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

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Abstract

The reordering of transactions from "high-to-low" is a controversial bank practice thought to maximize fees paid by low-income customers on overdrawn accounts. We exploit a series of class-action lawsuits that mandated that some banks cease the practice. 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.

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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.[1] 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.[2]

The commonly proposed solution to the costliness of alternative financial services is to bank the unbanked and underbanked populations.[3] A significant barrier to this approach, however, is that low-income consumers find traditional banking services to also 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.[4]

Overdraft fees, which accounted for a noteworthy $33 billion of bank revenue in 2018, constitute the bulk (around two-thirds) not only of all deposit account fees earned by banks, but also of bank account fees incurred by *low-income* consumers.[5] 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 is in their accounts and charge

[1] 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.”

[2] 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. As of 2019, nineteen states plus the District of Columbia prohibit payday lending, set interest rate caps, or enforce other limits that may effectively force payday lenders out of business.

[3] For example, on its economic inclusion website, 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 for emergency and long-term needs, 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.”

[4] 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.

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

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 particularly controversial practice, high-to-low reordering, thought to maximize overdraft fees earned from low-income consumers.

For consumers with low account balances, high-to-low reordering can generate significantly more overdraft fees than real-time chronological processing of transactions. For example, a high-to-low reordering bank would process a $500 rent debit against a $400 checking account balance before two smaller transactions of $50 each, even if the latter were posted earlier. High-to-low reordering would thus cause the individual to incur three overdraft fees compared to just one under chronological ordering. Given a standard overdraft fee of $35, high-to-low reordering imposes a fee burden of 53% of the original overdrawn balance compared to 17.5% under chronological ordering. High-to-low transaction reordering is widespread: roughly half of the 50 largest banks (by deposits) engaged in the practice as of 2016 according to a 2016 report by the Pew Charitable Trusts.[6] See Figure 1 for an illustrative example.

A negative deposit account balance (made more negative by overdraft charges) cannot be rolled over, and failure to repay overdrafts and fees promptly can entail 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.[7] Given the potential consequences of failing to repay overdrafts and related fees, consumers with hefty accumulated overdrafts may find it necessary to borrow elsewhere, as 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. Prominent banks have even advertised their own payday loan

[6] See Pew Charitable Trusts (2016) for details on the study.

[7] This is due to the banking system's centralized record keeping on deposit account customers. ChexSystems, the primary consumer reporting agency used by banks, records involuntary bank account closures that result from unpaid overdrafts and related fees. 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 other bank for up to five years. Without a checking account it becomes difficult to obtain credit or access basic financial services like check writing, debit cards, direct deposit, digital transfer, and bill payment.. See Campbell et al. (2012) for an empirical analysis of involuntary bank account closures.

products as a way for customers to restore overdrawn bank accounts to good standing.[8] These circumstances reflect a previously unstudied link between overdrafts and payday loan markets whereby the former can create demand for the latter.

Theoretically, the net effect of high-to-low reordering on consumer welfare is ambiguous. On the 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.[9] Overdraft borrowing may optimally lead to payday or other alternative lender 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.

To assess the impact of high-to-low reordering on consumers entails meeting several challenges, the first being lack of data. Underbanked consumers, lying at the intersection of the traditional and alternative financial systems, are not fully represented in the traditional credit bureau data 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 fully transparent about their procedures, and few organizations are incentivized to consistently track bank behavior over time. A third challenge is that banks' policies and behaviors are endogenous to its customer base; bank policies and behaviors, instead of being randomly assigned, are optimally driven by the type of depositors a bank attracts and will be correlated with local economic variables.

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 1 million 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.[10] We complement the Clarity data with traditional credit bureau data from Equifax, one of the three major consumer credit bureaus. The Equifax data enables us to hone in on roughly

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

[9]Banks have argued that high-to-low reordering benefits customers because it ensures that large, important payments - like rent, mortgages, and student loans - are made first.

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

the same population by focusing on installment loans made to borrowers in the dataset's lowest quintile of the income distribution.[11]

Second, we detect high-to-low reordering 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 high-to-low reordering was mandated), and geographic areas affected. The lawsuits provide the key source of variation in high-to-low reordering 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 high-to-low reordering contribute to alternative system borrowing and overall credit health deterioration for low-income consumers?"

Our empirical analysis proceeds in four steps. We begin by providing motivating evidence that shows that, within the same zip code, branches of high-to-low reordering banks are more likely than branches of non-high-to-low-reordering banks to be located in close proximity to payday lenders. This finding suggests that alternative finance providers and banks with aggressive overdraft policies co-locate and service similar customers. We confirm that high-to-low reordering bans led to meaningful changes in overdraft policies at affected banks, and document that both overdraft revenues and a proxy for overdraft balances declined significantly for banks required to cease high-to-low reordering. 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 high-to-low reordering ban. Our empirical strategy centers on comparing zip codes with branches of banks required to cease high-to-low reordering with neighboring zip codes with branches of banks that were sued but *not* required to cease the practice.[12] The choice of this control group ensures that we are comparing areas with

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

[12]Our goal is to estimate the effect of the high-to-low reordering 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 high-to-low reordering. If it is the same for both groups of consumers, this learning effect does not

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 borrowing from alternative lenders declines significantly. Specifically, payday borrowing declines by a statistically significant $84 per borrower per quarter, which translates to an economically significant decline of 16 percent relative to its mean. Installment loan borrowing similarly declines, by $200 per borrower per quarter, a six 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 high-to-low reordering 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 high-to-low reordering 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 high-to-low reordering bans.

We further find that affected households significantly increase consumption of durable goods related to home and auto as well as of essential non-durable goods.

In the fourth and final step of analysis, we investigate spillover effects of such bans. We hypothesize that high-to-low reordering bans may have the unintentional consequence of changing the way banks interact with low-income consumers.[13] Because overdraft fees are an important source of revenue for banks, forcing them to cease high-to-low reordering 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 high-to-low reordering bans and

detract from our ability to estimate the effect of the high-to-low reordering ban

[13]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.

that the effect is concentrated in low-income zip codes where banks had only a small number of branches to begin with.

We next investigate the impact of branch closures on other segments of the credit market, for example, whether regulating certain bank practices involves a tradeoff between mitigating the burden on low income borrowers and preserving the supply of mortgage or small business lending in a region. We find no evidence of reduced mortgage or small business lending due to bank branch closures resulting from high-to-low reordering bans. This could be because online banking has made branches less necessary to supplying credit or because branch closures are not sufficiently widespread to affect credit supply.

Examining loan acceptance rates in payday loan markets, we find no change following high-to-low reordering bans, which suggests an absence of supply-side effects stemming from payday lenders adjusting willingness to extend credit. This finding supports our conclusion that the observed decline in payday and installment loan borrowing is consistent with a decline in *demand* for these loans rather than a decline in supply.

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. In Section 2 we discusses related literature. In Section 3 we provide background on bank overdrafts and high-to-low related lawsuits lodged against banks. We describe the data in Section 4 and present motivating evidence in Section 5. Consumer responses to high-to-low reordering bans are discussed in Section 6 and bank responses in Section 7. Results of robustness tests are reported in Section 8 and Section 9 concludes.

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 that has to date focused largely on the payday loan product. On the one hand, there is evidence that access to payday loans improves consumer welfare. For example Morse (2011) finds that access to payday loans enables consumers to avoid foreclosure and continue making mortgage payments in the wake of natural disasters. Consistent with this, Zinman (2010) finds that restricting payday loan access causes consumers to shift to more expensive substitutes (bank overdraft and late bill payment) and to experience a deterioration in overall 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, while Skiba and Tobacman (2019) find that payday loan access increases personal bankruptcy rates by a factor of two. Bertrand and Morse (2011) provide evidence that disclosure about the costs and benefits of payday loans can significantly affect the uptake of these loans, thereby indicating that payday borrowers are not making fully informed, utility-maximizing choices and may not fully understand the true cost of obtaining a short-term, high-cost loan. Despite ample evidence that payday borrowers are also likely to be frequent overdrafters (see, for example, Zinman (2010), Morgan et al. (2012), Melzer and Morgan (2015)), regulation, policy attention, and academic research have been focused more on payday loans than on bank overdrafts. Our contribution to this pod of literature is to 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 with each other and that consumers use overdrafts and payday loans as substitutes. According to Cirillo (2004)'s survey of 2,000 payday loan customers, 66% of 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 can contribute 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 basic affordable financial services for all.

