Targeting Higher Credit Card Payments

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Abstract

In a field experiment, I test two nudges in a credit card payment app: shrouding the credit card minimum payment amount and an option to pay 50% of the statement balance. After six statements, neither nudge increases credit card payments. This shows that existing evidence from online experiments—that finds increases in hypothetical credit card payments from shrouding the minimum payment—does not extrapolate to the field. My results demonstrate that the minimum payment does not act as an anchor that lowers cardholders' payments. Instead, cardholders frequently lack liquid cash and target paying at least the minimum payment.

*Rice University, Jones Graduate School of Business. benedictgk@rice.edu. I thank Martin Becker, Patrick Moran, and participants at the International Behavioural Finance Conference for their feedback. I thank the anonymous lender for sharing data from this experiment, and their staff for helpful discussions. The lender had the right to review this paper before its dissemination to ensure that it did not contain confidential information. This research was approved by Rice University's Institutional Review Board (IRB-FY2024-399). This experiment's analysis was pre-registered with a pre-analysis plan on the AEA RCT Registry: AEARCTR-0014102 (Guttman-Kenney, 2025). Thanks to Amy Qian for excellent research assistance. From 2016 to 2018, I was employed at the Financial Conduct Authority and worked on credit card research informing policymaking related to this topic of this paper. The Financial Conduct Authority had no involvement in this paper.

1 Introduction

A large body of work across the fields of economics, finance, management, marketing, and psychology documents the importance of "credit card anchoring" to explain consumers' credit card payment choices. Thaler and Sunstein (2008) write that a credit card minimum payment "can serve as an anchor, and as a nudge that this minimum payment is an appropriate amount". Stewart (2009) tests this idea in a lab experiment and finds that when the credit card minimum payment amount was shrouded, to prevent consumers "anchoring" to this amount, there are large increases in hypothetical credit card payments. This lead to the conclusion that "minimum-repayment information has an unintended negative effect, because minimum repayments act as psychological anchors".

Stewart (2009)'s results are a robust empirical finding that have been repeatedly replicated in multiple online experiments over time and across countries, with the results in this literature attributed to "anchoring" (Navarro-Martinez et al., 2011; Jiang and Dunn, 2013; Salisbury, 2014; McHugh and Ranyard, 2016; Salisbury and Zhao, 2020; Guttman-Kenney et al., 2018; Hendy et al., 2021; Sakaguchi et al., 2022; Ortiz et al., 2024).[1] Further evidence in support of "anchoring" in credit cards has been provided in credit card administrative data by Keys and Wang (2019) and Medina and Negrin (2022), who both examine the responses of consumers to changes in minimum payments and find that these are inconsistent with only liquidity constraints. Vihriälä (2025) also attributes "anchoring" as contributing to the co-holding puzzle whereby consumers simultaneously hold high-cost debt and low-yield liquid assets. An implication of this research is that without anchoring, consumers would pay more on their credit card.[2]

In this paper, I provide a test for whether the results of the online experiments extrapolate into the field. I study data from a field experiment on 6,714 consumers with a FinTech lender that provides credit cards in the United States. The experiment varies the choice architecture to shroud the minimum payment amount when consumers decide how much to manually pay on their credit card with the lender. I evaluate outcomes after six credit card statement cycles in four anonymous linked datasets. My main data is administrative credit card data from the lender, as well as using clickstream data on payment choices, household transactions data on liquid cash, and consumer credit reporting data on the portfolio of credit cards.

[1] Appendix Figure A1 shows an example of such a result from Guttman-Kenney (2024), where shrouding the minimum payment significantly increases hypothetical payments (% statement balance) by 12.4 (s.e. 0.8) percentage points in an online experiment of credit cardholders in the United Kingdom.

[2] Keys and Wang (2016) write "Anchoring to minimum payments on debt contracts can lead to lower payments, higher interest costs, and higher default rates, so our results highlight minimum payments as a potential target for regulators and innovators aiming to improve financial health."

This paper advances the existing academic literature in three ways. First, I test a nudge that shrouds the credit card minimum payment amount in a field experiment, whereas prior work only tested such a nudge in hypothetical online experiments. I show that this nudge does not significantly increase credit card payments, and therefore the results of the prior online experiments do not extrapolate to the field. Shrouding this minimum payment option only makes consumers 25% less likely to pay exactly the minimum, and 57% more likely to only pay up to one dollar above the minimum. These are too small to affect behavior. Second, I test the effects of a new nudge intervention, adding an option to pay 50% of the statement balance, which had not previously tested online or in the field, and show that this option is also ineffective in significantly increasing credit card payments. Adding a 50% payment option makes consumers significantly more likely to choose that amount, and in the short-term less likely to pay in full. Third, my empirical evidence helps to understand why consumers choose to only pay the minimum payment, or amounts above it. Consumers frequently lack liquid cash, and they *target* paying at least the minimum payment. The minimum payment does *not* act as an anchor to lower consumers' credit card payment choices.

This paper has a direct policy implication. In 2016, the United Kingdom consumer financial protection regulator, the Financial Conduct Authority, wanted to test a potential policy to shroud minimum payments, however, by 2018, it had "attempted to trial this in the field with U.K. lender(s) on actual payments but no lender was willing and/or able to do so" (Guttman-Kenney et al., 2018). The Financial Conduct Authority later announced its intention to consult on a policy to require U.K. credit card lenders to shroud the minimum payment, however, this policy was put on hold due to COVID-19 and other factors, including waiting for an evaluation of the effectiveness of other credit card policies that it had implemented (Financial Conduct Authority, 2021). In the United States, there have also been some discussions about implementing a similar policy (e.g. Tescher and Stone, 2022; Gdalman et al., 2023). My study provides new field experimental evidence that such a policy would be expected to be ineffective in achieving policymakers' desired aim to increase credit card payments to reduce revolving debt.

Heterogeneity analysis rules out a purely traditional liquidity constraints explanation, as there are also null results for consumers that are not constrained in their borrowing. Instead, consumers frequently lack liquid cash balances, with a minimum balance of less than $1,001 in the last 90 days before the start of the experiment. This means that they often have limited ability to pay more, and rely on heuristics aiming to target at least paying the minimum. Examining clickstream data on consumers' initial choices when the minimum payment is shrouded, shows a consumer's revealed preference for how much they want to pay. This reveals that consumers commonly want to initially pay amounts below the minimum, and then revise their initial selections to the minimum or slightly above it.

The psychology of consumers targeting payment amounts can help to understand my results. Recent marketing research (Bartels et al., 2024; Herzog et al., 2025; Schwartz, 2025) shows that the distribution of credit card payments are asymmetric around the minimum payment amount, with payments commonly at or just above this amount and rarely just below this amount, is consistent with "targeting" behavior, where consumers exert effort to meet a threshold that acts as a reference point (e.g., Heath et al., 1999; O'Donoghue and Sprenger, 2018), and is inconsistent with "anchoring" behavior (Tversky and Kahneman, 1974; Ariely et al., 2003), the term applied in this credit card domain by Stewart (2009) and the literature that followed. Shrouding a payment amount may be expected to change consumers' credit card payment choices if that shrouded payment amount acts as an anchor, an arbitrary and irrelevant focal point that influences choices.[3] However, if the shrouded payment amount acts as a target, and so is a relevant reference point that consumers aim for, then it may be expected to be harder to shift consumers' credit card payment choices away from this amount. This is because this target amount may be linked to their personal preferences or goals, which delivers a discontinuous increase in utility, for example, as marathon runners feel when exceeding their target times (Allen et al., 2017). There is growing evidence that consumers have reference dependent preferences, and this helps to understand high-stakes financial decisions. For example, Genesove and Mayer (2001) and Andersen et al. (2022) show evidence in the housing market, Gianinazzi (2022) in mortgage refinancing decisions, and some consumers target round-numbered monthly payment amounts in disaster recovery loans (Collier and Ellis, 2024), mortgages (Ng, 2024), and auto loans (Argyle et al., 2020; Mateen et al., 2024; Momeni, 2024; Katcher et al., 2024). As shown in Argyle et al. (2020), such targeting behavior is inconsistent with classic economic models with credit constraints.

I find that payments are significantly more likely to be at or just above this minimum payment amount than just below this amount when this amount is salient. When this minimum payment amount is shrouded, there is a relatively limited change in the distribution, and the results are consistent with the predictions of targets, and is inconsistent with the minimum payment acting as an anchor. I test this formally by applying a new method, Visual Proximity Analysis proposed by Bartels et al. (2024), to distinguish be-

[3]In Tversky and Kahneman (1974), one anchor is randomly determined by spinning a wheel of fortune. In (Ariely et al., 2003), one anchor is asking consumers to think of their (randomly-assigned) last two-digits of their Social Security Number. In both of these examples, the anchors are irrelevant to the choices studied, yet it still affects consumer choices.

tween anchors and targets, and the evidence is also consistent with targeting. My click-stream results are also consistent with targeting, that the minimum payment is actually increasing rather than decreasing consumers' payment choices. The minimum payment amount acting as a target rather than an anchor makes sense, given the discontinuity in economic costs associated with paying at least this amount (e.g., avoiding late fees) it is a natural amount for consumers to target.

This paper contributes to household finance literatures on consumer financial protection regulation and credit cards, which demonstrates that consumer behavior in this domain is difficult to shift. It is well-established that consumer misunderstanding of credit cards is costly and common (e.g., Soll et al., 2013; Hershfield and Roese, 2015; Lusardi and Tufano, 2015; Seira et al., 2017; Adams et al., 2022; Hirshman and Sussman, 2022; Han and Yin, 2025), with naive present bias (e.g., Meier and Sprenger, 2010; Heidhues and Kőszegi, 2015; Kuchler and Pagel, 2021), and a variety of other behavioral mistakes (reviewed in Beshears et al., 2018; Gomes et al., 2021), Given this, there is a large potential for improved consumer financial decision-making, and an important role for testing what works to inform policymaking (Soll et al., 2013). Thus far, information and nudges around the world have been largely ineffective at reducing credit card debt (e.g. Agarwal et al., 2015; Keys and Wang, 2016; Seira et al., 2017; Adams et al., 2022; Guttman-Kenney et al., 2025; Batista et al., 2025). Even when nudges in this credit card domain have shown some successes, there may be offsetting responses by consumers on that card (e.g., Guttman-Kenney et al., 2025), or it may have unintentional harmful effects elsewhere in their financial portfolio that outweigh the direct benefits (e.g., Medina, 2021). One policy that does shift credit card payment behavior is a hard paternalistic approach of requiring higher credit card minimum payment amounts. Allen et al. (2024) find that a policy increasing credit card minimum payments in the province of Quebec in Canada to 5% of the balance has been successful at persistently reducing credit card revolving debt, however, this reduces access to credit cards, which may be costly for some consumers.[4]

More broadly, the paper contributes to the behavioral economics and finance literatures on nudging. DellaVigna and Linos (2022) show how publication bias gave the impression that nudges were more effective than they were. Choukhmane (2025) and Choi et al. (2024) show that automatic enrollment is less effective than early evidence suggested. My results are consistent with Guttman-Kenney et al. (2025), again show-

[4]Castellanos et al. (2025) found no effects on increasing credit card minimum payments from 5% to 10% in Mexico, where credit card use is much less common, whereas Agarwal et al. (2023) find a more stringent policy increasing minimum payments from 20% to 40% (and also imposing spending restrictions), to be effective at reducing credit card debt in Turkey, and Agarwal, Qian and Zou (2025) shows the effects of a ban on credit for highly indebted borrowers.

ing that a nudge that is effective at moving consumers away from only paying exactly the minimum, a proximate outcome, is ineffective at reducing debt, the distal outcome. Guttman-Kenney et al. (2025) is the closest prior work to this paper. That paper tests a policy to shroud an option to autopay the minimum payment amount, finding that anchoring does not explain behavior but left open the possibility that anchoring was still important in the manual payment domain that I study here.[5] My contributions are to show how, even without offsetting responses, consumers hack the nudge, and to pin down the psychology behind credit card payment decisions.

My study contributes to debates on the use of different research methods, and the challenges of scaling interventions (List, 2022), with an example where replicated evidence from online experiments does not translate into the field. Recent behavioral science research by Gandhi et al. (2025) compares the results of five field experiments that test nudges to the results testing such nudges in hypothetical studies via online experiments. They conclude that the hypotheticals are generally consistent in their direction with the field experiments, but vary widely in their magnitudes compared to the estimates from field experiments. The use of online experimental studies in the social sciences has grown over time as platforms such as Amazon Mechanical Turk (“MTurk”) and Prolific Academic have made them convenient and affordable for researchers, relative to physical lab experiments that can be high-cost for small sample sizes.[6] Gordon et al. (2023) documents how even with rich data and sophisticated machine learning methods, they cannot accurately estimate the causal effects of adverts, as can be done with field experiments. Researchers are now starting to use Large Language Models (LLMs) to simulate consumer behaviors, and therefore a promising direction for future research will be to consider whether testing policies through such simulations are consistent with field experimental results.

My study also contributes to the behavioral literature. It shows that forcing consumers to deliberate to make an “active choice” (Carroll et al., 2009) does not lead to making what may be considered “better” decisions, given the high-cost of borrowing. Instead, consumers hack the active choice design to reveal the minimum payment and avoid pay-

[5] Also, some of the offsetting effects to the nudge that were found in Guttman-Kenney et al. (2025), lower autopay enrollment and consumers offsetting higher autopay payments with lower manual payments, do not apply to the manual payment domain, making it harder for consumers to counteract the nudge. Autopay enrollment choices were a one-off decision at card opening, and setting the autopay amount may be quite a difficult decision as it depends on future usage of the card, in contrast to manual payments studied here where consumers are making an active choice each month and therefore, somewhat attentive, in contrast the payment decision with autopay where a consumer may set a payment and become inattentive to it (Sakaguchi et al., 2022) of how much to manually pay given how much debt they have accumulated.

