011).

We prefer our text-based measure of negative bank management sentiment from annual reports to the option-based CEO optimism dummy for our study for several reasons. First, the exercise or non-exercise of financial options in the CEO's company stock is likely influenced by the CEO's personal wealth position, diversification motives, and risk aversion, in addition to sentiment. In contrast, the 10-K is a professional accounting document without direct links to the managers' financial conditions, and so may more accurately reflect sentiment. Second, our text-based measure may be more representative of bank management as a whole because the 10-K is produced and vetted by the management team, rather than representing only the thinking of the CEO. Annual reports are closely reviewed by management teams and they also need to endorse the accuracy of the disclosure documents, especially after the Sarbanes-Oxley Act of 2002. Third, our measure is continuous, rather than a dummy. Thus, our measure allows for the possibility that stronger sentiment at the margin has greater effects, which we explore in our empirical analysis.

Table 2.2 reports summary statistics for the 57,841 bank-quarter observations from 1993:Q4 through 2016:Q4. *Negative bank sentiment* has a mean of 0.007 and substantial heterogeneity across banks over time, suggesting that sentiment revealed in banks' annual reports has significant bank-specific determinants. In an untabulated analysis, we also find that negative bank sentiment is widely dispersed during the 2008-2009 Global Financial Crisis period, implying that the market-level turmoil did not drive all banks' sentiment with the same magnitude.

Bank liquidity hoarding measures

We describe here the total bank liquidity hoarding measure, *LH(total)*, and its asset, liability, and off-balance sheet components *LH(asset)*, *LH(liab)*, and *LH(off)*. These measures are developed by Berger et al. (forthcoming) based on Berger and Bouwman's (2009) liquidity creation measures. The core rationale for this liquidity hoarding measure is that it includes all the sources of liquid funds as well as their uses. Importantly, this measure incorporates all assets, liabilities, and off-balance sheet activities with weights based on their contribution to liquidity hoarding. Table 2.1 presents a detailed list of these items.[3]

Specifically, total liquidity hoarding, *LH(total)*, is the sum of asset-side, liability-side, and off-balance sheet-side components, *LH(asset)* + *LH(liab)* + *LH(off)*. Asset-side liquidity hoarding, *LH(asset)* is the weighted sum of liquid assets and illiquid assets; *LH(asset)* = (+1/2) × liquid assets + (-1/2) × illiquid assets. Acquiring liquid assets, such as cash and securities, make the bank more liquid, as securities can usually be easily sold for cash. As well, procuring fewer illiquid assets, such as commercial and industrial (C&I) loans, can also free up cash. The magnitudes of 1/2 mean that making $1 fewer C&I loans and storing the funds as $1 of cash increases bank liquidity hoarding by $1. Similarly, liability-side liquidity hoarding *LH(liab)* is the weighted sum of liquid liabilities; *LH(liab)* = (+1/2) × liquid liabilities. The rationale is that banks can also raise liquid assets by

[3] Table 2.1 excludes items classified as semiliquid by Berger and Bouwman (2009), which are generally neutral that neither create nor hoard liquidity.

issuing more liquid liabilities like transaction deposits. Standard liquidity management practices generally are to fund short-term liquid assets with short-term liquid liabilities, which the bank can change quickly by altering deposit interest rates. Additionally, the FDIC's liquidity regulation manual (FDIC, 2020) considers core deposits, including transactions accounts, as low-cost sources of liquidity funding for banks. Off-balance sheet liquidity hoarding, *LH(off)* is the weighted sum of illiquid guarantees and liquid derivatives; *LH(off)* = (-1/2) × illiquid guarantees + (+1/2) × liquid derivatives.[4] Illiquid guarantees, such as loan commitments, can be withdrawn quickly and drain cash, and liquid derivatives, measured by their gross fair values, can be sold to raise cash. In our empirical analyses, the *LH* measures are normalized by gross total assets (GTA) to be comparable across banks and avoid dominance by the largest banks.[5]

In Table 2.2, we observe total bank liquidity hoarding normalized by GTA, *LH(total)/GTA* has a mean of 0.074, suggesting that banks hoard liquidity of 7.4% of GTA on average. The liquidity hoarding measure has a wide dispersion across banks with the 25th and 75th percentile values at -0.050 and 0.193, respectively. Asset-side liquidity hoarding, *LH(asset)/GTA*, has a mean value of -0.080 with the 25th and 75th percentile values at -0.183 and 0.013, respectively. The negative mean value of *LH(asset)/GTA* implies that banks often hold more illiquid assets (e.g., commercial loans) with negative weights than liquid assets (e.g., cash and due from other institutions, securities) with

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[4] In Berger and Bouwman (2009), "net participations acquired" is labeled as liquid guarantees. For expositional convenience, we include "net participation sold," its arithmetic inverse, as an item of illiquid guarantees.

[5] Gross total assets (*GTA*) equals total assets (*TA*) plus the allocation for loan and lease losses (*ALLL*), which accounts for expected losses, and the allocated transfer risk reserve (*ATRR*), a reserve for certain troubled foreign loans. *GTA* incorporates the full value of all the assets that are included in the bank liquidity hoarding measures.

positive weights. Mean liability-side liquidity hoarding (*LH(liab)/GTA*) is 0.239. The mean liquidity hoarding off the balance sheet (*LH(off)/GTA*) is -0.084. The negative sign mostly reflects loan commitments, which are illiquid from banks' point of view.

Main results – Extensive margin

To test our hypothesis that *Negative bank sentiment* is associated with more liquidity hoarding, we estimate regressions of the form:

$$\left(\frac{LH}{GTA}\right)_{i,t} = \beta Negative\ bank\ sentiment_{i,t-1} + \delta' X_{i,t-1} + \theta' W_{i,t-1} + \nu'^{S_{t-1}} + \gamma' EPU_{t-1} + \alpha_i + q_t + \epsilon_{i,t}, \tag{2.2}$$

where *i* and *t* index a bank and a calendar quarter, respectively. The dependent variable is one of the normalized liquidity hoarding measures: *LH(total)/GTA*, *LH(asset)/GTA*, *LH(liab)/GTA*, or *LH(off)/GTA*. The key independent variable is the *Negative bank sentiment*, measured from the most recent bank annual report (Form 10-K). To estimate the sentiment-driven liquidity hoarding effects, we control for a host of variables related to banks' fundamentals and market conditions. To mitigate potential reverse-causality concerns, we lag the independent variables. Our bank control variables (*X*) include the natural logarithm of bank gross total assets (*Ln(GTA)*), and its squared (*Sqr.Ln(GTA)*), capital ratio (*Capital ratio*), and the ratio of net income to gross total assets (*Earnings*) to account for bank size, leverage, and earnings. The capital ratio controls for the effects of banks' financial constraints on their liquidity hoarding decisions. We also control for the local market and corporate demand for investment (*W*): bank competition (*HHI*) based on bank deposits, local market size (*Population*), local firms' average value (*Tobin's Q*), and their cash flows (*Cash flows*). Bank customers' *Tobin's Q* and cash flows would represent their investment opportunities and funding abilities, affecting their demands for credits and subsequently influencing the banks' liquidity hoarding behavior. We include market-level sentiment measures (*S*) of corporate manager sentiment (*Corporate sentiment)* by Jiang et al. (2019), investor sentiment (*Investor sentiment*) by Baker and Wurgler (2006), and the consumer sentiment index by the University of Michigan (*Consumer sentiment*).[6] We also control for economic policy uncertainty (*EPU*) measure by Baker, Bloom, and Davis' (2016), which is found to influence banks' liquidity hoarding decisions (Berger, Guedhami, Kim, and Li, forthcoming). The controls for aggregate corporate, investor, and consumer sentiments and well as economic policy uncertainty are quite important so that our main coefficient on *Negative bank sentiment* represents the effects of the sentiment of an individual banking organization's managers beyond these other effects.

Additionally, we include bank fixed effects (α) to control for omitted bank characteristics that are invariant over time, and quarter dummies (q) to account for seasonality of bank liquidity hoarding. Importantly, we do not include time fixed effects in equation (2.2). Time fixed effects would remove the effects of any intertemporal changes in bank managerial sentiment, which we include as an important part of our hypothesis. Time fixed effects would also subsume the other aggregate sentiment and economic policy uncertainty measures and not allow us to cleanly differentiate between bank management sentiment and these other effects. We cluster standard errors at the bank holding company and year-quarter level to account for correlations of error terms.

We obtain bank-specific variables and local market conditions from Call Reports

[6] The corporate manager sentiment data is only available during 2003-2014. To avoid contracting the sample period, we create a dummy variable to indicate whether manager sentiment information is available or not and replace the missing values with the average values of available information. In regression models, we include the manager sentiment measure along with this dummy variable.

and the Federal Reserve Bank of St. Louis, respectively. We compute the economic conditions of banks' potential customers (*Tobin's Q,* and *Cash flows*) based on information from Compustat. Potential customer firms of banks are those in the banks' states of operation. We take the weighted average of these variables for each bank based on the proportion of a bank's deposits in each area (Metropolitan Statistical Area (MSA) or counties). We obtain bank deposit amounts per branch from the Summary of Deposits by FDIC (from 1994 to 2016) and Bouwman's website (from 1985 to 1993). Table 2.11 presents more detailed definitions of all variables used.

Summary statistics for the control variables

Table 2.2 also shows summary statistics for the control variables. The median size of banks *Ln(GTA)* is 13.53, or corresponding to $752 million.[7] *Capital ratio* has a mean of 0.078, and most banks have capital ratios between 0.06 and 0.09. *Earnings* is distributed around 0.011 (median) with an average value of 0.01. The average value of bank competition measure, *HHI* is 0.108. The *Tobin's Q* of firms in the states where a bank has operation is an average value of 2.419, similar to the average value for firms in the full CRSP/Compustat universe (e.g., Bertrand and Schoar 2003). *Cash flows* is widely dispersed across firms in different locations with the 25th percentile at -0.014 and the 75th percentile at 0.017. The average *Corporate sentiment* index is -0.072 with a standard deviation of 1.043. The percentiles of the *Investor sentiment* measure show there is wide variation in investor sentiment; the 25th percentile number of *Investor sentiment* is -0.077 and the 75th percentile number is 0.567. The average index of *Consumer sentiment* is 88.713 during the sample period. The average value for the economic policy uncertainty

[7] All dollar amounts in Table 2.2 are measured in real 2016 dollars.

(*EPU*) is 4.570.

Main regressions of liquidity hoarding on bank sentiment embedded in annual reports

Table 2.3 Panel A presents coefficient estimates from regressions of *LH(total)/GTA* on *Negative bank sentiment* and the controls. In column (1), we control for bank characteristics and bank and seasonal fixed effects. We observe that the estimated coefficient on *Negative bank sentiment* is positive and statistically significant at 1% level. In column (2), we additionally control for local market-level variables and continue to find a positive and statistically significant result. This result suggests that the impact of *Negative bank sentiment* on bank liquidity hoarding is not driven by local market characteristics, such as corporate demand for cash or investment opportunities.

Column (3) also controls for other well-known sentiment measures affecting corporate managers (*Corporate sentiment*), investors (*Investor sentiment*), and consumers (*Consumer sentiment*), as well as *EPU*. Column (3) is our main specification that allows us to measure the contribution of the sentiment of the managers of an individual banking organization beyond the effects of aggregate corporate, investor, or consumer sentiment or economic policy uncertainty. It also incorporates the effects of differences in bank managerial sentiment in both the cross-section of banks and over time for the same bank. The coefficient on *Negative bank sentiment* is still positive and statistically significant at the 1% level. This result suggests that bank sentiment has an incremental impact on their liquidity hoarding behavior beyond corporate-, investor-, and consumer-sentiment and *EPU*. The economic significance of the estimates is also sizable. A one-standard-deviation increase in *Negative bank sentiment* leads to 2.4 percentage points increase in

LH(total)/GTA.[8]

We next address a potential concern that our results could be spuriously driven in part by unobserved aggregate factors that trend over time, driving both bank managerial sentiment and bank liquidity hoarding. While there is no ideal way to deal with this issue, in column (4), we control for a time trend that may capture some unobserved aggregate factors. The estimated coefficient on *Negative bank sentiment* is somewhat reduced, but is still statistically significant at the 1% level. The reduced coefficient may occur because the time trend absorbs some effects of intertemporal changes in bank managerial sentiment over time. In the remainder of our analysis, we use the specification in column (3) without the time trend because our hypothesis includes both effects of cross-sectional and times-series variations in bank managerial sentiment. The results in Table 2.3 Panel A using the total liquidity hoarding measure (*LH(total)/GTA*) provide clear support for our hypothesis.

Table 2.3 Panel B presents coefficients estimates from regressions of bank liquidity hoarding components, *LH(asset)/GTA*, *LH(liab)/GTA*, and *LH(off)/GTA*, on *Negative bank sentiment* using the full preferred specification with all the controls. The estimated coefficients on *Negative bank sentiment* are positive and statistically significant for the asset, liability, and off-balance sheet sides. These results suggest that an increase in the *Negative bank sentiment* leads to an increase in all of the components of bank liquidity hoarding. In terms of economic significance, a one-standard-deviation increase in *Negative bank sentiment* is associated with 0.4 percentage points increase in *LH(asset)/GTA*, 1.7 percentage points increase in *LH(liab)/GTA*, and 0.2 percentage points increase in *LH(off)/GTA*, suggesting that the strongest effects are on the liability side. Collectively, the

[8] This is calculated as the coefficient (4.755)×standard deviation of bank sentiment (0.005) =2.4%.

Table 2.3 results support our main hypothesis – *Negative bank sentiment* increases bank liquidity hoarding.

To better understand the mechanisms behind the main findings, we also regress selected bank balance sheet and off-balance sheet items that make up much of the bank liquidity hoarding measure on *Negative bank sentiment* and again include all the controls and fixed effects. The findings are shown in Table 2.12. We find that when *Negative bank sentiment* increases, banks increase cash holdings, and the results are highly statistically significant. They also decrease loans and loan commitments. When bank managers have more negative sentiment, their banks also hoard more liquidity through increased deposits. This item-by-item analysis reinforces our main findings and suggests that several mechanisms are at work in explaining the findings.

Effects of bank sentiment by bank capital and time period

We next analyze whether our findings differ by bank capital ratios and time periods. In Table 2.4 Panel A, we regress *LH(total)/GTA* and its components on *Negative bank sentiment* and its interaction term with *High capital ratio*, a dummy equal to one if the lagged *Capital ratio* is greater than its 75th percentile for that point in time and zero otherwise. We include all of the controls and fixed effects from the full specification in column (3) of Table 2.3 Panel A.

We recognize that banks with high capital ratios differ in numerous ways from other banks, making the interaction effect difficult to predict. High capital is likely to be associated with better bank health and reduced likelihood of interventions or restrictions imposed by government supervisors. As a result, liquidity hoarding may be less sensitive to bank sentiment, as the management of these healthy, less encumbered banks need to worry less about the market and supervisory dangers of liquidity shortfalls. However, a high capital ratio may also be associated with more conservative or prudentially concerned managers that hold more capital to protect the bank's franchise value or their own employment. More conservative or prudent managers may respond more than other managers to negative sentiment because of a greater fear of illiquidity problems. It is an empirical question as to which of these effects may dominate.

In the first column of Table 2.4 Panel A, the interaction term is positive and highly statistically significant, suggesting that the impact of *Negative bank sentiment* on total liquidity hoarding is greater when a bank's *Capital ratio* is high. This is consistent with the prudent manager prediction, but we refrain from drawing strong causal conclusions because other factors that may affect the results are excluded from the specification. In terms of the economic significance, the result in column (1) (coeff. = 3.837, *t*-statistic = 6.85) suggests that a one-standard-deviation increase in *Negative bank sentiment* for high capital banks is associated with additional 0.8 percentage points increase in the *LH(total)/GTA* compared to low capital banks. This interaction term coefficient also exceeds the linear term coefficient on *Negative bank sentiment*, suggesting that the effects of sentiment on high-capital banks are more than double those on other banks. The interaction terms in columns (2)-(4) are also all positive and statistically significant, suggesting that high-capital banks increase all the components of liquidity hoarding – i.e., the asset-, liability-, and off-balance sheet-sides – more than other banks in response to negative management sentiment.

In Table 2.4 Panel B, we test whether the effects of negative sentiment vary across key time periods. Specifically, we examine the differences in the effects of bank sentiment on liquidity hoarding among the pre-crisis, Global Financial Crisis, and post-crisis time periods. Thus, following Berger and Bouwman's (2013) crisis definitions, we interact *Negative bank sentiment* with *Global Financial Crisis*, a dummy for the period 2007:Q3-2009:Q4, and *Post crisis*, a dummy from 2010:Q1-2016:Q4, leaving the pre-crisis period 1993:Q4-2007:Q2 as the omitted base case. As above for the tests of the effects of capital, we also include these time dummies and all of the controls and fixed effects from the full specification of the model.

Similar to the interactions with the *High capital ratio*, it is difficult to predict *ex ante* the effects of the interactions of *Negative bank sentiment* with *Global Financial Crisis* and *Post crisis* because several external forces acted on banks during these two periods. Market forces and government policies in some cases assisted banks with their liquidity hoarding, potentially easing banks' concerns about their liquidity and mitigating the effects of bank-specific negative sentiment on liquidity hoarding. During the crisis, deposits flowed into banks from those seeking safe havens (e.g., Acharya and Mora 2015), and government authorities provided liquidity as well (e.g., the Federal Reserves' discount window, Term Auction Facilities (TAF), and expansive conventional and unconventional monetary policies, the Troubled Asset Relief Program (TARP), and other bailouts). The Federal Reserve also encouraged bank liquidity hoarding through paying interest on bank reserves. During the post-crisis period, expansionary conventional and unconventional monetary policy and interest on reserves continued to help increase bank liquidity hoarding. The phasing in of the Basel III liquidity requirements also encouraged bank liquidity hoarding.

However, other market forces and government policies in some cases made bank liquidity hoarding more difficult during these two periods, potentially amplifying the effects of negative bank sentiment on liquidity hoarding. In the crisis, many business customers drew down their loan commitments (e.g., Ivashina and Scharfstein 2010), reducing liquidity hoarding.[9] The sometimes frozen or frosty conditions in interbank and syndicated loan markets also created difficulties for some banks in hoarding liquidity. In the post-crisis period, additional regulation and supervision from the implementation of the 2010 Dodd-Frank Act and the phasing in of the Basel III capital requirements could have also increased bank managers' concerns and magnified the effects of negative management sentiment on liquidity hoarding.

We again look to the empirical results to resolve these issues. The Table 2.4 Panel B findings suggest slightly stronger effects of negative bank sentiment during the crisis for total liquidity hoarding, stemming primarily from off-balance sheet liquidity hoarding. There are much stronger effects of negative sentiment on liquidity hoarding during the post-crisis period, except on the liability side. In economic significance terms, a one-standard-deviation increase in *Negative bank sentiment* is associated with additional 2.8 and 6.0 percentage points increases in *LH(total)/GTA* during the crisis and post-crisis periods, respectively, relative to the pre-crisis period.

Our findings of more pronounced impacts of negative bank sentiment on bank liquidity hoarding during the post-crisis period in Table 2.4 Panel B is consistent with our findings in Table 2.4 Panel A. The tougher regulation and supervision after the crisis may have encouraged bank managers to be more cautious, hoarding more liquidity in response

[9] Drawing down $1 of loan commitments decreases loan commitments by $1, increases loans by $1, and decreases cash by $1. Since loan commitments and loans have -1/2 weights and cash has a +1/2 weight, bank liquidity hoarding goes down by -$0.50.

to negative sentiment in the post-crisis period.