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, and Hayashi (1985), Hayashi (1987), Zeldes (1989), Jappelli (1990) and Gross and Souleles (2002) provide indirect and direct empirical evidence of liquidity constraints. A follow-up literature beginning with Bacchetta and Gerlach (1997) shows, 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, in our setting, to overdrafts and payday loans) causes consumers with likely binding liquidity constraints and low cash on hand to not only increase consumption, but also experience improvements in credit health and access to traditional credit. We note that, 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).[14] 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 exact type of discounting at play or type of non-optimal borrowing that results.[15] 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.

Finally, this paper also 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

[14]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.

[15]For example, hyperbolic discounting consumers may borrow non-optimally today in relation to their longer-term self.

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

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% of) 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 several lawsuits against banks for violations of the Regulation E opt-in requirement (CFPB, 2013, 2017).[16]

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

[16]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.

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, 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, and non-white individuals and individuals who do not possess a credit card 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 also documented that consumers are surprised by overdraft fees, uncertain about bank policy, and sometimes neglectful of automated payments that trigger overdrafts. Explains an interviewed consumer, “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).

Customers also tend to associate overdraft fees with payday loans, and overdrafters tend to be the focus of customer acquisition campaigns by payday lenders.[17] According to Rivlin (2010), the payday industry has grown considerably in recent times because “when the

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

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 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 high-to-low reordering of deposit account transactions. Details of these lawsuits can be found in Section 4.

4 Data

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 high-to-low transaction reordering. 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. Therefore, in order to observe the overdraft policies of banks over time, we 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 1 present summary statistics of our Pew-Infogroup-Summary of Deposits merged dataset. On average, 1.38 branches out of 4.72 total branches in a zip code employ high-to-low transaction reordering. This prevalence likely reflects the fact that several of the largest banks in our sample employed high-to-low reordering 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 versus those that do not practice high-to-low reordering. That there are more check cashers and payday lenders around branches of high-to-low-reordering 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 high-to-low reordering. 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. We identified relevant legal cases with which to build our lawsuits dataset 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 settled in court and exclude those dismissed or settled via arbitration. Our final dataset includes 37 lawsuits, for which we note key event dates and terms of settlement between each bank and its customers. In particular, we document whether and when each bank was required to institute behavioral relief, that is cease high-to-low transaction reordering.[18] See Table IA.1 for an overview of our lawsuits dataset. For each lawsuit, we report the name of the sued bank, the date when the lawsuit was filed, the

[18]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 high-to-low reordering. Given that our analysis is at the quarterly level, using this procedure in these few cases should not affect our results.

date when the judge granted final approval of the settlement, and the date when the bank was required to cease high-to-low transaction reordering, if at all.[19]

Haubrich and Young (2019), examining the different components of non-interest income for banks, 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.[20] One explanation for the onset of this wave of overdraft fee 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.

There is room for non-uniform ruling in these lawsuits because the practice of high-to-low reordering is not 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 generally claimed they were providing monetary (a cash payment), and in some cases also behavioral (an end to high-to-low reordering), relief 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' high-to-low reordering 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 and those that maintained the practice

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

of high-to-low reordering. JPMorgan Chase and Wells Fargo, for example, stopped high-to-low reordering, Bank of America and Citibank did not; Great Western Bank and Northwest Savings Bank ceased high-to-low reordering, 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 consumers' credit health, consumption, and demand for payday and installment loans). For example, one determinant of the lawsuit outcome was whether it became part of the multi-district litigation MDL 2036, the purpose of which was to consolidate, in order to handle with greater efficiency and speed, cases with shared key elements. In our setting, we find that lawsuits in MDL 2036 had a 68.2% probability of enacting behavioral relief, while the remaining lawsuits had a 53.3% probability of enacting behavioral relief. This is suggestive evidence that the MDL structure may have influenced lawsuit outcomes by increasing the likelihood of bank behavioral relief. Similarly, as noted in the CFPB 2015 Arbitration Study, while there was broad similarity in business practices and the legal claims against banks, there was variety in the contracts between consumers and banks and also in the approach to litigation. For example the CFPB note that, "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 the ex-ante variation in contracts likely led to different lawsuit outcomes, and that it is highly improbable that consumers were aware of these ex-ante contract differences. Hence, it is highly unlikely that there is selection into different banks or that customers of banks required to cease the practice of high-to-low reordering differ along any meaningful dimension from customers of sued banks that continued the practice. Empirically, we find no existence of pre-trends in any of our outcome variables, which is 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.

A full picture of the interactions between the traditional and alternative financial systems requires access to data on the latter, which also entails a data availability issue. The alternative financial system is neither as centrally organized nor as regulated as the banking system. Although 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. There is also no designated regulator in charge of jointly evaluating the different components of the alternative financial system, not only payday lenders but also check-cashers and issuers of prepaid debit cards. We overcome this data availability challenge in the alternative financial system by exploiting several data sources.

The Infogroup Historical Business database 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.

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.

Our main 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 2020 with details on prospective loan type and borrower characteristics. Tradelines are actual extended loans. We observe tradelines from 2013 to 2020 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 code. Panels A and B of Table 2 present summary statistics of the Clarity data used in this study. We draw a random sample of one million 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 recorded in the dataset.

We complement this data with information for a representative sample of borrowers present in Equifax. Although payday lenders do not report payday loans to the major credit bureaus, we can identify other loan types routinely used by credit-constrained borrowers. Installment loans are an alternative to payday loans for individuals with poor credit. An example is title loans, that is, secured loans where borrowers use their vehicle title as collateral. Among numerous online installment lenders that serve the same clientele as payday lenders are Oportun, Opploans, OneMain Financial, and Upgrade, none of which report to credit bureaus.[21] The largest payday lenders now offer installment loans in addition to conventional payday loans due in a single lump sum.[22] To try to address the debt spirals typical of payday lending, the Consumer Financial Protection Bureau (CFPB) proposed in June 2016 a rule requiring payday loans to be repayable in installments. This regulatory pressure is one of the main factors driving the trend toward offering installment loans. Panels C and D report statistics for these loans, in particular, for the borrowers in the lowest income quintile.

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 and debit card transaction-level data for a 6 million representative sample of US households. We use this data to construct measures of consumption and test whether depositors' expenditures are altered as a result of bank behavioral changes related

[21] See, for instance, this article https://www.nerdwallet.com/best/loans/personal-loans/installment-loans-bad-credit.

[22] See the information available here: https://www.pewtrusts.org/fr/research-and-analysis/issue-briefs/2016/08/from-payday-to-small-installment-loans.

to high-to-low reordering.

Table 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, control zip codes 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 of the treatment years identified by the lawsuits data and document the number of branches belonging to sued banks in each treatment and control zip code. Table 3 shows sued banks, on average, to 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 US financial systems at a relatively granular (zip code) level on multiple dimensions.

5 Motivating Facts

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 high-to-low reordering 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 4 tests whether banks, in particular those that employ high-to-low reordering, 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 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 and/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 (high-to-low reordering 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 high-to-low reordering are more likely to have check cashers/payday lenders in close proximity.

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.[23]

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

Although results in Table 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 might endogenously tailor their products and pricing to local demographics. Put differently, high-to-low reordering might be a way for banks to fairly price overdrafts when serving specific customer types.

We use lawsuits against banks that employed high-to-low reordering 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

[23] 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.

banks' use of high-to-low reordering after a specific date.[24]

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.

Figure 2 plots quarterly coefficients of a difference-in-differences regression of bank revenues associated with overdrafts for banks affected by lawsuits resulting in mandatory behavior changes relative to other non-sued banks operating in similar geographic areas, before vs after the high-to-low reordering ban.[25] In Figure 2, the dependent variable is deposit fee income divided by total revenue. The results translate to an average loss of approximately $15 million in overdraft balances per quarter, or around $1.3 billion annually for all sued banks with high-to-low reordering bans. These findings are confirmed in Table A.1 in which we report the point estimates of the corresponding difference-in-differences specification. Table A.1 also reports point estimates for other bank-level variables that help build a fuller picture of the effect of high-to-low reordering bans on banks. Specifically, we consider the log of "Other Consumer Loans" category in FFIEC 031 regulatory call report data. This category includes overdraft balances; banks that provide overdraft services are required to report overdraft balances as part of other consumer loans rather than negative deposits.[26] Measuring overdraft balances not being possible with Call Report data, the other consumer loan category is the best proxy for the amount of overdraft credit extended. The point estimate shows a significant decline in other consumer loans post high-to-low reordering ban. We also consider the total number of insured depositors. On one hand, consumers might respond to the lawsuits by leaving affected banks after learning of the aggressive pricing practices, on the other, banks no longer able to extract excessive revenues from customers who use overdrafts might close deposit accounts. We find no decline in the number of insured depositors indicating that it is unlikely that either of these effects is systematically affecting banks subject to high-to-low reordering bans.