[6] Online experimental studies are also known as artefactual or framed field experiments in the taxonomy of Harrison and List, 2004.

ing more. It also contributes to the literature by providing an example of how consumers make choices under multiple reference points. While the welfare effects of single reference points have been studied (e.g., Reck and Seibold, 2025), there is not clear theory or empirical evidence on how consumers act under multiple reference points (O'Donoghue and Sprenger, 2018). My work suggests that in the credit card domain there is an ordering of competing reference points, targeting at least paying the minimum, then aiming to pay the statement balance. The salience of other amounts in-between the minimum payment and statement balance targets, such as 50% of the statement balance tested in this experiment and three-year payment scenarios in the 2009 Credit Card Accountability Responsibility and Disclosure (CARD) Act tested in Agarwal et al. (2015) and Keys and Wang (2016), can detract from these targets to slightly reduce payments for some consumers. Illiquid consumers targeting the minimum payment can help to understand why lenders set such low minimums as shown in Katz et al. (2024) and why increasing minimums reduces debt in Allen et al. (2024). My results help to understand the findings of Keys and Wang (2019) and Medina and Negrin (2022). These papers show that when a lender increases their minimum payment formula, some consumers pay more than the minimum but ultimately debt is unaffected (the minimum payment amount remains salient in both of these studies). Keys and Wang (2016) consider liquidity constraints or anchoring as two theories, but a third theory is possible; targeting. My evidence provides a new reinterpretation of these results. Instead of anchoring, their evidence is consistent with consumers targeting the minimum payment, and so it is hard-to-shift consumers to pay much more than this.

The paper proceeds as follows. Section 2 explains the experiment's design, the data, and the empirical methodology. Section 3 presents my results. Section 4 examines heterogeneity in my results. Section 5 evaluates whether the evidence is consistent with anchoring or with targeting payment amounts. Finally, Section 6 briefly concludes.

2 Experimental Design, Data, and Empirical Methodology

2.1 Experimental Design

I study a field experiment that varies the credit card payment options on one lender's mobile app. The lender is an anonymous FinTech lender that provides credit cards in the United States. The experiment is a randomized controlled trial (RCT) on consumers who hold credit cards with this lender, and who meet the selection criteria of having had at least one statement issued, a credit limit of over $40, a non-zero statement balance, and have never been over 35 days past due at the time of randomization, and excludes 2,337 consumers who were incorrectly randomized to treatment groups in December 2024. For this experiment, 6,905 consumers are randomly allocated across three groups:

1. 68% of consumers are in the **Control (C)** group. The payment options displayed to consumers in this group are: current balance, statement balance, 50% of the statement balance, minimum payment, and a free text box to choose any payment amount. These are the status quo options that consumers already experience with this lender in the absence of the experiment. This is displayed in Panel A of Appendix Figure A2.

2. 16% of consumers are in the **Treatment 1 (T1)** group. Consumers in this group are not shown the minimum payment option which, in the control group, also displays the minimum payment amount. Consumers in T1 are displayed the following payment options: current balance, statement balance, 50% of the statement balance, and a free text box to choose any payment amount. This is displayed in Panel B of Appendix Figure A2.

3. 16% of consumers are in the **Treatment 2 (T2)** group. As with T1, this does not show consumers in this group the minimum payment option. However, T2 also does not show consumers the option for 50% of the statement balance. Consumers in T2 have the following payment options displayed to them: current balance, statement balance, and a free text box to choose any payment amount. This is displayed in Panel C of Appendix Figure A2.

The experiment went live in the field on March 24, 2025 with 6,714 consumers—of which 4,542 (68%) were in group C, 1,096 (16%) in group T1, and 1,076 (16%) in group T2. This 6,714 is smaller than the original 6,905 consumers because 191 (3%) consumers had either closed their accounts or no longer met the other selection criteria at the time the experiment went live.

Importantly, in all three groups (C, T1, and T2), if a consumer submits a choice to pay an amount less than the minimum payment, a warning prompt appears with the minimum payment amount (for a visual see Panel D of Appendix Figure A2). This enables the consumer to revise their initial payment choice to prevent accidentally paying less than the minimum. Sakaguchi et al. (2022) shows in an online experiment how such prompts appear effective at preventing such choices (also see prompts being more broadly useful Mrkva et al., 2025).

T1 and T2 are nudges because they do not restrict a consumer's choice of payment amount. Instead, they only change the choice architecture (Thaler and Sunstein, 2008) of how salient particular choices are presented. The motivation behind T1 is that the minimum payment amount acts as a psychological default (e.g., Stewart, 2009; Medina and Negrin, 2022; Keys and Wang, 2019; Sakaguchi et al., 2022), potentially reducing some consumer payment choices to exactly this amount or amounts just above it. By shrouding this option, it may increase payments by increasing the salience of the statement balance for consumers to use as a target, and enabling consumers to make a more "active choice" (Carroll et al., 2009; Keller et al., 2011) without their choice potentially being distorted by the presence of the minimum.

The motivation behind T2 is that the 50% statement balance option can also act as a target, potentially reducing some consumer payment choices to exactly 50%, or amounts close to it, that would otherwise be higher. Prior research of the CARD Act statement disclosures has indicated that more payment options may unintentionally reduce payments for some consumers (Agarwal et al., 2015; Hershfield and Roese, 2015; Keys and Wang, 2016). There are two reasons why there may be different results in my experiment. First, statements are easily ignored by consumers (Adams et al., 2022), whereas consumers are required to select a payment amount in the app and so this environment may affect consumers differently. Second, these earlier studies examined three-year payment scenarios that are far lower payment amounts than a 50% statement balance amount, which may act as a high anchor or target. By shrouding the 50% option, along with the minimum payment option, it may increase payments by enhancing the salience of the statement balance for consumers to use as a target.

Comparing outcomes in group C to those in group T1 shows the effect of shrouding the minimum payment option, to target the higher payment options. Comparing outcomes in group C to those in T2 shows the effect of shrouding both the minimum payment and the 50% statement balance options, to target the statement balance as a higher payment option. Comparing outcomes from group T2 to those in group T1 shows the effect of adding a 50% statement balance option.

2.2 Data

All of the data I access is anonymous. I primarily use administrative data from the credit card lender for every card in the experiment. This data contains statement-level information (e.g., statement balances, credit card limits, minimum payment amounts due, statement dates), and disaggregated information on each payment a consumer makes on their credit card - including the timing, amount, and payment method (e.g., manual or autopay).

I also use three other linked datasets for my analysis. First, I observe linked click-stream data that consists of each payment choice made by a consumer on the lender's app. Second, I observe linked credit reporting data (Gibbs et al., 2025) from Equifax, which contains consumer-level aggregated information on the portfolio of credit cards held by consumers each month. Third, the lender requires all of its cardholders to link their household financial transaction data (Baker and Kueng, 2022) from Plaid. This linked Plaid data contains balances on each checking and cash saving account linked held by consumers that I use calculate the liquid cash balances of a consumer each day.

Table 1 shows summary statistics on the cards in the experiment using credit card administrative data before the experiment went into the field. This shows that variables are balanced in covariates (credit score, APR, card tenure, and credit limits) across the control and the two treatment groups. The average credit card limit is approximately \$290, with a typical statement balance just greater than \$200, and a typical VantageScore credit score near 560. In my data, 92% of the total value of payments are made via manual payments (i.e., via the app) rather than through autopay.

A natural concern of this sample is its external validity to the broader credit card market. The main limitation of this data source is that the credit cards in the experiment have lower credit limits relative to the credit card market in the United States. Despite this, I find that the distribution of payments for the control group, with approximately a quarter at or just above the minimum payment amount, and a quarter that are paying the full statement balance amount (or more), follows a very similar pattern to market wide data across lenders in the United States shown in prior work (Keys and Wang, 2016; Guttman-Kenney and Shahidinejad, 2025). In addition, Guttman-Kenney et al. (2018) found large and statistically significant effects of shrouding the minimum payment in a hypothetical payment study that were similar across both a low statement balance (£532) and a high statement balance (£3,217) scenario. This indicates that, given the prior literature, one might still expect to find significant increases in payments from shrouding the minimum payment in my sample if the driver of payment choices is anchoring to the minimum payment.

2.3 Empirical Methodology

The analysis for this experiment was pre-registered (Guttman-Kenney, 2025) on the AEA RCT registry before the experiment was live in the field. This pre-registration included a

Pre-Analysis Plan (PAP), which is also included in Appendix A of this paper, following the best practices in experimental economics (e.g. Abrams et al., 2025; Brodeur et al., 2024; Imai et al., 2025; List, 2025).

I first use t-tests to evaluate statistical significance. To understand the dynamics of the treatment effects, I use the dynamic regression specified in Equation 1, using data across all completed statement cycles during the experiment:

$$Y_{i,t} = \sum_{\tau=0}^{6} \left[\delta_{\tau}^{T1(vsC)} \left(D_{\tau} \times T1_{i} \right) + \delta_{\tau}^{T2(vsC)} \left(D_{\tau} \times T2_{i} \right) \right] + X_{i}'\beta + \mu_{t} + \varepsilon_{i,t} \tag{1}$$

In Equation 1, $Y_{i,t}$ denotes outcome Y for consumer i at time t. Consumers can only hold one credit card with the lender; therefore, examining data at the credit card account-level corresponds to the consumer-level. The term $\varepsilon_{i,t}$ is the error term. My coefficients of interest are δ_{τ}^{k}, for $k \in \{T1(vsC), T2(vsC)\}$. These are the coefficients on the interactions between the treatment groups and event time indicators. The term $T1_i$ is an indicator for the consumers in the treatment 1 group ($T1$) and the term $T2_i$ is an indicator for for the consumers in the the treatment 2 group ($T2$). The term D_{τ} is an indicator for event time τ. Given random assignment to treatment, δ_{τ}^{k} produces unbiased estimates of the dynamic average effects of treatment k after τ statements. I calculate the effects of adding the 50% payment option by $\delta_{\tau}^{T1(vsT2)} = \delta_{\tau}^{T1(vsC)} - \delta_{\tau}^{T2(vsC)}$.

After examining the data, I made two departures from my pre-registered regression specification, however, I still report results for the original pre-registered specification.[7] First, there are time trends in the data, and therefore I include granular time fixed effects (μ_t), which are fixed effects for each statement end date (unreported results were similar when including less granular time fixed effects for event time). Second, although covariates are balanced, there is a significant difference in payments as a percent of the statement balance and also in whether they have missed a payment between the control group and T1, but not with T2, in the event month preceding the start of the experiment as shown in Table 1. This difference could occur by chance, as Table 1 contains 32 tests and therefore one or two false positives would be expected at the 5% statistical significance level. Given this, I include a vector, X_i', of pre-experiment controls for each consumer, which also helps to increase the precision of my estimates.[8] This vector contains linear

[7]There are some different views on PAPs in the literature, for example Abrams et al. (2025) shows registrations of experiment are often too vague without them, whereas Banerjee et al. (2020) emphasize the benefit of avoiding too strict adherence.

[8]Goldsmith-Pinkham et al. (2024) highlight how multiple treatments with controls may, in theory produce biased estimates, however, they show that in practice there is only limited evidence of bias in experimental studies. In unreported results, I obtain similar estimates from estimating the effects of each of the treatments in separate regressions. This shows that any contamination bias is small in my setting and does not

controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay).[9] As a robustness check, I also report results using consumer-level fixed effects (γ_i) instead of this vector of consumer controls, and to enable me to estimate this, I include an additional observation for each consumer for the event time preceding the start of the experiment.

I also examine a static version of my earlier regression specification, showing the average treatment effects pooled across cycles, δ^k, estimated by Equation 2:

$$Y_{i,t} = \delta^{T1(vsC)}\, T1_i + \delta^{T2(vsC)}\, T2_i + X_i'\beta + \mu_t + \varepsilon_{i,t} \quad (2)$$

I follow my PAP in clustering standard errors at the consumer level in my regressions. However, this may be conservative given how Abadie et al. (2023) show that there is not a need to cluster in such settings, writing that "if one has a random sample of units from a large population with randomized treatment assignment at the unit level, there is no reason to cluster the standard errors of the least squares estimator. Doing so can be harmful, resulting in unnecessarily wide confidence intervals".

My primary outcome in my PAP is payment (%), defined as the sum of payments (\$) divided by the statement balance (\$). If payment (%) is greater than one, it is winsorized at a value of one, and it is given a value of one if statement balance is zero.. After the third statement cycle, the minimum detectable effect sizes for this primary outcome, assuming 80% power, are 3.8 percentage points at a 5% significance threshold, 4.7 percentage points at a 1% threshold, and 5.0 percentage points at a 0.5% threshold. My use of regression controls and pooling data across cycles helps to detect smaller differences. This appears sufficiently powerful, given the effect sizes of nudges shrouding minimum payments previously tested in online experiments on hypothetical credit card payment scenarios. For example, in Guttman-Kenney et al. (2023), a nudge shrouding minimum payments causes an increase in hypothetical payments of 12.4 percentage points as a per-

affect my conclusions.

