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The Effect of BNPL on Consumer Debt and the Ability to Repay Non-BNPL Debt Obligations*

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Abstract

In this paper, we leverage a novel dataset of Buy Now, Pay Later (BNPL) applications from the six largest BNPL providers in the United States linked to de-identified credit records. We use a fuzzy regression discontinuity design that exploits discrete jumps in the probability of loan approval at provider score thresholds to estimate the impact of first-time BNPL use on non-BNPL consumer debt and consumers' ability to repay non-BNPL debt obligations. We find that borrowers increase their BNPL balances one quarter after their first application, but the increase dissipates over time. We do not detect analogous increases in non-BNPL debt balances and find some evidence of substitution between BNPL and similar loan products. The results do not support a conclusion of negative impacts of first-time BNPL use on the ability of borrowers to repay non-BNPL loan obligations nor on several measures of financial distress.

JEL: Keywords: BNPL, pay-in-four, unsecured credit, debt, repayment, financial distress

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1 Introduction

Buy Now, Pay Later (BNPL), or pay-in-four, loans are among the fastest growing consumer financial products. From 2019 to 2021, BNPL loan originations grew by 215 percent and from 2021 through 2023 by 31 percent.[1] BNPL lenders do not generally furnish any information, including loan amounts or performance, to credit bureaus. Consequently, some large banks and other market observers have deemed BNPL loans "shadow," or "phantom" debt and raised concerns about potential spill-over effects of BNPL loans onto other consumer credit products (Quinlan, 2023). While consumer advocates have called for increased regulation in this space due to potential consumer harms, very little is known about the product's impact on consumer financial outcomes.[2] This paper fills that gap by leveraging a novel dataset of pay-in-four applications from the six largest BNPL lenders in the United States.

Economic theory predicts possible income effects, substitution effects, or both from the extension of BNPL as a new type of credit to consumers. Holding consumption constant, BNPL borrowers could (partially) substitute from one form of unsecured credit to BNPL. Due to the high share of consumers who revolve on at least one credit card and thus typically pay an annual percentage interest rate (APR) above 20 percent, substitution from an interest-bearing loan to an interest-free loan may decrease the amount consumers pay to finance their debt and thus the amount of debt borrowers hold over time.[3] However, the historical record has documented that consumers generally do increase consumption and spending when granted access to new financial instruments or increased lines of credit for unsecured consumer debt (Feinberg, 1986; Karlan and Zinman, 2010; Hyman, 2012; Chava et al., 2021; Grodzicki and Koulayev, 2021; Fulford and Schuh, 2024). Therefore, it is an empirical exercise to quantify the impact of BNPL use on total unsecured consumer debt.

An increase in consumer debt could be welfare-increasing or welfare-decreasing. However,

[1] These numbers are measured as compound annualized growth rates.

[2] See for example Gdalman, Greene, and Celik (2022) and Kleinbard, Sollows, and Udis (2022).

[3] Shupe, Li, and Fulford (2023) find that 69 percent of self-reported BNPL borrowers between 2021 and 2022 carried interest on at least one credit card and they show that consumers with characteristics similar to the BNPL borrower population paid, on average, an APR of 22-24 percent when revolving on a credit card during this period.

over-indebtedness, which we define as the inability to repay debt obligations, is generally welfare-decreasing in the long-run and can have wide-reaching consequences for consumers.[4] Unique features of the pay-in-four product and borrower population render it particularly interesting for the study of its impact on over-indebtedness. First, as long as borrowers repay their loans on time, lenders do not charge interest or finance fees. Second, while lenders do conduct underwriting of these loans, credit score requirements are lower than for other traditional credit products and the product attracts applicants that exhibit higher levels of financial distress than the average consumer. Third, despite its popularity among consumers with subprime credit scores and those without a credit score, these consumers repaid their BNPL loans 98 percent of the time between 2019-2022. In comparison, credit card default rates for the same population and time period reached above 10 percent (Shupe and DeLuca, 2025). Fourth, and closely related to the high levels of BNPL repayment, most BNPL borrowers must sign up for automatic payments, which may mean that they repay their BNPL loans, but could be left without sufficient funds to repay non-BNPL debt obligations. Finally, BNPL continues to integrate into e-commerce, with the potential to grow in importance over time.[5]

In this paper, we combine a representative, de-identified sample of application-level BNPL loans linked to credit bureau data to study the causal effect of first-time BNPL use on non-BNPL unsecured debt and the ability of consumers to repay non-BNPL credit obligations. Causal identification stems from a fuzzy regression discontinuity design (RDD) using the credit score that BNPL firms used to reject or accept each loan application as the running variable. We follow the sample of de-identified consumers for 18 months after their first application to examine whether BNPL origination led to differences in overall credit balances, revolving debt, credit card finance fees,

[4]The economic and psychology literature has identified several potential factors that can lead consumers to consume more today than is in their long-run self-interest, including negative shocks, present bias or present-focused preferences (Laibson, 1997; Angeletos et al., 2001; Kőszegi and Szeidl, 2013; Lian, 2023; Laibson et al., 2024), temptation (Gul and Pesendorfer, 2001, 2004; Fudenberg and Levine, 2006), marketing cues (Laibson, 2001) self-control issues (Thaler and Shefrin, 1981), lack of financial literacy or sophistication (Heidhues and Kőszegi, 2010; Fernandes, Lynch Jr, and Netemeyer, 2014; Lusardi and Tufano, 2015) and social or attitudinal norms (Sotiropoulos and d'Astous, 2013).

[5]See for example Bian, Cong, and Ji (2023), who outline the widespread and growing use of BNPL in e-wallets in China, including its crowding out of other digital payment options.

credit card utilization and delinquencies on non-BNPL unsecured loans, comparing consumers with scores just above the approval thresholds to a control group of unsuccessful applicants with scores just below the approval thresholds.

We find that consumers increase their BNPL use for one quarter following their first BNPL loan origination, but that this effect dissipates in subsequent quarters. In contrast, non-BNPL consumer debt does not increase for consumers induced to take out a BNPL loan and we find suggestive evidence of some substitution between BNPL and closely-related non-BNPL products. The results do not support the conclusion that access to BNPL causes negative impacts on any of the measures of financial distress in non-BNPL products, including past-due non-BNPL debt, credit card utilization, the share of credit cards revolving some debt and the amount of finance charges paid on credit cards.

This paper contributes to the nascent economic literature on the impact of BNPL loans on consumers' financial health and is the first to focus exclusively on pay-in-four loans. Di Maggio, Williams, and Katz (2022) and DeHaan et al. (2024) use bank account and credit card transaction data to investigate how BNPL and larger POS installment loans impact overdraft fees and credit card costs. Guttman-Kenney, Firth, and Gathergood (2023) provide a first descriptive paper using credit card transactions to identify BNPL purchases on credit cards in the UK. Due to the transaction-level nature of these datasets, it is not possible to distinguish between traditional point-of-sale (POS) installment loans that typically charge interest and smaller, pay-in-four loans that do not, because many pay-in-four providers also offer traditional POS installment loans. Consequently, identifying BNPL loans by the name of the provider inevitably yields loans of both types.[6] Similarly, Papich (2024) measures the effect of the expansion of the Affirm-Walmart partnership on financial well-being, including both traditional and pay-in-four loans. Important differences exist between the pay-in-four and traditional POS installment products, from both a regulatory

[6]Some evidence that this is the case can be found by comparing the years of analysis, which include years prior to the pay-in-four product entering the US market, as well as amounts that are far larger than the average pay-in-four loan (between $135-$142). See Shupe and DeLuca (2025) and Kleinbard, Sollows, and Udis (2022) for descriptive statistics on the typical pay-in-four product based on data from the largest six BNPL lenders, which overlap with those listed in these studies.

and econometric perspective. We outline these differences and their importance for understanding impacts on consumers in Section 2.

In addition to causal studies of installment and pay-in-four loans, surveys of BNPL consumers have provided important descriptive evidence on the product and consumers who use it (Akana, 2022; Shupe, Li, and Fulford, 2023; Stavins, 2024; Akana and Doubinko, 2024). Many of these consumer survey-based studies acknowledge the difficulty *consumers* have distinguishing between BNPL products and other traditional forms of credit often provided by the same firms and encourage further research into the pay-in-four product specifically. In contrast, lenders have no difficulty distinguishing between pay-in-four BNPL loans and larger POS loans as they generally treat the pay-in-four BNPL product as a separate business model from POS loans. This implies that the BNPL firms were able to easily parse out this product for the data collection underlying this analysis.

Finally, this paper contributes more generally to understanding how the introduction of a new financial product impacts the borrowing behavior of consumers. In the personal loan space, Jagtiani and Lemieux (2018) and Jagtiani and Lemieux (2019) document the use of non-traditional alternative data by a larger fintech lender, LendingClub, to offer lower loan prices to some consumer segments compared to traditional credit products, suggesting that fintech lenders may have a unique impact on consumers with low traditional credit scores. Di Maggio and Yao (2021) find evidence that fintech lenders for unsecured personal loans may have transitioned their targeted consumer group over time from low-credit-score borrowers to higher quality borrowers with high credit card utilization. They also find that fintech borrowers are more likely to default compared to non-fintech borrowers. We show evidence that consumers with subprime credit scores are over-represented among the BNPL borrowers, but do not find that use of the BNPL fintech product leads them to default on non-BNPL obligations.

The rest of the paper is structured as follows. Section 2 provides institutional background on the BNPL product as well as important differences to traditional POS installment loans. Section 3 introduces the data, sample, quasi-experiments and our measures of financial distress. Section

4 outlines our fuzzy RDD research design, Section 5 presents results and several robustness and validation tests and Section 6 concludes.

2 Institutional Background

2.1 Product characteristics

BNPL, also referred to as pay-in-four, is generally understood as a point of sale financing option repayable in four or fewer installments, with zero interest or finance fees. Some of the lenders do, however, charge late fees when the consumer misses a payment. Consumers typically pay 25 percent of the gross merchandise value at the time of purchase and spread the remaining 75 percent across three further payments every two weeks from the purchase date. BNPL transaction amounts tend to be relatively small, with an average gross merchandise value of $135-$142 between 2019-2022 (Kleinbard, Sollows, and Udis, 2022; Shupe and DeLuca, 2025). Figure 1 displays the distribution of original transaction amounts for our empirical sample, which appears similar to the distribution of gross merchandise value for all originations from the six BNPL lenders used in a recent CFPB research report (Shupe and DeLuca, 2025).[7]

Figure 2 depicts the types of purchases borrowers made using BNPL in two samples; the raw (average) sample includes originations from the entire 16 percent random sample of all BNPL originations and the experimental sample includes only originations from the analysis dataset used in this paper. Both samples exhibit very similar shares, with over 50 percent of purchases in apparel, approximately 12 percent in mass market and roughly 10 percent each in beauty and personal effects.

Among the six largest BNPL providers in the U.S., one firm began lending with the pay-in-four product in 2017, three began in 2019 and two in 2020. Correspondingly, the number of BNPL loans originated increased from just over 100,000 daily across the U.S. in 2019 to over one million by 2022 (Shupe and DeLuca, 2025). The Consumer Financial Protection Bureau estimates that

[7] Compare to Figure 1 in Shupe and DeLuca (2025).

roughly one in five adults, or 21 percent, borrowed using BNPL at least once during 2022 (Shupe and DeLuca, 2025).

Regulators have flagged the practice of BNPL providers signing consumers up for automatic payment as a default account setting as a potential harm to consumers (Udis, Kleinbard, and Sollows, 2022). While default rates for BNPL loans remain far below those of credit cards, even among the same borrowers, regulators have expressed concerns that the installment nature of BNPL loans may lead consumers to overextend themselves which, paired with the automatic payment default setting for BNPL loans, may lead consumers to miss payments on their non-BNPL obligations (Udis, Kleinbard, and Sollows, 2022). This paper explores these potential concerns empirically.

Between 2019-2022, the years covered by the quasi-experiments in this paper, lenders generally conducted a soft pull of applicants' credit scores as part of their underwriting process to approve pay-in-four loans. Some firms used external consumer credit scores available for purchase from credit bureaus; other firms developed their own internal scores that used an external credit score as a central input variable.[8] Based on either an external or internal score threshold, firms approved or rejected applications for credit. In some cases, firms approved consumers for credit after extending them a "counteroffer" that either approved the consumer for a lower credit amount or required a higher down payment than the standard 25 percent of gross merchandise value. As we discuss later, we exploit BNPL approval discontinuities at the thresholds used by BNPL providers to determine loan approval for 20 different quasi-experiments. We define quasi-experiments based on the unique combination of the BNPL provider, merchant, and purchase date, which determines the distinct approval cut-off that we use for causal identification.

2.2 Differences between BNPL and POS installment loans

From a regulatory perspective, consumer protections under the Truth in Lending Act (TILA) and Regulation Z, including the Electronic Funds Transfer Act, apply to traditional POS installment loans, but not generally to pay-in-four products, provided they do not charge interest or finance

[8] We refer to the scores used by the BNPL firms as "credit scores" or "scores" throughout the rest of the paper.

fees and are structured to be repaid in four or fewer installments.[9] Consequently, the regulatory discussions surrounding BNPL have been focused on the pay-in-four product, although there currently exists no causal evidence focusing on the impact of this product on consumers' financial health.

From an econometric perspective, the purchase of large appliances that constitute typical transactions under traditional POS installment loans are more likely to coincide with other income shocks such as moving, job changes, marriage or changes to family composition, compared to the small purchases typically funded by the pay-in-four BNPL product, and the econometrician cannot typically observe or control for such simultaneous shocks. The average POS installment loan amount is around $800 (compared to $135-$142 for a six-week, pay-in-four loan), repaid over an average of 8-9 months, with some loans lasting 12 or even 24 months (Dikshit et al., 2021). Third, the fact that installment loans often charge interest can plausibly influence the impact of those products on consumers compared to zero-interest BNPL. Finally, as outlined further in the paper, borrower profiles plausibly differ, as the pay-in-four product only requires a soft credit pull while typical installment loans often require a hard credit pull and have different underwriting standards reflective of their larger loan amounts.

3 Data

The foundational data source we rely on is an approximately 16 percent random sample of all pay-in-four applications to the six largest BNPL providers from the time these firms began originating BNPL loans in the United States through December 2022. The sample was drawn based on having a day of birth on one of five days (in any month) throughout the year. We further restrict the sample to individuals born between 1953 and 2006. Based on the application-level data, we identified 20 quasi-experiments and then obtained the de-identified credit records for the consumers involved

[9]These regulations include protections such as ability to repay assessments, the types of disclosures available, the rights to assistance with questions or errors, and a consumer's private right of action. For more on these regulations, see https://www.consumerfinance.gov/rules-policy/regulations/1026/1/.

those experiments from one of the large national consumer reporting companies (NCRC).[10] We used probabilistic matching to link BNPL applicants to their de-identified credit records.[11] This method allowed us to identify the first time an individual applied for a BNPL loan with any of the BNPL firms. We allocate each consumer to the quasi-experiment that corresponds to their first BNPL application only, and therefore avoid including consumers in multiple quasi-experiments.

Quasi-experiments take place between August 2019 and December 2022. Figure 3 shows the timeline and duration for each. Across the 20 quasi-experiments, the dataset for analysis consists of approximately 392,000 unique consumer observations. For these consumers, we observe all of their BNPL loans beginning from the first application through 2022 as well as their traditional, non-BNPL loan obligations present in the credit records. The data contain credit scores, traditional credit tradelines and subprime alternative financial services from December 2018 through September 2023, on a quarterly basis. Specifically, when we refer to non-BNPL debt we include credit cards, personal loans, retail loans and subprime alternative financial services such as payday and subprime installment loans; when we refer to total unsecured debt, we additionally include BNPL borrowing.

3.1 Sample

The experimental sample in this paper focuses on the *marginal* BNPL borrower, which includes both applicants who take out a loan and those who do not. These consumers are, by definition, on the threshold of acceptance or rejection for BNPL credit and those most likely to be impacted by potential regulatory changes, underwriting strategies or other business decisions. Compared to the average BNPL applicant with a credit score, the marginal applicants in the quasi-experiments have lower credit scores on average. Figure 4 shows the share of first-time applicants in the experimental sample in different credit score categories, using a commercially available credit score. Nearly 80

[10]Each of the firms from which we identified quasi-experiments "hashed", or encrypted, their consumer-level information using the same method as the NCRC such that we could merge together the de-identified, encrypted fields.

[11]Available fields differed by firm, but included encrypted values for: social security number, date of birth, full name and address.

percent of first-time applicants have a subprime credit score and three percent have no score.

Among BNPL borrowers in the sample, all loans were originated with consumers on autopay. Of those borrowers, 9 percent removed themselves from autopay at some point during the six-week loan. Table 1 shows these shares for all BNPL borrowers in the sample. The table also displays default rates for the experimental sample of borrowers. Consumers in the experimental sample defaulted on 10 percent of trades, or 11 percent of the total amount borrowed.

Pre-treatment financial constraints are also reflected in applicants' credit card utilization and the share of consumers revolving on at least one credit credit card. Table 2 displays means of credit card utilization, the number of quarters sample consumers revolved debt on a credit card, the share of credit card tradelines on which consumers revolved some debt, and the total estimated amount of finance charges paid on revolving credit card debt during the four quarters directly preceding first-time BNPL use. On average, sample consumers were using 91% of their available credit card limits, and consumers who had at least one tradeline that could be classified were revolving some debt 90% of the time. Over the course of the year, consumers who were identified as paying some financing charges paid $188.27, on average.

3.2 Identifying quasi-experiments

In order to identify application approval discontinuities, we used information provided by the firms regarding score cutoffs at different points in time. We then validated this information by graphing the relevant score variable against the probability of acceptance for various groups. While these groups varied by firm, they often depended on time, the acquisition model and merchant. Because many merchants had bilateral agreements with the BNPL provider that allowed for merchant-specific application acceptance thresholds that we do not always observe, there could potentially be hundreds of merchant-specific thresholds. We prioritized large quasi-experiments with sufficient sample sizes and restricted the selection to the time period of August 2019 through December 2022 in order to prevent the attribution of quasi-experiments to firms in years in which only one firm was operating.

After identifying potential discontinuities, we conducted sharp, first-stage regression discontinuity analyses to assess how the probability of BNPL origination differed just above relative to just below each score-based threshold. We retained only quasi-experiments with a strong first stage, defined as having a p-value smaller than 0.05 and a jump in the probability of BNPL origination of at least 10 percent. Smaller quasi-experiments exist for particular merchants but we opt to focus on general score thresholds that exist outside of merchant-specific exceptions. Figure A.1 compares the first-stage RD coefficients before and after the match to the credit bureau data and demonstrates that coefficients remain virtually unchanged after the match.

