Bank Fees and Household Financial Well-Being

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PRELIMINARY DRAFT

Financial fees are primarily charged when people face temporary liquidity crises. In this paper, we examine the impact of permanent and temporary relief from financial fees on household financial well-being. We address this question by analyzing policy changes from large U.S. banks between 2017 and 2022, which eliminated non-sufficient funds (NSF) fees and relaxed overdraft policies. Using individual transaction-level data, we find that the elimination of NSF fees resulted not only in immediate reductions in NSF charges but also in subsequent declines in late fees, interest payments, and account maintenance fees. However, changes to overdraft policies resulted in only delayed reductions in overdraft fees. While these policy changes improved household financial well-being, as reflected in reduced fees and charges, their impact on the use of alternative financial services, such as payday loans, was limited. Additionally, we find that wealthier households disproportionately benefited from the relaxation of overdraft policies, while lower-income households saw smaller or no effects. Our results suggest that the policy changes were not substantial enough to significantly reduce the financial stress of the most vulnerable households. Finally, as our setting features multiple treatments and variation in treatment intensities, we theoretically motivate and empirically implement a new stacked event study estimator closely related to de de Chaisemartin and D'Haultfoeuille (2024) to address the biases arising from staggered DID specifications.

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1 Introduction

It is crucial to gain a deeper understanding of how to help households experiencing financial stress. While existing work has focused on how changes to income (Agarwal and Qian, 2014; Johnson et al., 2006; Baker, 2018) and credit supply (Gross and Souleles, 2002; Agarwal et al., 2018) affect household spending, there has been less focus on the effects of exogenous liquidity shocks when households face hardship. Many financial products charge fees, such as overdraft, late, maintenance, or non-sufficient fund fees, specifically at the point of low liquidity. We thus study how permanent and temporary relief in the fees charged affect household well-being in the short-, medium-, and longer-term and across the income distribution.

From 2017 and 2022 a number of large banks restructured their depositor fees. We investigate the immediate and long-term effects of bank-level policy changes on bank fees paid, as well as their impact on other financial behaviors, including timely payments and reliance on alternative financial services. Using detailed transaction-level data from individual bank and credit card accounts, combined with the metropolitan statistical area (MSA) level data on bank market shares, our findings suggest that the elimination of non-sufficient fund (NSF) fees had an immediate effect. In contrast, relaxing overdraft fee policies led to a delayed reduction in actual overdraft fees paid. While we observe long-term improvements in household financial health—measured by reductions in interest charges, late fees, and account maintenance fees—the decline in the use of alternative financial services, such as payday loans, is muted. Moreover, wealthier households disproportionately benefit from the changes in overdraft policies on the intensive margin, while lower-income households do not experience similar effects. These results imply that although relaxing budget constraints at the point of low liquidity can give some relief for households, the degree of generosity matters to significantly alter the financial trajectories of the most vulnerable people.

Since 2001, bank fees charged on deposit accounts have been a point of contention (Federal Reserve Board, 2001, 2009; CFPB, 2017, 2024). Consumer regulators view overdraft and NSF charges[1] as regressive since they disproportionately affect lower-income households. Additionally, they are levied at the moment of zero liquidity. Banks, on the other hand, justify these fees as a way to manage risk and enable broader access to deposit accounts.

Several banks revised their deposit fees between 2017 and 2022. Banks adjusted their policies on different fees differently. NSF fees saw only extensive margin changes, as nearly all banks eliminated this category of charge. Overdraft policies, on the other hand, changed

[1]Overdraft fees occur when a customer's bank allows a transaction to proceed despite insufficient funds, often charging a fee for this service. NSF fees, by contrast, are charged when a transaction cannot be processed due to insufficient funds.

along the intensive margin, affecting multiple dimensions. These include increasing the minimum overdraft amount before a fee is applied, implementing a 24-hour grace period before charging a fee, and reducing the number of overdrafts that incur a fee. At the core of these reforms lies the assumption that reducing or eliminating certain fees should directly improve financial well-being, reduce liquidity stress, and mitigate household financial insecurity.[2]

The actual impact of these fee reductions on household financial outcomes, however, remains unclear. On one hand, extending overdraft grace periods could provide much-needed relief to financially stressed households, improving liquidity and reducing reliance on high-cost alternative financial services, such as payday loans or pawnshops. On the other hand, critics argue that these policy changes may not go far enough to address the root causes of financial instability or that their impact may be diluted by other factors, such as ingrained financial habits, access to alternative credit sources, or behavioral responses to changes in fee structures.

We utilize transaction data from a large account aggregator, covering approximately 35,000 individuals, which enables us to observe transaction amounts and descriptions. We can then identify overdrafts, NSF fees, and other indicators of financial well-being, such as late fees, cash-advance fees, and payday loans. We then aggregate the account-level data to an MSA-week level and construct 2019 MSA-level bank market shares using Summary of Deposits (SOD) data from the Federal Deposit Insurance Corporation (FDIC). This provides a metric to determine which MSAs were affected by each policy change and the extent to which they were impacted.

The main empirical challenge is the staggered nature of the twenty-two bank policy changes we study. Since multiple MSAs were treated at different times, we utilize recent advancements in staggered difference-in-differences (DID) methodologies (Baker et al., 2022; Gardner, 2022; Callaway and Sant'Anna, 2021; Abraham and Sun, 2021), specifically the stacked estimation technique. However, we innovate beyond the standard control groups of never-treated and not-yet-treated MSAs. Using never-treated MSAs as controls raises concerns about parallel trends, as these areas lack any top-20 bank branches, making them fundamentally different from treated MSAs. Conversely, using not-yet-treated MSAs as controls limits statistical power—after the first policy change in 2017, most MSAs become treated, leaving only two or three policy shocks available for analysis. Given that treatment and control groups are based on our bank-share metric rather than a direct institutional measure, further reducing the number of policy shocks weakens our statistical power. Instead, we

[2]These motivations originate directly from the banks themselves. See, for example, press releases from Bank of America, TD Bank, and Citibank.

theoretically motivate and empirically implement a novel event study estimator designed to address biases introduced by staggered treatment adoption in DID settings. Building on the work of de Chaisemartin and D'Haultfoeuille (2024), our estimator adjusts for variations in the intensity and timing of policy changes across different regions with differing bank market shares. This approach allows us to more accurately estimate the effects of fee reductions on household financial well-being.

We first examine the immediate impact of banks' policy changes on NSF and overdraft fees. In the same quarter as the policy intervention, we observe a significant drop in fees. Within 18 months, fees decline by 5.5% relative to the average MSA-quarter. Given that NSF fees were eliminated, these results are unsurprising. Next, we analyze both the number of overdrafts and overdraft fees, finding a reduction in both. However, this effect is delayed by two quarters, with a smaller magnitude of only 2.5% relative to the average MSA-quarter. While initially puzzling, this delay is likely due to the nature of overdraft policy changes, which occurred at the intensive margin, limiting their overall impact.

Next, we explore spillover effects on household balance sheets. We find a significant reduction in interest payments and late fees, with declines of 5% to 6% relative to the mean MSA-quarter. This suggests that the policy changes achieved their intended goal—households used their improved liquidity to pay down loans and avoid late fees. We also observe a decrease in account maintenance charges. Since these fees can often be waived if a minimum balance is maintained[3], the policy changes likely improved liquidity, allowing households to meet minimum balance requirements more easily. We do not find a significant effect on payday, fintech, or other high-interest loans.

