Empirical Analyses on Non-Bank Financial Intermediaries

HARVARD UNIVERSITY
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Empirical Analyses on Non-Bank Financial Intermediaries

A dissertation presented

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Empirical Analyses on
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Abstract

This thesis examines the economics of non-bank financial intermediaries and aims to address two questions. First, why do non-bank intermediaries arise alongside banks in a given market? Second, what are the real effects of non-bank financial intermediation? In the following essays, I study these questions in three different settings: the business lending market, the consumer lending market, and the corporate going-public market.

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Introduction

This thesis examines the economics of non-bank financial intermediaries and aims to address two questions. First, why do non-bank intermediaries arise alongside banks in a given market? Second, what are the real effects of non-bank financial intermediation? In the following three essays, I study these questions in three different settings: the business lending market, the consumer lending market, and the corporate going-public market.

The first essay is "Commercial Eviction Moratoria, Liquidity Relief, and Business Closure." In this work, I turn to the business lending market and evaluate the effects of the commercial eviction moratorium (CEM) policy during the Covid-19 pandemic. Widely enacted by local governments, CEM temporarily prohibits commercial evictions and requires commercial landlords to pause rent payments. Effectively, CEM provides business tenants with liquidity relief in the form of zero-interest, non-forgivable credit from their landlords. To assess the causal effect of CEM, I hand-collect a novel dataset on CEM policy in all localities of California and construct an instrument based on pre-pandemic partisanship, measured as the difference between Democratic and Republican voter registration. I find that CEM significantly reduces business closure in the short run in both retail and food services but has long-run effects only in food services. One mechanism is that CEM is more effective in reducing long-run closure for businesses that are more solvent coming into the pandemic. Turning to employment, the impact of CEM operates along an extensive margin through reduction in business closure, rather than along an intensive margin through change in employment while a business is in operation. The total impact of CEM on employment is a preservation of 0.98 percentage points of pre-pandemic employment, which equals 39% of the estimated effects of the Paycheck Protection Program.

The second essay is "In the Red: Overdrafts, Payday Lending, and the Underbanked," joint with Marco Di Maggio and Emily Williams. In this work, we turn to the consumer lending market and examine the relationship between banks and alternative financial intermediaries, such as payday lenders. We exploit a series of class-action lawsuits that mandated that some banks cease the practice of high-to-low reordering, which is a controversial bank practice thought to maximize fees paid by low-income customers on overdrawn accounts. Using alternative credit bureau data, we find that after banks cease high-to-low reordering, low-income individuals reduce payday borrowing, increase consumption, undergo long-term improvements in financial health, and gain access to lower-cost loans in the traditional financial system. These findings, in suggesting that aggressive bank practices can create demand for alternative financial services, highlight an important link between the traditional and alternative financial systems.

The final essay is "Segmented Going-Public Markets and the Demand for SPACs," joint with Jessica Bai and Miles Zheng. In this work, we turn to the corporate going-public market and examine the economic role of the Special Purpose Acquisition Company (SPAC), which is an alternative going-public mechanism relative to the traditional IPO. We propose that SPAC sponsors act as non-bank intermediaries and that the SPAC market structure appeals to yield-seeking investors and riskier, high-growth issuers overlooked by downside-averse bank underwriters. By building a theoretical framework and augmenting SPAC data from 2003-2020, we show that SPAC firms are smaller, younger, and riskier than traditional IPO firms but grow revenue at higher rates after going public. Additionally, equity market investor sentiment strongly predicts SPAC capital raises relative to traditional IPOs. Finally, using a difference-in-differences analysis, we find that an increase in IPO litigation risk shifts going-public activity towards SPACs. In sum, we provide a litigation-risk-based explanation for the origin and proliferation of the SPAC.

Chapter 1

Commercial Eviction Moratoria, Liquidity Relief and Business Closure

1.1 Introduction

To combat the severe shock of the Covid-19 pandemic for a broad swath of businesses, policymakers reacted by enacting a series of unprecedented programs. First, the federal government intervened in extraordinary ways to aid businesses. For example, the Federal Reserve authorized the Main Street Lending Program and the Primary and Secondary Market Corporate Credit Facilities, while the Small Business Administration administered the Paycheck Protection Program (PPP) and the Economic Injury Disaster Loan (EIDL) program. Second, local governments also responded in unprecedented ways. Commercial eviction moratoria (CEM) were widely enacted and provided liquidity to businesses through rent deferral. 30 states in the U.S. enacted state-wide CEM, applying to 67% of U.S. small businesses. While the policy was widespread, its impact is not well understood. This paper fills the gap by providing the first evaluation of CEM.

In this paper, I address three research questions. First, what is the impact of CEM on business closure? Second, which types of businesses are aided by CEM in the short run and long run? Third, what is the impact of CEM on business employment? To answer these questions, I hand-collect all enactments and updates of CEM policy for the 482 incorporated cities and 58 counties in California over the period March 2020 through May 2023.[1] To identify the causal effect of CEM, I construct an instrument based on pre-pandemic partisanship, measured as the difference between the share of Democratic and Republican voter registration. I first show that more Democratic places enact longer CEM relative to more Republican places, confirming the relevance of the instrument. To address the exclusion restriction, my empirical strategy also controls for several alternative channels, including pre-pandemic characteristics, consumer stay-at-home behavior, business financial support, and exposure to the Covid-19 shock.

My main findings are as follows. CEM reduces business closure in the short run in both retail and food services but has long-run effects only in food services. Consistent with the mechanism that CEM provides liquidity relief, I show first that CEM is more effective in reducing long-run closure for businesses that are more solvent coming into the pandemic. Additionally, CEM reduces business take-up of costly loans but does not affect take-up of grants. Turning to employment, the impact of CEM operates along an extensive margin through a reduction in business closure, rather than along an intensive margin through a change in employment while a business is in operation. The total impact of CEM on employment is a preservation of 0.98 percentage points of pre-pandemic employment, which equals 39% of the estimated effects of PPP.

The direct consequence of CEM is liquidity relief. By pausing rent payments, CEM effectively requires landlords to offer business tenants zero-interest, non-forgivable credit with face value equal to the amount of rent accrued during the policy period. In the face of substantial frictions in credit markets, CEM provides a source of liquidity that can help liquidity-constrained but solvent businesses survive in both the short run and long run. For these businesses, CEM is bridge financing that enables them to survive in the short run

[1] California serves as an ideal laboratory for CEM because of its decentralized implementation. Governor Gavin Newsom gave cities and counties the legal option to enact CEM if they so choose. This feature creates variation in CEM across space and time that I exploit in the empirical analysis. Another advantage of studying California is that it contains a significant share of U.S. businesses, accounting for 13% of the number of small businesses and 12% of the small business employment.

and that they repay to continue operating in the long run. However, since CEM does not improve business solvency, the policy only puts off the inevitable for liquidity-constrained, insolvent businesses. While these businesses can use the liquidity relief of CEM to survive in the short run, they are unable to repay their borrowing and ultimately close in the long run. In contrast, CEM would have no effect on liquidity-unconstrained businesses that already have sufficient access to credit.

The challenge of identifying the causal impact of CEM stems from reverse causality from business closure to CEM. Because the purpose of enacting CEM is to prevent business closure, local policymakers gather information about business closure likelihood by monitoring local economic conditions and hearing directly from small business owners. This reverse causality results in selection bias where a place with more business closure selects into enacting longer CEM. Indeed, the data shows that longer CEM is correlated with higher rates of business closure during the policy period. However, the causal effect of CEM remains unclear because, even if the true effect of CEM is to reduce business closure, the selection bias may result in the observed, positive relationship between CEM and business closure.

To overcome this selection bias, I instrument for CEM policy using pre-pandemic partisanship. Specifically, I compute the net political leaning of an area prior to the pandemic using the Democratic-Republican spread, which is the difference between the share of Democratic and Republican voter registration as of February 2019. Democratic governments are associated with a greater propensity for government intervention relative to Republican governments, which are by contrast associated with a more laissez-faire approach. I find that pre-pandemic partisanship significantly predicts CEM policy; specifically, a 10 percentage point increase in the Democratic-Republican spread predicts a longer CEM (including repayment time) by 3.3 months. Therefore, pre-pandemic partisanship strongly predicts the enactment of CEM.

The exclusion restriction of my instrumental variable strategy is that pre-pandemic partisanship does not affect business closure except through CEM. There are three main alternative channels through which partisanship may affect business closure: consumer stay-at-home behavior, business financial support, and exposure to the Covid-19 economic shock. First, in more Democratic cities, consumers may be more likely to stay at home, thereby reducing demand for businesses and increasing the likelihood of business closure. Second, more Democratic governments may be more likely to intervene during the pandemic by allocating funds to business grants and thereby reducing the likelihood of business closure. Third, differently partisan areas may be differently exposed to the economic shock of the Covid-19 pandemic, leading to differences in likelihood of business closure. Therefore, I construct measures of these three channels and control for them, as well as for pre-pandemic economic characteristics, in both the first and second stages of the instrumental variable approach.

After confirming that the instrumental variable satisfies the first stage and controlling for alternative channels to make it more likely that the exclusion restriction holds, I estimate the causal effect of CEM on business closure. I find that CEM reduces business closure in the short run in both retail and food services but has long-run effects only in food services. In retail, the likelihood of business closure falls by 0.28 percentage points in 2020 and 0.54 percentage points in 2021, which is partly reversed by 0.44 percentage points in 2022. In food services, the likelihood of business closure falls by 0.60 percentage points in 2020, 0.94 percentage points in 2021, and 0.71 percentage points in 2022, which totals 2.24 percentage points over the period 2020-2022. The estimate suggests that the average CEM policy preserves 1,913 food services businesses in California.

I next examine how the effectiveness of CEM varies with business solvency. Using the 5-year pre-pandemic employment growth rate as a sub-industry-level proxy for business solvency, I find that CEM is more effective in reducing business closure in sub-industries that are more solvent coming into the pandemic, which is consistent with substantial frictions in credit markets. Specifically, greater business solvency strengthens the effectiveness of CEM in reducing closure primarily in the later period after CEM is lifted. The results suggest that CEM provides a source of liquidity that prevents the closure of liquidity-constrained but solvent businesses in the long run.

I then turn to the effect of CEM on business financing. By providing liquidity relief, CEM may affect business take-up of other sources of financing. Overall, there are two other types of financing available to businesses during the Covid-19 pandemic. First, there is loan financing that must be repaid, such as the EIDL loan program. There is likely to be substitution towards CEM and away from loan financing because the loan is more costly than the zero-interest liquidity relief of CEM. Second, there is grant financing that does not need to be repaid, such as the EIDL advance program and PPP. There is unlikely to be substitution between CEM and grants because grants improve business solvency. Turning first to the EIDL loan program, I find that the average CEM policy leads to a reduction in borrowing by approximately $14,300 per business, consistent with substitution towards CEM and away from loan financing. Turning next to the EIDL advance program and PPP, I find that CEM does not affect business take-up of these grants, consistent with businesses seeking out grants irrespective of CEM policy.

I next assess the employment effects of CEM. The finding that CEM reduces business closure implies that CEM preserves employment along an extensive margin. Specifically, through its effect on business closure, the average CEM policy preserves employment over the period 2020-2022 by a combined 0.98 percentage points of pre-pandemic employment. I then estimate the impact of CEM on employment along an intensive margin, i.e. conditional on the business being alive. Implementing the same empirical strategy of instrumenting for CEM with pre-pandemic partisanship, I find no effect of CEM on intensive-margin employment over the period 2020-2022 in both retail and food services. The effect of CEM on employment therefore operates primarily along the extensive margin. The total impact of the average CEM policy on employment is a preservation of 0.98 percentage points of pre-pandemic employment, which equals 39% of the effects of PPP estimated by Chetty *et al.* (Forthcoming). This is striking when we consider that CEM is a government intervention that does not inject government funds into businesses.

Finally, I discuss the policy implications of the results. I find that CEM temporarily reduces business closure in retail and permanently reduces business closure in food services.

Moreover, CEM is more effective in reducing long-run closure for businesses that are more solvent prior to the pandemic. The efficiency implications of the results are nuanced and require additional consideration. CEM may prolong the survival of some insolvent businesses, which could prevent reallocation towards more solvent businesses and deteriorate the financial health of commercial landlords who perform the valuable economic role of maintaining and filling spaces for businesses.

Related Literature

This paper relates to several strands of literature in financial economics. First, this paper connects to the literature on government intervention during the Covid-19 crisis. Several studies have examined the design and effect of federal business aid programs, including PPP (Autor *et al.*, 2022; Bartik *et al.*, 2023; Chetty *et al.*, Forthcoming; Granja *et al.*, 2022), Main Street Lending Program (Hanson *et al.*, 2020; English *et al.*, 2020; Arseneau *et al.*, 2022), and COVID-19 EIDL (Fairlie and Fossen, 2022; Li, 2021). This paper evaluates the effects of a widely enacted local policy that provides liquidity relief to businesses.

Second, it connects to the literature on business closure. Prior to the 2020 crisis, this literature primarily used "traditional" business data sources from the Bureau of Labor Statistics and U.S. Census Bureau. During the Covid-19 pandemic, in order to monitor business closure and employment in a more real-time way, this literature employed "alternative" data sources such as Google Maps (Rigobon *et al.*, 2022), SafeGraph/Advan (de Vaan *et al.*, 2021), Yelp (Bartik *et al.*, 2020a), ADP (Cajner *et al.*, 2020), Homebase (Bartik *et al.*, 2020b), Womply (Chetty *et al.*, Forthcoming), and Alignable (Bartik *et al.*, 2020a, 2023). One striking pattern documented by Iverson *et al.* (2022) is that there has been significantly less business closure & bankruptcy than would be expected based on historical precedent. This paper shows that CEM is one policy that reduces business closure in retail and food services during the Covid-19 crisis.

Third, this paper connects to the growing literature on commercial real estate. Studies have examined the different nodes of the commercial real estate ecosystem, including commercial leasing (Moszkowski and Stackman, 2023), commercial mortgage lending (Glancy *et al.*, 2021, 2022), commercial property investing (Allan *et al.*, 2021), the office-use submarket (Gupta *et al.*, 2023; Sadikin *et al.*, 2021), and the retail-use submarket (Liebersohn *et al.*, 2022; Moszkowski and Stackman, 2023). This paper contributes to the literature by evaluating the effects of a commercial rent forbearance policy.

Finally, this paper connects to the literature on non-bank financial intermediaries. In many financial markets, non-bank intermediaries have been shown to fill a gap left by banks. In the consumer loan market, payday lenders provide short-term financing to underbanked individuals who cannot access traditional credit (e.g. Morse, 2011; Di Maggio *et al.*, 2022). In the corporate loan market, CLOs provide financing to leveraged borrowers with low or no credit ratings (Benmelech *et al.*, 2012). In the corporate going-public market, SPACs take smaller, riskier, higher-growth companies public (Klausner *et al.*, 2022; Gahng *et al.*, 2023; Bai *et al.*, 2023). This paper contributes to the literature by evaluating the real effects of a policy that requires landlords to effectively act as non-bank intermediaries by extending credit to businesses during a crisis.

This paper proceeds as follows. Section 1.2 develops a conceptual framework of CEM in liquidity provision. Section 1.3 describes the data sources and provides summary statistics. Section 1.4 develops the instrumental variable empirical strategy and estimates the impact of CEM on business closure. Section 1.5 investigates the impact of CEM on business financing. Section 1.6 evaluates the impact of CEM on business employment. Section 1.7 discusses policy implications. Finally, Section 1.8 concludes.

1.2 A conceptual framework of CEM in liquidity provision

In this section, I develop a conceptual framework for the impact of CEM on business closure. Section 1.2.1 gives background on the CEM policy. Section 1.2.2 provides illustrative examples of different CEM policies. Section 1.2.3 introduces the setup of the model of CEM in liquidity provision during the Covid-19 crisis. Section 1.2.4 analyzes the impact of CEM on business closure and discusses empirical predictions.

1.2.1 Background on commercial eviction moratoria

Enacted during the Covid-19 pandemic to combat business stress, CEM prohibits commercial evictions under certain conditions. While CEM is in effect, a commercial landlord may not evict a business for not paying rent if the business has experienced a substantial decrease in income due to the Covid-19 pandemic or associated government interventions.[2] Evictions may still proceed if the tenant violates its lease in other ways, e.g. by damaging property or committing illegal acts. Enforcement of CEM happens through the judicial courts. If a landlord initiates commercial eviction proceedings, CEM can be cited by the business tenant as a legal defense. The vast majority of CEM apply to all businesses and are not targeted.

The stated goal of CEM is to prevent business closure by pausing eviction proceedings. The direct consequence of CEM is liquidity relief. By pausing rent payments, CEM effectively requires a landlord to offer its business tenant zero-interest, non-forgivable credit with face value equal to the amount of rent accrued during the policy period. Therefore, CEM is essentially a credit policy where businesses can borrow from their landlord by delaying rent payment.[3]

CEM is enacted by local governments, rather than the federal government. While the start date of CEM is almost uniformly March 2020, there is substantial variation in the end date. For example, in Massachusetts, the legislature and Governor approved a bill pausing non-essential small business evictions through October 2020. In Texas, multiple county courts paused non-essential commercial eviction hearings for varying amounts of time. In California, the Judicial Council enacted CEM through September 2020, while the Governor

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[2]CEM also prohibits commercial evictions in the case of property foreclosure. That is, if a commercial property enters foreclosure, the new property owner may not evict existing tenants while the CEM is in effect. However, the focus of this paper will be on the primary function of CEM, which is barring commercial evictions on the basis of non-payment of rent.

[3]The potential second consequence of CEM is solvency relief by spurring renegotiation between businesses and landlords. However, because renegotiation is not a direct requirement of the policy and is not observed in the data, this paper will only speak to the liquidity relief implication of CEM.

gave city and county governments the option to enact CEM through September 2021.

1.2.2 Illustrative examples of commercial eviction moratoria

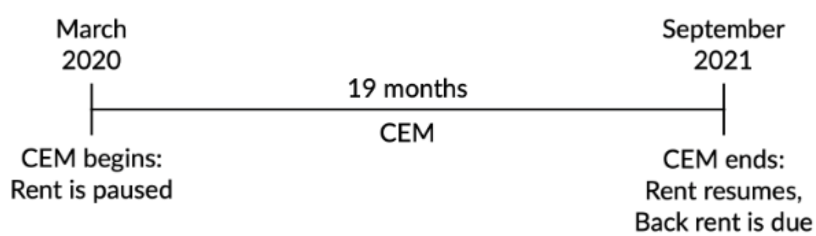
Figure 1.1 provides illustrative examples of the timeline of CEM policy. Panel A shows that in Oakland, California, CEM is enacted in March 2020, at which point the obligation of rent payment is paused. The policy lasts for 19 months and ends in September 2021, at which point the obligation of rent payment resumes. Additionally, all back rent comes due. Panel B shows that in San Diego, California, CEM is similarly enacted in March 2020 and ends in September 2021. In the case of San Diego, however, the local government gave businesses additional repayment time of 6 months, such that back rent comes due in March 2022. Therefore, the length of CEM plus repayment time (CEM+R) is 19 months in Oakland and 25 months in San Diego.

To concretely understand how CEM is a liquidity relief policy, Figure 1.2 presents illustrative examples of the effective loan balance that businesses can borrow under the CEM policy in these two cities. Panel A shows that in Oakland, California, businesses can steadily borrow each month by deferring monthly rent until CEM ends, at which point they must repay the loan balance. Specifically, given a restaurant with monthly rent of $5,000, CEM would enable the business to borrow $95,000 (19 * $5,000) over 19 months at zero interest. Panel B shows that in San Diego, CA, businesses can steadily borrow each month by deferring monthly rent. Then, they can maintain the loan balance until the additional 6 months of repayment time pass, at which point they must repay the loan balance. Under this policy, for the same restaurant with monthly rent of $5,000, CEM would enable the business to borrow $95,000 (19 * $5,000) over 25 (19 + 6) months at zero interest.

Figure 1.1: Illustrative examples of timeline of commercial eviction moratoria

This figure presents illustrative examples of the commercial eviction moratoria (CEM) policy. Panel A presents a timeline of CEM policy in Oakland, CA. Panel B presents a timeline of CEM policy in San Diego, CA.

Panel A: Commercial eviction moratoria policy in Oakland, CA



Panel B: Commercial eviction moratoria policy in San Diego, CA

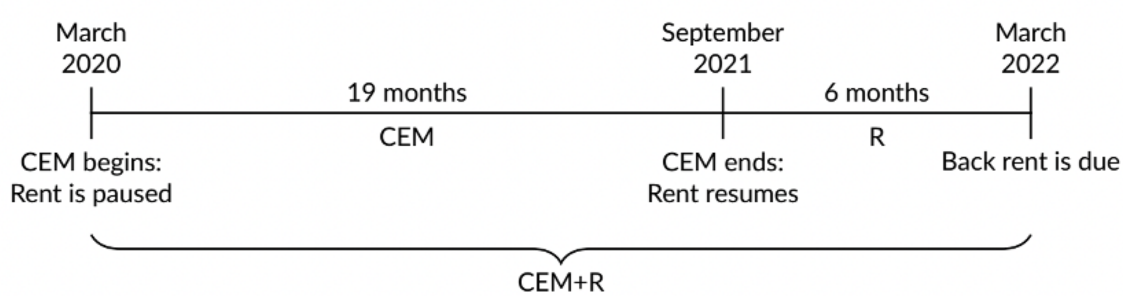
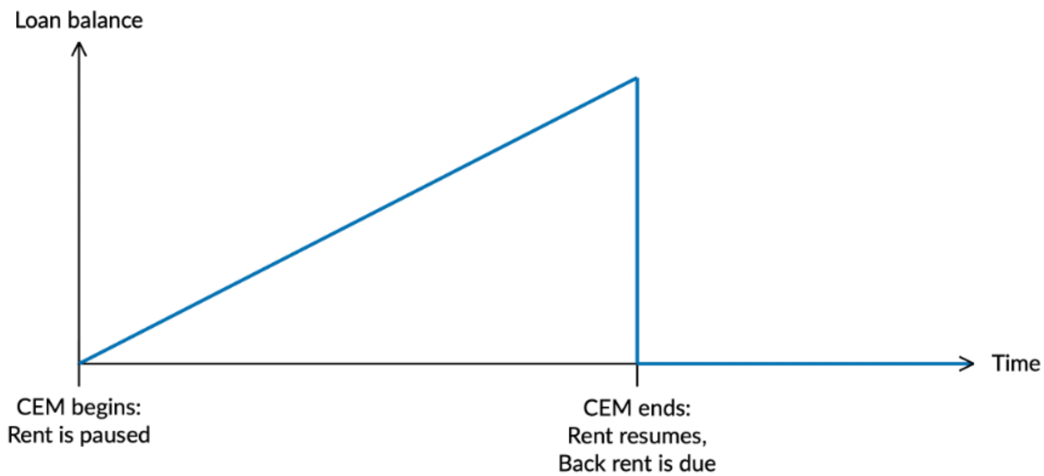


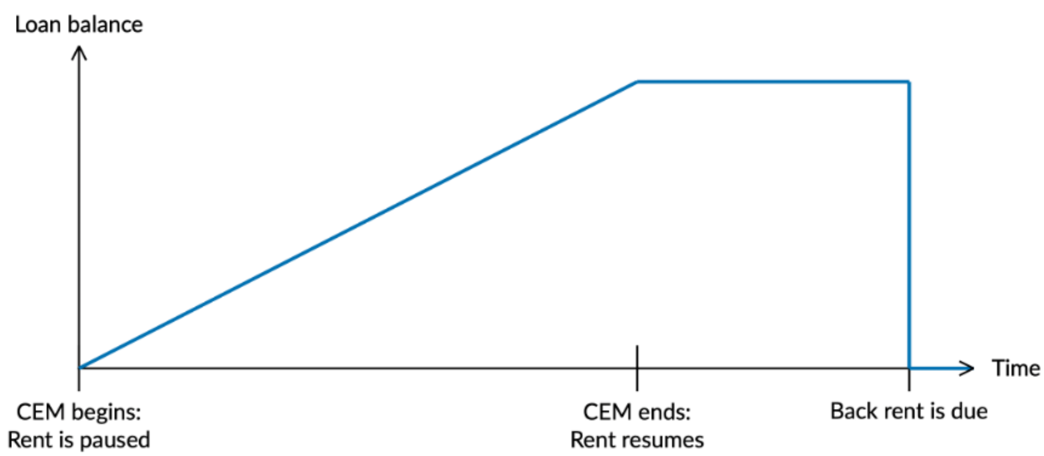
Figure 1.2: Illustrative examples of effective borrowing enabled by commercial eviction moratoria

This figure presents illustrative examples of liquidity relief from commercial eviction moratoria (CEM) policy. Panel A presents the effective loan balance that businesses can incur under the CEM policy of Oakland, CA. Panel B presents the effective loan balance that businesses can incur under the CEM policy of San Diego, CA.

Panel A: Commercial eviction moratoria policy in Oakland, CA



Panel B: Commercial eviction moratoria policy in San Diego, CA



1.2.3 Model setting

To develop a conceptual framework for the impact of CEM on business closure, I extend the model of government intervention in business credit markets during the Covid-19 crisis developed in Hanson *et al.* (2020). There are two added ingredients to this framework relative to Hanson *et al.* (2020). First, I introduce the CEM policy into the model and analyze the impact of CEM on the market outcome, specifically business closure. Second, I introduce business-level heterogeneity in pre-pandemic solvency and analyze its implications for the effectiveness of CEM in reducing long-run business closure.

The model has three periods. $t = 1$ is the early period of the pandemic, $t = 2$ is the later period of the pandemic, and $t = \infty$ is the steady state. There is a negative shock that arrives at $t = 1$, and there is a continuum of businesses $f \in [0, 1]$ that have differing exposure to the negative shock. More exposed businesses experience a larger decline in cash flows. At $t = 1$, there is aggregate uncertainty about whether the recession will be mild or severe at $t = 2$. At $t = 2$, it is revealed whether the economy is in a mild or severe recession, which also determines whether the economy will be in the post-mild-recession or post-severe-recession steady state at $t = \infty$. All agents in the economy are risk neutral with constant time discount factor $\delta \in (0, 1)$. F_{S_t} denotes the mass of businesses that are operating in state S_t at time t.

There are two key frictions in the model of Hanson *et al.* (2020). First, there are credit market frictions parameterized by $\phi > 0$. At $t = 1$, businesses can only borrow against $(1 - \phi)$ of their value in $t = 2$. The limited pledgeability problem is heightened during the Covid-19 crisis because short-run cash flows become less informative about long-run solvency of businesses. Second, there are aggregate demand externalities parameterized by $\gamma > 0$. At $t = 2$, a business experiences positive cash flow of $\gamma \times F_{S_2}$ from F_{S_2} total businesses operating. Intuitively, this captures the fact that a business will experience greater demand when the economy is functioning well and more businesses are open.

At each date t, a business may shut down or continue operating. If a business shuts down, it generates zero cash flow in the current and all future periods. If a business operates, it generates some cash flow. If cash flow is positive, it may go to outside investors. If cash flow is negative, then the business requires investment from investors to continue operating.

If business f operates at $t = 1$, it generates the following cash flow:

$$X_1(f, R_1) = \mu + \gamma - R_1 - \Delta \times f \tag{1.1}$$

where μ captures baseline cash flow which proxies for business solvency, R_1 is the common impact of the recession on business cash flows at $t = 1$, Δ captures the impact of the recession that scales with business exposure, and f captures business exposure to the recession.

If the business f operates in state S_2 at $t = 2$, it generates the following cash flow:

$$X_2(f, R_{S_2}, F_{S_2}) = \mu + \gamma \times F_{S_2} - R_{S_2} - \Delta \times f \tag{1.2}$$

where $\gamma \times F_{S_2} \geq 0$ is the aggregate demand externality that exists at $t = 2$ from having F_{S_2} businesses operating.

If the business f operates in state S_∞ at $t = \infty$, it generates the following cash flow:

$$X_\infty(f, R_{S_\infty}) = \mu + \gamma - R_{S_\infty} - \Delta \times f \tag{1.3}$$

To investigate the impact of CEM on business closure, I augment the model by introducing CEM as mandated zero-interest lending to businesses in amount ρ between $t = 1$ and $t = 2$. Specifically, if CEM is enacted, then businesses are injected with ρ at $t = 1$ and must repay ρ at $t = 2$.

1.2.4 The impact of CEM on business closure

In the online appendix, I solve the model by backward inducting from $t = \infty$ to $t = 2$ to $t = 1$. I enter each state S_t with all businesses $f \in \left[0, F_{S_{t-1}}\right]$ from the preceding state S_{t-1} at time $t - 1$. The task in each period is to find a cutoff $F_{S_t} \leq F_{S_{t-1}}$ such that all businesses $f \in [0, F_{S_t}]$ survive state S_t at time t.

At $t = \infty$, the private value of business f is:

$$V_\infty(f, S_\infty) = \frac{1}{1-\delta} \cdot \max\{X_\infty(f, R_{S_\infty}), 0\} \tag{1.4}$$

At $t = 2$, the private value of business f is:

$$V_2(f, R_{S_2}, F_{S_2}) = \max\{X_2(f, R_{S_2}, F_{S_2}) + \delta \cdot V_\infty(f, R_{s_\infty}), 0\} \tag{1.5}$$

At $t = 1$, the private value of business f is:

$$\begin{aligned} V_1(f, F_1) = \max\{&X_1(f, R_1) \\ &+ (1-\phi) \cdot \delta \cdot [(1-p) \cdot V_2(f, R_{G_2}, F_{G_2}(F_1)) + p \cdot V_2(f, R_{B_2}, F_{B_2}(F_1))], 0\} \end{aligned} \tag{1.6}$$

Let $F^*_{S_t}$ denote the cutoff in the private market outcome without CEM, and let $F^{c*}_{S_t}$ denote the cutoff in the private market outcome with CEM. The following propositions describe the key properties of the solutions.

Proposition 1

Under the assumptions outlined above, strictly more businesses operate at $t = 1$ with CEM than without CEM.

Specifically, we have: $F^{c*}_1 - F^*_1 > 0$.

By providing financial support to all businesses, CEM enables the survival of some liquidity-constrained businesses that could not procure sufficient financing from private markets alone. There are two reasons that these businesses would be supported by CEM but would not be supported by the private market. The first reason is that credit market frictions prevent these businesses from borrowing against the full value of their future cash flows. The second reason is that these businesses are insolvent with $V_1 < 0$ and private market investors would lose money by investing in these businesses at $t = 1$. Because CEM mandates that commercial landlords provide liquidity relief to their business tenants, these businesses are preserved by CEM at $t = 1$ when they would not have survived without CEM.

After analyzing the short-run effect of CEM on business closure at $t = 1$, I next explore the long-run effect of CEM on business closure at $t = 2$. I also examine the implications of heterogeneity in business solvency on the effectiveness of CEM.

Proposition 2

Under the assumptions outlined above, CEM reduces business closure by $t = 2$ if μ is sufficiently high.

Specifically, $F_2^{c*} - F_2^* > 0$ when μ is sufficiently high.

In the model, μ captures business solvency or fundamental health. A higher μ means that a business has higher cash flow in all time periods and states. When we arrive at $t = 2$, more solvent businesses will be more likely to remain open because they have higher cash flows that they can use to repay CEM borrowing and continue operating. By contrast, less solvent businesses will be more likely to shut down at $t = 2$. By accepting the liquidity relief of CEM at $t = 1$, they have used up the credit of CEM. As a result, at $t = 2$, they owe back rent ($-\rho$) to their landlords but cannot generate enough cash flow to repay back rent, leading them to close.

The propositions yield the following empirical predictions.

Prediction 1

Under substantial financial frictions, CEM reduces closure for businesses in the short run.

Prediction 2

Under substantial financial frictions, CEM is more effective in reducing closure in the long run for more solvent businesses.

1.3 Data and summary statistics

My sample of study will focus on California, where CEM is enacted by local governments for varying amounts of time during the Covid-19 pandemic. California serves as an ideal laboratory for CEM because of its decentralized implementation which creates variation in CEM across space and time that I exploit in the empirical analysis. Another advantage of studying California is that it contains a significant share of U.S. businesses. According to the Small Business Administration, in 2019, there were 4.0 million small businesses employing 7.1 million individuals in California, which accounts for 13% of the number of small businesses and 12% of the small business employment in the United States.

To evaluate the impact of CEM on business closure and employment, it is necessary to collect the enactments and updates of CEM and to observe the closure and employment behavior of businesses. To do so, I hand-collect CEM policies enacted by state, county, and city governments in California and connect them to business closure data from SafeGraph / Advan and business employment data from Homebase.

1.3.1 Commercial eviction moratoria data

One challenge of assessing the impact of CEM on business closure is data availability. CEM is a policy enacted by local governing bodies, rather than the federal government. Accordingly, CEM varies across geographies, and information about the policy is scattered across local government documents. Furthermore, CEM is a real-time response to business stress, which makes it necessary to continuously monitor the policy over time for any extension or premature termination.

By parsing City Council and County Board of Supervisors meeting agendas and minutes from March 2020 through May 2023, I collect a novel dataset on CEM enacted throughout California during the Covid-19 pandemic. Specifically, I record the following details about CEM: the geographic level of the policy (state, county, or city), start date, end date, additional repayment time, final due date of back rent, required number of repayment installments, and applicability to businesses (i.e. all businesses or small businesses only).

Table 1.1 summarizes the key characteristics of CEM at the state, county, and city level. At the state level, the California Judicial Council enacted a CEM through September 1, 2020 by stopping all courts from processing commercial evictions. Additionally, Governor Gavin Newsom issued an executive order giving county and city governments the option to enact CEM until September 30, 2021.

Table 1.1: Summary statistics for commercial eviction moratoria

This table summarizes the key characteristics of CEM policy at the state, county, and incorporated city level.

Panel A: State-level commercial eviction moratoria policy						
1. California Judicial Council enacts state-wide CEM through September 1, 2020						
2. Governor Newsom gives county and city governments the option to enact CEM through September 30, 2021						
Panel B: County-level commercial eviction moratoria policy						
	Obs.	Min.	Median	Max.	Mean	Std dev.
Indicator of CEM enacted by county	58	0.00	0.00	1.00	0.48	0.50
Indicator of CEM applying to unincorporated areas only	58	0.00	0.00	1.00	0.31	0.47
Indicator of CEM applying to entire county	58	0.00	0.00	1.00	0.17	0.38
CEM enacted by county:						
Length of CEM (in years)	58	0.00	0.00	3.24	0.39	0.66
Additional time for repayment (in years)	58	0.00	0.00	1.00	0.08	0.22
Length of CEM+R (in years)	58	0.00	0.00	3.24	0.47	0.77
Number of required installments for repayment	28	1.00	1.00	2.00	1.04	0.19
Panel C: Incorporated city-level commercial eviction moratoria policy						
	Obs.	Min.	Median	Max.	Mean	Std dev.
Indicator of CEM enacted by city	482	0.00	0.00	1.00	0.32	0.47
CEM enacted by city:						
Length of CEM (in years)	482	0.00	0.00	3.18	0.35	0.67
Additional time for repayment (in years)	482	0.00	0.00	2.00	0.13	0.26
Length of CEM+R (in years)	482	0.00	0.00	5.18	0.48	0.86
Number of required installments for repayment	156	1.00	1.00	16	1.53	2.11
CEM that binds:						
Length of CEM (in years)	482	0.50	0.50	3.24	1.02	0.72
Additional time for repayment (in years)	482	0.00	0.00	2.00	0.20	0.29
Length of CEM+R (in years)	482	0.50	0.50	5.22	1.22	0.92
Number of required installments for repayment	482	1.00	1.00	16.00	1.19	1.21

28 of the 58 counties in California exercised the option and enacted CEM. 18 counties enacted CEM over unincorporated areas only, while 10 counties enacted CEM county-wide. County-enacted CEMs averaged 0.39 years (4.6 months) in length with an additional 0.08 years (1.0 month) of repayment time. The vast majority of county-enacted CEMs allow for back rent to be paid in a single installment on the due date. 156 of the 482 cities in California exercised the option and enacted CEM. City-enacted CEMs averaged 0.35 years (4.2 months) in length with an additional 0.13 years (1.6 months) of repayment time. The vast majority of city-enacted CEMs also allow for back rent to be paid in a single installment on the due date.

When there are multiple policies in effect (e.g. state, county, and city), the binding CEM is the one that is most generous to business tenants. At the city level, binding CEMs averaged 1.02 years (12.2 months) in length with an additional 0.2 years (2.4 months) of repayment time. Once again, the vast majority of binding CEM policies allow for back rent to be paid in a single installment on the due date.

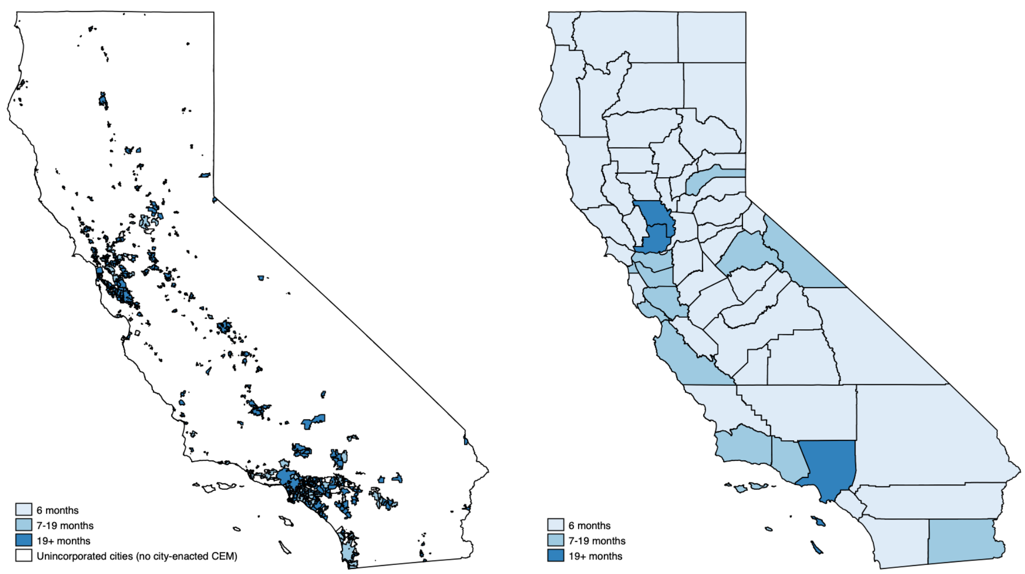
Figure 1.3 illustrates the length of CEM enacted throughout California. The three categories of CEM length correspond intuitively to three categories of local government behavior. The lightest blue category (with CEM of 6 months) consists of cities and counties that deferred to the state-wide CEM enacted by the California Judicial Council. These areas did not enact CEM for any longer than the state policy, although they did have the legal option to do so. The medium blue category (with CEM of 7 to 19 months) consists of cities and counties that exercised their option to enact CEM. The dark blue category (with CEM greater than 19 months) consists of cities and counties that extended their CEM past the maximum date set by Governor Newsom.

Figure 1.3: Commercial eviction moratoria in California

This figure illustrates the length of CEM enacted throughout California. Panel A shows the length of CEM enacted by cities, while Panel B shows the length of CEM enacted by counties. The three categories of length of CEM correspond intuitively to three categories of local government behavior. The lightest blue category (with CEM of 6 months) consists of cities and counties that deferred to the state-wide CEM enacted by the California Judicial Council. The medium blue category (with CEM of 7 to 19 months) consists of cities and counties that exercised the option given to them by Newsom and enacted CEM up to the September 30, 2021 maximum date. The dark blue category (with CEM greater than 19 months) consists of cities and counties that extended their CEM past the maximum date allowed by Newsom's executive order.

Panel A: City-level CEM

Panel B: County-level CEM



1.3.2 Business closure data

The business closure data comes from SafeGraph (now Advan) and covers the effective universe of all business establishments. SafeGraph collects and reconciles business information from many sources, including first-party data and open-source data. A business is flagged as closed when it consistently disappears from this data collection pipeline. The business closure date variable (*closed_on*) is expected to be accurate within 60 days.

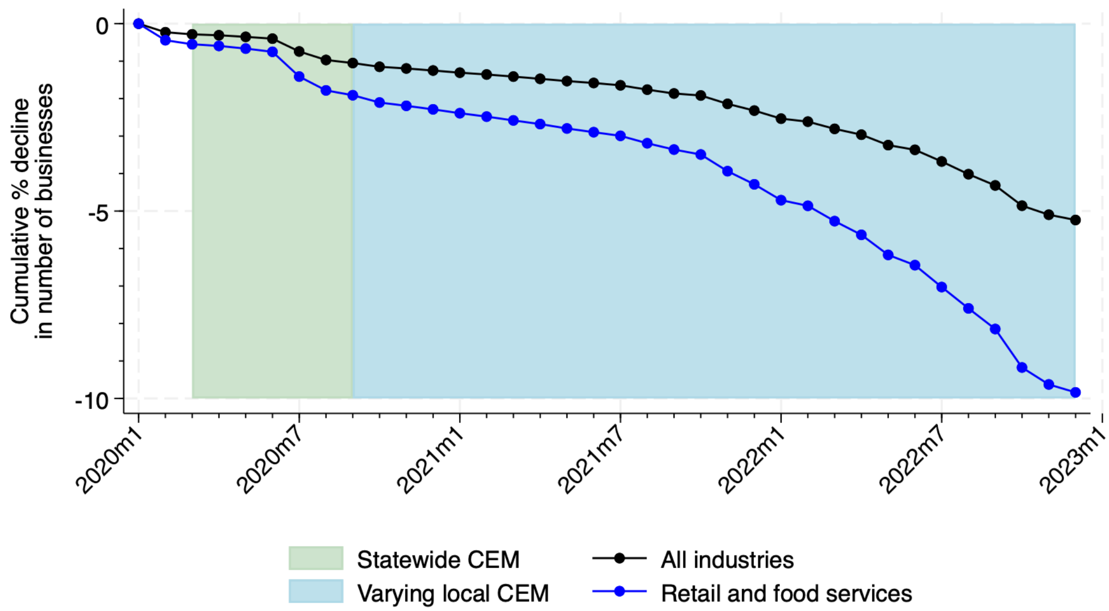
There are a few exceptions to the accuracy of the business closure date. Specifically, there are mechanical spikes in business closure attributed to October 2021, March 2022, April 2022, and June 2022. Non-chain businesses marked as closed on October 2021 actually closed before October 2021, but SafeGraph is not able to pinpoint when. Because some of these business closures could have happened even before the pandemic period, I remove these cases of inaccurate business closure date. After investigation and discussion with the SafeGraph & Dewey teams,[4] I learned that the closure date in March 2022, April 2022, and June 2022 is primarily accurate for businesses that also register customer foot traffic. Therefore, I keep the business closures on these three dates using this criteria and remove cases of inaccurate business closure date. My final sample consists of businesses where the closure date is accurate within 60 days with relatively high confidence.

Using the cleaned sample of business closures, Figure 1.4 illustrates the cumulative percentage decline in number of businesses in California from January 2020 through December 2022 for all industries and for retail and food services. Across all industries, approximately 5% of businesses operating in January 2020 close by the end of 2022. In retail and food services, two industries that are severely affected by the pandemic and that will be the focus of my study, approximately 10% of businesses operating in January 2020 close by the end of 2022.

[4]Please see the discussion post here for more detail: https://community.deweydata.io/t/spike-in-safegraph-closed-on-values-that-is-not-already-discussed-in-documentation/26331/4.

Figure 1.4: Closure of retail and food services businesses in California

This figure illustrates the cumulative percentage decline in number of businesses in California from January 2020 through December 2022 using SafeGraph data. Panel A examines all industries, while Panel B focuses on retail & food services.



Panel A of Table 1.2 provides summary statistics for the SafeGraph business closure data in the industries of retail and food services. Of the retail businesses identified as open in January 2020, 1.86% close in 2020, 1.53% close in 2021, and 4.32% close in 2022. Of the food services businesses identified as open in January 2020, 3.23% close in 2020, 2.80% in 2021, and 7.58% in 2022.

Table 1.2: Summary statistics for business closure and employment

This table provides summary statistics for SafeGraph business closure data in the industries of retail & food services. Observations are at the business level.

Panel A: SafeGraph business closure data				
Retail and food services				
	Obs.	Median	Mean	Std dev.
Likelihood of business closure:				
2020	340,343	0.00	2.35	15.16
2021	340,343	0.00	1.98	13.93
2022	340,343	0.00	5.49	22.78
2020-2022	340,343	0.00	9.83	29.77
Retail				
Likelihood of business closure:				
2020	218,335	0.00	1.86	13.52
2021	218,335	0.00	1.53	12.26
2022	218,335	0.00	4.32	20.33
2020-2022	218,335	0.00	7.71	26.67
Food services				
Likelihood of business closure:				
2020	122,008	0.00	3.23	17.68
2021	122,008	0.00	2.80	16.49
2022	122,008	0.00	7.58	26.47
2020-2022	122,008	0.00	13.61	34.29
Panel B: Homebase business payroll data				
Retail and food services				
	Obs.	Median	Mean	Std dev.
Average hours worked in January 2020	4,242	701.0	990.1	943.0
Scaled hours worked for:				
2020	4,242	0.78	0.79	0.33
2021	3,180	0.89	0.96	0.54
2022	2,688	0.93	1.02	0.57
2020-2022	2,688	0.89	0.95	0.44
Retail				
	Obs.	Median	Mean	Std dev.
Average hours worked in January 2020	787	418.7	652.1	727.0
Scaled hours worked for:				
2020	787	0.81	0.83	0.35
2021	580	0.94	1.03	0.60
2022	487	0.98	1.11	0.64
2020-2022	487	0.94	1.02	0.49
Food services				
	Obs.	Median	Mean	Std dev.
Average hours worked in January 2020	3,455	783.1	1,067.0	969.3
Scaled hours worked for:				
2020	3,455	0.77	0.78	0.33
2021	2,600	0.88	0.94	0.53
2022	2,201	0.92	1.01	0.55
2020-2022	2,201	0.88	0.93	0.43

1.3.3 Business employment data

Business employment data comes from Homebase, a company that provides hourly clock in & out software to small businesses. This data captures business employment at the employee shift level. As documented in the literature, it is not possible to discern whether a business stops appearing in the dataset because it has closed or because it has stopped using the Homebase software, and it can be economically meaningful to distinguish between the two causes (Kurmann *et al.*, 2022; Chetty *et al.*, Forthcoming). Consistent with the literature, I will rely on this dataset to analyze business employment *conditional* on the business being alive.

An important step of data cleaning is to focus on periods when businesses are actively and continuously using Homebase for their employment. First, I define a business as being alive when it first uses the software for 3 consecutive months and as being no longer alive when it stops using the software for 3 consecutive months. This step removes ramp-up periods when a business is only trying out Homebase software and ramp-down periods when a business is phasing out its usage of Homebase software. Next, I distinguish between cases where the business has paused operations during the pandemic (such that employment is truly zero) and cases where the business has stopped using Homebase software (such that employment is non-zero but unobservable in Homebase data). Specifically, if a business stops using Homebase for more than 12 months, it is more likely that the business has stopped using Homebase software than that it has paused operations. Therefore, I drop observations from the 12-month absence on.

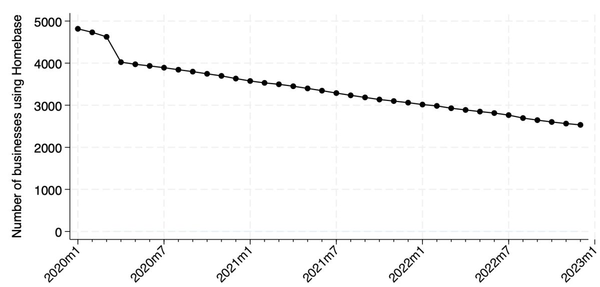
Figure 1.5 illustrates the monthly employment of retail and food services businesses in Homebase from 2020 through 2022. Panel A plots the number of businesses identified as alive on the platform, which declines over time. Panel B plots the average monthly hours worked as a share of hours worked in January 2020. This value declines to approximately 75% in March 2020 and 50% in April 2020 and then recovers in 2021 and 2022 for businesses that remain alive. Panel C plots the average monthly number of shifts worked as a share of number of shifts worked in January 2020. Similarly, this value declines to approximately

75% in March 2020 and 50% in April 2020 and then recovers in 2021 and 2022 for businesses that remain alive.

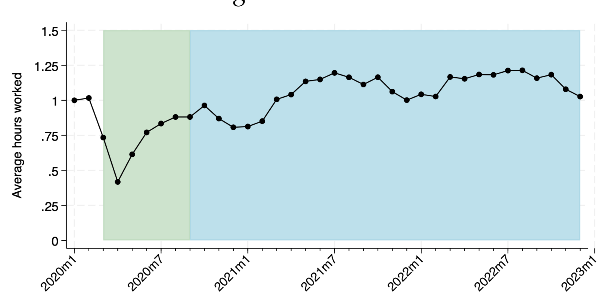
Figure 1.5: Employment of businesses in California

This figure illustrates the monthly employment of retail & food services businesses in Homebase from 2020 through 2022. Specifically, Panel A plots the number of businesses identified as alive on the platform, which declines over time. Panel B plots the monthly hours worked scaled by hours worked in January 2020. Panel C plots the monthly number of shifts worked scaled by number of shifts worked in January 2020.

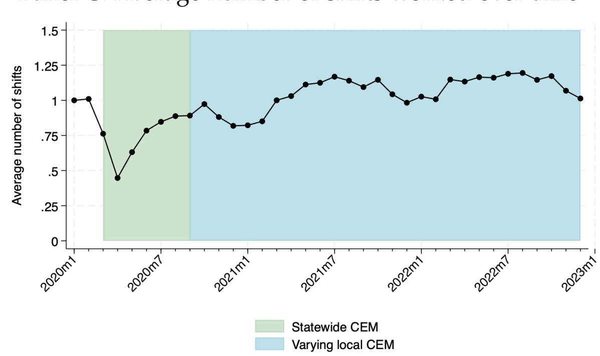
Panel A: Number of businesses using Homebase over time



Panel B: Average hours worked over time



Panel C: Average number of shifts worked over time



Panel B of Table 1.2 provides summary statistics for businesses in retail and food services in the Homebase data. Retail businesses employed workers for an average of 652.1 hours in January 2020, while food services businesses employed workers for an average of 1,067.0 hours in January 2020. As a share of January 2020 hours worked, the hours worked for retail businesses is on average 83% in 2020, 103% in 2021, and 111% in 2022 conditional on the business remaining alive. Meanwhile, as a share of January 2020 hours worked, the hours worked for food services businesses is on average 78% in 2020, 94% in 2021, and 101% in 2022 conditional on the business remaining alive.

1.4 The impact of CEM on business closure

In this section, I estimate the causal impact of CEM on business closure. In Section 1.4.1, I identify the causal impact of CEM on business closure by developing an instrument for CEM based on pre-pandemic partisanship after controlling for alternative channels. I also estimate the impact by year and by industry. In Section 1.4.2, I test the relationship between pre-pandemic business solvency and the effectiveness of CEM in reducing business closure.

1.4.1 Estimating the causal impact of CEM on business closure

In this section, I identify the causal effect of CEM on business closure. The main challenge is the reverse causality from business closure to CEM, which arises from local policymakers enacting CEM precisely to address business closure. For example, in April 2020, the city of San Jose prepared a memo assessing the extent of business financial stress and stating that the aim of CEM is to prevent business closure.[5] Panel A of Figure 1.6 shows a binned scatter plot of the business closure rate in 2020-2022 vs. the length of CEM (including repayment time) for cities in California. There is a positive relationship between business closure rate

[5]Specifically, the memo states, "During this rapidly changing environment caused by the COVID-19 pandemic small businesses across the city are experiencing widespread financial injury. The introduction of a temporary moratorium banning the eviction of small business tenants for the nonpayment of rent directly impacted by the COVID-19 pandemic aims to mitigate preventable business failure and support the city's economy." The document can be found at https://sanjose.legistar.com/LegislationDetail.aspx?ID=4400682&GUID=D54AC5C9-09E6-46DD-8338-1FF8E6A05A05.

and length of CEM. Panel B of Figure 1.6 shows a binned scatter plot of the length of CEM (including repayment time) vs. change in unemployment rate from 2019 to 2020 for cities in California. There is also a positive relationship between length of CEM and the change in unemployment rate, suggesting that cities that are more economically affected by the pandemic also enact longer CEM.[6]

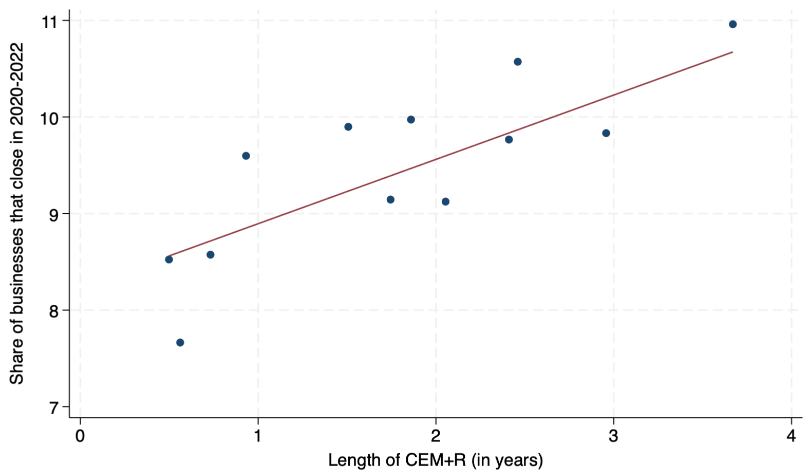
To address this endogeneity between CEM and business closure, I instrument for CEM using pre-pandemic partisanship, measured as the Democratic-Republican spread of the population as of 2019. The key intuition is as follows. Democratic governments have been associated with a greater propensity for government intervention, while Republican governments have been associated with a more laissez-faire approach (Lewis, 2018). To capture the net political leaning of a city, I compute the Democratic-Republican spread as the share of voters registered as Democratic minus the share of voters registered as Republican. This measure of pre-pandemic partisanship is likely to predict the extent of a city government's intervention via CEM. The higher the Democratic-Republican spread prior to the pandemic, the more likely a city government is to enact a longer CEM.