3.3 Measuring financial distress

We define several measures of financial distress. Credit card utilization rates are calculated as the sum of balances across all credit cards where the individual is a primary user divided by the sum of all credit card limits. Credit card utilization rates are around 30 percent in the general population, while those above 90 percent are considered binding (Athreya, Mustre-del Río, and Sánchez, 2019). In the experimental sample, a large share of consumers exhibit utilization rates above 90 percent or even exceeding their credit limits[12]. Table 2 shows that, among consumers in the experimental sample who have any open balances on credit cards, the average utilization rate is 91 percent, indicating highly binding credit card constraints.

Second, we follow Gibbs et al. (2024) in defining the number of accounts that are 30+ days past due as well as the past-due amount on all open accounts. We limit the types of accounts to those we consider similar to typical purchases consumers may make using BNPL: credit cards, retail loans and personal loans.

Third, we consider the share of a consumer's credit card accounts that are carrying a balance from month to month (i.e. "revolving") and thus–in most cases–charging interest, as well as the finance charges a consumer pays to revolve. Although we do not observe directly whether consumers

[12] We also note that consumers that lack access to credit cards entirely (i.e, have no credit cards), will have missing values for credit card utilization and therefore will not contribute to this measure. Thus, the mean credit card utilization rate likely *understates* constrained financial access for the consumers in our sample.

are revolving, we use the credit bureau data along with methods proposed in Guttman-Kenney and Shahidinejad (2023) to predict whether general purpose credit cards revolved debt in each sample month and, if so, how much they paid in total financing charges (i.e., interest and fees). While the largest credit card issuers do not report actual payment information to the National Credit Reporting Agencies (NCRAs) [13], Guttman-Kenney, Firth, and Gathergood (2023) point out that most set the minimum payment amount (Min_{fi}) for card i issued by furnisher f to be equal to a variant of the following:

$$\text{Min}_{fi} = \max\{\$\mu_f, \theta_f \widetilde{Bal}_{fi} + \text{F}_{fi}\}$$

where μ_f is a furnisher-specific payment floor amount (most commonly set to values such as \$20, \$25, \$30,...), θ_f is a furnisher-level percent of the new balance amount (typically between 1-3%), F_{fi} is total financing charges for the cycle (interest and all fees), and $\widetilde{Bal}_{fi} \equiv Bal_{fi} - \text{F}_{fi}$ is the end-of-cycle balance net of interest charges and fees. Though not specified in the equation above, credit card tradelines with an end-of-cycle balance amount less than the furnisher-level payment floor (μ_f) have their minimum payment set equal to the end-of-cycle balance amount.

We use the tradeline-level credit bureau data to assign the two furnisher-level parameters: μ_f is set to the minimum payment amount with the greatest tradeline-level density for each furnisher and θ_f is set to the minimum percent of end-of-cycle balance with a mass of non-revolving tradeline payment amounts (above μ_f). With these parameters and tradeline-level information on end-of-cycle balance amounts, minimum payments, and statement dates, we can classify tradelines as having revolved debt or transacted–paid the end-of-cycle balance in full–and calculate the amount paid in total finance charges for each tradeline-month in the data with a minimum payment amount that exceeds μ_f [14]. Specifically, we calculate the predicted minimum payment using the furnisher-

[13] Observing actual payment information for credit cards would allow us to classify tradelines as revolving if the payment was less than the previous statement balance amount or if the payment was not made within the non-interest accruing "grace period" for the credit card cycle.

[14] For tradelines with a minimum payment amount below μ_f we can estimate an upper bound on possible financing charges but we cannot classify the tradelines or directly estimate financing charges unless the end-of-cycle balance

level θ_f parameter and the reported end-of-cycle balance (which will be inclusive of potential financing charges) as $\widehat{\text{Min}} = \theta_f Bal_{fi}$. If the observed minimum payment Min $> \widehat{\text{Min}}$, it implies the tradeline revolved debt and financing charges can be calculated as:

$$\text{F}_{fi} = \frac{\text{Min} - \theta_f Bal_{fi}}{(1 - \theta_f)}$$

As mentioned above, we can only classify and estimate financing charges for tradelines where the minimum payment amount exceeded μ_f. At the most common μ_f and θ_f in the data ($25 and 1%), this requires an end-of-cycle balance amount greater than $2,500. The accounts we are not able to classify will be, by construction, low balance accounts that represent a relatively small share of total credit card balances.[15] Nevertheless, this is a limitation of the method we use.

4 Research Design

Identification of causal effects is based on a fuzzy regression discontinuity design (Lee and Lemieux, 2010) in which we exploit the discontinuity in BNPL loan acceptance at different score thresholds. The running variable is the consumer's credit score as used by the BNPL firm to accept or decline applications for credit.[16] It does not perfectly predict origination for two reasons. First, the applicant may be approved for a loan but decide not to finalize the purchase. Second, merchant exceptions may lead to approvals at lower consumer credit scores or rejections at scores above the general cut-off for the BNPL provider. We do not observe all of the merchant exceptions, but we perfectly observe both the score and the application outcome. Importantly, while we expect there to be two-sided non-compliance in the BNPL context (i.e., some consumers will take-out a BNPL

amount was $0, in which case the tradeline was trivially transacting and had finance charges of $0.

[15] In the CCIP tradelines matched to the BNPL sample, we are able to classify 83.2% of credit card balances.

[16] For some quasi-experiments, this score takes on a finite, albeit large, number of values and is therefore not formally continuous, while for other quasi-experiments the score is not an integer and therefore could take on an infinite number of values. Across quasi-experiments, the score takes on more than 67,000 distinct values, all of which are close to the RDD cutoff. We therefore opt to treat the scores as continuous for our analyses.

loan after being rejected by the first BNPL firm and other consumers will not take-out a BNPL loan even after being approved by the first BNPL firm), we do not expect there to be defiers, or consumers who only take-out a loan if they are initially rejected by the first BNPL firm. With this *monotonicity* assumption, modified versions of the continuity of regression functions that allow for imperfect compliance, and sharp changes in the likelihood of taking out a BNPL loan at the RDD discontinuities, we can interpret the fuzzy RDD estimates as local average treatment effects (LATE) of taking out a BNPL loan for compliers: consumers that take out a BNPL loan if they are just above the discontinuity but not if they are just below the discontinuity.

Due to the fuzzy nature of our RDD, we follow Hahn, Todd, and Van der Klaauw (2001) to estimate the effect of first-time BNPL use for compliers on each outcome using two-stage least-squares:

$$\text{originated}_i = \alpha_1 + \tau_1 \cdot \mathbf{1}(score_i \geq c) + f^{+}(score_{i:+} - c) + f^{-}(score_{i:-} - c) + \varepsilon_{1i} \tag{1}$$

$$\text{Y}_i = \alpha_2 + \beta \cdot \widehat{\text{originated}}_i + g^{+}(score_{i:+} - c) + +g^{-}(score_{i:-} - c) + \varepsilon_{2i} \tag{2}$$

where scores are centered around zero and $f(\cdot)$ and $g(\cdot)$ denote local polynomial approximations to the conditional expectations for the outcomes given the score, fit separately on observations with scores above the threshold $(i : +)$ and with scores below the threshold $(i : -)$ and the sample is limited to observations "close" to the threshold. In practice, we use local quadratic polynomials to approximate $f(\cdot)$ and $g(\cdot)$ and a triangular kernel weighting function that assigns greater weight to observations closer to the threshold. We use the mean squared error optimal bandwidth h and base inference on robust, bias-corrected variance estimates (see Calonico, Cattaneo, and Titiunik (2014)).

In addition to estimating RDD treatment effects for each of the 20 quasi-experiments in our data, we also estimate specifications that aggregate the experiment-specific effects[17] to produce a

[17]We limit the empirical sample for these aggregate effects to quasi-experiments that show balance in non-BNPL borrowing prior to the BNPL application.

single weighted RDD treatment effect for the main outcomes. The increased statistical power provided by these weighted specifications enables us to explore whether the BNPL impacts differ between one and six quarters after the initial BNPL application. To aggregate the experiment-specific effects in each quarter, we use weights that capture the relative number of observations within the selected bandwidth for each quasi-experiment (Cattaneo et al., 2016). Importantly, these specifications still focus on the experiment-specific RDD estimates and avoid allowing for comparisons between observations just above the threshold in one quasi-experiment and observations just below the threshold in another quasi-experiment.

5 Results

In addition to the monotonicity assumption, which we argue is plausible given the setting, the validity of the fuzzy RDD requires that two assumptions hold. First, we need a discontinuous change in the probability of loan origination at the internal score threshold. Second, other variables that could potentially influence the outcomes should be smooth at this cut-off. In this section, we assess whether these assumptions are likely to hold and provide evidence for the impact of first-time BNPL use on total unsecured consumer debt and the ability to repay non-BNPL debt obligations.

5.1 First-Stage Effect on BNPL Loan Originations

Figure 5 presents sharp RDD estimates of the impact of the experiment-specific score on the probability of BNPL loan origination. We observe a statistically significant and economically meaningful increase in the likelihood that a BNPL loan is originated for consumers just above the approval thresholds relative to consumers just below the approval thresholds for all 20 quasi-experiments in the data. The effects on the probability of a BNPL origination range from just above 10 percent (for quasi-experiments 5, 12, and 15) up to more than 70 percent (for quasi-experiment 16). There is also substantial heterogeneity in the size of the confidence intervals across quasi-experiments, driven primarily by differences in sample size. In Table A.1, we additionally confirm that all quasi-experiments have first-stage F-statistics well above the rule-of-thumb value of 20 (Cattaneo, Idrobo, and Titiunik, 2024).

5.2 Smooth Demographics around the Cut-Offs

As a second validation of our research design, we test whether other characteristics that could potentially impact the outcomes are smooth around the score threshold (Hahn, Todd, and Van der Klaauw, 2001). Figure 6 provides intent-to-treat (ITT) RDD plots of regressions of the re-centered scores on the age of the applicant at the time of their loan application. For this test, we follow Cattaneo, Idrobo, and Titiunik (2024) and display ITT effects as a sharp RDD. Age is predetermined and arguably impacts spending patterns and overall consumer debt. Plots generally show smooth trends in age around the cut-offs. Quasi-experiments 11, 13, 18 and 19 show very small kinks at the threshold that are not statistically significant at any standard level.

5.3 Pre-Application Period ITT Placebo Tests

As an additional assessment of the RDD identifying assumptions and to determine which quasi-experiments to include in the weighted RDD specifications, we present sharp, ITT estimates of the impact of taking out a BNPL loan on the total amount of non-BNPL debt (in 2024 US$) and whether consumers have any non-BNPL debt between two quarters prior to the BNPL application and 6 quarters after the BNPL application as outcomes. We interpret the RDD estimates for the two pre-BNPL application quarters as falsification tests. The RDD empirical strategy assumes that potential outcomes are continuous at the BNPL approval thresholds; evidence of differences in non-BNPL borrowing prior to the loan application for a quasi-experiment, which by construction cannot have been affected by the future BNPL loan approval, would suggest that the RDD empirical strategy is not appropriate for that quasi-experiment. Appendix Figures A.2 and A.3 present these results. We find evidence of pre-application imbalance for quasi-experiments 3, 11, 13, 14, and 15 and therefore drop these quasi-experiments from the subsequent analysis. We show versions of our main results without dropping these 5 quasi-experiments, 3 of which were from the same BNPL firm and all 5 of which used internal credit scores, in the Appendix.[18]

5.4 Consumer Debt

Figure 7 presents the weighted RDD estimates on total BNPL debt by quarter.[19] Consistent with the first stage estimates in Figure 5 and the standard BNPL loan duration, consumers with scores just above the approval thresholds have \$177.22 more BNPL debt one quarter after the BNPL application. This effect quickly fades, however, and these consumers have no more BNPL debt than consumers just below the approval thresholds 2-5 quarters after their initial BNPL application. In fact, we find evidence that approved consumers have *less* BNPL debt than consumers with scores that were just below the approval thresholds by 6 quarters after their initial BNPL application. In part, the quarter 6 finding could be explained by heterogeneous effects by quasi-experiment together with differences in the composition of the sample with information 6 quarters post-purchase. As shown in Figure 3, quarter six excludes observations from half of the quasi-experiments because we only observe BNPL purchases through the end of 2022. Table A.3 further displays heterogeneous LATE effects by quasi-experiment. However, the results on BNPL debt remain virtually unchanged when holding the composition of the experimental sample constant by conditioning the sample on being observed in the first and last quarter of analysis, suggesting that changes in the sample composition are not solely responsible for the results.

Figure 8 shows the analogous results for total non-BNPL debt by quarter while Figure 9 does the same for whether consumers have any non-BNPL debt.[20] In contrast to the results shown in

[18] We additionally show versions of the main results that include indicators for the calendar month when the outcome is observed to the main weighted RDD specifications in the Appendix in Figures A.6, A.7, A.8, and A.9, versions of the main results that use alternate methods to select the RDD bandwidth in Figures A.10, A.11, A.12, and A.13, and versions of the results that retain the quasi-experiments that displayed imbalance in Figures A.14, A.15, and A.16. The results are largely unchanged (and the main takeaways remain unaffected) when adding calendar month controls, using alternate bandwidth selection methods, or including all possible quasi-experiments, though the point estimates are sometimes less precisely estimated.

[19] Appendix Table A.7 presents the weights used to aggregate the quasi-experiment-specific RDD estimates in the first quarter after the BNPL application for the main outcomes.

[20] See Appendix Table A.2 for pre-treatment means of non-BNPL debt components.

Figure 7, there is no evidence that consumers that take out BNPL loans have different amounts of non-BNPL debt or that they are any more or less likely to have non-BNPL debt in the first six post-application quarters. In all six quarters, the 95% confidence intervals for both outcomes are wide and include $0. Taken at face value, the point estimates for the total debt amount figure–which are negatively signed in all six quarters–would imply reductions in non-BNPL debt of between $79 (in the sixth quarter after BNPL origination) and $955 (in the second quarter after origination) for consumers just above the approval threshold who were induced to take out a BNPL loan by having their BNPL application approved.

Appendix Figures A.4 and A.5 show results for total unsecured debt (i.e., the sum of BNPL and non-BNPL debt). Consistent with the estimates shown in Figure 8, we find no evidence that the BNPL loan leads consumers to take on significantly more total debt, on average. In terms of the likelihood of having any unsecured debt, the results in Figure A.5 are clearly driven by receipt of the BNPL loan; the point estimates follow a qualitatively similar pattern across quarters to that observed for the BNPL debt amount in Figure 7.

Taken together, the RDD estimates for non-BNPL debt and total unsecured debt do not suggest that BNPL borrowers end up with greater amounts of consumer debt than non-BNPL borrowers. This is consistent with the idea that the extension of BNPL credit does not significantly increase debt-financed consumption for affected consumers. While we always fail to reject the null hypothesis of no reduction in non-BNPL borrowing, we find point estimates that are universally negative suggesting that consumers may be substituting away from non-BNPL debt when they take out BNPL loans. However, a key contextual detail for this result is that BNPL debt amounts tend to be much smaller than existing non-BNPL debt amounts for our sample. Whether this finding would hold if consumers are able to take on larger amounts of BNPL debt remains an open question.

5.5 Past due debt

While the results in the previous subsection suggest BNPL borrowing is not associated with statistically significantly different levels of non-BNPL or total unsecured debt, taking on BNPL debt could still impact consumer financial health if it changes how consumers pay back existing debt or how much they pay to access available sources of debt. To assess the former, we estimate the weighted RDD specifications with the amount of past due non-BNPL debt and an indicator for whether consumers have any past due non-BNPL debt as outcomes.

Figure 10 plots the RDD estimates and 95% confidence intervals for the total amount of past due non-BNPL debt by quarter. As with the total non-BNPL debt, there is no evidence that BNPL borrowing increases the amount of past due debt for consumers. Point estimates in all six quarters are negatively signed and economically meaningful in size, albeit never statistically distinguishable from zero at the 5% level. Relative to the mean amount of past due non-BNPL debt in the quarter prior to the BNPL applications ($1,073), the point estimates in the first (-$690.7; p-value 0.1) and second (-$806.9; p-value 0.08) post-application quarters would represent 64% and 75% reductions in non-BNPL past due debt, respectively.

Figure 11 presents the same estimates for the binary outcome measuring whether consumers have any past due non-BNPL debt. One quarter after the BNPL application, consumers induced to borrow by their BNPL approval are 11.6 percentage points less likely to have any past due non-BNPL debt (p-value 0.06) than they otherwise would have been. This difference fades with time, and the point estimates suggest there is less than a 2 percentage point difference in the likelihood of having any non-BNPL debt 5-6 quarters after application. 95% confidence intervals always include 0, but in the first post-application quarter we can rule out increases larger than a 0.3 percentage point increase in the likelihood of having any past due non-BNPL debt.

5.6 Credit card use and financing charges

As credit card debt is the most commonly used credit source for consumers in our data and we observe some outcomes only for general purpose credit card borrowing (e.g., estimates of the amount paid in financing charges and the share of accounts revolving some debt) in this subsection we focus on these credit card-specific outcomes and estimate how BNPL borrowing impacts credit card utilization rates, the share of credit card tradelines on which consumers revolve some debt, and the amount consumers pay for revolving credit card debt. If consumers view BNPL primarily as an alternative source of credit to help fund existing levels of consumption, we should expect increased BNPL borrowing to reduce borrowing from close substitute credit sources. Conversely, if consumers respond to BNPL credit access by increasing total consumption, we might observe no change in borrowing on closely-related credit products such as credit cards, or even increases in borrowing on such products in addition to increases in BNPL borrowing.

Figures 12, 13, and 14 present the weighted RDD impacts on the credit card utilization rate, the share of credit cards revolving debt, and the estimated amount consumers paid in credit card financing charges, respectively. The LATE estimates for consumers induced to take out a BNPL loan indicate there was no impact on credit card utilization rates, which are universally high for consumers in the empirical sample (0.89 in the quarter just prior to the BNPL loan applications). The confidence intervals permit us to rule out increases in credit card utilization greater than 0.1 in most quarters but also include large potential reductions in credit card utilization (up to -0.2 in the first post-BNPL application quarter).

Figure 13, which displays estimates for the share of credit card tradelines revolving debt, similarly fails to produce evidence that consumers adjusted their credit card borrowing behavior in response to receiving a BNPL loan. The point estimates are close to zero and confidence intervals always include 0. Confidence intervals in Figure 14 also always include 0, but the point estimates consistently point towards BNPL borrowers paying slightly more in financing charges on credit cards during the study period.

In part, the wide confidence intervals for the share of tradelines revolving debt and the total amount paid in finance charges on credit cards reflect that we are able to classify only a subset of the credit cards used by the sample. By construction the most common minimum payment amount in credit bureau data is the credit card issuer payment floor, and we cannot classify credit cards with a minimum payment at that level or below. This widens the RDD confidence intervals for both the share of credit cards that revolved debt and the total financing charges paid by consumers. Nevertheless, for both outcomes, the confidence intervals still rule out a broad set of negative potential outcomes for consumers; for total financing charges paid, in particular, we can rule out increases greater than 4% of the mean end-of-cycle credit card balance in the quarter just prior to BNPL application.