[9]To ensure that no observations are dropped from these regressions with consumer controls, I winsorize credit limits at \$1,300, approximately the 99.5$^{\text{th}}$ percentile. There are five observations with unique credit limit values which I group with other observations that have a neighboring credit limit (randomly assigned above or below). There is one observation with a unique APR that I assign to the next APR value. The number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits variables added as controls are each winsorized at their 99$^{\text{th}}$ percentiles. None of these choices affect my results.

centage of the statement balance.

I also present results for eleven secondary outcomes specified in my PAP. These include measuring payments in dollars, and aspects of credit card behavior–spending in dollars and as a percent of the statement balance, revolving debt, and credit limits–, and, because of the non-normal distribution of credit card payments, binary indicators of payment amounts: paying less than the minimum, paying the minimum, paying 50% of the statement balance, paying the full statement balance, and paying the current balance.

I follow my PAP in evaluating the experiment after three completed statement cycles. The lender kept the experiment running for six cycles in case the effects took longer to emerge, and I report the dynamics over time and discuss my results over this longer horizon. Cycle zero is the month when the experiment started in the field. At this point-in-time, some consumers may have already made payments against their latest statement. Therefore, the effects of the RCT can only be clearly evaluated, from cycle one, the first statement with an ending date after the start of the experiment. I follow my PAP in analyzing the card-cycles observed, and given that 99.7% and 95.2% of cards remain open by the end of the third and sixth cycles, and there are no significant differences across treatments (Appendix Table A1), sample attrition is not an issue.

3 Experimental Results

3.1 Descriptive Analysis

I start by describing the CDFs of payments for the three experimental groups after three completed statement cycles in Figure 1. Panel A of Figure 1 shows the distribution of payments as a percent of the statement balance. Panel B shows, conditional on any payment being made, the distribution of excess payments. Excess payments are the amount of payment in excess of the minimum payment amount, and this is normalized as a percent of the statement balance less the minimum payment amount. In this figure, payments of exactly the minimum are denoted by the x-axis label "MIN", and payments of the full statement balance (or more) are denoted by the label "FULL".

The first takeaway from Figure 1 is that there is no substantial difference in the overall distribution of payments between the control group and the two treatments, T1 and T2. The second takeaway is that there are two small differences. Panel A of Figure 1 shows a clear discontinuous increase in the likelihood of payments being at 50% of the statement balances, compared to just below this amount, for the control group C and for T1, both of which display the 50% payment option. Whereas, there is no discontinuity at 50% for T2, which did not display the 50% payment option to consumers. This descriptive evidence is suggestive of the 50% payment amount acting as a salient target for consumers when it is presented to them. The second small difference is that payments at or just above the minimum become less likely in treatments T1 and T2 compared to the control group. This descriptive result appears consistent with the minimum payment amount acting as a salient target for consumers.

Table 2 shows the results of t-tests for the difference in means between the treatments and the control group for the primary outcome of interest, payments as a percent of the statement balance, and my secondary outcomes. These outcomes are all measured after three completed statement cycles. Consistent with Figure 1, Table 2 shows that there are no statistically significant differences in either the primary outcome measure of payments or in nine of the eleven secondary outcomes. The only two outcomes with significant effects are the probability of paying only exactly the minimum amount due and the probability of paying exactly 50% of the statement balance. Columns (4) and (5) of this table show that, compared to the control group C, T1 and T2 both make consumers statistically significantly 28% less likely to only pay exactly the minimum, with a 4.6 percentage point decrease on a control mean of 16.2%. There is no statistically significant difference in these outcomes when comparing T1 to T2 to examine the effect of adding a 50% payment option. Relative to the control group, T2 eliminates consumers choosing to pay exactly 50% of the statement balance, a payment choice that occurs for 2.7% of consumers in the control group. Unsurprisingly, T1 has no effect on this, as the 50% payment option remains salient in both T1 and the control group. The effect of adding a 50% payment option (i.e., comparing T1 to T2) is in column (6) of this table, making consumers percentage points more likely to pay exactly 50% of the statement balance, without having any other significant effects on consumer outcomes.

3.2 Regression Analysis of Primary Outcome

Table 3 shows the results of regression analysis on my primary outcome to estimate the effects of the treatments more precisely than in the unconditional t-tests. Panel A of this table shows the estimates from different regression specifications, starting with my regression with a constant and no controls in column (1), which is shown to follow my PAP for completeness, however, the results in this column are not robust to including time fixed effects, as shown in column (2). Column (3) shows the results of the regression specified in Equation 1 which includes both time fixed effects and pre-experiment consumer controls, where adding these consumer controls increases the R^2 from 0.02 to

0.27. Column (4) includes both time fixed effects and consumer fixed effects (instead of consumer controls), and this increases the R^2 to 0.51. These first four columns show the results after three completed statement cycles, and the next four columns, (5) to (8), show the results after six completed statement cycles maintaining the same ordering of the regression specifications as in columns (1) to (4).

I find no significant effects of the treatments on the primary outcome of increasing credit card payments as a percentage of the statement balance in this regression analysis, after either three or six cycles. After three cycles, column (3) of Table 3 Panel A shows that, relative to the control group, shrouding the minimum payment in T1 insignificantly changes payments, with a point estimate of 0.2 (s.e. 1.2) percentage points which is small relative to the control mean of 32.9% after three cycles. Relative to the control group after three cycles, shrouding both the minimum payment amount and the 50% payment amount in T2 insignificantly changes payments by 0.9 (s.e. 1.2) percentage points. While these estimates are insignificant from zero, they are positive, which may potentially suggest that there may be a small positive effect, without sufficient statistical power to detect it. However, when I examine the outcome after six cycles in column (7) of this table, the point estimates have noticeably declined and become negative. For T1, the estimate declines to -1.3 (s.e. 1.2) percentage points, and for T2 it declines to -0.5 (s.e. 1.2) percentage points. This indicates that any small positive short-term effect of such treatments is not sustained in the medium-term. These results are robust to the alternative specifications, without consumer controls in columns (2) and (6), and instead using consumer fixed effects, shown in in columns (4) and (8) of Panel A of this table, where estimates are also insignificant and decline over time.

The estimates from the regression specified in Equation 1 for all six statement cycles are shown in Figure 2, with the effects of T1 in orange, and the effects of T2 in blue, both estimated relative to group C. This figure demonstrates that there are not any significant positive effects on payments arising from either treatment, even for one cycle. Noticeably, even though the estimates on T2 are slightly positive up to cycle four, potentially suggesting that there may be a small short-term effect of T2 that I do not have power to detect, the estimates reduce to precise zeros by cycles five and six. The estimates for T1 start insignificantly negative and then become precise zeros, proving that T1 does not increase payments. The dynamics for the other regression specifications are shown in Panel A of Appendix Figures A4, A5, and A6, with the only result of note being that when including consumer fixed effects (Panel A of Appendix Figure A6), T1 temporarily significantly decreases payments by 2.9 and 3.2 (s.e. 1.4 and 1.5) percentage points during cycles zero and one respectively, but this then attenuates to be insignificant from zero in later cycles.

T2 has no significant effect over time in the specification with consumer fixed effects, with precise zero effect size estimates by cycles five and six.

Panel B of Table 3 increases statistical power by pooling observations across cycles (using the specification in Equation 2) with the same ordering of regressions as columns (1) to (4) of Panel A of this table. When interpreting these pooled estimates, it is useful to remember that pooling observations across cycles can provide a somewhat misleading indication of a treatment's medium-to-long-run effectiveness if a treatment's dynamics make it less effective over time. This panel shows that T1 and T2 cause no significant changes to payments with estimates of -0.7 (s.e. 0.8) and 0.3 (s.e. 0.8) percentage points respectively, which can be evaluated relative to the control group mean of 34.7%.

Comparing T1 relative to T2 shows the effects of adding a 50% payment option. Table 3 shows that this has no significant effect on increasing payments after three or six cycles or pooling across cycles, when including time fixed effects and consumer controls. The dynamic effects are shown in green in Figure 2, with no significant increases in any cycle. When using the pooled regression specification with consumer fixed effects, shown in Panel B column 4 of Table 3, there is a significant decrease in payments of 3.4 (s.e. 1.5) percentage points from adding the 50% payment option. This represents a 7.8% decrease relative to the omitted T2 mean in the cycle before the experiment. This significant decline in payments in the pooled estimates is driven by a significant decrease in payments during cycles zero to two before becoming statistically insignificant from zero during cycles three to six, shown in Panel A of Appendix Figure A6.

3.3 Regression Analysis of Secondary Outcomes

After three or more cycles, the primary outcome analysis finds no evidence of significant increases in payments from shrouding the minimum payment. There is some evidence that adding a 50% payment option actually reduces payments. I now consider my secondary outcomes to better understand the effects of these treatments.

Each row of Table 4 shows results for a different secondary outcome, with columns (1) and (2) respectively showing the effects of shrouding the minimum payment in T1 and T2, both are evaluated relative to the control group (C). Column (3) presents the effects of adding the 50% payment option by comparing T1 to T2. Table 4 shows results for secondary outcomes after three cycles using the regression specification with consumer controls (Equation 1) and Figure 3 displays the estimates across cycles. Appendix Table A2 shows results for secondary outcomes across the different regression specifications pooled across cycles, and Appendix Tables A3 and A4 do so for the dynamic estimates after three and six cycles respectively.[10]

Table 4 and Figure 3 displays that the results of my primary analysis are robust to examining payments in dollars, rather than normalized as a percent of the statement balance. They are also robust to examining statement balances or revolving debt (statement balances less payments made against these, bounded at zero), not finding that the treatments cause any significant reductions in these measures. In fact, adding the 50% option significantly increases statement balances by $9.6 (s.e. $4.5), a 4.1% increase on the control mean of $231 after three cycles, though this does not persist by cycle six ($2.1, s.e. $6.3, Appendix Table A4) as is visible in Panel B of Figure 3. The treatments do not lead to consumers experiencing higher or lower credit limits over time, which was a potential offsetting response. Consumers' spending behavior, another potential offsetting response, also appears unchanged by the treatments, except for the effect of adding the 50% payment option causes a significant increase in spending in cycle zero, as shown in Panels C and D of Figure 3, a result that is not sustained for other cycles. The treatments also do not lead to increased missed payments, a potential unintended effect, except for cycle six for T1.

As suggested by the descriptive analysis earlier in the paper, the treatments cause changes in the distribution of payments. First, relative to the control group, both treatments significantly reduce the probability of a consumer paying only exactly the minimum payment amount, and these effects persist over time as shown in Panel H of Figure 3. After three cycles, Table 4 shows how T1 and T2 significantly decrease this probability by 4.2 (s.e. 1.1) and 3.9 (s.e. 1.1) percentage points respectively, which are 25% and 24% decreases on the control group mean of 16.2%. The size of these effects on this outcome and this overall pattern of a credit card nudge being effective at reducing a proximate outcome, such as paying exactly the minimum, but unsuccessful at reducing distal outcomes, such as payments or revolving debt, is consistent with a nudge shrouding the autopay minimum option studied in Guttman-Kenney et al. (2025).

The treatments impact the probability of paying exactly 50% or paying in full. T1 makes consumers no more likely than the control group to pay exactly 50% of the statement balance over time, as displayed in Panel I of Figure 3. In contrast, T2, relative to the control group, effectively eliminates consumers choosing to pay exactly 50% of the statement balance. This effect is persistent across all cycles, with an estimate of -2.9 (s.e. 0.3) percentage points after three cycles as shown in Table 4. Putting these results together, I find that the effect of adding the 50% payment option, comparing T1 relative to

[10] Appendix Figures A4, A5, and A6 respectively present estimates across cycles for the dynamic regression model without controls, the model with time fixed effects, and the model with fixed effects for consumers and also for time.

T2, makes consumers significantly 3.6 (s.e. 0.6) percentage points more likely to pay the 50% amount, a result that persists across cycles as shown in Panel I of Figure 3.

T1 and T2 make consumers no more likely to pay in full, relative to the control group. Panel J of Figure 3 shows that adding a 50% payment option makes consumers temporarily significantly less likely to pay the full statement balance, but this effect does not persist after cycle two. Therefore, adding an additional payment option appears to be ineffective at increasing payments, partially because it detracts from consumers paying the statement balance in the short-term.

3.4 Alternative Payment Choices

What payment amounts are consumers choosing instead? I test this by using my earlier regression specification (Equation 2) that pools data across cycles to increase power, and changing the outcome to be dollar intervals relative to the minimum payment amount in Panel A of Figure 4. In Panel B, I change the outcome to be dollars relative to the 50% statement balance amount, and Panel C changes it to be dollars relative to the statement balance amount.

Figures 4 show precise zero effects of any of the treatments on dollar payment amounts slightly below the minimum, and the statement balance amounts. There is an asymmetric pattern to these results, with significant effects in dollars at or just above these target amounts and precise zero effects further up the distributions. T1 and T2 both make consumers significantly more likely to pay up to one dollar more than the minimum payment amount by 57% and 45% respectively, relative to the mean of 1.2% of consumers in the control group choosing this amount. This is consistent with consumers rounding it up to the next whole number rather than choosing exactly the minimum payment.

There is not consistent evidence that these treatments lead to consumers making payments much larger than this, T1 makes consumers more likely to pay some values \$x more than the minimum, $\$1 < x \leq \$2$ and $\$3 < x \leq \$4$, but less likely to pay other values \$x more than the minimum, $\$10 < x \leq \$11$ and $\$21 < x \leq \$22$. T1 makes consumers more likely to pay 50% statement balance and more likely to pay up to one dollar more than this amount, but not more than this. T2 has the opposite effect, making consumers less likely to pay 50% statement balance and less likely to pay up to one dollar more than this amount, but again not more than this. Putting these two results together, adding a 50% payment option makes consumers more likely to pay 50% or up to one dollar more than this amount, but not more than this amount. There are no significant effects for payment amounts around exactly the statement balance, shown in Panel C.