[6] Figure A.1 shows a binned scatter plot of business closure rate in 2020-2022 vs. length of CEM (including repayment time) for unincorporated cities, which are cities that have no municipal government and therefore cannot enact their own CEM. For unincorporated cities, the CEM policy will be determined at the county and state level. The relationship between CEM and business closure is mitigated for these unincorporated cities that do not set their own CEM. In my main analysis, I exclude unincorporated cities because of lack of voting registration data needed for my instrumental variable strategy.

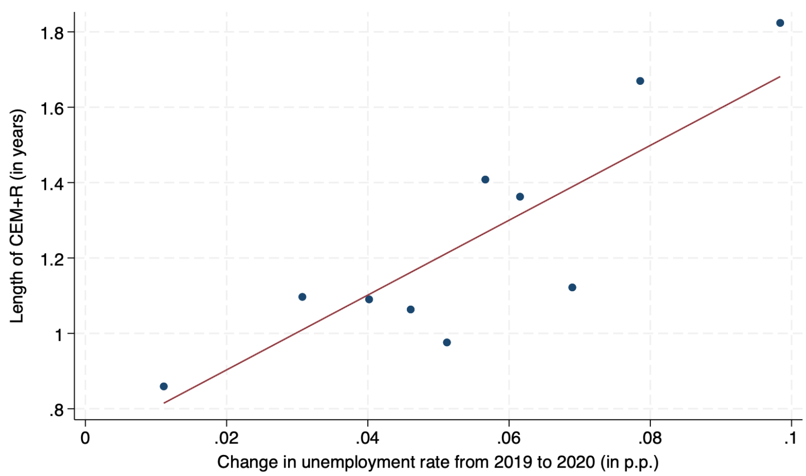
Figure 1.6: Relationship between CEM, business closure, and unemployment

This figure illustrates the binscatter relationship between commercial eviction moratoria policy, business closure rate, and unemployment rate for incorporated cities in California. Panel A presents a binscatter of business closure rate in 2020-2022 v.s. length of CEM (plus repayment time). Panel B presents a binscatter of length of CEM (plus repayment time) v.s. change in unemployment rate from 2019 to 2020.

Panel A: Relationship between business closure and CEM



Panel B: Relationship between CEM and unemployment



There are two requirements for the validity of the instrument: the first stage instrument relevance condition and the exclusion restriction. Figure 1.7 and column 1 of Table 1.3 present the first stage of the instrument. Observations are businesses in the retail and food services industries as of January 2020 in California. There is a positive, statistically significant relationship between partisanship and CEM, wherein a 10 percentage point increase in the Democratic-Republican spread predicts a longer CEM plus repayment time (CEM+R) by 0.28 years (0.10 * 2.77), or 3.3 months. The results confirm the strong first stage of the instrument relevance condition. Next, I consider ways in which the exclusion restriction may be violated by partisanship affecting business closure through non-CEM channels. First, I investigate and control for pre-pandemic economic characteristics to capture the ways that differently partisan places may differ *prior to* the pandemic. Second, I investigate and control for 3 alternative channels that may operate *during* the pandemic, which address consumer stay-at-home behavior, government financial support for businesses, and economic exposure to the Covid-19 shock.

Figure 1.7: Relationship between CEM and partisanship

This figure illustrates the binscatter relationship between the length of CEM (plus repayment time) and the Democrat-Republican spread in 2019 for retail and food services businesses in California.

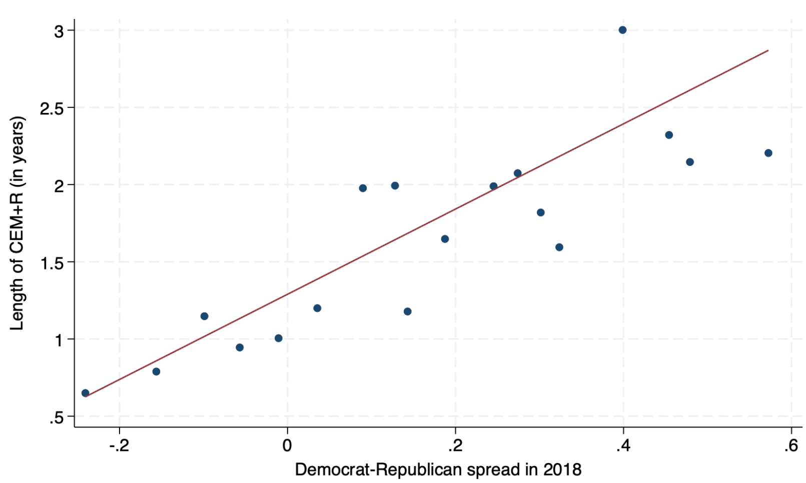


Table 1.3: First stage of pre-pandemic partisanship instrument for commercial eviction moratoria policy

This table shows the relationship between Democratic-Republican spread and length of CEM+R, controlling for zip code-level economic characteristics prior to the pandemic and city-level alternative channels through which partisanship may affect business closure during the pandemic. Observations are at the business level. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
			Years of CEM+R			
Democrat-Republican spread of population in 2019	2.77***	1.85***	1.59***	1.89***	1.73***	1.54***
	(0.29)	(0.43)	(0.46)	(0.41)	(0.42)	(0.46)
City-level characteristics						
Change in foot traffic from January to July-December 2020			-2.68**			-2.43**
			(1.12)			(1.07)
Indicator of having a pandemic business grant program				0.27***		0.18*
				(0.087)		(0.10)
Change in unemployment rate from 2019 to 2020					9.07*	7.24
					(5.37)	(4.94)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	1.29***	-2.25	-1.31	-1.92	-3.58*	-2.25
	(0.18)	(2.16)	(2.39)	(2.09)	(1.88)	(2.07)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.345	0.467	0.501	0.477	0.486	0.519
F-statistic	88.4	102.6	178.2	117.0	83.0	150.6

*** p<0.01, ** p<0.05, * p<0.1

Turning first to pre-pandemic economic characteristics, Table A.1 illustrates the balance of pre-pandemic covariates along the axis of pre-pandemic partisanship. Specifically, the table summarizes the way that more Democratic places observably compare to more Republican areas within California using zip code-level data as of 2019 from the 5-year American Community Survey. For each variable, the first 3 columns provide summary statistics for places with above-median Democratic-Republican spreads, while the next 3 columns provide summary statistics for places with below-median Democratic-Republican spreads. The last column computes the difference in mean and tests whether it is statistically significantly different from zero.

I find no significant difference between the two groups in terms of population, income, unemployment rate, or share of businesses in retail or food services. The observable differences are as follows. Relative to more Republican places, more Democratic places have a higher non-white population share, a lower homeownership rate, a higher population density, and a greater likelihood of being an urban area. After controlling for these pre-pandemic economic characteristics, column 2 of Table 1.3 shows that the relationship between partisanship and length of CEM remains statistically significant and economically meaningful.

Next, I consider 3 alternative channels that may operate during the pandemic, where partisanship may affect business closure through non-CEM channels. First, in more Democratic cities, individuals may be more likely to stay home. This can occur because more Democratic governments enact more pandemic restrictions, more Democratic individuals are more likely to adhere to those restrictions, and more Democratic individuals are more likely to stay at home even in the absence of restrictions.[7] Greater consumer stay-at-home behavior would reduce consumer demand and increase the likelihood of business closure. If not addressed, this channel could lead to under-estimating the impact of CEM on business closure. I address this threat to the exclusion restriction by controlling for a measure of consumer stay-at-home behavior using SafeGraph foot traffic data. Specifically, I compute the decline in number of visits to all businesses in a city from January 2020 to the latter half of 2020.[8] Column 1 of Table A.2 shows that there is a negative, statistically significant relationship between partisanship and change in foot traffic. After controlling for this change in foot traffic, column 3 of Table 1.3 shows that the relationship between partisanship and length of CEM remains statistically significant and economically meaningful.

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[7]Gollwitzer *et al.* (2020) document "partisanship differences in physical distancing." The Washington Post has documented less citizen adherence to coronavirus restrictions in Republican counties. Wang *et al.* (2021) documents that Democratic governors are more likely to issue stay-at-home orders. Canes-Wrone *et al.* (2022) document that more Democratic populations are more likely to wear masks and practice social isolation.

[8]For the latter half of 2020, I use the number of visits to businesses averaged over the months of July through December 2020.

Second, more Democratic governments may be more likely to engage in intervention that financially supports businesses, which would in turn reduce business closure. If not addressed, his channel could lead to over-estimating the impact of CEM on business closure. To address this threat to the exclusion restriction, I collect data on pandemic business grant programs administered by cities and control for an indicator of having a pandemic business grant program.[9] Column 2 of Table A.2 shows that there is a positive relationship between partisanship and an indicator for the city administering a pandemic business grant program. After controlling for this city business grant support, column 4 of Table 1.3 shows that the relationship between partisanship and length of CEM remains statistically significant and economically meaningful.

Third, more Democratic places may be differently exposed to the Covid-19 shock relative to more Republican places, which in turn leads to differences in business closure.[10] If more Democratic places are more (less) exposed to the Covid-19 shock, then this channel could lead to under-estimating (over-estimating) the impact of CEM on business closure. To address this threat to the exclusion restriction, I use the change in unemployment rate from 2019 to 2020 as a proxy for the economic severity of the pandemic for a city. Column 3 of Table A.2 shows that there is a positive, statistically significant relationship between partisanship and change in unemployment rate. After controlling for the change in unemployment rate, column 5 of Table 1.3 shows that the relationship between partisanship and length of CEM remains statistically significant and economically meaningful.

Finally, after controlling for all three alternative channels, column 6 of Table 1.3 shows that a 10 percentage point increase in the Democratic-Republican spread significantly predicts a longer CEM by 0.15 years (0.10 * 1.54), or 1.8 months.[11] Table A.3 shows that

[9]The main findings are similar if I instead control more continuously for city business support by using the share of city business grant funding over the city's federal funding received from the American Recovery Plan Act.

[10]For example, the New York Times has documented that more Democratic areas are harder hit by the disease due to more densely populated areas, greater share of ethnic minorities and lower-income individuals.

[11]The coefficient on the Democratic-Republican spread is 1.54 in column 6 relative to 2.77 in column 1, which suggests, that pre-pandemic partisanship maintains 56% of its strength in predicting CEM policy after

this relationship holds true when we instead measure the strength of CEM policy using the length of CEM only (not including repayment time R). Table A.6 shows that this relationship holds true when we control non-parametrically for alternative channels by using quintile fixed effects. Table A.9 shows that this relationship holds true when we control non-parametrically for both alternative channels and pre-pandemic characteristics using quintile fixed effects. In sum, the pre-pandemic partisanship instrument maintains a strong first stage in predicting CEM policy after addressing pre-pandemic economic characteristics and alternative channels during the pandemic.

Table 1.4 presents the second-stage estimate of the impact of CEM on business closure. In the second stage, I control for the 3 alternative pandemic channels as well as pre-pandemic zip code-level characteristics, which makes it more likely that the instrument exclusion restriction holds. The identifying assumption is that conditional on consumer stay-at-home behavior, business grant funding, exposure to the Covid-19 economic shock, and pre-pandemic characteristics, partisanship does not affect business closure except through CEM. In the first 3 columns, I progress from the OLS to the IV regression specification. In column 1, I regress an indicator for business closure in 2020-2022 on the length of CEM+R in years. The result shows that an additional year of CEM+R is associated with a higher probability of business closure, which stems from the endogenous relationship between CEM and business closure. In column 2, I control for pre-pandemic zip code-level characteristics, which leads to a reduction in the economic magnitude and statistical significance of the coefficient on length of CEM. In column 3, I estimate the instrumented specification, which changes the sign of the coefficient from positive to negative and reveals a significant, negative effect of CEM on business closure. The results suggest that after addressing the reverse causality issue and mitigating threats to the exclusion restriction, CEM has an overall negative causal effect on business closure.

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controlling for pre-pandemic characteristics and addressing the 3 alternative channels during the pandemic.

Table 1.4: Impact of commercial eviction moratoria on business closure

This table presents the second stage estimate of the impact of CEM on business closure after controlling for non-CEM channels through which partisanship may impact business closure. Observations are businesses in the industries of retail and food services. The first three columns progress from the OLS specification to the IV specification, while the last three columns decompose the IV estimate by year. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of business closing in: 2020-2022			Likelihood of business closing in:		
				2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM+R	0.65***	0.11	-0.80	-0.30	-0.55***	0.049
	(0.079)	(0.12)	(0.50)	(0.19)	(0.19)	(0.22)
City-level characteristics						
Change in foot traffic from January to July-December 2020			-3.09	-0.97	-2.15**	0.032
			(1.91)	(0.78)	(1.02)	(0.80)
Indicator of having a pandemic business grant program			0.11	0.018	0.065	0.028
			(0.31)	(0.072)	(0.12)	(0.20)
Change in unemployment rate from 2019 to 2020			12.1	2.41	3.80	5.95
			(9.41)	(3.00)	(3.63)	(4.14)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	8.66***	-10.9*	-14.1**	-4.28**	-3.42***	-6.41
	(0.20)	(6.08)	(6.03)	(2.03)	(1.28)	(4.08)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.001	0.003				

*** p<0.01, ** p<0.05, * p<0.1

In the last 3 columns, I estimate the impact of CEM on business closure by year. I observe reductions in business closure in 2020 and 2021, where the results are especially economically meaningful and statistically significant for 2021. An additional year of CEM+R leads to a reduction in business closure by 0.30 percentage points in 2020 and 0.55 percentage points in 2021 (12.8% and 27.8% of the mean, respectively). Then in 2022, after CEM is lifted, there is no further effect. Table A.4 shows that this finding holds true when we instead measure the strength of CEM policy using the length of CEM only (not including repayment time R). Table A.7 shows that this finding holds true when we instead control non-parametrically for alternative channels by using quintile fixed effects. Table A.10 shows that this finding holds true when we instead control non-parametrically for both alternative channels and pre-pandemic economic characteristics by using quintile fixed effects. In sum, the results suggest that CEM reduces business closure in 2020 and 2021 while the policy is in effect.

Next, I consider the differential effect of CEM in retail vs. food services. Table 1.5 presents the results for retail in the first 4 columns and food services in the last 4 columns. Turning first to retail, column 1 shows that an additional year of CEM+R reduces business closure by 0.31 percentage points over the period 2020-2022. By year, columns 2 through 4 show that an additional year of CEM+R reduces business closure by 0.23 percentage points in 2020 and 0.44 percentage points in 2021, which are partly reversed by an increase in business closure of 0.36 percentage points in 2022. Turning next to food services, column 5 shows that an additional year of CEM+R significantly reduces business closure by 1.84 percentage points over the period 2020-2022, which is a significantly larger effect than in retail. By year, columns 6 through 8 show that, an additional year of CEM+R reduces business closure by 0.49 percentage points in 2020, 0.77 percentage points in 2021, and 0.58 percentage points in 2022. Given that the total number of food services establishments in California in 2019 is 85,223 from the Statistics of U.S. Businesses (SUSB) data, these estimates implies that an additional year of CEM+R preserves 1,568 food services businesses in California over the period 2020-2022. When we consider that the average length of CEM+R is 1.22 years, these estimates suggest that the average CEM policy preserves 1,913 food services businesses in California over the period 2020-2022.

Table 1.5: Impact of commercial eviction moratoria on business closure: Retail v.s. food services

This table presents the second stage estimate of the impact of CEM on business closure by industry and by year. Observations are businesses in the industries of retail and food services. In the first four columns, I present the result for the retail industry, while in the last four columns, I present the result for the food services industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail				Food services			
	Likelihood of business closing in:				Likelihood of business closing in:			
	2020-2022	2020	2021	2022	2020-2022	2020	2021	2022
Years of CEM+R (instrumented)	-0.31	-0.23	-0.44**	0.36**	-1.84**	-0.49*	-0.77***	-0.58
	(0.38)	(0.16)	(0.18)	(0.17)	(0.81)	(0.28)	(0.27)	(0.50)
City-level characteristics								
Change in foot traffic from January to July-December 2020	-0.88	-0.67	-1.52*	1.31	-8.40**	-1.93	-3.45**	-3.03*
	(1.37)	(0.63)	(0.91)	(1.28)	(3.96)	(1.45)	(1.40)	(1.79)
Indicator of having a pandemic business grant program	0.073	0.14	0.080	-0.15	0.070	-0.24*	0.0099	0.30
	(0.28)	(0.10)	(0.12)	(0.18)	(0.50)	(0.14)	(0.14)	(0.32)
Change in unemployment rate from 2019 to 2020	11.3*	1.10	3.71	6.46**	16.1	5.87	4.02	6.26
	(5.83)	(3.35)	(3.13)	(2.96)	(19.0)	(4.25)	(5.68)	(11.3)
Zip code-level characteristics as of 2019	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-9.73*	-2.58	-1.98*	-5.16	-23.0**	-8.03***	-5.21**	-9.74
	(5.05)	(1.96)	(1.20)	(3.16)	(9.98)	(2.97)	(2.46)	(7.57)
Observations	218,335	218,335	218,335	218,335	122,008	122,008	122,008	122,008

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

The finding that CEM is substantially more effective in reducing business closure in food services than in retail holds true when we conduct the following robustness exercises. Table A.5 shows that the finding holds true when we instead measure the strength of CEM policy using the length of CEM only (not including repayment time R). Table A.8 shows that the finding holds true when we instead control non-parametrically for alternative channels by using quintile fixed effects. Table A.11 shows that the finding holds true when we instead control non-parametrically for both alternative channels and pre-pandemic characteristics uisng quintile fixed effects.

1.4.2 Business solvency & the impact of CEM on business closure

In this section, I analyze the relationship between business solvency and the impact of CEM on business closure.

Motivated by the differing effectiveness of CEM in retail v.s. food services, I consider differences in business solvency as one explanation. Prior to the pandemic, the retail industry was experiencing loss of market share to e-commerce while the food services industry was experiencing steady growth. Panel A of Figure 1.8 presents the evolution of employment for non-farm industries overall and for retail and food services specifically. Employment is indexed to 2010. The growth of employment in retail has slowed and even turned negative over the last 10 years, such that employment in retail has grown substantially more slowly than in non-farm employment overall. This fact is reflective of the "retail apocalypse" phenomenon established in the news and academic literature. Meanwhile, employment in food services has grown substantially more rapidly than non-farm industries overall and retail. In sum, this pre-pandemic context of the retail apocalypse and the steady growth of food services suggests that, coming into the pandemic, businesses in food services were relatively more solvent compared to businesses in retail.

To discipline this hypothesis, I take advantage of variation within the retail industry and test the prediction that CEM is more effective in reducing business closure in retail sub-industries that are more solvent coming into the pandemic. Specifically, I split the broad retail industry (NAICS 2-digit) into ten sub-industries (NAICS 3-digit).[12] Panel B of Figure 1.8 presents the evolution of employment in sub-industries within retail. Over the period 2010-2019, the sub-industry with the fastest growth in employment is motor vehicle & parts dealers (NAICS 441), while the sub-industry with the slowest growth in employment is electronics & appliance stores (NAICS 443).

Using pre-pandemic employment growth rate as a proxy for business solvency, I examine the relationship between business solvency and the impact of CEM on business closure across sub-industries. Figure 1.9 plots the impact of CEM on business closure vs. the 5-year

[12]Note that food services is already classified as a sub-industry (NAICS 3-digit).

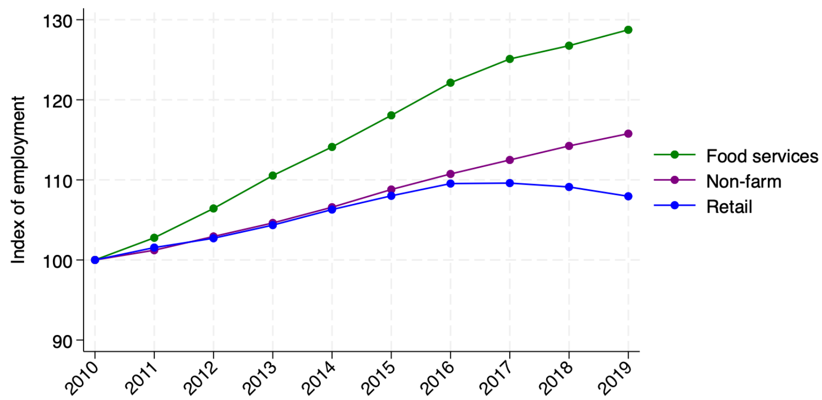
pre-pandemic employment growth across sub-industries. Retail sub-industries with faster pre-pandemic growth (e.g. motor vehicle & parts dealers, gas stations) tend to experience greater CEM-induced reductions in business closure relative to retail sub-industries with slower pre-pandemic growth (e.g. clothing & accessories stories, electronics & appliances stores). Also included in the figure is food services, which is already classified as a sub-industry (NAICS 3-digit). Food services experienced higher pre-pandemic employment growth than every retail sub-industry and accordingly a significant reduction in business closure from CEM.[13]

[13] Figure A.2 shows that this relationship holds true when we instead measure CEM business closure effects as a percentage of the closure rate over the period 2020-2022. Figure A.3, Figure A.4, and Figure A.5 show that this relationship holds true when we instead proxy for industry solvency using the 5-year pre-pandemic growth in sales, number of firms, or number of establishments, respectively.

Figure 1.8: Index of employment for non-farm, retail, and food services

This figure shows employment in different industries. Panel A presents non-farm, retail, and food services employment. Panel B presents employment for sub-industries within retail.

Panel A: Employment by industry



Panel B: Employment of sub-industries in retail

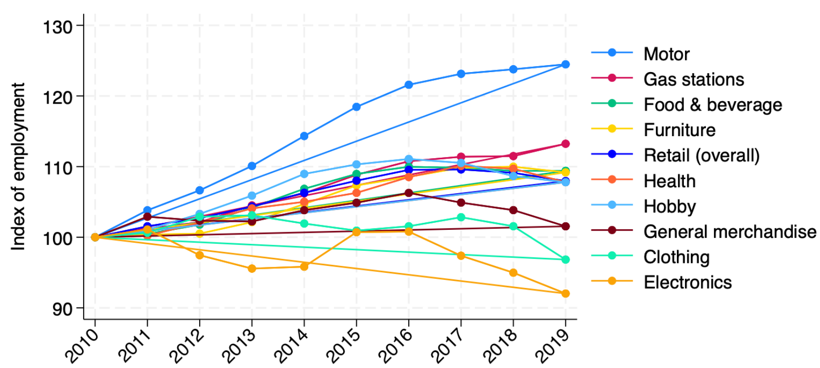


Figure 1.9: Relationship between pre-pandemic employment growth and CEM business closure effects

This figure shows the relationship between the 5-year pre-pandemic employment growth and the effect of CEM on business closure for subindustries in retail and food services.

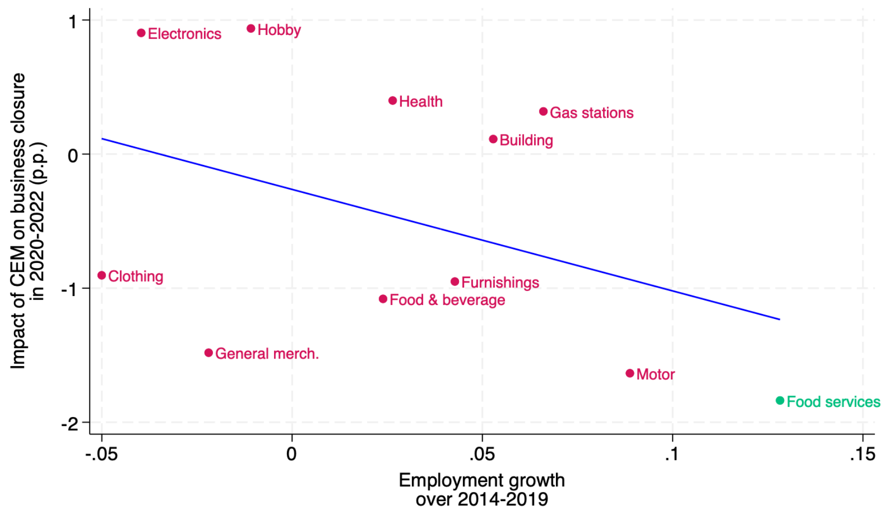


Table 1.6 examines the role that pre-pandemic growth plays in determining the effect of CEM on business closure. Specifically, I conduct the same analysis as in Table 1.5 but add an interaction term between length of CEM+R and the growth in sub-industry employment prior to the pandemic (from 2014 to 2019). The coefficient on this interaction term captures the way that an additional percentage point of pre-pandemic employment growth changes the effectiveness of CEM in reducing business closure. In column 1, I find that an additional percentage point of pre-pandemic employment growth leads an additional year of CEM+R to further reduce business closure by 0.035 percentage points over the period 2020-2022. In columns 2 through 4, I decompose this effect by year, finding that an additional percentage point of pre-pandemic employment growth leads an additional year of CEM+R to further reduce business closure by 0.021 percentage points in 2021 and 0.032 percentage points in 2022. Previously Table 1.5 established that, in the retail industry, CEM leads to reductions in business closure in 2020 and 2021, which are partly reversed in 2022 as CEM policies

are lifted. The finding in Table 1.6 additionally reveals that pre-pandemic growth has the largest effect on the effectiveness of CEM in 2022 and therefore makes CEM more effective by preventing increases in business closure in 2022 when CEM is lifted.

Table 1.6: Impact of commercial eviction moratoria on business closure: Variation in pre-pandemic growth within the retail industry

This table presents the second stage estimate of the impact of CEM on business closure by year for the retail industry. Observations are businesses in the retail industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)
	Retail			
	Likelihood of business closing in:			
	2020-2022	2020	2021	2022
Years of CEM+R (instrumented)	-0.17	-0.21	-0.43**	0.47**
	(0.33)	(0.19)	(0.18)	(0.22)
Growth in sub-industry employment from 2014 to 2019	-0.18***	-0.13***	0.0040	-0.051**
	(0.028)	(0.026)	(0.020)	(0.024)
Years of CEM+R x Growth in sub-industry employment from 2014 to 2019 (instrumented)	-0.035	0.018	-0.021*	-0.032***
	(0.025)	(0.016)	(0.012)	(0.013)
City-level characteristics				
Change in foot traffic from January to July-December 2020	-0.64	-0.41	-1.64*	1.41
	(1.26)	(0.65)	(0.96)	(1.43)
Indicator of having a pandemic business grant program	0.14	0.12	0.066	-0.040
	(0.28)	(0.10)	(0.12)	(0.19)
Change in unemployment rate from 2019 to 2020	9.61*	-0.26	4.12	5.75
	(5.17)	(3.22)	(3.08)	(3.85)
Zip code-level characteristics as of 2019	Yes	Yes	Yes	Yes
Constant	-6.05	-1.07	-1.77	-3.20
	(4.92)	(2.09)	(1.32)	(2.81)
Observations	192,247	192,247	192,247	192,247

*** p<0.01, ** p<0.05, * p<0.1

As well documented in the news & literature, there was already a retail apocalypse phenomenon underway prior to the pandemic.[14] While both retail and food services are very exposed to the pandemic, they had been experiencing different trends leading up to the pandemic, which can explain why food services experienced permanent reductions in business closure due to CEM while retail experienced temporary reductions. Focusing on the early period of the Covid-19 crisis, Bartik *et al.* (2020b) find that pre-pandemic firm health predicts a lower likelihood of firm closure and a higher likelihood of firm reopening. I find a similar pattern of pre-pandemic firm health mattering for firm survival that occurs at the industry level and over a longer horizon (2020-2022).

1.5 The impact of CEM on business financing

In this section, I investigate the impact of CEM on business financing. By providing liquidity relief, CEM may have effects on business take-up of other sources of financing. The reason that CEM may have ripple effects on businesses and shift their usage of other financing is because CEM is the lowest-cost form of financing with an interest rate of zero. Therefore, in Section 1.5.1, I estimate the effect of CEM on business borrowing and grant take-up from federal government programs. Additionally, by requiring commercial landlords to provide liquidity relief, CEM may have effects on their financial health. The reason that CEM may have ripple effects on commercial landlords and affect their financial health is because they can have commercial mortgages and CEM does not pause their payment obligations. Therefore, in Section 1.5.2, I estimate the effect on of CEM on the performance of commercial mortgages contained in commercial mortgage-backed securities.

1.5.1 The financing effects on businesses

In this section, I estimate the effect of CEM on business take-up of other sources of financing. There are two other types of financing available to businesses during the Covid-19

[14]For example, Trepp has reported that the Covid-19 crisis amplified the retail apocalypse phenomenon. The article can be found at https://www.trepp.com/trepptalk/covid-accelerated-the-retail-apocalypse.

pandemic. One type of financing is loans, which must be repaid. In this case, there is likely to be substitution away from loan financing and toward CEM liquidity relief because the loan is more costly than the zero-interest liquidity relief of CEM. Specifically, I examine business take-up of loans from the Economic Injury Disaster Loan program. Another type of financing is grants, which do not need to be repaid. In this case, there is unlikely to be substitution because grants improve business profitability. Specifically, I examine business take-up of grants from the EIDL Advance program and the Paycheck Protection Program.

Impact of CEM on business borrowing from EIDL

In this section, I estimate the effect of CEM on business borrowing from the Economic Injury Disaster Loan (EIDL) program. Through this federal business loan program, the Small Business Administration provides low-interest, fixed-rate, long-term loans directly to small businesses that can be used for working capital, operating expenses and debt repayment during the Covid-19 pandemic. To be eligible, a business must have fewer than 500 employees and must not have access to other credit. Under the latest program guidelines, EIDL loans have a maximum amount of $2 million, a term of 30 years, and a fixed interest rate of 3.75%. No payment is required for the first 2 years, and there is no penalty for prepayment. The program opened for applications in March 2020 and closed by January 2022.

CEM is a lower-cost source of liquidity relief for businesses than EIDL. While EIDL allows businesses to borrow at 3.75%, CEM effectively allows businesses to borrow at 0% interest. Therefore, if businesses are not so constrained that they demand full liquidity relief of both EIDL and CEM, then they will substitute away from the higher-cost EIDL borrowing and towards the lowest-cost CEM. In this case, CEM will have a negative effect on EIDL borrowing.

I obtain details on loans administered through this program from the public database on federal spending.[15] Throughout the U.S., the COVID EIDL program disbursed $\sim$3.9 million

[15]Specifically, the data is available at USASpending.gov.

loans totaling ∼\$380 billion. In my sample of California, the program disbursed ∼600,000 loans totaling ∼\$68 billion. My analysis proceeds at the zip code level. After connecting SafeGraph business establishment data and EIDL loan data at the zip code level, I compute the average dollars of borrowing per business by dividing the total dollars of EIDL loans disbursed to a zip code by the total number of business establishments in the zip code. Each zip code-level observation therefore represents the EIDL borrowing behavior of the average business in the zip code.

Table 1.7 presents the results on the effect of CEM on EIDL borrowing. In the first 3 columns, I progress from the OLS to the IV specification. In column 1, I find that an additional year of CEM+R is associated with higher EIDL borrowing. This remains true in column 2, after controlling for zip code characteristics. In column 3, instrumenting for CEM using pre-pandemic partisanship reveals that an additional year of CEM+R leads to a statistically significant reduction in EIDL borrowing by \$11,717 (25.0% of its mean). Given that CEM+R is 1.22 years on average, the average CEM policy reduces EIDL borrowing by \$14,295 per business. Therefore, I find overall evidence of businesses substituting away from EIDL borrowing in response to CEM liquidity relief.

I then decompose EIDL loan-takeup into the extensive margin (the likelihood of taking out an EIDL loan) and intensive margin (the size of EIDL loans taken out). In column 4, I find that an additional year of CEM+R leads to a statistically significant reduction in likelihood of borrowing by 7.42 percentage points, while in column 5, I find that an additional year of CEM+R leads to a statistically significant reduction in the amount of EIDL loans by \$8,723 (8.1% of its mean). In sum, the liquidity relief of CEM leads businesses to substitute away from EIDL borrowing. This happens primarily through the intensive margin as businesses take out loans of smaller amounts.

Table 1.7: Impact of commercial eviction moratoria on business borrowing from the EIDL loan program

This table presents the second-stage estimate of the impact of CEM on average business borrowing from the Economic Injury Disaster Loan program. Observations are zip codes that contain at least 20 businesses. The three columns progress from the OLS specification to the IV specification. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)
	Dollars of EIDL loan per business			Likelihood of taking out EIDL loan	Average size of EIDL loan
Specification:	OLS	OLS	IV	IV	IV
Years of CEM+R	4,948	6,898	-11,713*	-7.42*	-8,723*
	(3,336)	(5,048)	(6,356)	(4.46)	(4,920)
City-level characteristics					
Change in foot traffic from January to July-December 2020			-59,394**	-21.9	-80,733***
			(25,414)	(18.9)	(17,446)
Indicator of having a pandemic business grant program			10,648***	6.81***	6,350*
			(3,768)	(2.21)	(3,450)
Change in unemployment rate from 2019 to 2020			554,494***	553***	-97,905
			(172,538)	(146)	(69,249)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes
Constant	39,248***	-133,266	-189,112***	-211***	123,131
	(3,756)	(81,209)	(61,096)	(59.8)	(87,463)
Observations	977	977	977	977	977
R-squared	0.020	0.112			

*** p<0.01, ** p<0.05, * p<0.1

Impact of CEM on government grant take-up

In this section, I examine the effect of CEM on business grant take-up from the EIDL advance program and Paycheck Protection Program. The EIDL advance program provides grants of up to $15,000 to businesses that can be used for working capital and operating expenses during the Covid-19 pandemic. EIDL advances do not need to be repaid.[16]

[16] This program consists of two parts. First, the targeted EIDL advance provides grants of up to $10,000 to hard-hit businesses. To be eligible, a business must be located in a low-income community, experience a 30% or greater economic loss over an 8-week period during the pandemic, and have no more than 300 employees. Second, the supplemental EIDL advance provides additional grants of up to $5,000 for especially hard-hit businesses. To be eligible, a business must experience a 50% or greater economic loss and have no more than 10 employees.

Meanwhile, the Paycheck Protection Program provides forgivable loans to small businesses to be used primarily for payroll, rent, utilities, and interest on mortgages.[17] The loan amount is determined as a multiple of a business's monthly payroll cost. Loans are fully forgiven if the business retains employees and uses at least 60% of funds for payroll. In this way, PPP is effectively a business grant, albeit one with conditions attached.

While CEM gives businesses more time to pay rent without forgiving rent, grants give businesses funds that do not need to be repaid. Therefore, businesses will seek out grants irrespective of CEM policy because grants improve business profitability. As a result, there is unlikely to be substitution away from grants toward CEM liquidity relief, and it is likely that CEM has no effect on business grant take-up.

I obtain details on grants administered through the EIDL advance program from the public database on federal spending and details on forgivable loans administered through PPP from the Small Business Administration website. Throughout the U.S., the EIDL advance program disbursed ∼1 million grants totaling ∼$7.5 billion. In my sample of California, the program disbursed ∼150,000 grants totaling ∼$1 billion. Throughout the U.S., PPP disbursed ∼11.8 million forgivable loans totaling face value of ∼$800 billion. In my sample of California, the program disbursed ∼1.3 million forgivable loans totaling face value of ∼$100 billion. My analysis proceeds at the zip code level. I compute the average dollars of EIDL advance grants per business by dividing the total dollars of EIDL advances disbursed to a zip code by the total number of business establishments in the zip code. Similarly, I compute the average dollars of PPP forgivable loans per business by dividing the total dollars of PPP disbursed to a zip code by the total number of business establishments in the zip code. Each zip code-level observation therefore captures the EIDL advance take-up and PPP take-up of the average business in the zip code.

Table 1.8 presents the results on the effect of CEM on EIDL advance take-up. In the first 3 columns, I progress from the OLS to the IV specification. In column 1, I find that

[17] PPP loans are disbursed in three rounds. The first round allocated $349 billion to businesses over the course of two weeks in April 2020. The second round allocated $176 billion to businesses between April and August 2020. The third round allocated $284 billion to businesses between January and June 2021.

an additional year of CEM+R is associated with higher EIDL advance take-up, which is consistent with cities enacting longer CEM in response to greater business closure likelihood and distress. In column 3, instrumenting for CEM using pre-pandemic partisanship reveals that an additional year of CEM+R has no statistically significant effect on EIDL advance take-up. In column 4, I find that CEM has no significant effect on the extensive margin of EIDL advance take-up, which is the likelihood of receiving an EIDL advance. In column 5, I find that CEM has no significant effect on the intensive margin of EIDL advance take-up, which is the average size of EIDL advances.

Table 1.8: Impact of commercial eviction moratoria on business grant takeup from the EIDL advance program

This table presents the second-stage estimate of the impact of CEM on average business grant takeup from the EIDL advance program. Observations are zip codes that contain at least 20 businesses. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)
	Dollars of EIDL advance per business			Likelihood of receiving EIDL advance	Average size of EIDL advance
Specification:	OLS	OLS	IV	IV	IV
Years of CEM+R	119**	-5.42	62.7	0.50	135
	(54.0)	(38.3)	(93.6)	(0.66)	(244)
City-level characteristics					
Change in foot traffic from January to July-December 2020			696**	4.98*	-411
			(352)	(2.54)	(1,304)
Indicator of having a pandemic business grant program			7.62	0.078	18.6
			(52.0)	(0.37)	(215)
Change in unemployment rate from 2019 to 2020			5,986***	43.0***	7,886*
			(1,396)	(10.2)	(4,706)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes
Constant	651***	4,991***	4,233***	31.3***	21,285***
	(92.4)	(1,166)	(1,026)	(7.45)	(2,833)
Observations	977	977	977	977	977
R-squared	0.020	0.377			

*** p<0.01, ** p<0.05, * p<0.1

Table 1.9 presents the results on the effect of CEM on PPP take-up. In the first 3 columns, I progress from the OLS to the IV specification. In column 1, I find that an additional year of CEM+R is associated with higher PPP take-up. In column 3, instrumenting for CEM reveals that an additional year of CEM+R has no statistically significant effect on PPP take-up. In column 4, I find that CEM has no significant effect on the extensive margin of PPP take-up, which is the likelihood of receiving PPP. In column 5, I find that CEM has no significant effect on the intensive margin of PPP take-up, which is the average size of PPP forgivable loans. In sum, CEM does not significantly affect grant-takeup from the EIDL advance and PPP programs. Along with the findings on the EIDL loan program, these results are consistent with the hypothesis that by providing low-cost liquidity relief, CEM crowds out take-up of other sources of liquidity but not take-up of solvency relief.

Table 1.9: Impact of commercial eviction moratoria on forgivable loan takeup from the PPP program

This table presents the second-stage estimate of the impact of CEM on forgivable loan takeup from the Paycheck Protection program. Observations are zip codes that contain at least 20 businesses. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)
		Dollars of PPP per business		Likelihood of receiving PPP	Average size of PPP
Specification:	OLS	OLS	IV	IV	IV
Years of CEM+R	1,136	3,526	-4,789	-8.58	-148
	(1,600)	(2,347)	(6,909)	(5.31)	(7,226)
City-level characteristics					
Change in foot traffic from January to July-December 2020			-86,248***	-17.2	-95,203**
			(32,312)	(26.4)	(43,246)
Indicator of having a pandemic business grant program			4,992	8.47**	-1,211
			(5,090)	(3.63)	(4,822)
Change in unemployment rate from 2019 to 2020			197,666**	778***	-349,584**
			(83,410)	(186)	(144,363)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes
Constant	66,275***	-60,795	-39,847	-373***	323,109***
	(4,044)	(97,808)	(91,600)	(107)	(122,676)
Observations	977	977	977	977	977
R-squared	0.001	0.159			

*** p<0.01, ** p<0.05, * p<0.1

1.5.2 The financing effects on commercial landlords

In this section, I estimate the effect of CEM on commercial landlord financial health. Specifically, I examine the performance of commercial mortgages contained in commercial mortgage-backed securities (CMBS). CMBS are formed by pooling and tranching commercial mortgages in order to offer investors different exposures to credit risk. The CMBS market is therefore one source of financing for commercial property purchases. Relative to bank loans, CMBS loans tend to fund larger property purchases and have lower down payment requirements, lower interest rates, and longer terms. Additionally, because there are multiple CMBS investors in contrast to one bank lender, CMBS loans are managed by a third-party CMBS servicer. It can be relatively more difficult therefore to negotiate forbearance in the case that a CMBS loan becomes distressed. Because CMBS have regular reporting requirements, I focus on the CMBS market and use the performance of CMBS loans as a proxy for commercial landlord financial health.

CEM requires commercial landlords to provide tenants with liquidity relief in the form of rent deferral. However, commercial landlords may face their own financial constraints in the form of impending interest and principal payments on mortgages. If the requirement to provide liquidity relief is sufficiently burdensome for commercial landlords, then CEM may erode commercial landlord financial health.

I obtain data from Trepp on retail-use commercial mortgages contained in CMBS, including their payment behavior and performance on a monthly basis. In my sample of California, I observe nearly 1,000 retail-use CMBS loans with outstanding balance of ∼\$17 billion as of January 2020. My analysis proceeds at the loan level. My measure of loan distress is an indicator of 60+ day delinquency.[18]

Table 1.10 presents the results on the effect of CEM on CMBS loan delinquency. In the first 3 columns, I progress from the OLS to the IV specification. In column 1, I find that an additional year of CEM+R is associated with a higher likelihood of loan delinquency.

[18]The findings are robust to measuring loan distress using 30+ day delinquency, rather than 60+ day delinquency.

In column 2, after controlling for zip code characteristics, I observe that the coefficient on length of CEM changes from positive to negative. In column 3, instrumenting for CEM reveals that a longer CEM leads to an increase in loan delinquency. The coefficient is insignificant but still economically meaningful. An additional year of CEM+R leads to an increase in the likelihood of delinquency by 2.04 percentage points (which is 28.5% of its mean). In the last 3 columns, I find that CEM increases the likelihood of loan delinquency in 2020, 2021, and 2022, with increased magnitude over time, although the coefficients are statistically insignificant. In sum, the results show some evidence of the effects of CEM on commercial landlord financial health, although statistical significance is weak because this test is under-powered given the sample of less than 1,000 retail-use CMBS loans in California.

Table 1.10: Impact of commercial eviction moratoria on CMBS loan delinquency

This table presents the second-stage estimate of the impact of CEM on CMBS loan delinquency. Observations are CMBS loans. The outcome variable is the percentage point likelihood of the CMBS loan becoming delinquent by 60+ days. The three columns progress from the OLS specification to the IV specification. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of loan delinquency in 2020-2022			Likelihood of loan delinquency in		
				2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM+R	0.024	-1.44	2.04	0.90	3.96	5.57
	(1.21)	(1.69)	(3.46)	(3.67)	(3.51)	(4.32)
City-level characteristics						
Change in foot traffic from January to July-December 2020			29.5	27.8	30.6	39.3
			(26.8)	(24.9)	(21.0)	(28.8)
Indicator of having a pandemic business grant program			3.26	1.44	-0.29	0.12
			(2.71)	(2.94)	(2.26)	(2.09)
Change in unemployment rate from 2019 to 2020			29.2	56.6	36.4	19.1
			(59.7)	(45.9)	(59.9)	(68.3)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	7.12***	14.7	25.7	5.64	7.12	19.3
	(2.21)	(29.1)	(27.9)	(26.2)	(38.4)	(30.4)
Observations	796	796	796	909	861	796
R-squared	0.000	0.015				

*** p<0.01, ** p<0.05, * p<0.1

1.6 The impact of CEM on business employment

The impact of CEM on business closure established in the last section implies that CEM affects business employment along an extensive margin. In this section, I estimate the impact of CEM on business employment along an intensive margin by re-employing the instrumental variable approach and applying it to business payroll data. In Section 1.6.1, I estimate the impact of CEM on employment conditional on the business being alive, i.e. along an intensive margin. In Section 1.6.2, I take stock of the extensive and intensive margin employment effects and benchmark the overall employment effect of CEM against the Paycheck Protection Program.

1.6.1 The intensive-margin impact of CEM on business employment

To measure business employment, I use hours worked, which is the total number of hours worked by all employees of a business in the Homebase data. Because hours worked captures employment conditional on the business being alive, it is an intensive-margin measure of employment. To make this measure comparable across businesses, I next scale hours worked by the pre-pandemic benchmark of hours worked in January 2020. To understand the construction of the scaled variable, consider this simple example. Suppose a business had 100 hours worked in January 2020. In March 2020, when the pandemic begins, the business reduces hours worked to 50 hours. The scaled hours worked in March 2020 is therefore 50 / 100, or 0.5, which indicates that the business is operating at half of its pre-pandemic benchmark employment. While the SafeGraph business closure data captures the effective universe of business establishments, the Homebase data tends towards small businesses and contains the subset of small businesses that use the Homebase software. Therefore, in this section, I interpret the findings as the impact of CEM on intensive-margin employment for smaller businesses and acknowledge that this analysis works with a smaller sample by nature of the data source.

Table 1.11 presents the results on the impact of CEM on intensive-margin employment. In the first 3 columns, I progress from the OLS to the IV regression specification. In column

1, I find that an additional year of CEM+R is associated with lower employment. In column 2, controlling for zip code characteristics changes the sign of the coefficient on length of CEM from negative to positive, though it is statistically insignificant. In column 3, instrumenting for CEM using pre-pandemic partisanship further increases the magnitude of the coefficient, though it remains statistically insignificant. In the next 3 columns, I examine the impact of CEM on employment by year. I find statistically insignificant effects of CEM on employment in 2020, 2021, and 2022. Table 1.12 presents the analysis split by retail vs. food services, where we again find statistically insignificant effects for each sector and in each year.[19] In sum, the results suggest that there is no significant effect of CEM on intensive-margin employment in this sample of smaller businesses.

To provide a sense of how precisely estimated these non-effects are, I consider their 95% confidence intervals. 95% confidence interval for the estimated effect of CEM on intensive-margin employment ranges from -4.2% to +4.4% of pre-pandemic employment for the overall period 2020-2022. By year, the 95% confidence interval ranges from -3.6% to +4.6% of pre-pandemic employment for 2020, -1.9% to +8.3% of pre-pandemic employment for 2021, and -3.6% to +8.2% of pre-pandemic employment for 2022.[20] One possible explanation for CEM having no intensive-margin effect on employment is that businesses bucket their expenses by category. As a result, they may not change their prioritization of payroll relative to rent in response to CEM.

[19] Table A.12 and Table A.13 show that these findings hold true when we measure employment using the number of shifts worked instead of hours worked.

[20] The precision of the estimate decreases in the later period because of the reduction in number of observations.

Table 1.11: Impact of commercial eviction moratoria on business employment

This table presents the second stage estimate of the impact of CEM on business employment. Observations are businesses in the industries of retail and food services. The measure of employment is hours worked scaled by hours worked in January 2020. The first three columns progress from the OLS specification to the IV specification, while the last three columns decompose the IV estimate by year. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Scaled hours worked in 2020-2022			Scaled hours worked in		
				2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM+R	-0.014**	0.0050	0.0013	0.0049	0.032	0.023
	(0.0070)	(0.0063)	(0.022)	(0.021)	(0.026)	(0.030)
City-level characteristics						
Change in foot traffic from January to July-December 2020			0.35***	0.28***	0.42***	0.35**
			(0.12)	(0.089)	(0.14)	(0.15)
Indicator of having a pandemic business grant program			0.0026	-0.0013	-0.00063	0.026
			(0.021)	(0.018)	(0.022)	(0.024)
Change in unemployment rate from 2019 to 2020			0.37	0.34	0.49	-0.43
			(0.41)	(0.34)	(0.45)	(0.52)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	0.97***	2.19***	2.03***	1.64***	2.27***	2.07***
	(0.013)	(0.50)	(0.43)	(0.17)	(0.43)	(0.68)
Observations	2,688	2,688	2,688	4,242	3,180	2,688
R-squared	0.001	0.016				

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.12: Impact of commercial eviction moratoria on business employment: Retail v.s. food services

This table presents the second stage estimate of the impact of CEM on business employment by industry and by year. Observations are businesses in the industries of retail and food services. The measure of employment is hours worked scaled by hours worked in January 2020. In the first four columns, I present the result for the retail industry, while in the last four columns, I present the result for the food services industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail				Food services			
	Scaled hours worked in:				Scaled hours worked in:			
	2020-2022	2020	2021	2022	2020-2022	2020	2021	2022
Years of CEM+R (instrumented)	-0.0094	0.032	0.040	-0.0053	0.0028	-0.000046	0.029	0.028
	(0.034)	(0.046)	(0.039)	(0.050)	(0.022)	(0.018)	(0.026)	(0.032)
City-level characteristics								
Change in foot traffic from January to July-December 2020	0.46	0.40	0.50	0.45	0.32***	0.26***	0.40***	0.32**
	(0.33)	(0.26)	(0.31)	(0.48)	(0.11)	(0.073)	(0.14)	(0.13)
Indicator of having a pandemic business grant program	-0.037	0.033	-0.073*	-0.045	0.0081	-0.0091	0.014	0.036
	(0.038)	(0.025)	(0.039)	(0.047)	(0.023)	(0.021)	(0.024)	(0.026)
Change in unemployment rate from 2019 to 2020	-0.51	-0.29	-0.56	-0.98	0.59*	0.46	0.80*	-0.27
	(1.20)	(0.90)	(1.30)	(1.12)	(0.34)	(0.33)	(0.44)	(0.51)
Zip code-level characteristics as of 2019	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.36	1.20**	0.47	-0.017	2.38***	1.77***	2.64***	2.51***
	(0.96)	(0.52)	(1.29)	(1.39)	(0.45)	(0.22)	(0.42)	(0.68)
Observations	487	787	580	487	2,201	3,455	2,600	2,201

*** p<0.01, ** p<0.05, * p<0.1

1.6.2 The total impact of CEM on business employment

The total effect of CEM on business employment consists of both the extensive-margin and intensive-margin effects. The results in the last section show no intensive-margin effect. I now turn to the extensive-margin effect, which is captured by the results on business closure. This is because 100% of its employment is lost when a business closes. The results in Section 1.4.1 show an overall 0.80 percentage point decline in business closure likelihood over the period 2020-2022. This implies that the total impact of an additional year of CEM+R on business employment is approximately 0.80 percentage points of January 2020 employment. When we consider that the average length of CEM+R is 1.22 years, the total impact of average CEM+R on business employment is an increase of 0.98 percentage points relative to employment in January 2020.[21]

To benchmark the economic significance of the employment effects of CEM, we take stock of the employment effects of PPP estimated in the literature. Chetty *et al.* (Forthcoming) find that PPP increased the number of jobs by 2.48 percentage points of January 2020 employment.[22] The overall employment effects of CEM that I estimate are approximately 39% (0.98 / 2.48) of the employment effects of PPP. This is striking when we consider that CEM is an intervention that does not inject government funds into businesses and that instead provides liquidity relief through delayed rent payment. Another notable difference is that CEM and PPP have different units of policy. CEM is measured in number of years, such that policymakers must choose how long to pause evictions and delay rent payment. In comparison, PPP is measured in dollars of loans, such that policymakers must choose how much funding to inject into businesses.