Finally, we analyze the distributional effects of bank fee changes and find that the benefits are larger for wealthier MSAs than for poorer ones. For each individual, we estimate the average monthly income based on observed "payroll" transactions and compute the median across MSAs. We then divide MSAs into "rich" and "poor" groups based on this median. Preliminary results indicate that NSF fee reductions affected both groups; however, only richer MSAs experienced a significant decline in overdrafts and overdraft fees. This disparity underscores the original policy tension: intensive margin fee changes primarily benefit wealthier households, whereas extensive margin adjustments affect all consumers. A similar pattern emerges in our analysis of spillover effects.

The rest of the paper is organized as follows. Section 2 reviews the related literature, and Section 3 describes the data used in our analysis. Section 4 outlines the theoretical motivation of our unique stacked DID methodology. Section 5 details our empirical strategy, including the construction of our novel control groups, and Section 6 presents our findings.

[3]See `https://www.bankrate.com/banking/checking/how-to-avoid-monthly-fees/`.

Finally, Section 7 concludes.

2 Literature Review

Research on NSF and overdraft fees is present in behavioral economics, consumer finance, and banking regulation. These fees are particularly scrutinized due to their disproportionate effect on low-income and financially vulnerable individuals, as well as their role in enhancing bank profitability.

A significant strand of research focuses on how overdraft and NSF fees impact consumer behavior and welfare. Many studies highlight that banks often fail to adequately disclose fees, leading to consumer confusion and financial hardship. Researchers have studied the potential moral hazard that overdraft protection programs create. For example, banks might encourage customers to overdraw their accounts, assuming that they will either repay quickly or pay hefty fees for overdrafts. This raises concerns about whether overdraft fees are genuinely in the best interest of consumers or are simply a predatory revenue source for banks (Avery et al., 2008).

The decision-making process surrounding overdrafts often involves behavioral biases. Studies have shown that individuals may not fully understand the costs of overdraft protection, often relying on mental shortcuts (heuristics) that lead to suboptimal choices (Stango and Zinman, 2009). Consumers tend to underestimate the frequency with which they incur fees, leading to poor financial planning. Research also delves into the emotional and psychological impact of frequent overdraft fees on consumers. These fees are linked to feelings of financial insecurity, embarrassment, and stress, which can exacerbate existing financial difficulties (Hogarth and Hilgert, 2002).

Overdraft fees disproportionately affect low-income consumers, who may be more likely to face temporary cash shortfalls. These fees are regressive, meaning that they take up a larger percentage of the income of lower-income individuals compared to wealthier consumers. Some studies argue that overdraft fees prevent access to banking services for those who are financially vulnerable. Campbell (2011) argues that individuals who experience frequent overdraft fees may become disillusioned with traditional banking and turn to alternative financial services, such as payday loans or check-cashing services, which tend to be even more expensive.

A number of papers focus on the regulatory environment surrounding overdraft fees. In the US, for instance, the Federal Reserve has introduced regulations to limit the scope of overdraft protection and the ways in which banks can charge these fees (Tufano, 2009). Research has examined the effectiveness of such regulations in reducing consumer harm while

maintaining bank profitability.

From the perspective of banks, overdraft and NSF fees represent a profitable service, particularly in markets where consumers are unaware of the fees' terms or where there is limited competition. These fees contribute to the profitability of banks, especially in a low-interest-rate environment where traditional lending activities may be less profitable.

Finally, studies have looked at the pricing strategies banks use to structure their overdraft fees, such as differentiating between types of transactions or creating tiered fee structures. These practices have raised concerns about price discrimination, where banks charge higher fees to consumers who are least able to afford them (Baker and McIndoe-Calder, 2016). Finkelstein (2008) shows that in markets with limited banking competition, fees may be higher, and consumers may have fewer alternatives. Conversely, increased competition in financial markets may drive down overdraft fees, though such effects are often slow to materialize.

A number of studies explore the effects of regulations like the CARD Act (2009) and the Dodd-Frank Act (2010), which introduced more transparency and consumer protections related to overdraft fees. Researchers have found mixed results, with some evidence that consumers benefit from such reforms but also concerns about unintended consequences, such as the reduction in access to certain banking services (Linden, 2010). An alternative fee structure, such as offering more affordable short-term credit or incorporating a low-cost overdraft protection option, could better serve consumers without sacrificing bank profitability (Ben-David, 2012).

3 Data

3.1 Transactions-Level Bank Account Data

3.1.1 Description

In this paper, we use bank account and credit card transaction data from a large U.S. account aggregator. Partnering with thousands of financial institutions and FinTech firms, they link multiple financial accounts to a single individual while stripping private individual information. While the overall sample includes 50 million individuals, we use a random subsample of about 35 thousand people for an unbalanced panel between January 2018 and May 2023. Each observation consists of an individual identifier, a bank account identifier, a transaction amount, the day of the transaction, a description of the transaction, the zip code of the transaction location (if it is at a point-of-sale terminal), and a broad category name (i.e., "Groceries," "Rent," etc). We use the most frequently observed transaction zip

code for an individual across all their accounts to identify their zip code.

To answer our question, we must be able to identify transactions that are overdrafts and non-sufficient funds fees. We do so by manually searching within the description variable for keywords, such as "overdraft," "insufficient," "od fee," and "od item," to name a few. In turn, we restrict the relevant outgoing transactions to those below 100 USD. Given our interest in the spillover effects of bank policy changes on other indicators of financial well-being, we also do description searches for late fees, interest charges, maintenance fees, early wage access, payday loans, and FinTech loans. Using the board category names, we can also determine income transactions. Once the fee transactions have been identified, we collapse the data to the MSA-week level.

3.1.2 Summary Statistics

In this section, we quantitatively describe the transaction data being used. First, we want to understand the trends of non-sufficient funds fees and overdraft fees. Figure 1 shows the reduction of both these fees over time. A sharp decline in both categories of fees in March 2020 stands out, as indicated by the vertical dashed line. This is the month of the first round of stimulus checks that were part of the CARES Act in response to COVID-19. A reduction of bank fees is expected with this injection of liquidity. The first policy change that occurred in our sample was in September 2020. After March 2020, there is a clear downward trend in non-sufficient funds fees until the end of the sample. Overdraft fees also have a downward trend, though less pronounced.

Next, we consider summary statistics of all transactions, collapsed to the MSA-month-year level. In Table 1, we report means, counts, and percentiles for a number of spending and income variables, including income, bill payments, and essential spending, as well as overdraft fees, non-sufficient funds fees, interest payments, and late fees. As we collapse across all individuals, rather than conditioning on individuals who ever paid an overdraft fee or ever incurred an insufficient funds fee, bank fees are positive only after the 75 percentile value. Overall, this summary table tells us that in an MSA on an average month, we observe about 26 people, of which 60% have a credit card, who spend $1.66 on essential spending, housing, and bill payments for each dollar spent on discretionary spending, and has an after-tax income of $4,500. Consumption patterns and income levels are in line with the Bureau of Labor Statistics Consumer Expenditure Survey[4].

[4]See `https://www.bls.gov/news.release/cesan.nr0.htm`.