Benchmark liquidity level and excessive liquidity hoarding

In the main regression specification, we follow the literature's practice of using the level of liquidity held by banks as a variable of banks' liquidity hoarding. However, a potential issue is that "liquidity hoarding" may more appropriately indicate a bank's holding of liquidity beyond their expected needs or their benchmark level of liquidity. To address this concern, we perform several additional analyses. First, we add the lagged liquidity hoarding measure as an additional control variable, assuming that the benchmark level of liquidity is proportional to the lagged liquidity hoarding. However, the fixed effects combined with the lagged dependent variable will cause the error terms to be correlated with the regressors, making the OLS estimates biased, also known as Nickell bias (Nickell 1981). Thus, we estimate this regression model using the system GMM approach in the manner of Arellano and Bover (1995) and Blundell and Bond (1998). Table 2.5, column (1) presents the regression coefficients. The coefficient on the *Negative bank sentiment* remains positive and statistically significant, consistent with our main regression specification.

Second, we employ a multi-stage regression approach to estimate the benchmark level of bank liquidity hoarding. We first estimate the main regression Equation (2.2), excluding the *Negative bank sentiment* variable to predict the ($\widehat{LH/GTA}$). We then use the average value of the historical ($\widehat{LH/GTA}$) as a benchmark level of bank liquidity hoarding. Specifically, to reduce temporal noises in the estimated benchmark, we average the estimated ($\widehat{LH/GTA}$) up to twenty-four months. We can interpret the estimated benchmark as the level of liquidity a bank would need over the normal course of their business without the effect of *Negative bank sentiment*. We define a new dependent variable as *LH/GTA* minus its benchmark level, and then estimate the main regression model. The results are presented in Table 2.5, column (2). The coefficient on *Negative bank sentiment* again remains positive and statistically significant.

Overall, the benchmark analysis results show that the negative sentiment leads banks to hold more liquidity than their benchmark level rationalized by their fundamentals and market conditions.

Additional analyses and robustness checks

Some of the textual analysis literature focuses on the negative words only, arguing that negative words are used with more heed and care than positive words (e.g., Chen, De, Hu, and Hwang 2014). To address this issue, we include the ratios of negative words and positive words to total words separately in Table 2.13. The estimated coefficients on negative words and positive words are positive and negative, respectively, and are reasonably close in magnitude, justifying our combined treatment in our *Negative bank sentiment* measure.

We further test if negative bank management sentiment is a concern for banks of different sizes. Columns (1) and (2) of Table 2.6 Panel A report coefficient estimates on *Negative bank sentiment* for small and large banks, respectively, divided based on median GTA for each year. The results suggest that our main findings hold with positive and statistically significant coefficients for both small and large banks. The estimated coefficient on *Negative bank sentiment* is larger for the large banks than small banks, although the difference is not statistically significant. Given that large banks supply most of the liquidity to the economy and financial markets (Berger and Bouwman 2009), this result suggests that the bank sentiment can have substantial economic and financial effects. We discuss policy implications in Conclusion section.

A key policy issue in the U.S. since the 1930s is whether BHCs should be allowed to combine commercial and investment banking in the same corporate organization. After the systemic risk consequences of the 2008 bankruptcy of stand-alone investment bank Lehman Brothers, authorities encouraged other large investment banks, including Goldman Sachs and Morgan Stanley, to become BHCs with commercial banks to help reduce risks. Currently, many argue to the contrary that such combinations increase risks. Thus, a key issue here is whether the combined institutions are more or less swayed by management sentiment as stand-alone commercial banks. In column (3) of Table 2.6 Panel A, we estimate the main regression for banks and BHCs with commercial banks only, while column (4) reports results for BHCs with both commercial and investment banks. The coefficient estimates on *Negative bank sentiment* are positive and statistically significant for both sets, but the findings are several times stronger for the combined institutions. While policymakers consider systemic risk and factors in their policy choices about the structure of the industry, our findings here suggesting that the combined banking firms' liquidity hoarding decisions may also be influenced by managerial emotions may be an additional consideration.

A potential econometric concern with our analyses is that in some cases, we may be using "stale" sentiment information. Our baseline result is based on quarterly liquidity hoarding measure and annual sentiment measure from the most recent annual reports, which may not always be so recent. In column (5) of Table 2.6 Panel A, we reestimate the main regression equation (2.2) excluding the "stale" observations whose *Negative bank* *sentiment* is measured more than one quarter prior to the quarter of the liquidity hoarding dependent variable. The results continue to be positive and statistically significant.

Another potential concern is that the *Negative bank sentiment* measure may be confounded by the writing quality of the financial report. Banks with problematic assets may shroud their annual reports with confusing writing and hoard additional liquidity as a buffer at the same time. In column (6) of Table 2.6 Panel A, we re-estimate our regression by additionally controlling for the readability of the 10-K. We use the Gunning-Fog-Index to measure the readability of 10-Ks (Li 2008). The coefficient estimate is still positive and statistically significant with a similar magnitude to our baseline result, suggesting that this potential concern does not drive our findings.

We also examine the non-linearity of the relations between the *Negative bank sentiment* and liquidity hoarding using piecewise spline regressions. We use the median value of the *Negative bank sentiment* (0.007) as a knot to perform the piecewise regressions. Table 2.6 Panel B presents these regressions. The coefficient estimate on *Negative bank sentiment* is positive and statistically significant for the above-median category of *Negative bank sentiment*, suggesting a stronger impact of the *Negative bank sentiment* on the bank liquidity hoarding when sentiment is more negative.

Main results – Intensive margin

We acknowledge that our finding that *Negative bank sentiment* is associated with increased bank liquidity hoarding could be driven by both banks' supply and demand choices and customers' supplies and demands for the items that comprise bank liquidity hoarding. To ensure that our findings do not simply reflect customer choices, we examine the effects of *Negative bank sentiment* on interest rate spreads for loans, credit lines, and deposits, which are important liquidity hoarding elements in the asset-side, off-balance sheet-side, and liability-side. We see if these spreads move in the directions of bank supply and demand choices versus those of their customer counterparties. If they move in the directions of bank choices, this would essentially rule out the possibility that our findings are entirely driven by customer choices. More specifically, higher credit spreads in response to greater bank negative sentiment would suggest that the observed cutback in credit quantities incorporates bank volition in withdrawing credit supply at least to some extent, rather than being entirely driven by reduced borrower credit demand. Analogously, higher deposit spreads would suggest that increases in deposit quantities reflect at least in part increases in bank deposit demand, as opposed to being driven only by depositors' supply.

For the pricing of loans and credit lines, we employ credit spreads from Loan Pricing Corporation's (LPC's) DealScan database on commercial term loans and revolving lines of credit, representing on- and off-balance sheet credits, respectively.[10] We link the DealScan data with borrowers' accounting information from Compustat and bank characteristics from Bank Call Reports, because the credit risk of borrowing firms and the characteristics of lending banks are crucial determinants of credit spreads.[11] We include only the lead bank in our analyses because it is the main decision-maker on credit terms.[12]

[10] Term loans refer to loans of fixed amounts with fixed maturities. Revolvers refer to credits for which the borrower may draw down and repay any amount up to a fixed maximum as often as desired until maturity.
[11] We use the DealScan-Compustat link file available from WRDS for matching with Compustat before year 2012. Thanks to Raluca Roman for sharing her manually matched DealScan–Compustat links data from 2013 to 2014. We further extend the matched DealScan–Compustat links from 2015 to 2016. Based on bank names, locations, and other bank characteristics, we manually merge the DealScan with Bank Call Report.
[12] We acknowledge the potential difficulty with using DealScan data that most of the loans are at least partially syndicated, so that only parts of the credits are usually retained by the lead bank.

We exclude LBO transactions from our analysis because such credit packages include both term loans and credit lines.

We estimate regressions of the form:

$$Credit\ spread_{i,j,t} = \rho Negative\ bank\ sentiment_{i,t-1} + \pi' V_{j,t-1} + \vartheta' K_{i,j,t} + \omega' X_{i,t-1}$$
$$+ \psi'^{W_{i,t-1}} + \chi'^{S_{t-1}} + \zeta EPU_{t-1} + \alpha_i + q_t + \epsilon_{i,j,t}, \quad (2.3)$$

where i, j, and t index a bank, a borrower, and a calendar quarter, respectively. The dependent variable (*Credit spread*) is the borrowing credit spread plus annual fee (if any) the borrower pays in percentage over LIBOR, obtained from DealScan. We include bank fixed effects (α) to control for omitted bank characteristics that are invariant over time; quarter dummies (q) to account for seasonality; and borrower characteristics (V) to account for credit risk, including firm size (*Ln(ME)*), book-to-market ratio (*BE_ME*), leverage (*Leverage*), tangible asset ratio (*Tangible*), cash ratio (*Cash*), profitability (*profit*), and credit rating (*Credit rating*).[13] Because of the multi-dimensional aspect of bank credit contracts, we control for other credit contract characteristics (K) such as credit amount (*Credit size*), maturity (*Ln(Maturity)*), collateral (*Secured*), and covenants (*Covnt. index*). In addition, we control for bank-level characteristics (X), local market and corporate demand variables (W), other sentiment measures (S), and *EPU* as in equation (2.2). All variables are described in Table 2.14.

Table 2.7 Panel A presents the summary statistics of these variables by term loans

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Thus, some of the supply of credit is by other banks and other syndicate members. However, the lead bank generally retains significant portions of these loans, and DealScan has the benefit of reporting detailed pricing and contract terms on the loans, and being able to match it to Compustat data on the firms. Thus, we believe that the benefits of using these data outweigh any estimation noise introduced by the syndication.

[13] For the missing credit rating information in CRSP dataset, we create a dummy variable to indicate whether a borrower has a credit rating information or not. We replace the missing values with the average values of available information and include this dummy variable in the regression.

and revolvers. The final sample is at the loan facility-bank level, including 266 lead banks and 5,199 borrowing firms from 1993:Q4 through 2016:Q4. There are 12,660 observations for term loans and 36,317 for revolvers.

Table 2.8 columns (1)–(2) report the estimated impact of *Negative bank sentiment* on the *Credit spread* in equation (2.3) for term loans, and columns (3)-(4) report comparable information for revolvers. For both credit types, we follow the convention in the research literature of first excluding the other credit contract terms and then including them with the other controls. The inclusion of these terms is because credit spreads usually depend on the other terms that affect loan risk, such as collateral pledged. The exclusion is because, to some extent, all the other credit terms are determined endogenously with the spreads.

In all regression specifications, the estimated coefficients on *Negative bank sentiment* are positive, consistent with the direction of our hypothesized reductions in bank supply in response to negative bank management sentiment, rather than reduced demand for credit. Thus, our main findings of reduced credit likely reflect at least in part reductions in bank credit supply, rather than simply the effects of customer credit demand.

We next check the direction of the effects of *Negative bank sentiment* on deposit spreads using the following specification:

$$Deposit\ spread_{i,t} = gNegative\ bank\ sentiment_{i,t-1} + l'X_{i,t-1} + m''W_{i,t-1} + n'S$$
$$+qEPU_{t-1} + \alpha_i + q_t + \epsilon_{i,j,t} \qquad (2.4)$$

where *i* and *t* indicate a bank and a calendar quarter, respectively. The dependent variable is a *Deposit spread* for checking accounts, savings accounts, or money market accounts relative to the three-month T-bill rate. The key independent variable is *Negative bank sentiment*. We lag the independent variables to alleviate potential reverse-causality concerns and include the same set of controls with equation (2.2). Unlike the credit-spread analysis in Table 2.8, we include only bank and macro variables because depositor details are not available. In addition to the bank and seasonality fixed effects, we include *City* fixed effects where a bank branch operates to control for unobservable characteristics of local markets.

We obtain deposit spread information from the RateWatch database, which starts in 1998. Table 2.7 Panel B provides summary statistics for the deposit spreads. The data contain 605 unique banks and 398,428 observations at the bank-deposit product-calendar quarter level from 1998:Q1 to 2016:Q4.

Table 2.9 reports the findings. The coefficient estimates on *Negative bank sentiment* are all positively and statistically significant for the checking accounts, savings accounts, and money market accounts. A one-standard-deviation increase in *Negative bank sentiment* is associated with 0.2 percentage points increase in checking account deposit rate, 0.4 percentage points increase in saving account deposit rate, and 0.3 percentage points increase in money market account deposit rate. These results are consistent with our main findings of increased liability-side liquidity hoarding, reflecting at least to some degree increases in banks' demands for deposits, rather than only depositors' supplies.

Endogeneity and instrumental variable analysis

There are potential endogeneity concerns regarding *Negative bank sentiment*. Bank sentiment may be affected by the bank outputs and inputs that are part of our liquidity hoarding measures. In addition, omitted explanatory variables affecting both bank sentiment and liquidity hoarding may bias our OLS estimates. For example, banks may observe latent indicators of future economic conditions, which may drive both *Negative* *bank sentiment* and their liquidity hoarding. Another endogeneity concern arises if our negative sentiment measure mainly captures bank distress that provokes regulatory reactions that require additional liquidity holding. Finally, there is a possibility that our main OLS findings are driven by unobservable macro variables or time-specific events rather than bank-specific sentiment.

To address these concerns, we use local weather conditions in the vicinity of bank headquarters as instrument variables for bank sentiment. Weather conditions are appealing instruments for bank sentiment because weather is exogenously determined, and it is shown to have real effects on human sentiment (e.g., Lerner et al. 2015; DeHann, Madsen, and Piotroski 2017). We posit that the local weather conditions near bank headquarters influence the sentiment of bank officials, which then affects their liquidity hoarding decisions via their operating decisions such as credit supply and deposit demand.[14] Our identification strategy is to estimate the impact of weather-driven sentiment on bank behavior, so our empirical analysis estimates the local average treatment effect (LATE) of the banks whose sentiment is sensitive to changes in the exogenous weather conditions. This strategy can also effectively rule out the possibility that macro or time-specific factors drive our results since local weather conditions vary widely across the nation at any point in time.

From the National Oceanic and Atmospheric Administration (NOAA) Climate Database, we obtain a broad set of weather information, including cloud coverage, one-hour or six-hour precipitation, air temperature, dew point temperature, wind speed, wind

[14] For studies showing that weather conditions affect decision-making by investors and managers, see, e.g., Hirshleifer and Shumway (2003), Bassi, Colacito, and Fulghieri (2013), Goetzmann et al. (2015), Cortés, Duchin, and Sosyura (2016).

direction, and sea level pressure at the day-hour-weather station level. We select weather conditions only during local working hours 8:00AM – 5:00PM for the working days of a week.

The large set of weather conditions poses a challenge. Using many weather conditions as instruments carries the risk of overfitting the first-stage regressions. Hand-picking some of the instruments raises data-mining concerns.

To avoid overfitting and data-mining problems, we implement the least absolute shrinkage and selection operator (LASSO) to select the best instrumentals, following Belloni, Chernozhukov, and Hansen (2011) and Gilchrist and Sands (2016). LASSO offers a principled procedure for selecting instruments and provides well-performing results compared to other robustness procedures for instrumental variables (Belloni, Chen, Chernozhukov, and Hansen, 2012).

For LASSO, we consider 144 seasonally-adjusted weather conditions on weekdays (Tuesday only, Tuesday to Thursday, or Monday to Friday) during working hours with one- or two-quarter lags adjusted by prior one-, two-, or three-year average weather conditions in the same quarter of the year.[15] We choose one- or two-quarter lags of weather conditions based on the assumption that the annual report 10-K is prepared mainly in the last quarter of fiscal year, and it takes about two or three months for the completed 10-K files to be prepared and updated to the SEC EDGAR system.[16] We create dummies for each

[15] To account for seasonal variation of weather conditions, we construct seasonally adjusted weather conditions for cloud coverage, one-hour or six-hour precipitation, air temperature, dew point temperature, wind speed, wind direction, and pressure using Tuesday only, Tuesday to Thursday, or Monday to Friday working hours information and lagging one or two quarters.

[16] For example, Apple Inc. has fiscal year 2018 ending in September 29, 2018. The 2018 annual report was filed to the SEC EDGAR system on November 5, 2018. We posit that the annual report was mainly prepared during June – October 2018.

of the 144 local weather conditions to account for potential nonlinear relations between sentiment and weather conditions (Gilchrist and Sands 2016). Specifically, we create cloud coverage dummies in 1 okta bin for each of the cloud coverage variables, where an okta is a measure of cloud coverage ranging from 0 (completely clear sky) to 8 (completely overcast). Similarly, temperature dummies are in 5-degree celsius bins, sea level pressure dummies are in 5 hectopascals bins, where hectopascals are international units of barometric pressure that are increasing in this pressure, and one-hour or six-hour precipitation are in 20 millimeters bins. The selection of bin width is based on the previous literature (Gilchrist and Sands 2016) and the computational concern. In total, we include 2,090 dummy variables indicating local weather conditions of bank headquarters in our LASSO selection model.

The first LASSO-chosen instrument is the seasonally-adjusted cloud coverage dummy indicating a -3 to -2 oktas difference between the current and preceding three-year average from Monday to Friday working hours with one-quarter lags. For two instrumental variables, LASSO additionally chooses the cloud coverage dummy indicating the same difference (-3 to -2 oktas) between the current and previous three-years average from Monday to Friday with two-quarter lag. When choosing three instrumental variables, LASSO additionally selects the sea level pressure dummy for the 0 to 5 hectopascals difference between the current and previous three-years average from Tuesday to Thursday with two-quarter lags as the best instrumental variables for the bank sentiment measure. The choice of cloud coverage is largely consistent with prior studies in the literature (e.g., Goetzmann, Kim, Kumar, and Wang 2015; Chhaochharia, Kim, Korniotis, and Kumar 2019). We consider these three instrumental variables for the bank sentiment.

Table 2.10 Panel A reports the first-stage regressions of the *Negative bank sentiment* on the various numbers of LASSO-chosen instruments and other control variables. The coefficients on the instrumental variables based on seasonally-adjusted weather conditions are all statistically significant at 1% level individually. The partial *F*-statistics for up to two instrumental variables are above the conventional threshold for the weak instrumental variables (Stock and Yogo 2005). We use two instrumental variables for implementing the two-stage least squares analysis.[17]

In the second-stage regressions in Table 2.10 Panel B, we regress the liquidity hoarding measures on the instrumented *Negative bank sentiment* measure and the controls. The *t*-statistics are based on bootstrapped standard errors to mitigate biases from errors in the estimated independent variables. The estimated coefficients on the instrumented *Negative bank sentiment* all have the same positive signs as our main results in Table 2.3, and all are statistically significant. These findings suggest that our baseline OLS results are neither entirely driven by endogeneity concerns, nor do they entirely reflect macro or time-specific factors. The estimated coefficients on *Negative bank sentiment* in the second-stage regressions are greater in magnitude than the baseline OLS regressions because we are estimating the local average treatment effect (LATE) for bankers who are more sensitive to the weather conditions.[18] Regarding the economic magnitude, a one-standard-deviation increase in the *Negative bank sentiment* leads to a 9.7 percentage points increase in

[17] Results with only one instrumental variable are qualitatively and quantitatively similar.
[18] In other words, the IV analysis estimates the impact of bank sentiment on liquidity hoarding for the "emotionally sensitive" banks. Because the identification strategy is based on weather-driven sentiment, banks who are more emotionally sensitive would be likely to be affected by these exogenous shocks. And their tendency to change their liquidity hoarding due to sentiment would be greater than other banks whose sentiments are less dependent on weather conditions. The increased magnitude of estimated local average treatment effects is not uncommon in financial economics research (see Jiang 2017).