[24] Details of the lawsuits are recorded in Table IA.1.

[25] Each bank is assigned a primary state, which is the state within which the majority of bank branches reside, and a size decile, and tests are run within primary state and size decile.

[26] See, for example, Instructions for Preparation of Consolidated Reports of Condition and Income (FFIEC 031 and 041) for details of how overdrafts are accounted for.

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 income categories at banks subject to high-to-low reordering bans. For instance, changes in funding sources or investment opportunities at sued banks subject to high-to-low reordering bans are likely to result in broader changes in bank behavior and performance. We test this by running the same baseline specification in Table A.1, but for other outcome variables that should not be affected by the high-to-low reordering ban, such as other items from the income statement like interest income or expenses. Table A.2 contains results of this placebo test and reports that these other income statement items remain unchanged after the high-to-low reordering ban.[27]

Overall, these findings indicate that activity related to overdrafts declined significantly at banks required to cease the practice of high-to-low reordering.[28]

6 Consumer Responses to High-to-Low Reordering Bans

6.1 Consumer Demand for Alternative Loans

We now turn to our main analysis: assessing the effect of banning high-to-low reordering, an arguably aggressive bank policy, on consumer behavior. We begin with Table 5, which documents the effect of high-to-low reordering 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 high-to-low reordering bans to be followed simultaneously by quantity declines in payday borrowing *and* reduced overdraft activity.

If, on the other hand, overdraft services were simply fairly priced substitutes for payday loans, we would expect overdraft activity and payday borrowing to move in opposite

[27]Unreported tests in which we investigate the impact of high-to-low reordering 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.

[28]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.

directions. Specifically, if the price reduction resulted in a supply restriction, that is banks became less willing to offer overdraft services because of the cap on fees, 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.[29]

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

$$PaydayBorrowing_{zt} = \beta \cdot HTLR\,Ban_z \cdot Post_t + \eta_{nt} + \varepsilon_{zt} \qquad (1)$$

where $PaydayBorrowing_{zt}$ is the average per-borrower amount of payday loan disbursed within zip code z in quarter t, $Post_t$ is a dummy variable that takes a value of 1 for the four quarters following the high-to-low reordering 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 high-to-low reordering 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 high-to-low reordering. We compare zip codes containing branches of high-to-low reordering ban 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 high-to-low reordering ban. We choose this specification to further ensure that we are comparing local areas with similar types of consumers (i.e., those targeted by high-to-low reordering practices). Robustness checks in Table A.4 and Table A.3 demonstrate that choice of neighborhood radius and control zip codes more generally 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 to zip codes with sued banks present 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

[29]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.

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 high-to-low reordering 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 3 for confirmation that parallel trends between treatment and control zip codes holds.

Table 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,[30] 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 average total dollars disbursed per borrower-quarter (Columns 1-3) and total number of loans per borrower-quarter (Columns 4-6), allows us to study both the intensive and the extensive margins. We find a significant reduction in all outcome variables for the treated zip codes. Specifically, we find that following a high-to-low reordering ban dollars disbursed decrease by $84 per borrower-quarter, which translates to a 16 percent reduction relative to the per borrower-quarter mean. Table 5 shows number of loans to also decline, by 0.29 per borrower-quarter, post ban, which is equivalent to a 15% reduction relative to its mean.

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 6 complements the previous analysis by documenting results from the same baseline differences-in-differences specification as above, but for installment

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

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. Similar to the findings reported in Table 5, we find a significant reduction in the amount of installment loans following high-to-low reordering bans. The effects are also economically meaningful with a $200 reduction per borrower-quarter, which corresponds to an approximately 6 percent reduction per borrower-quarter.[31]

We next assess whether there is any heterogeneity in these findings resulting from differences in banking competition. Intuitively, if banks compete on overdraft prices, we would expect the effects of high-to-low reordering bans to vary with variation in deposit market competition. We test this hypothesis by interacting the high-to-low reordering dummy variable in Equation (1) with average zip code-level HHI. Results in Table A.7 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 competition does not induce firms to reveal information hence reducing high 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 5 and Table 6 indicate that demand for loans from alternative lenders declines significantly in locations in which banks are forced to cease high-to-low reordering. 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.[32] 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 high-to-low reordering banks documented in Table A.1 is consistent with the

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

[32]See Section 8 for tests and a discussion that helps to rule out supply effects as a driver of the decline in payday and installment borrowing.

hypothesis that overdrafts and payday loans are likely complements, not merely substitutes for one another.

6.2 Long-Term Demand for Alternative Loans

Next, we test whether these results reflect a longer term/permanent change in borrower behavior rather than simply a short-term response to the high-to-low reordering ban. If bank overdraft practices are a key driver of demand for alternative financial products, we should expect a permanent reduction in overdraft fees to result in a long-lasting decline in alternative credit market borrowing. Table 7 tests this hypothesis with the same difference-in-differences specification as in the previous analysis using both the Clarity (Panel A) and Equifax (Panel B) sample. The dependent variables are total dollars of loans outstanding per borrower-quarter. Each column focuses on a different horizon — one, two, and three years – by varying the period post high-to-low reordering ban included in the specification. Panel A shows the reduction in the single-period micro loans to persist over time, although the point estimates suggest its magnitude declines slightly, from $84 in the first year to $50 after three years. Panel B shows that, if anything, the magnitude of the decline to be slightly increasing, from $200 to $264.[33] Overall, these findings reinforce the hypothesis that aggressive overdraft policies might push borrowers to persistently borrow from alternative lenders.

6.3 Consumer Financial Health

We next investigate whether the financial health of low-income consumers improves following high-to-low reordering bans and the subsequent reduction in consumer demand for alternative credit market loans . 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, might not fully understand the true costs of overdraft credit and might make sub-optimal decisions when taking on overdrafts. A reduction in overdraft pricing after high-to-low reordering bans

[33] There is no statistical difference between the point estimates across rows in Panels A and B.

might reduce debt service costs to more sustainable levels through either a simple reduction in fees[34] or 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.

Second, if consumers turn to payday and other alternative lenders to repay overdraft fees and balances, then a high-to-low reordering ban should stems 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 the ability to service other existing debt as well as overall credit health.

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 balance and limits. Whereas Table 5 and Table 6 document that use of alternative loans respond relatively quickly to high-to-low reordering bans, we might expect consumer financial health to take longer to improve, and, indeed, this is what we find. Using the same empirical specification in the previous section, we report results from the following borrower-zip code-quarter-level regression in Table 8 at 1 to 3 year horizons:

$$CreditHealth_{zt} = \beta \cdot HTLR\,Ban_z \cdot Post_t + \eta_{nt} + \varepsilon_{zt} \tag{2}$$

We find a significant improvement in consumer financial health across these measures, and that these effects take time to materialize. Specifically, we find borrowers' total balance in good standing to increase by \$431 after two years and \$611 after three years following high-to-low reordering bans, 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 at least 10 points in their credit score after three years since the high-to-low reordering ban has

[34] Due to the price reduction and/or reduced need to go to high-cost payday/installment lenders to roll over the overdraft.

[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.

passed. We also find that both credit card balance and credit limits increase significantly, i.e. credit card balances increases by $110 after two years and $195 after three, while credit card limits increase by $190 and $335 respectively.

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 might 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.

6.4 Consumer Consumption

We further assess the impact of overdraft prices on consumers by examining the effect of high-to-low reordering 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 US households.

We estimate the following consumer-zip code-quarter-level specification:

$$Consumption_{zt} = \beta \cdot HTLR\,Ban_z \cdot Post_t + \eta_{nt} + \varepsilon_{zt} \tag{3}$$

where our consumption outcome variables include household dollars of expenditure and number of items of expenditure for durables, non-durable essentials, and non-durable other. 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 9 presents the results of this test. Focusing on within-neighborhood-quarter variation, we find that consumers increase durables and non-durables essential consumption by $44 and $14 respectively (and roughly one extra unit), and non-durables other consumption remains unchanged, following the high-to-low reordering ban.[37] This finding is consistent

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

with our hypothesis that low-income consumers likely experienced binding liquidity constraints prior to the high-to-low reordering ban and were more likely to consume only the necessities. By reducing their overdraft burden, improving their credit health, and relaxing their constraints, the high-to-low reordering 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 now have the capacity to increase consumption of durables and non-durable essentials, all likely essential expenditures.