3.2 Policy Changes

The Consumer Finance Protection Bureau (CFPB) has long followed bank overdraft and non-sufficient funds fee policies, providing data summaries and research reports. As banks began to change their fee policies, favoring depositors, the CFPB documented these changes for the top 15 financial institutions in the U.S. Figure 2 is a snapshot of their compilation. For each bank in the table, they report fee revenue in 2021, and various types of policy changes. Two patterns noticeably stick out. First, every large bank completely eliminated their non-sufficient funds fee. Second, besides Citibank, Capital One, and USAA Federal Savings Bank, overdraft fee changes were made on the intensive margin rather than the extensive margin. That is, instead of their complete removal, many financial institutions are now allowing for a 24-hour grace window, increasing the minimum amount withdrawn before an overdraft fee was charged, or reducing the number of overdrafts an individual could be charged for.

In order for us to study the impact of these changes in policies, we need the dates of the policy changes. Because overdraft and non-sufficient funds fees have always been controversial, banks were eager to put out news releases about their relaxation of these charges to their customers. Utilizing bank press releases and news articles reporting on the policy changes, we are able to pinpoint when they went into effect. Table 1 reports these dates. For most policy changes, we are able to find the exact day they went into effect. For a few, we can only identify the month or the quarter. In total, we have 30 policy changes across 15 large banks in the U.S.

3.3 Bank Market Shares

Our transactions-level data does not identify the financial institution of each observed account. This is key as policy changes are bank-specific. As a proxy for the actual banks that individuals bank with, we construct 2021 MSA-level bank market shares using bank branch data from the summary of deposits data provided by the Federal Deposit Insurance Corporation (FDIC).[5] For example, an MSA in Greensboro-High Point, NC, has 10 branches of Woodforest Bank, 4 branches of PNC Bank, 10 branches of Bank of America, 18 branches of Wells Fargo, and a total of 144 bank branches across all banks. The corresponding bank shares for Woodforest, PNC, Bank of America, and Wells Fargo are .07, .03, .07, and .13. We describe the use of these bank shares in our analysis in the section below.

[5]See `https://www7.fdic.gov/sod/dynaDownload.asp?barItem=6`

4 Theoretical Motivation

Following Wing et al. (2024), let $m = 1, \ldots, M$ index MSAs, and $t = T_1, \ldots, T_T$, index week-of-years. For each MSA m, let $A_m = \{A_{m_1}, \ldots, A_{m_\ell}\}$ be the ordered set of calendar periods when m is treated. The sub-subscript denotes the order of treatment, and for each $i \in \{1, \ldots, \ell\}$, $T_1 \leq m_i \leq T_T$. If an MSA is never treated, assume $A_m = \{\infty\}$.

We next want to represent causal relationships using potential outcomes. For each MSA m, for each $z \in A_m, z \neq A_{m_1}$, let z_{-d} represent the treatment date d treatments before z. In the case of $z = A_{m_1}$, let $z_{-1} = 0$. $Y_{mt}(z_{-1})$ represents the outcome MSA m would experience in calendar period t under the hypothetical scenario where MSA is never treated at time z, but *was* treated up to and including time z_{-1}; that is the counterfactual state where $\{A_{m_1}, \ldots, z_{-1}\} \subset A_m$, but $z \notin A_m$. Similarly, let $Y_{mt}(z)$ represents the outcome m would experience at t if treated up to including z; that is $\{A_{m_1}, \ldots, z_{-1}, z\} \subset A_m$.

In our modified DID setting, we also consider a pre-z treatment condition, $\Omega_z(\Theta_m, \theta_{m_{-1}})$, conditional on the *degree* of policy exposure for both the treatment and the control group. As discussed in detail in Section 5, we measure if an MSA is treated based on its share of bank branches for the institution making a policy change. We define $\Theta_m = \{\theta_{m_1}, \ldots, \theta_{m_\ell}\}$ to be the set of incremental bank branch share exposures, corresponding to each element in ordered set A_m. For example, consider m, the MSA of Greensboro-High Point, NC as discussed in Section 5. Its first event A_{m_1} is September 2020, when Wells Fargo made an policy change, with its respective bank share .13. This means $\theta_{m_1} = .07$. Finally, we also define $\theta_{m_{-1}}$, which is the cumulative exposure before our sample starts. For example, for the MS m of Greensboro-High Point, NC this would be its share of Wells Fargo, .13 which made a policy change in 2017, plus two times its share of Bank of America branches, $2 * .07 = .14$, which made a policy change in 204 and 2017, resulting in $\theta_{m_{-1}} = .27$. MSAs that are never treated have $\Theta_m = \{0\}$ and $\theta_{m_{-1}} = 0$.

The causal effect of treatment up to and including z relative to treatment up to and including z_{-1} is $\beta_{mt}(z) = Y_{mt}(z) - Y_{mt}(z_{-1})$. The subscripts of β_{mt} means the treatment effect can differ across MSAs and time periods. In our modified DID setting, we are interested in the average incremental treatment effect on the treated (AITT) in a particular week. This distinction is important, as we avoid the "bad" or biased effect (Wing et al., 2024, pp 12) by estimating the effect of every *additional* treatment, instead of measuring the effect from no treatment to treatment. We denote this as

$$AITT(z, t) = \mathbb{E}(\beta_{mt}(z) | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})).$$

The realized outcome at any event time $z \in \cup_{m=1,\ldots,M} A_m$ is defined as the outcome at time

z_{-1} plus the marginal treatment outcome at time z, if treatment at time z actually occurs.

$$Y_{mt} = Y_{mt}(z_{-1}) + \beta_{m_t}(z) * \mathbb{1}\{z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})\} \tag{1}$$

In order to recover causal relationships in our DID setting we need two main assumptions: a non-anticipation assumption, and a common trends assumption.

Assumption 1 *No Anticipation: The average causal effect of being treated at time z is zero for all calendar periods between z_{-1} and z. That is, for period $z_{-1} < t < z$, if pre-treatment condition Ω_z holds,*

$$\mathbb{E}(Y_{mt}(z) - Y_{mt}(z_{-1}) | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) = 0.$$

Assumption 2 *Common Trends: In the absence of treatment exposure at time z, the average change across post-z treatment time periods would be the same in the treated group, $\forall m \in \{1, \ldots, M\}$ such that $z \in A_m$, and the comparison group, $\forall m \in \{1, \ldots, M\}$ such that $z \notin A_m$ and a pre-treatment condition Ω_z holds. For periods $t > z$,*

$$E(Y_{mt}(z_{-1}) - Y_{mt-1}(z_{-1}) \mid z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) = E(Y_{mt}(z_{-1}) - Y_{mt-1}(z_{-1}) \mid z \notin A_m, \Omega_z(\Theta_m, \theta_{m_{-1}}))$$

As in the standard DID framework, these two assumptions are not testable, as they depend on unobserved counterfactual data.