LH(total)/GTA.

In Table 2.10 Panel C, we report the coefficient estimates from the second-stage regressions of credit and deposit spreads on the instrumented *Negative bank sentiment* measure and other controls to substantiate that the above result is not entirely driven by the bank customers. The estimated coefficients on the *Negative bank sentiment* are all positive and statistically significant, suggesting that banks with more negative sentiment reduces credit supply and increases deposit demand. These results are consistent with those in Tables 2.7 and 2.8, again suggesting that our main results reflect at least in part bank supplies and demands, rather than just customer choices.

A potential concern for the validity of the instrumental variable analysis is that the weather conditions near bank headquarters may also affect their customers. This concern is already somewhat mitigated in our empirical setting because our sample of publicly-traded banks and BHCs often have extensive geographic footprints beyond the headquarters where the weather is measured. Moreover, because inclement weather would reduce corporate risk-taking (Bassi, Colacito, and Fulghieri 2013) and investment (Chhaochharia et al. 2019), any resulting bias from local firms may make our estimate a lower bound for the true impact of sentiment on bank liquidity hoarding. Specifically, the estimated impact of instrumented bank sentiment on credit spreads (Table 2.10 Panel C) may be attenuated due to the local firms' decreased demand for credit. Nonetheless, to further address this concern, we restrict our sample to banking organizations operating in multiple states. The IV analysis results presented in Table 2.10, Panels D and E confirm the impact of negative bank sentiment on liquidity hoarding with similar statistical significance and economic magnitudes.

Conclusion

We test our hypothesis rooted in the psychology and behavioral finance literature that negative managerial sentiment increases corporate liquidity hoarding using the banking industry as an empirical setting. Employment of banking data overcomes three key empirical challenges: 1) detailed financial accounting data on assets, liabilities, and off-balance sheet activities; 2) a comprehensive measure of liquidity hoarding, combining these data based on theoretical and empirical research; and 3) avoiding major differences in liquidity needs across industries. We employ a managerial sentiment measure that is based on the negative and positive tones of the words in their annual corporate 10-K reports.

Our empirical analysis suggests that negative bank management sentiment increases bank liquidity hoarding, controlling for a large set of bank- and market-level characteristics. Additional analyses suggest that the effects occur on both sides of the balance sheet and off the balance sheet, that the findings reflect at least to some degree bank supply and demand choices as opposed to their customers' choices. Our findings are also highly robust to an advanced instrumental variable approach. The sentiment-driven liquidity hoarding behavior is more pronounced for banks with high capital ratios and during and especially after the Global Financial Crisis. The findings are also more pronounced for banking organizations with investment banks than for organizations with only commercial banks.

While we cannot say with certainty that our results from the banking industry fully generalize to other non-banking firms, we believe that our findings do advance the general understanding of the effects of corporate managerial sentiment on liquidity hoarding. Bank managers are not likely fundamentally different from other corporate managers in their emotional reactions, so we would expect qualitatively similar effects in the same direction for these other managers. We argue our paper provides important implications for the understanding of the effects of managerial sentiment on corporate liquidity hoarding.

Our findings have potential policy implications. First, our main results suggest that negative bank sentiment may interfere with the effective operations of monetary and prudential policies. For example, expansionary monetary policy may be thwarted by negative bank sentiment that causes more of the additional liquidity injected by the central bank to be hoarded by banks. Implementation of prudential policies could also be impeded by positive bank managerial sentiment that results in suboptimal excessive risk-taking.

Second, our finding that BHCs with investment banks may be much more swayed by management sentiment in their liquidity hoarding decisions than other commercial banking organizations adds a new argument to the ongoing debate on banking powers. Investigation of the full consequences of this finding is beyond the scope of this paper, but such consequences might help inform policymakers considering these banking powers.

Third, on a more speculative note, policymakers may be able to influence the effects of negative bank sentiment on bank liquidity hoarding behavior. Some of our analyses suggest that higher bank capital and harsher regulatory and supervisory treatment may increase the effects of negative bank sentiment on liquidity hoarding. Thus – subject to the Lucas critique that changes in policy may alter the underlying model – policymakers may be able to encourage more bank liquidity hoarding during a boom by requiring higher capital and other strict regulation and supervision. Such policies might be reversed during a bust. A consequence is that countercyclical capital requirements may be more effective than previously thought.

Finally, our findings of significant effects of bank managerial sentiment on bank actions may have implications for future research. The strands of the banking literature concerning procyclical lending behavior, "zombie lending," and the effects of various types of uncertainty on bank behavior may have alternative explanations involving the sentiment of bank managers. Our derivation of the sentiment measure from accounting statements may also help inspire additional text-based economic or financial measures from these statements. Lastly, we encourage others to test our same hypothesis for non-banking corporations, although the measurement challenges would also need to be addressed.

Table 2.1 Measures of liquidity hoarding

This table, adapted from Berger et al. (forthcoming), shows how the bank liquidity hoarding measures are constructed from the dollar values of balance sheet and off-balance sheet activities. Weights of +1/2 are assigned to items contributing to bank liquidity hoarding, and weights of (-1/2) are assigned to items reducing such hoarding. Total bank liquidity hoarding, *LH(total)* = *LH(asset)* + *LH(liab)* + *LH(off)*, where *LH(asset)* = (+1/2) × liquid assets + (-1/2) × illiquid assets; *LH(liab)* = (+1/2) × liquid liabilities; and *LH(off)* = (-1/2) × illiquid guarantees + (+1/2) × liquid derivatives. These liquidity hoarding measures are developed by Berger et al. (forthcoming) based on Berger and Bouwman's (2009) liquidity creation measures.

LH(asset)		***LH(liab)***	***LH(off)***	
Liquid assets (weight = + 1/2)	**Illiquid assets (weight = - 1/2)**	**Liquid liabilities (weight = + 1/2)**	**Illiquid guarantees (weight = - 1/2)**	**Liquid derivatives (weight= + 1/2)**
Cash and due from other institutions	Commercial real estate loans (CRE)	Transactions deposits	Unused commitments	Interest rate derivatives
All securities (regardless of maturity)	Loans to finance agricultural production	Savings deposits	Net standby letters of credit	Foreign exchange derivatives
Trading assets	Commercial and industrial loans (C&I)	Overnight federal funds purchased	Commercial and similar letters of credit	Equity and commodity derivatives
Fed funds sold	Other loans and lease financing receivables	Trading liabilities	Net participations sold	
	Other real estate owned (OREO)		All other off-balance sheet liabilities	
	Customers' liability on bankers' acceptances			
	Investment in unconsolidated subsidiaries			
	Intangible assets			
	Premises			
	Other assets			
LH(total) = LH(asset) + LH(liab) + LH(off)				

Table 2.2 Summary statistics

This table presents summary statistics for the variables used in the main analysis. The sample includes 2,965 banks (57,841 bank-quarter observations) from 1993:Q4 through 2016:Q4. The observations are on a bank-calendar quarter level. All dollar values are adjusted to real 2016 values using the implicit GDP price deflator. All control variables except macro variables are winsorized at 1% and 99% levels.

	N	Mean	StDev	25th Pctl	Median	75th Pctl
Dependent variables						
LH(total)/GTA	57841	0.074	0.177	-0.050	0.070	0.193
LH(asset)/GTA	57841	-0.080	0.139	-0.183	-0.083	0.013
LH(liab)/GTA	57841	0.239	0.070	0.193	0.237	0.285
LH(off)/GTA	57841	-0.084	0.052	-0.110	-0.073	-0.047
Key independent variable						
Negative bank sentiment	57841	0.007	0.005	0.004	0.007	0.011
Control variables						
Ln(GTA)	57841	13.615	1.474	12.497	13.526	14.643
Capital ratio	57841	0.078	0.029	0.057	0.071	0.092
Earnings	57841	0.010	0.020	0.008	0.011	0.015
HHI	57841	0.108	0.118	0.034	0.091	0.135
Population	57841	1.962	0.872	1.498	2.012	2.516
Tobin's Q	57841	2.419	1.007	1.808	2.152	2.653
Cash flows	57841	-0.002	0.030	-0.014	0.004	0.017
Corporate sentiment	27807	-0.072	1.043	-0.297	0.212	0.609
Investor sentiment	57841	0.262	0.631	-0.077	0.193	0.567
Consumer sentiment	57841	88.713	12.372	82.433	90.733	94.900
EPU	57841	4.570	0.272	4.333	4.509	4.734

Table 2.3 The effects of sentiment on liquidity hoarding

This table presents coefficient estimates from regressions of the bank liquidity hoarding on the *Negative bank sentiment* and controls. For the dependent variables, we consider total bank liquidity hoarding (*LH(total)*) in Panel A as well as its component (*LH(asset)*, *LH(liab)*, and *LH(off)*) normalized by the gross total asset (*GTA*) in Panel B. The sample includes 2,965 banks (57,841 bank-quarter observations) from 1993:Q4 through 2016:Q4. All variables are described in Tables 2.1 and 2.11. Coefficients on constant terms are omitted for brevity. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank holding company and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Panel A: Regressions of bank total liquidity hoarding (*LH(total)/GTA*) on *Negative bank sentiment*

	(1) *LH(total)/GTA*	(2) *LH(total)/GTA*	(3) *LH(total)/GTA*	(4) *LH(total)/GTA*
Negative bank sentiment	5.190***	5.234***	4.755***	3.442***
	(9.20)	(9.38)	(5.06)	(2.10)
Ln(GTA)	-0.045***	-0.043***	-0.038***	-0.040***
	(-13.31)	(-12.54)	(-4.15)	(-4.47)
Sqr.Ln(GTA)	0.000	0.000	0.000	-0.000
	(1.49)	(1.04)	(0.74)	(-0.37)
Capital ratio	-0.555***	-0.551***	-0.445***	-0.631***
	(-7.02)	(-7.10)	(-4.19)	(-5.88)
Earnings	0.123**	0.120***	0.124**	0.127**
	(2.60)	(2.76)	(2.50)	(2.63)
HHI		-0.003	-0.002	-0.004
		(-0.49)	(-0.12)	(-0.24)
Population		-0.018***	-0.012	-0.019*
		(-5.12)	(-1.17)	(-1.97)
Tobin's Q		0.003	0.007***	0.006***
		(1.27)	(4.08)	(3.79)
Cash flows		0.155***	0.169***	0.178***
		(3.32)	(3.37)	(3.61)
Corporate sentiment			-0.007	-0.008**
			(-1.58)	(-2.06)
Investor sentiment			-0.011***	-0.010***
			(-3.05)	(-2.64)
Consumer sentiment			0.001**	0.001**
			(2.33)	(2.17)
EPU			0.069***	0.062***
			(4.04)	(3.66)
Bank FE	Yes	Yes	Yes	Yes

Seasonal FE	Yes	Yes	Yes	Yes
Time trend	No	No	No	Yes
Adj. R-squared	0.805	0.806	0.820	0.849
Number of obs.	57841	57841	57841	57841

Panel B: Regressions of bank liquidity hoarding components on *Negative bank sentiment*

	(1) *LH(asset)/GTA*	(2) *LH(liab)/GTA*	(3) *LH(off)/GTA*
Negative bank sentiment	0.824*	3.495***	0.498**
	(1.73)	(5.54)	(2.48)
Ln(GTA)	-0.036***	0.001	-0.003
	(-6.01)	(0.20)	(-1.13)
Sqr.Ln(GTA)	0.000	0.000	-0.000
	(0.80)	(0.85)	(-0.68)
Capital ratio	-0.606***	0.233***	-0.074*
	(-9.03)	(2.98)	(-1.81)
Earnings	0.020	0.209***	-0.102***
	(0.68)	(4.76)	(-4.55)
HHI	-0.009	0.015*	-0.008***
	(-0.77)	(1.88)	(-2.72)
Population	-0.017*	0.011*	-0.007**
	(-1.81)	(1.74)	(-2.22)
Tobin's Q	0.003***	0.003***	0.000
	(3.06)	(3.71)	(0.91)
Cash flows	0.088***	0.039	0.041***
	(2.87)	(1.58)	(3.42)
Corporate sentiment	-0.006***	-0.000	-0.001
	(-3.12)	(-0.02)	(-0.64)
Investor sentiment	-0.004**	-0.005***	-0.002*
	(-2.17)	(-2.76)	(-1.69)
Consumer sentiment	0.001**	0.001***	-0.000**
	(2.23)	(2.99)	(-2.21)
EPU	0.035***	0.019**	0.016***
	(3.94)	(2.22)	(4.70)
Bank FE	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes
Adj. R-squared	0.841	0.749	0.801
Number of obs.	57841	57841	57841

Table 2.4 The effects of bank sentiment by bank capital and time period

This table presents coefficient estimates from regressions of the bank liquidity hoarding on the *Negative bank sentiment* and controls, including interaction terms between the *Negative bank sentiment* and *High capital ratio* or *Global Financial Crisis*. *High capital ratio* is a binary variable equal to one if *Capital ratio* is greater than its 75th percentile, otherwise equals to zero. *Global Financial Crisis* is a binary variable equal to one if a sample period is between 2007:Q3 and 2009:Q4, or zero otherwise. *Post crisis* is defined as a binary variable equal to one if a sample period is after 2009:Q4 or zero otherwise. The dependent variables include total bank liquidity hoarding (*LH(total)*) and its components (*LH(asset)*, *LH(liab)*, and *LH(off)*) normalized by the gross total assets (*GTA*). The sample includes 2,965 banks (57,841 bank-quarter observations) from 1993:Q4 through 2016:Q4. All variables are described in Tables 2.1 and 2.11. Coefficients on constant terms are omitted for brevity. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank holding company and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Panel A: Bank capital ratio and the impact of bank sentiment on liquidity hoarding

	(1) *LH(total) /GTA*	(2) *LH(asset) / GTA*	(3) *LH(liab) / GTA*	(4) *LH(off) / GTA*
Negative bank sentiment ×	3.837***	1.360***	2.095***	0.390***
High capital ratio	(6.85)	(4.12)	(7.24)	(2.90)
Negative bank sentiment	3.474***	0.458	2.679***	0.399*
	(3.95)	(0.99)	(4.55)	(1.98)
High capital ratio	-0.010***	-0.012***	0.005***	-0.004***
	(-3.49)	(-5.22)	(3.62)	(-4.75)
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.823	0.843	0.757	0.802
Number of obs.	57841	57841	57841	57841

Panel B: *Global Financial Crisis* and the impact of bank sentiment on liquidity hoarding

	(1) *LH(total)* */GTA*	(2) *LH(asset)* */ GTA*	(3) *LH(liab)* */ GTA*	(4) *LH(off)* */ GTA*
Negative bank sentiment ×	1.737*	0.840	-0.079	1.085**
Global Financial Crisis	(1.71)	(0.94)	(-0.13)	(2.06)
Negative bank sentiment ×	4.002***	2.623**	-0.217	1.633***
Post crisis	(2.88)	(2.54)	(-0.32)	(3.76)
Negative bank sentiment	-0.015	-0.704	1.150**	-0.441
	(-0.02)	(-1.20)	(2.54)	(-1.40)
Global Financial Crisis	-0.037***	-0.034***	0.001	-0.005
	(-4.42)	(-4.21)	(0.12)	(-0.95)
Post crisis	0.040*	-0.018	0.064***	-0.005
	(1.79)	(-1.25)	(4.91)	(-0.94)
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.839	0.846	0.788	0.805
Number of obs.	57841	57841	57841	57841

Table 2.5 Benchmark liquidity level and excessive liquidity hoarding

This table reports coefficients estimates from regressions of the total bank liquidity hoarding adjusted for its benchmark level on the *Negative bank sentiment* measures and controls. In column (1), we estimate the main regression with a lagged dependent variable as an additional control using a system GMM approach (Arellano and Bover, 1995; Blundell and Bond, 1998). In column (2), we estimate the main regression with a benchmark adjusted *LH(total)/GTA* estimated in two-staged regressions. Estimated coefficients on controls and constant terms are omitted for brevity. The sample period is from 1993:Q4 to 2016:Q4. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank holding company and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)
	Dept. = *LH(total)/GTA*	Dept.=*LH(total)/GTA* - $\widehat{LH/GTA}$
Negative bank sentiment	0.639***	4.860***
	(4.85)	(4.91)
Lagged LH(total) / GTA	0.864***	
	(99.66)	
Controls	Yes	Yes
Bank FE	Yes	Yes
Seasonal FE	Yes	Yes
Number of obs.	57841	57841

Table 2.6 Additional analyses and robustness checks

This table presents additional analyses and robustness checks. Panel A shows the coefficient estimates from regressions of the bank liquidity hoarding on the *Negative bank sentiment* with various subsamples of banks. The model specification is the same as the main regression (Table 2.3 Panel A, column (3)). Columns (1) and (2) include small and large banks, respectively. The small (large) banks are defined as those with below (above) the median gross total asset for each year. Column (3) includes organizations with commercial banking only, while column (4) shows results for BHCs with both commercial and investment banks. Column (5) includes observations whose *Negative bank sentiment* is measured within one quarter before the liquidity hoarding. Column (6) additionally controls for the readability (Gunning-Fog-Index) of annual reports (10-K). Panel B presents coefficient estimates from piecewise spline regressions of the bank liquidity hoarding on the *Negative bank sentiment.* The regression is separate at the knot of the median *Negative bank sentiment* (0.007). Estimated coefficients on controls and constant terms are omitted for brevity. The sample period is from 1993:Q4 to 2016:Q4. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank holding company and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Panel A: Additional analyses with subsamples

	Dep. = *LH(total)/GTA*					
	(1) Small banks (GTA<median)	(2) Large banks (GTA>median)	(3) Commercial banking only	(4) Commercial and investment banks	(5) No stale sentiment measures	(6) Controlling for readability of 10-K
Negative bank sentiment	3.517***	4.979***	1.357*	6.290***	4.083***	4.692***
	(3.96)	(5.47)	(1.96)	(5.10)	(3.85)	(5.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.858	0.807	0.890	0.827	0.818	0.820
Number of obs.	28922	28919	22014	34994	26838	57841

Panel B: Piecewise spline regressions of the bank liquidity hoarding on the *Negative bank sentiment*

	(1) *LH(total)/GTA*	(2) *LH(asset) / GTA*	(3) *LH(liab) / GTA*	(4) *LH(off) / GTA*
Negative bank sentiment > Median	10.521***	4.280***	4.348***	1.956***
	(7.13)	(3.82)	(4.17)	(3.73)
Negative bank sentiment < Median	-1.650	-1.782*	0.848	-0.693
	(-1.52)	(-1.98)	(1.23)	(-1.47)
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.824	0.842	0.753	0.803
Number of obs.	57841	57841	57841	57841

Table 2.7 Summary statistics for bank supply and demand sample

This table presents summary statistics for the variables used in bank supply/demand choice versus customer choice analyses. The variables in Panel A are at the loan type-loan facility-bank level from 1993:Q4 through 2016:Q4. The variables in Panel B are at bank-deposit product-calendar quarter level from 1998:Q1 to 2016:Q4. All variables are described in Table 2.14.