Panel A of Figure 1 shows that payments are significantly more likely to be at or just above the 50% payment amount than just below this amount, when this amount is salient, and when this amount is shrouded, there is a relatively limited change in the distribution of payments. The same asymmetric pattern occurs around the minimum payment amount. Appendix Figure A9 shows the distribution of payments in excess of the minimum across different cycles only persists up to cycle two. These results are consistent with Appendix Figure A8, which shows the distribution of the ratio of payments over the minimum, again across different cycles, most clearly changes up to cycle two.

Consistent with the descriptive results, Panel A of Figure 5 shows regression estimates across the distribution of my primary outcome measure of payments, again pooling across cycles to increase statistical power. This shows no significant effects on payments at the lower end of the payment distribution. In general, there are few significant effects, and I highlight these. Adding the 50% payment amount, denoted in this figure by the green category labeled T2 (vs T1), makes consumers less likely to pay in full, denoted by the 100 category on the x-axis of this figure. Adding the 50% payment amount makes consumers significantly more likely to pay around that amount ($45\% < x \leq 55\%$) and less likely to pay just below that ($30\% < x \leq 45\%$). Shrouding both the minimum payment amount and the 50% payment amount, corresponding to the blue category labeled T2 (vs C), makes consumers significantly less likely to pay in the range $45\% < x \leq 55\%$, and more likely to pay in the range $35\% < x \leq 45\%$. Shrouding the minimum payment amount without shrouding the 50% option, denoted by the orange category labeled T1 (vs C), has no significant effect across the payment distribution.

As is clear from Figure 1, there is a limited mass of payments in the proximity of 50%, and therefore, while the effects around this are significant, they are not large enough to change overall payments, in part because the increases in payments for some consumers (moving from just below 50%) are canceled out by decreases by others (moving from paying in full). The lack of significant changes in the distribution of payments when shrouding the minimum payment is in sharp contrast to the fundamental shifts in the distribution of hypothetical payments observed in the prior literature from online experimental studies shrouding the minimum payment (see Appendix Figure A1 for an example).

Appendix Table A7 shows that when the minimum payment is shrouded, consumers are 0.94 (s.e. 0.23) percentage points significantly less likely to pay multiples of the minimum payment amount—the "multiple heuristic" documented by Medina and Negrin, 2022. This is clearly visible in Panel A Figure 4 by the significant negative coefficients at \$11 more than the minimum, which is two times the common minimum payment of \$11. What payments are consumers choosing instead? When the minimum payment is shrouded, consumers are 3.61 (s.e. 0.62) percentage points significantly more likely to choose to pay small, round numbered amounts, also shown in Appendix Table A7.

This result contrasts with what might have been predicted based on the earlier work of Keys and Wang (2019) and Medina and Negrin (2022). This new result can be informative for considering the external validity of the experiment as it reveals the psychology that underpins consumers' payment choices. Consumers appear to rely on heuristics of the minimum payment when this amount is salient, as demonstrated by Keys and Wang (2019) and Medina and Negrin (2022) and also documented in the control group of my study. However, when the minimum payment amount is not salient, consumers heuristically choose round numbers, as found in the treatment groups of my study. Therefore, the overall effect of shrouding the minimum payment in other domains may be expected to depend on whether the dollar amount from using a heuristic of paying multiples of the minimum payment amount (or similar heuristics such as the minimum payment amount rounded up or the minimum payment plus some small round number) is greater than the dollar amount of small round numbers that consumers would otherwise choose.

3.5 Initial Payment Choices

The minimum payment remains salient in prior work with administrative data, such as Keys and Wang (2016), and therefore prior studies could only understand why consumers pay more than the minimum, such as due to anchoring. Instead, here I can also understand why consumers choose to pay only the minimum. I observe clickstream data, which records each payment choice a consumer made in the app, including their initial payment choice each statement cycle and any revisions to this. The initial payment choices of consumers can be especially informative for understanding consumers' ultimate decisions because they present a consumer's revealed preference for how much they would want to pay, without any distortion from the minimum payment amount. This is useful as it can help us understand which direction consumers' latent preferences are potentially being influenced by the presence of the minimum. Are they moved down, such as via anchoring, moved up, such as by targeting this amount, or unaffected?

I take the first payment choice of each consumer each statement cycle during the experiment, and apply my pooled regression specification to increase power. I study outcomes on consumers' initial choices. Panel A of Table 5 shows that T2 makes consumers significantly likely to initially choose a lower payment amount, measured by adapting my primary outcome to be *initial* payment choice (as a percent of the statement balance). The effect is 4.33 (s.e. 0.81) percentage points, relative to a control mean of 26.83%. Adding the 50% payment option increases this by 3.43 (s.e. 1.07) percentage points.

The main margin driving these results appears to be consumers making initial payment choices that are less than the minimum payment amount. Panel A of Table 5 shows that T1 and T2 are respectively 19.44 (s.e. 1.16) and 30.50 (s.e. 1.02) percentage points significantly more likely to lead consumers to initially choose an amount less than the minimum payment amount, compared to the control mean of 33.74%. Adding 50% payment option makes them 11.06 (s.e. 1.38) percentage points less likely to do so.

The full distribution of payment choices shows how consumers initially want to pay less than the minimum payment amount, but then revise their choices. Figure 6 shows CDFs of payment choices pooled across cycles. The solid lines denote consumers' first choices in a statement cycle and the dashed lines their last choices in a statement cycle. The colors of the lines denote the control, T1, and T2 consistent with earlier figures. Panels A, B, and C define payments in different ways. Panel A shows the payment choice in dollars, Panel B shows the payment choice less the minimum payment amount, and Panel C displays the payment choice as a percent of the statement balance.

Panel A of Figure 6 reveals that it is common for consumers to first choose amounts that are at zero, close to zero, or not making a choice at all (which I classify as zero). T1 and T2 make consumers much more likely to make such very low payment choices. While the control group shows a large spike at exactly \$11, a common minimum payment amount, this is much smaller for T1 and T2. This pattern of consumers initially choosing payments below the minimum when this payment amount is shrouded is clearly shown in Panel B, where values to the left of zero signify that a consumer chooses an amount below the minimum payment amount. The spike around $-$11 in Panel B corresponds to consumers initially choosing zero or near-zero payment amounts but having a minimum payment of \$11. Panel C shows the full distribution of payment amounts as a percent of the statement balance, where T2 makes consumers more likely to initially choose to pay less than 50% of the statement balance, whereas T1 makes consumers more likely to initially choose to pay less than 10% of the statement balance but slightly more likely to choose to initially pay under 50% of the statement balance. In this figure, I classify consumers who attempt to click through without making any selection as zeros, however, the results are robust to excluding such zero or no choice clicks, which may be warranted if these contain accidental choices as opposed to purposeful attempts by consumers to hack the nudge to reveal the minimum payment amount, as shown in Appendix Figure A11 and Panel B of Table 5.

These effects can be clearly seen in regression form in Panel B of Figure 5, which uses as outcomes initial payment choices (% statement balance) instead of actual payments (%

statement balance) made by a consumer, which are shown in Panel A of the same figure. Panel B clearly illustrates how T1 and T2 make consumers significantly more likely to choose zero payment amounts. This mass largely appears to come from consumers being less likely to choose initial payments of $5\% < x \leq 25\%$. If I exclude zeros and no choice clicks, the effects are significantly positive for $0\% < x \leq 5\%$ and significantly negative for $5\% < x \leq 20\%$ (Appendix Figure A12). Around 50%, the two treatments have different effects. While T2 makes consumers more likely to initially choose to pay in full, consistent with this payment amount being more salient, adding the 50% payment amount option makes consumers less likely to choose to pay this amount.

One potential explanation for choosing low payments is a lack of financial sophistication, such as consumers not understanding exponential discounting (Stango and Zinman, 2009), as is well-documented in the credit card market (e.g., Soll et al., 2013; Lusardi and Tufano, 2015; Seira et al., 2017; Adams et al., 2022). While this may be part of the explanation, research consistently shows that providing information to consumers on credit card repayment times does not significantly change their choices Agarwal et al. (2015); Seira et al. (2017); Adams et al. (2022), even if it reduces confusion (Adams et al., 2022), so this bias alone cannot explain consumers' payment choices.

Consumers revise their initial low choices, and across all three panels, there is limited important difference between the control group and the two treatments in the distribution of consumers' final payment choices. The only discernible difference in Panel B of Figure 5 is that the treatments smooth initial payments around 50% of the statement balance when this 50% payment option is shrouded, a result consistent with Panel A of 5.

The other effect of note from this clickstream data is that shrouding the minimum payment amount adds friction to the payment journey, with T1 and T2 causing significant 0.4 (s.e. 0.11) and 0.85 (s.e. 0.11) increases in the average total number of payment choices made per statement cycle from 3.8 in the control group, conversely adding a 50% payment option reduces the number of choices made (Panel A of Table 5).[11]

[11] When interpreting this estimate, the likely range of plausible effect sizes ranges between zero, if all consumers who would have chosen the minimum or above still did so, and one if all consumers select an amount less than the minimum. It was unlikely that consumers would make more than one additional click because if a consumer selects an amount less than the minimum, a prompt reveals the minimum payment amount in the control and also in both treatment conditions. One additional click may be considered (slightly) costly by taking up consumers' time, however, if it arises from making behavioral consumers more attentive to their payment decision it may be a benefit.

3.6 Autopay

Autopay enrollment does not explain my results. One potential reason for the null effects could be if consumers pay via autopay, they may not even view the manual payment screen, and so this may attenuate a positive effect on the subset of consumers not enrolled in autopay. I already controllled for autopay in my main results, and even among the 41% of consumers enrolled in autopay before the start of the experiment, 83% of their total value of payments are made manually (such an importance of manual payments for those enrolled in autopay is shown in Guttman-Kenney et al., 2025).

Nevertheless, I can more rigorously evaluate this concern excluding the consumers who are enrolled in autopay before the start of the experiment. Autopay enrollment is highly persistent in this and other credit card settings (e.g., Gathergood et al., 2020; Adams et al., 2022; Sakaguchi et al., 2022; Guttman-Kenney et al., 2025; Wang, 2024). When I apply my main regressions to the consumers who are non-autopay enrolees pre-experiment, I find no significant increase in payments, as shown in Appendix Table A5. Appendix Figure A7 shows the dynamics of the effects on primary and secondary outcomes for non-autopay enrollees are consistent with my main results. Non-autopay enrollees experience a stronger temporary effect of T2 to increase the probability of paying the full statement balance, and to decrease the probability of paying 50% statement balance in the first few cycles of the experiment. However, both of these effects attenuate towards zero over time, and are not large enough to reduce revolving debt, even temporarily. In this sample, T1 actually significantly reduces payments in dollars after six cycles, though there is no significant change in payments when measured as a percent of the statement balance.

3.7 Portfolio Effects

One caveat to discuss is that all of my outcomes are measured on the card in the experiment, however, consumers hold other credit cards with other lenders. As discussed in my pre-analysis plan, ideally I would evaluate the effects of the nudges on total payments across the portfolio of all credit cards held by a consumer. Unfortunately, also as I discuss in my pre-analysis plan, as documented in Guttman-Kenney and Shahidinejad (2025), consumer credit reporting data in the United States does not include credit card payments information for any of the six largest credit card lenders in America, and only 24% of credit cardholders have this information across all their cards held.[12] Instead, I

[12]This also means that I cannot estimate consumer use of the balance-matching heuristic Gathergood et al. (2019) as a proxy for behavioral consumers used in Agarwal, Presbitero, Presbitero, Silva and Wix (2025).

can only estimate the effects on the portfolio of statement balances observed in linked Equifax credit reporting data, as shown in (Guttman-Kenney and Shahidinejad, 2025), statement balances are a noisy proxy for the portfolio of revolving debt, although the noise is mainly an issue for high (721+) credit score consumers. This portfolio statement balance outcome is also the portfolio measure used by Keys and Wang (2019) (their Online Appendix Figure A7). Appendix Table A6 shows that, as expected, I find no evidence of the treatments reducing the portfolio of statement balances, consistent with the nudges not reducing consumer debt.

Even though the nudges do not increase payments for the cards in the experiment, one possibility is that consumers increase payments on other cards held by the consumer. While this is possible, it is extremely unlikely to occur given prior research findings, and would be hard-to-explain with any standard economic theory or even a behavioral one. The nudging literature finds that the strongest effects of nudges are observed on the most short-term and proximate outcomes (Beshears and Kosowsky, 2020; Laibson, 2020; Guttman-Kenney et al., 2025, e.g.,[]), which would be the card in the experiment in our case. If the nudge increases payments on the card in the experiment (which it did not), this would generally be expected to either leave payments unchanged elsewhere (for example, if consumers were not liquidity constrained and somewhat inattentive) or to have the effect crowded out by lower payments (for example, if consumers are liquidity constrained or somewhat attentive) or offsetting consumer effects elsewhere in their portfolio, as in Medina (2021). Prior work (Gathergood et al., 2019; Ponce et al., 2017; Hirshman and Sussman, 2022) shows that consumers often allocate payments across credit cards proportional to their statement balances ("balance-matching heuristic"), and this would predict that if consumers made no higher payments or changes to spending on the credit card in the experiment, and therefore statement balances are unchanged, then these consumers would also not make higher payments on their other cards. In both Adams et al. (2022) and Guttman-Kenney et al. (2025), credit card autopay nudges have no effect on increasing payments for the card in the experiments and also have no effect at increasing payments across the portfolio of cards held by a consumer.