We can now show how our estimation strategies, under the no anticipation and common trends assumptions, will recover the average incremental treatment effect $AITT(z, t)$,

$$\begin{aligned}
\Delta_{IT}^{z,z+k} &= \mathbb{E}(Y_{m,z+k} - Y_{m,z_{-1}} | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) - \mathbb{E}(Y_{m,z+k} - Y_{m,z_{-1}} | z \notin A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \\
&= \mathbb{E}(Y_{m,z+k}(z) - Y_{m,z_{-1}}(z) | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \; - \\
&\quad \mathbb{E}(Y_{m,z+k}(z_{-1}) - Y_{m,z_{-1}}(z_{-1}) | z \notin A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \\
&= \mathbb{E}(Y_{m,z+k}(z) - Y_{m,z_{-1}}(z_{-1}) | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \; - \\
&\quad \mathbb{E}(Y_{m,z+k}(z_{-1}) - Y_{m,z_{-1}}(z_{-1}) | z \notin A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \\
&= \mathbb{E}(\beta_{m,z+k} | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \; + \\
&\quad \Big[\mathbb{E}(Y_{m,z+k}(z_{-1}) - Y_{m,z_{-1}}(z_{-1}) | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \; - \\
&\quad\quad \mathbb{E}(Y_{m,z+k}(z_{-1}) - Y_{m,z_{-1}}(z_{-1}) | z \notin A_m, \Omega_z(\Theta_m, \theta_{m_{-1}}))\Big] \\
&= E(\beta_{m,z+k} | z \in A_m, \Omega_z(\Theta_m, \theta_{m_{-1}})) \\
&= AITT(z, z+k)
\end{aligned}$$

The second equality substitutes the potential outcomes, and the third equality holds by the first assumption. Substituting the treated outcomes expressed as the untreated outcome plus the treatment effect (1) we obtain the fourth equality. The fifth equality comes from re-writing $Y_{m,z+k}(z)$ in terms of $\beta_{m,z+k}$ and $Y_{m,z+k}(z_{-1})$. And finally, repeatedly applying the common trends assumption brings us to our desired conclusion.

In the following section we explain in detail the empirical challenges in our setting, why the literature's standard stacked framework fails to address our concerns, and the pre-treatment conditions Ω_z we use for each treatment and control group methodology we utilize to recover average incremental treatment effect on the treated in any particular relative quarter, $AITT(z, z+k)$.

5 Empirical Strategy

The new DID methodologies were developed to mitigate time-varying confounds (Baker et al., 2022; Gardner, 2022; Callaway and Sant'Anna, 2021; Abraham and Sun, 2021). This occurs as early-treated groups in control groups for later treatments can bias estimates, even if the standard assumptions of parallel trends and no anticipation hold. The solution of many of these strategies is to use the "never treated" or the "last to be treated" groups as the control. Given the empirical challenges of our setting, these standard approaches fall short. We instead develop a modified stacked differences-in-differences (DID) model that addresses both the empirical hurdles and temporal sources of bias.

5.1 Set-up

We need to be cognizant of two empirical challenges before setting up our model.

First, while the policy changes are on an bank level, the transaction data does not link between individual accounts and financial institutions. To overcome this obstacle, we define an MSA to be treated by, or "exposed" to, a bank's policy change if the bank has branches in that MSA in 2019. For example, as detailed in Section 3.3, Wells Fargo's exposure in the Greensboro-High Point, NC, MSA is .13. We utilize both extensive and intensive margin measures of exposure. The extensive measure is 0 or 1 – did a bank with a branch in an MSA have a policy change or not? The intensive margin considers every incremental policy change. For example, again consider the MSA in Greensboro-High Point, NC. Before our sample begins in 2018, Wells Fargo made an overdraft policy change in 2017, and Bank of America made two policy changes in 2014. This means in January 2020, this MSA has a cumulative pre-treatment intensive margin exposure of 1*.13 + 2*.07 = .27, but an incremental intensive

margin exposure of 0. When Wells Fargo makes an additional policy change in September 2020, this cumulative pre-treatment intensive margin exposure increases to .27 + 1*.13 = .40, with the incremental magin exposure just .13. While we use the extensive margin exposure as treatment for all our empirical specifications, for models that use all bank policy changes, we will use the increasing intensive margin exposure to construct control groups.

Second, as policy changes occur in different weeks over the three year period, we must address the issue of biased estimates in staggered DID designs, as discussed in the growing literature (Baker et al., 2022; Gardner, 2022; Callaway and Sant'Anna, 2021; Abraham and Sun, 2021). Following Baker et al. (2022) and Cengiz et al. (2019), we consider a stacked framework. The standard stacked estimation design, which considers only the first treatment and never-treated controls, is limited in scope for our setting for two reasons. One, given our treatment measure is indirect, constructed via an share of bank branch metric, limitation to the first event weakens estimation power. Two, as banks strategically open and close branches, exposed MSAs are not randomly treated; this implies never-treated MSAs are a poor control group. Next, we discuss control group construction innovation that both utilizes all policy change events and rigorously address early versus late treatment comparisons.

5.2 Control Group Construction

In our setting, the exact timing of bank policy changes is plausibly exogenous to the bank clients; however, exposure to bank branches in a given MSA is not random. For example, if we used the "never treated" MSAs as a control group, we would be comparing MSAs where large banks are present to MSAs where they are not. Given the strategic nature of bank branch placement, the underlying economic conditions of the never-treated and treated groups would be very different. The first panel of Figure 3 shows the evolution of overdraft fees between these two groups over time. The correlation of month-to-month percent changes in overdraft fees is -.11, indicating weak comparability. To try and do better, we leverage our measure of MSA-level bank exposure to construct tighter control groups while still minimizing time-varying confounds.

For each policy change, we construct the control and treatment group such that they match on pre-treatment exposure levels. Specifically, we construct a control group which has an equal coarsened distribution of pre-treatment incremental exposure for the past d events. That is, we minimize the differences of the control and treated distributions on six key distributional attributes, mean, min 25%, 50%, 75%, max, for each past event.

As an example in our data, take the Chase policy change in August 2021. We form a control group by choosing untreated MSAs with ex-ante exposure equal to the coarsened dis-

tribution of the treated. The second panel of Figure 3 shows, for this example, the evolution of overdraft fees between these two groups over time, with the red line indicating August 2021. The correlation of month-to-month percent changes in overdraft fees is .36, which is an improvement relative to using the never-treated group as a control. The differences between the treatment and control group ex-post treatment can then be interpreted as the effect of one additional policy change.

We can also map this strategy to our theoretical motivation in Section 4. Let f_z^T be the matrix containing the distributional attributes for any set of MSAs that are treated at time z, where each row represents a past event, and each column a distribution statistics. Analogously, let f_z^C be the matrix containing these attributes for any set of MSAs not treated at time z. Our pre-treatment condition is

$$\Omega_z(\Theta_m, \theta_{m_{-1}}) = \{m, n | z \in A_m, z \notin A_n, \arg\min_{m,n} |f_z^T - f_z^C|\}.$$

Immediately notice, that not all MSAs treated at time z will end up in stack z. For example, without some treated MSA m it is possible to find a control group with smaller differences from the treated. Also note, this minimization is done for each past d events, i.e., row by row. To implement this coarsened distribution match, we utilize the `cem` command in Stata.