Panel A: loan type-loan facility-bank level

	Term Loan						Revolvers					
	N	Mean	StDev	25th Percentile	Median	75th Percentile	N	Mean	StDev	25th Percentile	Median	75th Percentile
Bank loan variables												
Credit spread	12660	2.181	1.164	1.500	2.000	2.750	36317	1.426	0.968	0.625	1.250	2.000
Credit size	12660	18.960	1.502	18.133	19.114	19.979	36317	19.447	1.346	18.644	19.519	20.367
Covnt. Index	12660	2.822	2.221	1.000	3.000	5.000	36317	1.891	1.944	0.000	1.000	4.000
Secured	12660	0.516	0.500	0.000	1.000	1.000	36317	0.316	0.465	0.000	0.000	1.000
Ln(Maturity)	12466	4.015	0.447	3.912	4.111	4.290	35630	3.914	0.401	3.714	4.111	4.111
Borrower variables												
Ln(ME)	12660	13.939	1.683	12.877	14.060	15.132	36317	14.287	1.745	13.159	14.317	15.480
BE_ME	12660	0.691	1.130	0.243	0.430	0.732	36317	0.648	0.915	0.274	0.456	0.743
Leverage	12660	0.336	0.219	0.179	0.312	0.471	36317	0.271	0.190	0.134	0.254	0.376
Tangible	12660	0.290	0.222	0.106	0.235	0.438	36317	0.334	0.246	0.132	0.269	0.509
Cash	12660	0.081	0.106	0.014	0.042	0.102	36317	0.078	0.100	0.013	0.038	0.103
Profit	12660	0.133	0.075	0.091	0.127	0.169	36317	0.139	0.073	0.094	0.131	0.175
Credit rating	7684	9.291	2.372	8.000	9.000	11.000	22421	11.034	2.920	9.000	11.000	13.000

Panel B: bank-deposit product-calendar quarter level

Bank deposit variables	N	Mean	StDev	25th Percentile	Median	75th Percentile
Checking accounts deposit spreads	23027	-1.161	1.485	-2.496	-0.281	-0.003
Savings accounts deposit spreads	110014	-0.365	1.008	-0.220	-0.000	0.061
Money market accounts deposit spreads	265387	-0.274	0.982	-0.200	0.030	0.143

Table 2.8 The effects of bank sentiment on credit spreads

This table presents coefficient estimates from regressions of the credit spreads on the *Negative bank sentiment* measure and controls. The sample includes 266 lead banks and 5,199 borrowing firms from 1993:Q4 through 2016:Q4. *Controls* include *Ln(GTA)*, *Sqr.Ln(GTA)*, *Capital ratio*, *Earnings*, *HHI*, *Population*, *Tobin's Q*, *Cash flows*, *Corporate sentiment*, *Investor sentiment*, *Consumer sentiment* and *EPU*. All variables are described in Appendices A and D. For some observations, the *Credit rating* variable is not available. In such cases, we replace them with the average value of available *Credit rating* and include a dummy variable equal to one when the *Credit rating* variable is available and zero otherwise. The number of observations is slightly different between columns (1) and (2), and between (3) and (4) due to missing observations for control variables. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	Dep. = Credit spread over LIBOR			
	Term loans (On-balance sheet)		*Revolvers (Off-balance sheet)*	
	(1)	(2)	(3)	(4)
Negative bank sentiment	12.348**	9.905*	9.300***	10.532***
	(2.13)	(1.79)	(2.89)	(3.45)
Ln (ME)	-1.243***	-1.272***	-1.178***	-0.972***
	(-9.68)	(-10.83)	(-21.49)	(-17.32)
Sqr. Ln(ME)	0.036***	0.037***	0.035***	0.031***
	(8.00)	(9.11)	(18.27)	(15.85)
BE_ME	-0.011	-0.027	-0.042***	0.001
	(-0.56)	(-1.50)	(-3.38)	(0.13)
Leverage	0.293***	0.167**	0.509***	0.533***
	(4.08)	(2.25)	(11.03)	(12.54)
Tangible	0.260***	0.315***	-0.015	-0.006
	(3.44)	(4.12)	(-0.58)	(-0.25)
Cash	0.708***	0.686***	0.459***	0.230***
	(4.60)	(4.83)	(6.29)	(3.37)
Profit	-2.007***	-2.016***	-1.495***	-1.406***
	(-9.69)	(-11.34)	(-14.42)	(-14.09)
Credit rating	0.055	0.057	-0.020	-0.027
	(1.30)	(1.43)	(-1.09)	(-1.50)
Credit size		0.022		-0.095***
		(1.21)		(-10.29)
Ln(Maturity)		-0.111***		-0.277***
		(-3.11)		(-10.00)
Secured		0.512***		0.201***
		(13.82)		(8.14)

Covnt. index		0.019*		0.062***
		(1.93)		(8.31)
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.395	0.439	0.574	0.617
Number of obs.	12660	12466	36317	35630

Table 2.9 The effects of bank sentiment on deposit rate spread

This table presents coefficient estimates from regressions of the deposit interest rate spreads on the Negative bank sentiment measure and controls. The sample includes 605 banks and 398,428 deposit products×quarter observations from RateWatch covering the sample period 1998:Q1 through 2016:Q4. Controls include Ln(GTA), Sqr.Ln(GTA), Capital ratio, Earnings, HHI, Population, Tobins' Q, Cash flows, Corporate sentiment, Investor sentiment, Consumer sentiment and EPU. All variables are described in Appendices A and D. t-statistics are reported in parentheses and are based on standard errors clustered at a bank and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	Dep. = Deposit rate over 3-month T-bill		
	(1) *Checking accounts*	(2) *Savings accounts*	(3) *Money market accounts*
Negative bank sentiment	46.718***	80.938***	52.700***
	(2.87)	(6.06)	(4.15)
Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Adj. R^2	0.758	0.666	0.599
Number of obs.	23027	110014	265387

Table 2.10 Instrumental variable analysis with local weather conditions

This table presents coefficient estimates from instrumental variable analysis with local weather conditions near bank headquarters as instrumental variables for bank sentiment. In Panel A, we report the first-stage regression results with various numbers of LASSO-chosen instrumental variables. In Panel B, we report the second-stage regression of *Negative bank sentiment* on liquidity hoarding with the LASSO-chosen weather conditions as instrumental variables for the *Negative bank sentiment*. *Controls* include variables of Column (3) of Table 2.3 Panel A. In Panel C, we report the second-stage regression of *Negative bank sentiment* on credit- and deposit-spreads with the LASSO-chosen weather conditions as instrumental variables for the *Negative bank sentiment*. *Controls* include variables of Tables 2.7 and 2.8, respectively. Panels D and E replicate Panels B and C, respectively for banks operating in multiple states. Coefficients on *Controls* are omitted for brevity. All variables are described in Appendices A and D. *t*-statistics are reported in parentheses and are based on standard errors robust to heteroskedasticity and arbitrary correlations at the bank-time level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Panel A: First-stage regressions of *Negative bank sentiment* on LASSO-chosen instruments and control variables

Set of potential Instruments	**Count constraint**	**LASSO-chosen instrument(s)**	**Coefficient**	***F*-Statistic**
2,090 dummy variables created based on seasonally adjusted 144 local weather conditions, which include: 8 weather conditions (cloud coverage, precipitation (1hrs or 6hrs), air temperature, dew point temperature, wind speed, wind direction, pressure) × 3 different coverages of weekdays (8 am – 5 pm on Tuesday only, Tuesday – Thursday, Monday – Friday) × 2 different lags from annual reports filing date (one-, two-quarters) × 3 different de-seasonalizing (one-, two-, and three-years) For each weather condition, a dummy variable is created with equally-spaced bins (refer to Section V)	Choose 1	Monday-Friday, one-quarter lag, de-seasonalized by previous three years' average cloud coverage with the range of -3 to -2 oktas.	0.0005*** (3.76)	14.17
	Choose 2	Monday-Friday, one-quarter lag, de-seasonalized by previous three years' average cloud coverage with the range between -3 to -2 oktas.	0.0005*** (3.40)	10.42
		Monday-Friday, two-quarter lag, de-seasonalized by previous three years' average cloud coverage with the range between -3 to -2 oktas .	0.0004*** (3.64)	
	Choose 3	Monday-Friday, one-quarter lag, de-seasonalized by previous three years' average cloud coverage with the range between -3 to -2 oktas.	0.0005*** (2.84)	8.97
		Monday-Friday, two-quarter lag, de-seasonalized by previous three years' average cloud coverage with the range between -3 to -2 oktas.	0.0004*** (2.71)	
		Tuesday to Thursday, two-quarter lag, de-seasonalized by previous three years' average sea level pressure with the range between 0 to 5 hectopascals.	0.0013** (3.66)	

Panel B: Second-stage regressions of liquidity hoarding measures on *Negative bank sentiment* instrumented by LASSO-chosen instrument variables

	Second Stage			
	(1) *LH(total)/GTA*	(2) *LH(asset)/GTA*	(3) *LH(liab)/GTA*	(4) *LH(off)/GTA*
Negative bank sentiment (IV)	19.555***	3.888***	14.691***	0.976**
	(9.96)	(3.98)	(12.46)	(2.11)
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
Number of obs.	54585	54585	54585	54585

Panel C: Second-stage regressions of the price of bank loans and deposits on *Negative bank sentiment* instrumented by LASSO-chosen instrument variables

	Second Stage				
	(1) *Term loans Credit spreads*	(2) *Revolvers Credit spreads*	(3) *Checking accounts Deposit spreads*	(4) *Savings account Deposit spreads*	(5) *Money market accounts Deposit spreads*
Negative bank sentiment (IV)	114.797**	45.365*	262.810**	458.617***	347.425***
	(2.97)	(1.90)	(2.49)	(5.84)	(4.67)
Controls	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes	Yes
City FE	No	No	Yes	Yes	Yes
Number of obs.	11511	32493	21777	105574	255193

Panel D: Second-stage regressions of liquidity hoarding measures on *Negative bank sentiment* instrumented by LASSO-chosen instrument variables for banks operating in multiple states

	Second Stage			
	(1) *LH(total)/GTA*	(2) *LH(asset)/GTA*	(3) *LH(liab)/GTA*	(4) *LH(off)/GTA*
Negative bank sentiment (IV)	28.359***	7.040***	20.013***	1.306**
	(10.57)	(4.37)	(14.11)	(1.96)
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes
Number of obs.	34330	34330	34330	34330

Panel E: Second-stage regressions of the price of bank loans and deposits on *Negative bank sentiment* instrumented by LASSO-chosen instrument variables for banks operating in multiple states

	Second Stage				
	(1) *Term loans Credit spreads*	(2) *Revolvers Credit spreads*	(3) *Checking accounts Deposit spreads*	(4) *Savings account Deposit spreads*	(5) *Money market accounts Deposit spreads*
Negative bank sentiment (IV)	117.463**	19.301	252.274**	421.193***	326.770***
	(2.08)	(1.08)	(2.34)	(5.31)	(4.46)
Controls	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes	Yes
City FE	No	No	Yes	Yes	Yes
Number of obs.	8761	24523	7968	34265	83875

Table 2.11 Descriptions of variables of main sample

This table presents descriptions of the dependent and key independent variables for the main analysis. The sample includes 2,965 banks (57,841 bank-quarter observations) from 1993:Q4 through 2016:Q4. The observations are on a bank-calendar quarter level. All dollar values are adjusted to real 2016 values using the implicit GDP price deflator. All control variables except macro variables are winsorized at 1% and 99% levels.

Variable	Description
Dependent variables	
LH(total)/GTA	A bank's total liquidity hoarding measure including on- and off-balance sheet activities normalized by the gross total assets of a bank: *LH(total)* = *LH(asset)+LH(liab)+LH(off)*.
LH(asset)/GTA	A bank's liquidity hoarding measure in the asset-side, defined as (+1/2)×all items of liquid assets + (-1/2)×all items of illiquid assets normalized by the gross total assets of a bank. For a more detailed definition of all items belonging to liquid and illiquid assets, see Table 2.1.
LH(liab)/GTA	A bank's liquidity hoarding measure in the liability-side, defined as (+1/2)×all liquid liabilities normalized by the gross total assets of a bank. For a more detailed definition of all items belonging to liquid liabilities, see Table 2.1.
LH(off)/GTA	A bank's liquidity hoarding measure in the off-balance sheet-side, defined as (+1/2)×all items of illiquid guarantees + (-1/2) × all items of liquid derivatives normalized by the gross total assets of a bank. For a more detailed definition of all items belonging to liquid derivatives and illiquid guarantees, see Table 2.1.
Key independent variables	
Negative bank Sentiment	The ratio of the difference between the number of negative words minus positive words to total number of words in a bank's annual reports (form 10-K) based on the Loughran and McDonald (2011) dictionary of sentiment words.

Control variables

Ln(GTA)	The natural logarithm of the *GTA* of a bank defined as the total asset + allowance for loan and lease losses + allocated transfer risk reserve (a reserve for certain foreign loans) in $1000.
Capital ratio	The total equity capital as a proportion of *GTA* for each bank.
Earnings	Bank return on assets (ROA), measured as the ratio of the annualized net income to *GTA*.
HHI	A bank-level competition level calculated as a weighted average of the Herfindahl–Hirschman index in all areas (Metropolitan Statistical Area (MSA) or counties, if not included in MSA) in which a bank has a business. For each bank, the proportion of deposits in each area is used as weights.
Population	A bank-level population index calculated as the natural log of a weighted average of the population (in millions) in all areas (Metropolitan Statistical Area (MSA) or counties, if not included in MSA) in which a bank has a business. For each bank, the proportion of deposits in each area is used as weights.
Tobin's Q	A state-level cross-sectional average of normalized Tobin's Q defined as a firm-level Tobin's Q in quarter *t* normalized by a lagged total asset of each firm in the Compustat data whose headquarters are located in a corresponding state. Tobin's Q is defined as the market value of assets divided by the book value of assets (Compustat Item 6). A firm's market value of assets equals the book value of assets plus the market value of the common stock less the sum of the book value of common stock (Compustat Item 60) and balance sheet deferred taxes (Compustat Item 74).
Cash flows	A state-level cross-sectional average of operating cash flows for each firm in quarter *t* divided by lagged total assets of each firm in the Compustat data whose headquarters are located in a corresponding state. Cash flow is calculated as the sum of earnings

	before extraordinary items (Compustat Item 18) and depreciation (Compustat Item 14).
Corporate sentiment	Corporate manager sentiment index from Jiang et al. (2019)
Investor sentiment	Investor sentiment index from Baker and Wurgler (2006)
Consumer sentiment	The Consumer Sentiment Index by the University of Michigan
EPU (Economic Policy Uncertainty)	The natural log of the arithmetic average of the overall economic policy uncertainty measure developed by Baker et al. (BBD 2016) over the three months of calendar quarter *t*.

Table 2.12 The effects of bank sentiment on selected categories

This table presents coefficient estimates from regressions of selected bank balance sheet and off-balance sheet categories on the *Negative bank sentiment* measure and controls. The sample includes 2,965 banks from 1993:Q4 through 2016:Q4. All variables are described in Table 2.11. Coefficients on constant terms are omitted for brevity. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank holding company and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1) Cash/GTA	*(2) Loans/GTA*	*(3) Loan cmt./GTA*	*(4) Deposits/GTA*	*(5) Liquid deposits/GTA*
Negative bank sentiment	1.454***	-1.332**	-1.392***	3.270***	4.875***
	(4.02)	(-2.27)	(-3.19)	(7.48)	(5.11)
Ln(GTA)	-0.003	0.019***	0.010	-0.044***	-0.049***
	(-1.12)	(2.64)	(0.93)	(-5.18)	(-4.78)
Sqr.Ln(GTA)	-0.000*	0.000	0.000	0.001**	0.001***
	(-1.88)	(1.08)	(0.49)	(2.05)	(3.10)
Capital ratio	-0.081	-0.119	0.042	-0.302***	0.314***
	(-1.34)	(-1.21)	(0.39)	(-4.01)	(2.74)
Earnings	-0.100***	0.075	0.140*	0.032	0.314***
	(-3.34)	(1.66)	(1.66)	(0.67)	(4.71)
HHI	-0.003	0.003	0.018**	0.021**	0.028**
	(-0.67)	(0.27)	(2.06)	(2.37)	(2.22)
Population	-0.001	0.015	0.012	0.004	0.013
	(-0.23)	(1.64)	(0.79)	(0.43)	(1.06)
Tobin's Q	0.002***	-0.003***	-0.001	0.002**	0.004**
	(3.54)	(-2.91)	(-1.62)	(2.57)	(2.44)
Cash flows	0.030**	-0.069**	-0.062**	0.113***	0.126***
	(2.38)	(-2.06)	(-2.12)	(4.02)	(2.83)
Corporate sentiment	-0.001	0.007***	0.002	-0.000	-0.006
	(-1.38)	(3.15)	(1.03)	(-0.13)	(-1.57)
Investor sentiment	-0.002**	0.003*	0.003	-0.003	-0.009**
	(-2.30)	(1.68)	(1.49)	(-1.33)	(-2.56)
Consumer sentiment	0.000	-0.000	0.000***	-0.000	0.001**
	(0.52)	(-1.57)	(3.27)	(-0.01)	(2.53)
EPU	0.018***	-0.040***	-0.031***	0.005	0.025
	(4.48)	(-4.89)	(-4.86)	(0.65)	(1.60)
Bank FE	Yes	Yes	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.546	0.785	0.912	0.791	0.770
Number of obs.	57841	57841	57841	57841	57841

Table 2.13 Regressions of negative and positive words in 10-K

This table presents coefficient estimates from regressions of the bank liquidity hoarding on the fraction of negative and positive words and controls. For the dependent variables, we consider total bank liquidity hoarding (*LH(total)*) normalized by the gross total asset (*GTA*). The sample includes 2,965 banks (57,841 bank-quarter observations) from 1993:Q4 through 2016:Q4. All variables are described in Table 2.11. Coefficients on constant terms are omitted for brevity. *t*-statistics are reported in parentheses and are based on standard errors clustered at a bank holding company and year-quarter level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1) *LH(total)/GTA*	(2) *LH(total)/GTA*	(3) *LH(total)/GTA*
Negative only words	5.244***	5.299***	5.171***
	(8.27)	(8.50)	(4.20)
Positive only words	-5.089***	-5.070***	-3.373*
	(-7.07)	(-7.20)	(-1.96)
Ln(GTA)	-0.045***	-0.043***	-0.039***
	(-13.56)	(-12.59)	(-4.33)
Sqr.Ln(GTA)	0.000	0.000	0.000
	(1.48)	(1.03)	(0.74)
Capital ratio	-0.560***	-0.557***	-0.461***
	(-7.32)	(-7.38)	(-4.34)
Earnings	0.124**	0.121***	0.126**
	(2.58)	(2.74)	(2.50)
HHI		-0.003	-0.002
		(-0.46)	(-0.10)
Population		-0.018***	-0.013
		(-5.54)	(-1.31)
Tobin's Q		0.003	0.007***
		(1.27)	(4.09)
Cash flows		0.154***	0.168***
		(3.29)	(3.32)
Corporate sentiment			-0.007
			(-1.64)
Investor sentiment			-0.011***
			(-2.96)
Consumer sentiment			0.001**
			(2.36)
EPU			0.068***
			(4.04)
Bank FE	Yes	Yes	Yes
Seasonal FE	Yes	Yes	Yes
Adj. R-squared	0.805	0.806	0.820
Number of obs.	57841	57841	57841

Table 2.14 Description of variables for bank supply/demand sample

This table presents a description of the variables used in bank supply/demand choices versus customer choices analyses. The observations are at the credit facility–bank level from 1993:Q4 through 2016:Q4.