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 8 and Table 9 suggest that a reduction in debt service costs related to overdrafts results in consumers not only increasing consumption, but also experiencing improved credit health and increased access to traditional credit. These findings are consistent with the existence of liquidity constrained low-income consumers.

6.5 Magnitudes

To put the magnitudes of consumer responses in context, we approximate the per-consumer reduction in overdraft fees that resulted from high-to-low reordering bans. Using data from the CFPB, we estimate that there are roughly 48 million customers of banks sued because of high-to-low reordering practices during our sample period.[39]

The FDIC reports that 14% of bank account users incur five or more overdrafts in a year. We use this statistic to estimate how many consumers are likely materially affected by high-to-low reordering bans. Specifically, we argue that 14% of the 48 million, or 4.2 million customers of sued banks, are likely to be markedly affected by the high-to-low reordering bans.

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

[39] There were 28 million customers involved in the MDL 2036 class action lawsuit that involved 21 banks; linearly scaling this number up to account for there being 37 total banks in our lawsuits sample yields the 48 million customer estimate.

We next use estimates of overdraft revenue decline resulting from the lawsuit-mandated high-to-low reordering bans in order to approximate the per-customer reduction in overdraft fees. Sued banks required to cease the practice of high-to-low reordering experienced a decline in overdraft revenue of approximately $15 million per bank-quarter, which aggregates to $1.3 billion per year for the 23 banks subject to the ban.[40] A total loss of $1.3 billion translates to roughly $330 in savings per materially affected customer per year.

Our per-customer overdraft fee savings lie in the ballpark of the decline in alternative borrowing of $84 and $200 per borrower per quarter that we document in Table 5 and Table 6. Our back-of-the-envelope approximations are thus consistent with the hypothesis that some consumers turn to alternative lenders in order to repay overdraft balances and related fees.

Finally, turning to our consumption results, Table 9 shows expenditures to increase by roughly $60 per consumer per quarter, and this increase to be 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 high-to-low reordering 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 8). 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.

7 Spillover Effects

Because overdraft fees constitute a significant fraction of revenue for some banks, especially in low-income areas, the reduction in revenue consequent to being required to cease high-to-low overdraft practices might make it unprofitable for banks to operate in such areas. To gauge potential spillover effects of high-to-low reordering bans, we complement our consumer-level

[40] We arrive at this number by computing -0.00693 * $2.1 billion of average quarterly revenue =$15 million loss per quarter per bank,

[41] This is a per-consumer, not per-borrower result. Were we able to focus specifically on the consumption of affected borrowers, the magnitudes might be larger.

analysis with an examination of bank responses to lawsuit outcomes.

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

$$Exit_{izt} = \beta \cdot HTLR\,Ban_i \cdot Post_t + \eta_{zt} + \varepsilon_{izt} \qquad (4)$$

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 high-to-low reordering 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 high-to-low reordering ban and up to three years after and 0 for the three years prior to the ban. Zip code x year and bank x zip code fixed effects are included.

The first three columns of Table 10 document the results of this regression for all zip codes. Because incentives to close branches might also depend on the strength of banks' local presence, we focus in the second set of three columns on zip codes in which treated banks have two or fewer branches.[42] In the last set of columns, we restrict the tests to zip codes with low median household income in any given year as captured by the dummy variable Low-Income.

We find banks required to cease high-to-low reordering to exhibit as much as a 2% higher probability of closing branches. This effect is concentrated in zip codes deemed low-income areas and in which banks with high-to-low reordering 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 high-to-low reordering bans. These effects are illustrated graphically in Figure 4.

The data being at the zip code-year-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 might be subject to year-specific economic shocks that make it unprofitable for some banks to operate. We control for the latter pos-

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

sibility by including zip code by year fixed effects in columns 3, 6, and 9, which means that we are identifying within zip code-year-level variation in exits. Because there might 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 policy makers about possible adverse effects of these closures 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 hence plausible that although borrowers benefit overall from banks ceasing high-to-low reordering, even after branch closures there might be more general negative spillover effects in other parts of the economy.

We directly test this hypothesis in Table 11, 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 thousand, between $100 and $250 thousand, or above $250 thousand) declined after high-to-low reordering ban banks exited the neighborhood. We find for mortgage lending no significant effects, and for small business lending that loan amounts remain flat for all categories except the smallest loans, which exhibit a slight increase when we do not account for neighborhood x year variation. Intuitively, these results demonstrate that the exit of high-to-low reordering ban 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 IA.2 complements these findings by analyzing at the bank level, how other banks that are unaffected by lawsuit outcomes, responded to bank branch exit of high-to-low re-

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

ordering ban banks. We show that these other institutions were not more likely to enter or exit neighborhoods from which high-to-low reordering ban banks exited, and also did not experience any significant change in overdraft-related revenue and balances or number of insured depositors (Table IA.3).

8 Robustness

In this section we discuss a number of robustness and placebo tests. We first conduct checks to show that our main results do not hinge on the choice of control group or the choice of neighborhood. In Table A.3, we first check whether our restriction of control zip codes to those that contain branches of sued banks with no high-to-low reordering ban is material. In Columns (1) and (2) of Panels A and B, we restrict our control group zip codes to be within seven miles of the treated zip code, but eliminate zip codes that contain branches of sued but non-high-to-low reordering ban banks.

Even with this significantly more restrictive specification, we find similar reductions in payday and installment loan borrowing. In Columns (3) and (4) we eliminate the restriction that control group zip codes must be within the same 7-mile neighborhood of treated zip codes and instead simply compare treated zip codes (those with high-to-low reordering ban bank branches) with any other zip codes within the same state. Again, we find similar reductions in payday and installment loan borrowing comparable to our main results.

Table A.4 explores whether our results depend on our definition of neighborhood, which in the main specification is based on a radius of seven miles around treated zip codes. Rerunning our analyses of the Clarity (Panel A) and Equifax (Panel B) samples with neighborhood defined as within five (Columns 1 and 2) and ten (Columns 3 and 4) miles of a treated zip code yields consistent results across specifications. In sum, the results in Table A.3 and Table A.4 reassure us that choice of control group is not a key driver of our results.

We next test whether there are important heterogeneities in our main findings across income levels. We should expect our results to be concentrated among those with the lowest incomes, such individuals being more likely to have bank account balances near zero and hence more likely to be affected by bank overdraft practices. An advantage of the Equifax data is the possibility of observing borrower income.[44] We report our baseline specification by income quintile in Table A.5, Column 1 reporting results for borrowers with income in the bottom quintile, Column 5 reporting results for borrowers with income in the top quintile.

Consistent with the hypothesis that the borrowers most likely to be affected by high-to-low reordering bans are those with low income, we find the reduction in installment loan borrowing to be concentrated among the borrowers in the bottom two quintiles of income. Similarly, we report in Table IA.4 that the effects on payday and installment borrowing are not present in high-income zip codes. In other words, that both the statistical and economic significance of the results disappear when we focus on richer regions highlights potential within-bank heterogeneity in overdraft practices.

We next run our baseline payday loan and installment loan tests for zip codes with relatively few branches of high-to-low reordering ban banks. Given that we are able to observe only average borrowing per borrower per zip code, we should see muted effects in areas in which there are likely to be less affected consumers (i.e., areas with fewer branches engaged in high-to-low reordering that were subsequently required to eliminate the practice). Results of this test are reported in Table IA.5. This placebo test demonstrates that the main effects documented in Table 5 and Table 6 are, indeed, concentrated in zip codes in which a significant portion of all branches experienced high-to-low reordering bans.

We further test for possible spillover effects by assessing whether the supply of payday loans is likely affected by high-to-low reordering 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 high-to-low reordering bans might reduce the likelihood of payday lenders being repaid on time, payday loan repayments likely being among such larger ticket items. Hence, high-to-low reordering bans could affect the credit risk of payday borrowers and supply decisions of payday lenders.

We therefore analyze loan acceptance rates for payday loans and report in Table A.6 no evidence of any change subsequent to the imposition of high-to-low reordering bans. These findings suggest that the reductions in payday and installment borrowing documented in Table 5 and Table 6 are more likely due to declines in demand for alternative credit rather

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

than supply effects.

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 therefore play a role in "pushing" customers out of the traditional 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 high-cost short-term loans more generally 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 borrowing, 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.