1.7 Policy discussion

In this section, I discuss the policy implications of the results. The first implication is on the effectiveness of CEM in reducing business closure. The reduced-form relationship between CEM and business closure during the pandemic reveals that places with longer CEM tended to experience higher rates of business closure. This should not lead us to conclude that CEM has no effect or even counter-productively increases business closure. Estimating the causal effect of the policy requires overcoming reverse causality from business closure to CEM, stemming from the fact that local policymakers choose CEM to respond to business closure. Using plausibly exogenous variation in CEM from pre-pandemic partisanship orthogonalized with respect to pre-pandemic economic characteristics and alternative channels during the pandemic, I find that CEM significantly reduces business closure in

[21] The benchmark is based on the employment in January 2020 because the sample of my business closure results requires a business to operate as of January 2020.

[22] Autor *et al.* (2022), Granja *et al.* (2022), and Hubbard and Strain (2020) also find similarly small effects of PPP on employment.

retail and food services while the policy is in effect. These business closure reductions are sustained in food services even after the policy is lifted. Therefore, identifying the causal effect of CEM reveals that the policy is effective in temporarily reducing business closure and in some cases even effective in permanently reducing business closure.

The second implication of the results is on the effectiveness of CEM for different types of businesses. As discussed in Section 1.2, in the face of substantial frictions in credit markets, CEM can reduce closure for liquidity-constrained, solvent businesses in the long run. Meanwhile, it can only temporarily reduce closure for liquidity-constrained, insolvent businesses. Using pre-pandemic employment growth as a proxy for business solvency, I find that sub-industries with faster pre-pandemic employment growth experience greater CEM-induced reductions in business closure than sub-industries with slower pre-pandemic employment growth. Therefore, investigating heterogeneity in CEM effectiveness along the axis of business solvency reveals that CEM is more effective in reducing long-run closure for businesses that are fundamentally healthier prior to the pandemic.

The above results suggest that CEM is effective overall in reducing business closure and thereby preserving employment. However, drawing conclusions about the efficiency of CEM as a policy requires additional consideration. One consideration is the efficiency of the impact of CEM on business closure. It is socially valuable for CEM to help some solvent businesses survive the crisis. However, because CEM is a broad stroke policy, it also applies to insolvent businesses and can temporarily keep them alive, which has both benefits and costs. A major benefit of doing so is preserving the employment of these businesses during the crisis period. A major cost is preventing reallocation of their labor and capital towards more solvent businesses.

Another consideration is that CEM is rapid and low cost for the government to implement relative to other programs such as the Paycheck Protection Program, Economic Injury Disaster Loan Program, and Main Street Lending Program. Enacting CEM requires only the approval of local government officials and is a zero-expenditure, budget-neutral policy for the government. Indeed, in my sample of California, CEM is enacted within weeks or even days of the state's declaration of emergency. Additionally, by relying on pre-existing relationships between business tenants and commercial landlords, CEM requires no additional intermediation from banks or government agencies and dispenses rent-based liquidity relief that is naturally tied to business operating expenses.

Relatedly, a third consideration is that CEM shifts the burden of credit risk onto commercial landlords. In particular, the continuation of insolvent businesses is costly for their commercial landlords. CEM stipulates that landlords provide zero-interest, non-forgivable credit to all businesses, regardless of solvency. Therefore, insolvent businesses reap benefits from this policy by borrowing from their landlords at no cost despite being unable to repay, and landlords must absorb losses from the inefficient continuation of insolvent businesses. If CEM substantially deteriorates commercial landlord financial health, the policy can force landlords out of business and stop them from performing the valuable economic role of maintaining space and searching for tenants. In sum, my paper evaluates the causal effect of CEM on business closure and employment. Understanding the efficiency of these effects will require holistically taking into account these considerations, which requires further research.

1.8 Conclusion

The Covid-19 pandemic is a sudden shock to businesses, especially small businesses that generate the majority of employment in the U.S. and are an important driver of economic growth. In response to the Covid-19 shock, local governments rapidly enacted CEM for the first time, aiming to prevent immediate business closure by pausing eviction proceedings and to prevent permanent business closure by spurring renegotiation between businesses and landlords.

In this paper, I conduct the first evaluation of the effects of CEM on business closure and employment. Using pre-pandemic partisanship as an instrument, I find that CEM reduces the likelihood of business closure in food services by 0.49 percentage points in 2020 and 0.77 percentage points in 2021. These reductions in business closure are sustained. Meanwhile, in retail, CEM reduces the likelihood of business closure by 0.23 percentage points in 2020 and 0.44 percentage points in 2021, which are partly offset by a rebound of 0.36 percentage points in 2022. I find that CEM is more effective in sustainably reducing closure for more solvent businesses, which is consistent with substantial frictions in credit markets and CEM providing liquidity relief to businesses.

Turning to employment, I find no effect of CEM on intensive-margin employment conditional on the business being alive. Instead, the employment effect of CEM operates along the extensive margin through reduction in business closure. The estimated total impact of CEM on business employment is an increase of 0.98 percentage points, which is 39% of the employment effects of PPP estimated by Chetty *et al.* (Forthcoming). These effects are notable when we consider that CEM is an intervention that does not inject government funds into businesses but rather provides businesses with liquidity relief from their landlords.

Chapter 2

In the Red: Overdrafts, Payday Lending and the Underbanked[1]

2.1 Introduction

According to the FDIC, at least twenty-five percent of U.S. households are unbanked or underbanked (FDIC, 2017). Individuals in these households either do not have a bank account or have a bank account, but also routinely use alternative financial services outside of the traditional banking system such as payday loans. The issue of financial inclusion has caught the attention of policymakers.[2] However, although low-income individuals obtain financial services from both traditional and alternative financial institutions, the bulk of financial inclusion research and regulation in the U.S. has focused on the costly and allegedly predatory nature of the alternative system.[3]

The commonly proposed solution to the costliness of alternative financial services is to

[1]Co-authored with Marco Di Maggio and Emily Williams

[2]As Federal Reserve Chariman Jerome Powell stated in 2019, "Access to safe and affordable financial services is vital, especially among families with limited wealth — whether they are looking to invest in education, start a business, or simply manage the ups and downs of life."

[3]For example, Bertrand and Morse (2011) examines the costs associated with using alternative financial services. State and federal regulators have also expanded their supervision of the payday lending industry in particular.

bank the unbanked and underbanked populations.[4] A significant barrier to this approach, however, is that low-income consumers also find traditional banking services to be costly. Indeed, a primary reason underbanked households cite for not having or for not exclusively using a bank account is the fact that bank account fees are too high (FDIC, 2017). It has been estimated that low-income individuals pay at least three times as much as the rest of the population to simply maintain their checking accounts.[5]

Overdraft fees, which accounted for a noteworthy $33 billion of bank revenue in 2018, constitute the bulk of not only all deposit account fees, but particularly deposit account fees incurred by *low-income* consumers.[6] An overdraft can be thought of as a short-term, high-cost loan linked to a checking account: banks cover the difference when customers attempt to spend more than their account balance, and banks charge overdraft fees for providing this short-term liquidity.

In this paper we investigate whether banks' overdraft practices cause customers to migrate from the traditional to the alternative financial system. We focus on a controversial practice, high-to-low reordering (HTLR), thought to maximize overdraft fees earned from low-income consumers. For consumers with low account balances, HTLR can generate significantly more overdraft fees than real-time chronological processing of transactions. For example, as illustrated in Figure 2.1, a HTLR bank would process a $500 rent debit against a $400 checking account balance before two smaller transactions of $50 each, even if the latter transactions were posted earlier. HTLR would thus cause the individual to incur three overdraft fees compared to just one under chronological ordering. Given a standard overdraft fee of $35, HTLR imposes a fee burden of 53% of the original overdrawn balance compared to 17.5% under chronological ordering. HTLR is widespread: as of 2016, about

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[4]For example, the FDIC states, "Ownership of an account at a federally insured depository institution provides households with a safe place to keep deposits and to save ... and it facilitates households' financial transactions ... despite these benefits, millions of U.S. households continue to use services from high cost alternative financial services providers."

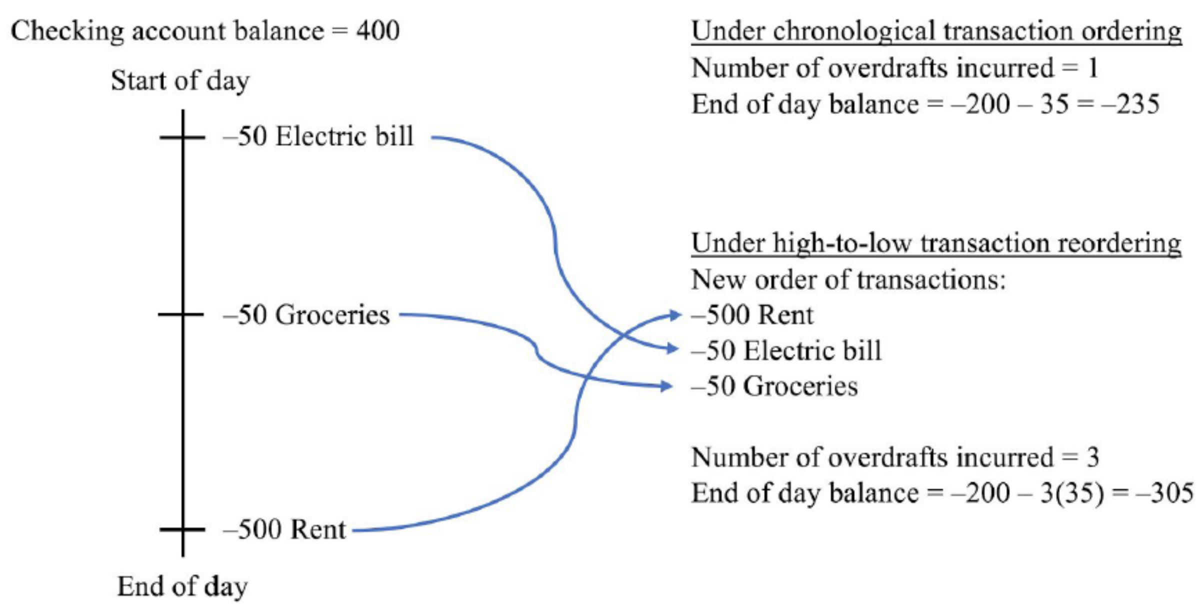
[5]This cost differential is documented by a 2017 Bankrate report available at https://www.bankrate.com/pdfs/pr/20171023-Best-Banks.pdf. Low-income is defined as earning less than $30,000 per year.

[6]See Moebs Services for detailed and frequently updated research on bank overdraft.

half of the 50 largest banks engaged in the practice.[7]

Figure 2.1: Illustrative example of high-to-low transaction reordering

This figure illustrates the mechanics of high-to-low transaction reordering for a consumer who begins the month with $400 in her checking account. Early in the day, her electric bill is deducted via automatic payment. During the day, she buys groceries. At the end of the day, her landlord deposits her rent check. Her bank charges a $35 fee per overdraft. Under chronological transaction ordering, she would only incur 1 overdraft for her rent payment. Under high-to-low transaction reordering, she incurs overdrafts for every transaction.



A negative deposit account balance (made more negative by overdraft fees) cannot be rolled over, and failure to promptly repay overdrafts and overdraft fees can have severe consequences. For example, failing to repay one bank can prevent an individual from opening an account at *any other bank* for up to five years.[8] As a result, consumers with

[7]See Pew Charitable Trusts (2016) for details on the study conducted by the Pew Charitable Trusts.

[8]ChexSystems, the primary consumer reporting agency used by banks, records involuntary bank account closures. Failure to pay overdraft fees and balances within two months can result in involuntary account closure, which can prevent a consumer from opening an account at any bank for up to five years. Without a checking account, it becomes difficult to obtain credit or access basic financial services like check writing and direct deposit. See Campbell *et al.* (2012) for an empirical analysis of involuntary account closures.

hefty accumulated overdrafts may find it necessary to borrow elsewhere, for example in payday loan markets, to bring account balances back above zero. This symbiotic relationship between payday lenders and banks is consistent with anecdotal evidence of consumers viewing payday loans as a way to bring overdrawn balances out of the red.[9] Altogether, these circumstances reflect a previously unstudied link between overdrafts and payday loan markets wherein the former can create demand for the latter.

Theoretically, the net effect of HTLR on consumer welfare is ambiguous. On one hand, if consumers understand the cost of high-to-low-reordered overdrafts ex-ante, they can optimally use the product as a source of liquidity in times of distress.[10] Overdraft borrowing may optimally lead to payday or other alternative borrowing if the consumer cannot immediately repay the overdraft fees. By contrast, if consumers improperly estimate the true cost of high-to-low-reordered overdrafts, they will over-borrow and incur unexpected overdraft fees. In this case, consumers turn to payday lenders and other alternative lenders to repay unaffordable, excessive bank overdraft balances and fees.

Assessing the impact of HTLR on consumers first requires overcoming several challenges. The first challenge is related to data availability. Underbanked consumers, lying at the intersection of the traditional and alternative financial systems, are not fully represented in the traditional credit bureau data that is routinely used in household finance studies. A second challenge is that bank policies and behaviors are difficult to observe. Banks are not incentivized to be transparent about their procedures, and few organizations are incentivized to track bank behavior over time. A third challenge is that bank policies and behaviors are endogenous to the customer base. Instead of being randomly assigned, bank policies and behaviors are optimally driven by the type of depositors a bank attracts and likely correlated with local economic variables.

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[9] See Center for Responsible Lending (2010) for anecdotal evidence on the use of payday loans to repay overdrafts.

[10] Banks have argued that HTLR benefits customers because it ensures that large, important payments, such as rent, mortgages, and student loans, are made first.

We address these challenges in the following ways. First, we obtain data from the alternative credit bureau Clarity Services. The data covers a random sample of individuals with non-traditional credit histories, such as customers of payday lenders and title lenders. The data contains a set of variables similar to those tracked by traditional bureaus.[11] We complement the Clarity data with traditional credit bureau data from Equifax, one of the main consumer credit bureaus. The Equifax data enables us to hone in on roughly the same population by focusing on installment loans made to borrowers in the dataset's lowest quintile of the income distribution.[12]

Second, we detect HTLR in action by exploiting a series of class action lawsuits that challenged the practice at banks across the United States. We hand-collect this lawsuit dataset, documenting the defendant bank, lawsuit outcome (including whether an end to HTLR was mandated), and geographic areas affected. The lawsuits provide the key source of variation in HTLR behavior over time, within zip codes, across zip codes, and across banks. This data strategy enables us to address the following key question: "Does the 'aggressive' pricing of bank overdraft via HTLR contribute to alternative system borrowing and overall credit health deterioration for low-income consumers?"

Our empirical analysis proceeds in three steps. We begin by providing motivating evidence on the co-location of alternative finance providers and banks with aggressive overdraft policies and on the binding nature of high-to-low reordering bans. Specifically, we show that within the same zip code, branches of HTLR banks are more likely than branches of non-HTLR banks to be located in close proximity to payday lenders. The implication is that alternative finance providers and banks with aggressive overdraft policies co-locate and service similar customers. We then confirm that HTLR bans led to meaningful changes in overdraft policies, as illustrated by declines in proxies for overdraft revenue and balances.[13]

[11] See Nuñez *et al.* (2016) for an in-depth exploration of the Clarity subprime lending data.

[12] Installment loans are known to be an alternative to payday loans for individuals with poor credit. For anecdotal evidence see https://www.nerdwallet.com/best/loans/personal-loans/installment-loans-bad-credit.

[13] We further show that no other source of bank revenue is affected, which suggests that the lawsuit-mandated behavior changes are not capturing an overall shock to these banks.

In the second step of analysis, we examine the response of consumer borrowing, financial health, and consumption to the HTLR ban. Our empirical strategy centers on comparing zip codes with branches of banks required to cease HTLR v.s. neighboring zip codes with branches of banks that were sued but *not* required to cease the practice.[14] The choice of this control group ensures that we are comparing areas with similar economic conditions and consumer demand dynamics. It also enables us to control for the effects of the lawsuit, independent of the final outcome. In our most conservative specifications, we include neighborhood by quarter fixed effects to capture any time-varying heterogeneity between these granularly defined areas.

We document that consumer borrowing from alternative lenders declines significantly. Specifically, payday borrowing declines by a statistically significant $85 per borrower per quarter, which translates to an economically meaningful decline of 11 percent relative to its mean. Installment loan borrowing similarly declines by $358 per borrower per quarter, an 8 percent decline relative to its mean. The effects persist for several years after the change in bank overdraft policies, indicating a permanent decline in borrowing from alternative lenders after HTLR bans. The simultaneous reduction in overdraft usage and alternative borrowing is consistent with the hypothesis that bank overdraft policies can cause borrowing in alternative credit markets.

We further find that consumers experience improved financial health as proxied by several measures. Following HTLR bans, affected consumers are better able to service existing debt and are more likely to experience improved credit scores and higher credit card limits, suggesting that traditional lenders expand credit access for consumers whose financial standing improves. Collectively, these findings indicate that consumers experience improved access to more mainstream, likely cheaper credit in the wake of HTLR bans. In line with this, we additionally find that affected households significantly increase consumption

[14]Our goal is to estimate the effect of the HTLR ban. Although we acknowledge that the lawsuits may also induce overdraft users to learn and become informed about the true cost of overdrafts, this learning effect would apply to the consumers of all sued banks independent of whether they were required to cease HTLR. If it is the same for both groups of consumers, the learning effect does not detract from our ability to estimate the effect of the HTLR ban.

of auto and home-related durable goods and of essential non-durable goods.

In the final step of analysis, we investigate spillover effects of such bans. We hypothesize that HTLR bans may have the unintentional consequence of changing the way banks interact with low-income consumers.[15] Because overdraft fees are an important source of revenue for banks, forcing them to cease HTLR could lead banks to close branches and exit low-income areas altogether. Indeed, we find that banks are significantly more likely to close branches after HTLR bans and that the effect is concentrated in low-income zip codes where banks have a small number of branches to begin with. Finally, we investigate the impact of branch closures on other segments of the credit market, such as mortgage and small business lending.

Our findings collectively point to a previously unstudied link between the traditional overdraft market and the alternative payday loan market. Our results suggest that overdrafts induce cash-strapped, low-income consumers who may not fully understand the true costs associated with using overdrafts, to seek loans from alternative finance providers in order to bring their bank balances out of the red. Although policymakers today are focused on ensuring that poorer areas are served by traditional financial institutions, our results caution that bank practices may be harmful to precisely the consumers in these areas and may be the original reason they turned to alternative credit.

The rest of the paper is organized as follows. Section 2.2 discusses related literature. Section 2.3 provides background on bank overdraft and lawsuits lodged against banks for HTLR. Section 2.4 describes the data. Section 2.5 presents motivating evidence. Section 2.6 addresses consumer responses to HTLR bans, while Section 2.7 addresses bank responses. Section 2.8 reports robustness test results. Finally Section 2.9 concludes.

[15]For example, Dlugosz *et al.* (2020) find that when national banks become exempt from state-imposed overdraft fee limits, they re-optimize, raising both their overdraft price and quantity of overdraft supplied.

2.2 Related Literature

Our findings contribute to a broad literature and active debate on the costs and benefits of consumer access to short-term, high-cost credit, which to date focus largely on payday loans. On one hand, there is evidence that access to payday loans improves consumer welfare. For example, Morse (2011) finds that payday loan access enables consumers to avoid foreclosure following natural disasters, while Zinman (2010) finds that restricting payday loan access causes consumers to shift to more expensive substitutes and experience deteriorated financial health. On the other hand, there is evidence that access to payday loans reduces consumer welfare. Melzer (2011) finds that payday loan access leads low-income consumers to experience difficulty paying bills and to delay necessary medical care. Skiba and Tobacman (2019) find that payday loan access increases personal bankruptcy rates by a factor of two. Bertrand and Morse (2011) find that disclosure about payday loans affects take-up, indicating that borrowers may not fully understand the cost of obtaining the short-term, high-interest loan. While there is ample evidence that payday borrowers are also likely to be frequent overdrafters (e.g. Zinman (2010), Morgan *et al.* (2012), and Melzer and Morgan (2015)), policy and academic research have focused on payday loans more than bank overdrafts. We provide direct evidence of how bank overdrafts in particular affect low-income consumers.

This paper also contributes to the academic literature and collection of anecdotal evidence on how consumers interact with short-term, high-cost credit markets. Melzer and Morgan (2015) and Morgan *et al.* (2012) show that overdraft providers and payday lenders compete and that consumers use overdrafts and payday loans as substitutes. According to Cirillo (2004), 66% of surveyed payday borrowers cite "avoiding bounced checks" as a benefit of payday loans, implying that borrowers consciously compare and substitute between borrowing from a payday lender and overdrawing at a bank. In this paper, our finding that bank overdraft usage contributes to demand for payday loans and other alternative credit implies that overdrafts and payday loans also have a complementary relationship. This connection between the traditional and alternative financial systems suggests that neither system exists in isolation and that both are relevant to financial inclusion policies that aim to ensure affordable basic financial services.

This paper also relates to the large literature on consumer liquidity constraints. Deaton (1991) introduces the standard framework for impatient consumers with uncertain income and liquidity constraints, while Hayashi (1985), Hayashi (1987), Zeldes (1989), Jappelli (1990) and Gross and Souleles (2002) provide direct and indirect empirical evidence of liquidity constraints. A follow-up literature beginning with Bacchetta and Gerlach (1997) shows that if some consumers are liquidity constrained, aggregate consumption will be excessively sensitive to credit conditions as well as to income. We contribute to this literature by demonstrating that a reduction in debt service costs (related to overdrafts and payday loans in our setting) causes consumers with likely binding liquidity constraints and low cash on hand to increase consumption and experience improvements in credit health and traditional credit access.[16]

Finally, this paper connects to the small literature on debt traps. As noted in Morgan *et al.* (2012), the debt trap concept is close to the poverty trap model in Sachs (1983) that illustrates how a nation may become trapped in poverty if its debt burden becomes too great: debt servicing slows capital accumulation, which slows income growth and reduces saving. Reduced saving feeds back to reduce capital accumulation further, leading to a downward spiral. A reduction in borrowing costs in this scenario can reverse the spiral. Our evidence that a reduction in overdraft costs improves consumer credit health and traditional credit access indicates that overdraft policies can lead to excessive short-term, high-cost credit accumulation. Our findings are consistent with ample anecdotal evidence (e.g. as provided

[16]According to the standard framework in Deaton (1991), liquidity constraints would heighten the precautionary savings motive, which is at odds with the empirical fact that 60% of Americans cannot come up with $1,000 to cover an emergency (CNBC, 2019). Dynan (1993) and Guiso *et al.* (1992) also document that the precautionary savings effect is far smaller in reality than would be predicted by theory. Whereas Laibson *et al.* (1998) and Harris and Laibson (2001) show that hyperbolic discounting can explain the missing precautionary savings effect, we do not take a stand on the type of discounting at play or the type of non-optimal borrowing that results. Instead, we provide broader evidence that overdraft users who are likely liquidity constrained are also likely borrowing non-optimally in overdraft markets. We show that less aggressive overdraft pricing can relax liquidity constraints and lead consumers to substitute towards more traditional, less expensive forms of credit.

in Faris and Stegman (2003)) that the financial performance of the short-term, high-cost loan industry is enhanced by the conversion of occasional users into chronic borrowers.

2.3 Background

This section draws from several recent policy studies to highlight the key features of the traditional and alternative financial systems relevant to our analysis.

We begin by noting that overdraft programs are widespread and well-established in the banking industry. According to a 2009 FDIC report, most (approximately 75%) banks automatically enroll customers in automated overdraft programs. Regulation E, which took effect in 2010, required that customers opt in or affirmatively consent to overdraft services for ATM and point-of-sale debit transactions. Although successful in reducing overdraft fees for customers that did not opt in, Regulation E had limited overall effectiveness owing to the opacity of the opt-in process. Implementation of the opt-in requirement varied across institutions, consumers expressed confusion about whether and when they had opted in, and the Consumer Financial Protection Bureau (CFPB) eventually brought lawsuits against several banks for violations of the Regulation E opt-in requirement (CFPB, 2013, 2017).[17]

Overdraft programs work in the following way. A so-called overdraft occurs when a customer account lacks sufficient funds to cover an attempted transaction. The host bank can either cover the transaction and charge an overdraft fee or decline the transaction and charge a non-sufficient funds (NSF) fee. In 2015, consumer overdraft and NSF fees accounted for almost two-thirds of all reported bank deposit account fee revenue (Stein, 2016). Overdrawn accounts can lead to a cascade of fees and eventually loss of access to traditional financial services according to the following timeline. Most banks will charge an additional daily fee on overdrafts not paid after one week. After roughly two months of maintaining a persistently negative account balance, a consumer will face involuntary account closure,

[17]For example, on January 19, 2017 the CFPB sued TCF National Bank in the United States District Court of Minnesota for devising a strategy to persuade customers to opt-in to overdraft services. A 2017 CFPB White Paper on Overdrafts also showed high rates of opt-ins from persistent overdrafters.

charge-off of unpaid balances, and blacklisting in ChexSystems, a centralized system used to verify their good standing with other banking institutions before allowing consumers to open a new bank account. Black-listing in ChexSystems is a severe consequence that makes it difficult, if not impossible, to access even the most basic traditional financial services. For context, 6% of all accounts opened in 2011 experienced involuntary closure by year end (CFPB, 2017).

The burden of these fees is not equally distributed, falling heavily on low-income consumers. CFPB data collected between June 2011 and June 2012 from a representative random sample of checking accounts at several large banks revealed that approximately 9% of all accounts incur more than 10 overdrafts in a 12-month period. This relatively small fraction of all overdrafters account for 79% of all overdraft fees earned by the banks studied. A 2014 study by the Pew Charitable Trusts that examined the demographic characteristics of overdrafters found that younger, lower-income, non-white individuals and individuals who do not possess credit cards are among those most likely to pay overdraft fees. Pew further reports that 28 percent of people who paid an overdraft fee decided to close their checking accounts because of overdraft fees. Through interviews, the CFPB further documented that consumers are surprised by overdraft fees, uncertain about bank policies, and sometimes neglectful of automated payments that trigger overdrafts. One interviewed consumer explains, "If you overdraft, the risk is that you are going to end up with your whole entire deposit being eaten up by overdraft fees" (CFPB, 2017).

Additionally, customers tend to associate overdraft fees with payday loans, and overdrafters tend to be the focus of customer acquisition campaigns by payday lenders.[18] According to Rivlin (2010), the payday industry has grown considerably in recent times because "when the cost of a payday loan is lower than the rising costs of a bounced check or credit card late fee, customers find it optimal to use alternative lenders to cover their monthly shortfalls." Consistent with this, UStatesLoans.org, a commonly used resource

[18]See for example Pew Charitable Trusts (2015) for further analysis of how overdraft frequency and payday borrowing correlate.

for prospective payday borrowers, clearly states as of 2020 that "it is a good idea to use payday loans to avoid overdrafts. Short-term loans provide fast money required to keep you on track. The loan fee is significantly lower than NSF fee and occurs just once in the loan duration, thus you always know what to expect. All this makes payday loan service much easier to use so you won't have to deal with overdrafts in the future."

Our paper investigates the relationship between bank overdraft policies and demand for alternative credit and estimates the impact of these policies on consumer financial health. To do so, we exploit a series of class action lawsuits against banks that engage in the high-to-low reordering of deposit account transactions. Details of these lawsuits can be found in Section 2.4.

2.4 Data

2.4.1 Data on the traditional financial system

A primary challenge in studying the interaction between the traditional and alternative financial systems is gathering data on each system. In the traditional financial system, we are rarely privy to the policies of banks over time, especially in the case of an arguably shrouded practice like HTLR. Bank policies are not highly publicized on a regular basis, and only the most updated policy can be gleaned from reading current bank account disclosures. In order to observe the overdraft policies of banks over time, we therefore bring in two data sources — one pre-existing, the other novel.

The first data source is a four-year study of large banks conducted by the Pew Charitable Trusts. Every year from 2012 to 2015, Pew identified the 50 largest banks by domestic deposits and obtained each bank's checking account disclosure whenever available. We use the data collected by Pew to create an indicator for whether a bank practices high-low transaction reordering at a given point in time. We combine this information with branch locations from the Federal Deposit Insurance Corporation (FDIC) Summary of Deposits data and with bank-level outcomes from the FR Y-9C quarterly bank reports. Panels A and B of Table 2.1 present summary statistics of our Pew-Infogroup-Summary of Deposits merged dataset. On average, 1.4 branches out of 4.7 total branches in a zip code employ HTLR. This prevalence likely reflects the fact that several of the largest banks in our sample employed HTLR at some time, and large banks operate branches throughout the United States. Panel A also shows that there is on average one check casher and one payday lender in a zip code. 40% percent of zip codes have at least one check casher and payday lender, which is consistent with the fact that these alternative finance establishments are not uniformly distributed and instead concentrate in particular areas with higher expected demand for their services. Panel B compares the presence of check cashers and payday lenders around branches that practice HTLR v.s. those that do not. The fact that there are more check cashers and payday lenders around branches of HTLR banks suggests that alternative finance institutions and aggressive banks may compete for the same customers.

The second data source is our hand-collected set of lawsuits lodged against banks for engaging in HTLR. In recent years, in an effort to force banks to refrain from aggressive overdraft practices, retail customers have sued financial institutions, arguing that aggressive overdraft practices disproportionately affect low-income clients. To build our lawsuits dataset, we identified relevant legal cases by querying Nexis Uni for case documents containing "overdraft," "resequenc," "re-sequenc," "reorder," or "re-order," and read through the court docket and official documents to determine the outcome of each case. We focus on lawsuits that were settled in court and exclude those that were dismissed or settled via arbitration. Our final dataset includes 37 lawsuits, for which we collect key event dates and terms of settlement between each bank and its customers. See Table B.1 for an overview of our lawsuits dataset. We report the name of the sued bank, the date when the lawsuit was filed, the date when the judge granted final approval of the settlement, the date when the bank was required to cease HTLR (if at all), the date of cash settlement disbursement, and the amount of cash settlement in total and on a per-customer basis.[19][20]

Table 2.1: Summary statistics for the largest 50 banks

This table provides summary statistics for the banks examined by the Pew Charitable Trusts over the period of 2012–2015. In each year, Pew examined the largest 50 US. banks (ranked by deposits) and documented, among other things, whether each bank employed high-to-low reordering of deposit account transactions. Bank-level data comes from the FDIC Summary of Deposits, and establishment-level data comes from Data Axle, formerly known as Infogroup. Panel A provides zip code-level statistics, and Panel B provides branch-level statistics.

Panel A: Zip code-level statistics

	Mean	Std. dev.	Min.	Max.
Number of branches of high-to-low reordering banks	1.4	2.3	0	30
Number of branches of large banks	2.1	3.2	0	51
Number of branches	4.7	5.2	1	66
Number of banks	3.7	3.4	1	42
Deposits of branches of high-to-low reordering banks ($1000's)	198.2	3,293.6	0	351,000
Deposits of branches of large, high-to-low reordering banks ($1000's)	309.4	4,503.7	0	427,000
Deposits of branches ($1000's)	483.7	5,011.7	0	429,000
Number of check cashers	1.0	2.2	0	25
Number of payday lenders	1.1	2.4	0	58
Number of establishments	728.6	902.0	1	14,133
Fraction of zip codes with any payday lenders or check cashers	0.4	0.5	0	1

Panel B: Branch-level statistics

	Branches with high-to-low reordering	Branches without high-to-low reordering	All branches
Average number of check cashers within:			
0.25 miles	0.3	0.2	0.3
0.5 miles	0.6	0.5	0.6
1 mile	1.4	1.2	1.3
1.5 miles	2.5	2.2	2.4
2 miles	3.9	3.3	3.7
Average number of payday lenders within:			
0.25 miles	0.3	0.3	0.3
0.5 miles	0.7	0.7	0.7
1 mile	1.6	1.5	1.6
1.5 miles	2.8	2.6	2.8
2 miles	4.3	3.9	4.1

[19] We deal with mergers and acquisitions in the following way: the lawsuit ruling is applied at the bank holding company level to all subsidiaries including acquired ones that may also have been subject to lawsuits in the past.

[20] In the cases of Trustmark National Bank, Webster Bank, U.S. Bank, and PNC Bank, for which the exact behavioral relief date could not be found in legal documents or news articles, we use the settlement final approval date or the date of the earliest document that reports that the bank has recently stopped HTLR. Given that our analysis is at the quarterly level, using this procedure in these few cases should not affect our results.

Examining the different components of non-interest income for banks, Haubrich and Young (2019) find that, in the wake of the 2008 crisis, securitization income dried up while service charge income (primarily overdraft fees and non-sufficient funds fees) increased dramatically.[21] One explanation for this wave of overdraft-related lawsuits is that the housing crash destroyed an important source of revenue for banks, which reacted by extracting more fees from deposit accounts. Another explanation is that the low interest rate environment following the Great Financial Crisis left banks scrambling to find other sources of non-interest income. Indeed, the surge of lawsuits in our dataset begins in 2008, perhaps because consumers were responding to bank practices that maximized deposit fees to make up for other lost income. See Section 2.8.1 for evidence that the lawsuit outcomes serve as quasi-exogenous shocks to the HTLR practices of banks.

2.4.2 Data on the alternative financial system

Turning to the alternative financial system, data availability is also a challenge because the alternative financial system is not centrally organized or regulated. While the Dodd-Frank Wall Street Reform and Consumer Protection Act endowed the CFPB with the ability to regulate payday lenders, there remains state-level variation in payday lending prohibition and rules. Furthermore, there is no regulator in charge of jointly evaluating the different components of the alternative financial system, which includes not only payday lenders but also check-cashers, issuers of prepaid debit cards, and more. We overcome the data availability challenge in the alternative financial system by exploiting several data sources.

Before describing the credit bureau data sets, we address the relationship between payday loans and installment loans, which we will observe in the alternative and traditional credit bureau datasets respectively. There is a spectrum of consumer financial products that ranges from alternative to traditional. On one end, payday loans come from the alternative financial system. On the other end, credit cards come from the traditional financial system. Installment loans are a product that lie at the nexus of the traditional and alternative systems.

[21] Figure 3 in Haubrich and Young (2019) documents the breakdown of non-interest income through time.

There are several useful regulatory reports that highlight this fact, and we review their key takeaways and anecdotal evidence on the similarity between payday and installment loans here.[22] First, many lenders that offer payday loans also offer installment loans. For example, Advance America is one of the largest payday lenders in the U.S. and also offers installment loans. Moreover, while Advance America does not report payday lending to credit bureaus, it does report installment lending to credit bureaus.[23] Payday loans and high-cost installment loans also operate similarly with the latter commonly referred to as "payday installment loans." These loans are repaid in multiple installments, where each installment is typically due on the consumer's payday and the lender generally has the ability to collect payment from the consumer's bank account or directly from the consumer's paycheck. Furthermore, payday loans and high-cost installment loans are not subject to ability-to-repay regulations, which makes it likely that these loans are accessed by the same borrower base.

Our alternative credit data source is Experian's proprietary alternative finance credit bureau Clarity Services. Launched in 2008, Clarity is now the largest alternative credit bureau overseen by the Fair Credit Reporting Act (FCRA). Clarity gathers data from alternative financial service providers, such as payday lenders, with a particular emphasis on non-prime and under-banked borrowers. The purpose of Clarity is to provide lenders with information about prospective borrowers, such as payday borrowing history, not tracked by a traditional credit bureau. Our Clarity dataset includes an inquiries file and a tradelines file. Inquiries are requests made by prospective borrowers to prospective lenders. We observe inquiries from 2012 to 2019 with details on prospective loan type and borrower characteristics. Tradelines are actual extended loans. We observe tradelines from 2013 to 2019 with details on loan amount, loan type, and repayment behavior. In the inquiries and tradelines dataset, the most granular information we have about borrower location is zip

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[22]See for example the June 2016 CFPB report "Supplemental findings on payday, payday installment, and vehicle title loans, and deposit advance products" and the 2016 Office of the Comptroller of the Currency handbook "Installment Lending."

[23]See https://www.advanceamerica.net/loans for details on product offerings.

code. Panels A and B of Table 2.2 present summary statistics of the Clarity data used in this study. We draw a random sample of 171,445 borrowers and observe the number of inquiries made by these borrowers as well as the number of tradelines and their characteristics (e.g., whether the loan has been repaid or charged off). We provide separate statistics for single payment micro loans (SPML), which are how payday loans are denoted in the dataset.

We complement this data with information for a representative sample of borrowers present in Equifax, one of the main consumer credit bureaus. Specifically, we observe a representative, ten percent sample of the population of traditional borrowers (680,856 borrowers) over the period 2005–2018. Equifax paints a full traditional financial picture of a borrower by tracking the monthly performance of all of the borrower's traditional loans (e.g. mortgages, credit cards, student loans, auto loans, and installment loans). The data includes borrower characteristics, such as annual income and age, and loan characteristics, such as account type, date opened, date due, account type, outstanding balance, and credit limit. As explained earlier, an advantage of this dataset is that it enables us to observe high-cost installment borrowing by low-income borrowers, which is a similar phenomenon to payday borrowing. Panels C and D of Table 2.2 report statistics for these loans, in particular for borrowers in the lowest income quintile.

Finally, we use the Infogroup Historical Business database, which consolidates business names, locations, and other details from public sources like the Yellow Pages. The data is available from 1997 to 2018. As in Bord (2020), we systematically identify check cashers, payday lenders, and pawn shops in Infogroup. A business is identified as a check casher if it has 6-digit SIC code 609903 or its name contains both "Check" and "Cash." A business is identified as a payday lender if it has 6-digit SIC code 614113 or its name contains "Cash" but not "Check" or "Gold." A business is identified as a pawnshop if it has 6-digit SIC code 593229.

Table 2.2: Summary statistics for Clarity alternative credit bureau and Equifax traditional credit bureau data

This table provides zip code x quarter level summary statistics for the Clarity alternative credit bureau dataset and the Equifax traditional credit bureau dataset. The Clarity alternative credit bureau dataset tracks the alternative credit usage of a random, representative sample of 171,445 alternative borrowers over the period 2013–2019. These alternative borrowers reside across 33,690 zip codes. The Equifax traditional credit bureau dataset tracks the traditional credit usage of a representative, ten percent sample of the population of traditional borrowers (680,856 borrowers) over the period 2005–2018. These borrowers reside across 34,874 zip codes. We provide summary statistics for all zip codes and for zip codes with below-median income (which is the sample for our tests).

Panel A: Clarity data				
Number of borrowers	171,445			
Number of zip codes	33,690			
Zip code x quarter-level observations				
Sample: All zip codes				
	Obs.	Median	Mean	Std dev.
Number of loans disbursed per borrower	179,740	1.0	1.5	0.9
Dollars of loans disbursed per borrower	179,740	1,437.5	2,772.5	3,536.0
Payday borrowers only:				
Number of payday loans disbursed per payday borrower	78,564	1.5	2.0	1.4
Dollars of payday loans disbursed per payday borrower	78,564	532.5	743.9	756.1
Sample: Zip codes with below-median income				
	Obs.	Median	Mean	Std dev.
Number of loans disbursed per borrower	53,225	1.0	1.6	1.0
Dollars of loans disbursed per borrower	53,225	1,075.0	2,257.0	3,089.4
Payday borrowers only:				
Number of payday loans disbursed per payday borrower	27,066	1.8	2.1	1.4
Dollars of payday loans disbursed per payday borrower	27,066	516.7	738.2	723.0
Panel B: Equifax data				
Number of borrowers	680,856			
Number of zip codes	34,874			
Zip code x quarter-level observations				
Sample: All zip codes				
	Obs.	Median	Mean	Std dev.
Lowest-income-quintile installment borrowers only:				
Number of installment loans disbursed per borrower	461,500	1.5	1.5	0.6
Dollars of installment loans disbursed per borrower	461,500	3,658.0	5,035.8	5,081.0
Average estimated annual income of borrowers ($000)	461,500	16.0	16.2	1.6
Average credit card balance per borrower	963,680	75.7	350.9	921.4
Average credit card limit per borrower	963,680	176.5	618.0	1,598.0
Average total balance in good standing per borrower	963,680	11,710.0	14,135.8	14,626.0
Sample: Zip codes with below-median income				
Lowest-income-quintile installment borrowers only:				
Number of loans disbursed per borrower	180,142	1.5	1.6	0.7
Dollars of loans disbursed per borrower	180,142	3,500.0	4,734.9	4,646.4
Average estimated annual income of borrowers ($000)	180,142	16.0	16.0	1.6
Average credit card balance per borrower	368,067	24.0	300.3	808.2
Average credit card limit per borrower	368,067	63.5	483.8	1,353.0
Average total balance in good standing per borrower	368,067	10,546.0	12,721.8	11,671.4

2.4.3 Other data sources

We use the five-year American Community Survey conducted by the Census Bureau to obtain zip code-level characteristics (on age, race, education, household type, poverty, income, public assistance, employment, and housing) on an annual basis from 2011 to 2018. To investigate the effects of lower overdraft fees on depositors' financial health, we obtain weekly zip code-level expenditure data at the household level from Earnest Research, a company that collects credit card and debit card transaction-level data for a representative sample of 6 million U.S. households. We use this data to construct measures of consumption and test whether depositors' expenditures are altered as a result of bank HTLR behavior changes.

Table 2.3 reports branch summary statistics of treatment and control zip codes, treated zip codes being those that contain branches of sued banks required to make behavior changes and control zip codes being those within seven miles of treated zip codes that contain branches of sued banks not required to make behavior changes. We show the number of branches in treatment and control zip codes in each treatment year identified by the lawsuits data and document the number of branches belonging to sued banks in each treatment and control zip code. Table 2.3 shows that sued banks on average constitute a large portion of the total branches within a zip code.

By connecting the described datasets, we are able to examine a relationship between the traditional and alternative financial systems in the U.S. at a relatively granular (zip code) level along multiple dimensions.

Table 2.3: Summary statistics for treated and control zip codes

This table provides summary statistics for treated and control zip codes. At the zip code level, we flag a zip code as treated when any of its bank branches belong to a bank that undergoes a high-to-low reordering ban. We flag a zip code as a control zip code when it is not a treated zip code and any of its bank branches belong to a bank that was sued but not high-to-low-reordering-banned. Panel A reports the number of high-to-low reordering-banned bank branches and the total number of bank branches in treated zip codes and in control zip codes in each of the years that high-to-low reordering bans happen. Panel B reports the total population, median income, unemployment rate, and population percentage below the poverty line for treated zip codes and control zip codes. Note: Because the Census zip code-level data begins only in 2011, we link 2010 bank branch data to 2011 Census demographic data.

Panel A: Branch statistics by zip code				
	Treated zip codes		Control zip codes	
Year	Number of high-to-low reordering branches	Total number of branches	Number of high-to-low reordering branches	Total number of branches
2010	1.1	8.1	1.1	9.0
2011	1.3	9.4	1.2	9.5
2013	1.3	9.4	1.3	7.7
2014	2.1	10.1	1.9	9.3

Panel B: Census statistics by zip code				
	Total population	Median income	Unemployment rate	Percent below poverty line
Treated zip codes	23,979	58,984	9.0%	13.8%
Control zip codes	28,261	62,699	9.8%	15.4%

2.5 Motivating Facts

2.5.1 Co-Location of Alternative Finance Providers and High-to-Low-Reordering Banks

We start by examining whether banks with aggressive overdraft policies and payday lenders cater to the same customers. If traditional banks that engage in HTLR tend to serve households with different characteristics than consumers served by payday lenders, changes in overdraft practices may not affect customer demand for alternative financial services.

Table 2.4 tests whether banks, in particular those that employ HTLR, are likely to cater to customers of alternative financial institutions. Since most individuals tend to favor financial institutions that are physically closer to their home or workplace, if banks and alternative lenders compete for the same customers, one can expect them to have physical locations relatively close to each other. Table 2.4 explores this hypothesis in a granular way by estimating a within-zip code, conditional logit regression. The dependent variable takes a value of 1 if there is a payday lender or check casher within 0.25 miles, 0.5 miles, 1 mile, 1.5 miles, or 2 miles, and 0 otherwise. The independent variable is a dummy variable that takes a value of 1 if the branch within the zip code belongs to a bank with aggressive overdraft policies (HTLR procedure as identified by Pew), and 0 if the branch belongs to a bank that is among the 50 largest banks studied by Pew that does not have an aggressive overdraft policy. Comparing branch locations of banks among the largest 50 ensures that we are not comparing locations mainly served by regional banks or credit unions with locations where large banks operate. We find the coefficient of interest to be positive and highly significant and to monotonically decline as the distance from an aggressive branch increases. This within-zip code test provides evidence that banks that practice HTLR are more likely to have check cashers and payday lenders in close proximity.

Table 2.4: Co-location of alternative financial institutions and high-to-low reordering banks

This table presents the results of a conditional logit regression using bank branch x year level data. The outcome variable is a dummy variable that takes on a value of 1 if there is a payday lender or check casher within a certain distance of a bank branch. The independent variable is a dummy variable that takes on a value of 1 if the branch belongs to a bank that practices high-to-low transaction reordering. Zip code fixed effects are included. Zip code-level data on payday lenders and check cashers comes from Infogroup. Zip code-level data on bank branches comes from the FDIC Summary of Deposits. Bank-level data on overdraft policies comes from the Pew Charitable Trusts study of the largest 50 banks over the period 2012–2015.

	Indicator of check casher or payday lender within:				
	0.25 miles	0.5 miles	1 mile	1.5 miles	2 miles
Indicator of high-to-low reordering branch	0.140***	0.124***	0.0364**	0.0272	0.0139
	(0.0167)	(0.0164)	(0.0178)	(0.0209)	(0.0242)
Zip code fixed effects	Y	Y	Y	Y	Y
Observations	102,618	104,635	90,492	71,495	55,823

*** p<0.01, ** p<0.05, * p<0.1

This evidence supports the hypothesis that banks with aggressive overdraft policies and alternative financial services providers like payday lenders and check cashers are likely to service the same customers.[24]

2.5.2 The Impact of High-to-Low Reordering Bans on Overdraft Revenues and Balances

Although Table 2.4 shows a clear correlation between the presence of branches belonging to banks with aggressive overdraft policies and alternative finance providers, these results do not prove a causal link between bank policies and activity in alternative finance markets. This is because banks located in particular locations may endogenously tailor their products and pricing to local demographics. Put differently, HTLR may be a way for banks to fairly price overdrafts when serving specific customer types.

We use lawsuits against banks that employed HTLR to investigate a causal link between bank overdraft polices and migration to the alternative finance market. Some of these lawsuits resulted in mandatory behavior changes, for example, prohibiting banks' use of HTLR after a specific date.[25]

We further investigate the effects of lawsuit-mandated behavior changes on income from overdraft fees and bank-level measures of overdraft balances. Intuitively, this analysis serves as our first stage test of whether lawsuit-mandated behavior changes resulted in any meaningful decline in bank overdraft activity.

Panel A of Figure 2.2 plots quarterly coefficients of a difference-in-differences regression of growth in overdraft-related revenue (proxied by deposit account service charges) for HTLR-banned banks relative to other sued but not HTLR-banned banks before v.s. after the HTLR ban. Panel B does the same for the outcome variable of overdraft balances (proxied

[24] Prager (2014)'s investigation of the determinants of alternative financial service providers' choice of location emphasizes demographic characteristics and the legal and regulatory environment. Our finding that aggressive banks and alternative financial service providers co-locate complements this perspective and is consistent with the hypothesis that traditional bank policy affects customer demand for alternative financial services.

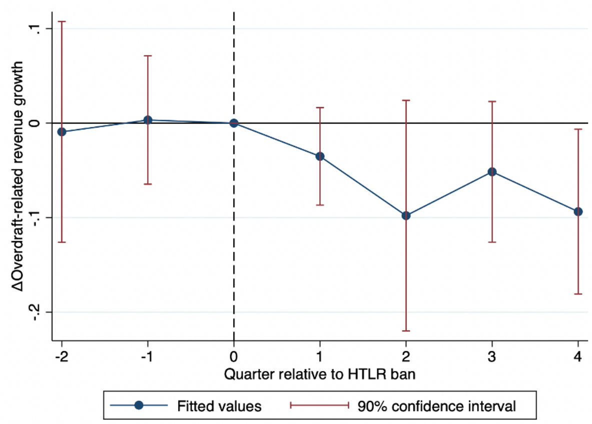
[25] Details of the lawsuits are recorded in Table B.1.

by the all other loans category). These findings are confirmed in Table B.2 in which we report the point estimates of the corresponding difference-in-differences specification.

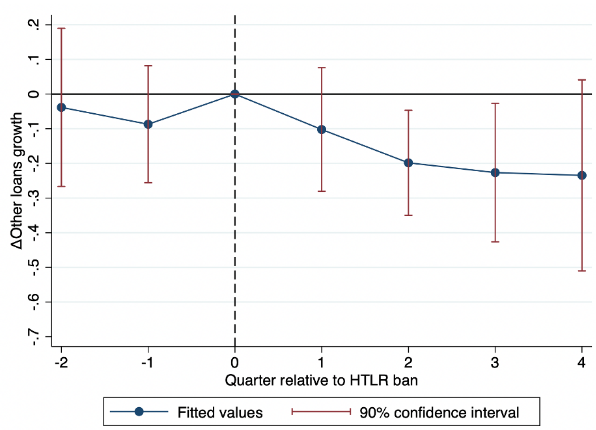
Figure 2.2: Bank overdraft activity in response to high-to-low reordering bans

This figure presents the results of our bank x quarter level difference-in-differences analysis using FFIEC Call Report data. In Panel A, the outcome variable is the growth in overdraft-related revenue (proxied by deposit account service charges). In Panel B, the outcome variable is the balance of overdrafts (proxied by all other loans). Coefficients are plotted from -2 to +4 quarters around the high-to-low reordering ban for a difference-in-differences regression of the outcome variable for banks with mandated high-to-low reordering bans relative to other sued banks that without mandated high-to-low reordering bans. Bank and quarter fixed effects are included. Standard errors are clustered at the bank level.

(a) Overdraft-related revenue growth



(b) Other loans growth



We proxy for overdraft balances using the log of the "All Other Loans" category in FFIEC 031 regulatory call report data for the following reasons. Measuring overdraft balances directly is not possible with call report data because it is not a required reported category. Therefore, we do the next best thing. Banks that provide overdraft services are required to report overdraft balances as a loan rather than as a negative deposit balance.[26] Specifically, *unplanned* overdraft balances are placed into the "all other loans" category. Throughout this paper, our focus has implicitly been on unplanned overdraft, which occurs when a consumer makes a withdrawal attempt on insufficient funds, and the withdrawal is honored without an advance contractual agreement to do so. By contrast, planned overdraft involves an agreement where the consumer is allowed to borrow a certain number of dollars of overdraft at low cost or no cost. Unplanned overdrafts are of particular concern to consumers and regulators because they tend to come as a surprise to low-income depositors and are significantly more expensive.

In Table B.2, we also consider the number of insured depositors as an outcome variable. On one hand, consumers may respond to the lawsuits by leaving affected banks after learning of the aggressive pricing practices. On the other hand, banks that are no longer able to extract excessive revenues from overdrafting customers may close deposit accounts. We observe no decline in the number of insured depositors, which indicates that it is unlikely that either of these effects is systematically affecting banks subject to HTLR bans.

If another factor unrelated to the lawsuit outcome is driving the change in overdraft revenue at sued banks, we should observe significant differences in other loan or income categories at banks subject to HTLR bans. For instance, changes in funding sources or investment opportunities at sued banks subject to HTLR bans are likely to result in broader changes in bank behavior and performance. We test this by running the same baseline specification as in Table B.2 but for other outcome variables that should not be affected by the HTLR ban, such as non-overdraft loan categories and income statement items like interest

[26] See the Instructions for Preparation of Consolidated Reports of Condition and Income (FFIEC 031 and 041) for full details on how overdrafts are accounted for in bank call reports.

income and expenses.[27] Table B.8 shows that growth in non-overdraft loans, specifically commercial & industrial loans and commercial real estate loans, are not significantly affected by HTLR bans. Table B.12 shows that interest income, non-interest income (excluding overdraft-related income), revenue (excluding overdraft-related income), and net income remain unchanged after the HTLR ban.[28]

Overall, these findings indicate that activity related to overdrafts declined significantly at banks required to cease the practice of HTLR.[29]

2.6 Consumer Responses to High-to-Low Reordering Bans

We now turn to our main analysis: assessing the impact of banning HTLR, which is an arguably aggressive bank policy, on consumer behavior. As described in Section 2.4, we are able to connect our datasets at the zip code level, so our analysis will proceed with zip code by quarter level observations. In order to interpret our results in an intuitive, consumer-level way, we compute outcome variable for the average/representative borrower in the zip code in the quarter.