Variable	Description
Bank loan variables	
Credit spread	The all-in spread drawn defined as the borrowing spread and annual fee (if any) the borrower pays in percentage over LIBOR or LIBOR equivalent for each dollar drawn down.
Credit size	Loaned amount scaled by the borrower's total asset.
Ln(Maturity)	The natural log of the loan maturity (in months) from the credit facility's issue date.
Secured	A binary variable equal to one if a credit facility is secured by collateral and zero otherwise.
Covnt. Index	Covenant intensity index based on Bradley and Roberts (2015), which is defined as the sum of all covenants embedded in the loan (i.e., two or more restricted accounting ratios, secured loans, dividend restriction, asset sweep, debt sweep, equity sweep).
Term loans	Credit types in the LPC DealScan data: Term Loan, Term Loan A, Term Loan B, Term Loan C, Term Loan D, Term Loan E, Term Loan F, Term Loan G, Term Loan H, Term Loan I, or Delay Draw Term Loan.
Revolvers	Credit types in the LPC DealScan data: Revolver/Line < 1 Yr or Revolver/Line ≥ 1 Yr.
Borrowing firms variables	
Ln(ME)	The natural log of the market value of a firm defined as the number of outstanding shares (in 1,000) multiplied by the market price per share.
BE_ME	The book value of equity defined as the total stockholder's equity plus deferred taxes and investment tax credit minus preferred stock value divided by the market value of a firm.
Leverage	Total debt (short-term debt + long-term debt) divided by total assets.

Tangible	Net property, plant, and equipment divided by the total assets.
Cash	Cash and short-term investment divided by total assets.
Profit	The ratio of EBITDA to sales.
Credit rating	A credit rating score ranging from zero (for C or below) to 20 (for AAA) with an increment of one for each rating category based on an issuer's long-term S&P credit rating.
Bank deposit variables	
Checking accounts deposit spreads	Checking account deposit spread defined as checking account rate minus 3-month T-bill rate. Checking account rate is defined as the average rate of same checking account products across all balances requirements in percentage.
Savings accounts deposit spreads	Savings account deposit spread defined as savings account rate minus 3-month T-bill rate. Savings account rate is defined as the average rate of same savings account products across all balances requirements in percentage.
Money market accounts deposit spreads	Money market account deposit spread defined as money market account rate minus 3-month T-bill rate. Money market account rate is defined as the average rate of same money market account products across all balances requirements in percentage.

Chapter 3.Bank Public Status and Mortgage Lending Discrimination [19]

Abstract

Comparing private banks that went public through mergers and acquisitions with similar private banks, we find that going public reduces the mortgage denial rates for African American borrowers by 6.1-6.3 percentage points. The results are not driven by changes in borrower risk characteristics, lender risk preferences, securitization, or increased disclosure requirements. The effect is more pronounced in areas suffering from stronger racial biases. Our results suggest that the dispersed ownership can mitigate biased preferences of concentrated private ownership and thereby alleviates taste-based discrimination in mortgage lending.

Introduction

The finance industry, or Wall Street, was blamed for economic inequality and social injustice in the aftermath of the financial crisis in 2008 and 2009. The anti-finance sentiment, manifested in the "Occupy Wall Street" movement in 2011, depicted the finance industry as greedy and corrupt that caused not only economic chaos but also socialinequality. Finance has long been perceived as a rent-seeking activity even before the financial crisis (Zingales 2015). In sharp contrast, the academics argue that finance plays crucial roles in our society as it can benefit firms and leads to economic growth and even good social outcomes (Jayaratne and Strahan 1996; Levine and Zervos 1998; Guiso et al.

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[19] Ma, X., Y. Chu, and T. Zhang. To be submitted to *Journal of Finance*.

2004; Levine 2005; Beck et al. 2010; Levine and Rubinstein 2013; Levine et al. 2014). Zingales (2015), however, argues that the benefits of finance may be inflated. In particular, this literature argues that there is little evidence on the positive effect of equity markets on economic growth, let alone on social outcomes. In this paper, we attempt to provide some evidence on the social impact the public equity markets can bring to society.

Specifically, we examine whether publicly traded banks are more or less likely to discriminate against minority mortgage borrowers than private banks. On the one hand, Becker (1957) argues that discrimination serves the ideological preferences of the discriminating entity and is a reflection of racial prejudice. Becker's theory suggests that taste-based dis- crimination leads to lower profits for the discriminators (see also Epstein 1995). Furthermore, Becker (1981), Thaler and Shefrin (1981), and Stulz (1988) suggest that private and concentrated ownership could expose firms to agency problems and ideological biases of the limited number of private owners.[20] Private banks with a limited number of owners are more likely to impose their ideological preferences on lending policy and credit culture at the expense of bank profits. These ideological preferences are costly because banks have to forego profitable lending opportunities. When a bank becomes public, however, more dispersed ownership can moderate group decisions and shift lending policy towards profit-maximization. [21] As such, we expect that dispersed public ownership could mitigate taste-based discrimination.

[20] Based on this theoretical literature, conflicts of interests may arise because some non-economically motivated preferences can cause owners to take actions that threaten their own welfare as well as those around them. These agency problems with one-self (Jensen 1998) persist because the utility individuals gain from indulging personal tastes (e.g., racial prejudice and ideological bias) is functionally indistinguishable from that gained from rationally motivated pursuits (Thaler and Shefrin 1981; Becker and Murphy 1988).

[21] The literature in economics and social psychology suggests that team diversity with more disperse authority moderates group decisions (Sah and Stiglitz 1986, Sah and Stiglitz 1991).

On the other hand, the separation of ownership and control is the main source of agency costs in firms, and private firms may have a better alignment of incentives and therefore have a stronger incentive to maximize profit (e.g., Fama and Jensen 1983; Jensen 1986). If not dominated by costly racial prejudice, private banks could be less likely to discriminate than public banks. Furthermore, shareholders' value-maximization incentives could also encourage firms to engage in statistical discrimination under which the agent uses race or ethnicity as a signal for applicants' credit risk even if the agent does not have a taste-based racial preference. Although statistical discrimination is illegal, lenders may still use statistical discrimination as a solution to the information-extraction problem when they do not have complete information about applicants. Because information acquisition is costly, statistical discrimination could lower the costs and improve profitability for banks. Therefore, when a bank goes from private to public, it can increase statistical discrimination due to shareholders' pressure to maximize profits. Given these competing theoretical predictions, how public status affects lending discrimination remains an empirical question.

It is often challenging to identify the impact of public status on firm behavior because the endogenous nature of public status. Directly comparing lending decisions of public banks with those of private banks is likely to be biased because the decision of going public can be related to lending decisions, and certain bank characteristics can be related to both public transition and bank lending. To alleviate this concern, we focus on private banks that went public through an acquisition by a public bank or a public bank holding company (BHC) as our treated banks. We then use a propensity score matching method and match treated banks with (private) control banks by year, headquarter state, and a large set of bank characteristics. We use the matching method to ensure that the treatment banks and control banks are similar in observable characteristics except for their public status.

In our baseline test, we use a triple-difference approach to identify the changes in mortgage lending to African American borrowers of treated banks and control banks, before and after the private to public transition (*African American* × *Treated* × *Post*). We show that mortgage denial rates for African American borrowers, relative to non-African Americans, drop by 6.1-6.3 percentage points (more than 19% of the average denial rate for African American borrowers or a third of the racial disparity in mortgage denial rates) after a bank becomes public. The results remain robust after including county × year fixed effects to remove the effect of any local conditions (such as credit demand) and the bank × year fixed effects to remove the effect of time-varying bank balance sheet changes. In the most restrictive specification, we also include bank × county fixed effects to control for the interaction effect of a bank-county pair, such as banks' superior information in their headquarter counties. To ensure that the results are truly driven by bank public transition but not systematic differences between treated and control banks, we explore the timing of the effect of going public on lending discrimination. Consistent with the parallel trend assumption, we find that the effect only appears after, but not before, the treated banks become public, suggesting that the results are not driven by systematic differences between treated and control banks.

To examine discrimination at the intensive margin, we use the loan performance data of mortgages sponsored by Fannie Mae and Freddie Mac. Using loans securitized by government sponsored entities (GSEs) also mitigates the concern of the omitted variable bias, the classic criticism in the lending discrimination literature, because the data contains a comprehensive set of borrower and loan characteristics, such as the credit score and loan-to-value (LTV) ratio. After controlling for the comprehensive set of borrower and loan characteristics, we find that banks lower the home-purchase mortgage rate by 10.5 bps for African American borrowers after they become public.

The results above can be driven by changes caused by the merger deal rather than changes in the private-to-public ownership. For example, merger-related considerations, such as gaining market share, lowering operating costs, and scaling investments more efficiently, could all affect lending decisions. To mitigate this concern, we use two alternative empirical strategies. First, we use public banks acquired by other public banks or public BHCs as the control group. By restricting both treated and control groups to be target banks in acquisitions, we can largely control for changes in the lending policy driven by the merger or acquisition deal. Second, we conduct a within-deal analysis by comparing the private target bank with the public acquiring bank in the same deal. This strategy largely controls for heterogeneity of bank mergers and mitigates the concern that certain unobservable variables that determine the merger deal can drive our results of lending discrimination. We find qualitatively and quantitatively similar results in both tests, suggesting that our baseline results are mostly driven by the private-to-public-ownership transition rather than determinants of the merger deal decision.

An alternative explanation to our baseline results is that, after going public, banks are more likely to be matched with low-risk African American borrowers, or low-risk African American borrowers that self-select into public banks. To address this concern, we examine the loan risk measures and find consistent results that African American-borrower loans at public banks are even riskier than those at private banks. An average loan issued to African American borrowers by public banks has a 2.3-percentage-point higher loan-to-value ratio and a 4.7-percentage-point higher debt-to-income ratio than those by private banks. These results suggest that the lower denial rates and lower loan rates are unlikely to be driven by banks matching with high-quality African American borrowers.

We next examine the main economic mechanism and show that dispersed ownership of public firms is likely to mitigate the ideological bias of a limited number of private owners and hence is able to alleviate taste-based discrimination. We first split the sample according to the headquarters location of banks into banks headquartered in the southern and other states. Our results show that the effect is concentrated among banks located in the southern states where racial biases are more prevalent. Next, motivated by recent studies (Butler et al. 2020; Buchak and Jørring 2021) that show that market competition can mitigate taste-based discrimination, we find that the effect is indeed weaker in areas where banking competition is more intense. Overall, these results suggest that dispersed ownership of public banks mitigates taste-based discrimination.

Finally, we address several alternative explanations. First, we examine whether our results are driven by changes in risk preferences after the private-to-public transition (Falato and Scharfstein 2016). We show that the bank's overall mortgage portfolio does not experience a significant increase in risk after the transition to public ownership, suggesting that our results of reduced discrimination is not driven by changes in risk aversion of banks' overall mortgage portfolios. We also examine whether our results are driven by changes in underwriting standards as the target banks adopt the underwriting standards of the acquirers. We follow the literature (Ross and Yinger 2002) and explicitly control for changes in underwriting standards and find that our results remain robust. We then examine whether securitization can drive our finding. We find that the likelihood of an African American- borrower loan being securitized does not change after the public transition. Furthermore, we examine whether the effect is driven by increased disclosure (Christensen et al. 2017). We use the bank opacity measure developed by Jiang et al. (2016) and find that the effect is similar for the two subsamples of transparent and opaque banks before going public, suggesting that the baseline results are unlikely to be driven by increased disclosure.

Our study contributes to a growing literature on lending discrimination in residential housing markets. To address the notorious omitted-variable bias, Munnell et al. (1996), famously known as the Boston Fed study, collect and control for a large set of characteristics and still find that mortgage applications from minorities are more likely to be denied. Most later studies find similar results (Stengel and Glennon 1999; Harrison 2001; Calem and Longhofer 2002; Charles and Hurst 2002; Bayer et al. 2017). Ghent et al. (2014) and Reid et al. (2017) examine subprime loans and show that minority borrowers face higher interest rates and other predatory lending practices. Bayer et al. (2018) find that minority borrowers are more likely to receive high-cost mortgages than other borrowers. Cheng et al. (2015) also show that black borrowers pay more than comparable white borrowers. Bhutta and Hizmo (2021) find statistically significant gaps by race and ethnicity in interest rates, but they show that these gaps are offset by differences in discount points. Fuster et al. (2020) analyze how algorithms are utilized in US mortgage markets and show that the use of machine-learning techniques to evaluate credit quality may lead to differential impact on loan provision across racial and gender groups. Bartlett et al. (2019) show that FinTech algorithms in the mortgage market lead to higher interest rates for minority borrowers, although rejection rates are lower relative to traditional lending. Buchak and Jørring (2021) analyze the effect of local concentration on lending discrimination and find that mortgage rejection rates and mortgage fees are both higher for minority borrowers in more concentrated areas.

Existing studies also find evidence of discrimination in other credit markets such as small business (Blanchflower et al. 2003), credit card and entrepreneurship (Chatterji and Seamans 2012), and auto loan market (Butler et al. 2020). Begley et al. (2018) examine the quality of consumer credit services and show that the level of complaints is significantly higher in markets with lower income and educational attainment, and especially in areas with a higher share of minorities, even after controlling for income and education. Avenancio-Leon and Howard (2020) study assessment accuracy and uniformity on existing property tax and racial disparities. Our paper contributes to this strand of literature by showing that banks' listing status can mitigate lending discrimination because diffuse ownership can reduce ideological biases and hence taste-based discrimination.

Our paper also contributes to the literature on differences between public and private firms. Brav (2009) shows that cheaper public equity can be an advantage for public firms. Stein (1989) and Falato and Scharfstein (2016) show that public firms are subject to short- term pressures and are more prone to myopic behavior. Gao et al. (2013) show that higher agency costs in public firms lead to more cash holing than private firms. Slutzky (2021) shows that public firms could suffer regulatory costs in emerging countries. Mortal and Reisel (2013), Asker et al. (2015), Gilje and Taillard (2016), and Phillips and Sertsios (2017) show that public firms differ from private firms in their ability to invest in new opportunities. Bernstein (2015) shows that public listing can affect how firms innovate, and Acharya and Xu (2017) show that the effect depends on firms' reliance on external capital. Investigating the role of ownership structure, Michaely and Roberts (2012) show that dispersed ownership and incentives, as well as the scrutiny of the public capital markets, have a significant impact on payout policy. Examining firm pollution externalities, Shive and Forster (2020) find that public firms emit more greenhouse gases than similar private firms to reduce the costs of compliance with environmental regulation. Gilje and Wittry (2021) find that workplace safety deteriorates under on payout policy. Examining firm pollution externalities, Shive and Forster (2020) find that public firms emit more greenhouse gases than similar private firms to reduce the costs of compliance with environmental regulation. Gilje and Wittry (2021) find that workplace safety deteriorates under public firm ownership due to information asymmetry between managers and shareholders of public firms. We contribute to this literature by showing that dispersed ownership can mitigate costly discriminatory lending driven by private owners' ideological biases.

Data and sample construction

Mortgage and bank balance sheet data

We obtain data on mortgage applications from the HMDA data. The sample covers loan applications from 1990 to 2016. All regulated financial institutions with more than $30 million in assets, such as commercial banks, credit unions, and mortgage companies, must report the data. The HMDA data contains the lender identity, location of the property, dollar amount of the loan, application year, whether the loan was approved, and whether the loan is sold to a third party during the year of origination. Borrower information, such as borrowers' reported income, race, and gender, is also provided. From the original data, we discard non-conventional loan applications (Federal Housing Administration-insured, Veterans Administration-guaranteed, Farm Service Agency, or Rural Housing Service loans), applications with incomplete race or location information, applications with a home improvement purpose, and applications for unusual products (manufactured housing or multi- family dwellings).[22] We use home purchase loans in our main sample and provide results on refinancing loans in Table 3.12.

We include all financial institutions that file the Reports of Condition and Income for commercial banks (Call Reports) - commercial banks regulated by the Federal Reserve Bank (FRB), Office of the Comptroller of the Currency (OCC), or the Federal Deposit Insurance Corporation (FDIC), thrifts and credit unions (we call them "banks" in this paper). [23] Using the lender identity, we merge the HMDA data with the bank-level data from Call Reports. We merge each loan application with the Call Report as of the fourth quarter of the year immediately before the mortgage application. [24] All other institutions from the HMDA dataset are then matched manually using the bank's name and location information. Using data from Call Reports, we construct bank balance sheet variables including bank size (log of total assets), capital ratio (scaled by total assets), return on assets, non-performing loans (scaled by total loans), and net interest margin (scaled by total assets). The summary statistics for our loan-level sample are presented in Table 3.1. On

[22] The information about property type in the HMDA data is available after 2004.
[23] We exclude non-commercial bank lenders such as mortgage companies.
[24] To merge with the HMDA bank identification number, we use the Call Report identification number (RSSD ID) for banks regulated by the Federal Reserve (FR), the Federal Deposit Insurance Corporation (FDIC) certificate ID (item RSSD9050 in the Call Report) for banks regulated by the FDIC, with the Office of the Comptroller of the Currency (OCC) ID (item RSSD9055 in the Call Report) for banks regulated by the OCC.

average, about 17.2% of the loan applications are denied for all purchase loans. The average denial rate for African American borrowers is 32.0%.

We obtain mortgage contractual rates and other loan terms from the Freddie Mac single family loan-level dataset and the Fannie Mae single family loan-level dataset. Fannie Mae and Freddie Mac loan-level datasets start from 2000 and 1999, respectively. In our analyses, we merge these datasets with HMDA data based on detailed information on approved loan characteristics, such as rate spread, lien status, and property type. To merge HMDA data with Fannie Mae/Freddie Mac data, we use 2004 as the starting point because HMDA data start reporting these variables in 2004. From the merged dataset, we obtain mortgage rates and measures of credit risk such as the loan-to-value ratio, the debt-to-income ratio, the FICO score, and other loan characteristics such as the occupancy status, mortgage insurance information, loan size, and the co-borrower status.

Bank public status transition data

Our treated bank sample consists of commercial banks or BHCs that completed a private- to-public transition during 1990-2016 in the United States. In our baseline analysis, we focus on private banks acquired by a publicly traded BHC or bank.[25] We obtain the merger and acquisition (M&A) deals during the sample period from the Thomson Reuters' SDC database. We restrict our sample to financial institutions with the SIC code from 6000-6999. Using this procedure, we identify 555 banks or BHCs going public through

[25] The primary difference in whether a purchase is considered a bank acquisition or merger is whether or not the target maintains its branded corporate identity to its customers and other stakeholders. The target bank retains brand identity and is technically still a separate bank. The only difference is that the target is owned by the acquiring bank holding company. In contrast, a bank merger erases the target banks identity, and the target bank is absorbed into the acquiring bank and then converted into branch offices. The target bank loses its bank charter, its management team and its board. In our sample, we keep both acquisitions and mergers because they are both consistent with our study purpose.

mergers or acquisitions, which serve as the treatment sample in our test. To construct the control sample, we start with all private banks that remain private throughout the sample period.[26] We then use a propensity score matching method to find control banks. In particular, we estimate the propensity score model using the following bank characteristics: size (log of assets), capital ratio, performance (ROA), asset quality (non-performing loan), and profitability (net interest margin) to calculate the propensity score and find the matched control banks head- quartered in the same state and in the same year.[27] By matching on the headquarters state, we can control for the local factors such as credit demand, competition, cultural and social values; by matching on year, we account for changes in macroeconomy over time. In our baseline sample, we obtain 1,227 banks or BHCs in our control group.

Table 3.2 compares the treatment and control banks. Among all the bank balance sheet variables, only the percent of non-performing loans (scaled by total assets) is significantly different across the two samples at the 1% level. Capital ratio, deposit ratio, ROA, and real estate-related non-performing loans are only marginally different. All other variables are similar across the two samples.