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Figure 1: Illustrative example of high-to-low transaction reordering

This figure illustrates the mechanics of high-to-low transaction reordering for a consumer we'll call Annie. Annie 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. Annie's bank charges a $35 fee per overdraft. Under chronological transaction ordering, Annie would only incur 1 overdraft for her rent payment. Under high-to-low transaction reordering, she incurs overdrafts for every single transaction.

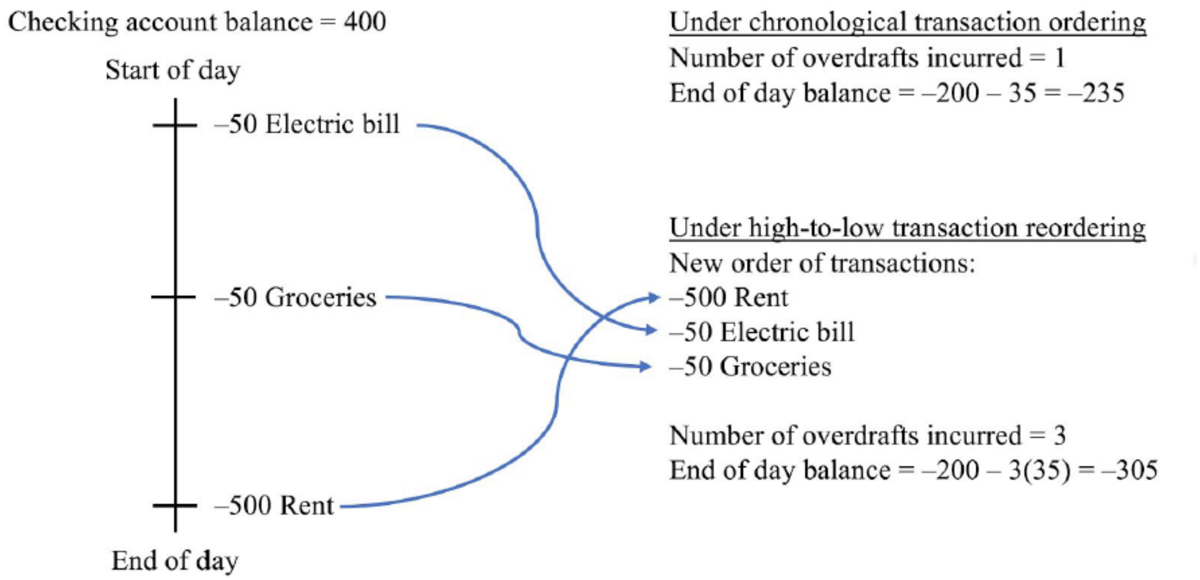


Figure 2: Bank overdraft in response to high-to-low reordering bans

This figure presents the results of our bank-quarter-level difference-in-differences analysis using FFIEC Call Report data. The dependent variable is overdraft-related revenue (defined as the sum of fees associated with deposit accounts and interest income on other consumer loans) as a share of total revenue. Coefficients are plotted for the 4 quarters around the high-to-low reordering ban for a difference-in-differences regression of overdraft-related revenue / total revenue for banks with mandated high-to-low reordering bans relative to matched banks that do not experience mandated high-to-low reordering bans, that share the same primary state, and that lie in the same size decile as the high-to-low reordering-banned bank. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was required to cease high-to-low reordering. Primary state x year-quarter fixed effects and bank fixed effects are included. Standard errors are clustered at the bank level and the year-quarter level.

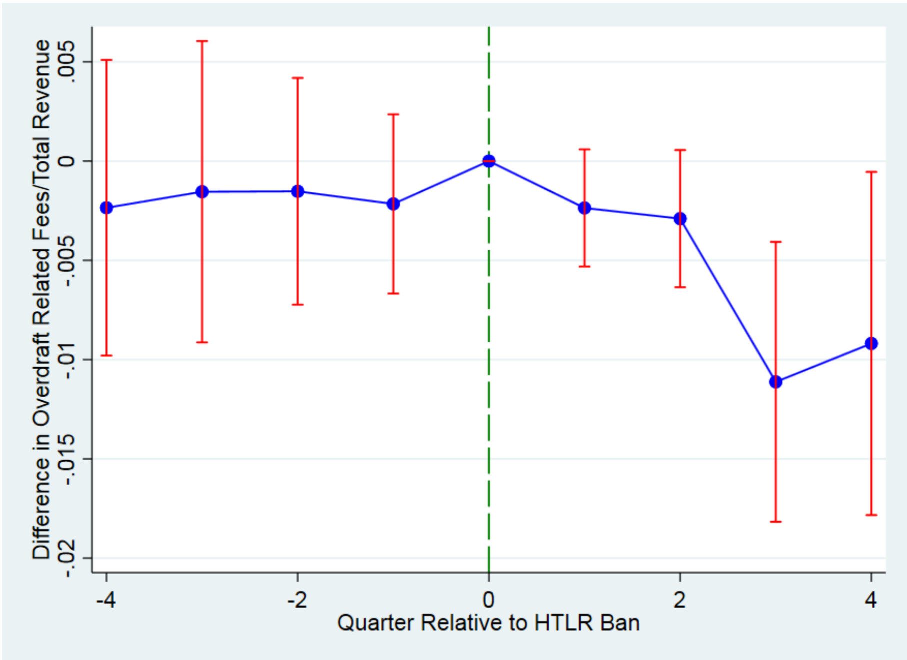
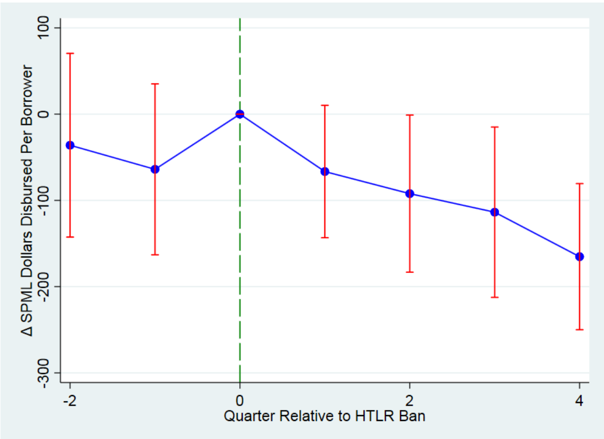


Figure 3: Household demand for payday loans and installment loans
in response to high-to-low reordering bans

This figure presents the results of our zip code-quarter-level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. The samples are restricted to zip codes with below-median income. Because traditional credit bureau data captures a far broader swath of the population than alternative credit bureau data, we focus on the underbanked population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. In Panel A, the dependent variable is the dollar amount of payday loans disbursed per payday borrower in the Clarity dataset. In Panel B, the dependent variable is the dollar amount of installment loans disbursed per low-income installment borrower in the Equifax dataset. Coefficients are plotted for the 4 quarters around the high-to-low reordering ban for a difference-in-differences regression of the dependent variable for zip codes that contain branches of a bank that was required to cease high-to-low reordering, relative to zip codes within 7 miles that contain branches of a bank that was sued but *not* required to cease high-to-low reordering. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was required to cease high-to-low reordering. Neighborhood x year-quarter fixed effects are included. Standard errors are clustered at the year-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.