In the Summary of Deposits data, the zip code is of the bank branch. In the Equifax traditional credit bureau data and the Clarity alternative credit bureau, the zip code is of the borrower's residence. In the Earnest consumption data, the zip code variable is of the business where the purchase was made. The implicit assumption behind comparing analyses across these datasets is that there is a link between the bank branch, consumer, and business locations such that individuals are more likely to bank at branches and frequent

[27]For these income statement variables, we subtract overdraft-related revenues when appropriate and report growth relative to pre-lawsuit levels.

[28]Unreported tests in which we investigate the impact of HTLR bans on other components of non-interest income reveal no significant changes, indicating that banks are not systematically trying to make up lost overdraft revenue in any other income category.

[29]Although a decline in overdraft related revenue does not directly translate to a decline in overdraft credit extended given that there is a flat fee per overdraft and not per dollar of overdraft credit, the decline in other consumer loans in combination with a decline in revenue associated with overdrafts is consistent with a decline in total overdraft credit extended; put differently, it is not consistent with an increase in overdraft borrowing.

businesses closer to their place of residence.[30]

2.6.1 Consumer Demand for Alternative Loans

We begin with Table 2.5, which documents the effect of HTLR bans on consumer demand for payday loans. If consumers burdened with hefty overdraft fees often turn to payday lenders to pay fees and balances and thereby avoid the severe consequences of defaulting, we would expect HTLR bans to be followed simultaneously by quantity declines in payday borrowing *and* reduced overdraft activity.

Table 2.5: Household demand for payday loans in response to high-to-low reordering bans

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data. The sample is restricted to zip codes with below-median income. The outcome variable is the dollar amount or the number of payday loans disbursed per payday borrower in a zip code in a quarter. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Varying levels of fixed effects are included across specifications. Standard errors are clustered at the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Payday loans disbursed per payday borrower					
	Dollar amount			Number		
HTLR Ban x Post	-45.35	-44.60	-84.84***	-0.222***	-0.210***	-0.289***
	(28.23)	(27.82)	(31.47)	(0.0775)	(0.0760)	(0.0905)
Neighborhood fixed effects	Y	Y	N	Y	Y	N
Quarter fixed effects	N	Y	N	N	Y	N
Neighborhood x quarter fixed effects	N	N	Y	N	N	Y
Observations	6,975	6,975	5,574	6,975	6,975	5,574
R-squared	0.311	0.317	0.408	0.319	0.334	0.384

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

[30] We know from the existing literature that proximity matters greatly for bank choice. For example, see Nguyen (2019) and Brevoort and Wolken (2009).

On the other hand, if overdraft services were simply fairly priced substitutes for payday loans, we would expect overdraft activity and payday borrowing to move in opposite directions. Specifically, if the price reduction resulted in a supply restriction (i.e. banks became less willing to offer overdraft services because of the cap on fees), then the excess unmet demand for short-term credit would result in an increase in demand for payday borrowing. Alternatively, if the price reduction resulted in only a price drop, consumers would substitute away from payday borrowing towards the now cheaper overdraft borrowing.[31]

To assess consumer alternative loan demand response to lawsuit induced bank behavior changes and the channel at play, we estimate the following zip code by quarter level specification:

$$PaydayBorrowing_{zt} = \beta \cdot HTLR\,Ban_z \cdot Post_t + \eta_{nt} + \varepsilon_{zt} \tag{2.1}$$

where $PaydayBorrowing_{zt}$ is the average dollars of payday loans disbursed per payday loan borrower residing in zip code z in quarter t, $Post_t$ is a dummy variable that takes a value of 1 for the four quarters following the HTLR ban and 0 for the four quarters prior to the ban, and $HTLR\,Ban_z$ is a dummy variable that takes a value of 1 if the zip code contains branches that belong to a sued bank mandated to cease HTLR and 0 if the zip code is within seven miles of a treated zip code and contains branches that belong to a sued bank not required to cease HTLR. We compare zip codes containing branches of HTLR-banned banks with zip codes within seven miles of a treated zip code that contain lawsuit banks subject to similar local dynamics but not to the HTLR ban. We choose this specification to further ensure that we are comparing local areas with similar types of consumers (i.e., those targeted by HTLR practices). Robustness checks in Table B.10 and Table B.11 demonstrate that choice of neighborhood radius and control zip codes do not affect our main results.

The coefficient of interest β measures the differential effect of the lawsuits in zip codes where banks had to stop reordering deposit account transactions from high to low, relative

[31]Note that the specific direction of overdraft and payday borrowing depends on the nature of competition in these markets, which we are unable to assess in this study.

to zip codes with sued banks with no such changes to overdraft practices. In other words, the variation we capture is restricted to regions in close proximity (i.e. within a seven miles radius), and where banks in both the treatment and control areas are subject to lawsuits.

To further control for heterogeneity across areas, such as changes in local economic conditions, we include neighborhood, quarter, and, in the most conservative specification, neighborhood by quarter fixed effects (η_{nt}), where again zip codes within seven miles of each other are defined as being in the same neighborhood. Intuitively, we are exploiting only variation within neighborhoods during the same quarter. This ensures that our results are not confounded by, for instance, a sudden unemployment shock correlated with HTLR bans that could drive both demand for payday loans and overdrafts. We also allow arbitrary correlation of standard errors within neighborhood and time by double-clustering at the neighborhood and quarter level. See Figure 2.3 for confirmation that the parallel trends assumption between treatment and control zip codes holds.

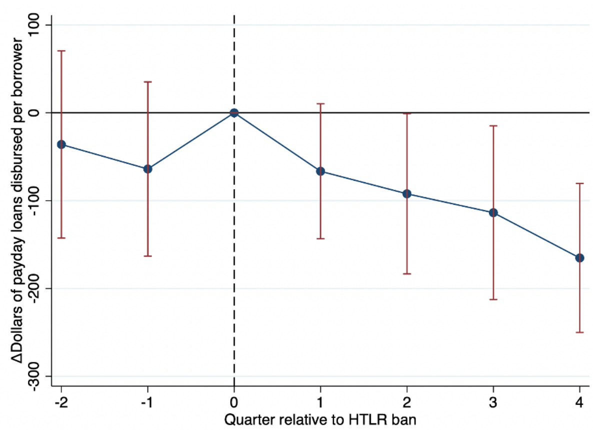
Table 2.5 presents the main result of this difference-in-differences specification using the Clarity data, which allows us to focus on single payment micro loans,[32] made to borrowers in zip codes below the median income in any given year. We find demand for high-cost loans to be concentrated in poorer zip codes, and argue that this could result from within-bank heterogeneity in overdraft policies that specifically target low-income consumers. Measuring credit demand from alternative lenders using both the dollars disbursed per borrower (Columns 1-3) and the number of loans per borrower (Columns 4-6) enables us to study both the intensive and extensive margins. We find a significant reduction in all outcome variables for the treated zip codes. Specifically, we find that following a HTLR ban, dollars disbursed decrease by $84.84 for the average borrower in a zipcode in a quarter, which translates to an 11 percent reduction relative to its mean.

[32]Payday loans are formally referred to as single payment micro loans (SPMLs).

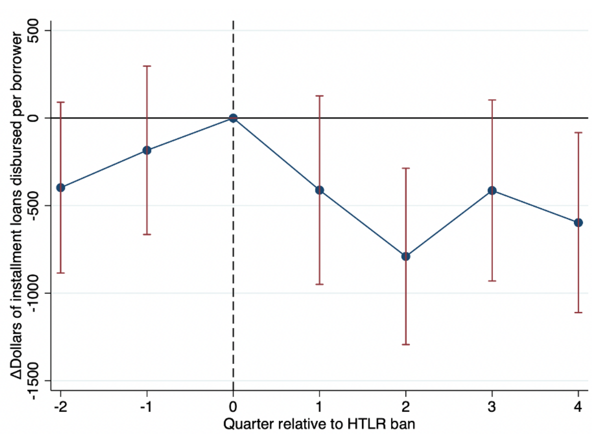
Figure 2.3: Household demand for payday loans and installment loans in response to high-to-low reordering bans

This figure presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In Panel A, the outcome variable is the dollar amount of payday loans disbursed per payday borrower in the Clarity dataset. In Panel B, the outcome variable is the dollar amount of installment loans disbursed per lowest-income-quintile installment borrower in the Equifax dataset. Coefficients are plotted from -2 to +4 quarters around the high-to-low reordering ban for a difference-in-differences regression of the outcome variable for zip codes that contain branches of a bank that was required to cease high-to-low reordering relative to zip codes that are within 7 miles and that contain branches of a bank that was sued but not required to cease high-to-low reordering. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

(a) Payday loans in Clarity data



(b) Installment loans to lowest-income-quintile borrowers in Equifax data



We next show that these effects are not confined to the payday loan segment, but also present when we consider other types of loans routinely used by individuals experiencing financial difficulties. Table 2.6 complements the previous analysis by documenting results from the same baseline differences-in-differences specification as above, but for installment loans extended to the lowest income quintile borrowers using Equifax data. The dependent variables are the dollar amount of loans disbursed and the number of loans disbursed. Similar to the findings on payday borrowing, we find a significant reduction in the amount of installment borrowing following HTLR bans. The effects are economically meaningful with a $358.3 reduction for the average borrower in a zipcode in a quarter, which corresponds to an approximately 8 percent reduction relative to its mean.[33]

Table 2.6: Household demand for installment loans in response to high-to-low reordering bans

This table presents the results of our zip code x quarter level difference-in-differences analysis using Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. The outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Varying levels of fixed effects are included across specifications. Standard errors are clustered at the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Installment loans disbursed per low-income installment borrower					
	Dollar amount			Number		
HTLR Ban x Post	-211.4**	-314.3***	-358.3***	-0.0147	-0.0329**	-0.0260
	(95.66)	(95.78)	(135.8)	(0.0149)	(0.0146)	(0.0204)
Neighborhood fixed effects	Y	Y	N	Y	Y	N
Quarter fixed effects	N	Y	N	N	Y	N
Neighborhood x quarter fixed effects	N	N	Y	N	N	Y
Observations	38,974	38,974	30,487	38,974	38,974	30,487
R-squared	0.094	0.118	0.278	0.110	0.144	0.294

*** p<0.01, ** p<0.05, * p<0.1

[33]That we find no results on the extensive margin suggests that borrowers are reducing overall loan size in this market.

To shed additional light on the alternative borrowing results, we examine variation in treatment intensity across zip codes. In Panel A of Table 2.7, we re-run our payday loan and installment borrowing tests for zip codes with relatively few branches of HTLR-banned banks. We expect to see muted effects of HTLR bans in areas where there are less affected consumers, i.e. where there are fewer branches engaged in HTLR that are subsequently required to cease the practice. Panel A shows that there is no statistically significant effect of HTLR bans on alternative borrowing in these zip codes. In Panel B of Table 2.7, we re-run our payday loan and installment borrowing tests for zip codes with low v.s. high treatment intensity. A zip code is classified as low (high) treatment intensity if it has a below-median (above-median) share of treated branches. We find that HTLR bans have a statistically significant, negative effect on alternative borrowing in high-treatment-intensity zip codes but no statistically significant effect on alternative borrowing in low-treatment-intensity zip codes. In sum, the investigation of zip code treatment intensity suggest that the main effects documented in Table 2.5 and Table 2.6 are concentrated in zip codes that have a higher share of bank branches experiencing HTLR bans and that are therefore more intensely treated.

Table 2.7: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Treatment intensity

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In Panel A, the sample is further restricted to zip codes with less than the median number of high-to-low-reordering-banned bank branches and greater than the median number of total bank branches. In the first two columns, the outcome variable is the dollar amount or the number of payday loans disbursed per payday borrower. In the last two columns, the outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. In Panel B, split the sample into zip codes with high treatment intensity (i.e. above-median share of treated branches) and low treatment intensity (below-median share of treated branches). HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but *not* required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

Panel A: Zip codes with few treated branches				
	Payday loans disbursed per payday borrower		Installment loans disbursed per low-income installment borrower	
	Dollar amount	Number	Dollar amount	Number
HTLR Ban x Post	30.77	0.0141	-90.01	-0.0303
	(46.51)	(0.104)	(210.3)	(0.0287)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	2,925	2,925	7,352	7,352
R-squared	0.384	0.435	0.120	0.188
Panel B: Zip code treatment intensity				
	Dollars of payday loans disbursed per payday borrower		Dollars of installment loans disbursed per low-income installment borrower	
Treatment intensity:	Low intensity	High intensity	Low intensity	High intensity
HTLR Ban x Post	-46.90	-125.9***	-311.6	-381.0**
	(45.03)	(41.50)	(197.8)	(180.7)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	3,762	1,812	19,397	11,090
R-squared	0.441	0.340	0.284	0.268

*** p<0.01, ** p<0.05, * p<0.1

We next assess whether there is heterogeneity in these findings resulting from differences in banking competition. Intuitively, if banks compete on overdraft prices, we would expect the effects of HTLR bans to vary with deposit market competition. We test this hypothesis by interacting the HTLR dummy variable in equation (1) with average zip code-level HHI. Results in Table B.13 for both payday and installment loans do not support the hypothesis that higher competition leads to lower cost of overdrafts and dampens demand for alternative borrowing. These findings are consistent with the shrouded attributes equilibrium in Gabaix and Laibson (2006), whereby firms hide information from customers and competition does not induce firms to reveal information and reduce add-on prices. Although overdrafts can effectively be used as short-term loans, banks do not explicitly advertise overdrafts as a credit product and anecdotal evidence suggests that depositors do not consider overdraft fees a key determinant of bank account choice.

Returning to our main results, the findings in Table 2.5 and Table 2.6 indicate that demand for loans from alternative lenders declines significantly in locations in which banks are forced to cease HTLR. In other words, our findings suggest that when banks are required to lower arguably aggressive and opaque overdraft prices, consumers borrow less in alternative financial markets.[34] These findings are consistent with the idea that overdrafts, in particular aggressively-priced overdrafts, can create demand for payday and installment loan borrowing. Put differently, the decline in alternative system borrowing and overdraft activity at HTLR banks documented in Table B.2 is consistent with the hypothesis that overdrafts and payday loans are likely complements, not merely substitutes for one another.

2.6.2 Long-Term Demand for Alternative Loans

Next, we test whether the results reflect a longer term, more permanent change in borrower behavior, rather than a short-term response to the HTLR ban. If bank overdraft practices are a key driver of demand for alternative financial products, we should expect a permanent

[34] See Section 2.8 for tests and a discussion that helps to rule out supply effects as a driver of the decline in payday and installment borrowing.

reduction in overdraft fees to result in a long-lasting decline in alternative credit market borrowing. Table 2.8 tests this hypothesis with the same difference-in-differences specification as in the previous analysis using the Clarity sample of payday borrowing and the Equifax sample of installment borrowing. The dependent variable is the dollars of payday loans per payday loan borrower residing in a given zip code in a given quarter or the dollars of installment loans disbursed per low-income installment borrower residing in a given zip code in a given quarter. Each column focuses on a different horizon — one, two, or three years — by varying the length of time following the HTLR ban that is included in the specification. The first three columns show that the reduction in the payday borrowing persists over time, although the point estimate suggests that the magnitude declines slightly from $84.84 in the first year to $50.47 in three years for the average borrower in a zip code in a quarter. The next three columns show a similar pattern for installment borrowing, with the magnitude declining from $358.3 in the first year to $276.5 in three years for the average borrower in a zip code in a quarter. Overall, these findings reinforce the hypothesis that aggressive overdraft policies can lead borrowers to persistently borrow from alternative lenders.

2.6.3 Consumer Financial Health

We next investigate whether the financial health of low-income consumers improves following HTLR bans. We argue that this improvement in financial health may occur through two channels.

First, as in Bertrand and Morse (2011), overdraft users, if not fully informed, may not fully understand the true costs of overdraft credit and may make sub-optimal decisions when taking on overdrafts. A reduction in overdraft pricing after HTLR bans may reduce debt service costs to more sustainable levels by reducing consumers' need to turn to high-cost payday and installment lenders to roll over the overdraft loan or by facilitating more informed borrowing choices. Consumers better able to service debt might be more likely to pay obligations on time, default less, and ultimately realize better credit scores.

Table 2.8: Household longer-term borrowing activity in response to high-to-low reordering bans

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In the first three columns, the outcome variable is the dollars of payday loans disbursed per payday borrower. In the last three columns, the outcome variable is the dollars of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2-quarter window before the high-to-low reordering ban and either a 4-quarter, 8-quarter, or 12-quarter window after the high-to-low reordering ban in order to examine both shorter-term and longer-term borrowing activity. Neighborhood x quarter fixed effects are included across specifications. Standard errors are clustered at the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Dollars of payday loans disbursed per payday borrower			Dollars of installment loans disbursed per low-income installment borrower		
Post-event horizon:	1 year	2 years	3 years	1 year	2 years	3 years
HTLR Ban x Post	-84.84***	-72.02***	-50.47**	-358.3***	-293.2**	-276.5**
	(24.04)	(19.62)	(17.58)	(135.8)	(125.4)	(120.2)
Neighborhood x quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	5,574	10,987	18,990	30,487	48,011	65,116
R-squared	0.408	0.442	0.455	0.278	0.255	0.258

*** p<0.01, ** p<0.05, * p<0.1

Second, if consumers turn to payday and other alternative lenders to repay overdraft fees and balances, then a HTLR ban should stem the flow of people into the alternative financial system. There is ample anecdotal evidence that payday loan users frequently become chronic borrowers[35] and become caught in "debt traps."[36] We argue that reducing the incentive to borrow from payday lenders could reduce the chances of entering the associated "debt traps", which could in turn have knock-on effects on ability to service other debt and on overall financial health.

[35] For example a 2014 study by the CFPB notes that 4 out of 5 payday loans are rolled over or renewed.

[36] The 2014 CFPB study also notes that 3 out of 5 payday loans are made to borrowers whose fee expenses exceed amount borrowed, indicating that the original payday loan spirals into ever increasing amounts owed.

Using Equifax data, we measure low-income consumers' financial health in terms of total borrowing in good standing, likelihood of experiencing an increase in credit score, and credit card balances and limits. Whereas Table 2.5 and Table 2.6 document that use of alternative loans respond relatively quickly to HTLR bans, we might expect consumer financial health to take longer to improve. Indeed this is what we find. Using the same empirical specification in the previous section, we report results from the following zip code by quarter level regression in Table 2.9 at 1 to 3 year horizons:

$$FinancialHealth_{zt} = \beta \cdot HTLR\,Ban_z \cdot Post_t + \eta_{nt} + \varepsilon_{zt} \tag{2.2}$$

We find a significant improvement in consumer financial health across these measures. For the average borrower in a zip code in a quarter, credit card balances increase significantly by \$33.96 in the first year, \$34.05 in the two years, and \$26.18 in the three years post HTLR ban, while credit card limits increase significantly by \$40.10 in the first year post HTLR ban and are unchanged over longer post-HTLR-ban horizons.

The effects on the credit health measures take more time to materialize. For the average borrower in a zip code in a quarter, the total balance in good standing increases by \$365.6 in the two years and \$546.0 in the three years post HTLR ban, which suggests that consumers are better able to service existing debt. We also find that borrowers are significantly more likely to experience an increase of 50+ points in their credit score by 0.0288 in the first year, 0.0293 in two years, and 0.0315 in three years after the HTLR ban.

Table 2.9: Household longer-term credit health in response to high-to-low reordering bans

This table presents the results of our zip code x quarter level difference-in-differences analysis using Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In Panel A, the outcome variables are credit card-related variables, specifically the credit card balance and credit card limit per low-income installment borrower. In Panel B, the outcome variables are credit health measures, specifically the likelihood of a 50-point increase in the Equifax VantageScore for low-income installment borrowers and the total credit balance in good standing per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a 2-quarter window before the high-to-low reordering ban and either a 4-quarter, 8-quarter, or 12-quarter window after the high-to-low reordering ban in order to examine both shorter-term and longer-term borrowing activity. Neighborhood x quarter fixed effects are included across specifications. Standard errors are clustered at the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

Panel A: Credit card-related variables						
	Credit card balance per low-income installment borrower			Credit card limit per low-income installment borrower		
Post-event horizon:	1 year	2 years	3 years	1 year	2 years	3 years
HTLR Ban x Post	33.96**	34.05**	26.18*	40.10**	26.01	12.26
	(13.94)	(15.30)	(15.62)	(20.10)	(22.01)	(22.75)
Neighborhood x quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	45,793	71,709	97,013	45,793	72,521	98,112
R-squared	0.271	0.287	0.303	0.265	0.290	0.316
Panel B: Credit health measures						
	Likelihood of 50+ point increase in credit score for low-income installment borrowers			Total balance in good standing per low-income installment borrower		
Post-event horizon:	1 year	2 years	3 years	1 year	2 years	3 years
HTLR Ban x Post	0.0288***	0.0293***	0.0315***	68.93	365.6*	546.0**
	(0.0111)	(0.0106)	(0.0106)	(211.7)	(220.5)	(237.9)
Neighborhood x quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	46,836	73,267	99,011	45,793	71,709	97,013
R-squared	0.260	0.258	0.258	0.257	0.264	0.270

*** p<0.01, ** p<0.05, * p<0.1

The increased credit card balances and limits represent a substitution away from costly alternative borrowing to cheaper mainstream credit. These findings further suggest that traditional institutions may perceive these borrowers, in light of increased credit card limits and hence credit availability, to be in better financial shape.

These results confirm borrower substitution away from expensive loan products towards more mainstream products and enhanced ability to keep finances in order following a reduction in aggressive overdraft fees.

2.6.4 Consumer Consumption

We further assess the impact of overdraft prices on consumers by examining the effect of HTLR bans on household consumption using zip code expenditure data from Earnest, which collects credit and debit card transaction-level data for a representative sample of U.S. households.

We estimate the following zip code by quarter level specification:

$$Consumption_{zt} = \beta \cdot HTLR\,Ban_z \cdot Post_t + \eta_{nt} + \varepsilon_{zt} \tag{2.3}$$

where our consumption outcome variables include household dollars of expenditure and number of items of expenditure for durable, non-durable essential, and non-durable other consumption. Durable refers to expenditures related to home and auto, e.g. car and roof repairs. Non-durable essential refers to expenditures related to food and clothing. Non-durable other includes all other non-durable expenditures.

Table 2.10 presents the results of this test. Focusing on within-neighborhood-quarter variation, we find that consumers increase durable and non-durable essential consumption by \$45.18 and \$15.57 respectively, while non-durable other consumption remains unchanged following the HTLR ban.[37] This finding is consistent with our hypothesis that low-income consumers likely experienced binding liquidity constraints prior to the HTLR ban and were more likely to consume only the necessities. By reducing their overdraft burden, improv-

[37] We discuss these magnitudes and how they relate to our other findings below.

ing their credit health, and relaxing their constraints, the HTLR ban ultimately afforded these consumers access to cheaper mainstream credit. The increased consumption that we document is likely the result of both direct substitution between fees and consumption and greater access to mainstream credit. After a reduction in overdraft fees, low-income households have the capacity to increase durable and non-durable essential consumption, both of which are likely essential expenditures.

Table 2.10: Household consumption in response to high-to-low reordering bans

This table presents the results of our zip code x quarter level difference-in-differences analysis using Earnest Research consumption data. The sample is restricted to zip codes with below-median income. The outcome variable is the dollar amount or the number of units purchased per consumer. In the first two columns, we examine durable consumption, which is defined to be home and auto-related expenditures. In the next two columns, we examine essential, non-durable consumption, which is defined to be food and clothing-related expenditures. In the last two columns, we examine non-essential, non-durable consumption, which is defined to be all other non-durable expenditures that are not food or clothing-related. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a 4-quarter window before the high-to-low reordering ban and an 8-quarter window after the high-to-low reordering ban. Neighborhood x quarter fixed effects are included across specifications. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Durable consumption per household		Essential, non-durable consumption per household		Non-essential, non-durable consumption per household	
	Dollar amount	Number of units	Dollar amount	Number of units	Dollar amount	Number of units
HTLR Ban x Post	45.18**	1.177**	15.57**	1.379**	10.21	0.456
	(21.76)	(0.565)	(7.380)	(0.622)	(11.15)	(0.557)
Neighborhood x quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	9,874	9,874	7,071	7,071	7,022	7,022
R-squared	0.523	0.518	0.583	0.673	0.566	0.585

*** p<0.01, ** p<0.05, * p<0.1

These findings are consistent with a large literature starting with Bacchetta and Gerlach (1997) that shows that if some consumers are liquidity constrained, aggregate consumption should be "excessively sensitive" to credit conditions.[38] Results in Table 2.9 and Table 2.10 suggest that a reduction in overdraft-related debt service costs leads consumers not only to

[38] In this case, aggregate consumption should also be excessively sensitive to income.

increase consumption but also to experience improved credit health and access to traditional credit. These findings are consistent with the existence of liquidity-constraine, low-income consumers.

2.6.5 Magnitudes

To put the magnitudes of consumer responses in context, we approximate the per-consumer reduction in overdraft fees that resulted from HTLR bans.

The FDIC reports that 14% of bank account users incur five or more overdrafts in a year. We use this statistic to estimate the fraction of consumers who are chronic overdrafters and materially benefitted from the HTLR bans. That is, we estimate that 14% of the 20.1 million total affected customers of the HTLR-banned banks, which is 2.8 million customers, are the primary beneficiaries of the HTLR bans.[39]

We next use estimates of overdraft revenue decline resulting from the lawsuit-mandated HTLR bans in order to approximate the per-customer reduction in overdraft fees. As reported in Table B.2, sued banks required to cease the practice of high-to-low reordering experience a decline in overdraft revenue of approximately 6.44% relative to pre-lawsuit overdraft related revenue. Average pre-lawsuit overdraft-related revenue is approximately $155 million per bank-quarter. Therefore, the decline of 6.44% translates into a loss of approximately $10 million per bank-quarter in overdraft-related revenue. A decline in $10 million per bank-quarter aggregates to $920 million per year for the 23 HTLR-banned banks. A total loss of $920 million divided by the 2.8 million customers who benefit from HTLR bans translates to roughly $330 in savings per chronic overdrafter per year.

Our per-customer overdraft fee savings lie in the ballpark of the $84.84 decline in payday borrowing and $358.3 decline in installment borrowing that we document in Table 2.5 and Table 2.6. Our back-of-the-envelope approximations are thus consistent with the hypothesis

[39] See Table B.1 for the number of affected customers (i.e. settlement class members) for each HTLR-banned bank, which we collected from lawsuit documents. We cross-checked these numbers against an additional source, the 2015 CFPB Arbitration Study. Please note that not every checking account holder is affected by the HTLR class action lawsuits and see the CFPB Arbitration Study for more detail.

that some consumers turn to alternative lenders in order to repay overdraft balances and related fees.[40]

Finally, turning to our consumption results, Table 2.10 shows that expenditures increase by approximately $60 per consumer per quarter, which is concentrated in durable goods spending.[41] Notwithstanding the constraints of the data, we note that this magnitude is consistent with the possible savings derived from a reduction in overdraft fees resulting from HTLR bans. There are likely other effects not captured by our data. For instance, customers may be willing to reduce their precautionary savings because of improved access to credit (documented in Table 2.9). Although we cannot obtain a full picture of consumers' savings and expenditures, we observe that the magnitudes of our analyses are roughly consistent across multiple datasets.

2.7 Spillover Effects

Because overdraft fees constitute a significant fraction of revenue for some banks, especially in low-income areas, the reduction in revenue from ceasing HTLR may make it unprofitable for banks to operate in such areas. To gauge potential spillover effects of HTLR bans, we complement our consumer-level analysis with an examination of bank responses to lawsuit outcomes.

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[40] An important point to acknowledge is that this interpretation of the magnitudes of payday and installment borrowing is a lower bound because it implicitly assumes that customers of HTLR-banned banks are the same population as payday and installment borrowers. We can estimate an upper bound by assuming instead that customers of HTLR-banned banks are no more likely to be payday and installment borrowers than customers of other banks. Specifically, using the fact in Table 3 that the average market share of HTLR-banned banks in treated zip codes is ≈20%, we infer that 20% of payday and installment borrowers are affected by HTLR bans and divide our alternative borrowing results by 20% to arrive at $424 in payday borrowing and $1,790 of installment loan borrowing per borrower per quarter. The true effect of HTLR bans on alternative borrowing lies between the lower and upper bounds, since neither assumption is fully accurate. Table 4 suggests that HTLR-banned banks and alternative financial service providers co-locate and may serve similar customers such that customers of HTLR-banned banks are more likely than customers of non-HTLR-banned banks to be payday and installment borrowers. The true scaling factor of alternative borrowing should be a value between 20% and 100%.

[41] This is a per-consumer, not per-borrower result. If we were able to focus specifically on the consumption of affected borrowers, the magnitude would likely be larger.

We begin by investigating whether banks subject to HTLR bans are more likely to close branches by estimating the following bank by zip code by year level regression:

$$Exit_{izt} = \beta \cdot HTLR\,Ban_i \cdot Post_t + \eta_{zt} + \varepsilon_{izt} \tag{2.4}$$

where the dependent variable $Exit_{izt}$ is a dummy variable that takes a value of 1 if bank i exited zip code z in year t and 0 otherwise, $HTLR\,Ban_i$ is a dummy variable that takes a value of 1 if bank i was sued and subject to a HTLR ban and 0 for all other banks operating in that zip code, and $Post_t$ is a dummy variable that takes a value of 1 for the year of the HTLR ban and up to three years after and 0 for the three years prior to the ban. Zip code by year and bank by zip code fixed effects are included.

The first three columns of Table 2.11 document the results of this regression for all zip codes. Because incentives to close branches may also depend on the strength of banks' local presence, we then focus on zip codes in which treated banks have two or fewer branches in the second set of three columns.[42] In the last set of three columns, we include an additional interaction term with the Low-Income dummy variable that takes on a value of 1 if the zip code has below-median household income in the given year.

We find that banks required to cease HTLR exhibit up to a 2% higher probability of closing branches. This effect is concentrated in zip codes deemed low-income areas and in which banks with HTLR bans have a low number of branches. These findings suggest that it is indeed in the most marginal areas that banks find it optimal to close branches subsequent to HTLR bans. These effects are illustrated graphically in Figure 2.4.

[42] These results are not dependent on the specific threshold of two branches.

Table 2.11: Bank branch closures in response to high-to-low reordering bans

This table presents the results of our zip code x bank x year level difference-in-differences analysis. The outcome variable is a dummy variable that takes on a value of 1 if the bank exits the zip code in that year, and a value of 0 otherwise. HTLR Ban is a dummy variable that takes on a value of 1 if the bank was required to cease high-to-low reordering, and a value of 0 for all other banks in the FDIC Summary of Deposits data. Post is a dummy variable that takes on a value of 1 in the years after the high-to-low reordering ban, and a value of 0 in the years leading up to the high-to-low reordering ban. We examine a 3-year window around the high-to-low reordering ban. In the first three columns, we examine the full set of zip codes. In the next three columns, we subset to zip codes where the high-to-low-reordering-banned bank had 2 or fewer branches. In the last three columns, we again examine the full set of zip codes but introduce a regressor: the triple interaction term HTLR Ban x Post x Low-income. Low-income is a dummy variable that takes on a value of 1 if the zip code has below-median household income in the given year, and a value of 0 otherwise. Varying levels of fixed effects are included. Standard errors are clustered by bank and zip code.

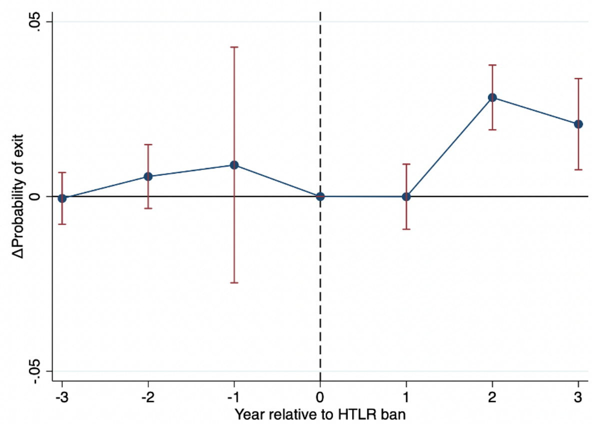
	Bank exit								
Sample:	All zip codes			Zip codes with $\leq$ 2 HTLR ban branches			All zip codes		
HTLR Ban x Post	0.00711*** (0.000811)	0.00896*** (0.000828)	0.0108*** (0.000939)	0.0164*** (0.000972)	0.0185*** (0.000989)	0.0206*** (0.00117)	0.00517*** (0.00136)	0.00682*** (0.00137)	0.00761*** (0.00158)
HTLR Ban x Post x Low-income							0.00298* (0.00170)	0.00334** (0.00169)	0.00481** (0.00196)
Zip code fixed effects	Y	Y	N	Y	Y	N	Y	Y	N
Year fixed effects	Y	Y	N	Y	Y	N	Y	Y	N
Zip code x year fixed effects	N	N	Y	N	N	Y	N	N	Y
Bank x zip code fixed effects	N	N	Y	N	N	Y	N	N	Y
Observations	509,807	509,807	496,461	457,857	457,857	444,292	509,807	509,807	496,461
R-squared	0.025	0.028	0.298	0.027	0.030	0.302	0.026	0.028	0.298

*** p<0.01, ** p<0.05, * p<0.1

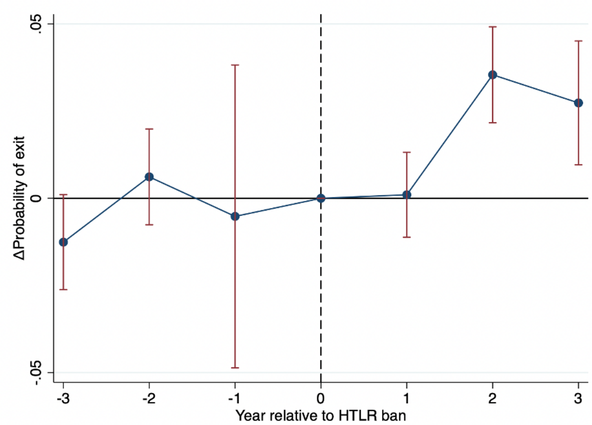
Figure 2.4: Bank branch closures in response to high-to-low reordering bans

This figure presents the results of our zip code x year x bank level difference-in-differences analysis using FDIC Summary of Deposits data. The outcome variable is a dummy variable that takes on a value of 1 if the bank exits the zip code in the year, and a value of 0 otherwise. In Panel A, we examine the full set of zip codes. In Panel B, we subset to zip codes where the high-to-low-reordering-banned bank has 2 or fewer branches. Coefficients are plotted from -3 to +3 years around the high-to-low reordering ban for a difference-in-differences regression of bank exit for high-to-low-reordering-banned banks relative to all other banks in the FDIC Summary of Deposits data. Zip code x year and bank x zip code fixed effects are included. Standard errors are clustered at the bank x year level.

(a) All zip codes



(b) Zip codes with few treated branches



With data at the zip code by year by bank level, we are able to control non-parametrically for a number of other factors that could affect a bank's exit decision. Time-invariant differences across zip codes and time do not seem to affect the results, as we control for zip code and year fixed effects. However, some zip codes may be subject to year-specific economic shocks that make it unprofitable for some banks to operate. We control for the latter possibility by including zip code by year fixed effects in columns 3, 6, and 9, which means that we are identifying within zip code by year level variation in exits. Because there may also be bank-specific preferences for closing some branches in some regions (e.g. economies of scale from having a larger market share in a particular location), we also control for bank by zip code fixed effects. Consistently across specifications, we find that banks are more likely to close branches after being forced to change their overdraft policies.

These results help to inform the debate on "financial deserts," large swaths of neighborhoods without bank branches. Since the Great Recession, more than 6,000 branches have closed throughout the United States.[43] This phenomenon has generated concern among policymakers about possible adverse effects on access to financial services and credit, especially for people most in need of these services. Furthermore, there is evidence that bank closures have negative real effects on income, (Ashcraft, 2005) as well as on small business lending and local employment (Nguyen, 2019). It is therefore plausible that although borrowers benefit overall from banks ceasing HTLR, there may be more general negative spillover effects of branch closures on other parts of the economy.

We directly test this hypothesis in Table 2.12, using HMDA and SBA lending data to examine whether the log of the total amount of mortgage lending or the log of the total amount of small business lending by size (i.e. below $100,000, between $100,000 and $250,000, or above $250,000) declined after HTLR-banned banks exited the neighborhood. For mortgage lending, we find no significant effect. For small business lending, we find that loan amounts remain steady for all categories except for the smallest loans, which exhibit

[43] See the statistics reported here:
https://ncrc.org/wp-content/uploads/2017/05/NCRC_Branch_Deserts_Research_Memo_050517_2.pdf.

a slight increase when we do not account for neighborhood by year variation. Intuitively, these results demonstrate that the exit of HTLR-banned banks did not affect the overall provision of credit, either because bank branch locations do not matter significantly for the provision of mortgage and small business credit or because other banks unaffected by the lawsuits stepped in to make loans.

Table 2.12: Spillover effects of branch closures due to high-to-low reordering bans: Mortgage lending and small business lending

This table presents the results of our zip code x year level difference-in-differences analysis using Home Mortgage Disclosure Act (HMDA) data and Small Business Administration (SBA) data. The outcome variable is the log of the loan amount. In the first two columns, we examine mortgage lending. In the next two columns, we examine small business lending with principal amounts between $1 and $100,000. In the next two columns, we examine small business lending with principal amounts between $100,000 and $250,000. In the last two columns, we examine small business lending with principal amounts between $250,000 and $1 million. HTLR Bank Exit is a dummy variable that takes on a value of 1 if the zip code experiences the exit of a high-to-low-reordering-banned bank, and 0 for all other zip codes. Post is a dummy variable that takes on a value of 1 in the years after the exit, and a value of 0 in the years leading up to the exit. We examine a 3-year window around the exit. Varying levels of fixed effects are included. Standard errors are clustered by zip code.

	Log(Loan amount)							
Sample:	All mortgage loans		Small business loans from $1k to $100k		Small business loans from $100k to $250k		Small business loans from $250k to $1m	
HTLR Bank Exit x Post	0.00346 (0.0131)	-0.0381 (0.0717)	0.0183* (0.00869)	-0.0174 (0.0290)	0.00974 (0.00838)	-0.0340 (0.0232)	0.0107 (0.00965)	-0.00571 (0.0249)
Zip code fixed effects	Y	N	Y	N	Y	N	Y	N
Year fixed effects	Y	N	Y	N	Y	N	Y	N
Neighborhood x year fixed effects	N	Y	N	Y	N	Y	N	Y
Observations	346,077	161,454	314,801	143,489	221,196	96,857	216,173	95,742
R-squared	0.638	0.669	0.444	0.471	0.216	0.289	0.206	0.276

*** p<0.01, ** p<0.05, * p<0.1

Table B.3 and Table B.4 complement these findings by analyzing at the bank level how other banks that are unaffected by lawsuit outcomes responded to the branch exit of HTLR-banned banks. We show that these other institutions were not more likely to enter or exit neighborhoods from which HTLR-banned banks exited, and they also did not experience significant change to overdraft-related revenue, overdraft-related balances, or number of insured depositors.

2.8 Robustness

In this section, we discuss several robustness and placebo tests related to the quasi-exogeneity of high-to-low reordering bans, payday lending bans, choice of control group, heterogeneity across income levels, payday loan supply effects, cash settlement, and alternative measures of average borrowing.

2.8.1 Quasi-Exogenous Shocks to Bank High-to-low Reordering

In this section, we provide evidence that the lawsuit outcomes serve as quasi-exogenous shocks to the HTLR practices of banks.

There is room for non-uniform ruling in the lawsuits because the practice of HTLR is not itself illegal. In the deposit account agreement, the contract that sets the rules of the consumer-bank relationship, banks often reserve the right to reorder transactions freely, which makes it difficult for consumers to subsequently claim unlawfulness or deception. All lawsuits in our sample were ultimately settled with no admission of liability or wrongdoing by banks. Instead, banks claimed they were providing monetary relief (a cash payment) and in some cases behavioral relief (an end to HTLR) in order to avoid an expensive, drawn-out legal process.

For our outcome variables of interest, we argue that these lawsuit outcomes constitute quasi-exogenous shocks to banks' HTLR practices. The lawsuits were lodged against a wide array of banks ranging from systemically important financial institutions (e.g., Bank of America, Citibank, JPMorgan Chase, and Wells Fargo) to regional banks (e.g., Independent Bank, Great Western Bank, Northwest Savings Bank, and Umpqua Bank). A similar presence of systemically important financial institutions and regional banks is observed when we compare banks required to cease HTLR v.s. those that maintained HTLR. For example, JPMorgan Chase and Wells Fargo ceased HTLR, while Bank of America and Citibank did not. Great Western Bank and Northwest Savings Bank ceased HTLR, while Independent Bank and Umpqua Bank did not.

More specifically, we argue that the determinants of lawsuit outcomes are plausibly unrelated to our outcome variables of interest (low-income consumer credit health, consumption, and demand for payday and installment loans). As noted in the 2015 CFPB Arbitration Study, while there was broad similarity in the business practices of banks and legal claims made against them, there was variety in the contracts between consumers and banks and also in the approach to litigation. For example, the CFPB notes, "some banks did not have arbitration clauses in their checking account agreements with consumers and settled the cases, generally providing both monetary and behavioral relief. Other banks had arbitration clauses in their agreements, moved to compel arbitration, and secured dismissal of federal class actions in favor of individual consumer arbitration. Yet other banks had arbitration provisions in their consumer agreements and nevertheless settled either without invoking the arbitration clause or after invoking the clause with something less than complete success."

We argue that ex-ante variation in contracts likely led to different lawsuit outcomes, and that it is improbable that consumers were aware of these ex-ante contract differences. Therefore it is unlikely that there is selection into different banks and that customers of banks required to cease HTLR differ along meaningful dimensions from customers of sued banks not required to cease HTLR. We provide several pieces of empirical support for this assumption.

First, one potential concern may be that zip codes where HTLR-banned banks operate are systematically different from zip codes where non-HTLR-banned banks operate. For example, if HTLR-banned banks operate in poorer areas where consumers are also more likely to be negatively affected by HTLR ex ante, then the HTLR ban may be correlated with unobservable time-varying factors specific to people living in those affected areas. We assuage concerns that HTLR-banned zip codes are fundamentally different from non-HTLR-banned zip codes by directly testing if there is a relationship between whether a zip code gets treated (i.e. whether the zip code contains branches belonging to a bank treated with a HTLR ban) and the characteristics of the zip code. In Table B.5, we report that key zip code characteristics are unrelated to the outcome of the lawsuit. For example, the characteristics of population size, ethnic minority fraction, education, family status, marital status, poverty status, income, employment, and housing status do not predict whether a zip code is treated. Put differently, poorer areas are not more likely to be treated. This finding suggests that treated zip codes do not differ significantly from control zip codes along these dimensions. It is therefore unlikely that treated zip codes are relatively more exposed to bank overdraft practices because of regional characteristics.

Building on this, we next test whether key bank characteristics are correlated with lawsuit outcomes. For example, if lawsuit outcome is correlated with level of harm inflicted on consumers ex ante, then we might expect customers of HTLR-banned banks to be systematically different from customers of non-HTLR-banned banks. This may then raise the concern that unobservable time-varying factors are correlated with the timing of the lawsuit outcome. Although unable to observe bank customer characteristics directly, we can observe key aggregate outcomes at the bank level, such as bank reliance on overdraft revenue. In Table B.6, we report that lawsuit outcomes are not related to ex-ante bank reliance on revenue from overdraft fees. We also report that HTLR bans are not related to bank size or return on assets (overall profitability). The small increase in R-squared from column (1) to (2) indicates that reliance on overdraft-related revenue has little additional explanatory power in predicting HTLR bans, and these findings are consistent with the idea that HTLR-banned banks are unlikely to be those that inflict the most financial harm on consumers ex ante. Put differently, the results in Table B.6 are not consistent with the argument that lawsuit outcomes are a function of the extent to which bank customers were harmed by bank overdraft practices ex ante. While we only have aggregate measures available to test this hypothesis, we argue that the zip code-level and bank-level tests combined cast doubt on the argument that lawsuit outcomes are related to unobservable consumer-level characteristics, which would threaten the validity of the exclusion restriction and confound interpretation of our results.

In our difference-in-differences analyses, we find no existence of pre-trends in our outcome variables, which is further consistent with the quasi-exogeneity of the behavioral relief treatment from the lawsuits. We therefore argue that the lawsuits serve as a suitable natural experiment for studying the impact of aggressive bank practices on consumer credit health and activity in the alternative financial system.

Finally, we note that non-banned banks are unlikely to quietly cease HTLR because it is likely very profitable. We find that consumer complaints about HTLR have reappeared in recent years (outside of our sample period) in the CFPB Consumer Complaints database, suggesting that the potential reputation and financial costs of HTLR are outweighed by its profitability. We note, however, that if a non-HTLR-banned bank were to voluntarily cease the practice during our sample period, it would downward bias our estimates.[44]

2.8.2 Payday Lending Bans

An important piece of context for our study is that there is variation in payday lending rules and prohibition across states.[45] High-to-low reordering bans can only have an effect on payday lending in states where payday lending is allowed. Therefore, as a robustness test, we conduct our main analysis in states without payday bans. In Table B.7, we remove observations from payday banned states and present results for our main specification. We find that the number of observations falls very little and that the results are similar in statistical significance and economic magnitude. This analysis highlights the fact that our original tests are already concentrated in states where payday lending is legal and therefore present. In particular, our outcome variables of payday borrowing and low-income installment borrowing constructed from the Clarity and Equifax datasets already hone in on

[44]We flag one case of a bank voluntarily ceasing HTLR which happens outside of our sample period. In October of 2013, Bank of America voluntarily ceased HTLR, nearly 2 years after its lawsuit settlement that did not include a HTLR ban. This case does not impact our empirical strategy because we examine a 4-quarter window following the HTLR ban. We note that this voluntary end to HTLR may be connected to regulatory scrutiny on the bank at the time in the form of a 2013 CFPB white paper on overdraft practices and a 2014 settlement Bank of America had to pay for consumer credit card practices. After investigating other banks' practices around this time, we found no other cases of voluntary changes in bank HTLR practices.

[45]For example, see Bhutta *et al.* (2016) for details on state regulation of payday lenders.

areas where payday lending is allowed.

2.8.3 Choice of Control Group

We conduct tests to investigate whether our main results hinge on the choice of control group: zip codes containing branches of sued but not HTLR-banned banks that lie within the neighborhood (i.e. within 7 miles) of the treated zip code. In Table B.10, in columns (1) and (2) of Panel A and B, we set the control group to be zip codes within seven miles of the treated zip code but exclude zip codes containing branches of sued but not HTLR-banned banks. With this significantly more restrictive control group definition, we find similar reductions in payday and installment loan borrowing in response to HTLR bans. In columns (3) and (4), we set the control group to be zip codes in the same state as the treated zip code. Again, we observe similar reductions in payday and installment loan borrowing relative to our main results.

Next, we explore whether our main results depend on the definition of a neighborhood being within a 7-mile radius. In Table B.11, we re-run our analyses of the Clarity sample (Panel A) and Equifax sample (Panel B) for neighborhood radii of 5 miles and 10 miles. We observe similar reductions in payday borrowing and installment borrowing across these columns compared to our main results.

In sum, the results in Table B.10 and Table B.11 are reassuring that the choice of control group is not a key driver of our results.

2.8.4 Heterogeneity Across Income Levels

We next test whether there is important heterogeneity in our main findings across income levels. We expect our results to be concentrated among those with the lowest incomes, because these individuals are more likely to have bank account balances close to zero and be affected by bank overdraft practices. An advantage of the Equifax data is the opportunity to observe borrower income.[46] We report our baseline specification by income quintile in

[46]Note that we do not have borrower income for our Clarity sample.

Table B.14, with column (1) subsetting to borrowers in the bottom income quintile and column (5) subsetting to borrowers in the top income quintile.

Consistent with the hypothesis that the borrowers most likely to be affected by HTLR bans are those with low incomes, we find the reduction in installment borrowing to be concentrated among borrowers in the bottom two income quintiles. Similarly, we report in Table B.15 that the effects on payday and installment borrowing are not present in high-income zip codes. The fact that the statistical and economic significance of the results disappear when we focus on higher-income consumer segments suggests that there is within-bank heterogeneity in overdraft practices.

2.8.5 Possible Payday Loan Supply Effects

We further test for possible spillover effects by assessing whether the supply of payday loans is affected by HTLR bans. Intuitively, if it is in fact the case that banks process transactions from high-to-low to ensure that large payments are more likely to be processed, then HTLR bans may reduce the likelihood of payday lenders being repaid on time because payday loan repayments are larger ticket items. HTLR bans therefore could affect the credit risk of payday borrowers and the supply decisions of payday lenders.

In Table B.16, we analyze loan acceptance rates for payday loans and find no evidence of any change following the HTLR bans. These findings suggest that the reductions in payday and installment borrowing documented in Table 2.5 and Table 2.6 are likely due to decline in demand for alternative credit, rather than supply.

2.8.6 Cash Settlement

Lawsuit settlements included cash relief in addition to potential behavioral relief. One concern therefore may be that the borrowing and credit health effects we find in response to HTLR bans are confounded by the positive liquidity shock of cash settlement payments. We address this concern in the following ways.

First, we document that the one-time, per-customer cash settlement amount is small relative to the quarterly per-consumer borrowing declines that we observe. As seen in Table B.1, for the 37 banks in our lawsuits sample, the total cash settlement amount was $1.45 billion. Before reaching consumers, however, this cash settlement had to be used first to pay settlement administration fees and plaintiff attorney fees, the latter of which were substantial and often consumed one-third of the cash settlement. After netting out settlement administration costs and attorney fees, the remaining cash settlement amount was $1.04 billion. Dividing by the 44.1 million total consumers impacted by HTLR and entitled to cash settlement, we find that the per-customer cash settlement amount was just $23.51. Additionally, we note that cash settlements were disbursed to consumers much later than the HTLR bans were enacted and therefore well outside of our window of analysis.

Second, in order to more explicitly account for cash settlements, we re-run our main tests with an additional control variable: the average cash payout per consumer. This variable is computed as the average per-consumer cash relief payment across all bank branches in a zip code.[47] In Table B.17, we find that controlling for cash settlement amounts makes no material difference to our main findings. This is likely because the cash settlement per person was very small and paid much later than our period of analysis.

We further note that treated (HTLR-banned) and control (non-HTLR-banned) banks paid cash settlements of similar amounts. The average cash disbursement was $25.04 for HTLR-banned banks and $18.74 for non-HTLR-banned banks. If there is a concern that cash settlements may confound interpretation of our results, the fact that the cash settlements were very similar for customers of HTLR-banned vs. non-HTLR-banned banks provides further reassurance that our results likely reflect the effect of HTLR bans.

Third, we generate event study plots that illustrate that installment and payday borrowing do not react around cash disbursement dates. It is not possible to set up the same difference-in-differences analysis with neighborhood by quarter fixed effects because both our treated (HTLR-banned) and control (non-HTLR-banned) groups received cash payments.

[47] The per-member cash relief for each lawsuit bank is documented in Table B.1, and the per-member cash relief for non-lawsuit banks is set to 0.

Instead, we do a similar exercise by computing event time around cash payments for the treated and control zip codes and then demeaning the outcome variable (payday borrowing and installment borrowing) by neighborhood and quarter. In Figure B.1, we find that borrowing does not differ between our treated and control groups around cash payouts.

In sum, these pieces of analysis suggest that it is unlikely that cash settlement can explain why customers of treated banks exhibited installment and payday borrowing declines relative to customers of control banks.

2.8.7 Alternative measures of average payday and installment borrowing per zip code

The dependent variable in our main tests is representative of the average borrower in a given zip code. Specifically, it is computed as the total borrowing in a zip code divided by the number of borrowers. To assuage concerns that the number of borrowers itself may be changing in response to HTLR bans and therefore affect results, we report our main findings where we normalize total borrowing by other plausibly exogenous variables.

Using U.S. Census Bureau American Community Survey 5-year data, we estimate our main borrowing tests using as an alternative dependent variable the amount of borrowing divided by the number of low-income individuals in the zip code (defined as those earning less than the median income for the zip code in a given year at a point prior to the window of analysis). The intuition is that the number of low-income individuals in a zip code at a point prior to the window of analysis is clearly exogenous to the HTLR ban. Given that the income distribution varies substantially across zip codes, this variable provides a zip code-specific measure of the number of lower-income individuals. Results of this analysis are reported in Table B.9 in column (4).