4 Heterogeneity

In theory, the welfare effects of a nudge can be positive even if it has no average effect on behavior (List et al., 2024; Allcott et al., 2025), although, in practice, it may be challenging for a regulator to legislate such a policy, given the opportunity costs of regulatory resources and the need to justify the benefits to some consumers outweigh not only the costs to other consumers but also the compliance costs. In the context of this experiment, the largest welfare gains from the nudge would be expected to arise from the subset of consumers who are able to pay more to reduce their credit card debt, but did not do so. There may be many behavioral reasons for consumers who are able to pay more not doing so, with anchoring to the minimum payment being one reason that is testable given the design of this experiment.

I examine heterogeneous treatment effects by segmenting consumers by whether they appear to be constrained. This focus is motivated by the online experiment in Guttman-Kenney et al. (2023), where a nudge shrouding the minimum payment causes the unconstrained consumers—measured by self-reporting experiencing no financial distress—to have the largest increases in hypothetical payments. In this paper, I consider three ways to measure whether a consumer is constrained in data. First, based on consumers' pre-experiment payment behavior on the card in the experiment. Second, based on how much pre-experiment credit card limit a consumer has not utilized, and third, based on measures of how much liquid cash consumers had available prior to the experiment.

4.1 Near Minimum Payers

In a simplistic way, it is clear that not all consumers in my data are liquidity-constrained, as Figure 1 and Table 1 show that the majority of consumers are paying more than the minimum payment, the corner solution a constrained consumer would attempt to pay. I follow Keys and Wang (2019) for isolating a group of consumers do not appear to be constrained: near minimum payers. Consumers who pay strictly more than the minimum, and therefore cannot have a binding constraint at the minimum payment amount, but pay an amount just above it may be most susceptible to the nudge if their choices are driven by anchoring to the minimum. In my data, I classify 18% of consumers as near minimum payers based on these consumers having paid more than the minimum but less than or equal to $50 more, and who have not paid in full, for at least 50% of the six statement cycles leading up to the start of the experiment.[13] This is close to the 20% of consumers that Keys and Wang (2019) classify as near minimum payers, paying greater than but within $50 of the minimum in the majority of months.

I pool data across cycles to increase power, however, even when I do so, I find no evidence that shrouding the minimum payment significantly increases payments for these near minimum payers, with the effect of T2 of -0.56 (s.e. 1.54) percentage points. There

[13]For the consumer who opened their card less than six statement cycles, I calculate with the available statement cycles.

is also no significant effect on payments for the other (non-near-minimum) consumers, which may be expected as it contains consumers who only pay the minimum, due to being constrained and will not pay more, as well as consumers who pay in full and also will not pay more. These results are in Appendix Table A8. The fact that the near-payers do not increase their credit card payments when the minimum payment amount is shrouded is inconsistent with such consumers anchoring to the minimum payment as proposed in Keys and Wang (2019).

4.2 Credit Card Utilization

One common way to evaluate constraints is to determine whether credit cardholders are at (or close to) their credit limit. Prior work (e.g. Agarwal et al., 2018; Aydin, 2022; Yin, 2025; Agarwal, Alok, Ghosh and Zhang, 2025) has consistently shown that lender-driven credit card limit increases cause cardholders to increase their borrowing and spending. Importantly, such responses occur for consumers who, before the limit increase, had balances far below their limit, and so were unconstrained in their borrowing. Lee and Maxted (2024) uses nationally-representative data to show that credit card borrowers with effectively zero liquid cash are frequently not borrowing at their available credit card limits, which is inconsistent with how liquidity constrained hand-to-mouth consumers appear in heterogeneous-agent macroeconomic models with both zero liquid cash and at their credit card borrowing constraint (e.g. Kaplan and Violante, 2022), but consistent with present bias.[14] Olafsson and Pagel (2018) also show evidence of consumers who do not experience binding liquidity constraints acting hand-to-mouth.

I split consumers by tertiles of their pre-experiment credit card utilization, calculated by statement balance divided by credit limit. I find a significant increase in payments for the lowest tertile of credit card utilization, finding T2 increases payments by 3.7 (s.e. 1.6) percentage points. There are no significant effects for the other two tertiles, with point estimates being negative but insignificant from zero. However, this result only occurs when I calculate utilization rates using only the card in the experiment. Consistent with Lee and Maxted (2024), consumers in my experiment frequently have available credit based on their portfolio of credit cards observed in linked Equifax credit reporting data. Median and mean credit card portfolio utilization rates are 25% and 36% respectively. Only 16% of consumers have a portfolio utilization rate of 90% or higher, indicating that they are effectively constrained. This means that consumers are generally not experienc-

[14] There is limited theoretical or empirical evidence on how multiple behavioral biases interact, beyond the taxonomy of Stango and Zinman (2023). The implications of interactions between present bias and targeting behavior in credit cards may be an interesting theoretical avenue to pursue.

ing a binding borrowing constraint. When I examine heterogeneous treatment effects by tertiles of portfolio utilization rates, I find no significant effects on payments, even for the least-utilized tertile. See Appendix Table A9 for these results.

4.3 Liquid Cash

The final source of heterogeneity I examine is consumers' liquid cash balances before the start of the experiment, calculated using linked Plaid data. Consumers with liquid cash available may be making behavioral mistakes by not paying more of their credit card debt, keeping cash that earns little-to-no interest while paying interest rates or 15% or higher on their credit card. I follow Guttman-Kenney et al. (2025) in calculating the *minimum* liquid cash balances in the 90 days before the start of the experiment. This is a useful measure as it captures the fact that incomes and expenditures do not perfectly align. For example, consumers may temporarily have liquid cash but plan to use it to meet their upcoming expenses such as rent, and therefore, this is not truly spare cash to pay down their credit card debt. I also show a more traditional point-in-time liquid cash balance. The choice of what constitutes a "high" amount of liquid cash available is a judgment, and therefore I show the sensitivity of my results for three thresholds: \$1+, \$501+, and \$1,001+. As discussed in Guttman-Kenney et al. (2025), when interpreting these measures, it is important to remember that liquid cash is an equilibrium outcome that may arise for a mixture of reasons, including overconsuming due to naive present bias (e.g. Kuchler and Pagel, 2021; Lee and Maxted, 2024) and financial illiteracy (e.g., Lusardi and Tufano, 2015; Jørring, 2024).

I find that consumers in my experiment frequently have limited liquid cash available. Only 4% of consumers have "high" balances with minimum liquid cash balances of at least \$1,001, which is significantly lower than the 30% of consumers implied by a point-in-time measure of having liquid cash balances of at least \$1,001. A similar pattern occurs when using a lower threshold for a "high" balance: 7% of consumers have minimum liquid cash balances of at least \$501, and 43% point-in-time liquid cash balances of at least \$501. Despite a very different sample and time period, these are consistent with the findings in Guttman-Kenney et al. (2025) studying consumers enrolling in autopay for a large U.K. bank who provides credit cards to the breadth of the credit card market, where only 11% have at least £1,001.[15] This suggests such patterns of frequently holding limited liquid cash may be a more general phenomenon among consumers that revolve credit

[15]It also appears consistent with Gathergood and Olafsson (2025), who examine Icelandic data and show how co-holding cash and overdrafting is substantially less common once using daily data than it appears using monthly data.

card debt.

I find no evidence that there is a subgroup of consumers by their liquid cash who significantly increase their credit card payments when the minimum payment amount is shrouded. Table 6 shows heterogeneous treatment effects by measures of liquid cash. Consumers with "low" minimum liquid cash balances do not increase their payments, which would be expected since these consumers do not have funds to pay more without changing other behaviors (e.g., reducing spending, increasing income). The small subset of consumers with "high" minimum liquid cash balances also do not significantly increase their payments. For the 4% of consumers with minimum liquid cash balances of at least $1,001, T2 has no significant effects on payments, although this is a noisy estimate of 5.9 (s.e. 4.6) percentage points. The 96% of consumers who have minimum liquid cash balances of less than $1,001 may have a more muted response, with an estimate for the effect of T2 of 0.21 (s.e. 0.82) percentage points, with a 95% confidence interval of the difference between the high and low minimum liquid cash groups being -14.7 to 3.4 percentage points.

Another use for these heterogeneous effects is that they can help inform how applying these nudges may affect other populations of cardholders in the United States or other countries. It appears that such nudges may only be effective in domains where there is a large fraction of credit cardholders who are borrowing while also having minimum liquid cash balances in the last 90 days of at least $1,001. This means that policymakers or researchers can calculate whether this criteria is met in their domain, and that may be sufficient to establish whether it is worthwhile replicating this field experiment or making policy.

5 Distinguishing Anchoring from Targeting

Putting these results together, it is clear that neither of the treatments that shroud the minimum payment amount have any substantial, lasting effect on consumers, beyond making them less likely to pay only exactly the minimum. This is in sharp contrast to the evidence from examining effects on hypothetical payments in online experiments, which predicts large increases in payments, attributed to anchoring to the minimum payment amount. I also find little substantive effect of adding a 50% payment option, indeed, there is some evidence that this option reduces rather than increases payments, making consumers more likely to pay 50% and less likely to pay the full statement balance. My heterogeneity analysis also shows results that are inconsistent with anchoring to the minimum payments, and null results even for consumers who are not liquidity constrained.

Consumers are making payment choices in the context of frequently facing limited liquid cash.

5.1 Value Proximity Analysis (VPA) Methodology

My experiment does not change the economic costs associated with paying the minimum, or other amounts, so the economic trade-offs are unchanged. The experiment varies the psychological factors, and can provide a test of anchoring. As Kleven (2016) note, "an issue that has received relatively little attention in the literature is that kinks and notches may represent reference points". The psychological theory of Bartels et al. (2024) provides an empirical test–Value Proximity Analysis (VPA)–for whether behavior around a focal point is best characterized as psychological anchoring or as targeting (i.e., reference dependent preferences). The idea behind this test is to examine the distribution of observations around a focal point. VPA plots the proportion of observations that are at or above a focal point, starting with the 1% of observations with absolute values in the closest proximity to the focal value and expanding the proximity window by one percentage point until 100% of observations are included. Anchors should have approximately 50% and increase under a low anchor as more observations are included (or decrease under a high anchor). In contrast, a target should have close to 100% of observations being above the focal value, as consumers exert effort to achieve at least this value, and slope down as more observations are included.

I estimate the relationship in an OLS regression with one observation, i, for each proximity window ($Proximity_i \in \{1, 2, ..., 99, 100\}$) of each treatment group (C, T1, and T2), using Equation 3:

$$Y_i = \alpha + \beta^{T1}\,T1_i + \beta^{T2}\,T2_i + \gamma Proximity_i + \theta^{T1}\,(Proximity_i \times T1) + \theta^{T2}\,(Proximity_i \times T2) + \varepsilon_i \tag{3}$$

In this equation, the intercept, α, reflects the value for the omitted category, the control group (C), and the regression includes two binary indicators for each of the treatments, T1 and T2. The regression includes a term, $Proximity_i$, to estimate how the relationship linearly varies with the proximity window, and then includes interactions with T1 and T2 to allow for the slope of this to differ between C, T1, and T2. I use heteroskedastic-robust standard errors. I run this regression separately for each outcome, Y_i, that could act as an anchor or a target for consumers: the minimum payment amount, 50% statement balance amount, and statement balance amount in columns 1, 2, and 3, respectively of Table 7.

The VPA approach has clear predictions for distinguishing anchors and targets. First, under anchoring, the intercept would be near 50%, whereas under targeting it would be near 100%. Second, under anchoring, the intercept and the slope would both significantly vary with the treatments that make particular amounts less salient, whereas if these are targets, the slope would not be significantly changed. VPA also predicts a negative slope for targets or high anchors, and a positive slope for low anchors.

5.2 Value Proximity Analysis (VPA) Results

Figure 7 describes the VPA results, and Table 7 shows the VPA regression results. My VPA results are are consistent with minimum payment amounts and statement balance amounts acting as strong targets and not as anchors. The intercept in column 1 of Table 7 is 98.5%, very close to 100%, consistent with the minimum payment acting as a target rather than as an anchor. There is a significant negative slope, and neither the intercept nor the slope is significantly affected by the treatments, again consistent with targeting. These VPA results are consistent with the rest of the analysis of my experiment, which shows that credit card payment choices are not due to anchoring to the minimum payment. The minimum payment amount acting as a relevant target value for consumers to aim to pay, rather than an irrelevant anchor, makes sense given how there are strong economic incentives to paying at least this amount, such as avoiding late fees. It is also consistent with qualitative evidence in Katz et al. (2024), where consumers self-report paying at least the minimum.

The results from column 2 of Table 7 considers the effects of T1 and T2 with the 50% statement balance amount. The intercept here is closer to 75% than to 50% or 100%, so this result is inconclusive. This makes sense as, in contrast to the minimum payment, there is not a discontinuous change in economic payoffs at this value, only a change in psychological payoffs. There is no significant change in slope from either treatments, relative to the control group. The impact of adding the 50% payment amount option can be studied by comparing T2 to T1, with a slight insignificant reduction in the slope relative to the T1 intercept of 82.1% ($\alpha + \beta^{T1}$), and an insignificant flattening of the slope relative to the T1 slope of -0.54 ($\gamma + \theta^{T1}$) per one percentage point increase in proximity. The overall slope remains significantly negative, inconsistent with being a low anchor but consistent with a high anchor or a target. The evidence is therefore mixed as to whether to interpret this amount as a weak high anchor or as a weak target. Either way, ultimately it is not an option that affects medium-term payment outcomes, and if anything, it is reducing rather than increasing payments in the short-term, so does not appear to be a useful policy for regulators who are looking for ways to reduce credit card debt.