As the first step of our empirical analysis, we plot the denial rates for treated and control banks before and after the private-public transition, as shown in Figure 3.1. Treated banks lower the denial rates of African American applications after treatment, and the effect is not present for non-African American borrowers or for control banks.

[26] We use the CRSP-FRB link provided by the Federal Reserve Bank of New York to identify banks' public or private status.

[27] To check robustness of our results, we also use different sets of bank characteristics such as bank assets only, and our results are not sensitive to the selection of these characteristics.

Bank acquisition and lending discrimination

In this section, we first describe the econometric model and the baseline results, we then investigate the economic mechanisms underlying the decreased lending discrimination after a bank transition from private to public. In addition, we present cross-sectional results to show that concentrated ownership and ideological biases both play a role in driving private banks' discriminatory lending.

Baseline results

<u>Extensive margin</u>

To identify the impact of the treatment on denial rates, we use the following triple-difference specification:

$$\begin{aligned} Denied_{i,c,b,t} = {} & \beta_1 African\ American_i \times Treated_b \times Post_{b,t} + \\ & \beta_2 African\ American_i \times Treated_b + \beta_3 Treated_b \times Post_{b,t} + \\ & \beta_4 African\ American_i \times Post_{b,t} + \beta_5 African\ American_i + \\ & \beta_6 Treated_b + \beta_7 Post_{b,t} + \beta_8 X_i + \alpha_{b,t} + \theta_{c,t} + \gamma_{b,c} + \varepsilon_{i,c,b,t}, \end{aligned} \tag{3.1}$$

where *i* indexes mortgage applications, *c* indexes a borrower's county, *b* indexes bank, and *t* indexes year. *Denied* is a dummy variable that equals one if the loan application is denied, and zero otherwise. *African American* is a dummy variable that equals one if the borrower is African American, and zero otherwise. *Treated* is a dummy variable that equals one if the bank becomes public through an acquisition or merger, and zero otherwise. *Post* is a dummy variable that equals one if the year is in or after the bank becomes public, and zero otherwise. *X* is a set of borrower controls, including applicant income, loan size, the percentage of African American borrowers in a census tract, and four dummy variables indicating whether the applicant is Asian, White, female, or has a co-borrower. In our analyses, we include different sets of fixed effects in the specification: (i) county and year fixed effects; (ii) $\alpha b, t$ bank × year fixed effects, to control for time-varying bank balance sheet effects; (iii) $\theta c, t$, county × year fixed effects to control for any credit demand factors within a county; and (iv) $\gamma b, c$, bank × county fixed effects to control for any bank-county interaction effects.

Table 3.3 presents the results of estimating the baseline specification (Equation (3.1)) on the matched sample. All regressions are based on the home purchase loan sample. Across all columns the coefficient estimates on the triple interaction term are negative and statistically significant, suggesting that denial rates of African American borrowers decrease at treated banks, relative to non-African American borrowers and matched control banks. Among our preferred specifications in Columns (3)-(5), the magnitudes of the coefficient on the triple interaction range from -0.061 (Column (3)) to -0.063 (Column (4)). In Column (3), the coefficient on the triple interaction term (*African American* × *Treated* × *Post*) is -0.061, suggesting a 6.1 percentage point decline in denial rates for African American borrowers, which amounts to 19.06% of the average denial rates of African American borrowers in our sample (32.0%). In addition to county fixed effects, we control for bank × year fixed effects to fully account for time-varying changes in banks' balance sheets. In Column (4), we further include county × year fixed effects to control for credit demand changes that vary across geographic areas over time. The coefficient estimate remains similar as in Column (3). In Column (5), we check the robustness of our results by additionally including bank × county fixed effects to account for any interactive effect at the bank-county pair level, such as superior information in the headquarters market. The coefficient remains robust. Overall, the results in Table 3.3 suggest that, after a bank's private-to-public transition, the mortgage denial rate for African American borrowers decreases by around 6.2 percentage points.

The coefficient estimates on the double interaction term (*African American* × *Treated*) are small and statistically insignificant, suggesting that the treated and control banks do not appear different in lending to African American borrowers before the treatment. In addition, the small magnitude and insignificance of the coefficient estimates on *African American* × *Post* suggest that the matched control banks are not different before and after the event year. Furthermore, the coefficient estimates on *Treated* and *Post* are small and insignificant, suggesting that the treated and control banks are very similar on overall mortgage denial rates, before and after the treatment.

We use refinancing loans and run the same baseline test on denial rates and find a weaker effect, both economically and statistically (Table 3.12). This is consistent with the intuition that refinancing loans carry more payment history information and the underwriting process is more standard, while purchase loan borrowers act more urgently and shop less, leaving more discretion to lenders to extract rents due to less competition.

Dynamics

To ensure that the results are not driven by a pre-existing time trend of decreasing lending discrimination by treated banks or any systematic differences between treated and control banks, we examine the dynamics of the effect. In particular, we estimate the following specification:

$$Denied_{i,c,b,t} = \sum_{k=-6}^{6} \beta_1^k African\ American_i \times Treated_b \times Year^k + \beta_2 African\ American_i \times Treated_b + \beta_3 Treated_b \times Year^k + \beta_4 African\ American_i \times Year^k + \beta_5 African\ American_i + \beta_6 Treated_b + \beta_7 Year^k + \beta_8 X_i + \beta_9 Y_{b,t} + \alpha_{b,t} + \theta_{c,t} + \gamma_{b,c} + \varepsilon_{i,c,b,t}, \quad (3.2)$$

where $Year^k$ equals one if the application occurred k years before or after the private-to- public transition, and zero otherwise. If there is a dynamic trend even without going public, β_1^k will be negative for $k < 0$, that is, before the going public attempt. On the other hand, if the effect is driven by going public, only β_1^k for $k \geqslant 0$ will be negative.

We then plot the triple difference coefficient estimates and their confidence intervals of the β_1^k's in Figure 3.2. Before going public ($k < 0$), all the triple coefficient estimates are small and statistically insignificant. In contrast, the estimates are mostly negative and statistically significant after going public ($k > 0$). The results suggest that the baseline effect documented in Table 3.3 is unlikely to be driven by pre-existing trend differences.

<u>Intensive margin</u>

In this subsection, we examine the effect of a public transition on mortgage interest rates for African American borrowers. Because the HMDA data do not have information on interest rates for all approved mortgages, we merge the HMDA data with the Fannie Mae and Freddie Mac loan performance data to retrieve information about mortgage interest rates. [28] We follow the literature (e.g., Sun and Gao 2019) and match the two datasets using key loan characteristics and delete any duplicates.

To merge HMDA data with loan performance data, we follow Sun and Gao (2019) and take advantage of additional loan information from HMDA such as the property type and the lien status.

Fannie Mae loan performance data collection began in 2000, and Freddie Mac loan performance data collection began in 1999. Since 2004, the HMDA data has included more detailed information on approved loan characteristics, such as rate spread, lien status (first, second, or non-secured), and property type (e.g., single-family, manufactured homes). To merge HMDA data with Fannie Mae/Freddie Mac data, we use 2004 as the starting point and exploit the expanded information from HMDA that is common in Fannie Mae/Freddie Mac data to implement the merging process.

From HMDA, we first remove incomplete or withdrawn loan applications. We then re- move home improvement loans. We only keep conventional, first-lien loans sold to Fannie Mae or Freddie Mac. We match with loan performance data by multiple categories including:

(1)location information (i.e., state, MSA, county);

(2)time information (i.e., origination year);

(3)loan characteristics (i.e., loan amount, loan purpose - purchase or refinancing, owner- occupancy, property type, and the presence of a co-borrower).

[28] Starting in 2004, the HMDA data provide a pricing variable, rate spread, which is the spread (difference) between the annual percentage rate (APR) on a loan and the rate on Treasury securities of comparable maturity, but only for loans with spreads higher than the designated thresholds. Given this new definition, disclosure is required if a rate spread is above 1.5%. Under these requirements, the rate spread variable is only available for a very small sample of loans.

County information for each loan in the Fannie Mae and Freddie Mac data is identified by its first three digits of the reported zip codes. We obtain zip code-county crosswalk data from HUDs Office of Policy Development and Research (PD&R). To minimize matching errors, we only retain one unique matched datapoint for each pair (i.e., we delete duplicates after matching).

Contractual interest rates are reported for all loans purchased by Fannie Mae and Freddie Mac, which allows us to examine the impact on mortgage contractual rates.[29] Furthermore, the data provide additional loan and borrower characteristics. For example, the data provide information on the borrower's FICO credit score, the debt payment to income ratio (DTI), the mortgage insurance percentage, the loan-to-value ratio (LTV), and the combined loan- to-value ratio (CLTV). These additional variables can reduce the potential omitted variable bias and improve estimation accuracy.

To estimate the effect of banks' public transition on mortgage rates, we replace the dependent variable in equation (3.1) with the contractual rates reported in the Fannie Mae and Freddie Mac data sets and re-run the specification. In addition to the HMDA control variables in the baseline equation, we also add a comprehensive set of control variables provided by the loan performance data, including CLTV, DTI, FICO, mortgage insurance percentage, the occupancy status, first-time homebuyer status, co-borrower status, and the borrower's race and gender.

The results are presented in Columns (1) and (2) of Table 3.4. In Column (1), we include multiple sets of fixed effects to account for time-varying changes in credit supply (bank × year-month fixed effects) and credit demand (county × year-month fixed effects).

[29] One potential concern is that the contractual rates do not capture the impact of origination points and fees (e.g., Bhutta and Hizmo 2021).

In Column (2), we additionally include bank-county interaction effects (bank × county fixed effects) to account for any interaction effects such as superior information in the bank's headquarter county. After controlling for a comprehensive list of borrower and loan variables in both columns, the triple-interaction coefficient estimate is negative and statistically significant at the 1% level, suggesting that public banks reduce the mortgage interest rates by around 10.5 basis points for African American borrowers.

We expand our analysis by examining the implications of reduced lending discrimination on homeownership for African American borrowers. We use the information on home occupancy status from HMDA and Fannie Mae/Freddie Mac datasets and first-time home buyer status from Fannie Mae/Freddie Mac datasets and use these two measures for homeownership. We find that after the private-to-public transition, banks increase credit supply to African American first-time home buyers by 13.7%.

Changes in credit risk

The lower denial rates and lower interest rates for African American borrowers approved by public banks may be driven by changes in risk characteristics of African American borrowers applying to public banks. This concern is valid if banks that have transitioned from a private to public status disproportionately attract more low-risk African American borrowers. To mitigate this concern, we use the Fannie Mae and Freddie Mac loan data and examine the loan risk characteristics measured by various variables such as the loan-to-value ratio of the loan and the debt-to-income ratio of the borrower. We re-estimate baseline equation (3.1) by replacing the dependent variable with either of the risk measures.

The results are presented in Columns (3)-(4) of Table 3.4. Across these columns, the coefficient estimates on the triple difference term are positive and statistically significant at least at the 5% level. For example, Column (3) shows that loans to African American borrowers issued by treated banks (and after it becomes public) have, on average, a 2.28% higher loan-to-value ratio than before the bank becomes public. Column (4) suggests that the debt-to-income ratio of African-American borrower loans goes up by 4.73 % after the bank becomes public. These results suggest that public banks lend to riskier African American borrowers after going public. This finding therefore suggests that the results are unlikely to be driven by the aforementioned "lower risk" explanation. An associated concern according to this observation is that banks may chase more risky portfolios after going public (Falato and Scharfstein 2016), and we will address this concern later in Section.

Identification and robustness

Public target banks as the control group

A major concern of our baseline results is that the likelihood of banks being acquired is correlated with their lending schemes such as the extent to which they discriminate against African American borrowers. We address this possibility by selecting the control group as those banks that were already publicly listed before being acquired. The treated group remains the same as in the baseline sample, that is, banks that go through a private-to-public transition through an ownership acquisition. Because both treated and control groups are banks that have gone through an acquisition, we can control for the effect of factors that are correlated with the likelihood of being acquired. To ensure that the treated and control banks are comparable, we use the same propensity score matching procedure as in the baseline test: in each year, we select public target banks that have the same headquarter state and similar bank characteristics (bank size, capital ratio, ROA, non-performing loans, and net interest margin) to match with the treated banks.

We run the baseline equation (3.1) using this sample and report results in Columns (1)-(3) of Table 3.5. In Column (1), we include the full set of borrower and loan controls, and we control for county fixed effects and bank × year fixed effects; in Column (2), we additionally control for county × year fixed effects; in Column (3), our most restrictive specification, we also control for bank × county fixed effects, and the coefficient on the triple interaction term (*African American* × *Treated* × *Post*) of -0.090, suggesting a decline of 9 percentage points in denial rates for African American borrowers, which amounts to 28.1% of the average denial rates of African American borrowers in our sample (32.0%).

<u>Within-deal estimation</u>

To further address the concern that the determinants of an acquisition deal can be correlated with the (private) target banks' mortgage lending, we run a within-deal estimation by including the deal fixed effects for each acquisition deal. In this estimation, we compare the lending decision of the public acquirer and the private target of the same deal before and after the acquisition. With the deal fixed effects, we can largely control for the unobservable factors that drive both the acquisition decision and changes in the target's mortgage lending. We run the baseline equation (3.1) using this sample, and present the results in Columns (4)-(6) of Table 3.5. In these regressions, we add deal fixed effects to ensure that we compare the private target and public acquirer in the same deal. The coefficient estimate on the triple interaction term is negative and significant at the 5% level at least. The magnitude is robust across specifications with different sets of fixed effects.

The result suggests that going public is associated with a 5.4-5.7 percentage-point decline in denial rates for African American borrowers. The coefficient estimate on the double interaction term, *African American* × *Treated*, is positive and large (0.046), suggesting that the difference in the denial rate for African American borrowers between the public acquirer and the private target is significant.

Taste-based discrimination and public ownership

We hypothesize that private banks with concentrated ownership may be subject to discriminatory lending because of biased preferences of a small number of private owners (Becker 1981; Thaler and Shefrin 1981; Stulz 1988). When a public holding company acquires all or a portion of a private bank's ownership, the ownership structure, as well as the control and voting rights of the target bank will be diluted. With a merger deal, the target bank is absorbed into the acquirer and then converted into a branch office of the public bank or BHC. The dispersed owners of the newly merged public banks are unlikely to share the biased preferences of the private owners, and hence could mitigate taste-based discrimination. In particular, Becker (1957) suggests that discrimination serves the ideological preference of concentrated decision-making authority and is costly and dispersed value maximizing shareholders would try to correct the inefficiencies.

In our first set of the cross-sectional test, we test our hypothesis that the effect of a public transition would be more pronounced in areas where racial biases are likely to be more prevalent. We split the sample based on the headquarters location of banks into southern states and non-southern states.[30] Table 3.6 presents the results. The effect

[30] The southern states include Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, Tennessee, Virginia, and West Virginia.

concentrates among banks headquartered in the southern states (Columns (1)-(3)). In contrast, we do not find significant results for banks headquartered in other states (Columns (4)-(6)). Overall, our results in Table 3.6 are consistent with our argument going public alleviates taste-based lending discrimination.

Next, we proceed to examine how intense lending competition impacts the changes in lending discrimination. Banks facing tough competition cannot afford to make business decisions based on costly biased preferences, and hence would engage less in lending discrimination. Therefore, the private to public transition would have a smaller impact on banks' lending in more competitive markets (Becker 1957; Epstein 1995; Chu 2019; Buchak and Jørring 2021). To measure competition, we use the mortgage-lending Herfindahl-Hirschman Index (HHI), which is defined as the sum of squared lenders' market shares of issued mort- gages in a given county. We split our sample by the median HHI and re-estimate our baseline specification on the two sub-samples. our results show that the effect of public transition is stronger, both economically and statistically, if a bank lends in counties with less competition (Table 3.7). This finding is consistent with the long-standing view that discrimination as a reflection of ideological preference is costly (Becker 1957) and competition can mitigate the racial gap of credit access caused by taste-based discrimination.

Alternative explanations and robustness tests

Changes in risk preference

We first examine whether risk preference changes could drive our results as banks increase risk-taking after they become public. Falato and Scharfstein (2016) show that the pressure to maximize short-term stock prices and earnings leads banks to increase risk when they transition from private to public ownership. We therefore need to address the concern that the decrease in denial rates for African American borrowers may be driven by changes in lenders' risk preference and show that our results are not driven by systematic changes in lenders' risk preference across all borrowers.

To examine whether banks increase risk preferences in mortgage lending after a public status transition, we use two borrower characteristics – the applicant's income and loan- to-income (LTI) ratio – to measure loan risk, and examine whether public banks are more likely to approve loans with lower income or higher LTI ratios for all borrowers. We modify our baseline equation (3.1) by replacing the dummy variable, African American, with the applicant's income (log(income)) and LTI.

We report the results in Table 3.8. The results in Columns (1)-(3) suggest that banks do not accept more low-income mortgage loans following a public status transition, and this result is robust across specifications with different sets of fixed effects. The results in Columns (4)-(6) suggest that there is not a significant change in a bank's mortgage portfolio risk, measured by LTI, following a public status transition. Our results show that banks do not significantly increase their risk preference in mortgage lending to all borrowers, implying that our baseline result is unlikely to be driven by changes in risk preferences.

Changes in business models

In this subsection, we examine whether changes in business models can drive our results. Target banks could change their business models or underwriting standards in order to be integrated with their parent banks or holding companies after the acquisition deal. To mitigate this concern, we follow the methodology in Ross and Yinger (2002) to explicitly control for changes in underwriting standards. Specifically, in addition to bank × year fixed effects, we allow banks to put different underwriting weights on various loan, borrower, and lender characteristics. To do this, we first identify a key underwriting variable using information from the HMDA dataset – the loan-to-income ratio, we then identify a group of key lender portfolio variables that might reflect changes in banks' underwriting standards, including the conforming securitization ratio, average loan size, average applicant income, and average loan-to-income ratio. Next, we add the pair-wise interaction terms of these lender portfolio variables with the loan-to-income ratio to our baseline model of equation (3.1).

We run the baseline tests parallel to the ones in Table 3.3. The results, as presented in Columns (1)-(2) of Table 3.9, show that the triple difference coefficient estimates remain negative and statistically significant, suggesting that our baseline results are unlikely to be biased by the differences in underwriting standards across different banks.

Next, we examine whether securitization can drive our results. Banks may approve more loans because they increase their securitization rates after going public. We calculate a bank-year level measure of securitization rate using the ratio of securitization volume scaled by total volume of mortgages originated by the bank. In the baseline specification, we add this measure as a control variable and our results are not altered by the inclusion of this control. In a more restrictive regression, the inclusion of bank × year fixed effects can partially mitigate this concern as well. It is also possible that banks approve more loans from African American borrowers because they are more likely to securitize these loans after they go through a public transition. We calculate the securitization rate for loans of

African American borrowers and visualize these results in Figure 3.3.[31] For treated banks, the securitization rate for loans of African American borrowers is 25.4% before the treatment and 24.6% after the treatment.

For control banks, these rates are 22.7% and 25.3% before and after treatment. We do not find evidence that securitization increases significantly for African American loans or securitization is likely to drive the changes in loan origination decisions for African American borrowers. We also examine the changes in the percentage of GSE sponsored loans over all securitized loans and find that securitization through the GSEs does not increase significantly either (Figure 3.3 Panel B).

Disclosure and opaqueness

Our findings of decreased discrimination can be driven by increased disclosure requirements after a bank goes public (e.g., Christensen et al. 2017). To examine whether this is a potential underlying mechanism, we construct an opacity measure for each bank-year observation and examine whether the decreased discrimination is more pronounced for banks that are opaquer before going public.