We provide the following guidance on how to interpret the coefficients. The median number of people earning below-median income in a treated zip code is approximately 5,000. The median number of installment borrowers per zip code in our sample is 10. The median number of payday borrowers per zip code in our sample is 1. The median number of installment borrowers who earn below the 20th percentile of income per zip code in our sample is 1. The interpretation of the payday borrowing magnitude in column (4) of Panel A is that a reduction of $0.0226 in payday borrowing per low-income consumer is equivalent to a total reduction of approximately $113 ($0.0226 * 5,000 / 1) in payday borrowing per payday borrower. The interpretation of the installment borrowing magnitude in column (4) of Panel B is that a reduction of $0.756 of magnitude of installment borrowing per low-income borrower is equivalent to a reduction of $378 (($0.756 * 5,000 / 10) * 1) per installment borrower in the lowest income quintile. Not only do we find significant, negative effects when we use the more exogenous scaling variable, but we also find that the magnitudes of the newly scaled results are very close to the magnitudes that we document in our main tests. These findings demonstrate that the main results are not driven by other factors responding to HTLR bans.

In order to enhance this analysis, we perform additional tests where we scale by other variables that are more plausibly exogenous than our original scaling variable. We report results of these tests in columns (2) and (3) of Table B.9. In column (1), we include the specification of our main result in Table 2.5 and Table 2.6. In column (2), we scale borrowing by the total number of alternative borrowers in a zip code in a quarter (i.e. the total number of borrowers in a zip code in a quarter in the Clarity alternative credit bureau data or the total number of installment borrowers in a zip-code in a quarter in the Equifax data). In column (3), we scale borrowing by the number of borrowers (again either payday borrowers or lowest-income-quintile installment borrowers) at a point prior to the window of analysis. The fact that the denominator quantity predates the HTLR ban ensures that the denominator cannot be responding to the ban.

In these tests, we observe that our main results hold across each specification. The estimates that are most intuitive to compare are those in column (1) v.s. column (3). Here we see that dividing by the pre-event-window number of borrowers yields similar estimates as dividing by the contemporaneous number of borrowers. The estimates are not statistically distinguishable from one another. In sum, the additional analysis in this section gives us confidence that the choice of scaling does not impact our main results.

2.9 Conclusion

A growing fraction of Americans are turning to alternative finance providers (such as payday lenders and check cashers) to fulfill their most basic financial needs.

This phenomenon has attracted the attention of federal and state regulators concerned that alternative lenders are exploiting the financial fragility of these individuals and placing them at risk of being denied access to traditional financial services. Our paper adds a different perspective to the policy conversation. Arguing that low-income consumers may turn to the alternative financial system if the traditional system is not serving them well, we suggest that banks may play a role in "pushing" customers out of the traditional system and into the alternative system.

Our findings provide evidence of a link between overdraft credit provided by traditional banks and payday loans provided by alternative financial institutions. Our finding that consumers borrow less in alternative credit markets following a reduction in costs associated with obtaining overdraft credit suggests that overdrafts may create a demand for payday and installment loan borrowing.

This may come at a hefty price. As is well documented in the literature, payday borrowing and more generally high-cost, short-term loans can trap consumers in a cycle of debt. Indeed, we find that, after a reduction in overdraft fees and a subsequent reduction in alternative credit usage, there is an improvement in consumer financial health and access to cheaper traditional credit.

The results reported in this paper may inform policymakers working to support the financial health of lower-income consumers and optimally regulate the financial markets used by them. Our findings also cast doubt on the notion that being "banked" is a panacea for low-income individuals.

Chapter 3

Segmented Going-Public Markets and the Demand for SPACs[1]

3.1 Introduction

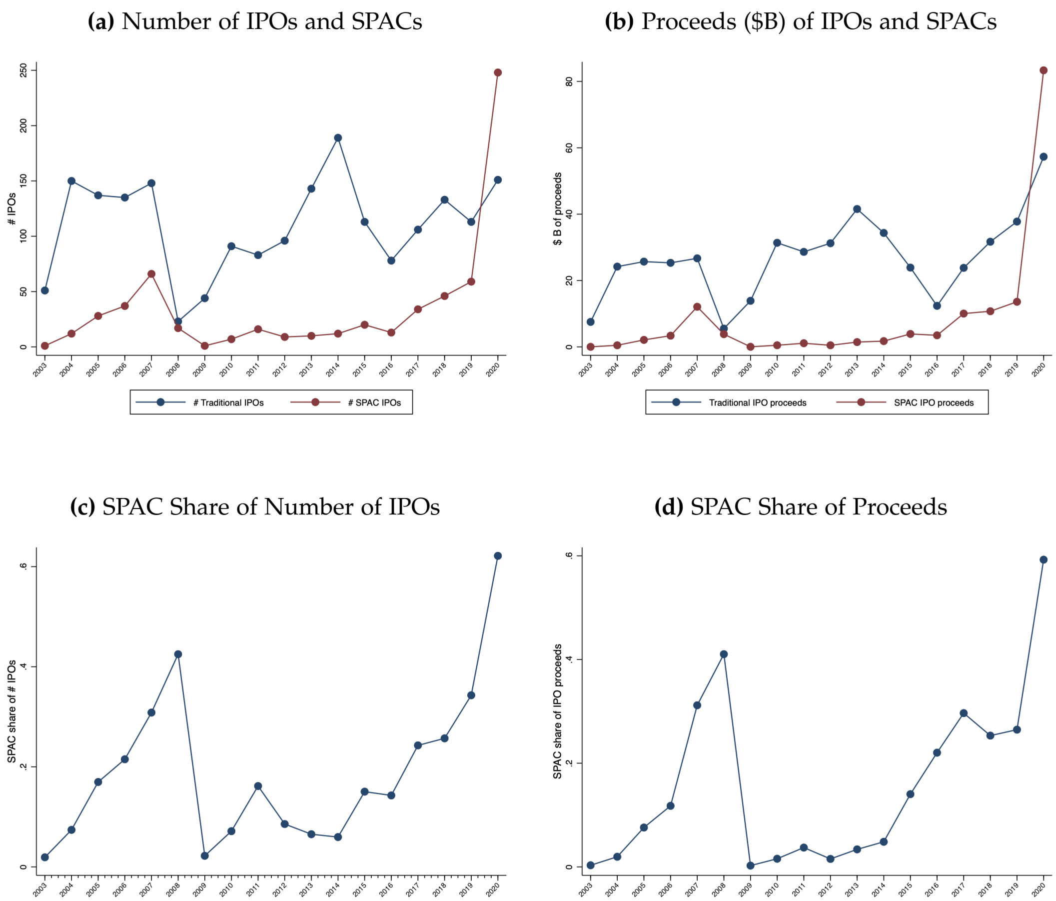
In recent years, the U.S. going-public market has witnessed the rise of the special purpose acquisition company (SPAC) – a non-operating entity with the sole purpose of taking a private firm public in a reverse merger acquisition. SPACs have become an increasingly popular alternative to traditional initial public offerings (IPOs). Figure 3.1 plots the counts and total proceeds of SPACs and traditional IPOs annually from 2003 to 2020 in Panels A and B, respectively.[2] The plot shows the dramatic increase in SPAC activity, as measured by the number of SPACs formed and the aggregate proceeds raised, over the past decade. In 2020 alone, the SPAC market raised over $83.4 billion, approaching the size of the traditional IPO market.

[1]Co-authored with Jessica Bai and Miles Zheng

[2]Please see the data section for our traditional IPO data cleaning procedure consistent with the literature.

Figure 3.1: Time-Series Trend of SPAC and Traditional IPO Activity

This figure shows the evolution of SPAC and traditional IPO activity over time. Each observation is a year from 2003 to 2020. Panel (a) displays the number of SPACs and traditional IPOs in each year. Panel (b) displays the aggregate proceeds for SPACs and IPOs (in USD billions) in each year. Panels (c) and (d) show the market share of SPACs. In Panel (c), the market share of SPACs is measured as #SPACs/(#SPACs + #IPOs), where #IPOs excludes SPAC issuances. In Panel (d), the market share of SPACs is measured as SPAC Proceeds/(SPAC Proceeds + IPO Proceeds), where IPO Proceeds excludes proceeds from SPAC issuances. See Section 3.1 for a full description of the SPAC and IPO samples.



(a) Number of IPOs and SPACs

(b) Proceeds ($B) of IPOs and SPACs

(c) SPAC Share of Number of IPOs

(d) SPAC Share of Proceeds

However, the recent enthusiasm for SPACs from high-tech companies and established investment managers is puzzling when juxtaposed against the skepticism voiced by journalists, regulators, and academics. The prevailing judgment of the SPAC market is that it enables "lemon" firms to bypass the rigorous SEC supervision and underwriting process that traditional IPOs must undergo.[3] Taken together, these observations underscore the mystery of SPACs and motivate our main research questions: Do SPACs play a valuable economic role that rationalizes their existence? What explains the time-series variation in SPAC activity? If the SPAC market does serve a purpose, what are the remaining frictions, and how can policy intervention alleviate them?

This paper formally examines the economic role of the SPAC market and addresses the questions highlighted above by providing a simple model, presenting cross-sectional and time-series facts, and identifying a litigation risk-based origin of the SPAC market. The main friction in our model is asymmetric information between firms and public investors, which leads to adverse selection. To mitigate adverse selection, investment banks play valuable certification roles by acting as underwriters in the IPO market (Booth and Smith, 1986; Chemmanur and Fulghieri, 1994; Brau and Fawcett, 2006; Puri, 1999). We show that the substantial legal liabilities borne by underwriters of traditional public offerings are important for investor protection but bring about the unintended consequence of segmented going-public markets. That is, in equilibrium, SPAC managers act as non-bank certification intermediaries and bring unserved risky, small, and potentially high-growth firms public via the SPAC market. In doing so, SPACs cater to yield-seeking investors. Meanwhile, investment banks bring safer and larger firms public via the traditional IPO market. Reaching for yield in the going-public market increases the demand for risky production firms and contributes to the rise of SPACs.[4] Consistent with the model, we

[3]For example, Arthur Levitt, a former SEC Chairman stated, "I have never found any blank-check investment vehicle attractive." See https://www.investingdaily.com/10914/special-purpose-acquisition-companies-spacs-will-investors-live-long-and-prosper/.

[4]Reaching for yield in our context refers to investors' propensity to buy riskier assets as in Becker and Ivashina (2015).

empirically show that: (1) Compared to IPO firms, SPAC firms are ex ante smaller and riskier, but grow at higher or similar rates after going public. (2) SPAC activity relative to traditional IPO activity is strongly positively correlated with equity market sentiment. (3) An increase in litigation risk of IPOs relative to SPACs shifts activity towards SPACs.

To begin, we provide background and develop a hypothesis of the role of the SPAC market that focuses on intermediary litigation risk and compensation structure. Our framework features operating firm heterogeneity along two dimensions: quality and riskiness. Firm quality is private information known to the firm and unknown to public investors, while firm riskiness and size are public information.[5] Public investors differ in their preferences for project riskiness: safety-seeking investors prefer safe projects, while yield-seeking investors prefer riskier projects.

We show that the differences in regulatory environment and compensation structure of the SPAC and traditional IPO markets affect the incentives of intermediaries to take firms public. First, the SPAC market faces a more lenient regulatory environment than the IPO market. Compared to investment banks, SPAC sponsors face substantially lower litigation risk because the SPAC going-public process is regulated under merger rules rather than public offering rules. SPAC sponsors are far less exposed to underwriter liability under Section 11 of the U.S. Securities Act of 1933.[6] In addition, SPACs are protected by the Private Securities Litigation Reform Act from liability for projections and other forward-looking statements.[7] Second, compensation practices in the IPO and SPAC markets

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[5]Firm quality is determined by the attributes of entrepreneurs, which is unobservable private information. Firm riskiness and size are determined by the attributes of the investment project, which are observable public information.

[6]Klausner *et al.* (2022) find that from 2015 to 2019, ∼15% of traditional IPOs have been the target of shareholder suits under Section 11, and ∼90% of those suits name the underwriter as a defendant. By comparison, from 2009 to 2019, there were no Section 11 suits against SPACs based on their IPOs, and there were only two Section 11 suits against post-merger SPACs. Under Section 11, investors can sue underwriters for misstatements or omissions, and there is no need to prove causation or scienter. The litigation cost for underwriters includes not only the direct costs of lawsuits but also the indirect costs of losing market share. Hanley and Hoberg (2012) estimate that if an IPO firm associated with a lead underwriter is sued under Section 11, the lead underwriter on average loses ∼\$59 million. The expected litigation cost is particularly high for IPOs because performing due diligence is fraught with difficulty and uncertainty (Tiniç, 1988).

[7]It is important to note that SPAC sponsors are not immune from liabilities. They face liabilities under

differ significantly. Investment banks receive a fixed proportion of capital raised in the IPO process, while SPAC sponsors receive a fraction of the equity of the going-public operating firm, which we refer to as the founder share. As a result, the intermediaries differ in their incentives: investment banks are downside-averse, while SPAC sponsors are less so and share in the upside of the operating firms.

The bond-like payoff structure induces investment banks to certify safer firms and take them public in the IPO market. The unintended effect, however, is an incomplete going-public market when only investment bank intermediaries exist. High expected litigation costs deter investment banks from taking value-creating, risky firms public. Moreover, this effect is amplified for smaller firms because they require relatively high due diligence effort from investment banks while delivering relatively low underwriting fees. The going-public market is closed to these value-creating, risky firms that fly beneath the radar of investment banks, despite the fact that these firms would be met with sufficient appetite from yield-seeking investors. Meanwhile, SPAC sponsors are less constrained by expected litigation costs. Moreover, the equity-based compensation structure rewards SPAC sponsors differently from the fixed-proportion fee structure in the traditional IPO market. Because sponsors are compensated with the founder equity share upon reverse merger close, their expected payoff increases in the expected payoff of the operating firms, which in turn incentivizes them to bring high-quality firms public. In equilibrium, the going-public markets are segmented: investment banks take larger and safer firms public in the IPO market,[8] while SPAC sponsors take potentially value-creating, smaller and riskier firms public in the SPAC market and match them with yield-seeking investors.

Section 14(a) which covers misstatement and omissions in proxy statement (which requires proof of negligence) and Section 10(b) which covers misstatement and omissions made with scienter. These liabilities help alleviate agency issues of SPAC sponsors by mitigating fraud. Relatedly, Klausner *et al.* (2022) find no evidence that the SPAC market is a hotbed of fraud or outright investor deception. Therefore, our assumptions of litigation costs are in relative terms, i.e. SPAC sponsors face lower expected litigation costs compared to investment banks in traditional IPOs.

[8] These firms stay in the IPO market as IPO costs are lower than SPAC costs. The SPAC founder share fraction is generally 20% of public equity, while the investment bank IPO underwriting fee is generally 7% of capital raised (Chen and Ritter, 2000).

There are three central implications of our theory. First, the cross-sectional implication is a segmented going-public market: larger, safer, and older firms go public in the IPO market, while smaller, riskier, and younger firms go public in the SPAC market. Second, the time-series implication pertains to the demand for SPACs: time-varying investor preferences for smaller and riskier going-public firms affect SPAC activity. Both SPAC IPO volume and the market share of SPACs in the going-public market increase with the propensity of investors to seek yield. Finally, regarding the origin of the SPAC market, we predict that an increase in litigation risk of traditional IPOs relative to SPACs leads to increased SPAC activity.

We find that these predictions are borne out in the data. By combining data on U.S. SPACs and traditional IPOs, we analyze the aggregate evolution of SPACs relative to traditional IPOs over time. By linking U.S. SPACs to their target companies, we also conduct analyses at the firm level. By exploiting a Supreme Court ruling that increased litigation risk for traditional IPOs relative to SPACs, we assess the causal impact of litigation risk on SPAC activity. Our findings therefore address the time series of going-public activity, the cross-section of going-public firms, and the effect of increased litigation risk.

We present the following empirical findings about the U.S. SPAC market. In the time series, we show that SPAC issuance boomed in 2007 prior to the Global Financial Crisis and accelerated from 2015 to 2020. Additionally, the market share of SPACs is strongly positively correlated with equity market sentiment. A one-standard-deviation increase in the Baker and Wurgler (2006) equity market sentiment index (orthogonalized with respect to macroeconomic variables) is associated with a 6.16 percentage point increase in the quarter market proceeds share of SPACs IPOs. The equity market sentiment measure can explain up to 11.4% of the variation in SPAC activity, and the result is robust to using a reconstruction of the Baker-Wurgler equity market sentiment index that excludes IPO-related inputs.[9] We also find that elevated equity sentiment at the time of SPAC IPO is associated with more rapid

[9]We also find overall market sentiment – proxied by the CBOE Volatility Index (VIX) – has little explanatory power for SPAC activity.

target search and approval. A one-standard-deviation increase in equity market sentiment at IPO is associated with a shorter time to approval by 0.94 quarters or approximately 85 days.

In the cross section, we show that compared to IPO firms, SPAC operating firms are smaller, younger, and riskier at the moment of going public. Specifically, they are smaller in terms of revenue, market capitalization, and assets, and they are riskier as proxied by having higher cash flow volatility over the first four years of going public, being less profitable, and being more cash-constrained (as measured by lower cash-to-assets and payout ratios). Importantly, we further find that SPAC operating firms grow at similar or even higher rates compared to IPO firms after going public. Specifically, in the years immediately after going public, SPAC firms have similar or even higher growth rates of revenue, market capitalization, and assets. These cross-sectional patterns that we document are consistent with the notion that these firms are more speculative investments that are more difficult to value but that also have the potential to execute higher growth. Therefore they are more sensitive to investor sentiment (Baker and Wurgler, 2007) as shown in the time-series findings. The results provide evidence against the hypothesis that the SPAC market is purely a "lemon" market for low-quality firms and suggest instead that SPAC firms may be of comparable quality on average to IPO firms.

Honing in on our hypothesis related to intermediary litigation risk, we conduct a difference-in-differences analysis to investigate whether the differential regulatory environment for intermediaries is one origin of the SPAC market. Our treatment is a Supreme Court ruling (*Cyan, Inc. v. Beaver County Employees Retirement Fund, et al.*) that increased litigation costs for defendants in the traditional IPO market by increasing the propensity that investors file Section 11 claims in state court, in addition to federal court (Klausner *et al.*, 2020). Our treated (control) group is comprised of states with courts that are more (less) friendly to shareholder lawsuit plaintiffs prior to the ruling and that therefore experience a greater (smaller) Cyan-induced increase in state court litigation. We find that following the Cyan ruling, the SPAC proceeds share and share of IPOs increased by 23.3 percentage points and 31.3 percentage points, respectively, in treated states relative to control states. In placebo tests, we do not find effects distinguishable from zero when we change the treatment year or randomly assign treatment to states.

Our paper generates implications for policy. On the one hand, our model highlights the potential for the SPAC market to encourage entrepreneurial and innovative activity, which are inherently risky (Knight, 1921; Kihlstrom and Laffont, 1979; Hall and Lerner, 2010), by granting public market access to firms unserved by traditional underwriters. If SPAC sponsors perform a certification role, the SPAC market can fill a gap by taking value-creating, small and risky firms public. This implication validates the economic role of the SPAC market and is consistent with the developing perspective that the SPAC is a legitimate going-public route.[10] On the other hand, if the riskiness of SPAC firms is combined with potential agency problems of sponsors, then the proliferation of these vehicles could result in excessive risk-taking.

This paper's findings relate to regulatory-based and market-based policy approaches that aim to address agency issues in the SPAC market. One key regulatory-based approach has been to impose underwriter liability on SPAC sponsors, analogous to that which investment banks face in the IPO market. Our results suggest that this policy may undermine one economic role of the SPAC market, which is bypassing downside-averse financial intermediaries and enabling risk-taking but potentially value-creating firms to go public. We propose instead that the compensation structure of SPAC sponsors be based on more long-run performance of the operating firms in order to align SPAC sponsors with long-term shareholders. We suggest that a long-term phase-in structure, earnout provisions, and an optimized stock-warrant mixture of SPAC sponsor compensation, may help achieve this goal. In Section 3.6, we discuss the the evolution of the U.S. SPAC market structure (which has helped to mitigate existing agency issues) and the details of these approaches.

[10]For example, see https://www.finra.org/investors/insights/spacs, https://techcrunch.com/2021/04/28/the-spac-boom-isnt-just-here-to-stay-its-changing-consumer-tech/, and https://newsletterest.com/message/56992/What-you-need-to-know-SPACs-and-ESG.

Related Literature

This paper contributes to several strands of the literature. A large literature in financial economics highlights the importance of certification intermediaries (Diamond, 1984; Admati and Pfleiderer, 1994; Lerner and Tirole, 2006). Our paper points to the development of two tiers of certification intermediaries in the going-public market: investment banks certify larger and safer firms in the traditional IPO market, while SPAC sponsors act as non-bank intermediaries and certify smaller and riskier firms in the SPAC market. Our paper additionally explains why different types of intermediaries arise in the SPAC and IPO markets. Our analysis suggests that underwriter liability and compensation practices cause market segmentation, leading to heterogeneous clientele and demand for specialized certification intermediaries across the two markets. This implication is consistent with the observation that a majority of SPAC sponsors have private equity, hedge fund, or entrepreneurial backgrounds in the specific industry, and thus may be well specialized at identifying promising, early-stage, risky firms using their private market experience. Our paper also implies that SPAC sponsors are inherently yield-seeking because they take on a large equity stake in the going-public firms.

This study also relates to the literature on law and finance. One consequence of IPO litigation identified in the literature is underpricing (Tiniç, 1988; Hughes and Thakor, 1992; Lowry and Shu, 2002), suggesting that some going-public firms may be *underserved* because of "money left on the table." Other studies have pointed to the inefficiencies of underpricing as a way of reducing lawsuits (Ritter and Welch, 2002). Issuers and investment banks mitigate litigation costs by providing strong disclosure (Hanley and Hoberg, 2012). Our findings point to another consequence: the incomplete going-public market. Some value-creating firms are *unserved* because of the "closed door to going public."[11] In our difference-in-differences analysis around the *Cyan* ruling, our paper identifies the causal

[11] Consistent with our hypothesis, Tiniç (1988) suggests that "many of the highly risky small issuers may not constitute an acceptable clientele for the prestigious investment bankers who find the standard underwriting fees inadequate for the risk of potential legal liabilities." Anecdotal evidence also suggests that certain small and risky firms can hardly go public through IPO. For example, see https://pitchbook.com/news/articles/2020-spacs-electric-cleantech-bonanza and https://privateequity.weil.com/insights/exit-strategies-ipos-versus-spacs.

impact of traditional IPO litigation risk on SPAC activity. The main message of our paper is not, however, that litigation costs harm the going-public market; imposing litigation costs provides legal provisions for shareholder protection, which help maintain a well-functioning traditional IPO market (Porta *et al.*, 1997; La Porta *et al.*, 2002; Bernstein *et al.*, 2020). Instead, our paper seeks to highlight how the SPAC market innovation can solve the unintended consequence of the incomplete going-public market through its market design.

This paper adds to the literature on the evolving going-public landscape. The U.S. traditional IPO market has declined in volume over the recent two decades (Gao *et al.*, 2013; Doidge *et al.*, 2017).[12] Existing explanations include (1) the increased regulatory burden for small firms to go public (e.g. Engel *et al.*, 2007), (2) the higher incentives small firms face to be acquired because of economies of scope (Gao *et al.*, 2013), and (3) the increased supply of private capital that has reduced the net benefits of going public for late-stage firms (e.g. Ewens and Farre-Mensa, 2019). The advent of alternative going-public methods complements the traditional IPO market and has the potential to help improve private firms' access to the public market. Zheng (2021), for instance, finds that the direct listing market caters to late-stage firms that desire to go public at lower cost. Our paper shows that the SPAC market is one IPO alternative that caters to small and risky firms, and that the risk appetite of public investors correlates with the demand for SPACs.

Our findings are also consistent with the relative underperformance in three-year holding period returns of smaller and younger offerings as well as poor aftermarket performance among firms that IPO in high-volume years documented by Ritter (1991) and Ritter (2011), together with findings of poor long-run returns to SPAC investors (see, e.g., Table 5 of Gahng *et al.* (2023)). We provide a theoretical framework of the SPAC market to explain the origin of the segmented going-public markets, and document empirically that SPAC firms are on average smaller, younger, and exhibit higher growth than comparable IPO firms. Thus, this paper's model and empirical patterns help provide an explanation for the underperformance of SPACs and their susceptibility to investor over-optimism and

[12] The IPO samples in these two studies exclude SPAC IPOs.

bubble-like phenomena. Our finding that the market share of SPACs is strongly positively related to investor sentiment in the equity markets further supports the observed longer-run underperformance of such offerings.

Finally, this paper contributes to a growing financial economics literature on SPACs. Existing empirical studies have focused on the performance of SPACs, their returns, and factors that might affect approval of the business combination (Lewellen, 2009; Cumming *et al.*, 2014; Kolb and Tykvova, 2016; Dimitrova, 2017; Gahng *et al.*, 2023). Kolb and Tykvova (2016) also observe that SPAC firms are smaller than IPO firms. Blomkvist and Vulanovic (2020) examine the relationship between SPAC activity and market uncertainty.[13] Gahng *et al.* (2023) document the high cost of going public through the SPAC route, high returns of IPO-stage investors, low returns of post-acquisition investors, and high returns of SPAC sponsors. Recent theoretical literature has examined why SPACs issue redeemable shares (Banerjee and Szydlowski, 2022) and analyzed the choice between PE v.s. SPAC (Gryglewicz *et al.*, 2021). Our paper provides an economic rationale for the existence of SPACs, analyzes the choice between SPAC v.s. traditional IPO, and documents new time-series patterns on the market share of SPACs and cross-sectional patterns on characteristics of SPAC and IPO going-public firms for all post-2003 SPACs.

The rest of the paper is organized as follows. Section 3.2 presents our conceptual framework of the SPAC market, including institutional details. Section 3.3 describes our data and empirical findings from the U.S. SPAC market. Section 3.4 presents our difference-in-differences analysis on how traditional IPO litigation risk impacts SPAC activity and discusses alternative hypotheses. Section 3.5 discusses alternative hypotheses. Section 3.6 raises potential agency issues and policy recommendations for an improved SPAC market structure. Section 3.7 concludes.

[13]The sample period of their paper ends in 2019, while that of our paper extends through 2020, capturing the SPAC boom period of 2020. Our result on the relationship between SPAC activity and volatility as measured by the VIX differs from theirs.

3.2 A Conceptual Framework: Segmented Going-Public Markets and the Demand for SPACs

This section presents a simple model of the SPAC market in an environment with adverse selection. The model is based on Tirole (2010), which considers the role of underwriters as certification intermediaries in the capital-raising process. Operating firms have private information about their quality that is unobservable to investors, and underwriters act as certification intermediaries in the IPO market. We introduce additional firm heterogeneity, investor heterogeneity, and the role of SPAC sponsors as non-bank certification intermediaries into the framework of Tirole (2010) in order to analyze the origin of the SPAC market. We demonstrate that the going-public market structures of SPACs and IPOs generate differing incentives for intermediaries. This in turn generates a set of empirical predictions about the cross-sectional patterns of SPAC and IPO going-public firms, the time series of SPAC and IPO activity, and the impact of a change in litigation risk on SPAC activity.

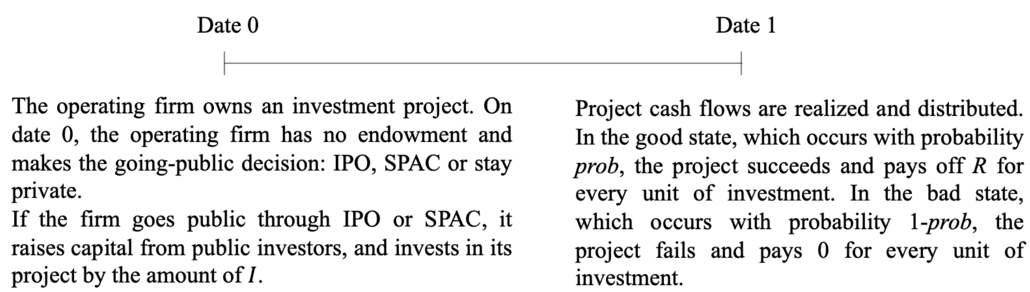
3.2.1 Players and Assumptions

There are five types of players: operating firms, public market investors, investment banks, SPAC sponsors, and a social planner.

Operating Firms: The operating firms are of measure 1. Each firm owns an investment project. The model has two dates and no discounting. Figure 3.2 depicts the model timeline. On date 0, the operating firm has no endowment and makes the going-public decision of whether to IPO, SPAC or stay private. If the firm goes public (through IPO or SPAC), it raises capital from public investors, and invests in its project by the amount of I. On date 1, project cash flows are realized and distributed. In the good state, which occurs with probability $prob$, the project succeeds and pays off R for every unit of investment. In the bad state, which occurs with probability $1 - prob$, the project fails and pays 0 for every unit of investment at date 0. The expected net return of per-unit firm investment is $ER = (prob * R) - 1$.

Figure 3.2: Model Timeline

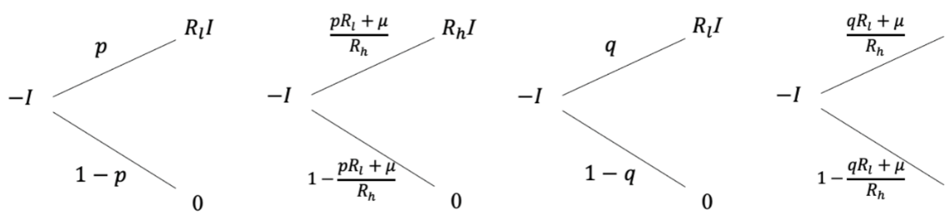
This figure summarizes the sequence of events of the model. See Section 2 of the paper for the full model description.



Operating firms differ along two dimensions: quality and riskiness. Along the quality dimension, there are good firms and bad firms. Good firms have higher expected returns than bad firms, and only good firms are creditworthy (able to repay capital through an NPV-positive project). Along the riskiness dimension, there are safe firms and risky firms. Compared to safe firms, risky firms have a higher return in the good state but a lower probability of success. The per-unit investment return in the good state is rate R_h for risky firms and rate R_l for safe firms, where $R_h > R_l$. The probability that a Good/Safe (G/S) firm succeeds equals $prob_{G/S} = p$ and the probability that a Bad/Safe (B/S) firm succeeds equals $prob_{B/S} = q$, where $p > q$. Meanwhile, the probability that a Good/Risky (G/R) firm succeeds is $prob_{G/R} = \frac{pR_l + \mu}{R_h}$ and the probability that a Bad/Risky (B/R) firm succeeds is $prob_{B/R} = \frac{qR_l + \mu}{R_h}$. The per-unit net expected return for risky projects has a risk premium ("yield") component μ relative to safe projects. $ER_{G/R} = pR_l + \mu - 1 > 0$; $ER_{G/S} = pR_l - 1 > 0$; $ER_{B/R} = qR_l + \mu - 1 < 0$; $ER_{B/S} = qR_l - 1 < 0$. Note that we do not restrict the sign of the risk premium component μ, which can be positive, zero, or negative. Figure 3.3 depicts the project payoffs for each type of operating firm, conditional on going public.

Figure 3.3: Project Payoffs

This figure summarizes the firm project payoffs conditional on the operating firm going public. See Section 2 of the paper for the full model description.



Firm quality is the private information of firms. Quality can be thought of as related to managerial ability. Firm riskiness is common knowledge. Riskiness can be thought of as related to the attributes of the investment project. The proportion of good firms in the economy is α, while the proportion of bad firms in the economy is $1 - \alpha$. The proportion of safe firms in the economy is β, while the proportion of risky firms in the economy is $1 - \beta$. It is assumed that there are $\alpha\beta$ G/S firms, $\alpha(1 - \beta)$ G/R firms, $(1 - \alpha)\beta$ B/S firms, and $(1 - \alpha)(1 - \beta)$ B/R firms. The firm quality composition α is an exogenous variable, which can be thought of as related to underlying macroeconomic factors that lie outside the model. The firm riskiness composition β is endogenously determined by the composition of public market investors, capturing the demand effect of public investors, discussed below.

Public market investors: The public capital market is competitive. Public investors have a total supply of capital S. Investors can invest in going-public firms or stay out of the going-public market entirely.

There are two types of public market investors: safety-seeking investors and yield-seeking investors. All investors prefer good firms over bad firms. Under symmetric information, investors would only invest in good firms. Proportion π of investors are safety-seeking and choose to invest only in safe projects. Proportion $1 - \pi$ of investors are yield-seeking. These investors invest in both safe and risky projects but prefer risky projects over safe projects. One psychological foundation for these yield-seeking investors is salience-thinking investors who overweight the salient high return in the good state (Bordalo *et al.*, 2012). Another economic foundation is the sustainable spending constraint faced explicitly or implicitly by certain investors (Campbell and Sigalov, Forthcoming). A higher $1 - \pi$ captures a greater appetite for risk-taking in the economy. We assume $1 - \beta = x(1 - \pi)$ where $x > 0$, i.e. the fraction of risky firms in the market increases in the proportion of yield-seeking investors and x measures the sensitivity of risky firms' market share to yield-seeking investors' share.

There are two types of financial intermediaries: investment banks (traditional regulated intermediaries) and SPAC sponsors (non-bank intermediaries). Investment banks and SPAC sponsors own certification technologies that can perfectly detect firm quality.[14] Certification costs are c_{ib} for investment banks and $c_{sponsor}$ for SPAC sponsors.[15] The non-identical certification costs reflect the different due diligence technologies owned by investment banks and SPAC sponsors.

Investment banks: Investment banks charge underwriting fees uI from the going-public operating firm. The IPO literature shows that the majority of the underwriting fee is the 7% spread earned by banks (Chen and Ritter, 2000).[16] With underwriter liability, if the firm project fails, investment banks incur litigation costs vI. Without underwriter liability, investment banks incur no costs if the firm project fails.

Litigation costs are economically substantial. Under Section 11 of the Securities Act,

[14]We assume perfect certification technologies to simplify the analyses. Our main conclusions are not affected by incorporating imperfect certification technologies.

[15]For simplicity, we assume the certification costs are fixed. The certification costs could be composed of both fixed costs and variable costs. Our entire analysis holds when we add variable costs.

[16]The IPO literature also documents that investment banks may have incentives to transfer value to institutional clients by "leaving money on the table" and capturing soft dollars via commissions paid by institutional investors in excess of direct execution costs (Nimalendran *et al.*, 2007; Goldstein *et al.*, 2011). The soft dollars can be regarded as an additional component of the proportional fee in the model.

investors can file suit against underwriters for a decline in firm value below the offering price due to material misstatements or omissions. Being named in a lawsuit can induce severe reputational damage and financial consequences for investment banks. The litigation cost for underwriters includes not only the direct costs of lawsuits but also the indirect costs of losing market share. Hanley and Hoberg (2012) show that if an IPO firm associated with a lead underwriter is sued under Section 11, the lead underwriter suffers substantial losses in market share in the following year, with an average dollar loss in value of ∼$59 million.

SPAC sponsors: SPAC sponsors receive founder share $frac$ of the going-public operating firm as compensation.[17] Their payoffs are proportional to the operating firm cash flow realized at date 1.[18] In the model, we assume SPAC sponsors do not incur underwriter liability. Therefore, they incur no costs if the firm project fails. Klausner *et al.* (2022) find that from 2009 to 2019, there were no Section 11 suits against SPACs based on their IPOs, and only two Section 11 cases against post-merger SPACs. Note, however, that SPAC sponsors are not completely immune from litigation risk. They face liabilities under Section 14(a), which covers misstatements and omissions in the proxy statement (requiring proof of negligence), and Section 10(b), which covers misstatements and omissions made with scienter. Therefore, our assumptions of litigation costs are in relative terms; SPAC sponsors face lower expected litigation costs compared to investment banks in traditional IPOs.

We assume $pR_l - 1 > u, pR_l + \mu - 1 > u$, so that G/S and G/R firms have incentives to purchase certification technologies. We assume $\alpha u I > c_{ib}$ and $frac(pR_l + \mu) > c_{sponsor}$, so that investment banks and SPAC sponsors earn a profit when they truthfully certify firm

[17]Because of its similar economic functions to stocks, for simplicity, we do not include warrants (the right to buy shares at a pre-specified price) in our model. SPAC founder warrant compensation may further incentivize SPAC sponsors to certify firm quality as their investment in warrants serves as at-risk capital (increased "skin in the game"). Public investor warrants can be thought of as an inducement to attract investment and a more aggressive yield-seeking mechanism, as warrants are even riskier than stocks.

[18]Halbhuber (2022) shows that IPO rules governing intermediaries in the IPO market limit the amount of compensation underwriters can earn relative to the amount of capital raised in the IPO, while SPAC sponsors are not subject to these limits and are therefore able to charge a multiple of those amounts. Specifically, FINRA RULE 5110(a)(1) requires that underwriting compensation must not be "unfair or unreasonable," which is generally understood to impose a cap of 10% of cash proceeds raised.

types, and the separating equilibrium is feasible.

We abstract from agency issues of SPAC sponsors in order to focus on the origin and economic role of the SPAC market. We discuss the evolution of the U.S. SPAC structure, which has helped to mitigate potential agency issues, and provide suggestions for further incentive alignments with regard to sponsor compensation practices in Section 3.6.

Social planner: The social planner imposes regulations in the going-public market and enforces legal procedures. There are two objectives of the social planner: maximize social welfare and protect public investors. Social welfare (W) is the aggregate welfare of market participants.

In the following subsection 2.2, we first introduce a benchmark where there is only the IPO market. Then, in subsection 2.3, we add the SPAC market, where the SPAC sponsors act as non-bank intermediaries.

3.2.2 Baseline Scenario with the IPO Market Only

Symmetric Information: First-Best

We first consider a benchmark scenario without asymmetric information, in which both firm quality and riskiness are observable by public investors. G/S firms and G/R firms go public and raise capital. Yield-seeking investors invest in G/R firms, while safety-seeking investors and the remaining yield-seeking investors invest in G/S firms. Bad firms stay private because they would generate negative returns. Capital allocation is efficient. There is no demand for the SPAC market or for certification intermediaries.

Because the public market is competitive, G/S firms secure the highest possible level of payoff $V^{G/S}$, consistent with investors breaking even on average: $V^{G/S} = (pR_l - 1)I$. Similarly, G/R firms secure the highest possible level of payoff $V^{G/R}$, consistent with investors breaking even on average: $V^{G/R} = (pR_l + \mu - 1)I = V^{G/S} + \mu I$. The payoffs for B/S firms and B/R firms are $V^{B/S} = V^{B/R} = 0$. When there is no going-public market, social welfare is $W_{np} = 0$.

In this baseline scenario with symmetric information, there would exist a separating equilibrium for safe firms and for risky firms. When public investors know firm types in the market, good firms (G/R and G/S) go public and raise capital, and bad firms (B/R and B/S) remain private. Capital allocation is efficient. Yield-seeking public investors invest in G/R firms to reach for yield; safety-seeking and the remaining yield-seeking public investors invest in G/S firms. There is no demand for the SPAC market or for certification intermediaries. Social welfare is maximized. $W_{si} = \alpha\beta(pR_l - 1)I + \alpha(1-\beta)(pR_l + \mu - 1)I > W_{np}$. The social planner does not intervene in the going-public market.

Asymmetric Information without Certification Intermediaries

Financial markets are characterized by asymmetric information between buyers and sellers; firms often have information about themselves that is unobservable to investors (Leland and Pyle, 1977; Myers and Majluf, 1984). We now incorporate information asymmetry between firms and public investors into the model, so that only the firm knows its project quality, while firm project riskiness is common knowledge. Let us examine the scenario where certification intermediaries do not exist.

First, consider the case of a safe firm approaching the going-public market. B/S firms have the incentive to mimic G/S firms and thereby obtain greater payoffs than they would receive if they were to reveal their types. Since public investors cannot distinguish between good and bad firms, B/S and G/S firms will be valued at the same price. Similar to the classic "lemon" market result of Akerlof (1970), the equilibrium outcome of the going-public market depends on the value of the prior average probability of success $m^{Safe} = \alpha p + (1-\alpha) q$. Defining m^{Safe*} as the critical threshold value for which G/S firms would be indifferent between pooling with B/S firms and staying private $V_{pool}^{Safe} = V_{private}^{Safe}$, we obtain the critical value $m^{Safe*} = \frac{1}{R_l}$.

If $m^{Safe} < m^{Safe*}$, G/S firms prefer to stay private over pooling with B/S firms. B/S firms cannot go public and raise capital without pooling with G/S firms, as they would receive a negative payoff, and investors would lose money. The market is in the breakdown equilibrium. No safe firms go public. There is underinvestment. G/S firms are hurt by the suspicion that they may be B/S firms. Their payoff is lower than the payoff they would obtain in the symmetric information scenario $V_{private}^{Safe} < V^{G/S}$.

If $m^{Safe} \geq m^{Safe*}$, G/S firms are willing to pool with B/S firms rather than stay private. Both G/S firms and B/S firms go public and raise capital. The market is in the pooling equilibrium. Adverse selection thus reduces the average quality of going-public firms. There is overinvestment because of the entry of bad firms. All firms are priced the same. Firms' payoffs are set so that public investors break even on average, $V_{pool}^{Safe} = [m^{Safe} R_l - 1] I$. Investors make money on the G/S firms and lose money on the B/S firms. There is cross-subsidization. G/S firms are still hurt by the presence of B/S firms but to a lesser extent than when the market breaks down. Their payoff is lower than the payoff they would obtain in the symmetric information scenario $V_{pool}^{Safe} < V^{G/S}$ but is higher than the payoff they would obtain in the breakdown equilibrium $V_{pool}^{Safe} > V_{private}^{Safe}$.

The same results apply for risky firms, except that risky firms raise capital only from yield-seeking investors, while safe firms raise capital from both safety-seeking investors and yield-seeking investors. The prior average probability of success for risky firms is: $m^{Risky} = \alpha \frac{pR_l + \mu}{R_h} + (1 - \alpha) \frac{qR_l + \mu}{R_h}$. In the pooling equilibrium, the payoff for G/R firms is $V_{pool}^{G/R} = \left(m^{Risky} R_h - 1\right) I$. In the breakdown equilibrium, the payoff for G/R firms is $V_{private}^{G/R} = 0$. The critical value of m^{Risky} at which G/R firms would be indifferent between pooling with B/R firms and staying private is: $m^{Risky*} = \frac{1}{R_h}$.

In this scenario with asymmetric information and without certification intermediaries, there exists either a pooling or a breakdown equilibrium for safe firms, and there exists a pooling or breakdown equilibrium for risky firms. If m^{Safe} (m^{Risky}) surpasses critical value m^{Safe*} (m^{Risky*}), then all safe (risky) firms will go public and raise capital from safety-seeking (yield-seeking) public investors. Else if m^{Safe} (m^{Risky}) falls below critical value m^{Safe*} (m^{Risky*}), then all safe (risky) firms will remain private, such that the safe (risky) firm going-public market breaks down.

IPO-only Market without Underwriter Liability: Myopic Underwriters

We now introduce investment banks who own certification technologies to detect firm quality but do not incur legal liabilities if the firm fails. G/R firms and G/S firms have incentives to purchase certification services. However, the classic agency issues come into play: investment banks should act on the interest of both good firms and public investors by producing and revealing information on the operating firms, but they can choose to shirk their certification role to maximize their own profits by taking bad firms into the going-public market.

Let us focus on safe firms as an example to characterize market equilibrium. First, consider the scenario where $m^{Safe} \geq m^{Safe*}$. When investment banks exert effort and truthfully reveal firm quality, the safe firm going-public market is in the separating equilibrium and the payoff for investment banks is $\alpha\beta uI - \alpha\beta c_{ib}$. When investment banks shirk their certification role, the safe firm going-public market is in the pooling equilibrium, and the payoff for investment banks is $\beta uI > \alpha\beta uI - \alpha\beta c_{ib}$. The gain in the payoff is a result of both fees accrued from bad firms and reduced certification costs from effort shirking. As a result, investment banks shirk their certification role.

Now consider the scenario where $m^{Safe} < m^{Safe*}$. When investment banks exert effort and truthfully reveal firm quality, the safe firm going-public market is in the separating equilibrium, and the payoff for investment banks is $\alpha\beta uI - \alpha\beta c_{ib}$. When investment banks shirk their certification role, the going-public market is in the breakdown equilibrium, and the payoff for investment banks is 0. However, investment banks have another strategy. They can exert effort to identify firm quality without revealing all information to public investors. They cut the proportion of B/S firms in the going-public market from $(1-\alpha)$ to γ^{Safe*} such that the safe firm going-public market is in the pooling equilibrium. Set the new prior probability of success $m^{Safe} = m^{Safe*}$, i.e., $\alpha p + \gamma^{Safe*} q = \frac{1}{R_l}$, where we can obtain $\gamma^{Safe*} = \frac{1}{qR_l} - \frac{\alpha p}{q}$. The payoff for investment banks is $\left(\alpha\beta + \gamma^{Safe*}\beta\right) uI - \beta c_{ib}$. When $\left(\alpha\beta + \gamma^{Safe*}\beta\right) uI - \beta c_{ib} \leq \alpha\beta uI - \alpha\beta c_{ib}$, i.e., $\gamma^{Safe*} \leq \frac{c_{ib}}{uI}(1-\alpha)$, investment banks exert effort and truthfully reveal firm quality. However, when $\gamma^{Safe*} > \frac{c_{ib}}{uI}(1-\alpha)$, investment banks exert effort to detect firm quality but do not reveal the information, and the safe firm going-public market is in the pooling equilibrium. The higher payoff of investment banks in the pooling equilibrium is attained by collecting fees from bad firms. The same conclusion also applies to risky firms.

Therefore, in this scenario with asymmetric information and without underwriter liability in the IPO market, under the conditions outlined above, investment banks will be incentivized to shirk effort or conceal bad information to collect more fees, leading bad firms to enter the going-public market and creating a pooling equilibrium. A social planner would create value through regulation that punishes myopic underwriters.

IPO-only Market with Underwriter Liability: Long-Term Underwriters

The social planner can improve social welfare and protect public investors by punishing myopic investment banks. The goal is to align investment banks' interests with public investors. Ideally, the first-best policy is to punish effort-shirking and information-concealing. However, the first-best policies are not feasible because these myopic behaviors are unobservable. The second-best policy is to punish investment banks conditional on firm failure, as the probability of firm failure is an increasing function of investment banks' myopic behaviors.

To protect investors, the U.S. Securities Act of 1933 imposes underwriter liability on investment banks, overseen by the SEC. In the model, we assume that with underwriter liability, if the firm project fails, investment banks incur litigation costs v. As a result, investment banks' expected compensation decreases operating firms' probability of failure. Thus, investment banks have strong incentives to reduce expected litigation costs by exerting effort in producing and truthfully providing information.

We repeat the analysis of investment banks' incentives in the previous subsection, now incorporating litigation costs conditional on firm failure. The two conditions that investment banks exert effort and certify firm quality are (1) the payoff for investment banks in the separating equilibrium is higher than the one in the pooling equilibrium, and (2) the payoff for investment banks in the separating equilibrium is positive. These two conditions impose constraints on the range of litigation costs. However, as we show in Proposition 1, the litigation costs could cause an unintended consequence: the incomplete going-public market.

PROPOSITION 1: IPO-only Market with Underwriter Liability

1. *Alleviated agency issues in the safe firm going-public market:*

   The litigation costs alleviate agency issues of investment banks in the safe firm going-public market. There is a range of v such that investment banks certify firm quality truthfully. When $m^{Safe} \geq m^{Safe*}$, the range of v is $\frac{(1-\alpha)uI+\alpha c_{ib}}{(1-\alpha)(1-q)I} \leq v < \frac{uI-c_{ib}}{(1-p)I}$. When $m^{Safe} < m^{Safe*}$, the range of v is $\frac{\gamma^{Safe*}uI-(1-\alpha)c_{ib}}{(1-\alpha)(1-q)I} \leq v < \frac{uI-c_{ib}}{(1-p)I}$. Investment banks certify safe firm quality. The safe firm going-public market is in the separating equilibrium.

2. *The incomplete going-public market:*

   However, in the risky firm going-public market, when the probability of success for G/R firms is lower than a critical value: $\frac{pR_l+\mu}{R_h} < 1 - \frac{u}{v} + \frac{c_{ib}}{vI}$, investment banks do not take G/R firms public. Only G/S firms go public and raise capital. Safety-seeking and yield-seeking public investors invest in G/S firms. Social welfare is $W_{ai,safeseparate,riskybreak} = \alpha\beta[(pR_l - 1)I - c_{ib}] < W_{si}$.

Proof: See Appendix A.1.

Proposition 1 shows that the litigation cost v has two consequences. First, it incentivizes investment banks to certify firm quality and truthfully reveal the information in the safe firm going-public market. However, the unintended consequence is that the litigation cost makes investment banks downside-averse and may deter them from taking risky firms public when these firms have a probability of success that is lower than a critical value. Value-creating risky firms may not go public through the traditional IPO market, even if they have positive expected returns. Yield-seeking investors cannot reach for yield.

Proposition 1 also shows that there is a size effect because of the fixed-proportion fee structure in the IPO market and the fixed certification costs incurred by investment banks. Investment banks do not take G/R firms public when $I < \frac{c_{ib}}{u-\left(1-\frac{pR_l+\mu}{R_h}\right)v}$.[19] Therefore, investment banks are especially deterred from underwriting small firms.

3.2.3 IPO + SPAC Market: Reaching for Yield in the Going-Public Market

In the SPAC market, SPAC sponsors raise capital from public investors, identify an operating firm, then accomplish a reverse merger acquisition to inject the capital raised into the operating firm and take the operating firm public. Since the acquisition is not an underwriting process, the SPAC market bypasses underwriter liability. However, with SPAC sponsors' certification technologies, the SPAC market retains certification intermediaries by transferring the certification role from underwriters to SPAC sponsors.

In the model, we assume SPAC sponsors take $frac$ founder share of the going-public operating firm as compensation. The expected payoff of SPAC sponsors increases in the expected payoff of the operating firms. This compensation structure in the SPAC market makes it so that the payoff of SPAC sponsors is contingent upon firm success, thereby aligning SPAC sponsors' interests with public investors. The equity-like compensation structure incentivizes SPAC sponsors to certify firm types and truthfully reveal the information.[20] It also gives SPAC sponsors incentives to take small firms public because they are compensated by the upside of the firms.

LEMMA 1: The incentive-compatible range of the SPAC founder share satisfies:

$$\frac{c_{sponsor}}{(pR_l+\mu)I} \leq frac \leq 1 - \frac{1}{pR_l+\mu}$$

Consistent with investors breaking even on average, the expected payoff for SPAC sponsors is $frac(pR_l+\mu)I$*, and the expected payoff for G/R firms is* $(pR_l+\mu-1)\,I - frac(pR_l+\mu)I$*. There are two conditions for the SPAC market to function. The first is the incentive compatibility constraint*

[19] Even for G/S firms, if they are small, investment banks will also not take these firms public.

[20] In the model, the payoffs of SPAC sponsors are realized when firm cash flows realize, which aligns their interests with long-term investors. In reality, imperfect compensation structures may induce myopic behavior from SPAC sponsors; we discuss these frictions and potential solutions in Section 5.

for SPAC sponsors. Their expected payoff must be higher than certification costs: $frac(pR_l + \mu)I \geq c_{sponsor}$*. The second is the incentive compatibility constraint for G/R firms. Their expected payoff must be higher than their payoff when they stay private:* $(pR_l + \mu - 1)\, I - frac(pR_l + \mu)I \geq 0$*. These restrictions imply the range of the founder share fraction:* $\frac{c_{sponsor}}{(pR_l+\mu)I} \leq frac \leq 1 - \frac{1}{pR_l+\mu}$*.*

If SPAC sponsors are monopolies, $frac = 1 - \frac{1}{pR_l+\mu}$. If the SPAC sponsor industry is perfectly competitive, $frac = \frac{c_{sponsor}}{(pR_l+\mu)I}$. In reality, the convention of the 20% SPAC founder share practice potentially suggests that SPAC sponsors have at least some market power and reflects their informational rents. The almost uniform 20% SPAC founder share structure is similar to the nearly uniform 7% underwriting fee structure in the IPO market.[21]

In Figure 3.4, we present comparative statics to illustrate how different model parameters affect the range of the SPAC founder share fraction. The figure plots the range of the SPAC founder share as a function of the certification costs $c_{sponsor}$, the quality of G/R firms $pR_l + \mu$, and the size of G/R firms I. Panel A shows that the lower bound of the founder share fraction increases in the certification cost, and Panel B shows that the range of the founder share fraction widens as the quality of G/R firms increases. Panel C shows that the lower bound of the founder share fraction decreases in the size of G/R firms. The figure also shows that the model-implied upper and lower bounds dovetail around the conventional 20% SPAC founder share fraction.

PROPOSITION 2: IPO Market with Underwriter Liability + SPAC Market

Segmented going-public market: Given LEMMA 1 holds, SPAC sponsors certify G/R firms in the SPAC market and G/R firms go public and raise capital from yield-seeking public investors. Investment banks certify the safe firm quality in the IPO market and G/S firms go public and raise capital from safety-seeking and yield-seeking public investors. Bad firms remain private. Yield-seeking investors can reach for yield. The going-public market is complete. Capital allocation is efficient.