There are strong economic incentives for consumers to pay the statement balance amount, as doing so avoids incurring interest. Column 3 of Table 7 shows the relationship with the statement balance amount. The intercept is near 90%, and there is a significant negative slope with proximity. As with the minimum payment amount, the statement balance amount also appears to act as a target rather than as an anchor, consistent with Schwartz (2025), who highlights the role of the statement balance as a target rather than an anchor. The intercept and the slope are not significantly affected by either treatment, although, of course, the statement balance amount is not shrouded in any of groups C, T1, or T2, it does become more prominent with the shrouding of alternative payment options in T1 and T2.

6 Conclusions

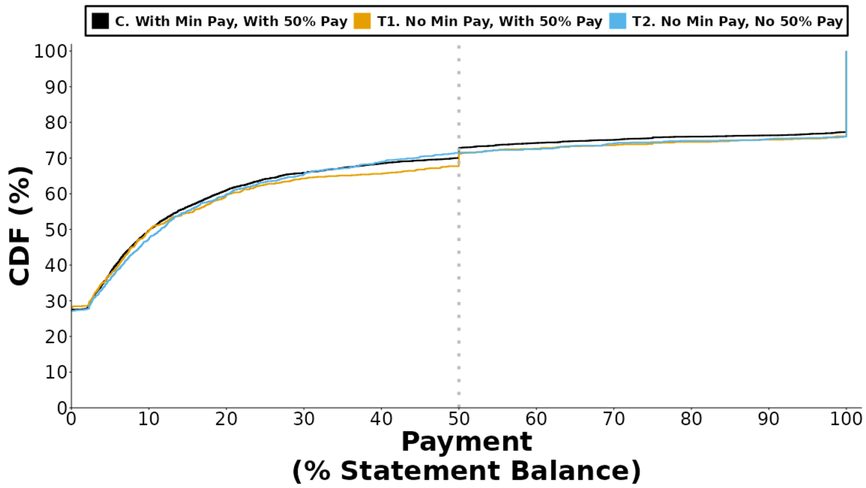
A large body of work across the fields of economics, finance, management, marketing, and psychology documents the importance of "credit card anchoring" to explain low credit card payments. In this paper, I test two nudges that vary a consumer's credit card payment options to target different amounts in a field experiment on 6,714 consumers evaluated over six credit card statement cycles. My results show that adding a 50% payment amount does not increase payments. Instead of increasing low payments, this 50% options makes consumers temporarily less likely to pay in full.

Shrouding the minimum payment amount does not increase credit card payments. This shows that existing evidence from online experiments, which demonstrates a large increase in hypothetical credit card payments from shrouding the minimum payment amount, does not extrapolate to the field. My results show that the minimum payment amount acts as a relevant target for consumers and not as an irrelevant anchor. Consumers are often not at their borrowing constraints, however, consumers do frequently have low liquid cash balances leaving them limited cash to pay more. Targeting paying at least the minimum on their credit card appears to be how consumers manage their finances.

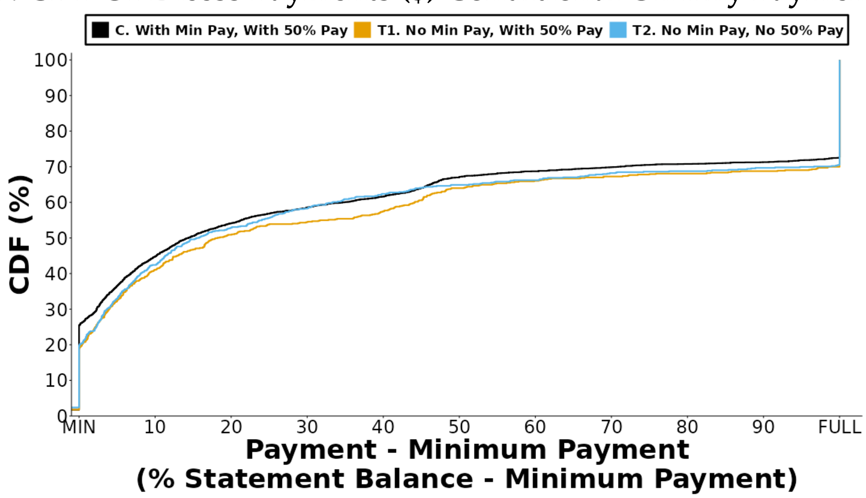
7 Figures & Tables

Figure 1: Distribution Of Credit Card Payments, After Three Statements

A. CDF Of Payments (% Statement Balance)

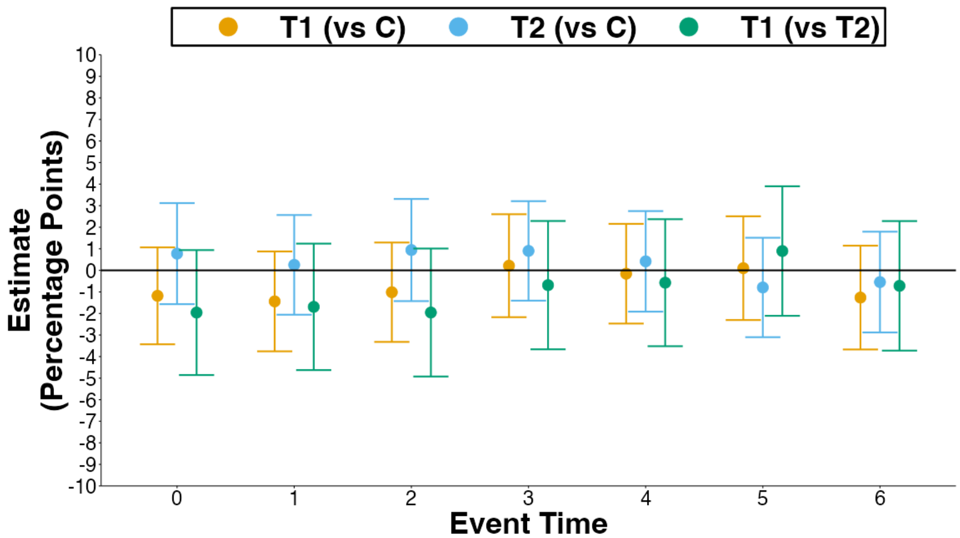


B. CDF Of Excess Payments ($) Conditional On Any Payment



Notes: These panels show the CDFs of payments. Panel A shows the CDF of payments as a percent of the statement balance. The dotted vertical line in Panel A denotes 50% of the statement balance amount. Panel B shows the CDF of excess payments, defined as the payments less the minimum payment (as a percent of the statement balance less minimum payment) conditional on consumers making any payment. In panel B, MIN denotes consumers that make exactly the minimum payment and FULL denotes those that pay their statement balance in full (or more). In both panels, each color shows the CDF for the control group (C) in black, treatment group T1 (that shrouds the minimum payment option) in orange, and treatment group T2 (that shrouds both the minimum payment option and also the 50% payment option) in blue. In both panels, the outcome is measured after three completed statement cycles. Panel A contains 6,693 consumers observed in cycle three. Panel B the subset of the consumers in Panel A that make any payment in the that third cycle, which is 4,592 consumers.

Figure 2: Dynamic Effects On Primary Outcome: Credit Card Payments (% Statement Balance)



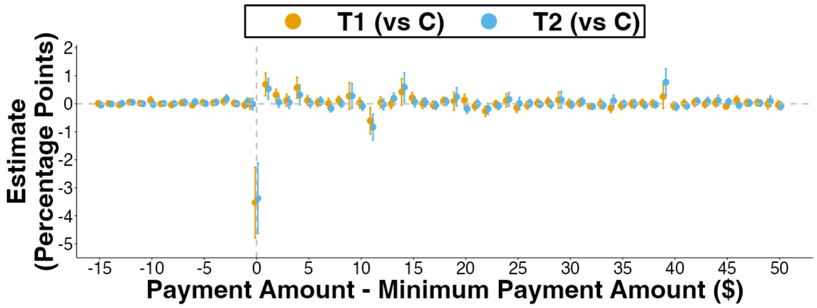
Notes: The outcome in this table is payments as a percent of the statement balance. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and group C not included in the regression. Both regressions also include time fixed effects for each statement end date and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). The regressions use data on 6,714 consumers and $N = 46{,}519$ *observations (statement cycles 0 to 6).*

Figure 3: Dynamic Effects On Secondary Outcomes

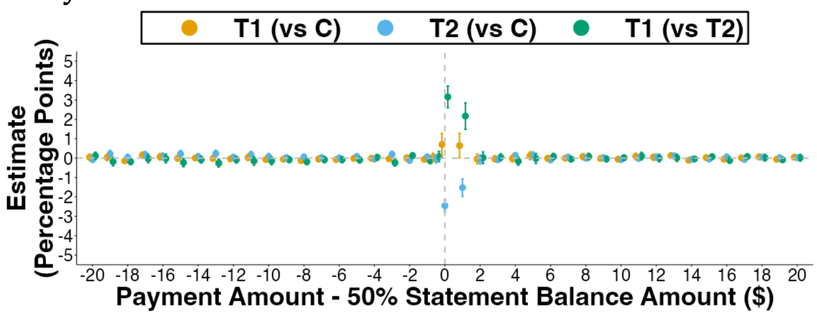
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. All regressions also include time fixed effects for each statement end date and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6).

Figure 4: Effects On Payments Proximate To Focal Amounts

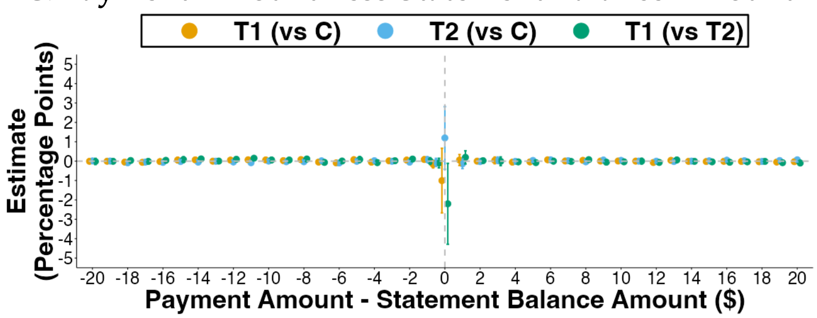
A. Payment Amount Less Minimum Payment Amount