6 Conclusion

In this paper, we leverage a novel dataset of BNPL applications from the six largest BNPL firms operating in the United States between 2019-2022 to explore how BNPL borrowing affects measures of non-BNPL debt and broader consumer financial health. We use credit scores and information provided by the firms about their loan acceptance thresholds over time to identify 20 quasi-experiments. We then use an RDD empirical strategy for each quasi-experiment that designates applicants with scores just above the acceptance thresholds as the treatment group and applicants with scores just below the acceptance thresholds as the control group. We then use RDD methods to estimate the impact of first-time BNPL use on non-BNPL consumer debt and the ability of BNPL borrowers to repay their non-BNPL debt obligations.

While the treatment group exhibits an increase in BNPL borrowing for one quarter after the first application, this difference dissipates after the first quarter. We do not find evidence that first-time BNPL use increases total unsecured consumer debt in non-BNPL categories of borrowing in the 18 months after their first BNPL application. The results provide suggestive evidence of some substitution away from other loan products. Importantly, the results do not support the conclusion that access to BNPL credit leads to negative impacts on any measures of financial distress related to non-BNPL borrowing, such as past-due non-BNPL debt, credit card utilization, the share of credit cards revolving debt, or finance charges paid on credit cards.

Although our results suggest that BNPL borrowers do not see large increases in non-BNPL indebtedness or decreases in broader measures of consumer financial health, this could be partly driven by several features of today's market for BNPL borrowing. First, consumers typically take out a comparatively small amount of BNPL debt. In the first quarter after BNPL loan origination, on average, consumers in our data have 28 times as much non-BNPL debt as they do BNPL debt. Furthermore, many of the first-time applicants in our experimental sample for BNPL loans did not have *any* non-BNPL credit card, personal loan, retail or AFS debt prior to their BNPL loan application: roughly 1/3 of applicants had no non-BNPL debt in the quarter just prior to their application. This may be partially a reflection of the relatively young ages of BNPL borrowers when they apply for a BNPL loan for the first time rather than a permanent difference between BNPL borrowers and non-BNPL borrowers. As these youngest cohorts gain more experience with non-BNPL debt products, and as BNPL borrowing becomes offered by a larger number of merchants, these patterns may change. It is therefore critical that researchers and policymakers continue to regularly assess how BNPL borrowing is affecting broader measures of consumer indebtedness and financial well-being.

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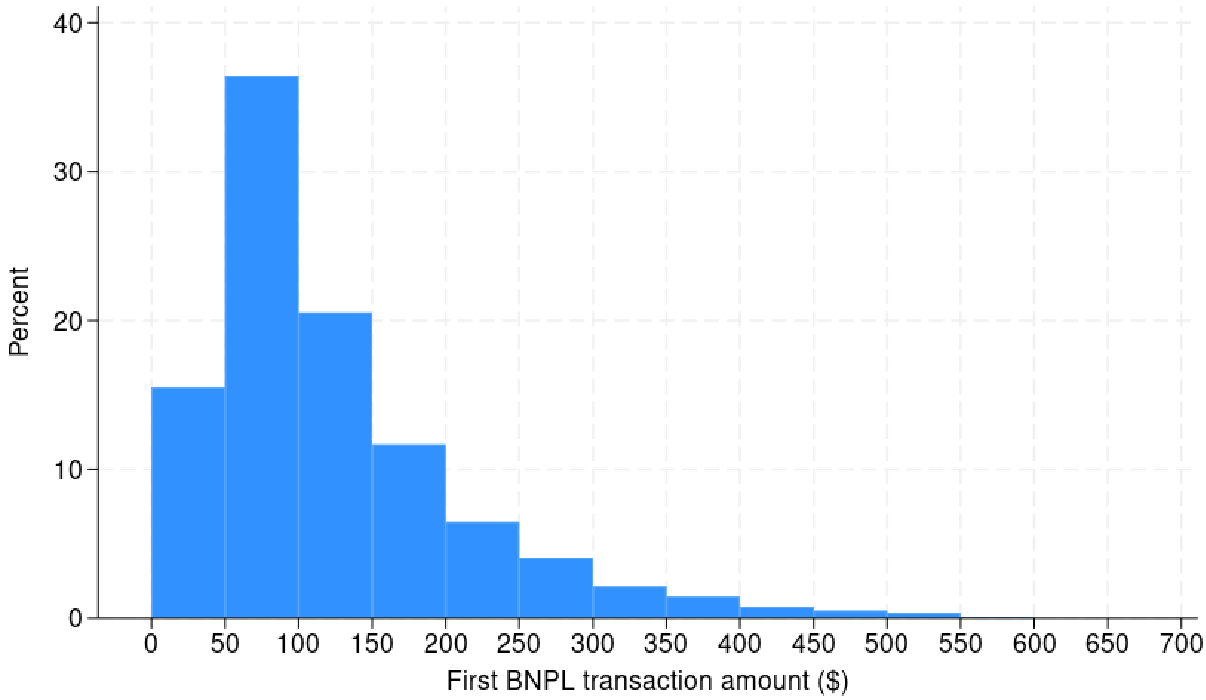
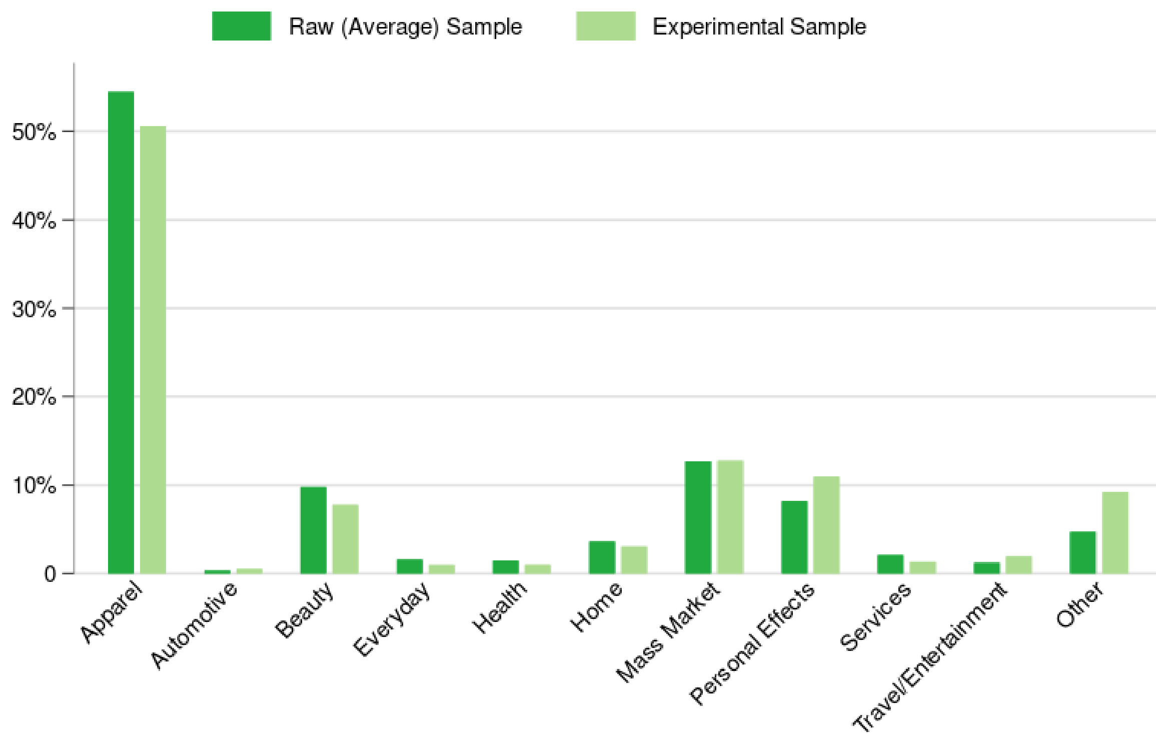
Main Tables and Figures

Figure 1: Gross merchandise value of first-time transactions



Notes: The figure includes all experimental originations. Amounts are CPI-adjusted to September 2024 dollars. In this graph, we omit outliers above the 99th percentile for exhibition purposes.

Figure 2: Share of originations by merchant vertical, August 2019-December 2022



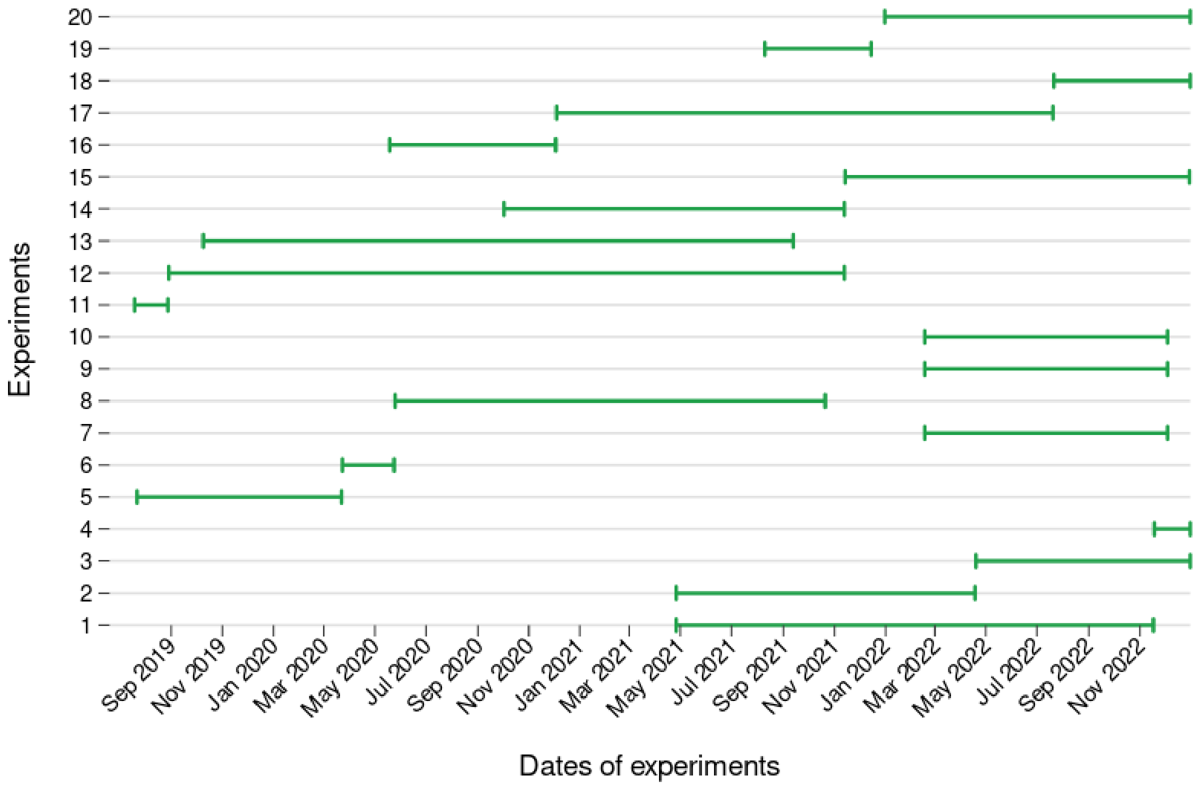
Notes: Quasi-experiments take place between August 2019-December 2022. The raw sample of originations (average sample) is therefore likewise limited to the same time period for comparison purposes. The raw sample contains approximately 78.7 million originations. The merchant vertical 'apparel' includes shoes as well as department stores specializing in fashion. The mass market vertical includes department stores offering a broad range of products, discount and wholesalers, general goods and general merchandise. Personal effects includes electronics, fitness and sporting equipment, games, hobbies and jewelry.

Table 1: Post-treatment means of auto-pay and default rates

	Share of borrowers originally on autopay (1)	Share of borrowers removed from autopay (2)	Share of BNPL trades defaulted (3)	Share of BNPL amount defaulted (4)
Share	1	.09	.1	.11
Observations	206,184	206,184	206,184	206,184

Notes: Shares for BNPL borrowers in the quasi-experimental sample. Borrowers could be in either the treatment or control group, as a rejected first-time applicant may take out a subsequent loan. The table displays averages across the four quarters following the consumer's first-time BNPL application. We first calculate the average for the consumer and then average across consumers in the sample.

Figure 3: Timing of quasi-experiments



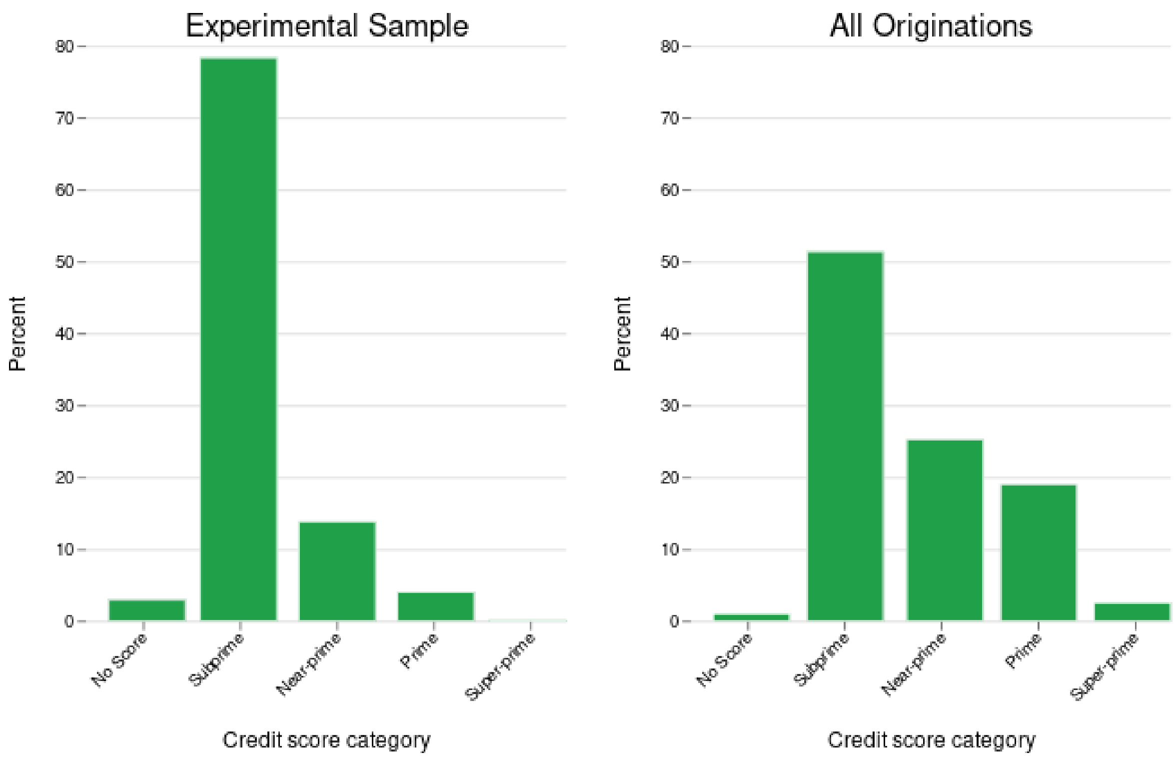
Notes: This figure depicts the first through the last day of each quasi-experiment.

Table 2: Pre-treatment means of credit card outcomes for experimental samples

	Credit card utilization (1)	Number of quarters revolving (2)	Share of accounts revolving (3)	Total finance charges ($) (4)
Avg. pos. amount	.91	2.57	.84	188.27
Share w/pos. amount	.61	.48	.9	.58
Observations	388,081	388,081	206,321	183,490

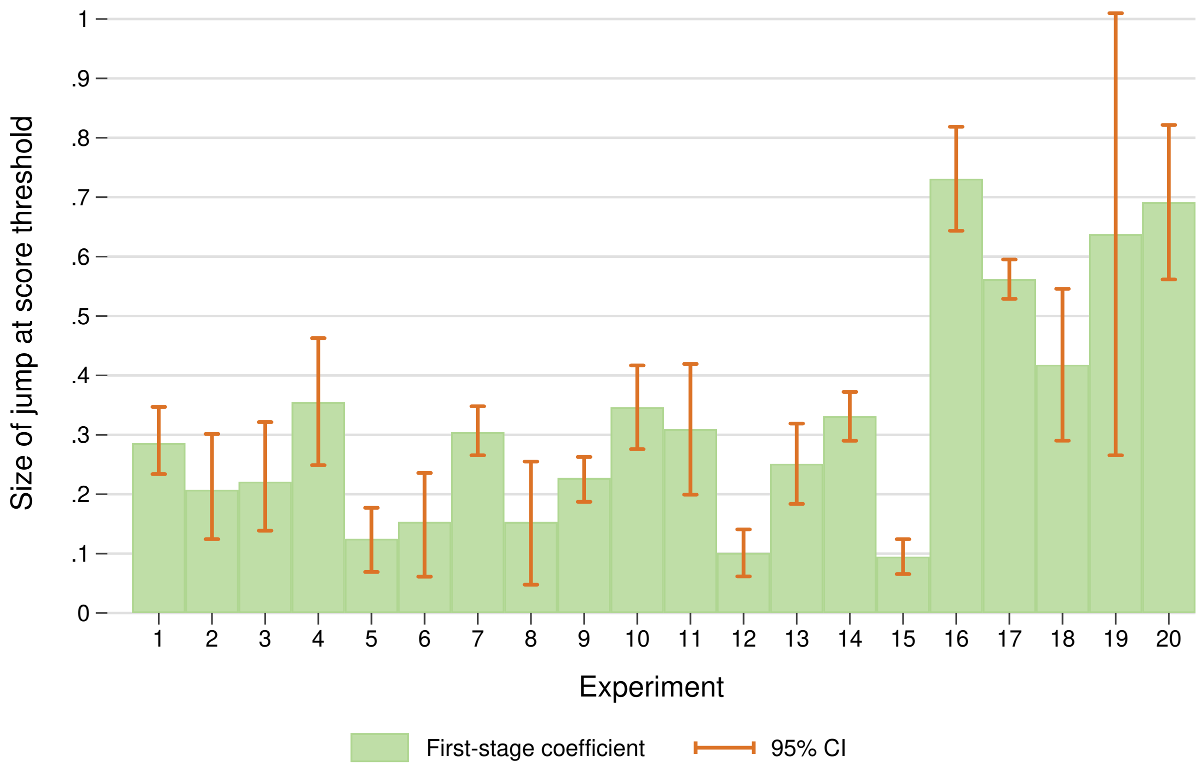
Notes: Averages for credit card utilization and the share of (credit card) accounts revolving are shown for all experimental observations across the four quarters preceding the consumer's first-time BNPL application. The number of quarters revolving (column 2) is the sum of quarters in which the consumer revolved on at least one credit card account. Total finance charges (column 4) include the sum of all late fees and interest paid in the four quarters preceding the application.