To construct the opacity measure, we follow Jiang et al. (2016) and estimate the following specification:

$$LLP_{b,t} = \beta_1 dNPA_{b,t+1} + \beta_2 dNPA_{b,t} + \beta_3 dNPA_{b,t-1} + \beta_4 SIZE_{b,t-1} + \beta_5 dLOAN_{b,t} + \beta_6 CSRET_t + \beta_7 dGDP_t + \beta_8 dUNEMP_t + \varepsilon_{b,t} \tag{3.3}$$

where $LLP_{b,t}$ is loan loss provisions scaled by lagged total loans. $dNPA_{b,t}$ represents the change in nonperforming assets between year t and $t-1$, divided by total loans in year $t-1$ for bank b. We include both next period $dNPA_{b,t+1}$ and previous

[31] We obtain the securitization information from the HMDA data. HMDA reports whether a loan is sold during the year of origination.

$dNPA_{b,t-1}$ because banks might use forward-looking information on nonperforming assets and historical changes in nonperforming assets in deciding LLPs. $SIZE_{b,t-1}$ is the natural logarithm of total assets in year $t-1$. $dLOAN_{b,t}$ is the change in total loans over the year, divided by lagged total loans. We also include three economic measures that might influence LLP: the Case-Shiller Real Estate Index ($CSRET_t$), the change in gross domestic product ($dGDP_t$), and the change in the unemployment rate (dUNEMPt). We construct the proxy for the bank-year level opacity (i.e., discretionary LLPs) with the natural logarithm of the absolute values of the errors estimated from equation (3.3).

We then split the sample into two sub-samples by the median of the opacity measure before going public. We re-run the baseline regression in equation (3.1) on these two sub- samples. The results are presented in Columns (3)-(6) of Table 3.9. The triple difference coefficient estimates across both sub-samples are negative with similar magnitudes, suggesting that our baseline result is unlikely to be driven by increased disclosure. The magnitude of the coefficient estimate is even slightly larger for non-opaque banks (Columns (3)-(4)) than for opaque banks (Columns (5)-(6)).

Robustness tests

We conduct a battery of robustness tests to check whether our main results are sensitive to changes in the sample, the way we classify the treatment event, or the estimation methods. The results are reported in Table 3.10. Column (1) removes the financial crisis years of 2007 – 2010 to rule out the possibility that the results are driven by the changes during the crisis; Column (2) removes the event year (merger or acquisition year) to confirm that our results are not driven by the event years; Column (3) further removes the year before and the year after the event year and finds a robust result; Column

(4) uses only bank assets instead of multiple bank characteristics when conducting the matching method. In all columns, the coefficient for the triple interaction term remains negative and significant, and the magnitude is largely comparable to the baseline result.

Conclusion

Using a sample of private banks acquired by public banks or BHCs, we examine how public ownership of banks affects mortgage lending discrimination. We find that banks increase approval rates and lower contractual interest rates for African American borrowers after they become public. At the extensive margin, a bank's denial rate for African American mortgage borrowers decreases by 6.1-6.3 percentage points after the bank goes public. This magnitude amounts to 19.1%-19.7% of the mean denial rate for African American borrowers. We show that our findings are not driven by changes in risk preferences or securitization activities after a bank goes public. At the intensive margin, we find that the mortgage contractual rates for African American borrowers decrease after a bank goes public. Going public reduces the mortgage interest rates by about 10.5 basis points for African American borrowers' home purchase loans. We also compare banks that go through a private-to-public transition through an acquisition or a merger deal to those banks that go through the same types of deals but are already public before the deal. By doing this, we can control for the systematic differences between banks that are acquired and those that are not. To the best of our knowledge, our study is the first study to investigate the impact of banks' public status on lending discrimination.

Understanding whether the public status of financial institutions promotes or inhibits discrimination is critically important, given both long-standing challenges of eliminating discrimination and the economic significance of mortgage loans for an average household. In this paper, we provide evidence consistent with the theoretical predictions, including Becker (1957)'s view that discrimination reflects ideological biases of a limited number of private owners and thus is costly, and Becker (1981), Thaler and Shefrin (1981), and Stulz (1988)'s prediction that private and concentrated ownership could exacerbate individuals biases and agency costs. Our results have an implication that disperse ownership may help mitigate the costs associated with non-economically motivated individual preferences, which alleviates lending discrimination and credit misallocation, and eventually yields positive effects on social outcomes.

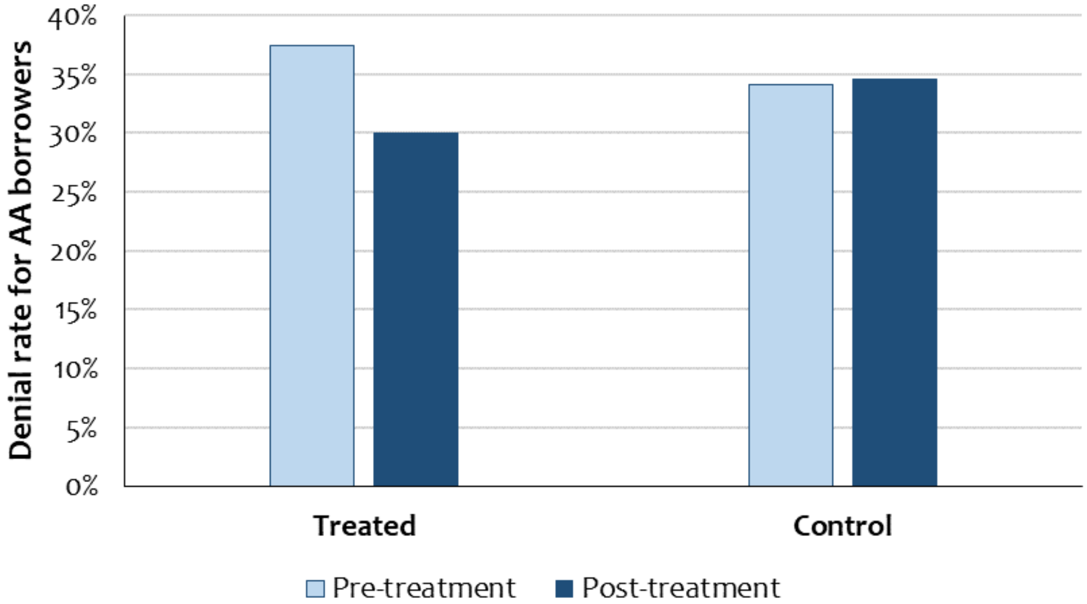
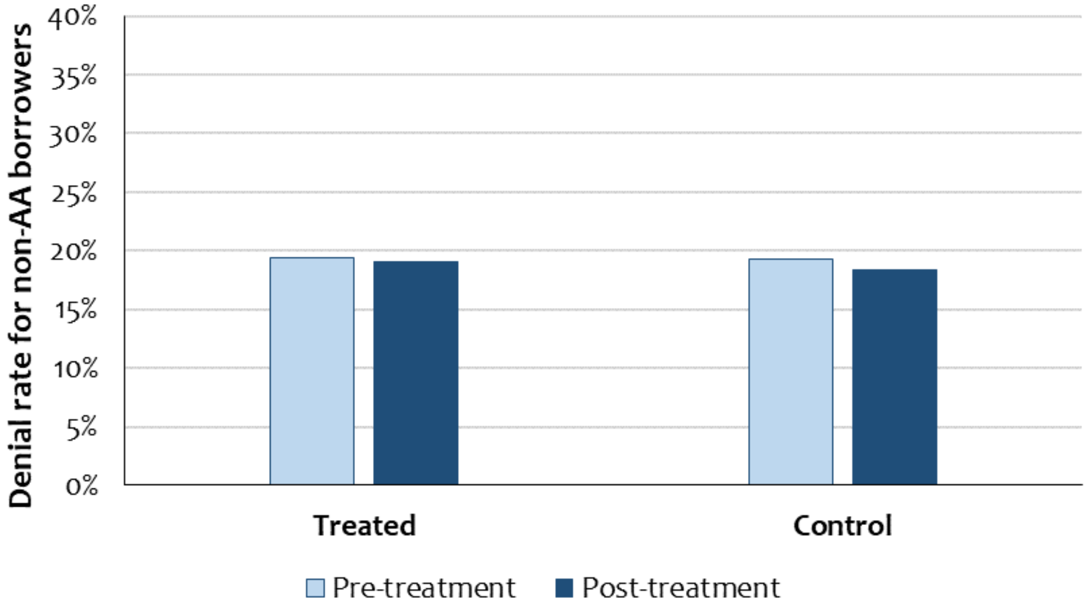


Figure 3.1 Mortgage denial rates for treatment and control groups

Panel A. Denial rate for African American borrowers

Panel A presents the denial rates for African American borrowers for the treatment and control groups before and after the treatment. Panel B presents the same statistics for non-African American borrowers. The denial rate data are obtained from the HMDA dataset and the sample period is 1990-2016.



Panel B. Denial rate for non-African American borrowers

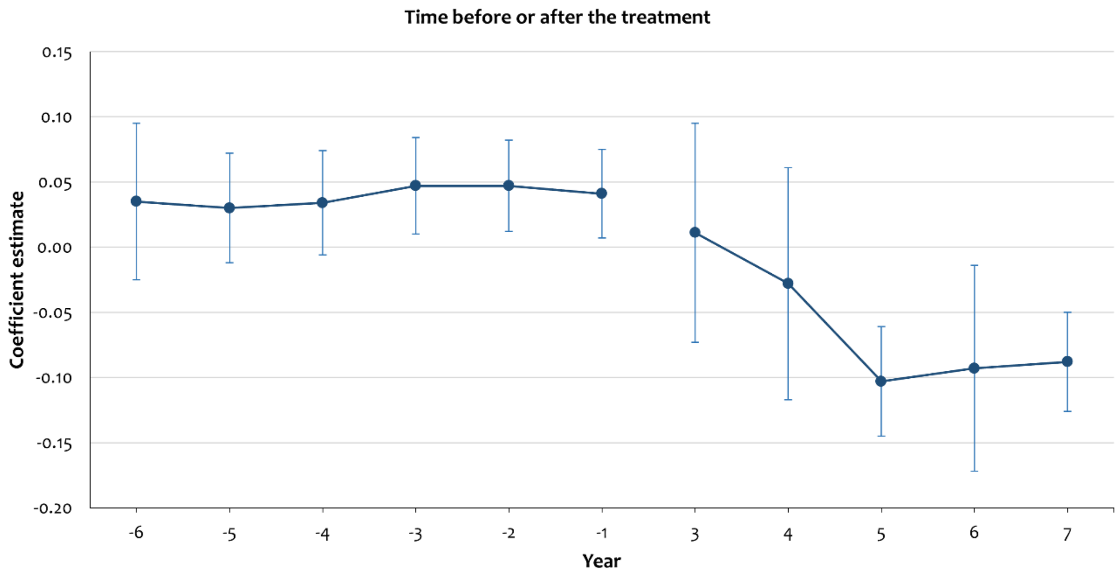


Figure 3.2 The dynamics of the coefficient estimate

This figure presents the triple difference coefficient estimates and confidence interval of β_1^k's of estimating

$$Denied_{i,c,b,t} = \sum_{k=-6}^{6} \beta_1^k African\ American_i \times Treated_b \times Year^k + \beta_2 African\ American_i \times Treated_b + \beta_3 Treated_b \times Year^k + \beta_4 African\ American_i \times Year^k + \beta_5 African\ American_i + \beta_6 Treated_b + \beta_7 Year^k + \beta_8 X_i + \beta_9 Y_{b,t} + \alpha_{b,t} + \theta_{c,t} + \gamma_{b,c} + \varepsilon_{i,c,b,t}, \qquad (3.2)$$

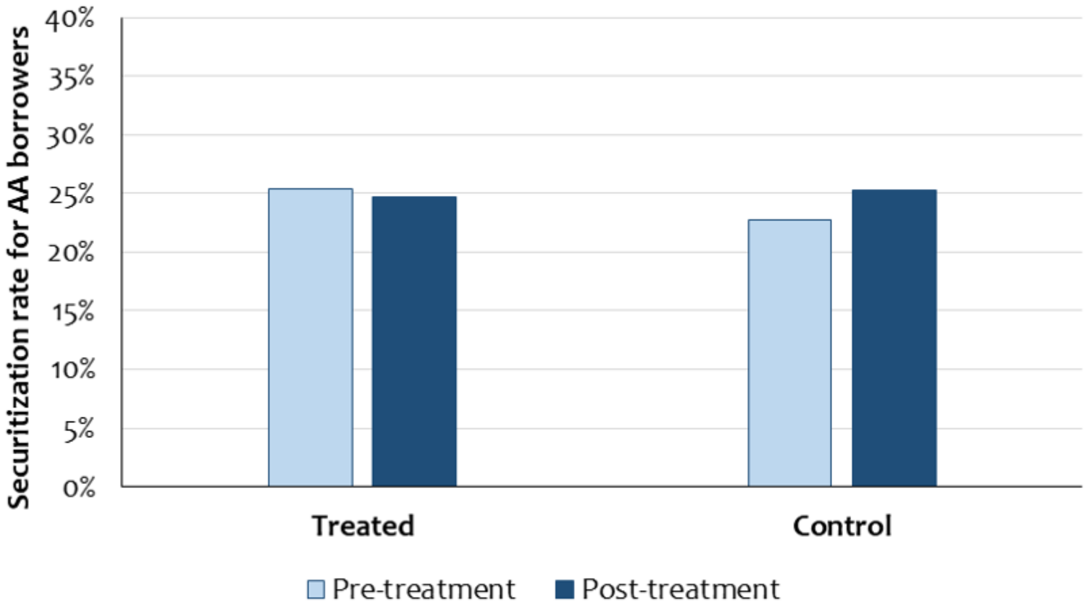
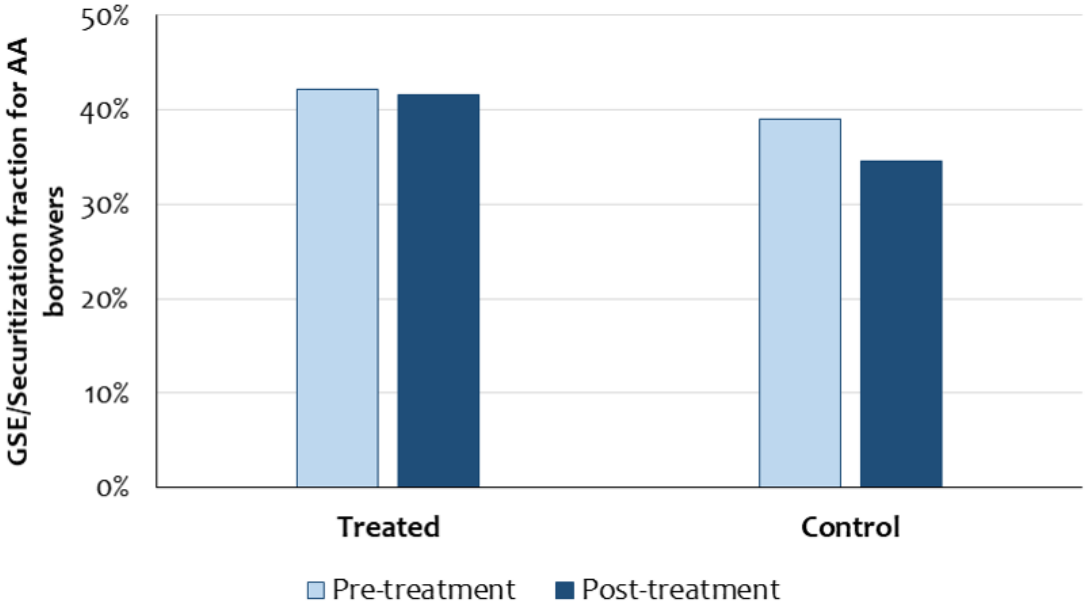


Figure 3.3 Mortgage securitization ratios for treatment and control groups

Panel A. Securitization rate for African American borrowers

Panel A presents the securitization ratio (defined as the securitized volume scaled by total issued volume by a bank) for African American borrowers for the treatment and control groups before and after the treatment. Panel B presents the fraction of loans sponsored by the (GSEs) over all securitized loans for African American borrowers for the treatment and control groups before and after the treatment. The securitization data are obtained from the HMDA dataset and the securitization status is only in the same calendar year in which the loan was originated or purchased. The sample period is 1990-2016.



Panel B. GSE/Securitization rate for African American borrowers

Table 3.1 Summary statistics

This table presents the summary statistics of the regression analysis variables. Panel A presents the baseline HMDA sample; Panel B presents the HMDA sample matched with Fannie Mae and Freddie Mac loan data. For both panels, the unit of observation is a loan-year. The sample period is 1990-2016. Statistics include the number of observations (N), mean, standard deviation (SD), the 1st percentile (P1), median, and the 99th percentile (P99). Table 3.11 provides formal definitions of the variables.

Variable	N	Mean	Std. Dev.	P1	P50	P99
Panel A. Baseline HMDA sample						
Denial rate	604843	0.172	0.377	0	0	1
Denial rate (African American borrowers)	21226	0.32	0.467	0	0	1
Denial rate (White borrowers)	502197	0.151	0.358	0	0	1
Denial rate (Asian borrowers)	20009	0.171	0.376	0	0	1
Denial rate (Other borrowers)	61411	0.288	0.453	0	0	1
Log(Applicant income)	604843	4.123	0.813	2.398	4.094	6.413
Log(Loan size)	604843	4.29	0.984	1.792	4.344	6.477
Female indicator	604843	0.195	0.397	0	0	1
Co-borrower	604843	0.626	0.484	0	1	1
African American % neighborhood	604843	0.042	0.107	0	0	0.625
Panel B. FNM & FDM matched sample						
Interest rate	53495	5.264	1.057	3	5.5	7.125
Loan-to-value ratio	53495	76.004	15.788	27	80	100
Debt-to-income ratio	53495	34.917	11.038	11	35	62
FICO score	53495	744.905	51.086	607	757	816
Occupied	53495	0.945	0.229	0	1	1
First-time home buyer	53495	0.293	0.455	0	0	1
Mortgage insurance %	53495	6.574	11.857	0	0	35
Log(Loan size)	53495	5.116	0.547	3.761	5.124	6.033
Female indicator	53495	0.245	0.43	0	0	1
Co-borrower	53495	0.591	0.492	0	1	1

Table 3.2 Summary statistics of treated and control banks

This table presents the summary statistics of the treated and control banks. Data are obtained from Call Reports except for the securitization ratio from HMDA. The unit of observation is a bank-year. Control banks are private banks matched with treated banks by year and headquarter state. The sample period is 1990-2016. Statistics include the mean, median, and standard deviation (SD) for treated and control bank-years, as well as the difference between means of the two samples, *t*-statistic and *p*-value from the two-sample test. The null hypothesis of the test is that the population means of two groups are equal. Table 3.11 provides formal definitions of the variables.