5.3 Specification

Every week in which a bank policy change occurs is a treatment event, which has its own treatment-specific "stack" s. Each stack is its own DID event study. As some bank policy changes happen in the same week, a treatment event can represent more than one bank policy change. We then stack all the individual stacks s into a single dataset and measure β, the weighted average of each stack's treatment effect. In our main specification, we estimate dynamic regressions. For an event time k, β_k measures the difference between treated and control observations at k relative to the period before the policy change, $k = -1$. For each stack, we use data within a specified window, K relative periods before and K relative periods after treatment.

$$y_{s,m,t} = \sum_{k \in [-K,K], k \neq -1} \beta_k \mathbb{1}_{s,m}\{t = k\} + \theta_{s,m} + \phi_{s,\tau} + \rho_{s,g,\tau} + \epsilon_{s,g} \tag{2}$$

The outcome variable for a given stack s, MSA m, in week t, is $y_{s,m,t}$. The treatment effects β_k is measured via dummies $\mathbb{1}_{s,m}\{t = k\}$, which take on the value of 1 if MSA m is treated in stack s, and week t is within k relative periods pre/post policy change. We also

include a rich set of fixed effects, where $\theta_{m,s}$ is MSA-by-stack, $\phi_{\tau,s}$ is calendar month, τ-by-stack, and $\rho_{g,\tau,s}$ is state g-by-month-by-stack. Respectively, this allows us to control, within a stack, for MSA attributes that are time-invariant, time trends common to all MSAs, and state-wide time trends both across and between MSAs. Our standard errors are clustered on a state-by-stack level. Finally, when the outcome variable is a count variable, such as the number of overdrafts, we estimate an analogous probit model (see Cohn et al., 2020).

6 Main Results

In this section, we present the causal effects of changes to non-sufficient funds and overdraft fee policies on household financial well-being.

6.1 Effects on Overdraft and Non-Sufficient Funds Fees

In Figure 4, we study the preliminary effect of the banks' policy changes. The top panel reports the effects of non-sufficient funds (NSF) fees. Starting from the quarter of bank policy intervention, there is a significant drop in fees. Within a year and a half of a bank policy change, the NSF fees in an MSA reduce by nearly \$5. Relative to an average MSA quarter, this is an 5.5% reduction. Given the policy changes regarding non-sufficient funds fees were unilaterally at the extensive margin, seeing the immediate drop is as expected.

The bottom panel of Figure 4 report the effect of bank policy changes on overdraft fees. While we observe a reduction in fees paid, it is delayed and not as large of a decline relative to non-sufficient funds fees. As seen in Table 1, for an average MSA, in any given period, the total overdraft fee payments are over 3 times the total NSF fee payments. However, in response to bank policies, the effect is roughly the same in magnitude for both fees. One explanation for this muted result could be the *intensive margin* nature of the adjustments to overdrafts. Only three banks completely removed this type of fee. The others either allowed for a 24-hour grace window, increased the minimum amount withdrawn before an overdraft fee was charged, or reduced the number of overdrafts an individual could be charged for. Given the fuzziness of the policy change, observing only mild effects is a reasonable outcome.

6.2 Spillover Effects

After observing a decline in overdrafts and NSF fees, we now turn to other financial well-being indicators. Once again, applying Equation 2 to the transactions data, we now consider four outcomes: late fees, interest rate charges, account maintenance fees, and any high-interest loan. We see a significant drop in all variables, indicating these policies improve household

financial well-being. Even though 2018 to 2023 was a time period of both decreasing and increasing interest rates, our month-by-stack fixed effects should control for these changes.

6.3 Heterogeneity: Income

In this section, we study the distributional impact of the bank fee changes. We begin by examining the effects based on income levels: how do poorer areas respond compared to richer areas? For each individual, we determine their average monthly income by manually searching the description and category variables in the transaction data. We then find the median of these averages for each MSA and winsorize at the 1st and 99th percentiles. This process provides a distribution of monthly incomes across MSAs, with an overall median of $4,033. We replicate our previous estimations using Equation 2 with two subsamples: poorer MSAs, with median monthly incomes below $4,033, and richer MSAs, with median monthly incomes at or above $4,033.

The effects of the changes in bank policies on non-sufficient funds (NSF) fees and overdraft fees are shown in Figure 6. Interacting the pre- and post- dummies with a dummy for an MSA being above median income, we plot the results for **above median income** MSAs and **below median income** MSAs. Poorer MSAs experience a nearly $5 reduction in NSF fees, consistent with the results in Figure 4. However, the impact of overdraft policy changes does not appear in these geographic areas; there is no change in the overdraft fees following the policy change. In contrast, richer MSAs see a smaller reduction in NSF fees but a significant decrease in both the number of overdrafts and the overdraft fees. These differences suggest that intensive margin changes in bank fee policies benefit wealthier households more than poorer ones, compared to extensive margin adjustments.

Figure 7 shows the estimates of the spillover effects of these bank policy changes by income level. As before, we divide the sample into poorer and richer MSAs based on the median income. Both types of MSAs indirectly benefit from bank policy changes through reductions in late fees, interest payments, account maintenance costs and high interest loans. However, the magnitude of the decrease relative to income is greater for poorer areas. This may be due in part to the fact that both types of policy changes affect wealthier households, whereas only the elimination of NSF fees has a significant impact on poorer households. Finally, it is important to note that assessing differences in interest charges is challenging due to the lack of data on the original borrowed amounts, which complicates the interpretation of differences in interest levels.

These results provide insight into the actual effects of bank policy changes on households. While intensive margin changes to overdraft policies are promoted as a way to alleviate

financial burdens for struggling households, they tend to disproportionately benefit wealthier individuals. In contrast, the elimination of NSF fees offers substantial liquidity improvements for poorer households.

7 Conclusion

This paper examines the impact of recent changes in depositor fee policies by large U.S. banks on household financial well-being. We find that while the elimination of NSF fees led to immediate cost reductions, the effects of relaxed overdraft fee policies were more gradual and less pronounced. Despite reductions in late payments, interest charges, and account maintenance fees, there is limited evidence of significant changes in households' reliance on high-interest loans, such as payday loans. Additionally, we observe a distributional impact: wealthier households benefit more from the intensive margin changes to overdraft policies, while poorer households experience relatively smaller spillover effects from both policy changes. We estimate these effects using a novel control group construction, matching previously treated bank market shares to the coarsened distribution of the previously treated bank share of the newly treated observations.

Our findings suggest that although the policy changes provided some immediate financial relief, they did not fully address the deeper financial stress faced by vulnerable households. The delayed effects of overdraft fee policy changes and the muted impact on the use of alternative credit indicate that reducing these fees alone may not be sufficient to improve long-term financial stability.

Overall, this paper contributes to the literature on financial regulation by providing a nuanced view of the impacts of fee reductions, challenging the assumption that such reforms inherently lead to improved household financial health. It highlights the need for more comprehensive banking policies to effectively address underlying financial vulnerabilities.