Figure 4: Percent of experimental sample in each credit score category, pre-treatment



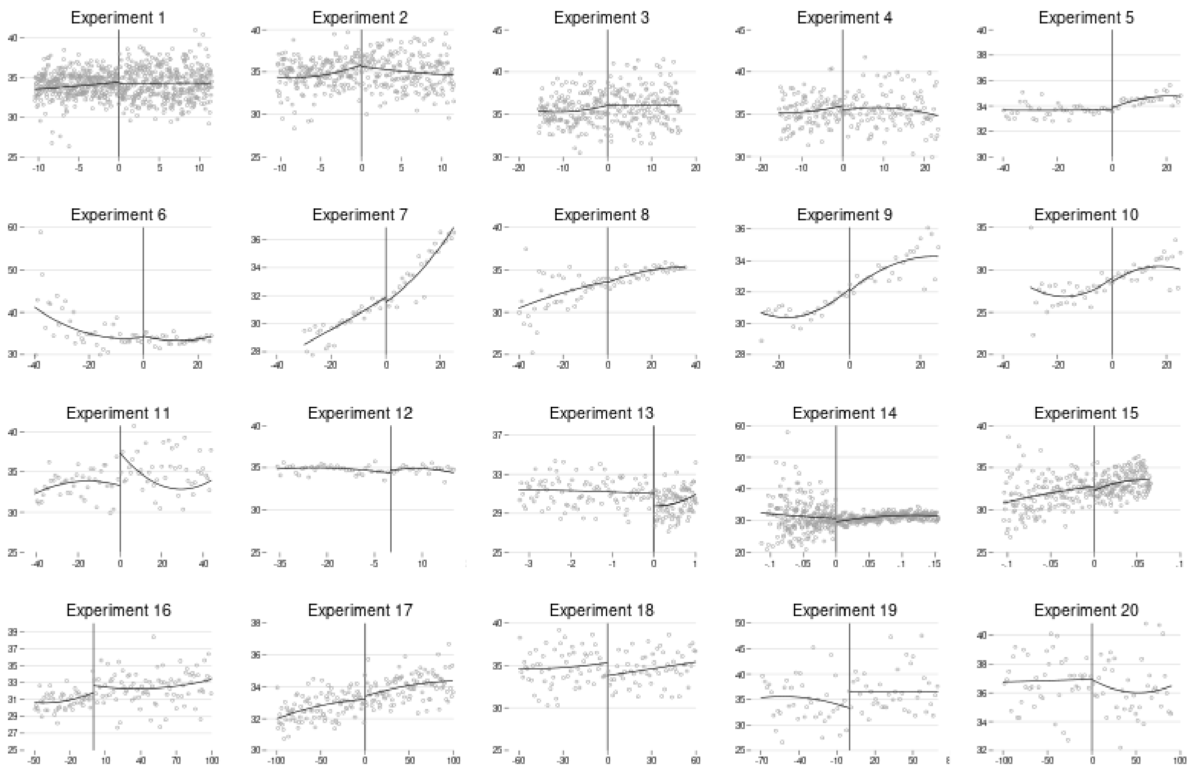
Notes: The left panel shows the score distribution based on a commercially available credit score for the combined sample of all quasi-experiments. The right sample shows the distribution for the average BNPL borrower with a credit record between September 2019 and December 2022. For more on the later dataset, see Shupe and DeLuca (2025). Over 3 percent of experimental sample observations do not have a value of the commercially available credit score.

Figure 5: Size of first-stage jump at the score threshold



Notes: First-stage coefficients from 20 separate RDD regressions of the re-centered score variable on the probability of BNPL loan origination for first-time applications. Coefficients are bias-corrected and whiskers show robust 95 percent confidence intervals.

Figure 6: ITT Effects of Score Cut-Off on Age at Application (Placebo)

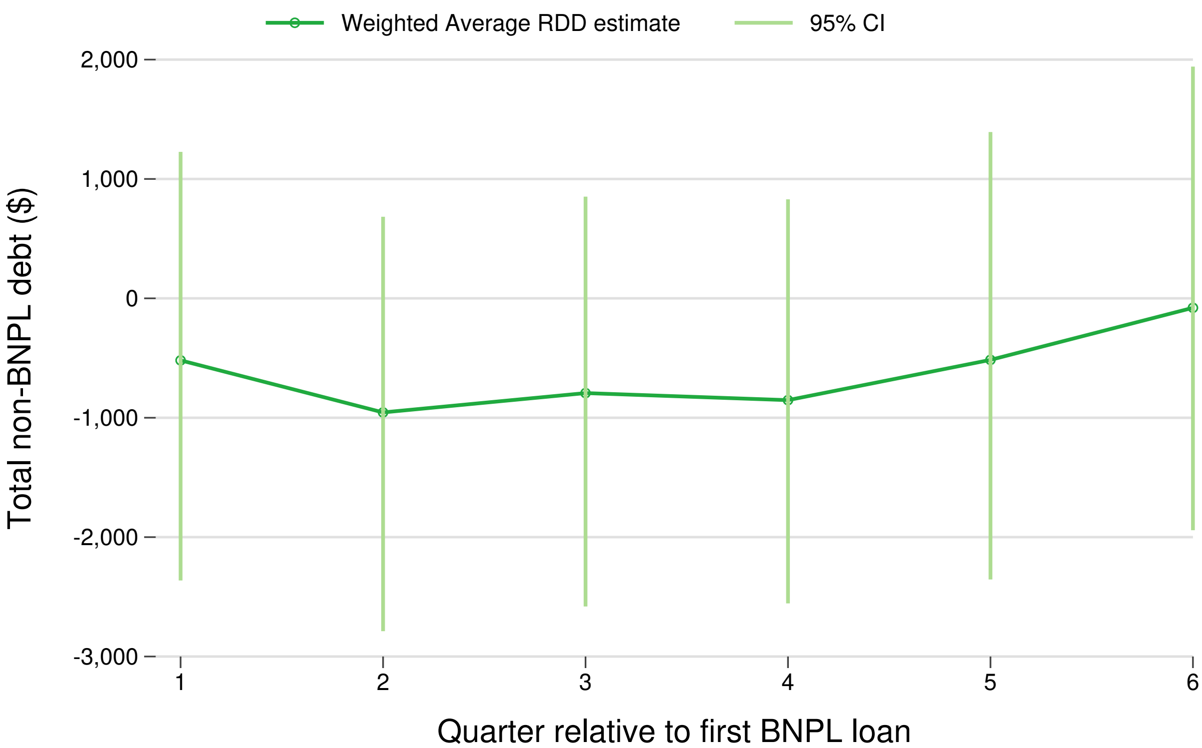


Notes: RDD plots for ITT estimates of re-centered scores on age at application. All estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific thresholds. None of the quasi-experiments have a statistically significant jump at the score cut-off.

Figure 7: BNPL debt by quarter

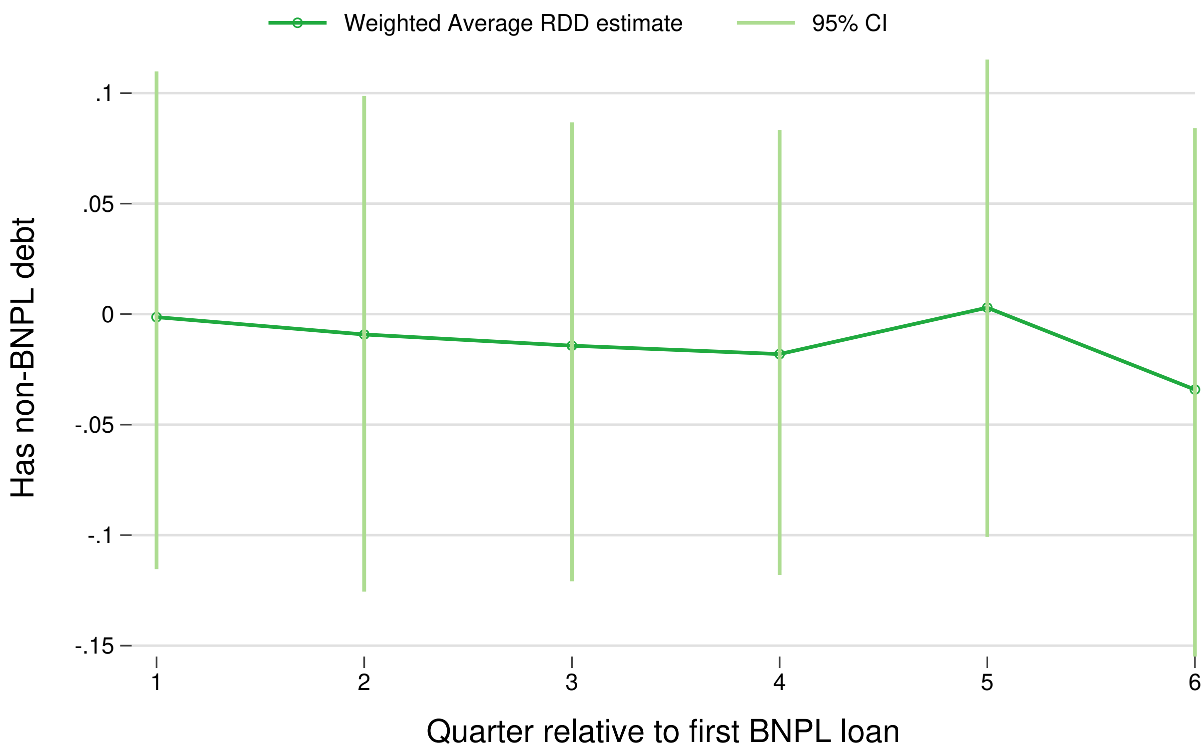
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on BNPL debt amounts in 2024 US$ by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure 8: Non-BNPL debt by quarter



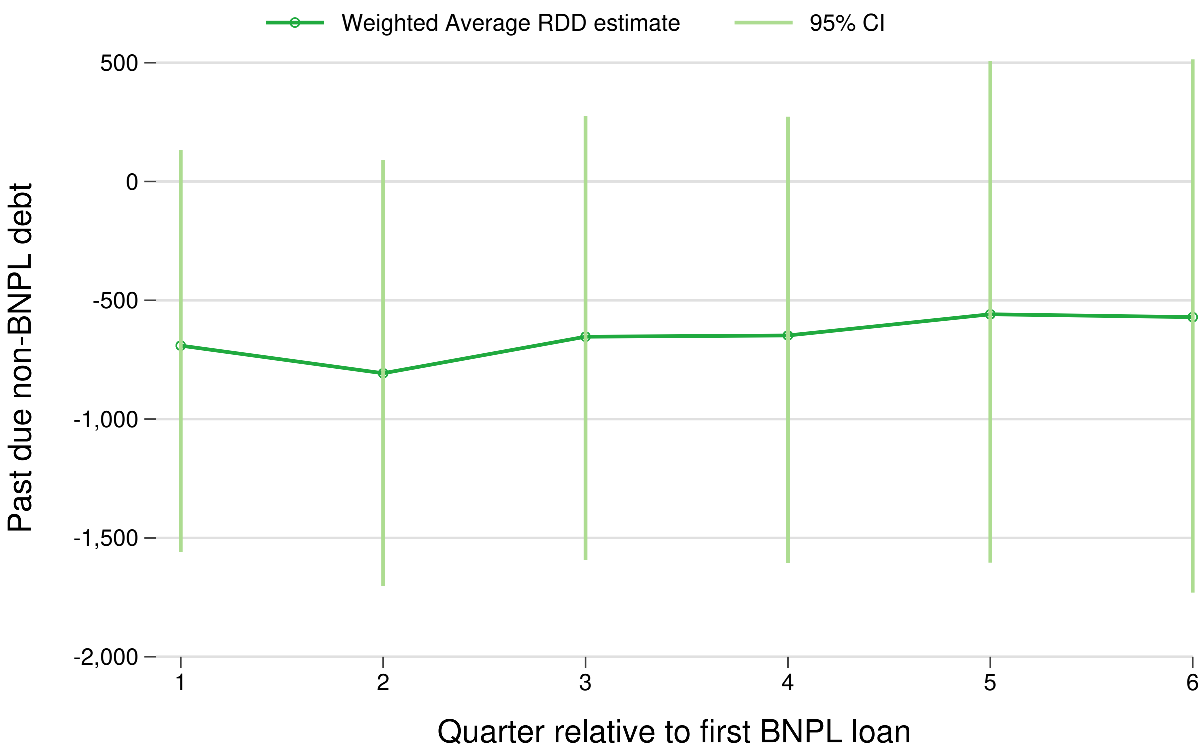
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on total non-BNPL debt amounts in 2024 US$ by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure 9: Any non-BNPL debt by quarter



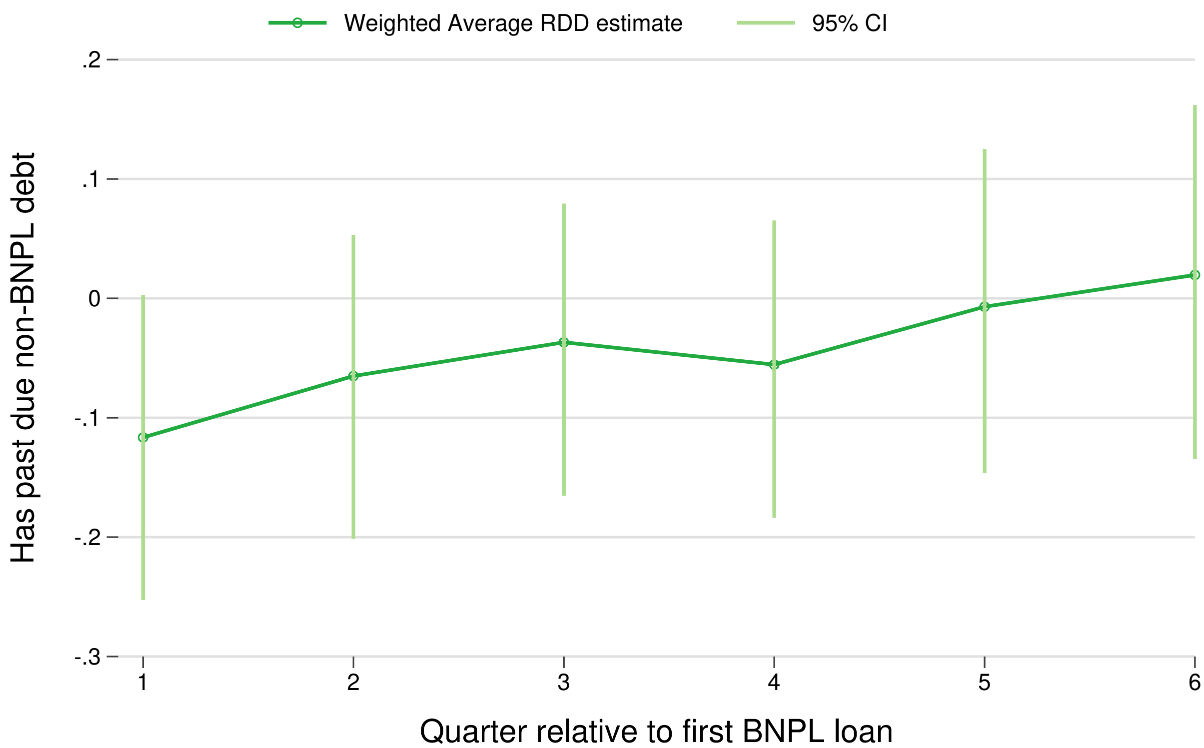
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on whether consumers have any non-BNPL debt by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure 10: Past due non-BNPL debt by quarter



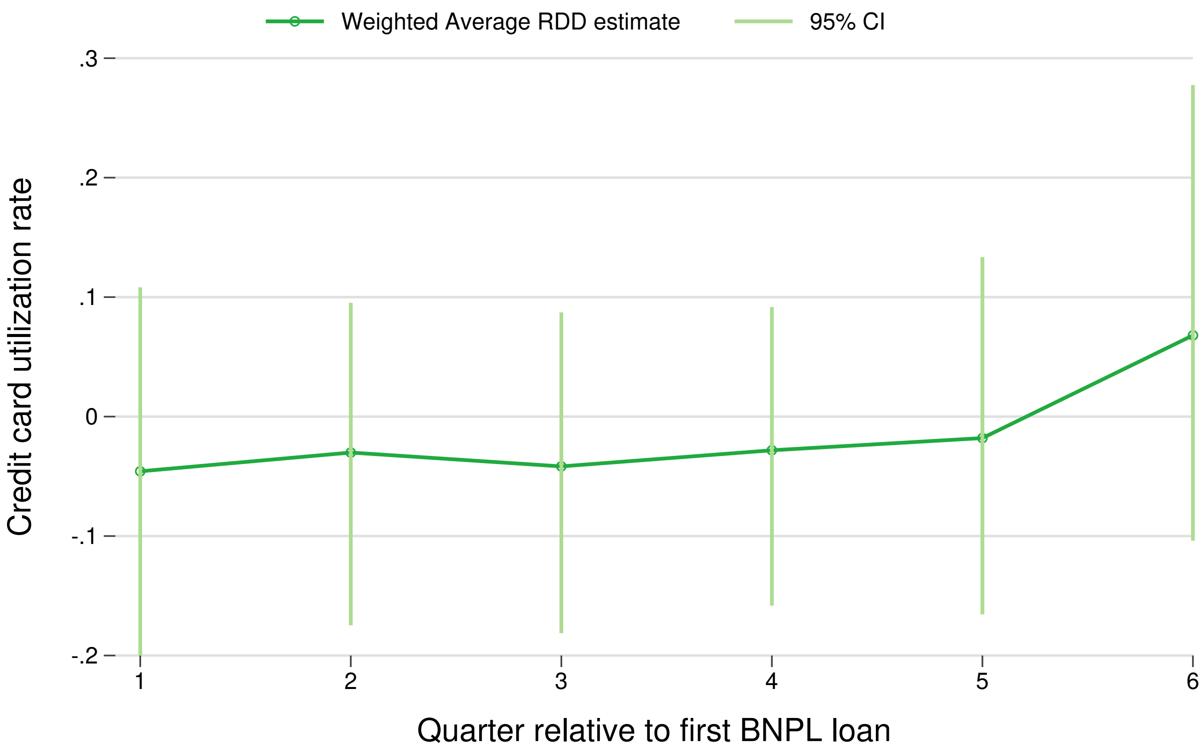
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on total past due non-BNPL debt amounts in 2024 US$ by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure 11: Any past due non-BNPL debt by quarter



Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on whether consumers have any past due non-BNPL debt by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure 12: Credit card utilization rate by quarter