B. Payment Amount Less 50% Statement Balance Amount

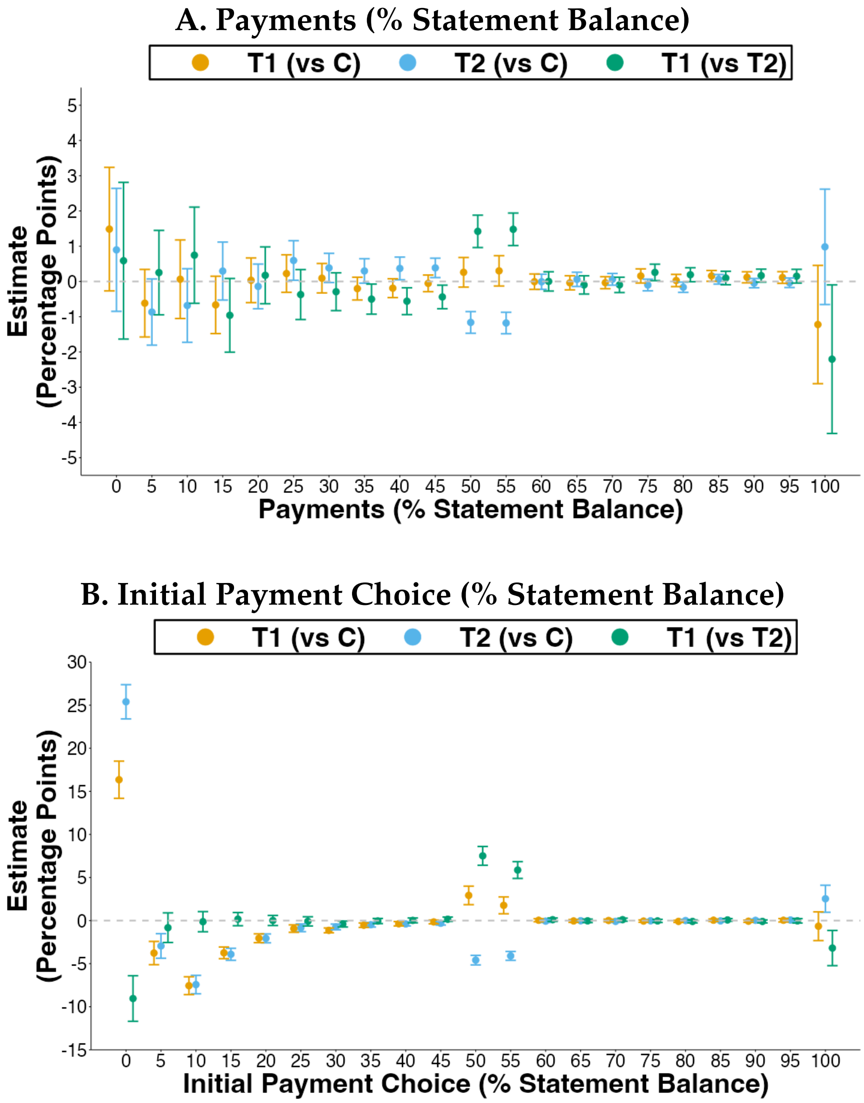


C. Payment Amount Less Statement Balance Amount



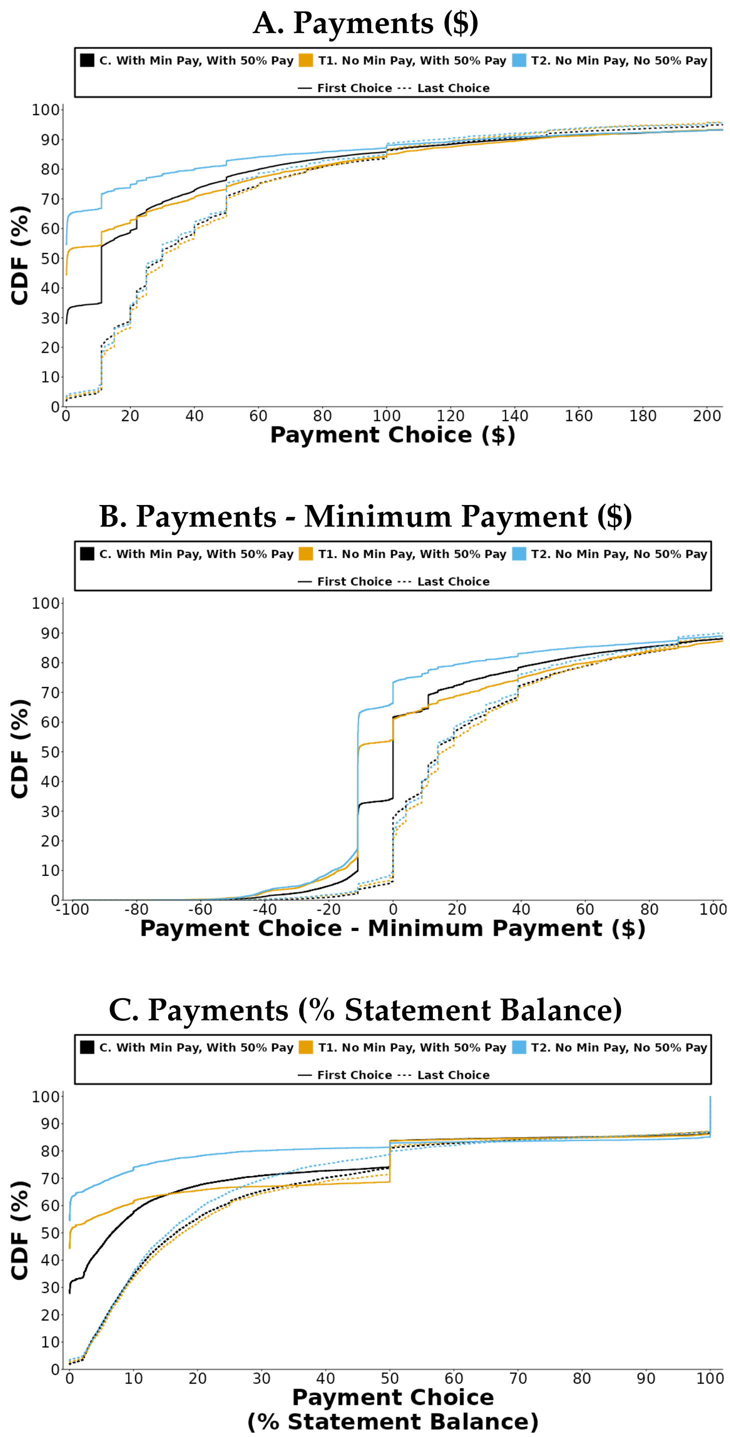
Notes: This figure shows the estimates on treatment indicators, showing the average effects pooled across statements, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions (green dots are excluded from Panel A to ease presentation since all of these estimates are insignificant from zero). All regressions also include time fixed effects for each statement end date and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6). Each panel shows results for a different measure. Within each panel, the estimates corresponding with each x-axis level show results for a different binary outcome that takes a value of one if a consumer paid that amount, and zero otherwise. The estimates corresponding to where there is the vertical dashed line at zero represent an outcome where a consumer pays exactly that amount. The other estimates are $1 intervals of payments.

Figure 5: Effects On Payment Distribution



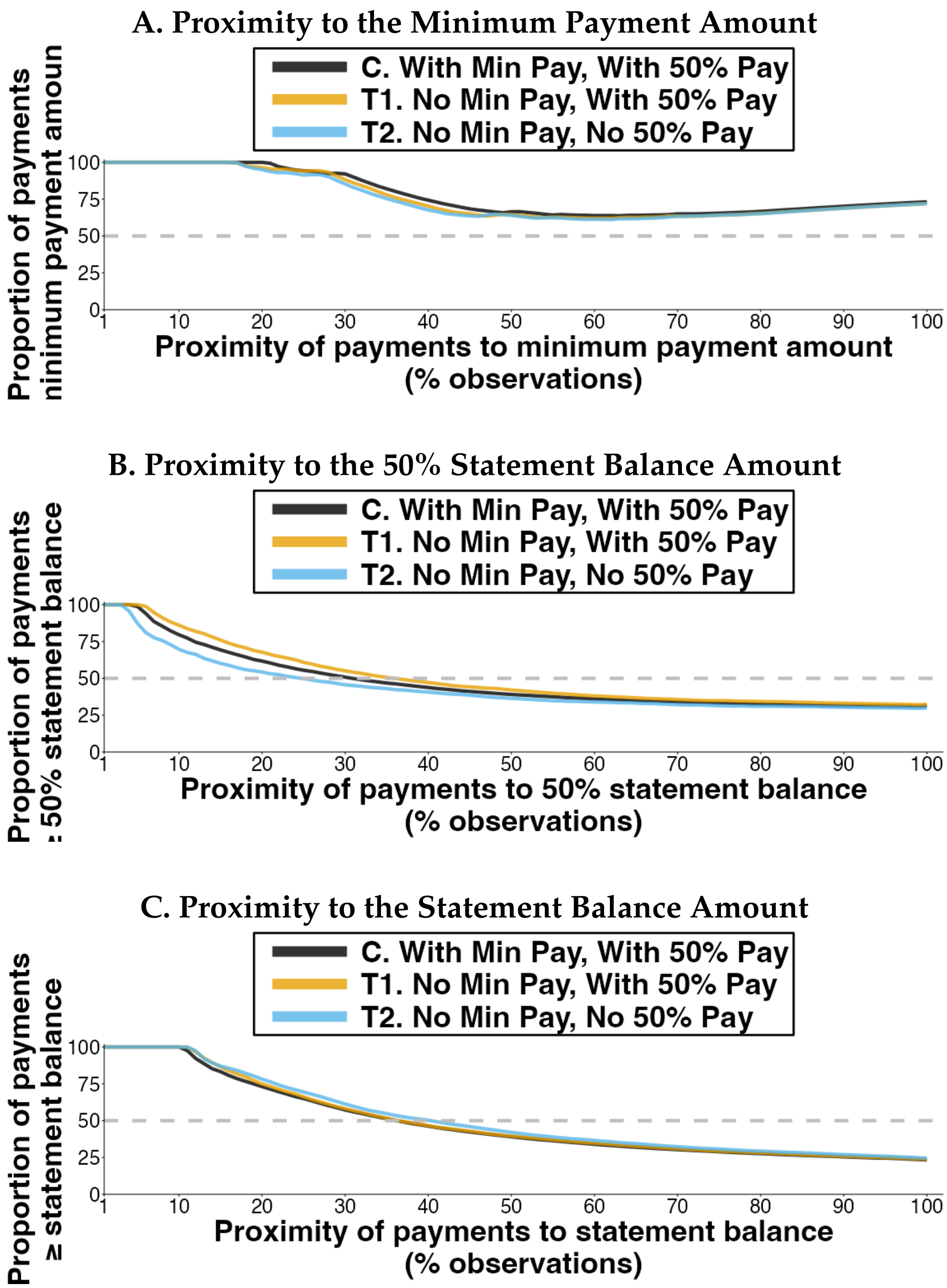
Notes: Both panels of this figure show the estimates on treatment indicators, showing the average effects pooled across statements, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. All regressions also include time fixed effects for each statement end date and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). The regressions in Panel A use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6) for consumers ultimate payments that cycle. The regressions in Panel B use data on 5,644 consumers and $N = 25{,}928$ observations, the initial payment choices of consumers in the app each cycle (statement cycles 0 to 6). The estimates for each x-axis level show results for a different binary outcome that takes a value of one if a consumer paid 5 percentage points up to that amount, and zero otherwise. The estimates corresponding to the x-axis value of zero are if a consumer made no payment, and those corresponding to the x-axis value of one are if a consumer paid in full.

Figure 6: CDFs Of First And Last Payment Choices



Notes: This includes consumers who make at least one payment and have a non-zero statement balance and non-zero minimum payment due in that cycle. This excludes blank choices and choices of zero dollars. Each panel shows CDFs calculated a different way, Panel A shows CDFs for payments in dollars, Panel B shows for excess payments (i.e., payments less the minimum payment), and Panel C shows for payments as a percentage of the statement balance. The colors show the CDFs for consumers in the control group, C in black, and the two treatment groups, T1 in orange and T2 in blue. The solid lines show the CDF of consumers' first payment choices in a payment cycle and the dashed lines show the CDF of consumers' last payment choices in a payment cycle. All CDFs use data pooled across cycles zero to six.

Figure 7: Value Proximity Analysis (VPA)



Notes: Each panel is calculated by collapsing the raw data for 6,714 consumers containing payments across all cycles from zero to six, $N = 46{,}519$ consumer cycles, to 300 observations. In each panel, there is one observation for each proximity window (denoted by the x-axis) that contains from 1% to 100% of observations in windows that incrementally include an additional one percentage point of observations, and this is calculated separately for each of the three groups: C, T1, and T2. The black lines denotes group C, the orange lines denotes group T1, and the blue lines denote group T2. In each panel, the y-axis shows for a given proximity window, the fraction of observations that are greater than or equal to a particular payment amount. Each panel shows the results for a different outcome payment amount. Panel A shows the fraction of observations that are greater than or equal to the minimum payment amount. Panel B shows the fraction of observations that are greater than or equal to 50% of the statement balance amount. Panel C shows the fraction of observations that are greater than or equal to the statement balance amount.

Table 1: Pre-Experiment Summary Statistics

	C Mean (1)	T1 Mean (2)	T2 Mean (3)	T1 - C (4)	T2 - C (5)
Age	45.51	45.68	45.84	0.17	0.33
				(0.38)	(0.39)
				[0.652]	[0.390]
Credit Score	561.70	561.06	562.10	-0.64	0.40
				(2.06)	(2.10)
				[0.757]	[0.847]
APR (%)	15.71	15.72	15.72	0	0
				(0.01)	(0.01)
				[0.826]	[0.85]
Tenure (days)	265.58	257.53	262.24	-8.05	-3.34
				(5.10)	(5.10)
				[0.114]	[0.512]
Autopayer (%)	41.19	43.59	39.69	2.39	-1.50
				(1.68)	(1.65)
				[0.154]	[0.362]
Payments (%)	45.06	48.00	44.34	2.95	-0.72
				(1.44)	(1.39)
				[0.040]	[0.608]
Payments	77.07	85.00	76.99	7.93	-0.08
				(4.21)	(3.79)
				[0.060]	[0.984]
Statement Balance	207.94	206.38	208.75	-1.55	0.82
				(7.57)	(7.41)
				[0.837]	[0.912]
Spending	98.13	99.81	101.3	1.68	3.17
				(4.67)	(4.82)
				[0.719]	[0.510]
Spending (%)	50.11	51.95	50.25	1.84	0.14
				(1.46)	(1.44)
				[0.207]	[0.925]
Revolving Debt	149.09	144.62	149.98	-4.47	0.89
				(7.15)	(6.98)
				[0.532]	[0.898]
Credit Limit	292.09	287.64	294.11	-4.45	2.02
				(8.16)	(8.13)
				[0.585]	[0.803]
Miss Pay (%)	17.00	14.59	15.88	-2.41	-1.12
				(1.21)	(1.24)
				[0.047]	[0.365]
Pay Min (%)	17.48	17.38	16.79	-0.10	-0.69
				(1.29)	(1.26)
				[0.937]	[0.583]
Pay 50% (%)	3.74	3.07	4.20	-0.68	0.45
				(0.60)	(0.67)
				[0.257]	[0.497]
Pay Full (%)	31.73	34.39	30.02	2.66	-1.71
				(1.61)	(1.55)
				[0.098]	[0.270]
Pay Current Balance (%)	9.25	11.15	9.67	1.91	0.42
				(1.05)	(0.99)
				[0.070]	[0.668]

Notes: Columns (1), (2), and (3) respectively show the means for the control group C of 4,542 consumers, the Treatment 1 (T1) group of 1,096 consumers, and the Treatment 2 (T2) group of 1,076 consumers. Column (4) is the result of t-tests, showing the difference in the means between T1 and C, with standard errors in the row below in (.) and p-values in the row below that in [.]. Similarly, column (5) is the result of t-tests, showing the difference in the means between T2 and C, with standard errors in the row below in (.) and p-values in the row below that in [.]. Data for these calculations uses the last statement cycle before the start of the experiment. The units shown for the means and standard errors are in dollars, except for credit score (points), card tenure (days), and for outcomes with (%) are in percentage points.