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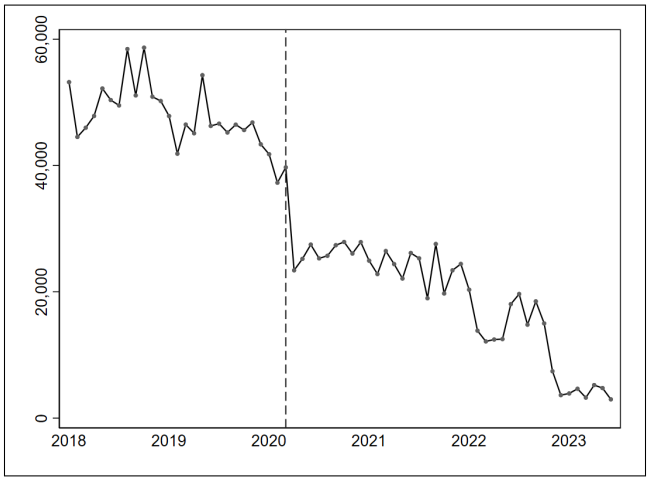
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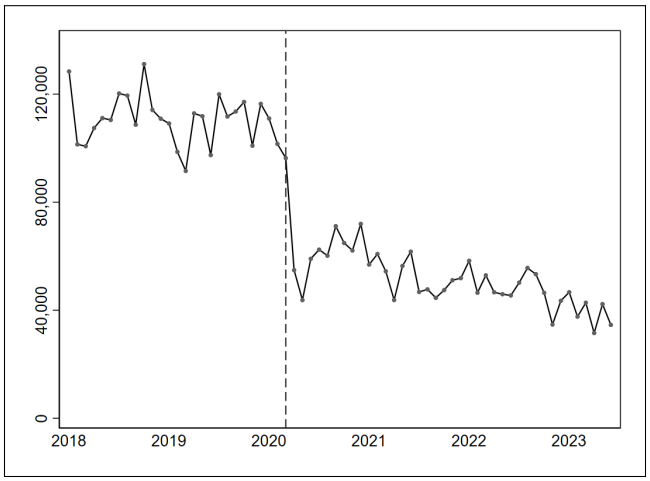
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Figures



(a) Non-sufficient Fund Fees ($)



(b) Overdraft Fees ($)

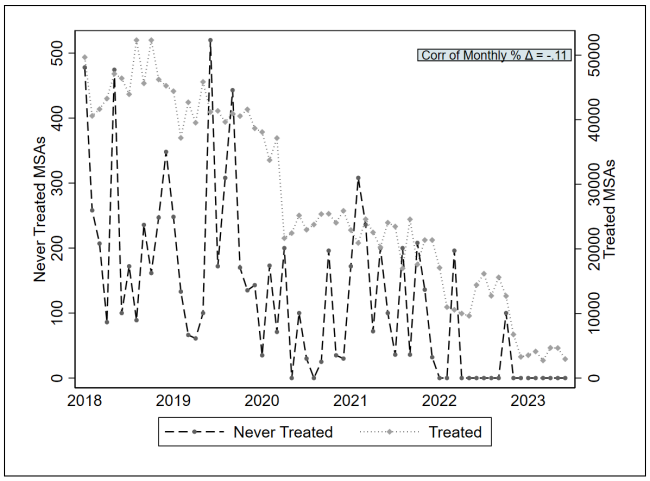
Figure 1: Bank Fees by Month-Year

Notes: This figure shows total amount of bank fees by month between January 2018 and May 2023. Figure (a) plots the amount of non-sufficient funds fees over time, and Figure (b) plots the amount of overdraft fees over time. The dotted line at March 2023 indicates when the first stimulus payments were distributed via the CARES act in response to COVID. As can be seen by this sharp down, this liquidity mitigated bank fees. The first endogenous bank policy change in our sample occurred 6 months later, in September 2020.

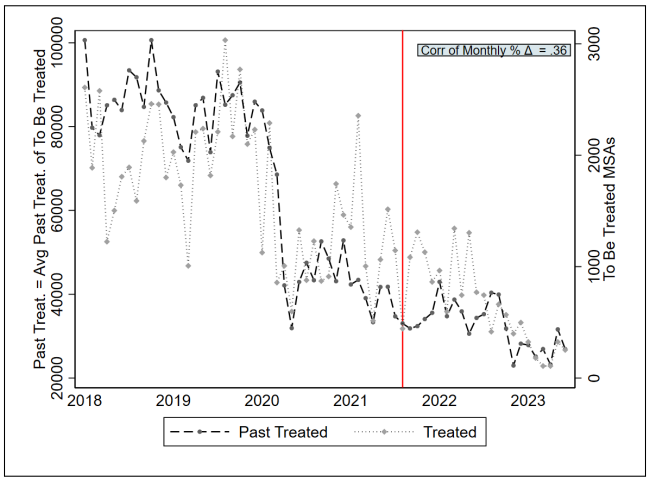
Bank	Overdraft/NSF Revenue Reported for 2021	No overdraft fees on any transactions[1]	No NSF fees	No overdraft fees on debit card purchases[2]	No overdraft fees on ATM withdrawals	No extended/ sustained overdraft fees	Size of overdraft and/or NSF fee[3]	Daily limit on number of overdraft/NSF fees[4]	Cushion before overdraft fee is charged[5]	Extended grace period
Truist Bank	$415 million		✓			✓	$36	3 ($108)	$5	--
U.S. Bank N.A.	$338 million		✓			✓	$36	3 ($108)	$50	next day
Regions Bank	$300 million		✓			✓	$36	3 ($108)	$5	next day
PNC Bank, N.A.	$269 million		✓			✓	$36	1 ($36)	$5	next day
USAA Federal Savings Bank	$197 million		✓	✓	✓	✓	$26	1 ($29)	$50	--
Huntington National Bank	$152 million		✓			✓	$15	3 ($45)	$50	next day
Citizens Bank, N.A.	$145 million		✓			✓	$35	5 ($175)	$5	next day
Woodforest National Bank	$145 million		✓			✓	$32	3 ($96)	$1	--
KeyBank N.A.	$116 million		✓				$20	3 ($60)	$20	--
First National Bank Texas DBA First Convenience Bank	$108 million						$34	2 ($68)	$25	next day

Figure 2: CFPB Bank Fee Policy Changes Snapshot

Notes: The CFPB compiled a list of overdraft and insufficient fund fee policy changes for the top 20 banks in the U.S.. This is a partial snapshot of the table. The complete table can be found on their website.



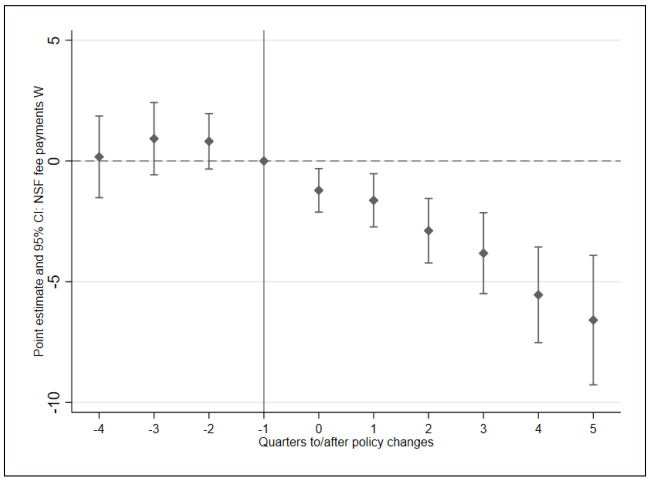
(a) Overdraft Fees ($)



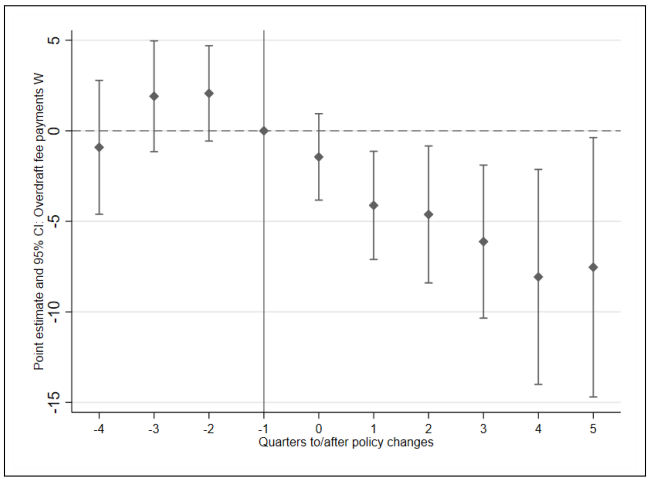
(b) Overdraft Fees ($)

Figure 3: Bank Fees by Month-Year

Notes: This figure shows total amount of bank fees by month between January 2018 and May 2023, by different treated and control groups. Figure (a) plots the amount of overdraft fees overtime for all treated MSAs, and for the never treated MSAs. The correlation between the month-to-month percent changes of each group is -.11. Figure (b) plots the amount of overdraft fees overtime for all MSAs treated in August 2021 and for all MSAs who are not treated in August 2021, but have an ex-ante distribution of bank policy exposure coarsely matched to the ex-ante distribution of bank policy exposure of the August 2021 treated MSAs. The red line indicates the month of the policy change. The correlation between these two groups is .36.