(a) Payday loans in Clarity data



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

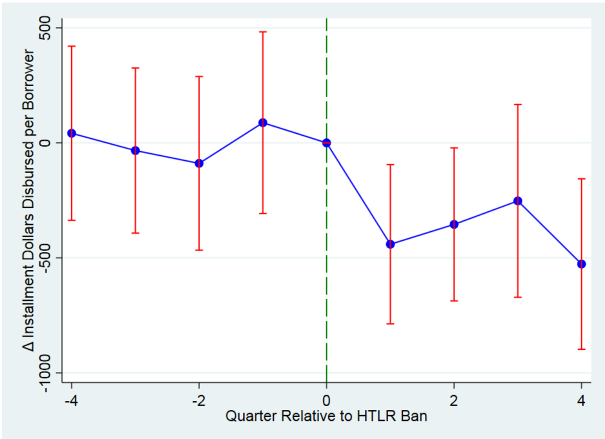
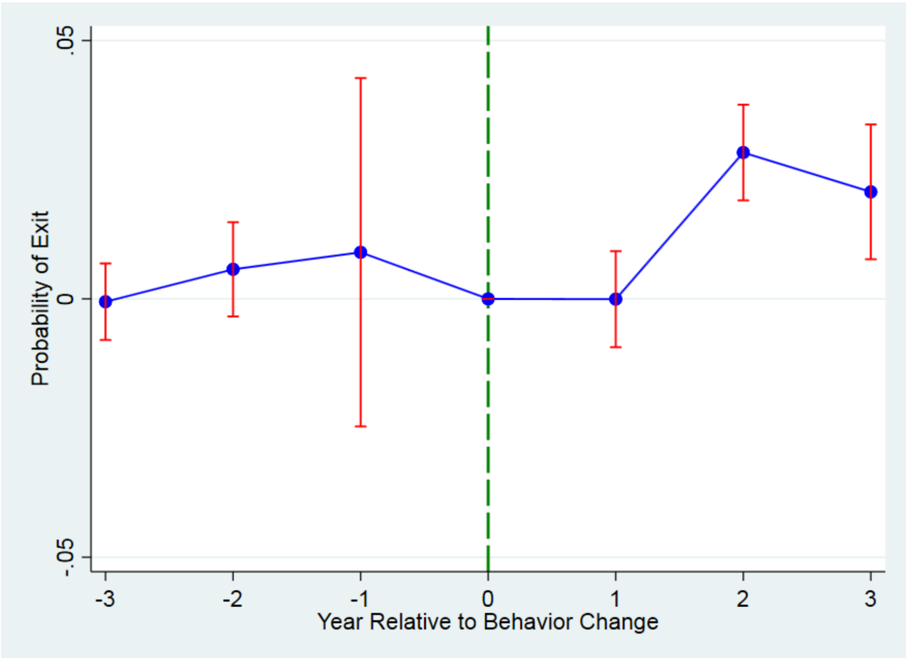


Figure 4: Bank branch closures in response to high-to-low reordering bans

This figure presents the results of our zip code-year-bank-level difference-in-differences analysis using FDIC Summary of Deposits data. The dependent 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. 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 had 2 or fewer branches. Coefficients are plotted for the 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was required to cease high-to-low reordering. Zip code x year and bank x zip code fixed effects are included. Standard errors are clustered at the bank-year level.

(a) All zip codes



(b) Zip codes with few treated branches

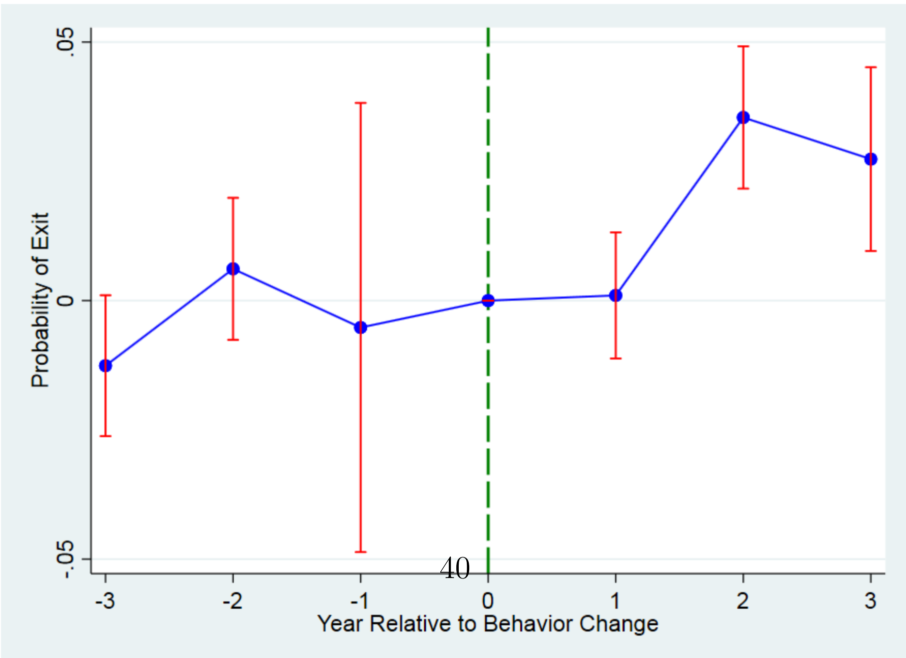


Table 1: Summary statistics for the largest 50 banks

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

Table 2: Summary statistics for Clarity alternative credit bureau data and Equifax traditional credit bureau data

This table provides zip code-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 1 million alternative borrowers over the period 2013–2020. The Equifax traditional credit bureau dataset tracks the traditional credit usage of a representative, ten percent sample of traditional borrowers over the period 2005–2019. We subset to zip codes with below-median income. Panel A provides summary statistics for single-period micro loans (SPML), which are more colloquially known as payday loans, and Panel B provides summary statistics for all loans in the Clarity dataset. Panel C provides summary statistics on installment loans extended to borrowers in the lowest income quintile, and Panel D provides summary statistics on all installment loans in the Equifax dataset.

Panel A: Clarity data: Single payment micro loans (SPML)						
	Mean	Min.	P25	P50	P75	Max.
Dollars disbursed	522	42	300	450	600	3,300
Number opened	1.4	1.0	1.0	1.0	1.0	8.0
Panel B: Clarity data: All loans						
	Mean	Min.	P25	P50	P75	Max.
Dollars disbursed	967	42	400	600	1,000	20,920
Number opened	1.3	1.0	1.0	1.0	1.0	7.0
Panel C: Equifax data: Installment loans extended to the lowest-income-quintile borrowers						
	Mean	Min.	P25	P50	P75	Max.
Dollars disbursed	4483	84	2,670	3,662	5,153	74,116
Number opened	1.6	1.0	1.3	1.5	2.0	6.0
Credit card balance	1,266	0	84	695	1,800	27,304
Credit card limit	2,005	0	204	1,137	2,875	45,303
Total balance in good standing	13,623	0	9,107	12,430	16,691	99,897
Panel D: Equifax data: All installment loans						
	Mean	Min.	P25	P50	P75	Max.
Dollars disbursed	10,790	84	5,969	8,919	13,168	307,394
Number opened	1.4	1.0	1.2	1.4	1.6	6.0
Credit card balance	9,751	0	3,840	8,147	13,741	60,502
Credit card limit	21,221	0	8,616	17,589	29,623	132,372
Total balance in good standing	58,378	0	40,958	56,250	72,993	392,024

Table 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 any of its bank branches belong to a bank that was sued with no high-to-low reordering ban, and it is not a treated zip code. See Table A.1 for the date of the high-to-low reordering ban treatment for each bank. Panel A reports the number of high-to-low reordering 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 reports the total population, median income, unemployment rate, and population proportion 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 alternatively link 2010 bank branch data to 2011 Census demographic data.

Panel A: Branch statistics by zip code

Year	Treated zip codes: Number of high-to-low reordering branches	Treated zip codes: Total number of branches	Control zip codes: Number of high-to-low reordering branches	Control zip codes: 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%

Table 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-year-level data. The dependent 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

Table 5: Household demand for payday loans in response to high-to-low reordering bans

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau data. The sample is restricted to zip codes with below-median income. The dependent variable is the dollar amount or the number of payday loans disbursed per payday 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 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 year-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					
	Dollar amount			Number		
HTLR Ban x Post	-45.35*	-44.60*	-84.84***	-0.222**	-0.210*	-0.289***
	(22.37)	(22.20)	(24.04)	(0.0902)	(0.0930)	(0.0714)
Neighborhood fixed effects	Y	Y	N	Y	Y	N
Year-quarter fixed effects	N	Y	N	N	Y	N
Neighborhood x year-quarter fixed effects	N	N	Y	N	N	Y
Observations	9,870	9,870	9,870	9,870	9,870	9,870
R-squared	0.311	0.317	0.408	0.319	0.334	0.384

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

Table 6: Household demand for installment loans in response to high-to-low reordering bans

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Equifax traditional credit bureau data. The sample is restricted to zip codes with below-median income. Because traditional credit bureau data captures a far broader swath of the population than alternative credit bureau data, we focus on the underbanked population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The dependent 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 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 year-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.