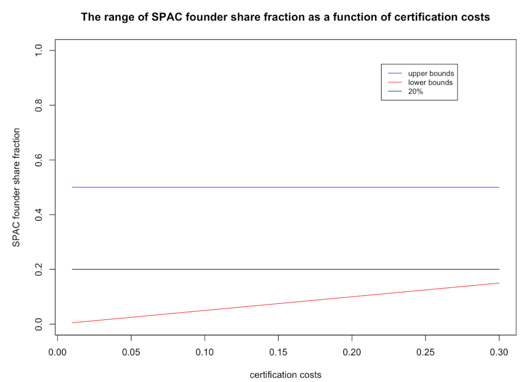
[21]The IPO literature suggests two explanations for the uniform 7% underwriting fee: strategic pricing of investment banks (e.g., Chen and Ritter (2000), as well as non-price competition of investment banks (e.g., Hansen (2001)).

Social welfare is $W_{ai,allseparate} = \alpha\beta[(pR_l - 1)I - c_{ib}] + \alpha\,(1-\beta)\,[(pR_l + \mu - 1)\,I - c_{sponsor}]$.

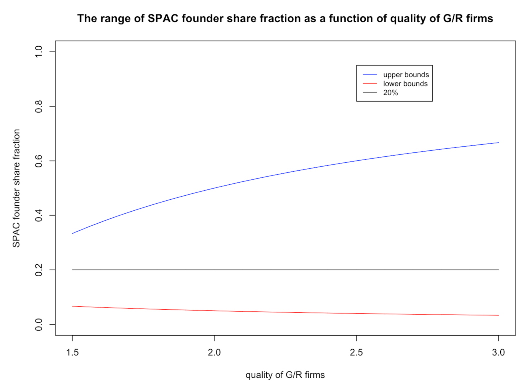
Figure 3.4: Comparative Statics

This figure plots the range of the SPAC founder share $\frac{c_{sponsor}}{(pR_l+\mu)I} \leq frac \leq 1 - \frac{1}{pR_l+\mu}$ (LEMMA 1) as a function of the certification cost $c_{sponsor}$ (Panel A), the quality of G/R firms $pR_l + \mu$ (Panel B), and firm size I (Panel C). The benchmark parameters are set to $c_{sponsor} = 0.1; pR_l + \mu = 2; I = 1$. See Section 2 of the paper for the full model description.

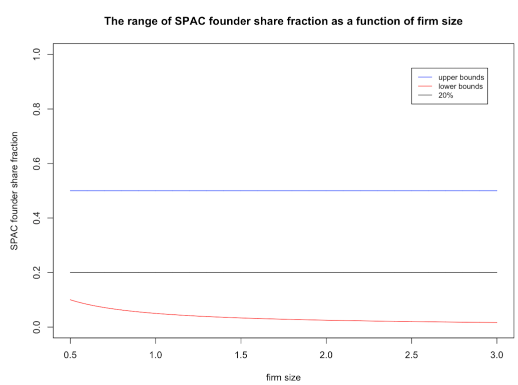
(a) Certification cost



(b) Quality of operating firm



(c) Firm size



Proposition 2 shows that, under the condition of Lemma 1, the existence of the SPAC market completes the going-public market and solves this inefficiency of capital allocation. SPAC sponsors play important certification roles. In equilibrium, investment banks take safer firms public in the IPO market, and SPAC sponsors take riskier firms public in the SPAC market. Bad firms stay private. Yield-seeking investors have the opportunity to reach for yield.

Why is it that SPACs do not obtain full market share? The relatively high costs of merging with a SPAC provide one explanation. We assume that the equilibrium founder share fraction of the SPAC sponsor satisfies the condition for G/S firms to go public through the IPO market rather than the SPAC market, which requires that the cost of going public through SPAC for G/S firms, $frac(pR_l I)$, is higher than the cost of going public by IPO, uI. This implies $frac > \frac{u}{pR_l}$. This condition is easily satisfied in practice given the 20% SPAC founder share of public equity and 7% IPO underwriting fee charged by investment banks; $pR_l > 1$ for all G/S firms by definition. We treat the equilibrium founder share fraction as exogenous as there are very few deviations from the 20% share in reality, which suggests that increased competition has not driven down the SPAC founder share fraction. The structure may be an optimal design that has evolved as an efficient response to the market environment, or a persistent form of rent extraction by SPACs that is costly for both firms and investors.

COROLLARY 1: *When the proportion of yield-seeking investors increases, (1) there are more SPAC IPOs; (2) there is a higher market share of SPAC IPOs.*

Corollary 1 states that when the investor composition tilts towards yield-seeking, the demand for G/R firms increases and SPAC activities increase, both in terms of the absolute number of SPACs and the share of SPAC IPOs out of total IPOs. For simplicity, we do not explicitly model reaching-for-yield in the private market. When reaching-for-yield co-moves in the public and private market, it increases both the supply and demand of G/R firms in the going-public market, which amplifies our results.

There are three central predictions from our model: First, in the cross-section, larger, safer, and older firms go public in the IPO market, while smaller, riskier, and younger firms go public in the SPAC market. Second, in the time series, time-varying investor preferences for riskier firms affect SPAC volume. Third, an increase in the litigation risk of traditional IPOs relative to SPACs will shift activity towards SPACs. In the following section, we show that these predictions are borne out in the data.

3.3 Data and Empirical Facts from U.S. SPACs

In this section, we assess the first two empirical predictions generated by the framework of the previous section. Specifically, we analyze the cross-sectional patterns of SPAC and IPO going-public firms, as well as the evolution of SPAC formation over time relative to IPOs. We link U.S. SPACs that completed a business combination between 2003 Q2 and 2020 Q4 to their target companies and firm-level fundamentals data. We then analyze the characteristics of SPAC operating firms in comparison to traditional IPO operating firms.

3.3.1 Data

We obtain data on U.S. SPACs from commercial provider SPAC Analytics. While sources like S&P Capital IQ and Bloomberg are helpful for identifying SPACs, they typically do not provide more detailed information on the features of the SPAC such as the number of warrants per unit in the offering or the sponsors' at-risk capital relative to the total raise. SPAC Analytics provides a timeline of key dates in the SPAC's lifecycle, as well as details of the reverse merger transaction, including valuations of the target company and purchase price. This information is sourced from publicly available Securities and Exchange Commission (SEC) filings, investor presentations, and detailed press releases. Since SPACs first raise capital via an IPO, they are required to file documentation with the SEC, which can be accessed via the Electronic Data Gathering and Retrieval (EDGAR) database. Information on shareholder voting thresholds and other structural details of each SPAC are reported in these public reports, particularly in quarterly and annual reports, securities registration statements, and prospectus filings. For example, voting threshold data can be found in the SEC's S-1/F-1 registration filings, and data on whether the SPAC acquisition is approved or denied by shareholders can be found from 10-K, 6-K, and 8-K filings.

We augment the SPAC Analytics dataset in a few ways. First, we collect the Central Index Key (CIK) for each SPAC in order to access the complete text of each SPAC's SEC filings. Next, we collect the Global Company Key (GVKEY) for each SPAC in order to connect to Compustat fundamentals data. For some SPACs, there are separate GVKEYs for the blank check company versus the post-reverse-merger operating company, in which case we make sure to gather all GVKEYs to cover the full lifetime of the SPAC. Finally, we manually collect the founding dates of SPAC operating companies by following an approach consistent with Loughran and Ritter (2004)'s method for gathering the founding dates of traditional IPO firms. We hand-collect target company founding years from company websites, Crunchbase profiles, press releases, and SEC reports. Age of SPAC operating firm is defined as calendar year of merger approval date minus calendar year of founding date, while age of IPO firm is defined as calendar year of issue date minus calendar year of founding date. For some SPACs, there are multiple target operating companies, in which case we make sure to gather all founding dates for all targets. We present results for the equal-weighted average target age, the minimum target age, and the maximum target age, to address these cases.

Of the 636 SPACs that went public and raised capital from 2003 to 2020, 123 had liquidated, 455 had completed an acquisition, 55 were seeking or pending an acquisition, and 2 were pending liquidation as of November 2022. Of the 455 SPACs that completed an acquisition, we ultimately observe annual fundamentals data for 391 SPAC operating companies. This narrowing of sample occurs because some SPAC operating companies do not end up existing long enough to report any annual fundamentals data (e.g., because they get acquired) and because we drop SPACs for which the time from merger approval to first Compustat annual fundamentals report exceeds two years. Of these 391 SPAC operating companies, 58 trade on over-the-counter (OTC) markets.[22] Both OTC and non-OTC SPAC operating firms are included in our analysis.

Next, we gather data on U.S. Initial Public Offerings (IPOs) from Thomson Financial's New Issues database. We start with all common stock offerings and drop offerings prior to 2003, secondary offerings, and IPOs that were withdrawn, rejected, or postponed. We also exclude IPO filings of financial firms (those with Standard Industrial Classification (SIC) codes between 6000 and 6999), unit offers, American depositary receipts (ADRs), and offers with warrants. Finally, we drop offerings with missing or zero global proceeds across all markets, as well as those with missing offer prices. The remaining IPOs exclude any initial capital raises of SPACs or other similar offerings, thus serving as a comparison group for the target firms that are acquired by acquisition vehicles. The mean and median proceeds across all markets of these IPOs are $249.2 million and $110.4 million, respectively.

Of the 2,479 traditional IPOs from 2003 to 2020, we ultimately observe annual fundamentals data for 2,259 traditional IPO firms. This narrowing of sample occurs because some traditional IPO firms do not end up existing long enough to report any annual fundamentals data (e.g., because they get acquired) and because we remove traditional IPO firms for which the time from issue to the first Compustat annual fundamentals report exceeds two years. Of these 2,259 traditional IPO firms, 214 trade on over-the-counter markets. Both OTC and non-OTC traditional IPO firms are included in our analysis. To obtain founding dates for traditional IPO firms, we use the Field-Ritter dataset of company founding dates as used by Loughran and Ritter (2004).

We obtain information on the fundamentals of the SPAC target companies and traditional IPO firms from the Compustat annual fundamentals dataset and present summary statistics in Table 3.1. In order to best measure the characteristics of firms around the time they go public, the summary statistics are calculated using only the first four years of data in Compustat for each firm. Values are winsorized at the 1st and 99th percentiles. The

[22]A previous version of this paper used the CRSP/Compustat Merged (CCM) fundamentals dataset, which excludes firms that trade over-the-counter as a result of the merge with CRSP.

table shows that SPAC firms are, on average, smaller than IPO firms in terms of market capitalization, assets, and revenue. Specifically, the average market capitalization of IPO and SPAC firms are \$1.90 billion and \$1.02 billion, respectively. The average total assets of IPO and SPAC firms are \$1.15 billion and \$1.10 billion, respectively. The average revenue of IPO and SPAC firms are \$798.4 million and \$350.8 million, respectively. On average, SPAC firms also have lower cash-to-asset ratios, lower Tobin's Q, and lower R&D to asset ratios than IPO firms around the time of going public.

Table 3.2, Panels A and B compare the growth rates of total assets, market capitalization, revenue, and R&D for IPO and SPAC firms, using the same sample restrictions as Table 3.1. While Table 3.1 shows that SPAC firms tend to be smaller than IPO firms, Table 3.2 shows that the mean growth rates of market capitalization and revenue are higher for SPAC firms. The third column shows that the standard deviations of nearly all of these growth measures are higher for SPAC firms as well. Taken together, the summary statistics suggest that companies that choose to go public via SPAC tend to be smaller, riskier, and higher-growth companies than companies that go public through a traditional public offering.

Table 3.1: Summary Statistics for IPO Firms and SPAC Target Firms

This table presents summary statistics of firm characteristics. Each observation is a firm-year, where only the first four annual reports after the public offering are included. Panel A reports summary statistics for firms that go public through an IPO. Panel B reports summary statistics for firms that go public through a SPAC. Fiscal years with zero revenue are excluded for metrics with revenue in the denominator. Values are winsorized at the 1st and 99th percentiles. See Section 3.1 for a full description of the SPAC and IPO samples.

Using first four annual reports after IPO				
Panel A: IPO Firms				
	N	Mean	Median	Std. Dev.
Market Cap ($m)	7,033	1,904.97	516.68	7,887.84
Total Assets ($m)	7,074	1,151.82	262.49	4,760.98
Revenue ($m)	7,073	798.36	142.46	4,238.04
Cash Flow / Assets	7,031	-0.17	-0.01	0.45
Cash / Assets	7,041	0.30	0.20	0.28
Debt / Assets	7,033	0.38	0.10	8.55
Net Income / Assets	7,051	-0.21	-0.05	0.45
Net Income / Revenue	6,247	-7.86	-0.03	40.53
R&D / Assets	7,073	0.14	0.06	0.20
R&D / Revenue	6,268	4.10	0.06	20.21
CapEx / Assets	7,039	0.05	0.02	0.08
Market to Book	7,025	4.58	3.12	9.34
Tobin's Q	7,024	3.20	2.27	2.88
Panel B: SPAC Target Firms				
	N	Mean	Median	Std. Dev.
Market Cap ($m)	858	1,022.85	312.94	3,061.20
Total Assets ($m)	871	1,098.37	334.84	3,198.31
Revenue ($m)	869	350.78	123.76	683.03
Cash Flow / Assets	851	-0.16	-0.01	0.42
Cash / Assets	867	0.21	0.10	0.25
Debt / Assets	867	0.33	0.26	0.41
Net Income / Assets	860	-0.19	-0.04	0.42
Net Income / Revenue	806	-5.96	-0.09	37.59
R&D / Assets	871	0.04	0.00	0.10
R&D / Revenue	815	2.17	0.00	16.22
CapEx / Assets	855	0.05	0.02	0.08
Market to Book	858	2.36	1.58	9.02
Tobin's Q	858	2.30	1.54	2.30

Table 3.2: Growth Summary Statistics of IPO Firms and SPAC Target Firms

This table presents summary statistics of firm growth as measured by asset growth, market capitalization growth, revenue growth, and R&D growth. Each observation is a firm-year, where only the first four annual reports after the public offering are included. Panel A reports summary statistics for firms that go public through an IPO. Panel B reports summary statistics for firms that go public through a SPAC. Values are winsorized at the 1st and 99th percentiles. See Section 3.1 for a full description of the SPAC and IPO samples.

Using first four annual reports after IPO				
Panel A: IPO Firms				
	N	Mean	Median	Std. Dev.
1 Year Asset Growth	5,949	0.20	0.09	0.54
2 Year Asset Growth	5,021	0.46	0.20	0.97
3 Year Asset Growth	4,239	0.76	0.35	1.52
1 Year Market Cap Growth	5,901	0.22	0.01	0.95
2 Year Market Cap Growth	4,974	0.54	0.06	1.77
3 Year Market Cap Growth	4,190	0.80	0.10	2.27
1 Year Revenue Growth	5,338	0.52	0.16	1.98
2 Year Revenue Growth	4,572	1.10	0.29	4.29
3 Year Revenue Growth	3,903	1.93	0.42	8.18
Panel B: SPAC Target Firms				
	N	Mean	Median	Std. Dev.
1 Year Asset Growth	579	0.21	0.05	0.66
2 Year Asset Growth	436	0.33	0.06	1.05
3 Year Asset Growth	351	0.49	0.11	1.60
1 Year Market Cap Growth	560	0.20	-0.06	1.10
2 Year Market Cap Growth	416	0.47	-0.13	1.89
3 Year Market Cap Growth	335	0.60	-0.15	2.47
1 Year Revenue Growth	557	0.74	0.13	2.68
2 Year Revenue Growth	424	1.15	0.19	4.89
3 Year Revenue Growth	342	1.93	0.21	9.19

We obtain data on several sentiment indices in the literature in order to observe the time-varying investor demand for different securities. To gauge the equity market, we use the Baker and Wurgler (2006) composite equity sentiment index, which is based on the first principal component of the following established proxies of equity sentiment: value-weighted dividend premium, first-day IPO returns, IPO volume, closed-end fund discount, and equity share of new corporate issues. Since these proxies may be directly related to systematic risk, we also examine a version of this sentiment index that orthogonalizes each of the five proxies with respect to the following macroeconomic indicators: industrial production growth, real durables consumption growth, real nondurables consumption growth, real services consumption growth, the NBER recession indicator, and employment growth. In order to rule out a mechanical link between the IPO-related proxies and our SPAC variables, we further construct a non-IPO version of the sentiment index using only the 3 non-IPO-related equity sentiment proxies. Finally to gauge the overall economic environment, we use the CBOE Volatility Index, colloquially known as the "fear gauge" of Wall Street.

3.3.2 The Time Series of SPAC Activity

Fact 1: SPAC issuance boomed in 2007 prior to the Global Financial Crisis and accelerated from 2015 to 2020.

We begin by examining the time-series of SPAC activity. Figure 3.1 plots the quarterly counts and total proceeds of SPACs and traditional IPOs from 2003 to 2020. The plot shows the dramatic increase in SPAC activity from 2005 to 2007 and from 2015 to 2020, as measured by number of SPACs formed and total proceeds raised. In 2020 alone, the SPAC market raised $83.4 billion in gross proceeds, approaching the size of the traditional IPO market.

Fact 2: The market share of SPACs is strongly positively correlated with equity market sentiment.

We next turn to the time-series relationship between investor sentiment and SPAC activity. We begin by regressing measures of SPAC activity in quarter t on equity market sentiment in quarter $t-1$. The dependent variables are the number of SPACs, total SPAC proceeds, market share of the number of SPACs, and market share of SPAC proceeds. The date used to measure SPAC activity is the date the SPAC first goes public as a blank check company. Table 3.3 reports summary statistics of the investor sentiment and market volatility measures and the five components of the equity market sentiment index from 2003 to 2020.

Figure 3.5 illustrates the clear co-movement between equity market sentiment and SPAC activity. The regression results reported in Table 3.4 also show that equity market sentiment is strongly positively correlated with SPAC activity. This result is robust to using different variations of the composite sentiment measure. Specifically, Panel A uses the first principal component of the five proxies discussed in Section 3.1. Panel B uses an "orthogonalized" version of the composite measure, which removes business cycle variation from the individual proxies before extracting the principal component. Panel C uses the non-IPO equity market sentiment measure, which removes the two IPO-related proxies prior to the principal component analysis. The results in all three panels show a strong positive association between SPAC formation and lagged equity market sentiment. The robustness of the result to the last measure shows that the IPO-related components of investor demand for equity are not driving the result in Panels A and B.

As the results are similar across panels, we focus attention on Panel B of Table 3.4, which examines equity market sentiment after removing macro variables. The coefficient on equity market sentiment is statistically significant at the 1% level across all columns, each of which examines a different measure of SPAC activity as the outcome variable. Equity sentiment explains 8.0% of the variation in number of SPACs, 5.5% of the variation in SPAC proceeds, 15.2% of the variation in the share of number of SPACs, and 11.4% of the variation in the share of SPAC proceeds in the going-public market.

Table 3.3: Summary Statistics of Quarterly Time-series Measures: 2003-2020

This table presents summary statistics of the quarterly SPAC activity and market sentiment measures. Each observation is a quarter from 2003 Q2 to 2020 Q4. Share of SPAC IPOs is defined as #SPACs/(#SPACs + #IPOs), where #IPOs excludes SPAC issuances. Share of SPAC Proceeds is defined as SPAC Proceeds/(SPAC Proceeds + IPO Proceeds), where IPO Proceeds excludes proceeds from SPAC issuances. The equity market sentiment index is from Baker and Wurgler (2006) and the VIX is the Chicago Board Options Exchange (CBOE) volatility index. The table also reports summary statistics of the individual sentiment proxies of the equity market sentiment index. See Section 3.1 for a full description of the SPAC and IPO samples.

	N	Mean	Median	Std. Dev.
SPAC activity measures				
Number of SPAC IPOs	71	6.35	3.00	11.10
Total SPAC Proceeds	71	1.54	0.42	3.66
Share of SPAC IPOs	71	0.15	0.11	0.14
Share of SPAC Proceeds	71	0.15	0.06	0.18
Market sentiment measures				
Equity market sentiment	71	-0.20	-0.19	0.28
Equity market sentiment (orthogonalized)	71	-0.05	-0.00	0.35
Equity market sentiment (non-IPO)	71	-0.01	-0.02	0.29
VIX at quarter close	71	19.13	16.52	8.62
Equity market sentiment components				
Value-weighted dividend premium	71	-4.54	-5.77	6.26
IPO volume (past 12 months)	71	186.31	196.00	72.79
First-day IPO returns (smoothed)	71	13.61	13.66	3.81
Closed end fund discount	71	7.26	8.66	4.24
Equity share in new issues	71	0.11	0.09	0.05

A one-standard-deviation increase in lagged equity market sentiment is associated with a 5.46 percentage point increase in the quarterly SPAC market share and a 6.16 percentage point increase in the quarterly SPAC proceeds share.[23] These magnitudes represent an increase of 36.4% relative to the mean SPAC market share and 41.1% relative to the mean SPAC proceeds share. As we interpret higher sentiment to denote higher demand for speculative securities from the marginal investor (Baker and Wurgler, 2007), the results

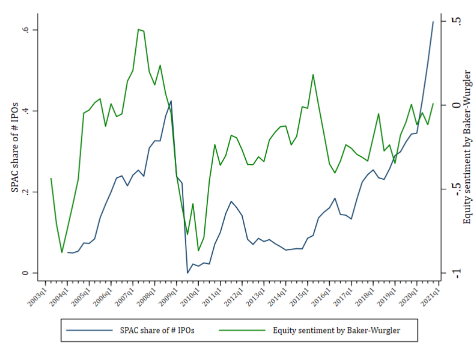
[23]This is found by multiplying the standard deviation of the equity sentiment index (orthogonalized with respect to macroeconomic variables) reported in Table 3.3 (0.35) by the estimated coefficient on orthogonalized equity market sentiment in Column (3) (0.156) and (4) (0.176), respectively.

suggest that a preference for riskier securities in the equity market strongly predicts the formation of SPACs.

Figure 3.5: Equity Market Sentiment and the Market Share of SPACs

This figure displays equity market sentiment (Baker and Wurgler 2006) and the 4-quarter moving average market share of SPACs. Each observation is a quarter from 2003 Q2 to 2020 Q4. In Panel (a), the market share of SPACs is measured as #SPACs/(#SPACs + #IPOs), where #IPOs excludes SPAC issuances. In Panel (b), the market share of SPACs is measured as SPAC Proceeds/(SPAC Proceeds + IPO Proceeds), where IPO Proceeds excludes proceeds from SPAC issuances. See Section 3.1 for a full description of the SPAC and IPO samples.

(a) Equity Sentiment and SPAC Share



(b) Equity Sentiment and SPAC Proceeds Share

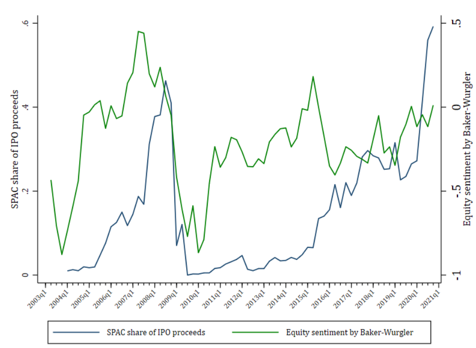


Table 3.4: SPAC Activity and Equity Market Investor Sentiment

This table presents time-series regressions of measures of SPAC activity on lagged equity market sentiment (Baker and Wurgler 2006). Each observation is a quarter from 2003 Q2 to 2020 Q4. SPAC share in Column (3) is defined as #SPACs/(#SPACs + #IPOs), where #IPOs excludes SPAC issuances. SPAC proceeds share in Column (4) is defined as SPAC Proceeds/(SPAC Proceeds + IPO Proceeds), where IPO Proceeds excludes proceeds from SPAC issuances. In Panel B, the equity market sentiment index is orthogonalized with respect to macroeconomic variables. In Panel C, the equity market sentiment index is constructed without the IPO-related measures. See Section 3.1 for a full description of the SPAC and IPO samples, as well as the construction of the equity market sentiment index. Robust standard errors are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	$\#SPACs_t$	Total SPAC $proceeds_t$	SPAC $share_t$	SPAC proceeds $share_t$
		Panel A		
L.eqsent	9.093**	2,376**	0.165***	0.181***
	(3.474)	(1,035)	(0.0505)	(0.0630)
Constant	8.251***	2,033***	0.186***	0.189***
	(1.923)	(620.1)	(0.0227)	(0.0288)
Observations	71	71	71	71
R-squared	0.052	0.033	0.106	0.076
		Panel B		
L.eqsent_orth	8.955***	2,436**	0.156***	0.176***
	(2.933)	(950.0)	(0.0378)	(0.0480)
Constant	6.859***	1,675***	0.160***	0.161***
	(1.422)	(475.4)	(0.0167)	(0.0221)
Observations	71	71	71	71
R-squared	0.080	0.055	0.152	0.114
		Panel C		
L.eqsent_nonipo	6.745**	1,700**	0.131**	0.149**
	(2.706)	(794.7)	(0.0510)	(0.0639)
Constant	6.429***	1,556***	0.153***	0.153***
	(1.326)	(440.0)	(0.0164)	(0.0215)
Observations	71	71	71	71
R-squared	0.032	0.019	0.075	0.057

The regression results reported in Table 3.5 show that overall market sentiment – proxied by the log of VIX at quarter close – has little explanatory power for SPAC activity. SPACs are often described as a means of gaining valuable certainty in deal terms, which would suggest that SPACs are more in demand during times of high volatility. Using the VIX as a concrete proxy for market volatility, however, we do not find any statistically significant relation between market volatility in one quarter and SPAC formation in the next.

Table 3.5: SPAC Activity and Broad Market Sentiment

This table presents time-series regressions of measures of SPAC activity on lagged market volatility, as measured by the VIX. Each observation is a quarter from 2003 Q2 to 2020 Q4. SPAC share in Column (3) is defined as #SPACs/(#SPACs + #IPOs), where #IPOs excludes SPAC issuances. SPAC proceeds share in Column (4) is defined as SPAC Proceeds/(SPAC Proceeds + IPO Proceeds), where IPO Proceeds excludes proceeds from SPAC issuances. See Section 3.1 for a full description of the SPAC and IPO samples. Robust standard errors are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	$\#SPACs_t$	Total SPAC $proceeds_t$	SPAC $share_t$	SPAC proceeds $share_t$
$ln(VIXatquarterclose)_{t-1}$	3.703	1,910	0.00276	-0.0164
	(4.527)	(1,553)	(0.0562)	(0.0620)
Constant	-4.310	-3,964	0.143	0.199
	(11.94)	(4,098)	(0.154)	(0.174)
Observations	71	71	71	71
R-squared	0.015	0.038	0.000	0.001

Fact 3: The time to merger approval for SPACs is strongly negatively correlated with equity market sentiment.

We next turn to the time-series relationship between investor sentiment and SPAC time to merger approval. In Table 3.6, we regress the time between SPAC IPO and target company merger approval (in number of quarters) on the equity market sentiment (orthogonalized with respect to macroeconomic variables) in the year-quarter of the SPAC IPO. Across the specifications in Columns (1) through (4), we find that higher equity sentiment at IPO is associated with more rapid SPAC merger approval (i.e. shorter time between SPAC IPO and merger approval) and that the coefficient on equity market sentiment is statistically significant at the 1% or 5% level. In our preferred specification that controls for SPAC size and includes period fixed effects and industry focus fixed effects (Column (4)), we find that a one-standard-deviation increase in equity market sentiment is associated with a shorter time to merger approval by 0.94 fewer quarters or approximately 85 days.[24]

Together, Facts 2 and 3 can be thought of as covering the extensive margin and intensive margin of SPAC activity. When equity sentiment is high, increased demand for riskier securities results in more SPACs (extensive margin) and the SPAC process happening more rapidly (intensive margin).

[24]This is found by multiplying the standard deviation of the equity sentiment index (orthogonalized with respect to macroeconomic variables) reported in Table 3.3 (0.35) by the estimated coefficient on orthogonalized equity market sentiment in Column (4) (0.943).

Table 3.6: Equity Market Investor Sentiment and Time to Merger Approval

This table investigates the relationship between equity market investor sentiment and the time between the SPAC IPO and approval of the target company merger, measured in number of quarters. The sentiment measure is the Baker and Wurgler (2006) equity market sentiment index (orthogonalized with respect to macroeconomic variables) in the year-quarter of the SPAC IPO. The index is standardized to have mean 0 and standard deviation 1 over the sample period. Time period fixed effects control for the SPAC IPO occurring in the period 2003-2008, 2009-2014, or 2015-2020. See Section 3.1 for a full description of the SPAC and IPO samples. Robust standard errors clustered by year-quarter are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Quarters between SPAC IPO and merger approval			
Equity Sentiment at IPO	-1.042***	-1.020***	-0.939**	-0.943***
	(0.378)	(0.346)	(0.371)	(0.339)
ln(SPAC Proceeds)			-0.330*	-0.282
			(0.194)	(0.218)
Constant	6.754***	6.743***	8.372***	8.129***
	(0.250)	(0.246)	(0.998)	(1.101)
Observations	384	384	384	384
R-squared	0.100	0.145	0.112	0.152
Period FE	Yes	Yes	Yes	Yes
Industry Focus FE	No	Yes	No	Yes

3.3.3 The Cross-Section of Going-Public Firms

Next, we examine the cross-sectional differences between firms that go public through a SPAC and firms that go public through a traditional IPO. Table 3.1 and Table 3.2 present summary statistics. We undertake a regression analysis with fixed effects to address time-invariant heterogeneity across sectors and across the countries of origin of the going-public firms, as well as general variation across different going-public years. In addition to examining relative size, we construct firm-level measures of riskiness and growth to be used as outcome variables in the analysis. Specifically, the specification takes the following form:

$$Y_i = \beta_0 + \beta_1 SPAC_i + \alpha_k + \mu_t + \gamma_c + \epsilon_i$$

where the main independent variable $SPAC_i$ is a binary variable equal to one if the firm goes public through the SPAC market and equal to zero if the firm goes public through the traditional IPO market. All specifications include GICS sector fixed effects (α_k), going-public year fixed effects (μ_t), and country of incorporation fixed effects (γ_c). Standard errors are clustered at the GICS sector level.

Fact 4: Compared to IPO firms, SPAC operating firms are smaller, younger, and riskier at the moment of going public.

Table 3.7 investigates the sizes of SPAC and IPO firms. The table shows that, in terms of assets, revenue, and market capitalization, SPAC firms are significantly smaller than firms that undergo traditional public offerings. Specifically, SPACs have 20.4% lower assets, 48.8% lower revenue, and 45.9% lower market capitalization at the time of going public.[25]

[25]This is computed as 100*(exp(coefficient) - 1).

Table 3.7: Size of SPAC Target Firms v. Traditional IPO Firms

This table investigates differences in assets, revenue, and market capitalization for firms that undergo a traditional IPO versus SPAC target firms. The specification includes GICS sector fixed effects, fixed effects for the going-public year, as well as fixed effects for country of incorporation of the operating firm. Information from only the first annual report of each firm after the IPO year is used. See Section 3.1 for a full description of the SPAC and IPO samples. Robust standard errors clustered by GICs sector are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Using first annual report after IPO		
	(1)	(2)	(3)
	ln(assets)	ln(revenue)	ln(mktcap)
SPAC Indicator	-0.228**	-0.670*	-0.615***
	(0.0818)	(0.302)	(0.116)
Observations	2,641	2,260	2,622
R-squared	0.195	0.258	0.182
GICS sector FE	Yes	Yes	Yes
IPO year FE	Yes	Yes	Yes
Country FE	Yes	Yes	Yes

Table 3.8 examines the age of SPAC and IPO firms at the time of going public. Age of SPAC firms is defined as calendar year of merger approval minus calendar year of founding, and age of IPO firms is defined as calendar year of issue minus calendar year of founding. The outcome variable is the natural logarithm of age, and the three columns in the table differ in how SPACs with multiple targets are treated. The most extreme example of this is cloud software-focused SPAC GTY Technology Holdings Inc., which acquired six software-as-a-service operating companies in 2019 to form one combined entity that would trade on Nasdaq. To handle SPACs like this, the first column uses the average age of all acquired operating companies at the time of going public, the second column uses the age of the youngest company, and the third column uses the age of the oldest company. The table shows that, across columns and even when using the maximum age of the multiple-target SPAC, SPAC targets tend to be younger than traditional IPO firms around the time of the

offer. The most conservative estimated difference shows that SPAC firms are 11.78 years younger than IPO firms. Column (2), which uses the minimum target age of SPACs with multiple targets, implies that SPAC firms are 12.18 years younger than IPO firms at the time of offering.

Table 3.8: Age of SPAC Target Firms v. Traditional IPO Firms

This table investigates differences in age for firms that undergo a traditional IPO versus SPAC target firms. The three columns differ in terms of how SPACs with multiple targets are treated, i.e., we take the mean, minimum, or maximum age of the targets. Age of SPAC firms is defined as calendar year of SPAC merger approval minus calendar year of founding, and age of IPO firms is defined as calendar year of offer minus calendar year of founding. The specification includes GICS sector fixed effects, fixed effects for the going-public year, as well as fixed effects for country of incorporation of the operating firm. See Section 3.1 for a full description of the SPAC and IPO samples. Robust standard errors clustered by GICs sector are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) average age	(2) minimum age	(3) maximum age
SPAC Indicator	-11.98*	-12.18*	-11.78*
	(6.465)	(6.472)	(6.460)
Observations	2,364	2,364	2,364
R-squared	0.096	0.096	0.096
GICS sector FE	Yes	Yes	Yes
IPO year FE	Yes	Yes	Yes
Country FE	Yes	Yes	Yes

Table 3.9 investigates the relative riskiness of SPAC firms. In Column (1), we find that, relative to traditional IPO firms, SPAC firms exhibit higher cash flow volatility, which is defined as the standard deviation of cash flow-to-assets over the first four years after going public. Column (2) shows that, relative to traditional IPO firms, SPAC target firms have comparable cash on balance sheet. Column (3) illustrates that SPAC firms are significantly less profitable than traditional IPO firms as measured by return on assets. Specifically, their

net-income-to-asset ratio is lower than that of traditional IPO firms by 10.2 percentage points. Finally, Columns (4) to (6) show that SPAC firms offer significantly lower payouts in the form of dividends and share repurchases.

Table 3.9: Fundamentals of SPAC Target Firms v. Traditional IPO Firms

This table investigates differences in fundamentals for firms that undergo a traditional IPO versus SPAC target firms. The specification includes GICS sector fixed effects, fixed effects for the going-public year, as well as fixed effects for country of incorporation of the operating firm. Cash flow volatility in Column (1) is defined as the standard deviation of cash flow / assets over the first four annual reports after IPO, where cash flow is constructed as income before extraordinary items plus depreciation. In Columns (2) to (6), information from only the first annual report of each firm after the IPO year is used. See Section 3.1 for a full description of the SPAC and IPO samples. Robust standard errors clustered by GICs sector are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

		Using first annual report after IPO				
	(1)	(2)	(3)	(4)	(5)	(6)
	cash flow vol.	$\frac{\text{cash}}{\text{assets}}$	$\frac{\text{profits}}{\text{assets}}$	$\frac{\text{payout}}{\text{income}}$	$\frac{\text{dividends}}{\text{income}}$	$\frac{\text{share repurchases}}{\text{income}}$
SPAC Indicator	0.0785**	-0.0275	-0.102***	-0.828***	-0.188***	-0.406***
	(0.0297)	(0.0407)	(0.0168)	(0.148)	(0.0418)	(0.113)
Observations	2,082	2,626	2,631	2,318	2,621	2,330
R-squared	0.092	0.263	0.151	0.029	0.038	0.035
GICS sector FE	Yes	Yes	Yes	Yes	Yes	Yes
IPO year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes

Altogether, the results demonstrate that SPAC target firms are riskier and younger than IPO firms. Not only are they substantially smaller and less profitable but they also have significantly higher cash flow volatility and are more cash-constrained (as measured by cash-to-assets and payout ratios).

Fact 5: SPAC operating firms grow at similar or even higher rates compared to IPO firms after going public.

Next, we examine the relative growth of SPAC firms in the one to three years after going public. Table 3.10 reports the regression results for the growth measures. Our measures include the firm's growth rates of revenue, market capitalization, and assets in the years immediately after the firm becomes a public company. We calculate all growth measures one year, two years, and three years after the firm officially goes public. The results indicate that relative to IPO firms, SPAC firms have similar or even higher growth rates. SPAC firms grow revenue dramatically faster than traditional IPO firms: 87.0 percentage points faster over the first year, 128.9 percentage points faster across the first two years, and 312.9 percentage points faster across the first three years. Meanwhile, SPAC market capitalization growth and asset growth rates are not statistically significantly different from those of traditional IPO firms.

Table 3.10: Growth of SPAC Target Firms v. Traditional IPO Firms

This table investigates differences in asset growth, revenue growth, and market capitalization growth for firms that undergo a traditional IPO versus SPAC target firms. The specification includes GICS sector fixed effects, fixed effects for the going-public year, as well as fixed effects for country of incorporation of the operating firm. See Section 3.1 for a full description of the SPAC and IPO samples. Robust standard errors clustered by GICs sector are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Revenue growth over:			Market cap growth over:			Asset growth over:		
	first 1 year	first 2 years	first 3 years	first 1 year	first 2 years	first 3 years	first 1 year	first 2 years	first 3 years
SPAC Indicator	0.870***	1.289***	3.129**	-0.00595	-0.0646	-0.129	0.0143	0.0193	-0.307
	(0.268)	(0.402)	(1.179)	(0.115)	(0.226)	(0.288)	(0.0782)	(0.143)	(0.207)
Observations	1,810	1,536	1,318	2,071	1,717	1,449	2,093	1,737	1,467
R-squared	0.079	0.075	0.110	0.111	0.119	0.096	0.064	0.084	0.066
GICS sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IPO year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Overall, the results from the cross-sectional analysis of going-public firms are consistent with and provide a fitting explanation for our finding that lagged equity market sentiment strongly predicts SPAC issuance. The fact that SPAC operating firms are smaller, younger, less profitable, have more volatile cash flows and lower payout ratios, but have the potential for comparable or even higher growth, is consistent with these firms being more speculative investments. The formation of vehicles to seek out and acquire these types of companies should be sensitive to investor sentiment in equity markets.

Moreover, the comparable or faster growth rates of SPAC and IPO firms are at odds with the prevailing view that SPAC sponsors fail to play a meaningful certification role and that the SPAC market is ultimately a market for "lemons." In contrast, the observed relative growth rates are more consistent with the hypothesis that the SPAC market may serve firms of similar quality to IPO firms. In addition, by revealing observable differences between SPAC and IPO operating firms in terms of their size, age, and riskiness, our cross-sectional results indicate segmentation in the going-public market. In the following section, we present a difference-in-differences analysis that hones in on the litigation risk that is central to our conceptual framework. We exploit a change in the relative litigation risk between traditional IPOs and SPACs that was induced by a Supreme Court ruling and examine its impact on SPAC activity.

3.4 Increased IPO Market Litigation Risk and the Shift towards SPACs

In this section, we assess the third empirical prediction in our regulatory-arbitrage explanation for the proliferation of SPACs: an increase in the litigation risk faced in the traditional IPO market generates a shift towards SPACs. The ideal experiment would randomly assign a shock to litigation risk across regions and compare SPAC activity in treated versus control regions. A difference in SPAC activity between the two groups would be causally attributable to the litigation risk shock. While this ideal experiment is infeasible, we consider a shock to litigation risk that plausibly affected firms in some regions more than others, generating differential exposure to the treatment. We make use of this setting as a laboratory by undertaking a difference-in-differences approach wherein the treatment is the March 2018 *Cyan* Supreme Court ruling, the treated group is the set of states with greater exposure to the shock, and the control group is the set of states with smaller exposure to the shock.

3.4.1 Background on Litigation Risk Before and After *Cyan*

This section provides background on how the *Cyan* ruling increased traditional IPO litigation risk. In the aftermath of the 1929 stock market crash and during the Great Depression, Congress enacted the Securities Act of 1933, which instituted litigation risk for IPO companies in both state and federal court. Specifically, under Section 11 of the Act, issuers and underwriters became strictly liable for material misstatements and omissions in security registration statements. Because of the requirement to prove a connection between the stock purchase and the registration statement, Section 11 claims were filed primarily by IPO investors, rather than secondary market investors. Importantly, Congress stipulated that state and federal courts had concurrent jurisdiction over the 1933 Act claims such that investors could file in both state and federal court.

In a first-best world, regulation would perfectly gatekeep litigation such that lawsuits are lodged only when a company has caused investor loss through material misstatement or omission. This ideal has been difficult to achieve in practice. During the early 1990s, an overarching theme of regulatory back-and-forth surrounding the 1933 Act has been the challenge of establishing corporate responsibility to investors while avoiding burdensome, meritless lawsuits against companies.

In 1995, in response to increased concern about excessive litigation, Congress enacted the Private Securities Litigation Reform Act (PSLRA), which created barriers to class action litigation in federal court, e.g. an increase in the plaintiff pleading standard. When claims swiftly shifted to state courts, Congress acted once more in 1998 by enacting the Securities Litigation Uniform Standards Act (SLUSA), which aimed to prevent large securities class actions from being pursued in state court. In the aftermath of SLUSA, many expected that that 1933 Securities Act claims could be pursued only in federal court because cases filed in state court would be removed to federal court.

On March 20, 2018, the Supreme Court ruled in the case of *Cyan, Inc. v. Beaver County Employees Retirement Fund, et al.* that 1933 Act claims could be pursued in both federal and state court. In other words, the Supreme Court decided in favor of an interpretation of SLUSA that does *not* disturb the original concurrent jurisdiction of the 1933 Act. Investors became free to file Section 11 claims in state court without worrying that their state court litigation would be removed and dismissed. Klausner *et al.* (2020) find that the *Cyan* ruling took swift effect with a large increase in state court litigation, especially in the form of parallel litigation filed both in federal court and state court. Specifically, the proportion of federal-only litigation decreased from 67% to 24%, and the proportion of parallel litigation increased from 16% to 50%. These findings indicate that the majority of plaintiffs brought federal-only litigation during the pre-Cyan period and switched to bringing parallel federal and state litigation during the post-Cyan period.

3.4.2 Identification Strategy

While all issuers face higher Section 11 litigation exposure due to the Cyan ruling, we expect issuers in states associated with more plaintiff-friendly districts to be more intensely treated and to experience a greater Cyan-induced increase in expected litigation costs, as plaintiffs in these states concurrently embrace state court venues. This prediction motivates our state-level difference-in-differences analysis where the treatment is the Cyan ruling, the treatment group comprises states that have above-median pre-Cyan federal court litigation risk, and the control group comprises states that have below-median pre-Cyan federal court litigation risk.

When filing a federal court lawsuit against a company, plaintiffs can do so in the state of the company's headquarter, incorporation, or both. We measure plantiff friendliness using the dismissal rate of Section 11 federal court cases filed against issuers headquartered in a state because there is little variation in incorporated states. A lower dismissal rate points to a more plaintiff-friendly court and therefore a higher federal court litigation risk for issuers headquartered in a given state. To obtain data on stockholder suits filed in US federal court, we use the Federal Judicial Center's (FJC) Integrated Database, which contains federal court case information reported to the Administrative Office of the US Courts. The dismissal rate is computed as the number of dismissed cases divided by total number of cases for federal stockholder suits in the FJC database. We drop states with fewer than ten cases over the ten-year period preceding Cyan as these states have very little stockholder lawsuit activity and their dismissal rates will be noisy. The dismissal rate is a well-established measure in the literature on U.S. litigation (e.g. Klausner *et al.*, 2020; Kempf and Spalt, 2023).

In sum, the specification for our state-year-level difference-in-differences analysis is the following:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{PlaintiffFriendly}_i \times \text{Post}_t + \epsilon_{it}$$

where Y_{it} is a measure of SPAC activity for companies headquartered in state i in year t (e.g. the absolute number of SPACs or the SPAC share of total proceeds), α_i denotes state fixed effects, γ_t denotes time fixed effects, $\text{PlantiffFriendly}_i$ is an indicator equal to one for states that are friendly to home-court plaintiffs (as captured by a pleading-stage Section 11 case dismissal rate that is below the median dismissal rate across states in the ten years leading up to the Cyan ruling), and Post_t is an indicator equal to one in the year 2018 and onwards. Our sample period for this test is 2016-2019, which includes the 2 years preceding the Cyan ruling (2016, 2017) and the 2 years following the ruling (2018, 2019).

3.4.3 Results

Panel A of Table 3.11 presents the main results from this analysis. Columns (1) and (2) show that the Cyan ruling increases the SPAC number share by 31.3 percentage points and the SPAC proceeds share by 23.3 percentage points in treated states relative to control states. Figure 3.6 plots the dynamic effects. Importantly, we find no existence of pre-trends in our outcome variable in 2016, which is consistent with the quasi-exogeneity of the treatment.

The increase in SPAC share due to the Cyan ruling is most apparent in 2019, indicating that the effect is delayed.

One potential concern is that the state of California could be driving the results, given that many high-tech companies that access the public markets may be headquartered there. Columns (3) and (4) repeat the analyses in the preceding columns, but dropping California. The results remain economically and statistically significant.

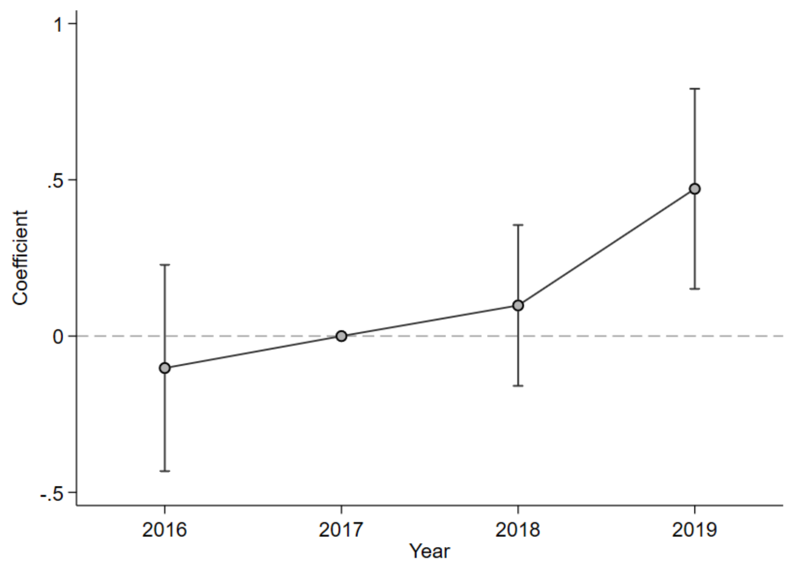
Table 3.11: Increased IPO Market Litigation Risk and the Shift towards SPACs

This table presents the difference-in-differences estimates of the annual SPAC share of new issuances as measured by counts and by gross proceeds. The treatment is the Supreme Court ruling on *Cyan, Inc. v. Beaver County Employees Retirement Fund* in March 2018. The treated states are those with above-median plaintiff friendliness on stockholder suits, and the control group those with below-median plaintiff friendliness. In Panel A, Columns (3) and (4) drop the state of California as a robustness check. Panel B presents two separate placebo tests. Columns (1) and (2) shift the year of treatment to 2017. Columns (3) and (4) randomly assign half of the states to the treatment and half to the control groups.

Panel A: Main Result	Full Sample		Drop California	
	(1)	(2)	(3)	(4)
	SPAC Share	Proc. Share	SPAC Share	Proc. Share
Plaintiff Friendly × Post	0.313**	0.233*	0.327**	0.245*
	(0.108)	(0.100)	(0.112)	(0.102)
FE: State	Yes	Yes	Yes	Yes
FE: Year	Yes	Yes	Yes	Yes
State Clusters	26	26	25	25
R^2	0.346	0.378	0.343	0.374
Mean	0.176	0.172	0.176	0.172
Observations	93	93	89	89
Sample	2016-2019	2016-2019	2016-2019	2016-2019
Panel B: Placebo Tests	Post $\geq$ 2017		Random States	
	(1)	(2)	(3)	(4)
	SPAC Share	Proc. Share	SPAC Share	Proc. Share
Post (2017) × Plaintiff Friendly	-0.016	0.088		
	(0.116)	(0.126)		
Random States × Post			-0.035	0.015
			(0.127)	(0.114)
FE: State	Yes	Yes	Yes	Yes
FE: Year	Yes	Yes	Yes	Yes
State Clusters	27	27	26	26
R^2	0.304	0.375	0.267	0.336
Mean	0.154	0.149	0.154	0.149
Observations	93	93	93	93
Sample	2015-2018	2015-2018	2016-2019	2016-2019

Figure 3.6: Increased IPO Market Litigation Risk and the Shift towards SPACs: Estimates by Year

This figure plots the yearly difference-in-differences estimates of annual SPAC share of new issuances. The treatment is the Supreme Court ruling on *Cyan, Inc. v. Beaver County Employees Retirement Fund* in March 2018. The treated states are those with above-median plaintiff friendliness on stockholder suits, and the control group those with below-median plaintiff friendliness. The year prior to the treated year is the baseline and normalized to zero. 95% confidence intervals are shown.



In addition to the aforementioned robustness check, we conduct two different placebo tests in Panel B of Table 3.11. The findings from these tests support the notion that the increase is due to the Cyan ruling and not to other factors that may have increased SPAC activity during this time. The first test estimates the diff-in-diff regression over the period of 2015 to 2018, the years immediately preceding the *Cyan* ruling. The placebo $Post_t$ indicator is equal to one in the years 2017 and 2018. Note that the true ruling occurred in 2018, biasing us against estimating a null effect. However, as Columns (1) and (2) show, the coefficient on the interaction term becomes insignificant, supporting the claim that our main finding can be attributed to the Cyan ruling. Next, in Columns (3) and (4), we randomly allocate the treatment by setting the variable $\text{PlaintiffFriendly}_i$ equal to 1 for a randomly selected half of the states in our sample. Once again, we find no effect, further validating that it is in fact the 2018 Cyan litigation shock that generated increased SPAC market share.

In sum, our difference-in-differences analysis provides evidence that increased litigation risk faced in the traditional IPO market leads to a shift towards SPACs, consistent with our prediction that the intermediary regulatory environment is one explanation for the use of these non-traditional going-public vehicles.

3.5 Discussion of Alternative Hypotheses

Our theory focuses on the incentives of intermediaries and the preferences of investors in the going-public market. We demonstrate how differences in regulation and compensation can impact the incentives of financial intermediaries who perform certification roles, and how these differing incentives can generate market segmentation. In doing so, we provide a rationale for the continued existence of SPACs. Our theory provides an explanation for the cross-sectional and time-series patterns we document and sheds light on why firms are willing to pay such high costs to go public through SPAC rather than IPO. For example, Gahng *et al.* (2023) find that the cost of going public through SPAC as a fraction of proceeds and post-issuance market cap is substantially larger than that of IPO. This cost difference amounts to 22 percentage points for proceeds and 9.3 percentage points for total market cap, for the median firm. The calculated cost of traditional IPOs accounts for underpricing costs.

In this section, we discuss other potential benefits of SPACs: speed to deal completion, relative certainty in negotiation, and avoidance of underpricing of traditional IPOs. These alternative hypotheses based on firm incentives could be meaningful factors that influence the going-public decision of private firms. However, these hypotheses are not widely agreed upon by academics and industry participants and may not fully explain the large cost gap between SPAC and IPO.

One often mentioned advantage of SPACs is that the SPAC process is faster than a traditional IPO due to less onerous paperwork requirements and fewer interactions with investors (such as on roadshows). Another often mentioned advantage of SPACs is that they provide greater price and deal certainty because the SPAC price is generally set before the closing of the merger. However, industry practitioners document that there is no meaningful speed difference between de-SPAC and IPO.[26] Moreover, Klausner *et al.* (2022) suggest that the certainty benefits of SPACs are often overstated, as SPAC merger agreements are often amended during the negotiation process between SPAC and investors, and there are also risks of shareholder redemption. In accordance with higher demand during times of greater uncertainty not driving SPAC activity, we find no statistically significant association between volatility as measured by the VIX and SPAC issuance in Section 3.2. Consistent with the model, we instead find a strong positive correlation between equity market sentiment, which we interpret as a measure of investor demand for risky or speculative securities, and SPAC issuance.

Another potential advantage of merging with a SPAC is that it is less costly than going public via a traditional IPO. In particular, one often mentioned criticism of the traditional IPO process is the underpricing of newly issued shares, particularly the IPO first-day pop that amounts to "money being left on the table." However, even after accounting for the underpricing, Gahng *et al.* (2023) and Klausner *et al.* (2022) both find that IPOs are much less costly than SPACs for going-public firms. Therefore, cost saving via the avoidance of underpricing cannot explain why firms go public through SPACs. Our theory suggests instead that operating firms that may be closed off from the traditional IPO market provide an explanation for the demand for SPACs.