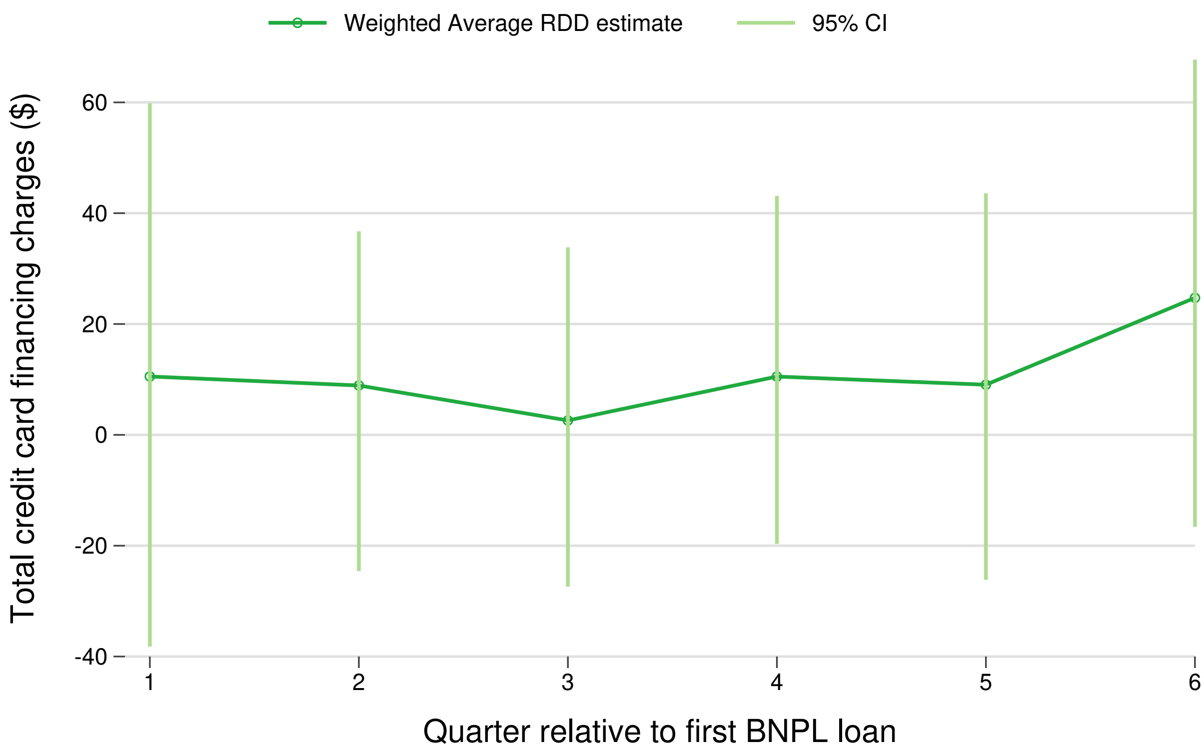


Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the credit card utilization rate (total credit card balance scaled by total credit card limit) across all credit card tradelines by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure 13: Share of credit cards revolving debt by quarter

Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the share of credit card tradelines on which consumers are estimated to be revolving some debt by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

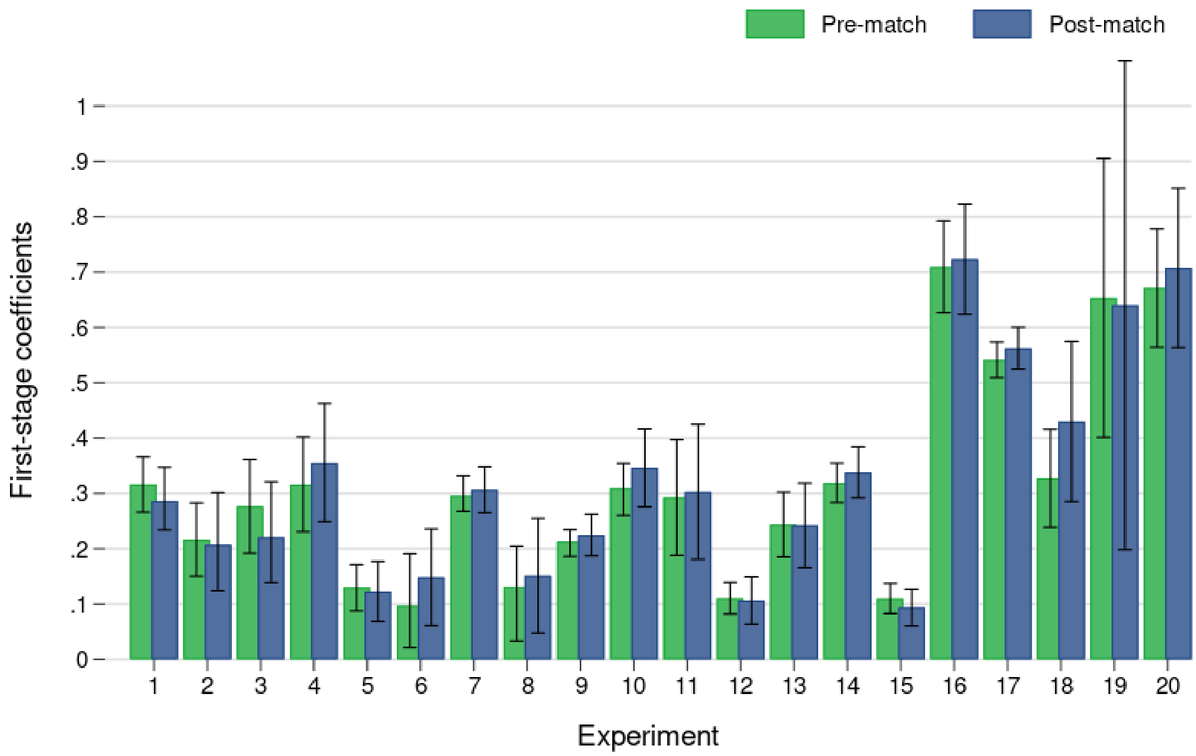
Figure 14: Total credit card finance charges paid by quarter



Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount consumers paid in credit card finance charges (in 2024 US$) by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

A Appendix A: Additional Tables and Figures

Figure A.1: First-stage coefficients before and after matching



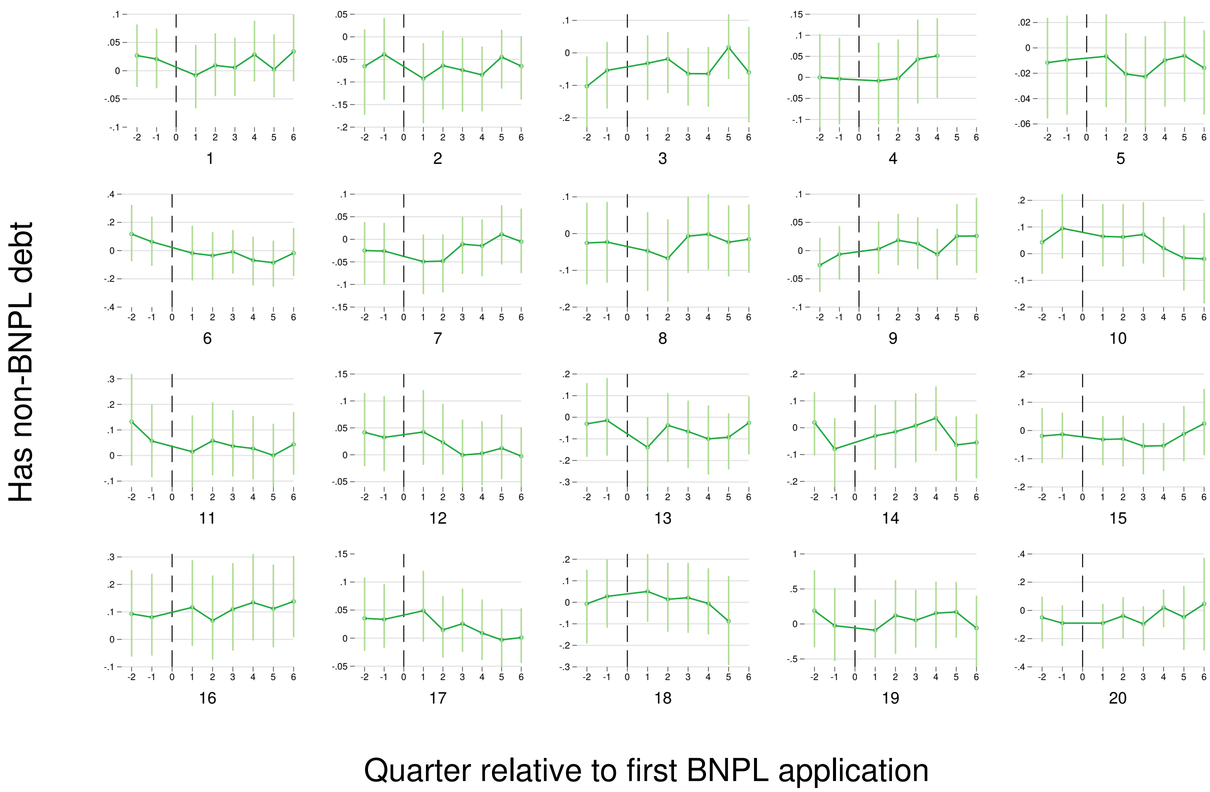
Notes: First-stage coefficients resulting from an RDD regression of the re-centered, quasi-experiment-specific scores on the probability of origination. Coefficients are bias-corrected with robust standard errors. Pre-match bars indicate first-stage RDD results from the raw data, prior to being matched to credit bureau outcomes. Post-match bars show changes to first stages when omitting observations that could not confidently be matched to credit bureau records.

Table A.1: First-Stage F-Statistics

Experiment 1	7,439
Experiment 2	2,133
Experiment 3	2,976
Experiment 4	6,768
Experiment 5	5,733
Experiment 6	2,927
Experiment 7	24,551
Experiment 8	3,597
Experiment 9	37,348
Experiment 10	11,242
Experiment 11	1,647
Experiment 12	1,106
Experiment 13	3,494
Experiment 14	34,726
Experiment 15	2,291
Experiment 16	21,564
Experiment 17	53,136
Experiment 18	1,674
Experiment 19	1,252
Experiment 20	3,778

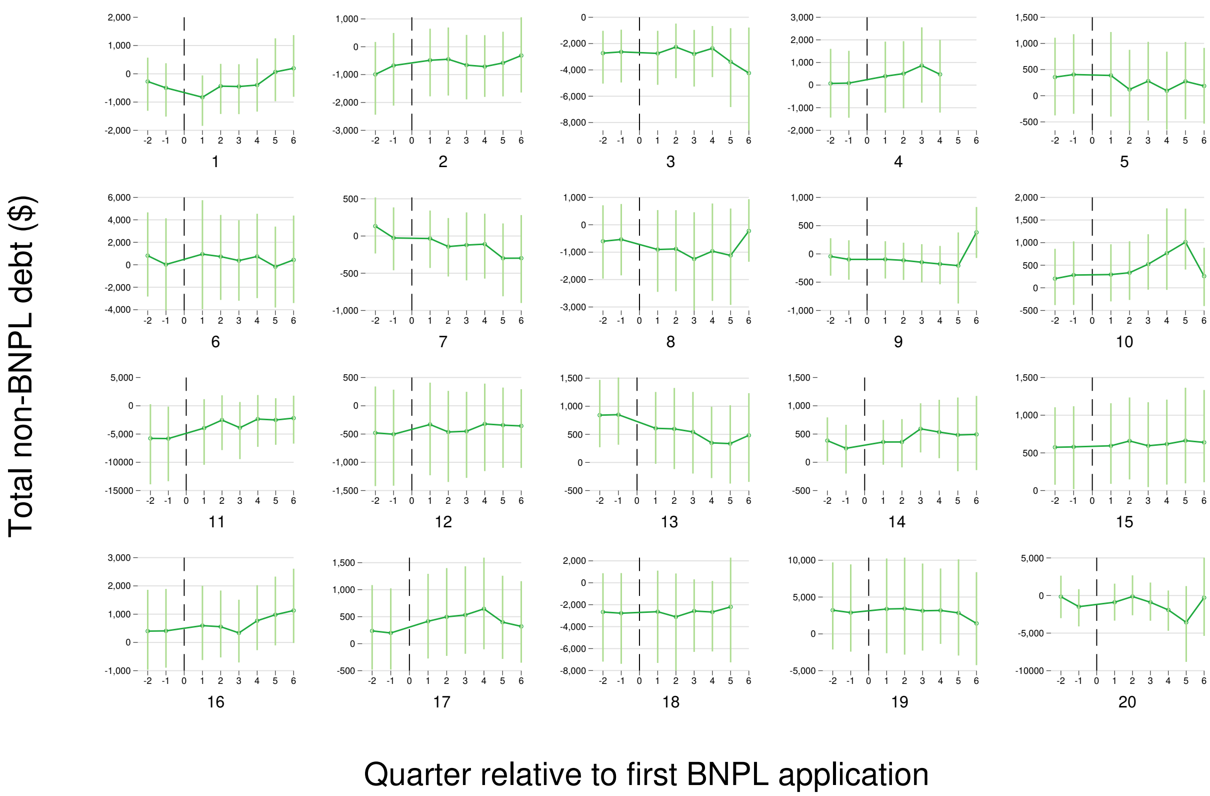
Notes: This table provides a parametric approximation of the fuzzy RD F-statistic in two stages for each quasi-experiment, estimated separately. First, we create a treatment indicator that is equal to one if the consumer has a score above the acceptance threshold. In a second stage, the treatment indicator serves as the instrument for whether the loan was originated. We estimate this second equation using two-stage least squares and display the resulting first-stage F-statistic above.

Figure A.2: Falsification test: Has any non-BNPL debt by quarter



Notes: Figure presents regression discontinuity estimates and 95% confidence intervals of the sharp RDD impact of being approved for a BNPL loan on the likelihood consumers have any non-BNPL debt by quarter for each of the 20 quasi-experiments in the data. All estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on the robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.3: Falsification test: Non-BNPL debt by quarter



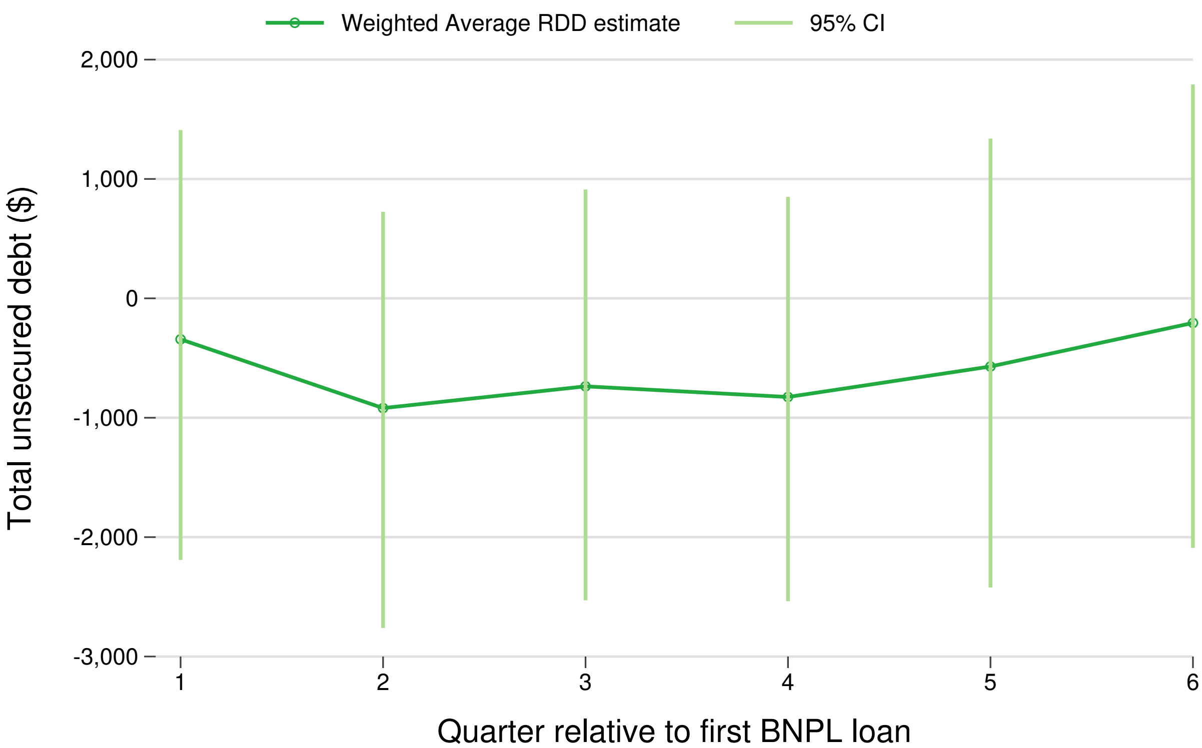
Notes: Figure presents regression discontinuity estimates and 95% confidence intervals of the sharp RDD impact of being approved for a BNPL loan on the amount of non-BNPL debt (in 2024 US$) by quarter for each of the 20 quasi-experiments in the data. All estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on the robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Table A.2: Pre-treatment means of credit balances for experimental samples

	Non-BNPL balances (1)	Credit card balances (2)	Retail loans (3)	personal loans (4)	AFS balances (5)
All balances					
Avg. pos. amount ($)	3,954	2,665	1,583	3,200	1,312
Share w/pos. amount	.64	.62	.28	.28	.03
Past-due balances					
Avg. pos. amount ($)	2,016	1,574	850	1,624	1,496
Share w/pos. amount	.41	.38	.16	.14	.09
Observations	388,081	388,081	388,081	388,081	388,081

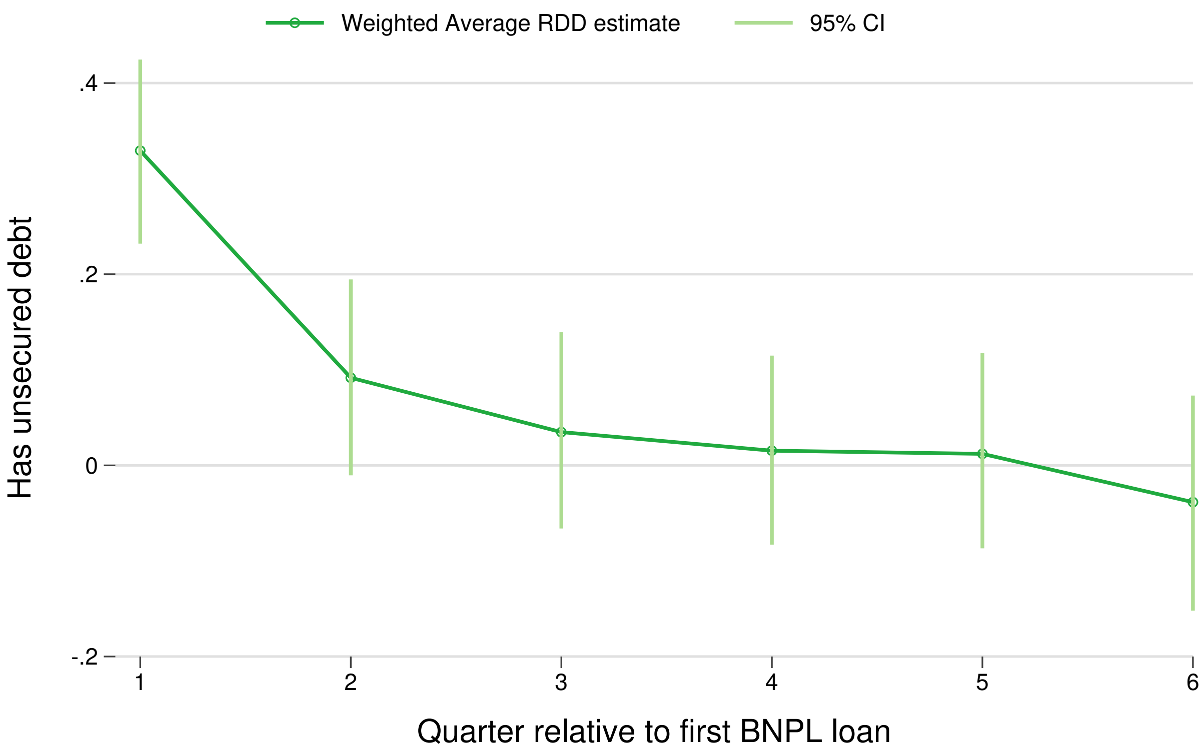
Notes: Balances for all quasi-experiments, CPI-adjusted to September 2024 dollars. The table displays averages across the four quarters before the consumer's first-time BNPL application. Balances include only open accounts. Past-due amounts for credit card balances, retail loans and personal loan amounts are those on open accounts. AFS = alternative financial services. Past-due amounts for AFS include open and closed accounts due to the information available in the data.