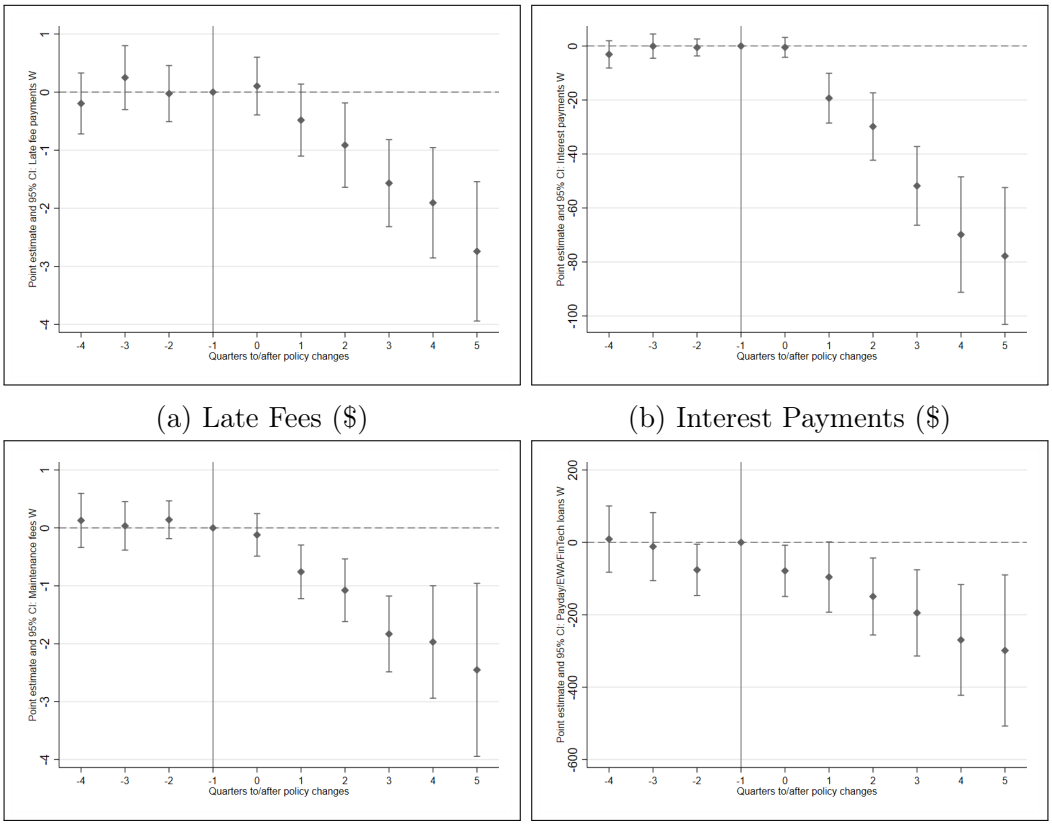
(a) Overdraft Fees ($)



(b) Non-sufficient Fees ($)

Figure 4: Effect of Bank Policy Changes on Depositor Fees

Notes: This figure is the effect an additional change in bank fee policy has on both overdraft and non-sufficient fund fees.The effects are estimated using Equation 2.



(a) Late Fees ($)

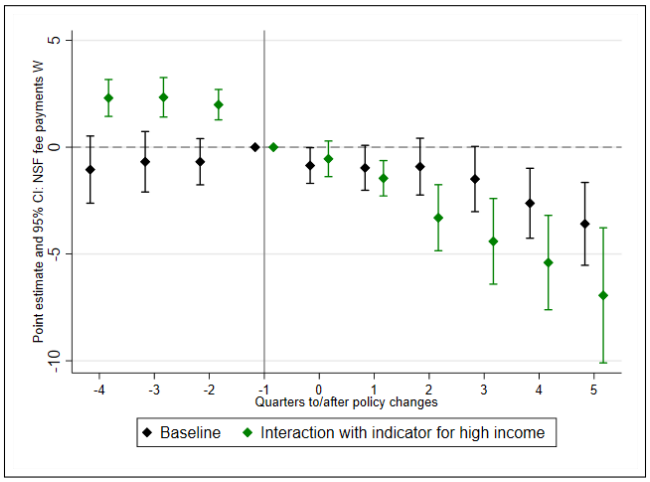
(b) Interest Payments ($)

(c) Account Maintenance Fees ($)

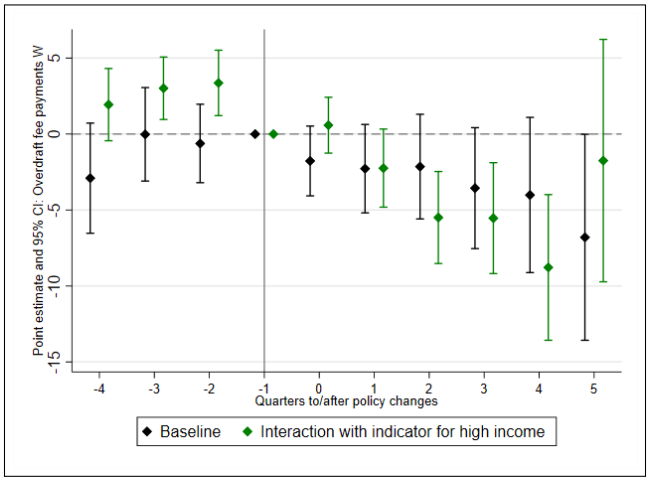
(d) A ($)

Figure 5: Spillover Effect of Bank Policy Changes

Notes: This figure reports ny High Interest Loansthe spillover effects of bank policy changes on overdraft and non-sufficient fund fees on late fees, interest charges, and any high interest loans.