	Treated banks			Control banks					
Variable	Mean	Median	Std Dev	Mean	Median	Std Dev	Diff(C-T)	t-Stat	p-Value
Log(Assets)	11.928	11.883	1.037	11.776	11.701	1.037	-0.152	-0.960	0.338
Capital/Assets	0.094	0.087	0.036	0.090	0.086	0.026	-0.004	-2.200	0.028
Loan/Assets	0.605	0.619	0.139	0.576	0.591	0.152	-0.029	0.030	0.974
Real estate loans/Assets	0.393	0.382	0.156	0.364	0.356	0.155	-0.029	0.900	0.367
Deposits/Assets	0.858	0.873	0.069	0.870	0.888	0.067	0.013	2.020	0.043
Wholesale funding/Assets	0.152	0.131	0.100	0.157	0.134	0.103	0.004	0.450	0.653
Letters of credit/Assets	0.007	0.004	0.008	0.006	0.003	0.011	-0.001	-0.280	0.783
ROA	0.008	0.009	0.026	0.009	0.010	0.010	0.001	2.320	0.021
ROE	0.067	0.102	0.509	0.087	0.110	0.578	0.019	0.010	0.992
NPL/Loans	0.015	0.008	0.021	0.012	0.007	0.017	-0.002	-2.800	0.005
NPL real estate/Loans	0.013	0.005	0.024	0.009	0.004	0.018	-0.004	-2.230	0.027
NPL family loans/Loans	0.013	0.005	0.025	0.010	0.004	0.020	-0.003	-2.010	0.045
Net interest margin/Assets	0.039	0.039	0.043	0.039	0.038	0.008	0.000	0.250	0.799
Securitization ratio	0.179	0.000	0.542	0.158	0.000	0.310	-0.021	-0.600	0.547

Table 3.3 Public transition and lending discrimination: Baseline results

This table presents the discrimination effect estimated from the matched sample, at the loan-year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin); Post is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides detailed definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
African American × Treated × Post	-0.049**	-0.067***	-0.061***	-0.063***	-0.062***
	(0.025)	(0.021)	(0.020)	(0.019)	(0.019)
African American × Treated	0.002	0.013	0.017	0.017	0.16
	(0.019)	(0.015)	(0.014)	(0.014)	(0.014)
African American × Post	0.013	0.015	0.022*	0.022*	0.025*
	(0.019)	(0.014)	(0.013)	(0.013)	(0.013)
Treated × Post	-0.015				
	(0.011)				
Treated	0.001				
	(0.007)				
Post	-0.001				
	(0.006)				
African American	0.067***	0.142***	0.059***	0.058***	0.059***
	(0.014)	(0.007)	(0.009)	(0.009)	(0.010)
Log(Applicant income)	-0.079***		-0.077***	-0.078***	-0.079***
	(0.003)		(0.003)	(0.003)	(0.003)
Log(Loan size)	0.006***		0.009***	0.009***	0.009***
	(0.002)		(0.002)	(0.002)	(0.002)
Asian	-0.062***		-0.049***	-0.049***	-0.048***
	(0.011)		(0.012)	(0.012)	(0.012)
White	-0.071***		-0.064***	-0.063***	-0.062***
	(0.005)		(0.004)	(0.004)	(0.004)
Female applicant	0.002		0.001	0.001	0.001
	(0.002)		(0.002)	(0.002)	(0.002)
Co-borrower	-0.003*		-0.002	-0.000	-0.000
	(0.002)		(0.002)	(0.002)	(0.002)
African American%	0.032**		0.029***	0.029***	0.030***
	(0.013)		(0.010)	(0.010)	(0.011)
Securitization%	-0.016				
	(0.011)				
Log(Assets)	-0.006***				
	(0.002)				
Capital ratio	-0.151**				
	(0.076)				

ROA	-0.515*				
	(0.267)				
NPL	0.327**				
	(0.141)				
NIM	0.265*				
	(0.147)				
Observations	604,843	625,282	604,829	599,189	591,439
Adjusted R^2	0.0768	0.107	0.130	0.135	0.128
Borrower & loan controls	Yes		Yes	Yes	Yes
Year FE	Yes				
County FE	Yes	Yes	Yes		
Bank × Year FE		Yes	Yes	Yes	Yes
County × Year FE				Yes	Yes
Bank × County FE					Yes

Table 3.4 Mortgage rate and risk measures

This table presents the mortgage rate and performance estimated from the HMDA-FNM FDM matched sample, at the loan-year level during the 1990-2016 sample period. The dependent variable in Columns (1)-(2) is the mortgage rate and in Columns (3)-(6) are loan-to-value ratio, debt-to-income ratio, occupancy status, and first-time home buyer status, respectively. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin); Post is a dummy that equals one if the loan application occurred after the bank went public (the same *Post* dummy of treated banks is assigned to matched control banks). Borrower and loan controls are included: combined loan-to-value ratio, debt-to-income ratio, FICO, insurance%, log(loan size), occupied status, first-time home buyer status, racial dummies, and female and co-borrower dummies. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Contractual rate	Contractual rate	Loan-to-value ratio	Debt-to-income ratio	Occupancy status	First-time buyer status
African American × Treated × Post	-0.105***	-0.105***	2.280**	4.726***	0.161***	0.137**
	(0.037)	(0.037)	(1.113)	(1.354)	(0.023)	(0.067)
African American × Treated	0.047***	0.047***	-0.567	-0.606**	-0.050***	-0.081***
	(0.007)	(0.007)	(0.375)	(0.269)	(0.018)	(0.021)
African American × Post	0.039**	0.038*	-0.526***	-2.359**	0.046**	0.014
	(0.019)	(0.019)	(0.193)	(1.112)	(0.018)	(0.060)
African American	-0.005	-0.004	0.069	-0.124	-0.029	0.003
	(0.011)	(0.012)	(0.208)	(0.169)	(0.022)	(0.013)
Observations	50,541	50,347	50,347	50,347	50,347	50,347
Adjusted R^2	0.915	0.914	0.880	0.0668	0.335	0.118
Borrower & loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Year - Month FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × County FE		Yes	Yes	Yes	Yes	Yes

Table 3.5 Public transition and lending discrimination: Identification

This table presents the discrimination effect estimated from different control groups, at the loan-year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (in Columns (1)-(3) the control banks are public target banks that are acquired by public acquirers; in Columns (4)-(6) the control banks are the public acquirer in the same acquisition deal and we add the deal fixed effects); Post is a dummy that equals one if the loan application occurred after the bank went public (or after the acquisition deal went through for control banks); African American is a dummy variable that equals one if the applicant is African American, and zero other otherwise. Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Public targets as control group			Within-deal estimation		
African American × Treated × Post	-0.103***	-0.092**	-0.090**	-0.057**	-0.057**	-0.054**
	(0.038)	(0.039)	(0.040)	(0.024)	(0.024)	(0.024)
African American × Treated	0.043*	0.042	0.043	0.046**	0.046**	0.046**
	(0.026)	(0.026)	(0.026)	(0.018)	(0.018)	(0.018)
African American × Post	0.062*	0.054	0.053	0.016	0.019	0.017
	(0.036)	(0.037)	(0.038)	(0.017)	(0.016)	(0.016)
African American	0.031	0.032	0.031	0.028**	0.027**	0.028**
	(0.023)	(0.024)	(0.024)	(0.013)	(0.013)	(0.013)
Observations	322,049	317,314	314,780	519,260	513,993	508,879
Adjusted R^2	0.131	0.134	0.125	0.125	0.131	0.125
Borrower & loan controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes			Yes		
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE		Yes	Yes		Yes	Yes
Bank × County FE			Yes			Yes
Deal FE				Yes	Yes	Yes

Table 3.6 Bank headquarter location and lending discrimination

This table presents the discrimination effect estimated from the matched sample, at the loan-year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. Columns (1)-(3) include banks that are headquartered in the southern states; Columns (4)-(6) include banks that are headquartered in the other states. The southern states include Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, Tennessee, Virginia, and West Virginia. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin); Post is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). The matching method is kernel matching. Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		South			North	
African American × Treated × Post	-0.073**	-0.087**	-0.086**	-0.034	-0.032	-0.031
	(0.036)	(0.038)	(0.038)	(0.024)	(0.023)	(0.023)
African American × Treated	0.052**	0.057***	0.056***	-0.01	-0.011	-0.012
	(0.021)	(0.021)	(0.022)	(0.019)	(0.019)	(0.019)
African American × Post	0.021	0.029	0.028	0.014	0.012	0.015
	(0.030)	(0.031)	(0.031)	(0.014)	(0.014)	(0.014)
African American	0.053***	0.048***	0.048***	0.064***	0.064***	0.065***
	(0.015)	(0.015)	(0.015)	(0.011)	(0.011)	(0.011)
Observations	128,341	126,424	125,287	487,787	483,196	477,297
Adjusted R^2	0.117	0.118	0.103	0.133	0.139	0.133
Borrower & loan controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes			Yes		
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE		Yes	Yes		Yes	Yes
Bank × County FE			Yes			Yes

Table 3.7 Bank competition and lending discrimination

This table presents the discrimination effect estimated from the matched sample, at the loan-year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. Columns (1)-(3)include high-competition counties; Columns (4)-(6) include low-competition counties. To measure competition, we use the mortgage-lending Herfindahl-Hirschman Index (HHI), which is defined as the sum of squared lenders' market shares of issued mortgages in a given county. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched by year and headquarter state); Post is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). The matching method is kernel matching. Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	High competition			Low competition		
African American × Treated × Post	-0.049**	-0.049**	-0.051**	-0.073**	-0.089***	-0.084***
	(0.023)	(0.023)	(0.023)	(0.032)	(0.031)	(0.033)
African American × Treated	0.009	0.007	0.007	0.035*	0.040**	0.038*
	(0.015)	(0.015)	(0.015)	(0.019)	(0.020)	(0.020)
African American × Post	0.023	0.024	0.030*	0.016	0.018	0.016
	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)	(0.018)
African American	0.054***	0.053***	0.053***	0.062***	0.063***	0.065***
	(0.011)	(0.011)	(0.011)	(0.013)	(0.014)	(0.014)
Observations	310,281	309,016	305,031	293,303	289,175	284,861
Adjusted R^2	0.131	0.133	0.125	0.133	0.137	0.122
Borrower & loan controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes			Yes		
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE		Yes	Yes		Yes	Yes
Bank × County FE			Yes			Yes

Table 3.8 Public transition and risk preference

This table presents the risk preference effect estimated from the matched sample, at the loan- year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. Income is the applicant's reported income in HMDA; *LTI* is the loan-to-income ratio of a loan application; *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin); Post is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). Borrower and loan controls are included: log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Income × Treated × Post	0.049	0.046	0.042			
	(0.030)	(0.032)	(0.033)			
Income × Treated	-0.033	-0.03	-0.031			
	(0.020)	(0.021)	(0.023)			
Income × Post	-0.030*	-0.024	-0.021			
	(0.018)	(0.019)	(0.020)			
Income	-0.072***	-0.077***	-0.080***			
	(0.010)	(0.011)	(0.012)			
LTI × Treated × Post				0.03	0.027	0.026
				(0.063)	(0.062)	(0.061)
LTI × Treated				-0.006	-0.006	-0.006
				(0.012)	(0.012)	(0.012)
LTI × Post				0.03	0.03	0.03
				(0.042)	(0.041)	(0.041)
LTI				0.021**	0.020**	0.020**
				(0.008)	(0.008)	(0.008)
Observations	604,917	599,277	591,527	604,829	599,189	591,439
Adjusted R^2	0.116	0.121	0.114	0.116	0.121	0.114
Borrower & loan controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes			Yes		
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE		Yes	Yes		Yes	Yes
Bank × County FE			Yes			Yes

Table 3.9 Ruling out changes in underwriting standards and disclosure requirement

This table presents the discrimination effect estimated from the matched sample, at the loan-year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin); *Post* is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). In Columns (1)-(2), we explicitly add controls for changes in the underwriting standards: the pairwise interaction terms of the loan-to- income ratio with banks conforming securitization ratio, average loan size, average applicant income, and average loan-to-income ratio. Columns (3)-(4) include banks that have a pre-treatment opacity measure lower than median opacity, while Columns (5)-(6) include banks that have the measure higher than median opacity. We measure bank opacity using the residual loan loss provision estimated by equation (3.3). Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Controlling for Underwriting standards		Non-opaque banks		Opaque banks	
African American × Treated × Post	-0.061***	-0.063***	-0.074***	-0.075***	-0.065**	-0.066**
	(0.020)	(0.019)	(0.025)	(0.026)	(0.027)	(0.027
African American × Treated	0.017	0.017	0.006	0.003	0.031	0.033*
	(0.014)	(0.014)	(0.016)	(0.016)	(0.019)	(0.020)
African American × Post	0.022*	0.022*	0.045**	0.048**	0.009	0.012
	(0.013)	(0.013)	(0.019)	(0.019)	(0.017)	(0.017)
African American	0.059***	0.058***	0.053***	0.054***	0.066***	0.064***
	(0.009)	(0.009)	(0.013)	(0.013)	(0.010)	(0.010)
Observations	604,829	599,189	305,857	301,736	298,592	294,564
Adjusted R^2	0.13	0.135	0.128	0.132	0.133	0.138
Control for portfolio factors	Yes	Yes				
Control for underwriting standards	Yes	Yes				
Control for interactions	Yes	Yes				
Borrower & loan controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes		Yes		Yes	
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE		Yes		Yes		Yes

Table 3.10 Robustness checks

This table presents the robustness checks for the discrimination effect estimated from the matched sample, at the loan-year level during the 1990-2016 sample period. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero other- wise. Column (1) removes years 2007-2010; Column (2) removes the treatment year for each bank; Column (3) removes the treatment year, the year before and the year after; Column (4) uses the propensity score matching that only uses year, headquarter state, and bank assets. *Treated i*s a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin except for Column (4)); Post is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). The matching method is kernel matching. Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1) Remove 2007-2010	(2) Remove treatment year	(3) Remove year [-1,1]	(4) Only match on bank assets
African American × Treated × Post	-0.073***	-0.058**	-0.081***	-0.051***
	(0.020)	(0.024)	(0.023)	(0.017)
African American × Treated	0.018	0.017	0.027	0.02
	(0.014)	(0.014)	(0.018)	(0.013)
African American × Post	0.027*	0.009	0.025	0.013
	(0.015)	(0.020)	(0.016)	(0.010)
African American	0.060***	0.063***	0.052***	0.051***
	(0.009)	(0.008)	(0.011)	(0.008)
Observations	543,874	540,760	433,679	925,622
Adjusted R^2	0.134	0.133	0.133	0.13
Borrower & loan controls	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes

Table 3.11 Variable definitions

Variable	Description	Data Sources
Borrowers & Loan		
Denied	A dummy variable that equals one if the loan application is denied, and zero otherwise.	HMDA
African American	A dummy variable that equals one if the borrower is African American, and zero otherwise.	HMDA
Asian	A dummy variable that equals one if the borrower is Asian, and zero otherwise.	HMDA
White	A dummy variable that equals one if the borrower is White, and zero otherwise.	HMDA
Log(Applicant income)	The natural logarithm of applicant income reported in HMDA.	HMDA
Log(Loan size)	The natural logarithm of the mortgage size reported in HMDA.	HMDA
Female	A dummy variable that equals one if the borrower is female, and zero otherwise.	HMDA
Co-borrower	A dummy variable that equals one if the borrower has a co-borrower, and zero otherwise.	HMDA
African American%	The percent of African American applicants over all applicants in a census tract.	HMDA
Combined LTV	The ratio of the original mortgage loan amount on the note date plus any secondary mortgage loan amount disclosed by the seller.	FNM & FDM
Debt-to-income ratio	The ratio of (1) the sum of the borrower's monthly debt payments, including monthly housing expenses that incorporate the mortgage payment the borrower is making at the time of the delivery of the mortgage loan to FNM or FDM, divided by (2) the total monthly income used to underwrite the loan as of the date of the origination.	FNM & FDM
FICO score	The borrower's FICO score.	FNM & FDM
Occupied	A dummy variable that equals one if the property to which the loan application relates will be the owner's principal dwelling.	FNM & FDM
Mortgage insurance%	The percentage of loss coverage on the loan, at the time of the GSEs purchase of the mortgage loan that a mortgage insurer is providing to cover losses incurred as a result of a default on the loan.	FNM & FDM
Interest rate	The original note rate as indicated on the mortgage note.	FNM & FDM
First-time home buyer	A dummy variable that equals to one if the borrower or co-borrower qualifies as a first-time homebuyer.	FNM & FDM
Bank characteristics		
Treated	A dummy variable that equals one if the bank becomes public through an acquisition, and zero otherwise. The control sample may vary across empirical tests.	SDC
Post	A dummy variable that equals one if the year is in or after the bank becomes public, and zero otherwise.	SDC
Log(Assets)	The logarithm of bank total assets.	Call Report
Capital Ratio	Ratio of capital to total assets.	Call Report
ROA	Net income to total assets.	Call Report

ROE	Net income to total equity.	Call Report
Loans/Assets	Ratio of loans to total assets.	Call Report
Real estate loans/Assets	Ratio of real-estate loans to total assets.	Call Report
Deposits/Assets	Ratio of deposits to total assets.	Call Report
Wholesale funding/Assets	Share of wholesale funding (sum of wholesale deposits, federal funds, and repo borrowing) to total assets.	Call Report
Letters of credit/Assets	Ratio of letters of credit to total assets.	Call Report
NPL/Loans	Share of non-performing loans (90-day past due and non-accruals) to total loans.	Call Report
NPL real estate/Loans	Share of real estate non-performing loans (90- day past due and non-accruals) to total loans.	Call Report
NPL family loans/Loans	Share of 1-4 family non-performing loans (90- day past due and non-accruals) to total 1-4 family loans.	Call Report
Net interest margin	Share of net interest income to total assets.	Call Report
Securitization ratio	Share of securitized mortgages to total loan origination.	HMDA

Table 3.12 Public transition and lending discrimination: Refinancing loans

This table presents the discrimination effect estimated from the matched sample, at the loan-year level during the 1990-2016 sample period. The sample contains only refinancing loans. The dependent variable is the denied dummy that takes the value one if the loan is rejected, and zero otherwise. *Treated* is a dummy that equals one if the bank went from private to public during the sample period, and zero otherwise (controls banks are private banks matched on the propensity score by year, headquarter state, and bank characteristics including log(assets), capital ratio, ROA, non-performing loans, and net interest margin); *Post* is a dummy that equals one if the loan application occurred after the bank went public (the same Post dummy of treated banks is assigned to matched control banks). Borrower and loan controls are included: log(applicant income), log(loan size), racial dummies, female and co-borrower dummies, and African American population% in a census tract. Table 3.11 provides formal definitions of the variables. Fixed effects are included and indicated in the column bottom. Standard errors in parentheses are clustered by bank. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Var			Denied		
Sample			Refinancing loans		
African American × Treated × Post	-0.025	-0.051**	-0.058**	-0.050**	-0.047*
	(0.038)	(0.026)	(0.028)	(0.025)	(0.025)
African American × Treated	-0.027	-0.014	-0.006	-0.006	-0.008
	(0.024)	(0.016)	(0.016)	(0.015)	(0.015)
African American × Post	-0.014	0.016	0.026	0.025	0.025
	(0.031)	(0.017)	(0.017)	(0.016)	(0.016)
Treated × Post	-0.026				
	(0.017)				
Treated	0.017				
	(0.011)				
Post	-0.004				
	(0.008)				
African American	0.045**	0.135***	0.024**	0.023**	0.024**
	0.002	(0.008)	(0.011)	(0.011)	(0.011)
Observations	658,039	685,035	657,983	652,498	646,231
Adjusted R^2	0.0714	0.114	0.133	0.14	0.134
Borrower & loan controls	Yes		Yes	Yes	Yes
Year FE	Yes				
County FE	Yes	Yes	Yes		
Bank × Year FE		Yes	Yes	Yes	Yes
County × Year FE				Yes	Yes
Bank × County FE					Yes

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