Figure A.4: Unsecured debt by quarter



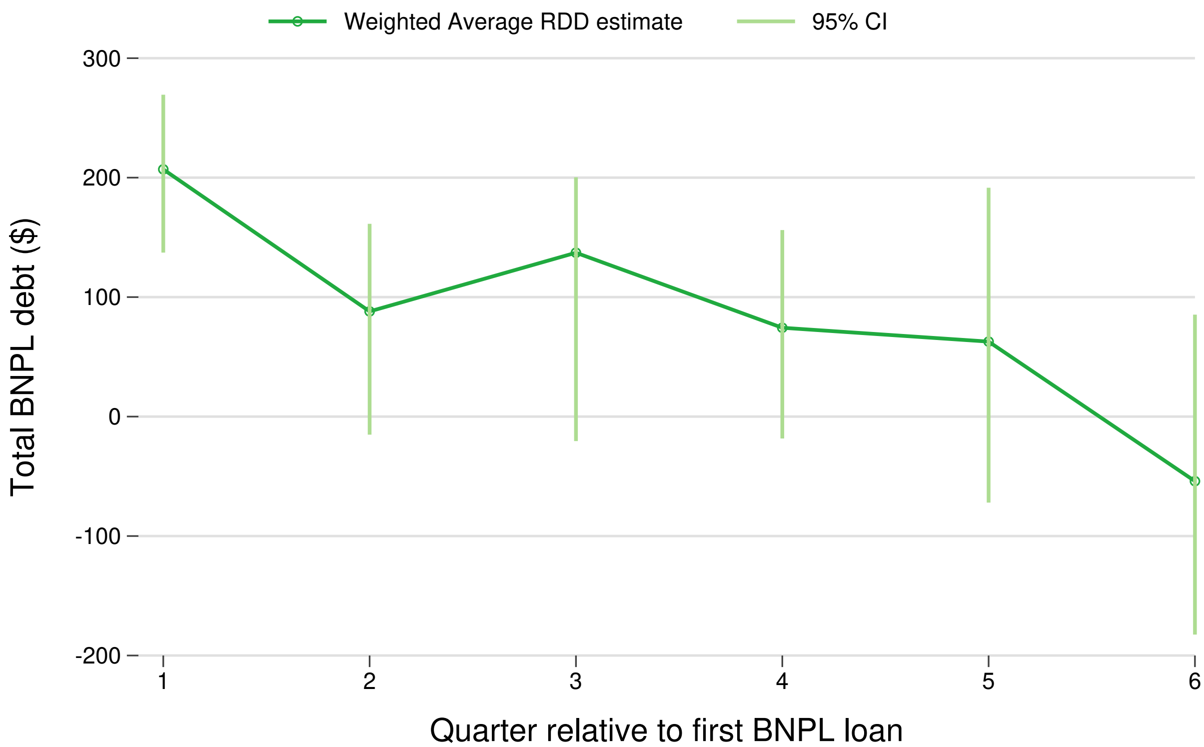
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on total unsecured debt amounts in 2024 US$ by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.5: Any unsecured debt by quarter



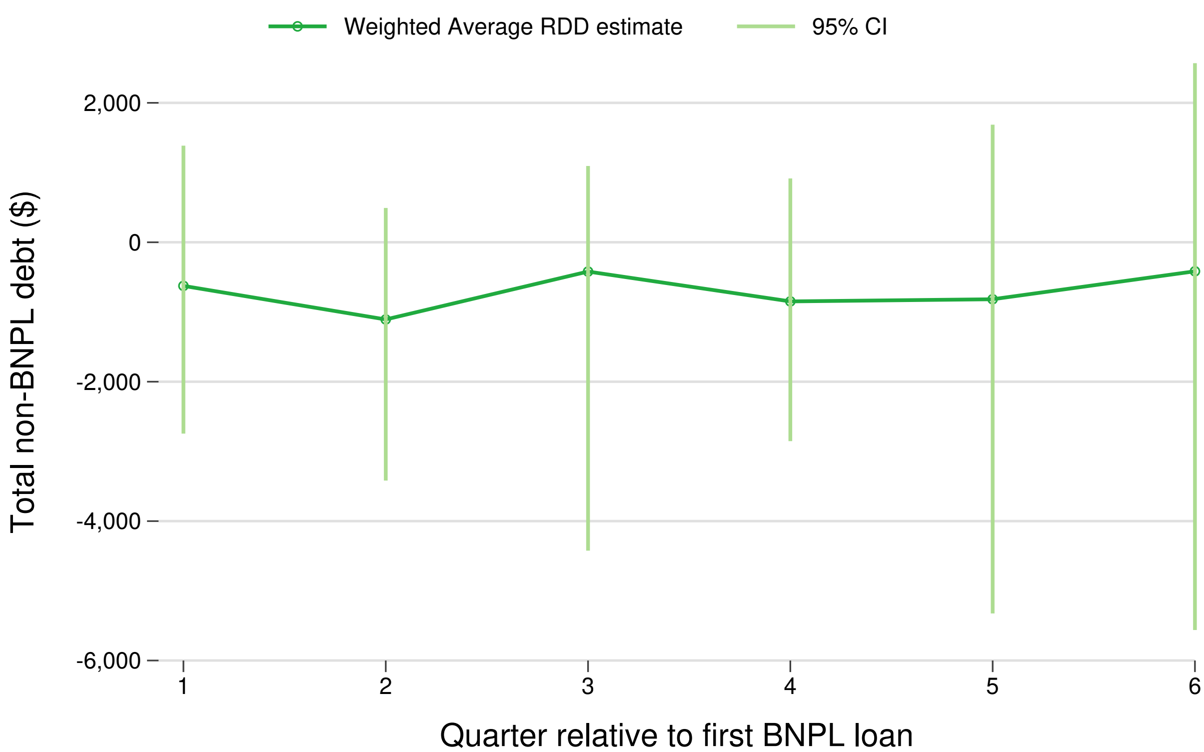
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on whether consumers have any unsecured debt by quarter. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.6: BNPL debt by quarter with controls for calendar month



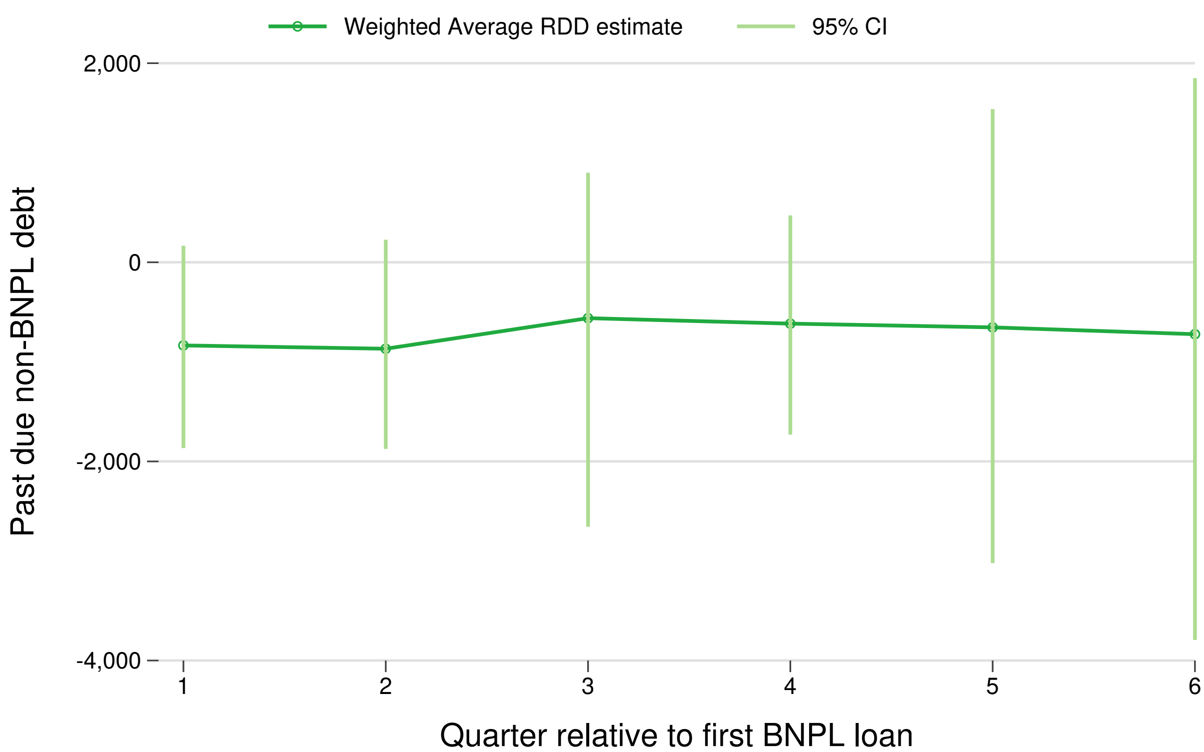
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of BNPL debt consumers have in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). Indicators for the calendar month when the outcome is observed are included as controls. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.7: Non-BNPL debt by quarter with controls for calendar month



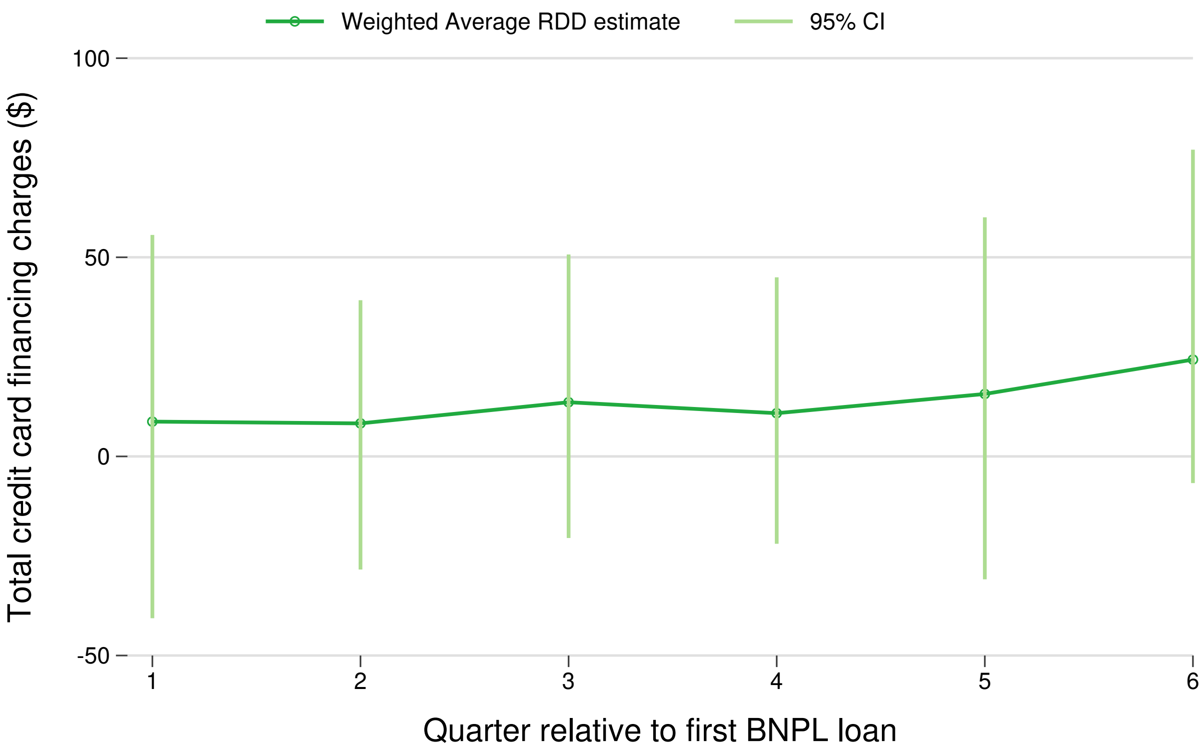
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of non-BNPL debt consumers have by quarter in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). Indicators for the calendar month when the outcome is observed are included as controls. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.8: Past due non-BNPL debt by quarter with controls for calendar month



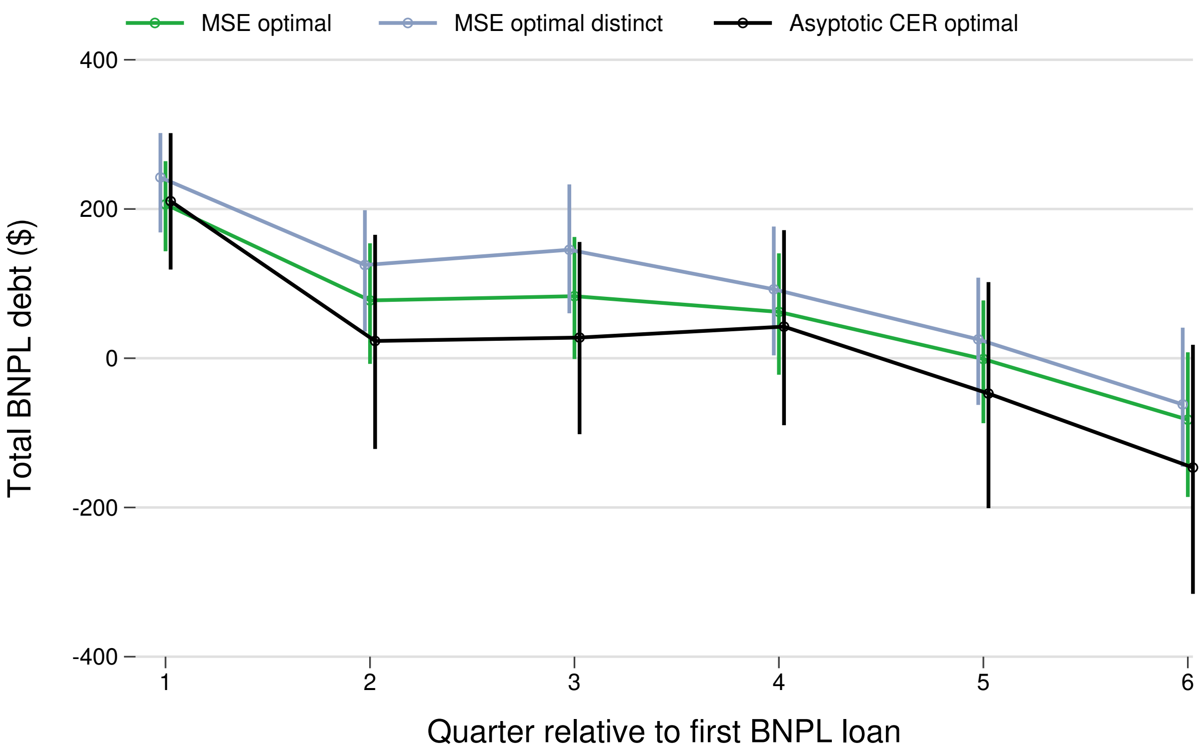
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of past due non-BNPL debt consumers have by quarter in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). Indicators for the calendar month when the outcome is observed are included as controls. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.9: Total credit card finance charges paid by quarter with controls for calendar month



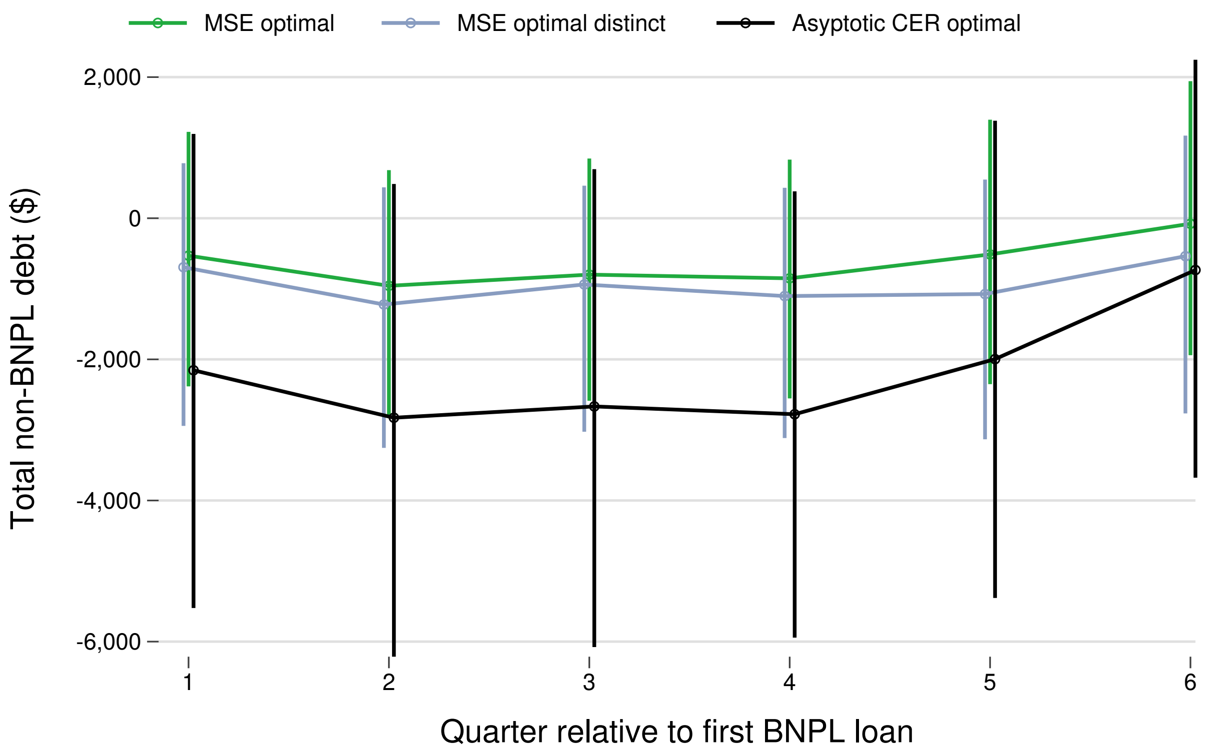
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount consumers paid in total financing charges by quarter in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). Indicators for the calendar month when the outcome is observed are included as controls. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.10: BNPL debt by quarter with alternate bandwidths