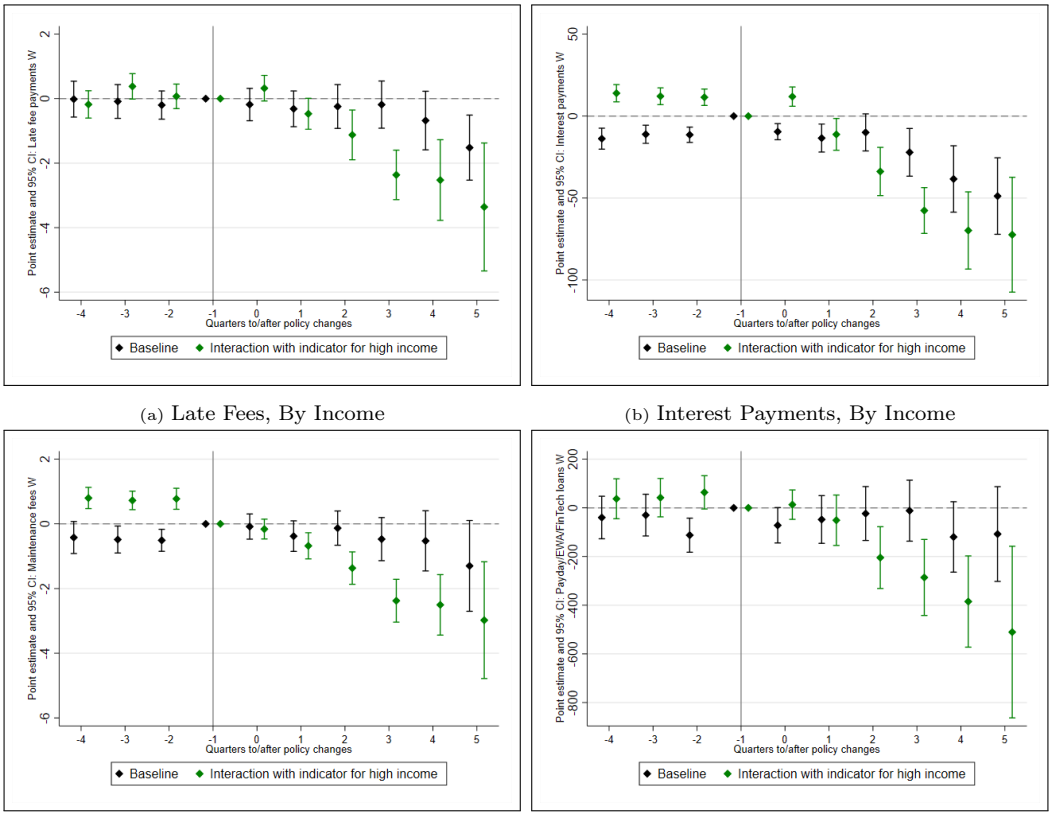
(a) NSF Fees, By Income



(b) OD Fees, By Income

Figure 6: Effect of Bank Policy Changes on Depositor Fees: By Income

Notes: This figure is the effect an additional change in bank fee policy has on both overdraft and non-sufficient fund fees, by income group. The results for **below median income** MSAs are in **black** and the results for **above median income** MSAs are in **green**. These results suggest that intensive marginal changes to bank fee policies (i.e., overdrafts) benefit the rich more than the poor, and extensive marginal changes (i.e., NSF fees) benefit the poor over the rich.



(a) Late Fees, By Income

(b) Interest Payments, By Income

(c) Account Maintenance Fees, By Income

(d) High Int Loan, By Income

Figure 7: Spillover Effect of Bank Policy Changes: By Income

Tables

Bank	Date	Policy Change
Wells Fargo	Nov 2017	"Rewinds" overdraft fee if sufficient funds arrive by next morning
	Sep 2020	Introduced Clear Access Banking - account without overdraft fees; no service fee for ages 13-24
	Q1 2022	Eliminated NSF fees, transfer fees between linked accounts; introduced 24-hour overdraft grace period and small dollar loans
Chase	August 2021	No overdraft fees for amounts <$50; next-day grace period for sufficient funds
Bank of America	2010	Eliminated overdraft fees with debit/ATM fees
	Mar 2014	Introduced SafeBalance account - no overdraft fees, $5 monthly fee, no paper checks
	Sep 2021	Allowed linkage of up to 5 accounts to avoid overdraft fees
	Feb 2022	Eliminated NSF fees
	May 2022	Reduced overdraft fees from $35 to $10; eliminated transfer fees
TD Bank	Approx. Feb 2022	Eliminated NSF fees; no overdraft fee for amounts <$50; next-day grace period
US Bank	Jan 3, 2022	Eliminated NSF fees
	Jun 2022	Increased no-fee overdraft threshold from $5 to $50; 24-hour grace period
Regions Bank	Mar 17, 2022	Eliminated overdraft transfer fees
	Jun 16, 2022	Eliminated NSF fees; maximum three overdraft fees per day
PNC Bank	Apr 13, 2021	Introduced 24-hour grace period for overdraft fees
	Aug 2022	Eliminated NSF fees
Citibank	Summer 2022	Eliminated overdraft fees, returned item fees, NSF fees, and overdraft transfer fees
Capital One	Feb 2022	Eliminated overdraft and NSF fees
USAA Bank	Dec 8, 2022	Eliminated NSF and overdraft fees
Huntington	Jul 2022	Reduced overdraft fee from $36 to $15
Citizens Financial	Oct 7, 2021	Introduced 24-hour overdraft reversal
	Sep 26, 2022	Eliminated NSF fees
Woodforest Bank	May-Sep 2022	Eliminated NSF fees
Key Bank	Sep 16, 2022	No fee for overdrafts $20 or less; eliminated NSF fees; reduced overdraft fees and frequency
Fifth Third Bank	Jun 23, 2022	Eliminated NSF fees

Table 1: Timeline of Bank Overdraft and NSF Policy Changes (2010-2022)

Notes: To determine the time of the policy change, we crosscheck each policy change reported by the CFPB with official bank press releases and newspaper articles. The most granular dates are to the day, most broad is within quarter. Corresponding to our transaction data date range, we are able to study 22 of 25 bank policy changes made between 2018 and 2023.

	Count	Mean	10%	25%	50%	75%	90%
Overdraft Fees	51,494	94.5	0.0	0.0	0.0	72.0	261.0
Interest Payments	51,494	406.6	0.0	0.0	78.2	359.6	1,088.7
Late Fees	51,494	16.9	0.0	0.0	0.0	15.0	50.0
Non-Sufficient Funds Fees	51,494	28.5	0.0	0.0	0.0	0.0	70.0
Has High-Interest Loan	51,494	1,626.1	0.0	0.0	0.0	75.0	3,276.2
Essential Spending	51,494	23,469.7	439.9	1,495.8	5,920.0	19,907.1	54,835.2
Discretionary Spending	51,494	32,332.1	471.6	1,896.9	8,114.5	28,119.3	76,809.4
Bill Payments	51,494	17,418.9	283.1	1,029.3	4,089.2	14,181.2	40,005.6
Housing Expenses	51,494	13,049.0	0.0	31.8	2,612.5	10,103.9	27,375.3
Other Spending	51,494	64,672.6	537.5	2,553.2	10,933.7	44,905.4	139,862.9
Income	51,494	117,228.5	1,609.8	6,878.8	26,896.3	95,202.7	256,405.4
Savings	51,494	736.6	0.0	0.0	0.0	193.0	1,620.8
Number of People	51,494	25.7	1.0	2.0	8.0	23.0	60.0
Has Credit Card	51,492	0.6	0.1	0.4	0.6	0.8	1.0

Table 2: Summary Statistics

Notes: This table reports the distributions of various spending and income categories. These statistics are from the transactions data, collapsed on an MSA-month level. The categorizations of spending come from the account aggregator's categorization, as well as, manual searches in transaction descriptions. After collapse, all variables are winsorized to the 1st percentile and the 99th percentile. An individual is considered to have a credit card if they ever have at least one credit card payment. Only "Has Credit Card" is a binary variable, all others are continuous.