3.6 Policy Discussion: Alleviating Agency Issues in the SPAC Market

The SPAC is the descendant of the blank check company, a "shell" that raises money via an initial public offering with the stated purpose of merging with an operating company.

[26] See https://media.velaw.com/wp-content/uploads/2020/10/09134747/Alternative-Routes-to-Going-Public-Initial-Public-Offering-De-SPAC-or-Direct-Listing.pdf.

Blank check companies were popular in the late 1980s but quickly diminished in prevalence due to fraudulent activity involving unsophisticated investors. This was soon followed by the formation of the modern SPAC. In 1992, the SPAC was developed by a small team of lawyers and underwriters to create a form of the blank check company with sufficient investor protections in place to gain the approval of the SEC (Heyman, 2008).

Investor-protection policies that have emerged since include the one-year lockup period of founder shares, the requirement that the majority of proceeds from the SPAC IPO must remain in a trust account, and the requirement that a shareholder vote be conducted to approve or reject a proposed merger. These improvements in the SPAC market structure have contributed to greater participation in the U.S. SPAC market on the part of both companies looking to go public and sponsors looking to raise SPACs.[27] We aim to identify frictions that exist in the current SPAC market and propose suggestions for improvement. We begin this section with an overview of the current structure and lifecycle of a U.S. regulated SPAC.

3.6.1 Current U.S. SPAC Market Structure

SPAC IPO: The SPAC sponsor (manager) first raises capital through an initial public offering of the SPAC. The SPAC then exists as a publicly-traded non-operating financial firm. The SPAC IPO consists of a unit offering with both common shares with voting rights and warrants that cannot be exercised until after the SPAC completes a reverse merger.[28] The

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[27]The agency issues can potentially explain low SPAC activity in the U.K. SPAC market. To offer more certainty when SPAC sponsors negotiate deals, U.K. SPAC market investors do not have an opt-out option; their shares are suspended until a deal prospectus is published. This structure may intensify agency issues and cause market breakdown. In 2020, the U.K. SPACs raised only £30m. See https://pitchbook.com/news/articles/spac-IPOs-london-stock-exchange-sluggish and https://www.theguardian.com/business/2021/mar/06/will-spacs-new-stars-of-corporate-finance-take-off-in-the-uk. On 27 July 2021, the U.K. Financial Conduct Authority (FCA) published a policy statement that proposes SPAC rule changes which remove the share suspension rule for certain SPACs and promote the redemption (opt-out) option for SPAC shareholders. See https://www.fca.org.uk/publication/policy/ps21-10.pdf.

[28]One criticism of the common SPAC structure is that the warrant provisions attract SPAC arbitrageurs. Some hedge funds known as the "SPAC mafia" may buy units in the SPAC IPO process and redeem shares before the closing of the acquisition while keeping the warrants. The hedge funds take almost no downside risk but enjoy the upside potential of the warrants. The recent innovation of the tontine structure of the warrant provision by Pershing Square Tontine Holdings (i.e. SPAC IPO investors lose 2/3 of the warrants if they redeem

warrant affords the exerciser the right to purchase shares in the going-public firm at a pre-specified price following the acquisition close. The proceeds raised by the SPAC are placed in a trust account and are then invested in U.S. Treasuries.

At the time of going public, SPACs often have a sectoral or geographic focus for their target company when they IPO, which will generally be related to the SPAC sponsor's area of expertise. However, they are not allowed to specify a particular target company at the time of the initial capital raise; rather, SPACs are formed with the intention of searching for the target company once the capital has been raised from external shareholders. Regarding compensation, SPAC sponsors do not receive management fees. They do, however, receive a portion of the firm's equity upon the closing of a successful acquisition. This is generally 20% of the firm's public equity and is represented by $frac$ in our model.

Post-IPO: After the SPAC goes public, the SPAC sponsor identifies a target operating company and seeks shareholder approval to combine with the target firm. Shareholders can redeem shares to receive the pro-rata value of the trust fund. Shareholders are also given the right to vote on proposed mergers. In order for the combination to materialize, the majority of shareholders must approve the transaction and must be below the redemption threshold; that is, a certain portion of shareholders must not redeem their shares. Note that outside shareholders can redeem their shares and receive a pro-rata share of the trust value if the acquisition is ultimately approved. Since shareholders can redeem their shares, oftentimes, a private placement (PIPE) transaction will occur following the SPAC's initial capital raise via the IPO. This PIPE transaction provides additional funds to finance the business combination.

De-SPAC: In the final phase of the SPAC lifecycle, either the SPAC and target operating company combine in reverse merger acquisition, or the SPAC liquidates. If the first proposed acquisition does not receive shareholder approval, managers can continue to search for another target company. However, they must find a firm within the specified time frame.

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their stock before the deal closure and the shareholders who stay in receive a pro-rata portion of the redeeming shareholders' warrants) discourages this exercise and encourages long-term investors. Gahng *et al.* (2023) document a downward trend in public warrant offering in SPAC IPOs.

Typically, the SPAC sponsor is given 18-24 months before liquidation. The exact duration allowed for target company search is specified in the S-1 filing that the SPAC must file with the SEC. If the SPAC liquidates, the warrants of the sponsor expire, and the funds in the trust plus accumulated interest are returned to initial shareholders. This amounts to a loss of the managers' at-risk capital, which consists of shares and warrants purchased at the time of the SPAC's IPO. From 2003 Q2 to 2020 Q4, out of 636 total U.S. SPACs that were formed, 455 SPACs had completed their reverse mergers and 123 SPACs had been liquidated as of November 2022.

3.6.2 Policy Discussion

The current market structure of the SPAC raises two policy-relevant questions in relation to our theoretical framework. The first question involves identifying frictions in the current SPAC market and the sources of these frictions. SPAC sponsors face agency issues that may originate from inefficient contracts. When the demand for value-creating small and risky firms exceeds their supply, SPAC sponsors may struggle to find available value-creating firms and have misaligned incentives to take "lemons" public in the SPAC market. Another potential friction is due to deviations of the true structure from our theoretical model. Our current model has two periods. The payoff of the SPAC sponsors is based on the operating firm's long-term outcome; that is, SPAC sponsors sell shares and realize the payoffs when the project returns are realized. In reality, it may be difficult to determine the exact timing of cash flow realization ex ante. The convention of the one-year lockup period could induce short-termism from SPAC sponsors. Consider the inclusion of myopic SPAC sponsors and an interim period between the going-public date and the cash-flow realization date to the model. Myopic SPAC sponsors would then have an incentive to shirk their certification role, bring bad firms into the market, and conceal the bad news while exiting at the interim period before the release of this unfavorable news. Therefore, short-termism of myopic SPAC sponsors, asymmetric information between SPAC sponsors and public investors, and inefficient contracts may generate agency issues. These possibilities are consistent with the current scrutiny from the SEC on the compensation structure of SPAC sponsors.[29]

The second question asks how to alleviate these potential agency issues by improving the current SPAC market structure. The objective of the improved SPAC market structure is to discourage SPAC sponsors from taking lemons public, while not discouraging them from taking value-creating small and risky firms public.

This paper's findings relate to two recent proposals on SPAC market regulation from Klausner *et al.* (2022): (1) requiring more disclosure in the SPAC process, such as on side payments in the form of shares or warrants to public shareholders, and (2) imposing underwriter liability analogous to those faced by investment banks. The results in our paper support the first initiative, which would help mitigate asymmetric information between SPAC sponsors and public investors and thus alleviate agency issues in the SPAC market. However, our results do not support the second initiative, as elevated litigation risk for intermediaries may undermine one economic role of the SPAC market that bypasses the downside-averse financial intermediaries and enables risk-taking but potentially value-creating firms to go public.

Additionally, turning from regulatory-based solutions to more market-based solutions, we propose that the compensation structure of SPAC sponsors be based on more *long-run* performance of the operating firms in order to align SPAC sponsors with long-term shareholders. We suggest that a long-term phase-in structure, earnout provisions, and an optimized stock-warrant mixture of SPAC sponsor compensation, may help achieve this goal. These suggestions address the timeline, benchmark, and instrument of SPAC sponsor compensation.

(1) Compensation timeline: We suggest a long-term phase-in structure. This structure would resemble the practice of phasing in stock-based compensation of firm managers over a number of years to align their incentives with the long-run performance of firms rather than the short-term stock price. This phase-in structure would bind SPAC sponsors to the

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[29]See https://www.wsj.com/articles/blank-check-firms-offering-ipo-alternative-are-under-regulatory-scrutiny-11600979237.

operating firms for a longer period, making it more difficult to conceal unfavorable news and exit ex-post. This long-term alignment would more closely resemble a private equity fund structure, which typically involves a focus on medium-term or long-term investment horizons. Indeed, many SPAC sponsors have private equity backgrounds. One additional benefit of the long-term phase-in structure is that SPAC sponsors may not only perform certification role as information specialists, but also affect real operating activities of going-public firms. For example, SPAC sponsors can help increase firm value by contributing to firm production using their past private market experience. This long-term phase-in structure thus incentivizes SPAC sponsors to increase an operating firm's value over the long run by aligning SPAC sponsors with the operating firm's long-term performance. Consistent with our proposal, in February 2021, Reinvent Technology Partners announced that the SPAC sponsors and its acquired electric aircraft company Joby Aviation have agreed to have a lockup period of up to five years for part of the sponsor shares.[30]

(2) Compensation benchmark: We suggest the continued use of earnout provisions. Another policy to align SPAC sponsors with long-term investors is to have the equity-based compensation vested conditional on the long-term performance of the operating firms, that is, to have SPAC sponsors only earn their compensare 1tion if their operating firms perform well in the long run. This feature would reduce SPAC sponsors' payoffs in the bad states. One recent example is that in March 2021, NightDragon announced that it will divide its 20 percent SPAC sponsor compensation into four equal tranches: three slices are earned only if the shares hit $12, $15 and $20, while the first slice is earned only once an acquisition is complete.[31] More widespread implementation of earnout provisions could help align SPAC sponsors with long-term investors.

(3) Compensation instrument: We suggest the design of an optimized stock-warrant mixture. The potential agency issue in a SPAC where sponsors are fully compensated with

[30]See https://sec.report/Document/0001193125-21-054596/. Also note that the first draft of this paper was posted on SSRN in December 2020.

[31]See https://www.sec.gov/Archives/edgar/data/1837067/000119312521034596/d61319ds1.htm.

common stock is as follows: because SPAC sponsors receive common stock with low costs (generally with an investment of $25,000), they receive a high payoff even if the value of the common stock drops below the issuing price. In contrast, warrants are only valuable if the value of the common stock is higher than its strike price. Therefore, warrant compensation may incentivize SPAC sponsors to exert effort. However, we caution that using warrant compensation is not without its downsides. In particular, it can also increase incentives for risk-taking for SPAC sponsors. The optimization of the stock-warrant mixture ultimately depends on the trade-off between effort incentivization and risk-taking motives of sponsors. The compensation structure should optimize the stock-warrant mixture to incentivize SPAC sponsors to exert effort while moderating motives for risk-taking. Estimating such an optimum may be an area for future research. Note that both stocks and warrants could be combined with a long-term phase-in structure as well as earnout provisions to further align SPAC sponsors with long-term investors.

3.7 Conclusion

This paper examines the economic role of SPACs and provides a unified explanation for the existence, time-series variation, and recent boom of SPACs. We build a conceptual framework that shows that the more lenient regulatory requirements and equity-based compensation structure can incentivize SPAC sponsors to take value-creating but smaller and riskier operating firms public in the SPAC market. In doing so, SPAC sponsors serve as non-bank certification intermediaries and effectively fill a gap in the going-public market left by traditionally regulated intermediaries. Consistent with this framework, we empirically show that equity market sentiment explains a large proportion of SPAC market activity; compared to traditional IPO firms, SPAC firms are ex-ante smaller and riskier but grow at similar or higher rates after going public; an increase in the litigation risk of traditional IPOs relative to SPACs leads to increased SPAC activity.

On the one hand, our results suggest one bright side of the SPAC market: a well-functioning SPAC market can open the going-public door for value-creating, but smaller and riskier firms to go public. Given that the ability to become a publicly listed firm provides opportunities for firm capital growth and share liquidity improvement, the existence of such a mechanism may stimulate entrepreneurial activity ex ante. On the other hand, our paper also highlights the riskiness of SPAC firms and the importance of aligning sponsors with long-term investors. The growing market share of SPACs implies an increasing impact of SPAC firms on the economy. Thus, mitigating the potential short-termism of SPAC sponsors is important for protecting public investors and maintaining financial stability. Increasing expected litigation costs for intermediaries in the SPAC market may not be optimal as it could disrupt a core economic function of SPACs of taking value-creating risky firms public. We suggest instead that an improved SPAC sponsor compensation structure through a long-term phase-in structure, earnout provisions, and optimized stock-warrant mixture may help alleviate agency issues by better aligning the incentives of SPAC sponsors with those of long-term investors. If implemented, we predict a healthier SPAC market with potential to attract broader participation from investors.

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Appendix A

Appendix to Chapter 1

A.1 Model Solution

A.1.1 Baseline model

In this section, I present the solutions of the baseline model in Hanson *et al.* (2020), including the private market outcome without CEM and the social planner's solution. In the next section, I will solve for the private market outcome with CEM.

Private market outcome

To solve the model, we backwards induct from $t = \infty$.

Steady state: $t = \infty$

We start in state S_∞ at $t = \infty$. Assuming they have survived at both $t = 1$ and $t = 2$, only viable firms with $X_\infty(f, R_{S_\infty}) \geq 0$ (or equivalently $f \leq \bar{F}_{S_\infty} = \dfrac{\mu + \gamma - R_{S_\infty}}{\delta}$) will continue operating in state S_∞ at $t = \infty$. Thus, if firm f survives until $t = \infty$ in state S_∞, its value to private investors will be:

$$V_\infty(f, S_\infty) = \frac{1}{1-\delta} \cdot \min\{X_\infty(f, R_{S_\infty}), 0\}$$
$$= \frac{1}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f] \cdot 1_{\{f \leq \bar{F}_{S_\infty}\}} \tag{A.1}$$

where $1_{\{f \leq \bar{F}_{S_\infty}\}}$ is a binary indicator that switches on when $f \leq \bar{F}_{S_\infty}$.

Interim date: $t = 2$

We next backwards induct to state S_2 at $t = 2$. Suppose all firms $f \in [0, F_1]$ survived at t = 1. If the mass of firms that continue operating in state S_2 at $t = 2$ is equal to F_{S_2}, the private value of firm f will be:

$$V_2(f, R_{S_2}, F_{S_2}) = \min\{X_2(f, R_{S_2}, F_{S_2}) + \delta \cdot V_\infty(f, R_{S_\infty}), 0\}$$
$$= \min\{[\mu + \gamma \times F_{S_2} - R_{S_2} - \Delta \times f]$$
$$+ \delta \cdot \frac{1}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f] \cdot 1_{\{f \leq \bar{F}_{S_\infty}\}}, 0\} \tag{A.2}$$

There are two cases.

Case 1: $V_2(F_1, R_{S_2}, F_1) \geq 0$

This is the case where all firms that survive at $t = 1$ are privately valuable in state S_2 at $t = 2$. Then, no additional firms will be shut down in S_2. In sum, we have $F^*_{S_2} = F_1$.

Case 2: $V_2(F_1, R_{S_2}, F_1) < 0$

This is the case where the marginal firm that survives in $t = 1$ has negative private value in state S_2 at $t = 2$. This marginal firm will be shut down in S_2. Then $F^*_{S_2} = \hat{F}^*_{S_2} < F_1$ where $F^*_{S_2}$ is the solution to $V_2(\hat{F}^*_{S_2}, R_{S_2}, \hat{F}^*_{S_2}) = 0$. Solving for $\hat{F}^*_{S_2}$, we have:

$$\hat{F}^*_{S_2} = \frac{(1-\delta) \cdot (\mu - R_{S_2}) + \delta \cdot (\mu + \gamma - R_{S_\infty})}{(1-\delta) \cdot (\Delta - \gamma) + \delta \cdot \Delta} \tag{A.3}$$

Combining these two cases, we have:

$$F^*_{S_2}(F_1) = \min\{F_1, \hat{F}^*_{S_2}\} \tag{A.4}$$

Initial date: t = 1

Finally, we consider what happens at t = 1. If the mass of firms that continue operating at $t = 1$ is equal to F_1, then the private value of firm f is given by:

$$\begin{aligned} V_1(f, F_1) = \min\{&X_1(f, R_1) \\ &+ (1-\phi)\cdot\delta\cdot\left[(1-p)\cdot V_2(f, R_{G_2}, F^*_{G_2}(F_1)) + p\cdot V_2(f, R_{B_2}, F^*_{B_2}(F_1))\right], 0\} \end{aligned} \tag{A.5}$$

where $\phi \in (0,1)$ reflects the credit market frictions that exist at $t = 1$ and $F^*_{S_2} = \min\{F_1, \hat{F}^*_{S_2}\}$ is agents' rational expectation of the mass of firms that will continue operating in state S_2 at $t = 2$ if all firms $f \in [0, F_1]$ continue operating at $t = 1$. Thus, the marginal firm who continues operating at $t = 1$ satisfies $0 = V_1(F^*_1, F^*_1)$, or

$$\begin{aligned} 0 = &[\mu + \gamma - R_1 - \Delta \times F^*_1] \\ &+ (1-\phi)(1-p)\delta([\mu + \gamma \times F^*_1 - R_{G_2} - \Delta \times F^*_1] + \frac{\delta}{1-\delta}\cdot[\mu + \gamma - R_{G_\infty} - \Delta \times F^*_1])\cdot 1_{\{F^*_1 \leq \hat{F}^*_{G_2}\}} \\ &+ (1-\phi)p\delta([\mu + \gamma \times F^*_1 - R_{B_2} - \Delta \times F^*_1] + \frac{\delta}{1-\delta}[\mu + \gamma - R_{B_\infty} - \Delta \times F^*_1])\cdot 1_{\{F^*_1 \leq \hat{F}^*_{B_2}\}} \end{aligned} \tag{A.6}$$

Let $\bar{F}_1 = \dfrac{\mu + \gamma - R_1}{\Delta} < 1$ denote the index of the firm that generates zero free cash flows at $t = 1$. We assume $\bar{F}_1 < \bar{F}^*_{G_2}$. This means that there are firms who require outside investment to survive at $t = 1$, i.e. firms with negative free cash flow that have positive value in state G_2 at $t = 2$. This assumption then implies that the marginal firm who continues operating at $t = 1$ must satisfy $\bar{F}_1 < F^*_1 < \hat{F}^*_{G_2}$.

There are then 2 relevant cases.

Case 1: $\hat{F}^*_{B_2} < F^*_1 < \hat{F}^*_{G_2}$

In other words, the marginal firm who continues operating at $t = 1$ survives in the good state at $t = 2$ but is shut down in the bad state. In this case, the marginal firm that survives at $t = 1$ is given by:

$$F^*_1 = \frac{(1-\delta)\cdot[\mu+\gamma-R_1] + (1-\phi)\cdot(1-p)\cdot\delta\cdot\left[(1-\delta)\cdot(\mu-R_{G_2}) + \delta\cdot(\mu+\gamma-R_{G_\infty})\right]}{(1-\delta)\cdot\Delta + (1-\phi)\cdot(1-p)\cdot\delta\left[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta\right]} \quad \text{(A.7)}$$

Case 2: $F^*_1 \leq \hat{F}^*_{B_2} < \hat{F}^*_{G_2}$

In other words, the marginal firm that continues operating at $t = 1$ survives in both states at $t = 2$. In this case, the marginal firm that survives at $t = 1$ is given by:

$$F^*_1 = \frac{(1-\delta)\cdot[\mu+\gamma-R_1] + (1-\phi)\cdot\delta\cdot\left[(1-\delta)\cdot(\mu-\bar{R}_{S_2}) + \delta\cdot(\mu+\gamma-\bar{R}_{S_\infty})\right]}{(1-\delta)\cdot\Delta + (1-\phi)\cdot\delta\left[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta\right]} \quad \text{(A.8)}$$

where $\bar{R}_2 = pR_{B_2} + (1-p)R_{G_2}$ and $\bar{R}_\infty = pR_{B_\infty} + (1-p)R_{G_\infty}$ are the average recession severity at $t = 2$ and $t = \infty$, respectively.

What are the conditions for being in case 1 or case 2? Given that we must have $\bar{F}_1 < F^*_1$, we will be in case 1 (where the marginal firm operating at $t = 1$ fails in state B_2) if $\hat{F}^*_{B_2} < \bar{F}_1$. Otherwise, we will be in case 2 (where the marginal firm operating at $t = 1$ survives in both states at $t = 2$) if $\hat{F}^*_{B_2} > \bar{F}_1$ and $0 < V_1(\hat{F}^{c*}_{B_2}, \hat{F}^{c*}_{B_2})$.

Social planner's solution

Steady state: $t = \infty$

We start in state S_∞ at $t = \infty$. Since there are no market failures in the long run, the planner places the same value as the private market on firms in the steady state.

$$
\begin{aligned}
W_\infty(f, S_\infty) &= V_\infty(f, S_\infty) \\
&= \frac{1}{1-\delta} \cdot \min\{X_\infty(f, R_{S_\infty}), 0\} \\
&= \frac{1}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f] \qquad \text{(A.9)}
\end{aligned}
$$

Interim date: $t = 2$

We next backwards induct to state S_2 at $t = 2$. Suppose all firms $f \in [0, F_1]$ survived at $t = 1$. If the mass of firms that continue operating in state S_2 at $t = 2$ is equal to F_{S_2}, total social value is given by:

$$
\begin{aligned}
W_2(R_{S_2}, F_1) &= \max_{F_{S_2} \leq F_1} \{ \int_0^{F_{S_2}} (X_2(f, R_{S_2}, F_{S_2}) + \delta \cdot V_\infty(f, R_{S_\infty}))df \} \\
&= \max_{F_{S_2} \leq F_1} \{ \int_0^{F_{S_2}} (\mu + \gamma \times F_{S_2} - R_{S_2} - \Delta \times f) \\
&\quad + \delta \cdot \frac{1}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f])df \} \qquad \text{(A.10)}
\end{aligned}
$$

There are two cases.

Case 1: $F^{**}_{S_2} = \hat{F}^{**}_{S_2} < F_1$

where $\hat{F}^{**}_{S_2}$ is the unconstrained maximizer of $W_2(R_{S_2}, F_1)$ In other words, some firms that survive at $t = 1$ may be shut down by the planner in state S_2 at $t = 2$

Case 2: $\hat{F}^{**}_{S_2} > F_1$

In other words, no firms are shut down in S_2. Then, the planner sets $F^{**}_{S_2} = F_1$.

Combining the two cases, we have:

$$
F^{**}_{S_2}(F_1) = \min\{F_1, \hat{F}^{**}_{S_2}\} \qquad \text{(A.11)}
$$

and the planner's social value function is given by:

$$W_2(R_{S_2}, F_1) = \int_0^{F^{**}_{S_2}(F_1)} \Big(\big[\mu + \gamma \times F^{**}_{S_2} - R_{S_2} - \Delta \times f\big] + \frac{\delta}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f] \cdot 1_{\{f \leq \bar{F}_{S_\infty}\}} \Big) df \tag{A.12}$$

Initial date: $t = 1$

Finally, we consider what happens at $t = 1$. If the mas sof firms that continue operating at $t = 1$ is equal to F_1, then total social value is given by:

$$W_1(F_1) = \int_0^{F_1} \Big(X_1(f, R_1) + \delta \cdot \big[(1-p) \cdot W_2(R_{G_2}, F^{*}_{G_2}(F_1)) + p \cdot W_2(f, R_{B_2}, F^{*}_{B_2}(F_1))\big] \Big) df \tag{A.13}$$

The first-order condition for maximizing total social value is:

$$0 = X_1(F_1, R_1) + \delta \cdot \left[(1-p) \cdot \frac{\partial W_2}{\partial F_{G_2}} \cdot \frac{\partial F^{**}_{G_2}(F_1)}{\partial F_1} + p \cdot \frac{\partial W_s(f, R_{B_2}, F^{**}_{B_2}(F_1))}{\partial F_{B_2}} \cdot \frac{\partial F^{**}_{B_2}(F_1)}{\partial F_1} \right] \tag{A.14}$$

Writing this out, we obtain:

$$\begin{aligned} 0 = {} & [\mu + \gamma - R_1 - \Delta \times F_1^{**}] \\ & + (1-p) \cdot \delta \cdot \Big([\mu + 2\gamma \times F_1^{**}] + \frac{\delta}{1-\delta} \cdot [\mu + \gamma - R_{G_\infty} - \Delta \times F_1^{**}] \Big) \cdot 1_{\{F_1^{**} \leq \hat{F}^{**}_{G_2}\}} \\ & + p \cdot \delta \cdot \Big([\mu + 2\gamma \times F_1^{*} - R_{B_2} - \Delta \times F_1^{**}] + \frac{\delta}{1-\delta} \cdot [\mu + \gamma - R_{B_\infty} - \Delta \times F_1^{**}] \Big) \cdot 1_{\{F_1^{**} \leq \hat{F}^{**}_{B_2}\}} \end{aligned} \tag{A.15}$$

There are then two relevant cases.

Case 1: $\hat{F}^{**}_{B_2} < F_1^{**} < \hat{F}^{**}_{G_2}$

In other words, the marginal firm at $t = 1$ survives in the good state at $t = 2$ but is shut down by the planner in the bad state. In this case, the marginal firm at $t = 1$ is given by:

$$F_1^{**} = \frac{(1-\delta)\cdot[\mu+\gamma-R_1] + (1-p)\cdot\delta\cdot\left[(1-\delta)\cdot(\mu-R_{G_2}) + \delta\cdot(\mu+\gamma-R_{G_\infty})\right]}{(1-\delta)\cdot\Delta + (1-p)\cdot\delta\cdot[(1-\delta)\cdot(\Delta-2\gamma)+\delta\cdot\Delta]} \tag{A.16}$$

Case 2: $F_1^{**} \leq \hat{F}_{B_2}^{**} < \hat{F}_{G_2}^{**}$

In other words, the marginal firm at $t = 1$ will survive in both states at $t = 2$. In this case, the marginal firm at $t = 1$ is given by:

$$F_1^{**} = \frac{(1-\delta)\cdot[\mu+\gamma-R_1] + \delta\cdot\left[(1-\delta)\cdot(\mu-\bar{R}_{S_2}) + \delta\cdot(\mu+\gamma-\bar{R}_{S_\infty})\right]}{(1-\delta)\cdot\Delta + \delta\cdot[(1-\delta)\cdot(\Delta-2\gamma)+\delta\cdot\Delta]} \tag{A.17}$$

What are the conditions for being in case 1 or case 2? Given that we must have $\bar{F}_1 < F_1^{**}$, we will be in case 1 (where the marginal firm operating at $t = 1$ fails in state B_2) if $\hat{F}_{B_2}^{**} < \bar{F}_1$. Otherwise, we will be in case 2 (where the marginal firm operating at $t = 1$ survives in both states at $t = 2$) if $\hat{F}^{**} > \bar{F}_1$ and $0 < V_1(\hat{F}_{B_2}^{**}, \hat{F}_{B_2}^{**})$

A.1.2 Private market outcome with CEM policy

In this section, I solve for the private market outcome with CEM. CEM requires landlords to provide liquidity to business tenants by prohibiting evictions and pausing the obligation of rent payments. Because CEM is a mandated liquidity policy, commercial landlords do not optimize over a lending decision. CEM policy can be directly incorporated into the model by adjusting the cash flow equations. Let X_t^c denote the cash flow function in a world with CEM.

At time $t = 1$, all firms receive funds from their landlords. The cash flow of firm f is increased by ρ relative to the world without CEM:

$$\begin{aligned} X_1^c(f, R_1) &= X_1(f, R_1) + \rho \\ &= \mu + \gamma - R_1 - \Delta \times f + \rho \end{aligned} \tag{A.18}$$

At time $t = 2$, accumulated back rent must be repaid to the landlord. In other words, the zero-interest credit from landlords comes due. The cash flow of firm f is decreased by ρ

relative to the world without CEM.

$$\begin{aligned} X_2^c(f, R_{S_2}, F_{S_2}) &= X_2(f, R_{S_2}, F_{S_2}) - \rho \\ &= \mu + \gamma \times F_{S_2} - R_{S_2} - \Delta \times f - \rho \end{aligned} \tag{A.19}$$

At time $t = \infty$, the cash flow of firm f is the same as in a world without CEM:

$$\begin{aligned} X_\infty^c(f, R_{S_\infty}) &= X_\infty(f, R_{S_\infty}) \\ &= \mu + \gamma - R_{S_\infty} - \Delta \times f \end{aligned} \tag{A.20}$$

To solve the model, we backwards induct from $t = \infty$.

Steady state: $t = \infty$

We start in state S_∞ at $t = \infty$. Since the CEM policy does not apply in the steady state, there is no difference between the private market valuation of firms without v.s. with CEM. If firm f survives until $t = \infty$ in state S_∞, its value to private investors will be:

$$\begin{aligned} V_\infty^c(f, S_\infty) &= \frac{1}{1-\delta} \cdot \min\{X_\infty^c(f, R_{S_\infty}), 0\} \\ &= \frac{1}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f] \cdot 1_{\{f \le \bar{F}_{S_\infty}\}} \end{aligned} \tag{A.21}$$

where $1_{\{f \le \bar{F}_{S_\infty}\}}$ is a binary indicator that switches on when $f \le \bar{F}_{S_\infty}$.

Interim date: $t = 2$

We next backwards induct to state S_2 at $t = 2$. Suppose all firms $f \in [0, F_1]$ survived at $t = 1$. If the mass of firms that continue operating in state S_2 at $t = 2$ is equal to F_{S_2}, the private value of firm f will be:

$$\begin{aligned}V_2^c(f, R_{S_2}, F_{S_2}) &= \min\{X_2^c(f, R_{S_2}, F_{S_2}) + \delta \cdot V_\infty^c(f, R_{S_\infty}), 0\} \\ &= \min\{[\mu + \gamma \times F_{S_2} - R_{S_2} - \Delta \times f - \rho] \\ &+ \delta \cdot \frac{1}{1-\delta} \cdot [\mu + \gamma - R_{S_\infty} - \Delta \times f] \cdot 1_{\{f \le \bar{F}_{S_\infty}\}}, 0\}\end{aligned} \tag{A.22}$$

There are two cases.

Case 1: $V_2^c(F_1, R_{S_2}, F_1) \geq 0$

In other words, all firms that survive at $t = 1$ are privately valuable in state S_2 at $t = 2$. Then no additional firms will be shut down in S_2. In sum, we have $F_{S_2}^{c*} = F_1$.

Case 2: $V_2^c(F_1, R_{S_2}, F_1) < 0$

In other words, the marginal firm that survives in $t = 1$ has negative private value in state S_2 at $t = 2$. The marginal firm will be shut down in S_2. Then $F_{S_2}^{c*} = \hat{F}_{S_2}^{c*} < F_1$ where $\hat{F}_{S_2}^{c*}$ is the solution to $V_2^c(\hat{F}_{S_2}^{c*}, R_{S_2}, \hat{F}_{S_2}^{c*}) = 0$. Solving for $\hat{F}_{S_2}^{c*}$, we have:

$$\hat{F}_{S_2}^{c*} = \frac{(1-\delta) \cdot (\mu - R_{S_2} - \rho) + \delta\,[\mu + \gamma - R_{S_\infty}]}{(1-\delta) \cdot (\Delta - \gamma) + \delta \cdot \Delta} \tag{A.23}$$

Initial date: t = 1

Finally, we consider what happens at t = 1. If the mass of firms that continue operating at $t = 1$ is equal to F_1, then the private value of firm f is given by:

$$\begin{aligned}V_1^c(f, F_1) = \min\{&X_1^c(f, R_1) \\ &+ (1-\phi) \cdot \delta \cdot ((1-p) \cdot V_2^c(f, R_{G_2}, F_{G_2}^{c*}(F_1)) \\ &+ p \cdot V_2^c(f, R_{B_2}, F_{B_2}^{c*}(F_1))), 0\}\end{aligned} \tag{A.24}$$

where $\phi \in (0, 1)$ reflects the credit market frictions that exist at $t = 1$ and $F_{S_2}^{c*} = \min\{F_1, \hat{F}_{S_2}^{c*}\}$ is agents' rational expectation of the mass of firms that will continue operating in state S_2 at $t = 2$ if all firms $f \in [0, F_1]$ continue operating at $t = 1$. Thus, the marginal firm who continues operating at $t = 1$ satisfies $0 = V_1^c(F_1^{c*}, F_1^{c*})$, or:

$$
\begin{aligned}
0 = & [\mu + \gamma - R_1 - \Delta \times F_1^{c*} + \rho] \\
& + (1-\phi) \cdot (1-p) \cdot \delta \cdot ([\mu + \gamma \times F_1^{c*} - R_{G_2} - \Delta \times F_1^{c*} - \rho] \\
& + \frac{\delta}{1-\delta} \cdot [\mu + \gamma - R_{G_\infty} - \Delta \times F_1^{c*}]) \cdot 1_{\{F_1^{c*} \leq \hat{F}_{G_2}^{c*}\}} \\
& + (1-\phi) \cdot p \cdot \delta \cdot ([\mu + \gamma \times F_1^{c*} - R_{B_2} - \Delta \times F_1^{c*} - \rho] \\
& + \frac{\delta}{1-\delta} \cdot [\mu + \gamma - R_{B_\infty} - \Delta \times F_1^{c*}]) \cdot 1_{\{F_1^{c*} \leq \hat{F}_{B_2}^{c*}\}}
\end{aligned}
\tag{A.25}
$$

Comparing equation (A.25) with (A.6) sheds light on the relationship between F_1^{c*} and F_1^*. For all firms, CEM adds cash flow ρ at $t = 1$. For those firms that choose to continue operating at $t = 2$, they will have to repay ρ. In sum, the positive cash flow of CEM is unconditionally provided to all firms at $t = 1$ while the negative cash flow of CEM is conditionally repaid by firms that continue operating at $t = 2$. It becomes clear that the right-hand side of (A.32) is higher relative to the right-hand-side of (A.6) holding fixed F_1. Therefore, it must be the case that $F_1^{c*} > F_1^*$.

Let $\bar{F}_1^c = \dfrac{\mu + \gamma - R_1 + \rho}{\Delta} < 1$ denote the index of the firm that generates zero free cash flows at $t = 1$. We assume $\bar{F}_1^c < \hat{F}_{G_2}^{c*}$. This means that, when CEM provides cash flow ρ to firms at $t = 1$, there are still firms who require outside investment to survive at $t = 1$, i.e. firms with negative free cash flow that have positive value in state G_2 at $t = 2$. This assumption then implies that the marginal firm who continues operating at $t = 1$ must satisfy $\bar{F}_1^c < F_1^{c*} < \hat{F}_{G_2}^{c*}$.

There are then 2 relevant cases.

Case 1: $\hat{F}_{B_2}^{c*} < F_1^{c*} < \hat{F}_{G_2}^{c*}$
In other words, the marginal firm who continues operating at $t = 1$ survives in the good state at $t = 2$ but is shut down in the bad state. In this case, the marginal firm that survives

at $t = 1$ is given by:

$$F_1^{c*} = \frac{(1-\delta)\cdot[\mu+\gamma-R_1+\rho]}{(1-\delta)\cdot\Delta+(1-\phi)\cdot(1-p)\cdot\delta\,[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta]}$$

$$+\frac{(1-\phi)\cdot(1-p)\cdot\delta\cdot\left[(1-\delta)\cdot(\mu-R_{G_2}-\rho)+\delta\cdot(\mu+\gamma-R_{G_\infty})\right]}{(1-\delta)\cdot\Delta+(1-\phi)\cdot(1-p)\cdot\delta\,[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta]} \tag{A.26}$$

Case 2: $F_1^{c*} \leq \hat{F}_{B_2}^{c*} < \hat{F}_{G_2}^{c*}$

In other words, the marginal firm that continues operating at $t = 1$ survives in both states at $t = 2$. In this case, the marginal firm that survives at $t = 1$ is given by:

$$F_1^{c*} = \frac{(1-\delta)\cdot[\mu+\gamma-R_1+\rho]+(1-\phi)\cdot\delta\cdot\left[(1-\delta)\cdot(\mu-\bar{R}_{S_2}-\rho)+\delta\cdot(\mu+\gamma-\bar{R}_{S_\infty})\right]}{(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta\,[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta]} \tag{A.27}$$

where $\bar{R}_2 = pR_{B_2} + (1-p)R_{G_2}$ is the average recession severity at $t = 2$ and $\bar{R}_\infty = pR_{B_\infty} + (1-p)R_{G_\infty}$ are the average recession severity at $t = \infty$.

What are the conditions for being in case 1 or case 2? Given that we must have $\bar{F}_1^c < F_1^{c*}$, we will be in case 1 (where the marginal firm operating at $t = 1$ fails in state B_2) if $\hat{F}_{B_2}^{c*} < \bar{F}_1^c$. Otherwise, we will be in case 2 (where the marginal firm operating at $t = 1$ survives in both states at $t = 2$) if $\hat{F}_{B_2}^{c*} > \bar{F}_1^c$ and $0 < V_1(\hat{F}_{B_2}^*, \hat{F}_{B_2}^*)$.

I next examine how μ affects $F_{S_2}^{c*} - F_{S_2}^*$. The relevant case to focus on is when $F_{S_2}^* = F_1^*$. Intuitively, this means that in the world without CEM, all firms that survive at $t = 1$ also survive at $t = 2$ because the initial shock at $t = 1$ is sufficiently severe. There are then two cases to consider.

First, I consider the case where $F_{S_2}^{c*} = F_1^{c*}$. In this case, the effect of CEM is $F_{S_2}^{c*} - F_{S_2}^* = F_1^{c*} - F_1^*$. From equation (A.25) and (A.6), we know that $F_1^{c*} - F_1^* > 0$ and that an increase in ϕ (i.e. an increase in the severity of credit market frictions) increases $F_1^{c*} - F_1^* > 0$ (strengthens the effect of CEM) because F_1^{c*} decreases less than F_1^* decreases, leading $F_1^{c*} - F_1^*$ to increase. From equation (A.22), we observe that an increase in μ increases V_2 and decreases

$F^*_{S_2}$, making it more likely that we are in the case where $F^*_{S_2} = F^*_1$. In sum, for firms with sufficiently high μ, CEM will be effective and specifically more effective as credit market frictions are more severe.

Second, I consider the case where $F^{c*}_{S_2} = \hat{F}^{c*}_{S_2}$. In this case, the effect of CEM will be smaller than in the case of $F^{c*}_{S_2} = F^{c*}_1$. Because $\hat{F}^{c*}_{S_2} \leq F^{c*}_1$ in this case (such that F^{c*}_1 is not he limiting factor on business survival at $t = 2$) and F^*_1 is continuous in μ, then $\hat{F}^{c*}_{S_2} - F^*_1$ is upper bounded by $F^{c*}_1 - F^*_1$. Specifically, CEM may reduce, have no effect on, or even increase business closure. From equation (A.22), we observe that a decrease in μ decreases V_2 and decreases $F^*_{S_2}$, making it more likely that we are in the case where $F^*_{S_2} = \hat{F}^{c*}_{S_2}$. In sum, for firms with sufficiently low μ, CEM will be less effective.

I next examine how the effectiveness of CEM changes with μ within each of these cases. First, I consider the case where $F^{c*}_{S_2} = F^{c*}_1$. There are two scenarios for the value of F^{c*}_1 as shown in (A.26) and (A.27). Under the first scenario in (A.26), the partial derivative of F^{c*}_1 with respect to μ is:

$$\frac{\partial F^{c*}_1}{\partial \mu} = \frac{1 - \delta + (1-\phi)(1-p)\delta}{(1-\delta)\Delta + (1-\phi)(1-p)\delta\left[(1-\delta)\cdot(\Delta-\gamma) + \delta\cdot\Delta\right]}$$

$$= \frac{1 - \delta + (1-\phi)(1-p)\delta}{(1-\delta)\Delta + (1-\phi)(1-p)\delta A} \tag{A.28}$$

where A denotes the quantity $[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta] > 0$.

Under the second scenario in (A.27), the partial derivative of F^{c*}_1 with respect to μ is:

$$\frac{\partial F^{c*}_1}{\partial \mu} = \frac{(1-\delta) + (1-\phi)\delta}{(1-\delta)\cdot\Delta + (1-\phi)\cdot\delta A} \tag{A.29}$$

As a reminder, the relevant case we focus on is when $F^*_{S_2} = F^*_1$. There are two scenarios for the value of F_1^* as shown in (A.7) and (A.8). Under the first scenario in (A.7), the partial derivative of F_1^* with respect to μ is:

$$\frac{\partial F_1^*}{\partial \mu} = \frac{1-\delta+(1-\phi)(1-p)\delta}{(1-\delta)\Delta+(1-\phi)(1-p)\delta\left[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta\right]}$$

$$= \frac{1-\delta+(1-\phi)(1-p)\delta}{(1-\delta)\Delta+(1-\phi)(1-p)\delta A} \tag{A.30}$$

where A denotes the quantity $[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta] > 0$.

Under the second scenario in (A.8), the partial derivative of F_1^* with respect to μ is:

$$\frac{\partial F_1^*}{\partial \mu} = \frac{(1-\delta)+(1-\phi)\delta}{(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A} \tag{A.31}$$

We will never be in the scenario of (A.27) and (A.7) because $F_1^{c*} > F_1^*$ and it will never be the case that F_1^{c*} takes on the lower scenario value while F_1^* takes on the higher scenario value. If we are in the scenarios of (A.26) and (A.7), then the partial derivative of $F_{S_2}^{c*} - F_{S_2}^*$ with respect to μ is 0. If we are in the scenarios of (A.27) and (A.8), then the partial derivative of $F_{S_2}^{c*} - F_{S_2}^*$ with respect to μ is 0. If we are in the scenarios of (A.26) and (A.8), then the partial derivative of $F^{c*}_{S_2} - F^*_{S_2}$ with respect to μ is:

$$\frac{\partial}{\partial \mu}(F^{c*}_{S_2} - F^*_{S_2}) = \frac{1-\delta+(1-\phi)(1-p)\delta}{(1-\delta)\Delta+(1-\phi)(1-p)\delta A} - \frac{1-\delta+(1-\phi)\delta}{(1-\delta)\Delta+(1-\phi)\delta A}$$

$$= \frac{[1-\delta+(1-\phi)(1-p)\delta]*[(1-\delta)\Delta+(1-\phi)\delta A]}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]}$$

$$- \frac{[1-\delta+(1-\phi)\delta]*[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]}$$

$$= \frac{(1-\delta)(1-\phi)\delta A+(1-\phi)(1-p)\delta(1-\delta)\Delta}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]}$$

$$- \frac{(1-\delta)(1-\phi)(1-p)\delta A+(1-\phi)\delta(1-\delta)\Delta}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]}$$

$$= \frac{p(1-\delta)(1-\phi)\delta A-p(1-\phi)\delta(1-\delta)\Delta}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]}$$

$$= \frac{p(1-\delta)(1-\phi)\delta(A-\Delta)}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]}$$

$$= \frac{p(1-\delta)(1-\phi)\delta\gamma(\delta-1)}{[(1-\delta)\Delta+(1-\phi)(1-p)\delta A]*[(1-\delta)\Delta+(1-\phi)\delta A]} \tag{A.32}$$

In all of the scenarios within this case, $F^{c*}_{S_2} - F^*_{S_2}$ does not change with μ when $\gamma = 0$.

Second, I consider the case where $F_{S_2}^{c*} = \hat{F}_{S_2}^{c*}$. The partial derivative of $F_{S_2}^{c*}$ with respect to μ is:

$$\begin{aligned}
\frac{\partial \hat{F}_{S_2}^{c*}}{\partial \mu} &= \frac{1}{(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta} \\
&= \frac{1}{A} \qquad \text{(A.33)}
\end{aligned}$$

Again, we consider the two scenarios for the value of F_1^* as shown in (A.7) and (A.8). Under the first scenario in (A.7), the partial derivative of F_1^* with respect to μ is:

$$\begin{aligned}
\frac{\partial F_1^{c*}}{\partial \mu} &= \frac{1-\delta+(1-\phi)(1-p)\delta}{(1-\delta)\Delta+(1-\phi)(1-p)\delta\left[(1-\delta)\cdot(\Delta-\gamma)+\delta\cdot\Delta\right]} \\
&= \frac{1-\delta+(1-\phi)(1-p)\delta}{(1-\delta)\Delta+(1-\phi)(1-p)\delta A} \qquad \text{(A.34)}
\end{aligned}$$

Therefore, the partial derivative of $F_{S_2}^{c*} - F_{S_2}^*$ with respect to μ is:

$$\begin{aligned}
\frac{\partial}{\partial \mu}(F_{S_2}^{c*} - F_{S_2}^*) &= \frac{1}{A} - \frac{1-\delta+(1-\phi)(1-p)\delta}{(1-\delta)\Delta+(1-\phi)(1-p)\delta A} \\
&= \frac{(1-\delta)\Delta+(1-\phi)(1-p)\delta A - A(1-\delta+(1-\phi)(1-p)\delta)}{A((1-\delta)\Delta+(1-\phi)(1-p)\delta A)} \\
&= \frac{(1-\delta)(\Delta-A)}{A((1-\delta)\Delta+(1-\phi)(1-p)\delta A)} \\
&= \frac{(1-\delta)(\Delta-(1-\delta)\cdot(\Delta-\gamma)-\delta\cdot\Delta)}{A((1-\delta)\Delta+(1-\phi)(1-p)\delta A)} \\
&= \frac{\gamma(1-\delta)^2}{A((1-\delta)\Delta+(1-\phi)(1-p)\delta A)} \\
&> 0 \qquad \text{(A.35)}
\end{aligned}$$

Under the second scenario in (A.8), the partial derivative of F_1^* with respect to μ is:

$$\frac{\partial F_1^*}{\partial \mu} = \frac{(1-\delta)+(1-\phi)\delta}{(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A} \qquad \text{(A.36)}$$

Therefore, the partial derivative of $F_{S_2}^{c*} - F_{S_2}^{*}$ with respect to μ is:

$$\frac{\partial}{\partial\mu}(F_{S_2}^{c*} - F_{S_2}^{*}) = \frac{1}{A} - \frac{(1-\delta)+(1-\phi)\delta}{(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A}$$

$$= \frac{(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A - A[(1-\delta)+(1-\phi)\delta]}{A[(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A]}$$

$$= \frac{(1-\delta)(\Delta - A)}{A[(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A]}$$

$$= \frac{\gamma(1-\delta)^2}{A[(1-\delta)\cdot\Delta+(1-\phi)\cdot\delta A]}$$

$$> 0 \tag{A.37}$$

In sum, in this case, $F_{S_2}^{c*} - F_{S_2}^{*}$ increases with μ.

A.2 Supplementary Tables and Figures

Figure A.1: Relationship between length of CEM and business closure

This figure presents the binscatter relationship between length of CEM (plus repayment time) and business closure rate in 2020-2022 for unincorporated cities in California.

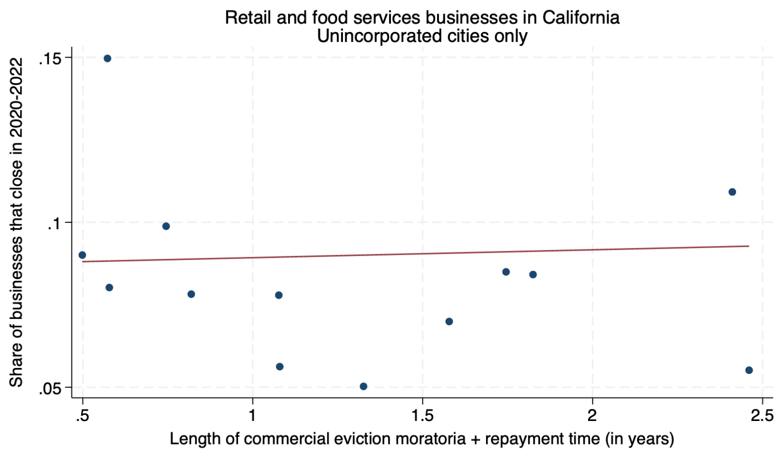


Figure A.2: Robustness check on pre-pandemic growth and business closure effects of CEM: Scaling business closure effects

This figure shows the relationship between the 5-year pre-pandemic employment growth and the effect of CEM on business closure as a percentage of the mean business closure rate in 2020-2022 for subindustries in retail and food services.

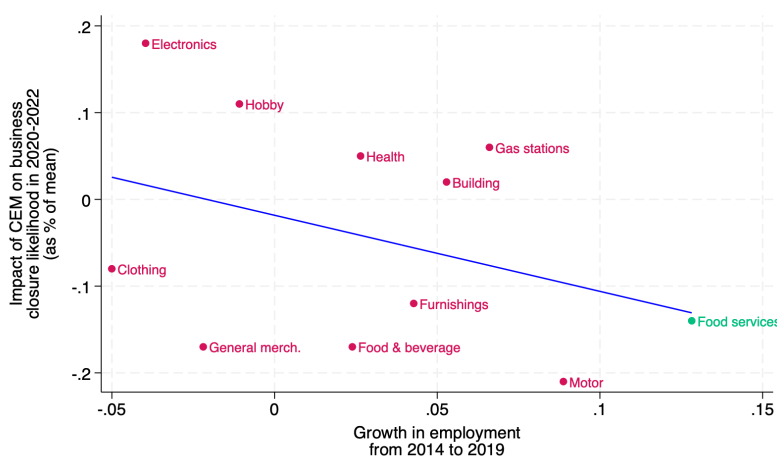


Figure A.3: Robustness check on pre-pandemic growth and business closure effects of CEM: Alternative measure of industry solvency

This figure shows the relationship between pre-pandemic growth in sales and the effect of CEM on business closure for sub-industries in retail and food services.

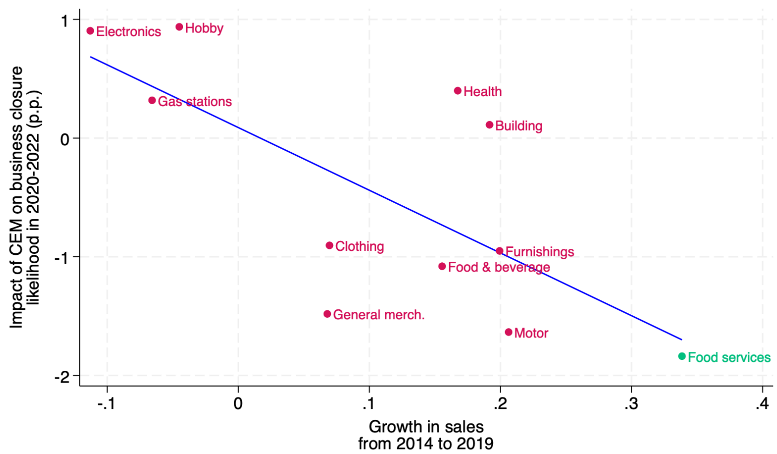


Figure A.4: Robustness check on pre-pandemic growth and business closure effects of CEM: Alternative measure of industry solvency

This figure shows the relationship between pre-pandemic growth in number of firms and the effect of CEM on business closure for sub-industries in retail and food services.

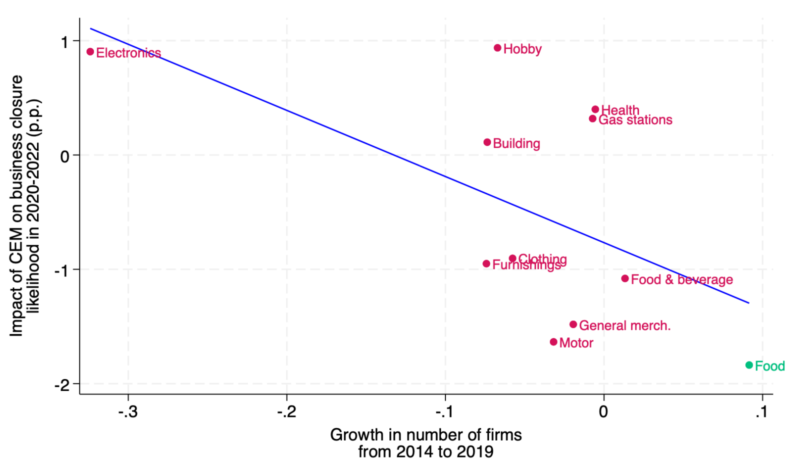


Figure A.5: Robustness check on pre-pandemic growth and business closure effects of CEM: Alternative measure of industry solvency

This figure shows the relationship between pre-pandemic growth in number of establishments and the effect of CEM on business closure for sub-industries in retail and food services.