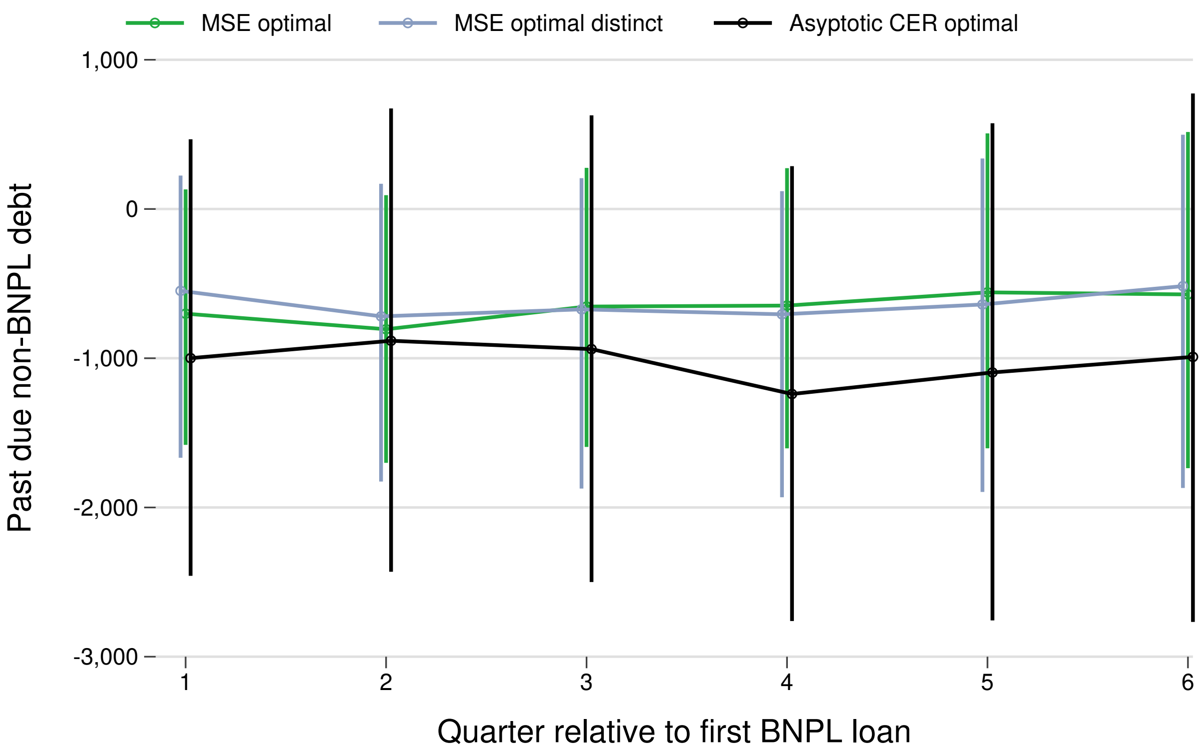
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of BNPL debt consumers have in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The figure plots point estimates and 95% confidence intervals separately under three different methods for selecting the RDD bandwidth: the MSE-optimal method used to produce the main figures, an MSE-optimal method that selects a different bandwidth above and below the cutoff, and a third approach that selects a bandwidth to minimize the estimated coverage error of the confidence interval. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.11: Non-BNPL debt by quarter with alternate bandwidths



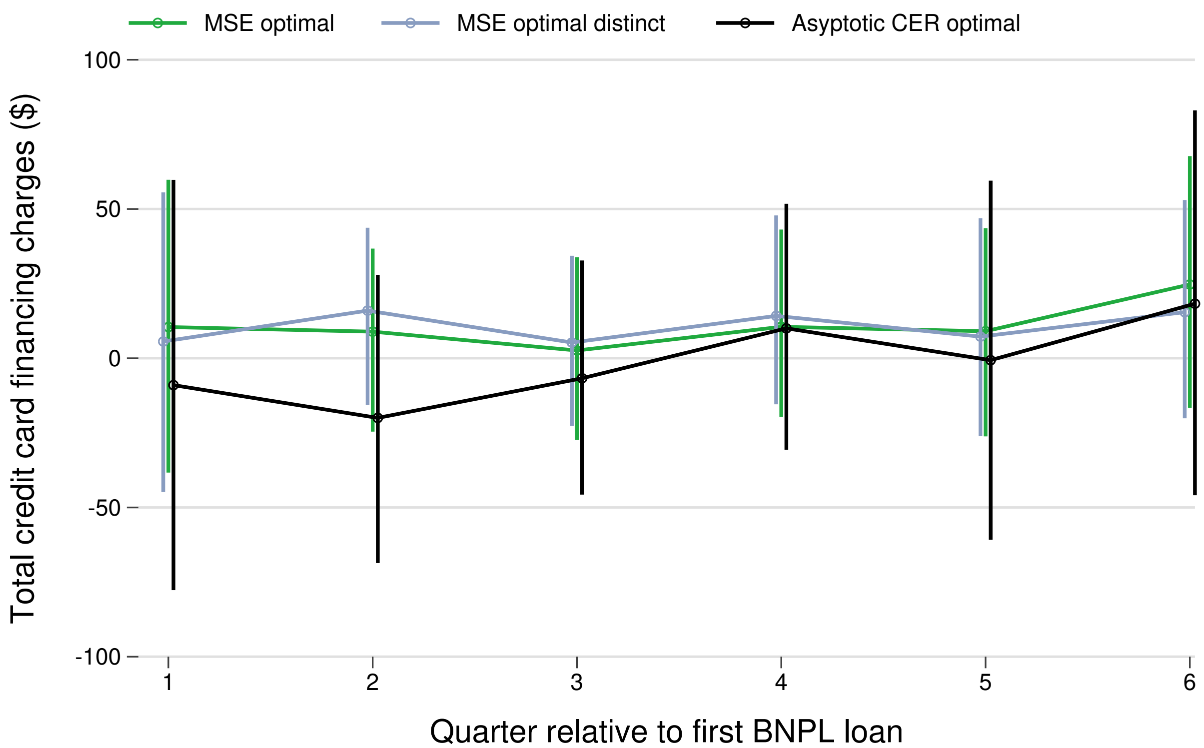
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of non-BNPL debt consumers have by quarter in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The figure plots point estimates and 95% confidence intervals separately under three different methods for selecting the RDD bandwidth: the MSE-optimal method used to produce the main figures, an MSE-optimal method that selects a different bandwidth above and below the cutoff, and a third approach that selects a bandwidth to minimize the estimated coverage error of the confidence interval. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.12: Past due non-BNPL debt by quarter with alternate bandwidths



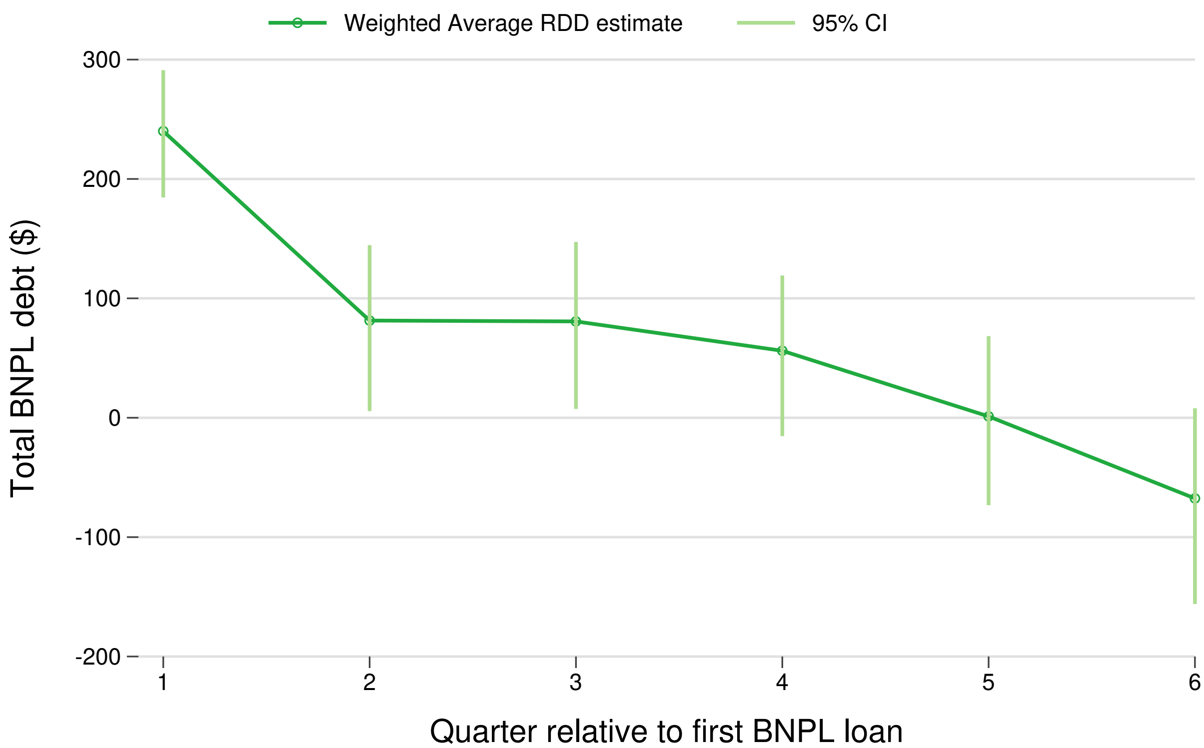
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of past due non-BNPL debt consumers have by quarter in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014).The figure plots point estimates and 95% confidence intervals separately under three different methods for selecting the RDD bandwidth: the MSE-optimal method used to produce the main figures, an MSE-optimal method that selects a different bandwidth above and below the cutoff, and a third approach that selects a bandwidth to minimize the estimated coverage error of the confidence interval. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.13: Total credit card finance charges paid by quarter with alternate bandwidths



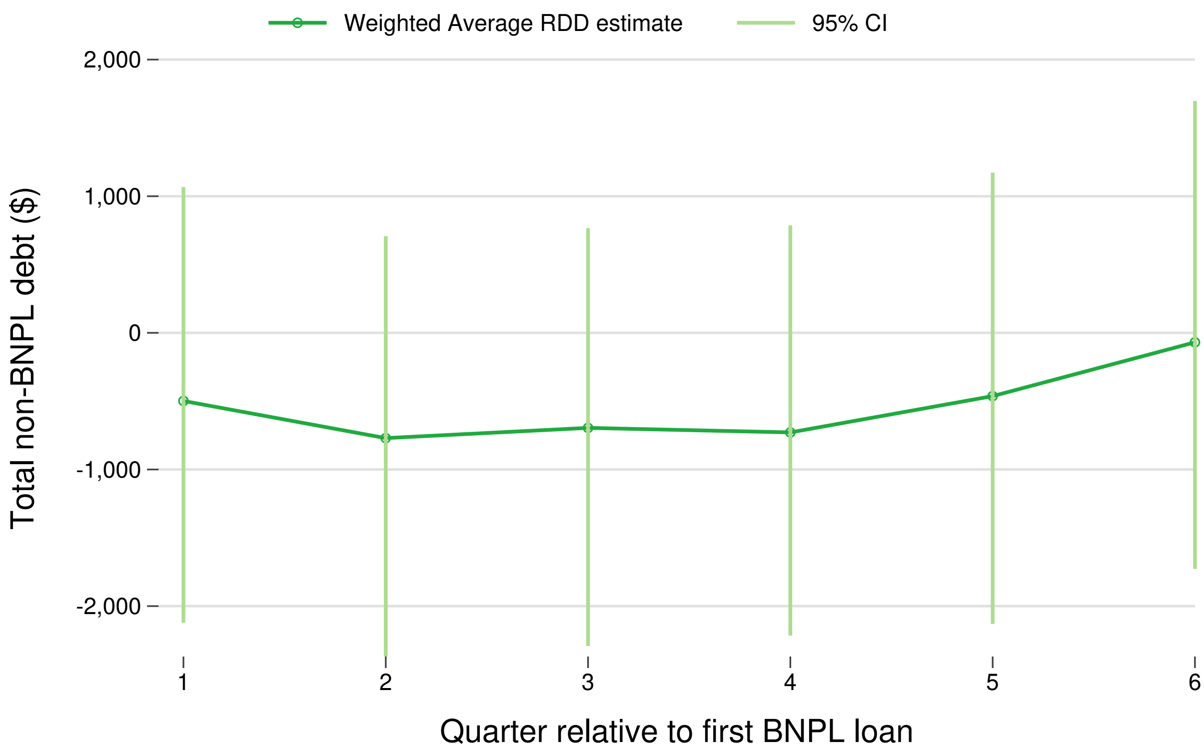
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount consumers paid in total financing charges by quarter in 2024 US$. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The figure plots point estimates and 95% confidence intervals separately under three different methods for selecting the RDD bandwidth: the MSE-optimal method used to produce the main figures, an MSE-optimal method that selects a different bandwidth above and below the cutoff, and a third approach that selects a bandwidth to minimize the estimated coverage error of the confidence interval. The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.14: BNPL debt by quarter with all quasi-experiments



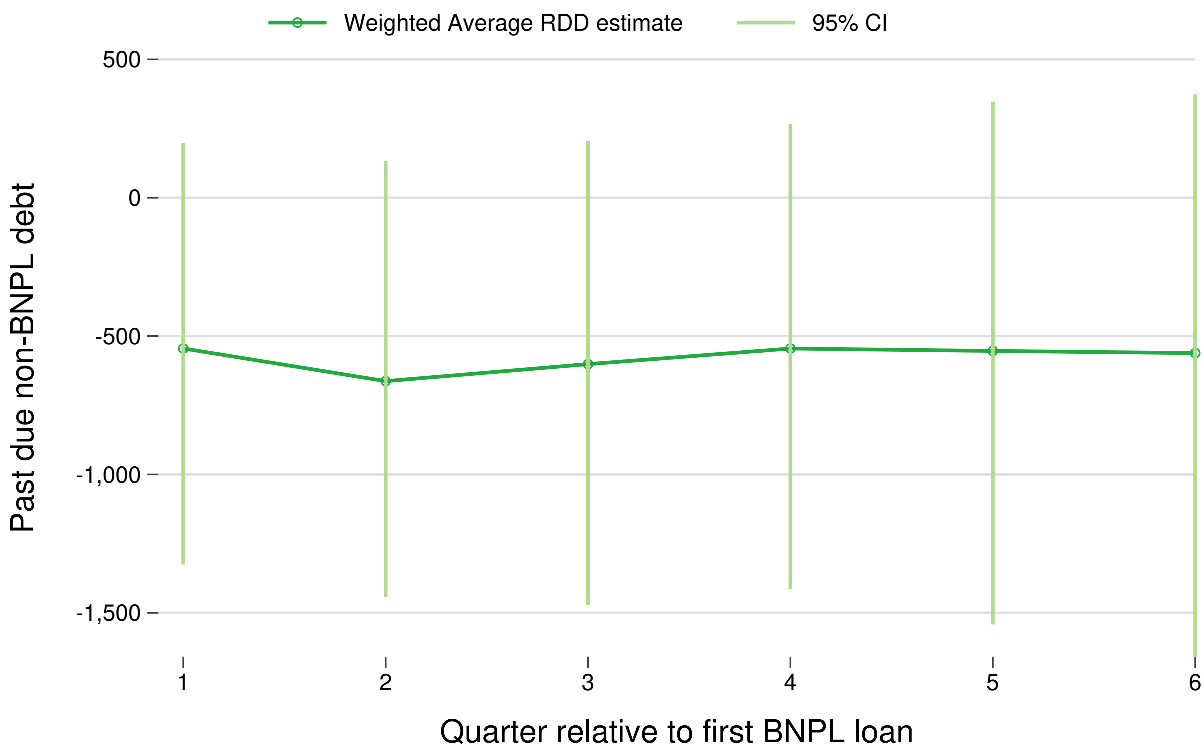
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of BNPL debt consumers have in 2024 US$. The sample includes consumers from all 20 quasi-experiments, regardless of whether they displayed imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.15: Non-BNPL debt by quarter with all quasi-experiments



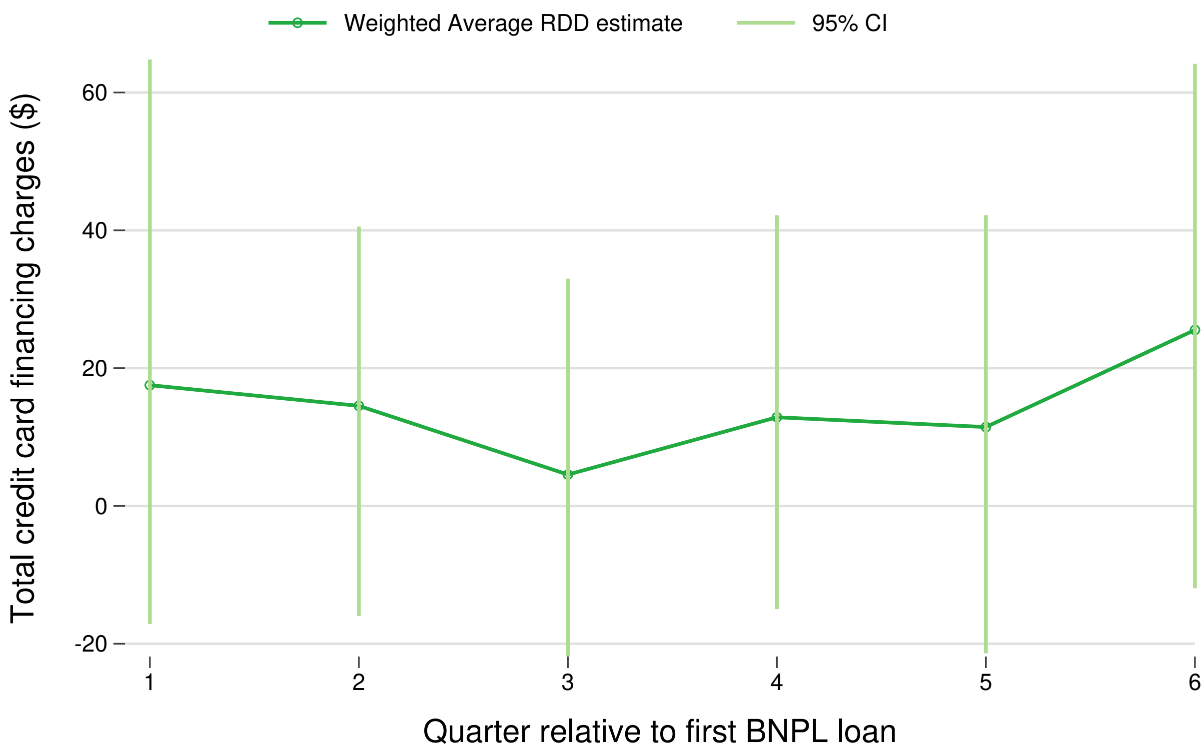
Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of non-BNPL debt consumers have by quarter in 2024 US$. The sample includes consumers from all 20 quasi-experiments, regardless of whether they displayed imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.16: Past due non-BNPL debt by quarter with all quasi-experiments



Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount of past due non-BNPL debt consumers have by quarter in 2024 US$. The sample includes consumers from all 20 quasi-experiments, regardless of whether they displayed imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Figure A.17: Total credit card finance charges paid by quarter with all quasi-experiments



Notes: Figure presents weighted regression discontinuity estimates and 95% confidence intervals for the local average treatment effect (LATE) for compliers of accepting the BNPL offer on the amount consumers paid in total financing charges by quarter in 2024 US$. The sample includes consumers from all 20 quasi-experiments, regardless of whether they displayed imbalance in pre-treatment consumer financial outcomes. All point estimates are based on quadratic local polynomial regressions fit separately above and below the quasi-experiment-specific cutoffs with mean squared error-optimal bandwidths, triangular kernel function-based weights, and 95% confidence intervals based on robust, bias-adjusted variance estimates from Calonico, Cattaneo, and Titiunik (2014). The weights capture the probability that an included observation faced the cutoff for that quasi-experiment.