	Installment loans disbursed per low-income installment borrower					
	Dollar amount			Number		
HTLR Ban x Post	-98.61	-166.6**	-200.3**	-0.0352	-0.0342	-0.0314
	(58.10)	(62.05)	(74.70)	(0.0263)	(0.0223)	(0.0297)
Neighborhood fixed effects	Y	Y	N	Y	Y	N
Year-quarter fixed effects	N	Y	N	N	Y	N
Neighborhood x year-quarter fixed effects	N	N	Y	N	N	Y
Observations	38,313	38,313	38,313	38,313	38,313	38,313
R-squared	0.084	0.104	0.278	0.091	0.128	0.294

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

Table 7: Household longer-term borrowing activity in response to high-to-low reordering bans

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. The sample is restricted to zip codes with below-median income. We focus on the underbanked population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. In the first three columns, the dependent variable is the dollars of payday loans disbursed per payday borrower. In the last three columns, the dependent 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window before the high-to-low reordering ban and then 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 year-quarter fixed effects are included across specifications. Standard errors are clustered at the year-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 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**	-200.3**	-230.0**	-264.8***
	(24.04)	(19.62)	(17.58)	(74.70)	(86.77)	(81.08)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	9,870	18,813	31,348	38,313	54,847	70,852
R-squared	0.421	0.450	0.460	0.278	0.283	0.278

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

Table 8: Household longer-term credit health in response to high-to-low reordering bans

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Equifax traditional credit bureau data. The sample is restricted to zip codes with below-median income. We focus on the underbanked population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. In Panel A, the dependent variables are credit card-related variables, specifically the credit card balance and credit card limit per low-income borrower. In Panel B, the dependent variables are credit health measures, specifically the likelihood of a 10-point increase in the Equifax VantageScore for low-income borrowers and the total credit balance in good standing per low-income 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window before the high-to-low reordering ban and then 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 year-quarter fixed effects are included across specifications. Standard errors are clustered at the year-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: Credit card-related variables						
	Credit card balance per low-income borrower			Credit card limit per low-income borrower		
Post event horizon:	1 year	2 years	3 years	1 year	2 years	3 years
HTLR Ban x Post	53.07	110.9*	195.5***	124.8	190.7**	334.8***
	(57.27)	(63.29)	(69.42)	(76.43)	(91.08)	(116.0)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	60,085	86,516	112,260	60,785	87,510	113,541
R-squared	0.366	0.383	0.420	0.386	0.406	0.444
Panel B: Credit health measures						
	Likelihood of a 10+ point increase in credit score for low-income borrowers			Total balance in good standing per low-income borrower		
Post event horizon:	1 year	2 years	3 years	1 year	2 years	3 years
HTLR Ban x Post	0.0473	0.0420	0.0549**	134.4	431.1*	611.5**
	(0.0281)	(0.0260)	(0.0235)	(219.2)	(237.8)	(238.1)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	60,085	86,516	112,260	59,397	85,605	111,196
R-squared	0.269	0.257	0.256	0.251	0.259	0.266

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

Table 9: Household consumption in response to high-to-low reordering bans

This table presents the results of our consumer-quarter-level difference-in-differences analysis using Earnest Research consumption data. The sample is restricted to zip codes with below-median income. The dependent 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window before the high-to-low reordering ban and an 8-quarter window after the high-to-low reordering ban. Neighborhood x year-quarter fixed effects are included across specifications. Standard errors are clustered at the year-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	44.16**	1.148**	13.97*	1.227**	8.885	0.379
	(21.56)	(0.560)	(7.369)	(0.623)	(11.13)	(0.556)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y	Y	Y
Observations	9,912	9,912	7,077	7,077	7,028	7,028
R-squared	0.523	0.518	0.583	0.673	0.566	0.586

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

Table 10: Bank branch closures in response to high-to-low reordering bans

This table presents the results of our zip code-bank-year-level difference-in-differences analysis. The dependent 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 triple interaction term regressor 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***	0.00896***	0.0108***	0.0164***	0.0185***	0.0206***	0.00517***	0.00682***	0.00761***
	(0.000811)	(0.000828)	(0.000939)	(0.000972)	(0.000989)	(0.00117)	(0.00136)	(0.00137)	(0.00158)
HTLR Ban x Post x Low-Income							0.00298*	0.00334**	0.00481**
							(0.00170)	(0.00169)	(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

Table 11: 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-year-level difference-in-differences analysis using Home Mortgage Disclosure Act (HMDA) data and Small Business Administration (SBA) data. The dependent 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 $100k. In the next two columns, we examine small business lending with principal amounts between $100k and $250k. In the last two columns, we examine small business lending with principal amounts between $250k and $1m. 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 and year.

	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 Ban x Post	0.00346	-0.0381	0.0183*	-0.0174	0.00974	-0.0340	0.0107	-0.00571
	(0.0131)	(0.0717)	(0.00869)	(0.0290)	(0.00838)	(0.0232)	(0.00965)	(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 A.1: Bank overdraft in response to high-to-low reordering bans

This table presents the results of a bank-quarter-level difference-in-differences analysis using FFIEC Call Report data. In the first column, the dependent variable is overdraft-related revenue (defined as the sum of fees associated with deposit accounts and the interest income on other consumer loans) as a share of total revenue. In the second column, the dependent variable is the log of the total balance of other consumer loans, which is a proxy for overdraft balances. In the last column, the dependent variable is the log of the total number of 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 a matched bank, i.e. the bank operates in the same primary state as the high-to-low-reordering-banned bank and lies in the same size decile. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. Primary state x year-quarter fixed effects and bank fixed effects are included. Standard errors are clustered at the bank level and the year-quarter level.

	$\frac{\text{Overdraft fees}}{\text{Revenues}}$	Log(Other consumer loans)	Log(Number of insured depositors)
HTLR Ban x Post	-0.00693***	-0.155*	-0.0242
	(0.00185)	(0.0834)	(0.0152)
Primary state x year-quarter fixed effects	Y	Y	Y
Bank fixed effects	Y	Y	Y
Observations	3,671	3,676	3,335
R-squared	0.988	0.993	0.996

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

Table A.2: Bank income and expense in response to high-to-low reordering bans

This table presents the results of our bank-quarter-level difference-in-differences analysis using FFIEC Call Report data. The dependent variable is interest income, non-interest income, revenue (the sum of interest income and non-interest income), or net 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 a matched bank, i.e. the bank operates in the same primary state as the high-to-low-reordering-banned bank and lies in the same size decile. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. Primary state x year-quarter fixed effects and bank fixed effects are included. Standard errors are clustered at the bank level and the year-quarter level.

	Interest income	Non-interest income	Revenue	Net Income
HTLR Ban x Post	-0.00284	0.00499	0.00201	0.00259
	(0.00200)	(0.00293)	(0.00341)	(0.00269)
Primary state x year-quarter fixed effects	Y	Y	Y	Y
Bank fixed effects	Y	Y	Y	Y
Observations	3,671	3,676	3,676	3,671
R-squared	0.957	0.901	0.923	0.674

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

Table A.3: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Alternative control groups by varying geographic area

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the underbanked 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 dependent variable is the dollar amount or the number of payday loans disbursed per payday borrower. In Panel B, the dependent 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 no high-to-low-reordering-banned branches. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. In specifications with neighborhood x year-quarter fixed effects, standard errors are clustered at the year-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 year-quarter fixed effects, standard errors are clustered at the year-quarter level and the state level.

Panel A: Payday loans				
	Dollars of payday loans disbursed per payday borrower			
	Dollar amount		Number	
HTLR Ban x Post	-64.61**	-114.9***	-0.232***	-0.248***
	(29.03)	(25.28)	(0.0813)	(0.0806)
Neighborhood x year-quarter fixed effects	Y	N	Y	N
State x year-quarter fixed effects	N	Y	N	Y
Observations	6,658	13,927	6,658	13,500
R-squared	0.443	0.271	0.408	0.328
Panel B: Installment loans				
	Dollars of installment loans disbursed per low-income installment borrower			
	Dollar amount		Number	
HTLR Ban x Post	-190.1**	-131.00**	-0.0273	-0.0260
	(81.77)	(64.43)	(0.0267)	(0.0124)
Neighborhood x year-quarter fixed effects	Y	N	Y	N
State x year-quarter fixed effects	N	Y	N	Y
Observations	48,798	60,128	48,798	60,128
R-squared	0.286	0.061	0.304	0.059

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

Table A.4: 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 borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau data and Equifax traditional credit bureau data. We focus on the underbanked 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 dependent variable is the dollar amount or the number of payday loans disbursed per payday borrower. In Panel B, the dependent 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 no high-to-low-reordering-banned branches and the zip code lies within the stated neighborhood radius of a treated zip code. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. Neighborhood x year-quarter fixed effects are included. Standard errors are clustered at the year-quarter and the neighborhood level, where each neighborhood is systematically drawn to include treated zip codes and control zip codes within 5 or 10 miles of each other.