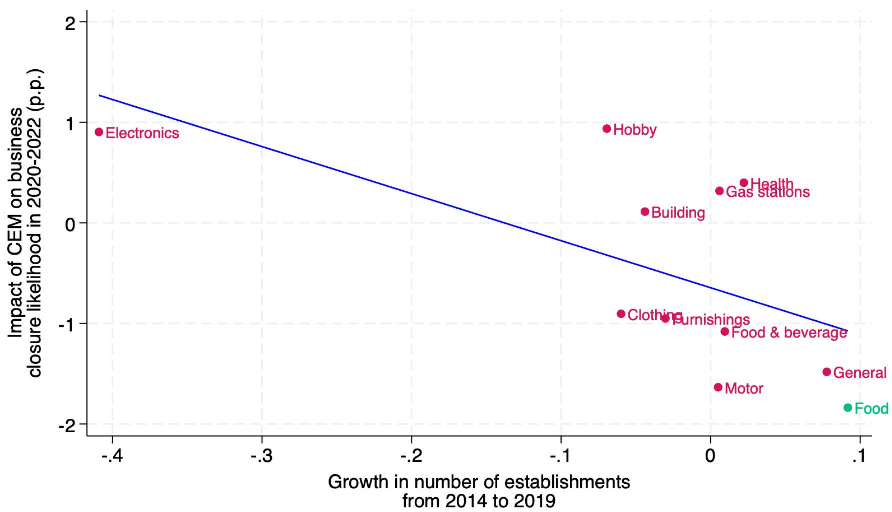


Table A.1: Covariate balance along the axis of pre-pandemic partisanship

This table presents the balance of covariates along the axis of pre-pandemic partisanship. Observations are at the zip code level. For each variable, the first 3 columns provide summary statistics for places with above-median Democratic-Republican spreads, while the next 3 columns provide summary statistics for places with below-median Democratic-Republican spreads. The last column computes the difference in mean and tests whether it is statistically significantly different from zero.

Zip code-level characteristics as of 2019							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Above-median Democratic			Below-median Democratic			
As of 2019:	Number of obs.	Mean	Std dev.	Number of obs.	Mean	Std dev.	Difference in mean
Ln(Population)	626	10.19	0.92	616	10.23	0.90	-0.039
Ln(Per-capita income)	628	10.50	0.58	616	10.48	0.46	0.018
Unemployment rate	628	0.06	0.03	616	0.06	0.03	-0.001
Share of population that is non-white	628	0.45	0.20	616	0.30	0.15	0.15***
Homeownership rate	628	0.48	0.19	616	0.60	0.16	-0.12***
Population density	628	6,998	7,030	616	3,142	3,237	3,856***
Indicator of urban area	628	0.60	0.49	616	0.47	0.50	0.13*
Ln(Number of businesses)	626	6.25	2.11	616	6.02	2.14	0.231**
Share of businesses in retail	628	0.18	0.14	616	0.17	0.12	0.009
Share of businesses in food services	628	0.11	0.11	616	0.10	0.12	0.004

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.2: Alternative channels through which partisanship may affect business closure

This table presents the relationship between pre-pandemic Democratic-Republican spread and the alternative channels through which partisanship may affect business closure during the pandemic. Observations are at the city level.

City-level characteristics			
	(1) Change in foot traffic from January to July-December 2020	(2) Indicator of having a pandemic business grant program	(3) Change in unemployment rate from 2019 to 2020
Democrat-Republican spread of population in 2019	-0.11***	0.11	0.020**
	(0.029)	(0.14)	-0.0086
Constant	-0.42***	0.59***	0.051***
	(0.018)	(0.036)	-0.003
Observations	465	465	465
R-squared	0.041	0.003	0.031

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.3: First stage of pre-pandemic partisanship instrument for commercial eviction moratoria policy: Alternative measure of CEM policy

This table shows the relationship between Democratic-Republican spread and length of CEM (which does not include repayment time R), controlling for zip code-level economic characteristics prior to the pandemic and city-level alternative channels through which partisanship may affect business closure during the pandemic. Observations are at the business level. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of CEM					
Democrat-Republican spread	2.38***	1.54***	1.40***	1.57***	1.41***	1.32***
of population in 2019	(0.37)	(0.36)	(0.36)	(0.34)	(0.32)	(0.35)
City-level characteristics						
Change in foot traffic from January			-1.44			-1.18
to July-December 2020			(1.05)			(0.94)
Indicator of having a pandemic				0.21***		0.13
business grant program				(0.071)		(0.081)
Change in unemployment rate					9.98**	8.92**
from 2019 to 2020					(3.97)	(3.77)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	1.05***	-1.78	-1.27	-1.52	-3.24***	-2.52**
	(0.14)	(1.36)	(1.65)	(1.30)	(1.01)	(1.20)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.345	0.481	0.494	0.489	0.511	0.524
F-statistic	41.1	193.9	220.0	212.8	112.8	155.6

*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Impact of commercial eviction moratoria on business closure: Alternative measure of CEM policy

This table presents the second stage estimate of the impact of CEM on business closure. The strength of CEM policy is measured length of CEM (which does not include repayment time R). Observations are businesses in the industries of retail and food services. The first three columns progress from the OLS specification to the IV specification, while the last three columns decompose the IV estimate by year. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of business closing in: 2020-2022			Likelihood of business closing in: 2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM	0.68***	0.098	-0.94*	-0.35*	-0.64***	0.056
	(0.099)	(0.13)	(0.54)	(0.20)	(0.20)	(0.26)
City-level characteristics						
Change in foot traffic from January to July-December 2020			-2.24	-0.65	-1.56*	-0.020
			(1.44)	(0.61)	(0.83)	(0.72)
Indicator of having a pandemic business grant program			0.088	0.0094	0.049	0.029
			(0.28)	(0.069)	(0.10)	(0.19)
Change in unemployment rate from 2019 to 2020			14.7	3.35	5.52	5.79
			(9.71)	(3.16)	(3.53)	(4.70)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	8.82***	-11.0*	-14.7**	-4.49**	-3.79***	-6.37
	(0.20)	(5.91)	(6.48)	(2.18)	(1.30)	(4.14)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.000	0.003				

*** p<0.01, ** p<0.05, * p<0.1

Table A.5: Impact of commercial eviction moratoria on business closure: Retail v.s. food services: Alternative measure of CEM policy

This table presents the second stage estimate of the impact of CEM on business closure by industry and by year. The strength of CEM policy is measured length of CEM (which does not include repayment time R). Observations are businesses in the industries of retail and food services. In the first four columns, I present the result for the retail industry, while in the last four columns, I present the result for the food services industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail				Food services			
	Likelihood of business closing in:				Likelihood of business closing in:			
	2020-2022	2020	2021	2022	2020-2022	2020	2021	2022
Years of CEM (instrumented)	-0.36	-0.27	-0.50***	0.41**	-2.16**	-0.57*	-0.90***	-0.69
	(0.42)	(0.18)	(0.19)	(0.19)	(0.88)	(0.32)	(0.30)	(0.56)
City-level characteristics								
Change in foot traffic from January to July-December 2020	-0.56	-0.43	-1.07	0.94	-6.36**	-1.39	-2.60**	-2.38*
	(1.12)	(0.51)	(0.76)	(1.20)	(3.19)	(1.16)	(1.13)	(1.39)
Indicator of having a pandemic business grant program	0.064	0.13	0.069	-0.14	0.0094	-0.25*	-0.015	0.28
	(0.27)	(0.099)	(0.11)	(0.18)	(0.46)	(0.14)	(0.13)	(0.30)
Change in unemployment rate from 2019 to 2020	12.1**	1.71	4.85	5.53*	23.7	7.87	7.16	8.66
	(6.16)	(3.43)	(3.17)	(3.29)	(20.1)	(4.90)	(5.32)	(12.7)
Zip code-level characteristics as of 2019	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-9.93*	-2.74	-2.26*	-4.93*	-24.3**	-8.37***	-5.74**	-10.1
	(5.22)	(2.07)	(1.33)	(2.98)	(10.9)	(3.21)	(2.35)	(8.07)
Observations	218,335	218,335	218,335	218,335	122,008	122,008	122,008	122,008

*** p<0.01, ** p<0.05, * p<0.1

Table A.6: First stage of partisanship instrument for commercial eviction moratoria: Control non-parametrically for alternative channels

This table shows the relationship between Democratic-Republican spread and length of CEM+R. The columns control for zip code-level economic characteristics prior to the pandemic and city-level alternative channels through which partisanship may affect business closure during the pandemic. City-level characteristics are controlled for non-parametrically by including quantile fixed effects. Observations are at the business level. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of CEM+R					
Democrat-Republican spread of population in 2019	2.77***	1.85***	1.40***	1.89***	1.70***	1.41***
	(0.29)	(0.43)	(0.44)	(0.41)	(0.42)	(0.48)
City-level characteristics						
Change in foot traffic from January to July-December 2020: Quantile fixed effects			Yes			Yes
Indicator of having a pandemic business grant program				Yes		Yes
Change in unemployment rate from 2019 to 2020: Quantile fixed effects					Yes	Yes
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	1.29***	-2.25	1.23	-1.92	-3.19	0.91
	(0.18)	(2.16)	(3.19)	(2.09)	(2.07)	(2.84)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.345	0.467	0.540	0.477	0.483	0.555
F-statistic	88.4	102.6	179.1	117.0	77.7	239.8

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.7: Impact of commercial eviction moratoria on business closure: Control non-parametrically for alternative channels

This table presents the second stage estimate of the impact of CEM on business closure. The strength of CEM policy is measured length of CEM+R. City-level characteristics are controlled for non-parametrically by including quantile fixed effects. Observations are businesses in the industries of retail and food services. The first three columns progress from the OLS specification to the IV specification, while the last three columns decompose the IV estimate by year. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of business closing in: 2020-2022			Likelihood of business closing in:		
				2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM+R	0.65***	0.11	-0.86	-0.30	-0.58***	0.022
	(0.079)	(0.12)	(0.57)	(0.19)	(0.21)	(0.26)
City-level characteristics						
Change in foot traffic from January to July-December 2020: Quantile fixed effects			Yes	Yes	Yes	Yes
Indicator of having a pandemic business grant program			Yes	Yes	Yes	Yes
Change in unemployment rate from 2019 to 2020: Quantile fixed effects			Yes	Yes	Yes	Yes
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	8.66***	-10.9*	-11.8*	-3.09*	-1.47	-7.26
	(0.20)	(6.08)	(6.16)	(1.81)	(1.81)	(4.56)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.001	0.003				

*** p<0.01, ** p<0.05, * p<0.1

Table A.8: Impact of commercial eviction moratoria on business closure: Retail v.s. food services: Control non-parametrically for alternative channels

This table presents the second stage estimate of the impact of CEM on business closure by industry and by year. The strength of CEM policy is measured length of CEM+R. City-level characteristics are controlled for non-parametrically by including quantile fixed effects. Observations are businesses in the industries of retail and food services. Observations are businesses in the industries of retail and food services. In the first four columns, I present the result for the retail industry, while in the last four columns, I present the result for the food services industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail				Food services			
	Likelihood of business closing in:				Likelihood of business closing in:			
	2020-2022	2020	2021	2022	2020-2022	2020	2021	2022
Years of CEM+R (instrumented)	-0.33	-0.20	-0.47**	0.34*	-2.02**	-0.54*	-0.81***	-0.67
	(0.40)	(0.16)	(0.21)	(0.18)	(1.01)	(0.29)	(0.26)	(0.65)
City-level characteristics								
Change in foot traffic from January to July-December 2020: Quantile fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indicator of having a pandemic business grant program	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Change in unemployment rate from 2019 to 2020: Quantile fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip code-level characteristics as of 2019	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-9.53*	-1.60	-0.67	-7.26*	-16.5*	-6.22*	-2.29	-8.00
	(5.66)	(1.58)	(1.69)	(4.21)	(9.32)	(3.20)	(2.78)	(6.93)
Observations	218,335	218,335	218,335	218,335	122,008	122,008	122,008	122,008

*** p<0.01, ** p<0.05, * p<0.1

Table A.9: First stage of partisanship instrument for commercial eviction moratoria: Control non-parametrically for alternative channels & pre-pandemic characteristics

This table shows the relationship between Democratic-Republican spread and length of CEM+R, controlling for zip code-level economic characteristics prior to the pandemic and city-level alternative channels through which partisanship may affect business closure during the pandemic. City-level and zip code-level characteristics are controlled for non-parametrically by including quantile fixed effects. Observations are at the business level. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of CEM+R					
Democrat-Republican spread of population in 2019	2.77***	1.74***	1.33***	1.77***	1.60***	1.34**
	(0.29)	(0.44)	(0.46)	(0.44)	(0.50)	(0.51)
City-level characteristics						
Change in foot traffic from January to July-December 2020: Quantile fixed effects			Yes			Yes
Indicator of having a pandemic business grant program				Yes		Yes
Change in unemployment rate from 2019 to 2020: Quantile fixed effects					Yes	Yes
Zip code-level characteristics as of 2019: Quantile fixed effects		Yes	Yes	Yes	Yes	Yes
Constant	1.29***	0.52**	1.37***	0.43*	0.47*	1.24**
	(0.18)	(0.25)	(0.49)	(0.25)	(0.27)	(0.47)
Observations	340,343	340,343	340,343	340,343	340,343	340,343
R-squared	0.345	0.488	0.557	0.494	0.499	0.567
F-statistic	88.4	1066.2	62135.4	1862.9	9223.4	63207.5

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.10: Impact of commercial eviction moratoria on business closure: Control non-parametrically for alternative channels & pre-pandemic characteristics

This table presents the second stage estimate of the impact of CEM on business closure. The strength of CEM policy is measured length of CEM+R. City-level and zip code-level characteristics are controlled for non-parametrically by including quantile fixed effects. Observations are businesses in the industries of retail and food services. The first three columns progress from the OLS specification to the IV specification, while the last three columns decompose the IV estimate by year. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of business closing in: 2020-2022			Likelihood of business closing in:		
				2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM+R	0.65***	0.14	-0.89	-0.30*	-0.58**	-0.015
	(0.079)	(0.12)	(0.64)	(0.18)	(0.23)	(0.31)
City-level characteristics						
Change in foot traffic from January to July-December 2020: Quantile fixed effects			Yes	Yes	Yes	Yes
Indicator of having a pandemic business grant program			Yes	Yes	Yes	Yes
Change in unemployment rate from 2019 to 2020: Quantile fixed effects			Yes	Yes	Yes	Yes
Zip code-level characteristics as of 2019: Quantile fixed effects		Yes	Yes	Yes	Yes	Yes
Constant	8.66***	6.94***	8.47***	1.89***	2.19***	4.39***
	(0.20)	(0.78)	(1.38)	(0.43)	(0.47)	(0.72)
Observations	340,343	340,343	340,343	340,343	340,343	340,343

*** p<0.01, ** p<0.05, * p<0.1

Table A.11: Impact of commercial eviction moratoria on business closure: Retail v.s. food services: Control non-parametrically for alternative channels & pre-pandemic characteristics

This table presents the second stage estimate of the impact of CEM on business closure by industry and by year. The strength of CEM policy is measured length of CEM+R. City-level and zip code-level characteristics are controlled for non-parametrically by using quantile fixed effects. Observations are businesses in the industries of retail and food services. Observations are businesses in the industries of retail and food services. In the first four columns, I present the result for the retail industry, while in the last four columns, I present the result for the food services industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail				Food services			
	Likelihood of business closing in:				Likelihood of business closing in:			
	2020-2022	2020	2021	2022	2020-2022	2020	2021	2022
Years of CEM (instrumented)	-0.41	-0.22	-0.45**	0.27	-2.05*	-0.51*	-0.84**	-0.70
	(0.48)	(0.18)	(0.19)	(0.23)	(1.14)	(0.27)	(0.33)	(0.67)
City-level characteristics								
Change in foot traffic from January to July-December 2020: Quantile fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indicator of having a pandemic business grant program	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Change in unemployment rate from 2019 to 2020: Quantile fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip code-level characteristics as of 2019: Quantile fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.40***	1.31***	1.60***	2.50***	13.8***	2.96***	3.20***	7.66***
	(1.28)	(0.43)	(0.43)	(0.65)	(2.10)	(0.70)	(0.69)	(1.16)
Observations	218,335	218,335	218,335	218,335	122,008	122,008	122,008	122,008

*** p<0.01, ** p<0.05, * p<0.1

Table A.12: Impact of CEM on number of shifts

This table presents the second stage estimate of the impact of CEM on business employment. Observations are businesses in the industries of retail and food services. The measure of employment is number of shifts worked scaled by number of shifts worked in January 2020. The first three columns progress from the OLS specification to the IV specification, while the last three columns decompose the IV estimate by year. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Scaled number of shifts in: 2020-2022			Scaled number of shifts in:		
				2020	2021	2022
Specification:	OLS	OLS	IV	IV	IV	IV
Years of CEM+R	-0.014*	0.0046	0.0034	-0.0013	0.036	0.032
	(0.0079)	(0.0070)	(0.022)	(0.022)	(0.027)	(0.031)
City-level characteristics						
Change in foot traffic from January to July-December 2020			0.31**	0.23**	0.41***	0.32**
			(0.12)	(0.092)	(0.13)	(0.16)
Indicator of having a pandemic business grant program			0.0019	0.0033	-0.0034	0.016
			(0.020)	(0.018)	(0.019)	(0.023)
Change in unemployment rate from 2019 to 2020			0.17	0.41	0.29	-0.71
			(0.40)	(0.36)	(0.42)	(0.49)
Zip code-level characteristics as of 2019		Yes	Yes	Yes	Yes	Yes
Constant	0.98***	1.88***	1.77***	1.50***	1.91***	1.89***
	(0.015)	(0.43)	(0.38)	(0.18)	(0.38)	(0.58)
Observations	2,688	2,688	2,688	4,242	3,180	2,688
R-squared	0.001	0.018				

*** p<0.01, ** p<0.05, * p<0.1

Table A.13: Impact of CEM on number of shifts: Retail v.s. food services

This table presents the second stage estimate of the impact of CEM on business employment by industry and by year. Observations are businesses in the industries of retail and food services. The measure of employment is number of shifts worked scaled by number of shifts worked in January 2020. In the first four columns, I present the result for the retail industry, while in the last four columns, I present the result for the food services industry. Zip code-level controls include log population, log per-capita income, unemployment rate, proportion of population that is non-white, homeownership rate, population density, indicator of urban area, log number of businesses, share of businesses in retail, and share of businesses in food services as of 2019. Standard errors are clustered by county, and robust standard errors are given in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail				Food services			
	Scaled number of shifts in:				Scaled number of shifts in:			
	2020-2022	2020	2021	2022	2020-2022	2020	2021	2022
Years of CEM+R (instrumented)	-0.015	0.015	0.022	-0.0056	0.0052	-0.0046	0.036	0.038
	(0.040)	(0.046)	(0.041)	(0.053)	(0.023)	(0.019)	(0.028)	(0.036)
City-level characteristics								
Change in foot traffic from January to July-December 2020	0.37	0.28	0.43	0.38	0.29***	0.23***	0.39***	0.29**
	(0.30)	(0.23)	(0.29)	(0.45)	(0.10)	(0.078)	(0.12)	(0.13)
Indicator of having a pandemic business grant program	-0.079*	0.027	-0.12***	-0.13*	0.016	-0.0022	0.019	0.040*
	(0.048)	(0.026)	(0.040)	(0.065)	(0.022)	(0.021)	(0.023)	(0.024)
Change in unemployment rate from 2019 to 2020	-0.51	-0.19	-0.34	-0.77	0.37	0.53	0.54	-0.61
	(1.13)	(0.85)	(1.23)	(1.04)	(0.35)	(0.34)	(0.41)	(0.52)
Zip code-level characteristics as of 2019	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-0.34	0.85*	-0.35	-0.82	2.19***	1.67***	2.37***	2.45***
	(0.83)	(0.51)	(1.23)	(1.14)	(0.42)	(0.21)	(0.39)	(0.62)
Observations	487	787	580	487	2,201	3,455	2,600	2,201

*** p<0.01, ** p<0.05, * p<0.1

Appendix B

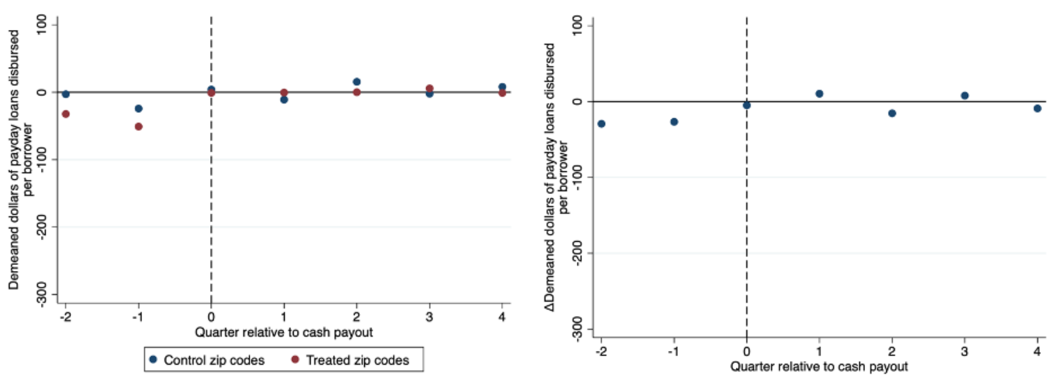
Appendix to Chapter 2

B.1 Supplementary Tables and Figures

Figure B.1: Household demand for payday loans and installment loans in response to cash payouts

This figure presents the results of our zip code x quarter level analysis of payday borrowing and installment borrowing around lawsuit cash settlement payouts. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. It is not possible to conduct the exact same difference-in-differences analysis around cash disbursement because both our treated group (HTLR-banned bank customers) and control (non-HTLR-banned bank customers) received cash payments. However, we do a similar exercise below by computing event time around cash payments for the same treated and control zip codes of our main tests and then demeaning the outcome variable by neighborhood and quarter. In Panel A, the outcome variable is the dollar amount of payday loans disbursed per payday borrower in the Clarity dataset. In Panel B, the outcome variable is the dollar amount of installment loans disbursed per lowest-income-quintile installment borrower in the Equifax dataset. Values are plotted from the 2 preceding quarters to the 4 following quarters around the cash payout for a comparison of zip codes that contain branches of a bank that was required to cease high-to-low reordering relative to zip codes that are within 7 miles and that contain branches of a bank that was sued but not required to cease high-to-low reordering. Neighborhood x quarter fixed effects are included. Each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

(a) Payday loans in Clarity data



(b) Installment loans to lowest-income-quintile borrowers in Equifax data

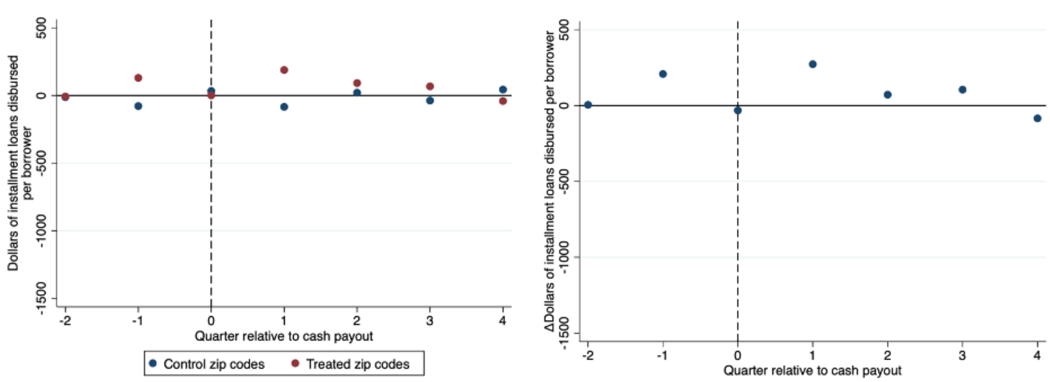


Table B.1: Key events for lawsuits lodged against banks for high-to-low transaction reordering

This table presents key data for our sample of banks that were sued by their customers for practicing high-to-low reordering of transactions posted to deposit accounts. The date of lawsuit filing is the date when the lawsuit was initially filed. The date of settlement approval is the date when the litigation reached a final settlement. The date of high-to-low reordering ban is the date stated in official court documents (often in the settlement agreement itself) when the defendant bank must cease the practice of high-low transaction reordering. A blank value indicates that the sued bank was not subject to a high-to-low reordering ban. The date of cash settlement disbursement is the date that the cash settlement is paid out to eligible customers. The total cash settlement is the gross amount of cash relief stipulated in the settlement agreement. The net cash settlement is the total cash settlement minus attorney attorney fees, litigation expenses, and class representative service awards, all of which were paid out before consumers received their allotment. The number of settlement class members is the number of consumers that were identified to be impacted by high-to-low reordering and therefore eligible recipients of cash relief. A blank value indicates that the data could not be found in court documents.

Bank	Date of lawsuit filing	Date of settlement approval	Date of high-to-low reordering ban	Date of cash settlement disbursement	Total cash settlement	Net cash settlement	Number of settlement class members	Average cash settlement per member
Associated Bank	2-Apr-2010	2-Aug-2013	1-Feb-2011	21-Sep-2013	$13,000,000	$8,988,744	197,050	$45.62
Banco Popular North America	14-Nov-2012	7-Aug-2018	1-Aug-2013	7-Oct-2018	$5,200,000	$5,200,000	50,683	$102.60
BancorpSouth Bank	18-May-2010	15-Jul-2016		18-Sep-2016	$24,500,000	$15,576,395	190,541	$81.75
Bank of America	1-Dec-2008	22-Nov-2011		29-Jul-2012	$410,000,000	$286,900,000	13,280,225	$21.60
Bank of the West	5-Apr-2010	18-Dec-2012	1-Jul-2011	6-Feb-2013	$18,000,000	$12,401,565	386,113	$32.12
BOKF	17-Aug-2010	13-Sep-2012		2-Nov-2012	$19,000,000	$13,206,863	270,603	$48.81
Capital One	18-May-2010	22-May-2015		25-Aug-2015	$31,767,200	$20,923,177	594,852	$35.17
Citibank	19-Dec-2011	14-Nov-2014		18-Jan-2015	$5,000,000	$3,717,746	820,438	$4.53
Citizens	26-Jan-2010	12-Mar-2013	30-Jun-2013	14-Aug-2013	$137,500,000	$95,954,565	2,143,489	$44.77
Comerica Bank	17-Feb-2010	10-Jun-2014		22-Nov-2014	$14,580,000	$9,824,974	184,731	$53.19
Commerce Bank	6-Apr-2010	2-Aug-2013	29-Mar-2013	5-Nov-2013	$18,300,000	$11,226,066	393,169	$28.55
Community Bank	20-Jul-2012	25-Nov-2013	1-Mar-2011	14-Jan-2014	$2,500,000	$1,628,634	48,976	$33.25
Compass Bank	4-May-2010	7-Aug-2013	12-Mar-2013	10-Nov-2013	$11,500,000	$7,938,700	826,481	$9.61
Fifth Third Bancorp	21-Oct-2009	29-Jul-2011	1-Apr-2011	12-Dec-2011	$9,500,000	$6,277,527	4,137,612	$1.52
Great Western Bank	15-Jun-2010	2-Aug-2013	1-Jul-2010	5-Nov-2013	$2,200,000	$1,448,425	27,490	$52.69
Harris	23-Apr-2010	5-Aug-2013	31-Mar-2013	8-Dec-2013	$9,400,000	$6,463,638	116,132	$55.66
HSBC Bank USA	1-Mar-2011	18-Oct-2016		31-Jan-2017	$32,000,000	$23,515,249	230,198	$102.15
IBERIABANK Corporation	18-Feb-2011	26-Apr-2012	1-Nov-2011	15-Jun-2012	$2,500,000	$1,802,500	51,652	$34.90
Independent Bank Corporation	31-Jul-2013	11-Jan-2018		2-Mar-2018	$2,215,000	$2,215,000		
JPMorgan Chase Bank	24-Jul-2009	19-Dec-2012	29-Mar-2010	3-May-2013	$110,000,000	$61,050,674	5,330,157	$11.45
M & I Marshall & Ilsley Bank	16-Jun-2010	2-Aug-2013	31-Mar-2013	21-Sep-2013	$4,000,000	$2,727,638	189,560	$14.39
M&T Bank	21-Aug-2009	13-Mar-2015	1-Jan-2013	2-May-2015	$4,000,000	$2,702,374	497,902	$5.43
National City Bank	17-Feb-2010	1-Dec-2011		8-Apr-2012	$13,800,000	$10,707,143	2,430,182	$4.41
Northwest Savings Bank	7-May-2012	7-Apr-2015	1-Jul-2011	27-May-2015	$2,350,000	$1,630,770	35,365	$46.11
PNC Bank	8-Oct-2009	5-Aug-2013	5-Aug-2013	23-Nov-2013	$90,000,000	$62,786,698	960,534	$65.37
RBC Bank (USA)	2-Jul-2010				$7,500,000	$4,772,101	148,437	$32.15
Susquehanna Bank	29-Jul-2011	1-Apr-2014	1-Oct-2011	21-May-2014	$3,680,000	$2,452,682	60,737	$40.38
Synovus Bank	21-Sep-2010	2-Apr-2015		21-Jun-2015	$3,750,000	$2,509,688	44,244	$56.72
TD Bank	15-Dec-2009	18-Mar-2013		22-May-2013	$62,000,000	$43,148,765	1,006,309	$42.88
TD Bank, including Carolina First Bank and Mercantile Bank	21-Aug-2013	24-Jan-2020		29-Mar-2020	$70,000,000	$48,085,000	4,723,942	$10.18
Trustmark National Bank	2-Dec-2011	25-Mar-2014	25-Mar-2014	18-Jul-2014	$4,000,000	$2,450,454	141,237	$17.35
U.S. Bank	17-Apr-2009	3-Jan-2014	24-Jul-2013	28-Aug-2014	$55,000,000	$38,260,915	2,712,743	$14.10
Umpqua Bank	29-Dec-2011	28-Apr-2015		17-Jul-2015	$2,900,000	$2,054,310	41,633	$49.34
Union Bank	16-Jul-2009	4-Oct-2012	1-Aug-2010	23-Nov-2012	$35,000,000	$24,233,342	307,324	$78.85
Webster Bank	29-Apr-2010	28-Mar-2011	30-Sep-2010	1-Jun-2011	$2,800,000	$2,090,423	103,410	$20.21
Wells Fargo & Company	21-Nov-2007	5-Aug-2013	1-Jan-2010	18-Apr-2016	$203,000,000	$183,900,000	1,144,577	$160.67
Woodforest National Bank	11-Jan-2012	19-May-2014	1-Mar-2010	8-Jul-2014	$7,750,000	$5,133,341	231,942	$22.13

Table B.2: Bank overdraft activity in response to high-to-low reordering bans

This table presents the results of a bank x quarter level difference-in-differences analysis using FFIEC Call Report data. In the first column, the outcome variable is the growth in overdraft-related revenue (proxied by deposit account service charges). In the second column, the outcome variable is the growth in overdraft balances (proxied by the all other loans category). In the last column, the outcome variable is the growth in number of FDIC-insured depositors. HTLR Ban is a dummy variable that takes on a value of 1 if the bank is required to cease high-to-low reordering, and a value of 0 if the bank is sued but not required to cease high-to-low reordering. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Bank fixed effects and quarter fixed effects are included. Standard errors are clustered at the bank level and the quarter level.

	Growth in:		
	Overdraft-related revenue	Other loans	Number of depositors
HTLR Ban x Post	-0.0644*	-0.213**	0.0303
	(0.0333)	(0.104)	(0.0267)
Bank fixed effects	Y	Y	Y
Quarter fixed effects	Y	Y	Y
Observations	540	685	685
R-squared	0.960	0.822	0.826

*** p<0.01, ** p<0.05, * p<0.1

Table B.3: The entry and exit of non-high-to-low-reordering-banned banks in response to the exit of high-to-low-reordering-banned banks

This table presents the results of our zip code x bank x year level difference-in-differences analysis. In the first three columns, the outcome variable is a dummy variable that takes on a value of 1 if a non-high-to-low-reordering-banned bank enters the zip code in the given year, and a value of 0 otherwise. In the last three columns, the outcome variable is a dummy variable that takes on a value of 1 if a non-high-to-low-reordering-banned bank exits the zip code in the given year, and a value of 0 otherwise. HTLR Bank Exit is a dummy variable that takes on a value of 1 if the zip code experiences the exit of a high-to-low-reordering-banned bank, and a value of 0 otherwise. Post is a dummy variable that takes on a value of 1 in the years after the exit, and a value of 0 in the years leading up to the exit. We examine a 3-year window around the exit. Varying levels of fixed effects are included. Standard errors are clustered at the year level and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Entry of non-high-to-low reordering-banned bank			Exit of non-high-to-low reordering-banned bank		
HTLR Bank Exit x Post	0.00336	0.00370	-0.00536	-0.0187	-0.0114	-9.45e-05
	(0.00331)	(0.00288)	(0.00668)	(0.0136)	(0.0131)	(0.0137)
Neighborhood fixed effects	Y	Y	N	Y	Y	N
Year fixed effects	Y	Y	N	Y	Y	N
Neighborhood x year fixed effects	N	N	Y	N	N	Y
Bank x zip code fixed effects	N	N	Y	N	N	Y
Observations	171,960	171,960	171,248	171,960	171,960	171,248
R-squared	0.161	0.163	0.259	0.185	0.243	0.372

*** p<0.01, ** p<0.05, * p<0.1

Table B.4: The overdraft activity of non-high-to-low-reordering-banned banks in response to the exit of high-to-low-reordering-banned banks

This table presents the results of our bank x quarter level difference-in-differences analysis using FFIEC Call Report data. In the first column, the outcome variable is the growth in overdraft-related revenue (proxied by deposit account service charges). In the second column, the outcome variable is the growth in overdraft balances (proxied by the all other loans category). In the last column, the outcome variable is the growth in number of FDIC-insured depositors. HTLR Bank Exit is a dummy variable that takes on a value of 1 for banks that operate in zip codes with a high-to-low-reordering-banned bank exit, and a value of 0 for banks that operate in neighboring zip codes with no high-to-low-reordering-banned bank exit. Post is a dummy variable that takes on a value of 1 in the quarters after the exit, and a value of 0 in the quarters leading up to the exit. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Quarter fixed effects and bank fixed effects are included. Standard errors are clustered at the bank level and the quarter level.

	Growth in:		
	Overdraft-related revenue	Other loans	Number of depositors
HTLR Bank Exit x Post	0.0121	-0.171	-0.00244
	(0.0193)	(0.155)	(0.00633)
Bank fixed effects	Y	Y	Y
Quarter fixed effects	Y	Y	Y
Observations	5,310	5,310	5,208
R-squared	0.815	0.378	0.825

*** p<0.01, ** p<0.05, * p<0.1

Table B.5: Relationship between HTLR ban treatment and zip code characteristics

This table tests whether there is a relationship between whether a zip code gets treated and the characteristics of the zip code. The outcome variable is an indicator of whether the zip code gets treated, i.e. whether the zip code contains branches belonging to a bank treated with a HTLR ban. The independent variables include zip code-level measures of population size, ethnic minority fraction, education, family status, marital status, poverty status, income, employment, and housing status from the five-year American Community Survey conducted by the Census Bureau.

	Indicator for whether zip code is treated
Log(Total population)	0.0583
	(0.0528)
Proportion of population that is non-white	-0.168
	(0.233)
Proportion of population with high school diploma	-0.638
	(0.912)
Proportion of population that is college educated	0.403
	(0.837)
Proportion of households that are family households	-0.447
	(0.623)
Proportion of population that is separated or divorced	0.751
	(1.375)
Proportion of population that is below the poverty line	0.154
	(0.959)
Median annual household income	-2.52e-06
	(1.50e-05)
Proportion of population receiving government aid	0.0612
	(0.864)
Per capita annual income	3.19e-06
	(1.86e-05)
Proportion of labor force that is unemployed	-0.354
	(1.256)
Proportion of occupied housing units that are renter occupied	0.0692
	(0.340)
Constant	-0.300
	(6.447)
Observations	4,557
R-squared	0.629

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.6: Relationship between HTLR ban treatment and bank characteristics

This table tests whether there is a relationship between whether a bank gets treated with a HTLR ban and the characteristics of the bank. The outcome variable is an indicator of whether the bank gets treated with a HTLR ban. The independent variables include bank size, return on assets, and fraction of overdraft-related revenue (proxied by deposit account service charges).

	Indicator for whether bank is treated with HTLR Ban	
Log(Total assets)	-0.0498	-0.0320
	(0.0579)	(0.0554)
Return on assets	0.114	0.0967
	(0.0891)	(0.0980)
Overdraft-related revenue / total revenue		0.0576
		(0.0610)
Constant	0.722***	0.722***
	(0.0789)	(0.0801)
Observations	36	36
R-squared	0.076	0.089

*** p<0.01, ** p<0.05, * p<0.1

Table B.7: Household demand for payday loans and installment loans in response to high-to-low reordering bans: States with no payday lending bans

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. Additionally, we exclude observations in states with payday bans. In the first two columns, the outcome variable is the dollar amount and the number of payday loans disbursed per payday borrower. In the next two columns, the outcome variable is the dollar amount and the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included across specifications. Standard errors are clustered at the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Payday loans disbursed per payday borrower		Installment loans disbursed per low-income installment borrower	
	Dollar amount	Number	Dollar amount	Number
HTLR Ban x Post	-83.34***	-0.289***	-334.0**	-0.0180
	(23.98)	(0.0718)	(142.8)	(0.0208)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	5,549	5,549	29,049	29,049
R-squared	0.404	0.382	0.276	0.295

*** p<0.01, ** p<0.05, * p<0.1

Table B.8: Bank non-overdraft loan activity in response to high-to-low reordering bans

This table presents the results of a bank x quarter level difference-in-differences analysis using FFIEC Call Report data. In the first column, the outcome variable is the growth in commercial & industrial loans. In the second column, the outcome variable is the growth in commercial real estate loans. HTLR Ban is a dummy variable that takes on a value of 1 if the bank is required to cease high-to-low reordering, and a value of 0 if the bank is sued but not required to cease high-to-low reordering. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Bank fixed effects and quarter fixed effects are included. Standard errors are clustered at the bank level and the quarter level.

	Growth in:	
	Commercial & industrial loans	Commercial real estate loans
HTLR Ban x Post	-0.0301	-0.175
	(0.0440)	(0.262)
Bank fixed effects	Y	Y
Quarter fixed effects	Y	Y
Observations	652	555
R-squared	0.839	0.892

*** p<0.01, ** p<0.05, * p<0.1

Table B.9: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Different measures of consumers of interest

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. Panel A examines payday borrowing. The outcome variable is the dollars of payday loans disbursed divided by the number of payday borrowers, number of alternative borrowers, number of payday borrowers prior to the window of analysis, or number of low-income individuals prior to the window of analysis. Panel B examines installment borrowing. The outcome variable is the dollars of installment borrowing divided by number of low-income installment borrowers, number of low-income borrowers, number of low-income installment borrowers prior to the window of analysis, or number of low-income individuals prior to the window of analysis. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but *not* required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

Panel A: Payday borrowing				
	Dollars of payday loans disbursed divided by:			
	Number of payday borrowers	Number of alternative borrowers	Pre-event-window number of payday borrowers	Pre-event-window number of of low-income individuals
HTLR Ban x Post	-84.84***	-83.50***	-135.4**	-0.0226**
	(31.47)	(30.08)	(53.21)	(0.0115)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	5,574	5,574	5,574	5,574
R-squared	0.408	0.461	0.426	0.740

Panel B: Installment borrowing				
	Dollars of installment loans divided by:			
	Number of low-income installment borrowers	Number of low-income borrowers	Pre-event-window number of low-income installment borrowers	Pre-event-window number of of low-income individuals
HTLR Ban x Post	-358.3***	-310.4**	-411.3**	-0.759**
	(135.8)	(122.8)	(206.0)	(0.339)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	30,487	30,487	30,268	30,487
R-squared	0.278	0.313	0.255	0.577

*** p<0.01, ** p<0.05, * p<0.1

Table B.10: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Alternative control groups

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In Panel A, the outcome variable is the dollar amount or the number of payday loans disbursed per payday borrower. In Panel B, the outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that is required to cease high-to-low reordering, and a value of 0 if the zip code contains no high-to-low-reordering-banned branches. In the specifications with neighborhood x quarter fixed effects, the control group now additionally includes eventually treated zip codes. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. In specifications with neighborhood x quarter fixed effects, standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other. In specifications with state x quarter fixed effects, standard errors are clustered at the quarter level and the state level.

Panel A: Payday loans				
	Payday loans disbursed per payday borrower			
	Dollar amount		Number	
HTLR Ban x Post	-84.84***	-59.74**	-0.289***	-0.243***
	(31.44)	(26.35)	(0.0904)	(0.0682)
Neighborhood x quarter fixed effects	Y	N	Y	N
State x quarter fixed effects	N	Y	N	Y
Observations	9,870	7,136	9,870	7,136
R-squared	0.421	0.242	0.407	0.293
Panel B: Installment loans				
	Installment loans disbursed per low-income installment borrower			
	Dollar amount		Number	
HTLR Ban x Post	-367.5***	-303.9***	-0.0264	-0.0164
	(136.2)	(101.1)	(0.0205)	(0.0154)
Neighborhood x quarter fixed effects	Y	N	Y	N
State x quarter fixed effects	N	Y	N	Y
Observations	31,083	38,883	31,083	38,883
R-squared	0.278	0.098	0.295	0.105

*** p<0.01, ** p<0.05, * p<0.1

Table B.11: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Alternative control groups by varying neighborhood radii

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In Panel A, the outcome variable is the dollar amount or the number of payday loans disbursed per payday borrower. In Panel B, the outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that is required to cease high-to-low reordering, and a value of 0 if the zip code contains no high-to-low-reordering-banned branches and the zip code lies within the stated neighborhood radius of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 5 miles or 10 miles of each other.

Panel A: Payday loans				
	Payday loans disbursed per payday borrower			
	Dollar amount		Number	
HTLR Ban x Post	-102.1***	-50.17*	-0.371***	-0.221***
	(38.13)	(22.68)	(0.117)	(0.0783)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Neighborhood radius	5 miles	10 miles	5 miles	10 miles
Observations	3,166	18,718	3,166	18,718
R-squared	0.421	0.362	0.427	0.382
Panel B: Installment loans				
	Installment loans disbursed per low-income installment borrower			
	Dollar amount		Number	
HTLR Ban x Post	-527.1***	-330.6***	-0.0311	-0.0152
	(163.4)	(119.5)	(0.0251)	(0.0176)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Neighborhood radius	5 miles	10 miles	5 miles	10 miles
Observations	16,503	56,740	16,503	56,740
R-squared	0.339	0.221	0.350	0.238

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.12: Bank income and expense in response to high-to-low reordering bans

This table presents the results of our bank x quarter level difference-in-differences analysis using FFIEC Call Report data. The outcome variable is interest income, non-interest income*, revenue*, or net income. We use adjustments of non-interest income and revenue that exclude service charges on deposit account which is mechanically affected by the high-to-low reordering ban. Therefore, non-interest income* is non-interest income minus service charges on deposit accounts. Revenue* is non-interest income* plus interest income. HTLR Ban is a dummy variable that takes on a value of 1 if the bank is required to cease high-to-low reordering, and a value of 0 if the bank is sued but not required to cease high-to-low reordering. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Bank fixed effects and quarter fixed effects are included. Standard errors are clustered at the bank level and the quarter level.

	Growth in:			
	Interest income	Non-interest income*	Revenue*	Net income
HTLR Ban x Post	-0.0352	0.0886	0.00867	-0.0197
	(0.0429)	(0.0793)	(0.0398)	(0.119)
Bank fixed effects	Y	Y	Y	Y
Quarter fixed effects	Y	Y	Y	Y
Observations	685	575	564	469
R-squared	0.938	0.839	0.918	0.710

*** p<0.01, ** p<0.05, * p<0.1

Table B.13: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Varying bank concentration

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In the first two columns, the outcome variable is the dollar amount or the number of payday loans disbursed per payday borrower. In the last two columns, the outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that is required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. HHI is the zip code-level Herfindahl-Hirschman Index by deposits averaged across neighborhoods. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Payday loans disbursed per payday borrower		Installment loans disbursed per low-income installment borrower	
	Dollar amount	Number	Dollar amount	Number
HTLR Ban x Post	-105.5**	-0.311**	-570.0**	-0.0461
	(46.94)	(0.132)	(256.0)	(0.0405)
HTLR Ban x Post x HHI	57.10	0.129	405.6	0.0454
	(73.20)	(0.195)	(382.1)	(0.0378)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	5,574	5,574	30,299	30,299
R-squared	0.409	0.385	0.279	0.298

*** p<0.01, ** p<0.05, * p<0.1

Table B.14: Household demand for installment loans in response to high-to-low reordering bans: By income level

This table presents the results of our zip code x quarter level difference-in-differences analysis using Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. The outcome variable is the dollar amount of installment loans disbursed per installment borrower in a given income quintile. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that is required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Dollars of installment loans disbursed per low-income installment borrower				
Income quintile:	1	2	3	4	5
HTLR Ban x Post	-358.3***	-607.1**	-105.3	-156.0	-1,582
	(135.8)	(256.6)	(379.7)	(593.8)	(1,091)
Neighborhood x quarter fixed effects	Y	Y	Y	Y	Y
Observations	30,487	27,371	24,195	16,545	9,058
R-squared	0.278	0.274	0.308	0.297	0.345
	Log of dollars of installment loans disbursed per low-income installment borrower				
Income quintile:	1	2	3	4	5
HTLR Ban x Post	-0.0829***	-0.0957***	-0.0192	0.0294	-0.0452
	(0.0249)	(0.0361)	(0.0399)	(0.0439)	(0.0657)
Neighborhood x quarter fixed effects	Y	Y	Y	Y	Y
Observations	30,487	27,371	24,195	16,545	9,058
R-squared	0.287	0.290	0.321	0.325	0.358

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B.15: Household demand for payday loans and installment loans in response to high-to-low reordering bans: High-income zip codes

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with above-median income. In the first two columns, the outcome variable is the dollar amount or the number of payday loans disbursed per payday borrower. In the last two columns, the outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that was required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but *not* required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Neighborhood x quarter fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Payday loans disbursed per payday borrower		Installment loans disbursed per low-income installment borrower	
	Dollar amount	Number	Dollar amount	Number
HTLR Ban x Post	27.22	0.0132	-262.2	-0.0268
	(39.56)	(0.0854)	(187.6)	(0.0208)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	4,275	4,275	30,450	30,450
R-squared	0.334	0.327	0.268	0.320

*** p<0.01, ** p<0.05, * p<0.1

Table B.16: Payday loan inquiry acceptance rate in response to high-to-low reordering bans

This table presents the results of our zip code x quarter level difference-in-differences regressions using Clarity alternative credit bureau. The outcome variable is the payday loan acceptance rate defined as the number of payday loans extended divided by the number of payday loan inquiries made. The sample is restricted to zip codes with below-median income. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that is required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. We examine a -2 to +4-quarter window around the high-to-low reordering ban. Varying levels of fixed effects are included. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Payday loan acceptance rate		
HTLR Ban x Post	0.00374	0.00411	-0.0262
	(0.0130)	(0.0121)	(0.0279)
Neighborhood fixed effects	Y	Y	N
Quarter fixed effects	N	Y	N
Neighborhood x quarter fixed effects	N	N	Y
Observations	8,590	8,590	8,590
R-squared	0.368	0.387	0.500

*** p<0.01, ** p<0.05, * p<0.1

Table B.17: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Controlling for cash payout

This table presents the results of our zip code x quarter level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The sample is restricted to zip codes with below-median income. In the first two columns, the outcome variable is the dollar amount of the number of payday loans disbursed per payday borrower. In the next two columns, the outcome variable is the dollar amount or the number of installment loans disbursed per low-income installment borrower. HTLR Ban is a dummy variable that takes on a value of 1 if the zip code contains branches of a bank that is required to cease high-to-low reordering, and a value of 0 if the zip code contains branches of a bank that was sued but not required to cease high-to-low reordering and the zip code lies within 7 miles of a treated zip code. Post is a dummy variable that takes on a value of 1 in the quarters after the high-to-low reordering ban, and a value of 0 in the quarters leading up to the high-to-low reordering ban. The dollars of cash settlement per affected consumer is computed by taking the average per-consumer cash relief payment across all bank branches in a zip code. (For each lawsuit bank, the per-member cash relief for each lawsuit bank is documented in Table B.1. For each non-lawsuit bank, the per-member cash relief is 0.) We examine a -2 to +4-quarter window around the high-to-low reordering ban. Varying levels of fixed effects are included across specifications. Standard errors are clustered at the quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 7 miles of each other.

	Payday loans disbursed per payday borrower		Installment loans disbursed per low-income installment borrower	
	Dollar amount	Number	Dollar amount	Number
HTLR Ban x Post	-85.30***	-0.290***	-350.4*	-0.0257
	(31.52)	(0.0904)	(180.0)	(0.0317)
Dollars of cash settlement per affected consumer	-0.162	-7.55e-05	-2.211	0.000195
	(0.227)	(0.000577)	(1.351)	(0.000299)
Neighborhood x quarter fixed effects	Y	Y	Y	Y
Observations	5,574	5,574	30,487	30,487
R-squared	0.408	0.384	0.272	0.294

*** p<0.01, ** p<0.05, * p<0.1

Appendix C

Appendix to Chapter 3

C.1 Proofs

Proof of PROPOSITION 1: Consider the scenario in which $m^{Safe} \geq m^{Safe*}$. When investment banks exert effort and certify firm types, the going-public market is in the separating equilibrium and the payoff for investment banks is $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta(1-p)vI$. When the investment banks shirk their certification role, the going-public market is in the pooling equilibrium. The payoff for investment banks is $\beta uI - \alpha\beta(1-p)vI - (1-\alpha)\,\beta(1-q)vI$. Therefore, investment banks exert effort and certify firm types when (1) the payoff for investment banks in the separating equilibrium is higher than that of the pooling equilibrium: $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta(1-p)vI \geq \beta uI - \alpha\beta(1-p)vI - (1-\alpha)\,\beta(1-q)vI$; and (2) the payoff for investment banks in the separating equilibrium is positive: $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta\,(1-p)\,vI > 0$. From these restrictions we obtain the range of v: $\frac{(1-\alpha)uI+\alpha c_{ib}}{(1-\alpha)(1-q)I} \leq v < \frac{uI-c_{ib}}{(1-p)I}$.

Now consider the scenario in which $m^{Safe} < m^{Safe*}$. When investment banks exert effort and certify firm types, the safe firm going-public market is in the separating equilibrium and the payoff for investment banks is $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta(1-p)vI$. When investment banks shirk their certification roles, the safe firm going-public market is in the breakdown equilibrium and the payoff for investment banks is 0. When investment banks exert efforts to identify firm types but do not reveal the information they reduce the proportion of B/S firms to

γ^{Safe*} and set the new prior probability of success $m^{Safe} = m^{Safe*}$, i.e., $\alpha p + \gamma^{Safe*} q = \frac{1}{R_l}$ or $\gamma^{Safe*} = \frac{1}{qR_l} - \frac{\alpha p}{q}$. The safe firm going-public market is in the pooling equilibrium and the payoff for investment banks is $\left(\alpha\beta + \gamma^{Safe*}\beta\right) uI - \beta c_{ib} - \alpha\beta(1-p)vI - (1-\alpha)\,\beta(1-q)vI$. Therefore, investment banks exert effort and certify firm types when (1) the payoff for investment banks in the separating equilibrium is higher than that of the pooling equilibrium: $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta(1-p)vI \geq \left(\alpha\beta + \gamma^{Safe*}\beta\right) uI - \beta c_{ib} - \alpha\beta(1-p)vI - (1-\alpha)\,\beta(1-q)vI$; and (2) the payoff for investment banks in the separating equilibrium is positive: $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta\,(1-p)\,vI > 0$. These two inequalities imply v: $\frac{\gamma^{Safe*} uI - (1-\alpha)c_{ib}}{(1-\alpha)(1-q)I} \leq v < \frac{uI - c_{ib}}{(1-p)I}$. In the risky firm going-public market, the payoff for investment banks in the separating equilibrium is $\alpha\beta uI - \alpha\beta c_{ib} - \alpha\beta\left(1 - \frac{pR_l + \mu}{R_h}\right) vI$. When the payoff is negative, i.e., when the probability of success for G/R firms is lower than a critical value, $\frac{pR_l+\mu}{R_h} < 1 - \frac{u}{v} + \frac{c_{ib}}{vI}$, investment banks do not take risky firms public.