Table A.3: LATE Effects for Non-BNPL Balances, by Experiment

	Non-BNPL balances (1)	Credit card balances (2)	Retail loans (3)	Personal loans (4)	AFS balances (5)	Obs. (6)
Exp. 1	-0.10	-0.11	-0.50	1.14	-0.57	43,630
	[-0.74,0.54]	[-0.78,0.56]	[-1.35,0.35]	[-0.41,2.68]	[-1.69,0.55]	
Exp. 2	-0.78	-0.28	-1.03	-1.91	-1.27	18,957
	[-2.59,1.02]	[-1.81,1.26]	[-3.40,1.33]	[-5.18,1.37]	[-9.15,6.61]	
Exp. 4	0.03	-0.23	-1.49*	-0.38	10.56	8,720
	[-0.98,1.04]	[-1.16,0.70]	[-2.99,0.02]	[-2.12,1.37]	[-111.13,132.25]	
Exp. 5	0.12	-0.34	1.74	0.66	-1.06	48,087
	[-0.91,1.16]	[-1.32,0.63]	[-0.62,4.09]	[-1.40,2.73]	[-7.81,5.69]	
Exp. 6	1.11	1.43	1.06	-3.04	1.66	5,063
	[-2.62,4.84]	[-2.63,5.49]	[-5.05,7.16]	[-11.62,5.55]	[-15.97,19.29]	
Exp. 7	-0.32	-0.13	-1.06*	-0.09	-1.27	26,928
	[-1.04,0.39]	[-0.86,0.59]	[-2.15,0.04]	[-1.53,1.36]	[-4.05,1.51]	
Exp. 8	0.06	0.13	1.18	-2.75	-4.33	24,055
	[-2.32,2.44]	[-1.94,2.20]	[-3.11,5.46]	[-8.69,3.18]	[-23.00,14.35]	
Exp. 9	-0.72**	-0.39	1.12	-0.24	0.48	50,783
	[-1.34,-0.10]	[-0.99,0.20]	[-0.45,2.68]	[-1.86,1.38]	[-0.96,1.92]	
Exp. 10	0.69	0.80	0.68	-0.02	0.63	10,989
	[-0.35,1.73]	[-0.41,2.02]	[-1.04,2.39]	[-1.96,1.93]	[-0.94,2.19]	
Exp. 12	-2.46	-0.62	-14.47	-3.45	-0.30	31,969
	[-6.35,1.44]	[-3.51,2.28]	[-53.50,24.56]	[-7.74,0.85]	[-4.38,3.77]	
Exp. 16	-0.09	-0.16	0.55	-0.95*	1.34	7,055
	[-0.88,0.70]	[-0.93,0.60]	[-0.68,1.77]	[-2.05,0.15]	[-1.00,3.69]	
Exp. 17	0.35*	0.56***	0.11	-0.48	-0.00	39,004
	[-0.02,0.72]	[0.22,0.89]	[-0.45,0.67]	[-1.16,0.21]	[-0.83,0.83]	
Exp. 18	-0.99	-0.37	0.21	-2.19	1.02	4,057
	[-2.85,0.87]	[-1.83,1.08]	[-1.62,2.03]	[-6.06,1.68]	[-23.40,25.45]	
Exp. 19	2.04	1.41	-0.71	0.08	3.17	740
	[-1.92,6.00]	[-1.66,4.49]	[-2.54,1.12]	[-4.77,4.93]	[-3.11,9.45]	
Exp. 20	0.75	0.38	0.30	0.61	0.66	2,471
	[-0.48,1.98]	[-0.66,1.42]	[-0.67,1.27]	[-1.51,2.74]	[-0.76,2.08]	

Notes: LATE estimates for separate fuzzy RD regressions for each quasi-experiment. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalances in pre-treatment financial outcomes. All point estimates are based on quadratic local polynomial bandwidths, triangular kernel weights with bias-adjusted coefficients and 95% confidence intervals. All coefficients stated in logs, with zero balances omitted. *** indicates a p-value < .01, ** < .05, * < .1.

Table A.4: LATE Effects for the Probability of Having any Non-BNPL Delinquencies, by Experiment

	Non-BNPL delinquency (1)	Credit card delinquency (2)	Retail loan delinquency (3)	Personal loan delinquency (4)	AFS delinquency (5)	Obs. (6)
Exp. 1	0.01	0.05	-0.13	-0.05	0.06	43,630
	[-0.21,0.23]	[-0.17,0.26]	[-0.30,0.04]	[-0.19,0.10]	[-0.07,0.18]	
Exp. 2	-0.77***	-0.41*	-0.14	-0.35**	0.12	18,957
	[-1.33,-0.21]	[-0.89,0.06]	[-0.51,0.23]	[-0.69,-0.01]	[-0.20,0.44]	
Exp. 4	0.09	0.09	-0.04	0.00	-0.09	8,720
	[-0.20,0.38]	[-0.21,0.39]	[-0.29,0.20]	[-0.20,0.20]	[-0.28,0.09]	
Exp. 5	-0.36*	-0.34*	-0.08	0.06	-0.08	48,087
	[-0.74,0.02]	[-0.71,0.02]	[-0.35,0.20]	[-0.19,0.32]	[-0.32,0.16]	
Exp. 6	-0.44	-0.53	0.06	-0.15	-0.02	5,063
	[-2.07,1.18]	[-2.28,1.22]	[-1.06,1.18]	[-1.18,0.88]	[-1.13,1.09]	
Exp. 7	-0.10	0.08	-0.08	-0.12	0.04	26,928
	[-0.33,0.13]	[-0.17,0.33]	[-0.23,0.08]	[-0.33,0.10]	[-0.12,0.20]	
Exp. 8	-0.00	0.04	-0.02	-0.03	0.34	24,055
	[-0.69,0.68]	[-0.70,0.79]	[-0.52,0.47]	[-0.67,0.60]	[-0.30,0.97]	
Exp. 9	0.01	-0.17	0.14	0.21*	-0.07	50,783
	[-0.20,0.21]	[-0.40,0.07]	[-0.03,0.32]	[-0.00,0.43]	[-0.24,0.11]	
Exp. 10	0.27	0.14	0.11	0.05	0.03	10,989
	[-0.09,0.64]	[-0.24,0.52]	[-0.17,0.38]	[-0.27,0.37]	[-0.22,0.28]	
Exp. 12	0.25	0.30	0.45	-0.07	-0.37	31,969
	[-0.48,0.98]	[-0.50,1.09]	[-0.17,1.07]	[-0.70,0.56]	[-0.99,0.26]	
Exp. 16	0.08	-0.06	0.02	0.02	-0.00	7,055
	[-0.15,0.30]	[-0.27,0.15]	[-0.14,0.18]	[-0.12,0.16]	[-0.15,0.14]	
Exp. 17	0.07	0.08	0.00	0.01	0.02	39,004
	[-0.04,0.18]	[-0.02,0.19]	[-0.07,0.07]	[-0.07,0.10]	[-0.04,0.08]	
Exp. 18	0.17	0.16	0.09	-0.18	0.03	4,057
	[-0.28,0.63]	[-0.26,0.57]	[-0.23,0.41]	[-0.56,0.20]	[-0.20,0.27]	
Exp. 19	0.58	0.66	0.78	-0.24	0.11	740
	[-0.28,1.44]	[-0.50,1.82]	[-0.29,1.85]	[-1.27,0.80]	[-0.42,0.63]	
Exp. 20	0.15	0.33	0.18	-0.31	-0.07	2,471
	[-0.22,0.52]	[-0.10,0.75]	[-0.20,0.56]	[-0.70,0.09]	[-0.38,0.23]	

Notes: LATE estimates for separate fuzzy RD regressions for each quasi-experiment. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalances in pre-treatment financial outcomes. All point estimates are based on quadratic local polynomial bandwidths, triangular kernel weights with bias-adjusted coefficients and 95% confidence intervals. *** indicates a p-value < .01, ** < .05, * < .1.

Table A.5: LATE Effects on Past-Due Balances, by Experiment

	Non-BNPL delinquency (1)	Credit card delinquency (2)	Retail loan delinquency (3)	Personal loan delinquency (4)	AFS delinquency (5)	Obs. (6)
Exp. 1	0.12	-0.47	0.58	-0.12	0.29	43,630
	[-1.03,1.28]	[-2.02,1.07]	[-0.99,2.15]	[-2.35,2.11]	[-0.98,1.56]	
Exp. 2	-0.01	-0.24	-1.84	-4.65	0.43	18,957
	[-1.81,1.79]	[-2.58,2.09]	[-6.77,3.10]	[-12.19,2.89]	[-1.43,2.29]	
Exp. 4	0.26	0.69	0.49	0.43	2.09	8,720
	[-0.90,1.41]	[-0.70,2.08]	[-1.71,2.69]	[-1.67,2.53]	[-0.93,5.12]	
Exp. 5	0.93	0.03	0.55	-0.61	2.30*	48,087
	[-0.66,2.52]	[-1.72,1.79]	[-1.45,2.56]	[-2.89,1.66]	[-0.41,5.00]	
Exp. 6	2.87	12.45	0.16	0.71	-4.81	5,063
	[-5.69,11.42]	[-11.94,36.83]	[-11.12,11.44]	[-12.43,13.84]	[-19.25,9.63]	
Exp. 7	0.07	-0.68	-0.09	0.12	0.30	26,928
	[-0.84,0.98]	[-1.76,0.41]	[-2.18,2.00]	[-1.60,1.83]	[-1.25,1.85]	
Exp. 8	1.25	0.55	-1.73	-0.84	-0.58	24,055
	[-1.10,3.59]	[-1.76,2.87]	[-5.75,2.28]	[-5.31,3.62]	[-3.12,1.95]	
Exp. 9	-0.63	-0.24	0.41	-1.64	-0.74	50,783
	[-1.43,0.17]	[-1.49,1.02]	[-1.17,2.00]	[-3.90,0.62]	[-2.79,1.31]	
Exp. 10	-0.17	0.28	1.24	0.24	2.01	10,989
	[-1.80,1.46]	[-1.10,1.67]	[-1.00,3.48]	[-2.84,3.33]	[-2.81,6.83]	
Exp. 12	-2.35	-2.01	0.26	-5.36**	1.74	31,969
	[-5.59,0.89]	[-5.75,1.73]	[-4.50,5.02]	[-10.38,-0.33]	[-2.36,5.84]	
Exp. 16	-0.35	0.52	-3.29**	-1.98	-1.47	7,055
	[-1.41,0.72]	[-0.78,1.81]	[-6.49,-0.08]	[-4.49,0.53]	[-4.27,1.33]	
Exp. 17	-0.17	-0.06	-0.38	-0.83	0.87*	39,004
	[-0.65,0.32]	[-0.65,0.54]	[-1.40,0.63]	[-1.93,0.28]	[-0.15,1.89]	
Exp. 18	-0.25	0.92	0.07	3.85	4.09	4,057
	[-2.40,1.91]	[-1.73,3.57]	[-2.55,2.70]	[-2.34,10.03]	[-220.90,229.08]	
Exp. 19	-0.17	0.78	-0.46	-0.94	-4.64	740
	[-3.17,2.84]	[-2.25,3.81]	[-2.93,2.00]	[-6.18,4.31]	[-12.68,3.40]	
Exp. 20	-0.75	-0.44	-0.47	1.20	-0.06	2,471
	[-1.98,0.47]	[-1.88,0.99]	[-2.02,1.08]	[-1.32,3.73]	[-1.71,1.58]	

Notes: LATE estimates for separate fuzzy RD regressions for each quasi-experiment. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalances in pre-treatment financial outcomes. All point estimates are based on quadratic local polynomial bandwidths, triangular kernel weights with bias-adjusted coefficients and 95% confidence intervals. Point estimates are shown in logs. *** indicates a p-value < .01, ** < .05, * < .1.

Table A.6: LATE Effects for Credit Card Outcomes, by Experiment

	Credit card utilization (1)	Nr. of quarters revolving (2)	Share of accounts revolving (3)	Total finance charges (4)	Obs. (5)
Exp. 1	-0.04	0.26	-0.09	-87.76	43,630
	[-0.19,0.12]	[-0.47,0.98]	[-0.25,0.07]	[-253.92,78.40]	
Exp. 2	-0.24	-1.46*	-0.29	38.82	18,957
	[-0.69,0.22]	[-3.15,0.23]	[-0.69,0.11]	[-237.68,315.32]	
Exp. 4	0.14	0.15	-0.19	-51.41	8,720
	[-0.10,0.38]	[-0.84,1.13]	[-0.44,0.06]	[-213.08,110.26]	
Exp. 5	-0.22	-0.69	-0.04	129.69	48,087
	[-0.49,0.06]	[-1.98,0.61]	[-0.27,0.19]	[-125.75,385.13]	
Exp. 6	0.28	2.39	0.17	-142.63	5,063
	[-0.70,1.26]	[-2.47,7.25]	[-0.79,1.14]	[-1036.49,751.23]	
Exp. 7	0.15	-0.13	0.10	56.71**	26,928
	[-0.14,0.44]	[-0.83,0.57]	[-0.13,0.34]	[2.26,111.15]	
Exp. 8	0.24	-0.04	0.32	29.41	24,055
	[-0.51,0.98]	[-2.03,1.95]	[-0.36,1.00]	[-193.88,252.70]	
Exp. 9	-0.07	-0.32	0.22*	3.87	50,783
	[-0.35,0.21]	[-0.97,0.33]	[-0.02,0.46]	[-43.81,51.55]	
Exp. 10	0.31	0.14	0.11	44.41	10,989
	[-0.22,0.85]	[-0.88,1.17]	[-0.43,0.66]	[-58.16,146.98]	
Exp. 12	0.07	0.94	-0.02	30.34	31,969
	[-0.79,0.93]	[-1.48,3.36]	[-0.73,0.69]	[-330.27,390.96]	
Exp. 16	-0.25*	0.07	0.14	-49.07	7,055
	[-0.55,0.05]	[-0.59,0.72]	[-0.09,0.36]	[-138.03,39.88]	
Exp. 17	-0.01	0.46***	0.06	-0.95	39,004
	[-0.10,0.09]	[0.12,0.80]	[-0.04,0.15]	[-74.75,72.85]	
Exp. 18	-0.11	-0.45	0.09	-220.93*	4,057
	[-0.53,0.30]	[-1.82,0.91]	[-0.23,0.40]	[-481.60,39.75]	
Exp. 19	0.39	2.12	0.13	-23.53	740
	[-0.81,1.59]	[-1.63,5.87]	[-0.60,0.87]	[-234.41,187.35]	
Exp. 20	0.22	0.73	0.05	54.38	2,471
	[-0.15,0.59]	[-0.66,2.13]	[-0.27,0.37]	[-169.84,278.60]	

Notes: LATE estimates for separate fuzzy RD regressions for each quasi-experiment. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalances in pre-treatment financial outcomes. All point estimates are based on quadratic local polynomial bandwidths, triangular kernel weights with bias-adjusted coefficients and 95% confidence intervals. Credit card utilization (column 1) and the share of revolving accounts (column 3) are represent averages across the four quarters after the first BNPL application. Columns 2 and 4 are totals across four quarters. *** indicates a p-value < .01, ** < .05, * < .1.

Table A.7: Aggregation Weights for RDD Estimates in Quarter 1

Quasi Experiment	BNPL Debt ($) (1)	Has BNPL Debt (2)	Non-BNPL Debt ($) (3)	Has Non-BNPL Debt (4)	Past due Non-BNPL Debt ($) (5)	Has past due Non-BNPL Debt (6)
1	.136	.133	.136	.122	.123	.134
2	.051	.05	.048	.047	.057	.046
4	.031	.035	.035	.033	.03	.033
5	.199	.21	.181	.21	.206	.194
6	.012	.009	.013	.013	.011	.012
7	.074	.091	.092	.081	.099	.091
8	.044	.039	.044	.039	.039	.039
9	.164	.16	.178	.175	.172	.175
10	.03	.029	.03	.029	.029	.029
12	.096	.094	.096	.094	.092	.094
16	.014	.014	.016	.016	.015	.016
17	.129	.113	.112	.116	.111	.116
18	.013	.016	.014	.016	.01	.013
19	.002	.002	.002	.002	.002	.002
20	.005	.005	.004	.005	.004	.005

Notes: Table presents aggregation weights for the RDD estimates from each quasi-experiment for the main outcomes. The sample is limited to consumers from the 15 quasi-experiments that did not display imbalances in pre-treatment financial outcomes. All quasi-experiment-specific specifications are based on quadratic local polynomial bandwidths, triangular kernel weights with bias-adjusted coefficients and 95% confidence intervals.