Panel A: Payday loans

	Payday loans disbursed per payday borrower			
	Dollar amount		Number	
HTLR Ban x Post	-106.4**	-67.91**	-0.386**	-0.257**
	(36.95)	(27.46)	(0.125)	(0.0987)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y
Neighborhood radius	5 miles	10 miles	5 miles	10 miles
Observations	4,359	16,302	4,359	16,302
R-squared	0.444	0.363	0.450	0.391

Panel B: Installment loans

	Installment loans disbursed per low-income installment borrower			
	Dollar amount		Number	
HTLR Ban x Post	-205.5**	-218.7***	-0.0430	-0.0197
	(96.93)	(54.09)	(0.0303)	(0.0247)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y
Neighborhood radius	5 miles	10 miles	5 miles	10 miles
Observations	20,662	71,555	20,662	74,183
R-squared	0.326	0.226	0.349	0.241

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

Table A.5: Household demand for installment loans in response to high-to-low reordering bans: By income level

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Equifax traditional credit bureau data. The sample is restricted to zip codes with below-median income. We focus on the underbanked population of interest in the Equifax dataset by subsetting to borrowers in the lowest income quintile. The dependent 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 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. Neighborhood x year-quarter fixed effects are included. Standard errors are clustered at the year-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	-200.3***	-494.8**	70.63	-220.4	-270.2
	(74.96)	(211.4)	(292.6)	(499.8)	(866.9)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y	Y
Observations	38,313	33,422	29,938	19,948	11,064
R-squared	0.278	0.282	0.302	0.312	0.373

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

Table A.6: Payday loan inquiry acceptance rate in response to high-to-low reordering bans

This table presents the results of our zip code-quarter-level difference-in-differences regressions using Clarity alternative credit bureau. The dependent 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 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window before the high-to-low reordering ban and a 2-quarter window after the high-to-low reordering ban (because our Clarity sample begins in 2013). Varying levels of fixed effects are included. Standard errors are clustered at the year-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
Year-quarter fixed effects	N	Y	N
Neighborhood x year-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 A.7: 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 borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the underbanked 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 dependent variable is the dollar amount or the number of payday loans disbursed per payday borrower. In the last two columns, the dependent 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. HHI is the zip code-level Herfindahl-Hirschman Index by deposits averaged across neighborhoods. Neighborhood x year-quarter fixed effects are included. Standard errors are clustered at the year-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	-91.59**	-0.278**	-272.0***	-0.0283
	(46.50)	(0.130)	(100.2)	(0.0247)
HTLR Ban x Post x HHI	50.93	0.113	-167.9	0.0110
	(73.16)	(0.195)	(127.2)	(0.0364)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y
Observations	9,870	9,870	38,313	38,313
R-squared	0.408	0.384	0.278	0.295

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

Internet Appendix for "In the Red: Overdrafts, Payday Lending and the Underbanked"

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Abstract

This Internet Appendix provides additional data details and results to complement the findings in the paper.

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Table IA.1: Key events for lawsuits lodged against banks for high-to-low transaction reordering

This table contains a list of banks that were sued by their customers for practicing high-to-low reordering of transactions posted to customers' 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.

Bank	Date of lawsuit filing	Date of settlement approval	Date of high-to-low reordering ban (if any)
Associated Bank	2-Apr-2010	2-Aug-2013	1-Feb-2011
Banco Popular North America	14-Nov-2012	7-Aug-2018	1-Aug-2013
BancorpSouth Bank	18-May-2010	15-Jul-2016	
Bank of America	1-Dec-2008	22-Nov-2011	
Bank of the West	5-Apr-2010	18-Dec-2012	1-Jul-2011
BOKF	17-Aug-2010	13-Sep-2012	
Capital One	18-May-2010	22-May-2015	
Citibank	19-Dec-2011	14-Nov-2014	
Citizens	26-Jan-2010	12-Mar-2013	30-Jun-2013
Comerica Bank	17-Feb-2010	10-Jun-2014	
Commerce Bank	6-Apr-2010	2-Aug-2013	29-Mar-2013
Community Bank	20-Jul-2012	25-Nov-2013	1-Mar-2011
Compass Bank	4-May-2010	7-Aug-2013	12-Mar-2013
Fifth Third Bancorp	21-Oct-2009	29-Jul-2011	1-Apr-2011
Great Western Bank	15-Jun-2010	2-Aug-2013	1-Jul-2010
Harris	23-Apr-2010	5-Aug-2013	31-Mar-2013
HSBC Bank USA	1-Mar-2011	18-Oct-2016	
IBERIABANK Corporation	18-Feb-2011	26-Apr-2012	1-Nov-2011
Independent Bank Corporation	31-Jul-2013	11-Jan-2018	
JPMorgan Chase Bank	24-Jul-2009	19-Dec-2012	29-Mar-2010
M & I Marshall & Ilsley Bank	16-Jun-2010	2-Aug-2013	31-Mar-2013
M&T Bank	21-Aug-2009	13-Mar-2015	1-Jan-2013
National City Bank	17-Feb-2010	1-Dec-2011	
Northwest Savings Bank	7-May-2012	7-Apr-2015	1-Jul-2011
PNC Bank	8-Oct-2009	5-Aug-2013	5-Aug-2013
RBC Bank (USA)	2-Jul-2010		
Susquehanna Bank	29-Jul-2011	1-Apr-2014	1-Oct-2011
Synovus Bank	21-Sep-2010	2-Apr-2015	
TD Bank	15-Dec-2009	18-Mar-2013	
TD Bank, including Carolina First Bank and Mercantile Bank	21-Aug-2013	24-Jan-2020	
Trustmark National Bank	2-Dec-2011	25-Mar-2014	25-Mar-2014
U.S. Bank	17-Apr-2009	3-Jan-2014	24-Jul-2013
Umpqua Bank	29-Dec-2011	28-Apr-2015	
Union Bank	16-Jul-2009	4-Oct-2012	1-Aug-2010
Webster Bank	29-Apr-2010	28-Mar-2011	30-Sep-2010
Wells Fargo & Company	21-Nov-2007	5-Aug-2013	1-Jan-2010
Woodforest National Bank	11-Jan-2012	19-May-2014	1-Mar-2010

Table IA.2: 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-bank-year-level difference-in-differences analysis. In the first three columns, the dependent 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 dependent 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 IA.3: The overdraft 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-quarter-level difference-in-differences analysis. In the first column, the dependent variable is overdraft-related revenue (defined as the sum of fees associated with deposit accounts and the interest income on other consumer loans) as a share of total revenue. In the second column, the dependent variable is the log of the total balance of other consumer loans, which contains chronic overdraft balances. In the last column, the dependent variable is the log of the total number of 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 12-quarter window around the exit. Year-quarter and bank fixed effects are included. Standard errors are clustered at the bank level and the year-quarter level.

	Non-high-to-low-reordering-banned bank		
	Overdraft fees/Revenues	Log (Other consumer loans)	Log(Number of insured depositors)
HTLR Bank Exit x Post	-3.51e-05	0.114	0.0410
	(0.000590)	(0.0908)	(0.0240)
Year-quarter fixed effects	Y	Y	Y
Bank fixed effects	Y	Y	Y
Observations	30,868	30,868	30,868
R-squared	0.911	0.889	0.956

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

Table IA.4: 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 borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the underbanked 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 dependent variable is the dollar amount or the number of payday loans disbursed per payday borrower. In the last two columns, the dependent 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. Neighborhood x year-quarter fixed effects are included. Standard errors are clustered at the year-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	15.95	-0.0195	-68.46	-0.0267
	(30.04)	(0.0798)	(101.1)	(0.0252)
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y
Observations	5,910	5,910	40,839	40,839
R-squared	0.262	0.251	0.293	0.315

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

Table IA.5: Household demand for payday loans and installment loans in response to high-to-low reordering bans: Zip codes with few treated branches

This table presents the results of our borrower-quarter-level difference-in-differences analysis using Clarity alternative credit bureau and Equifax traditional credit bureau data. We focus on the underbanked 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, with less than the median number of high-to-low-reordering-banned bank branches, and with greater than the median number of total bank branches. In the first two columns, the dependent variable is the dollar amount or the number of payday loans disbursed per payday borrower. In the last two columns, the dependent 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. See Table IA.1 for detail on the lawsuit banks, including whether and when each bank was 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 4-quarter window around the high-to-low reordering ban. Neighborhood x year-quarter fixed effects are included. Standard errors are clustered at the year-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	27.2	0.0143	-86.82	-0.0465
	-40.36	-0.109	-108.4	-0.028
Neighborhood x year-quarter fixed effects	Y	Y	Y	Y
Observations	3,949	3,949	14,364	14,364
R-squared	0.258	0.265	0.302	0.300

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