Table 2: T-Tests, After Three Statements

	C Mean (1)	T1 Mean (2)	T2 Mean (3)	T1 - C (4)	T2 - C (5)	T1 - T2 (6)
Payments (%)	32.92	34.4	33.83	1.48	0.92	0.57
				(1.39)	(1.37)	(1.76)
				[0.285]	[0.503]	[0.747]
Payments	59.01	63.18	55.75	4.16	-3.26	7.42
				(4.00)	(3.61)	(4.81)
				[0.297]	[0.367]	[0.123]
Statement Balance	231.01	233.85	225.52	2.84	-5.49	8.33
				(7.93)	(7.65)	(9.89)
				[0.720]	[0.473]	[0.400]
Spending	71.94	73.64	67.50	1.70	-4.43	6.14
				(4.54)	(4.27)	(5.57)
				[0.708]	[0.299]	[0.271]
Spending (%)	36.03	38.04	36.55	2.01	0.52	1.49
				(1.43)	(1.42)	(1.82)
				[0.158]	[0.714]	[0.412]
Revolving Debt	187.21	187.88	182.58	0.67	-4.63	5.30
				(7.70)	(7.48)	(9.65)
				[0.931]	[0.536]	[0.583]
Credit Limit	320.21	319.02	321.35	-1.20	1.14	-2.34
				(8.75)	(8.64)	(11.02)
				[0.891]	[0.895]	[0.832]
Miss Pay (%)	28.82	29.58	29.09	0.76	0.28	0.49
				(1.55)	(1.53)	(1.96)
				[0.622]	[0.857]	[0.803]
Pay Min (%)	16.20	11.63	11.62	-4.57	-4.58	0.01
				(1.12)	(1.11)	(1.38)
				[0]	[0]	[0.995]
Pay 50% (%)	2.72	3.35	0	0.63	-2.72	3.35
				(0.60)	(0.24)	(0.55)
				[0.293]	[0]	[0]
Pay Full (%)	22.76	24.19	24.25	1.42	1.48	-0.06
				(1.45)	(1.44)	(1.84)
				[0.326]	[0.303]	[0.974]
Pay Current Balance (%)	5.90	5.86	6.50	-0.04	0.60	-0.64
				(0.80)	(0.82)	(1.03)
				[0.960]	[0.470]	[0.539]

Notes: This table uses data for the consumers that are observed after three completed statement cycles. Columns (1), (2), and (3) respectively show the means for the control group C of 4,525 consumers, the Treatment 1 (T1) group of 1,093 consumers, and the Treatment 2 (T2) group of 1,075 consumers. Column (4) is the result of t-tests, showing the difference in the means between T1 and C, i.e., the effect of shrouding the minimum payment option while retaining the 50% payment option, with standard errors in the row below in (.) and p-values in the row below that in [.]. Similarly, column (5) is the result of t-tests, showing the difference in the means between T2 and C, i.e., the effect of shrouding both the minimum payment option and the 50% payment option, with standard errors in the row below in (.) and p-values in the row below that in [.]. Column (6) is the result of t-tests, showing the difference in the means between T1 and T2, i.e., the effect of displaying a 50% payment option, with standard errors in the row below in (.) and p-values in the row below that in [.]. The units shown for the means and standard errors are in dollars, or for outcomes with (%) are in percentage points.

Table 3: Regression Estimates On Payments (% Statement Balances)

A: Dynamic Estimates After Three and Six Statements

	Dynamic (δ_3^k)				Dynamic (δ_6^k)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1 (vs C)	-0.21	1.47	0.22	-1.55	-7.09	0.01	-1.26	-2.83
	(1.33)	(1.39)	(1.22)	(1.57)	(1.26)	(1.30)	(1.23)	(1.62)
T2 (vs C)	-0.78	1.00	0.90	1.68	-7.42	-0.38	-0.54	0.10
	(1.31)	(1.37)	(1.18)	(1.44)	(1.24)	(1.29)	(1.19)	(1.56)
T1 (vs T2)	0.57	0.47	-0.69	-3.23	0.33	0.40	-0.72	-2.93
	(1.75)	(1.75)	(1.52)	(1.90)	(1.64)	(1.64)	(1.53)	(2.01)
Time F.E.		X	X	X		X	X	X
Consumer Controls			X				X	
Consumer F.E.				X				X

B: Pooled Estimates Across Statements

	Pooled (δ^k)			
	(1)	(2)	(3)	(4)
T1 (vs C)	0.49	0.55	-0.67	-2.39
	(1.06)	(1.06)	(0.81)	(1.23)
T2 (vs C)	0.35	0.37	0.29	1.04
	(1.05)	(1.05)	(0.80)	(1.16)
T1 (vs T2)	0.14	0.18	-0.96	-3.44
	(1.35)	(1.35)	(1.02)	(1.51)
Time F.E.		X	X	X
Consumer Controls			X	
Consumer F.E.				X

Notes: The first four columns of Panel A of this table shows the estimates from interactions between treatment indicators and an indicator for the third statement cycle in the experiment, and the last four columns of Panel A show this for the interaction with an indicator for the sixth statement cycle. All of the Panel A regressions also include interactions between treatments and the other statement cycles. All columns of Panel B only include treatment indicators, showing the average effects pooled across statement cycles 0 to 6. All regressions in both panels use the same outcome, payments as a percent of the statement balance, and the units shown are percentage points. All columns except Panel A columns (1) and (5), and Panel B column (1) include time fixed effects for each statement end date in the regressions. Panel A columns (3) and (7), and Panel B column (3) include in the regressions a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). Panel A columns (4) and (8), and Panel B column (4) include an extra observation per consumer for the statement cycle before the experiment to enable including consumer fixed effects in the regressions. In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. Rows T1 (vs C) and T2 (vs C) are from regressions where the control group (C) is the omitted group. The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6), except for the models with consumer fixed effects that contain $N = 53{,}233$ observations (statement cycles -1 to 6).

Table 4: Regression Estimates On Secondary Outcomes, After Three Statements

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)
Payments	1.33	-3.48	4.81
	(3.37)	(3.08)	(4.08)
Statement Balance	5.10	-4.48	9.57
	(3.65)	(3.38)	(4.48)
Spending	-2.69	-4.57	1.88
	(3.68)	(3.60)	(4.59)
Spending (%)	0.09	0.39	-0.31
	(1.20)	(1.19)	(1.51)
Revolving Debt	3.79	-3.25	7.03
	(4.03)	(3.70)	(4.90)
Credit Limit	0.39	-0.50	0.89
	(2.20)	(2.05)	(2.75)
Miss Pay (%)	1.42	0.45	0.98
	(1.47)	(1.44)	(1.85)
Pay Min (%)	-4.17	-3.93	-0.24
	(1.04)	(1.06)	(1.3)
Pay 50% (%)	0.71	-2.88	3.59
	(0.60)	(0.26)	(0.56)
Pay Full (%)	0.02	1.66	-1.64
	(1.31)	(1.26)	(1.63)
Pay Current Balance (%)	-0.32	0.60	-0.92
	(0.78)	(0.80)	(1.01)
Time F.E.	X	X	X
Consumer Controls	X	X	X

Notes: Each row of this table shows estimates for a different outcome. All of the estimates are from interactions between treatment indicators and an indicator for the third statement cycle in the experiment. All of these regressions also include interactions between treatments and the other statement cycles. All regressions include time fixed effects for each statement end date in the regressions and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6). The units shown are in dollars, or for outcomes with (%) are in percentage points.

Table 5: Regression Estimates On Initial Payment Choices

A: Initial Payment Choices

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)
Initial Payment Choice (% Statement Balance)	-0.90	-4.33	3.43
	(0.87)	(0.81)	(1.07)
Initial Payment Choice Less Than Minimum Payment	19.44	30.50	-11.06
	(1.16)	(1.02)	(1.38)
Number of Payment Choices	0.40	0.85	-0.46
	(0.11)	(0.11)	(0.14)
Time F.E.	X	X	X
Consumer Controls	X	X	X

B: Initial Payment Choices, Excluding Blank Choices And Choices Of Zero Dollars

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)
Initial Payment Choice (% Statement Balance)	-0.22	-1.82	1.60
	(0.86)	(0.83)	(1.07)
Initial Payment Choice Less Than Minimum Payment	15.59	23.57	-7.98
	(1.08)	(1.04)	(1.38)
Number of Payment Choices	0.40	0.87	-0.47
	(0.11)	(0.11)	(0.14)
Time F.E.	X	X	X
Consumer Controls	X	X	X

Notes: Each row of this table shows estimates for a different outcome. All of the estimates are from treatment indicators, showing the average effects pooled across statement cycles 0 to 6. All regressions include time fixed effects for each statement end date in the regressions and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions in Panel A use data on 5,644 consumers and $N = 25{,}928$ *observations, the initial payment choices of consumers in the app each cycle (statement cycles 0 to 6). The regressions in Panel B use data on 5,635 consumers and* $N = 25{,}820$ *observations, the initial payment choices of consumers in the app each cycle (statement cycles 0 to 6) after excluding blank choices or choices of zero dollars. The units shown are in percentage points, except for the number of payment choices which shows the total number of payment choices by a consumer during a cycle.*

Table 6: Heterogeneous Treatment Effects On Payments (%) By Pre-Trial Liquid Cash Balances, Pooled Across Statements

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Consumers
Liquid Cash <$1	1.32	1.01	0.31	1,025
	(2.28)	(2.22)	(2.97)	
Liquid Cash $1+	-1.36	0.20	-1.56	5,689
	(0.88)	(0.86)	(1.10)	
Liquid Cash <$501	-0.46	0.70	-1.16	3,796
	(1.06)	(1.06)	(1.34)	
Liquid Cash $501+	-0.80	-0.45	-0.35	2,918
	(1.3)	(1.23)	(1.60)	
Liquid Cash <$1,001	-1.11	0.42	-1.53	4,726
	(0.95)	(0.95)	(1.21)	
Liquid Cash $1,001+	0.25	-0.22	0.47	1,988
	(1.58)	(1.50)	(1.96)	
Minimum Liquid Cash <$1	-1.47	0.68	-2.15	4,230
	(1.02)	(1.03)	(1.31)	
Minimum Liquid Cash $1+	-0.25	-0.15	-0.09	2,484
	(1.37)	(1.33)	(1.70)	
Minimum Liquid Cash <$501	-0.88	0.61	-1.49	6,252
	(0.83)	(0.83)	(1.06)	
Minimum Liquid Cash $501+	1.50	0.43	1.07	462
	(3.33)	(3.34)	(4.07)	
Minimum Liquid Cash <$1,001	-0.95	0.21	-1.16	6,422
	(0.82)	(0.82)	(1.04)	
Minimum Liquid Cash $1,001+	1.77	5.87	-4.10	292
	(4.46)	(4.55)	(5.77)	
Time F.E.	X	X	X	
Consumer Controls	X	X	X	

Notes: This table shows the estimates from treatment indicators, showing the heterogeneous effects, in percentage points, pooled across statement cycles 0 to 6. Each row of this table shows estimates for a different subsample, with the fourth column showing the number of consumers in each subsample. The rows starting with the prefix 'Liquid Cash' segment consumers by their liquid cash balance the day before the experiment went into the field, for example 'Liquid Cash ¡$1' denotes consumes who had a liquid cash balance of less than $1 at this point-in-time, and 'Liquid Cash $1+' is the remaining consumers who had a balance of $1 or more. The rows starting with the prefix 'Minimum Liquid Cash' segment consumers by their minimum liquid cash balance in the 90 days up to the day before the experiment went into the field, for example 'Minimum Liquid Cash ¡$1' denotes consumes who had a minimum liquid cash balance of less than $1 at any point in those 90 days, and 'Minimum Liquid Cash $1+' is the remaining consumers who had a minimum balance of $1 or more. For consumers who opened credit cards less than 90 days before the start of the experiment, we calculate this using 60 or 30 days, as available. The outcome in all regressions is the primary outcome: payments (% statement balance). All regressions include time fixed effects for each statement end date and a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis.

Table 7: Value Proximity Analysis (VPA) Regression Estimates

	Minimum Payment	50% Statement Balance	Statement Balance
	(1)	(2)	(3)
Intercept	98.510	76.831	89.091
	(1.411)	(2.718)	(2.308)
T1	−1.403	5.290	1.380
	(2.121)	(3.645)	(3.244)
T2	−2.840	−7.568	3.377
	(2.212)	(3.986)	(3.052)
Proximity	−0.406	−0.584	−0.798
	(0.028)	(0.042)	(0.037)
Proximity x T1	−0.005	−0.044	−0.012
	(0.041)	(0.057)	(0.052)
Proximity x T2	0.008	0.078	−0.016
	(0.042)	(0.062)	(0.050)
Observations	300	300	300
Adjusted R^2	0.647	0.773	0.873

Notes: Each column shows the estimates from a separate OLS regression examining a different outcome. Column (1) shows the fraction of observations that are greater than or equal to the minimum payment amount. Column (2) shows the fraction of observations that are greater than or equal to 50% of the statement balance amount. Column (3) shows the fraction of observations that are greater than or equal to the statement balance amount. The units are percentage points. Each regression is run by collapsing the raw data for 6,714 consumers containing payments across all cycles from zero to six, $N = 46{,}519$ consumer cycles, to 300 observations. There is one observation for each proximity window that contains from 1% to 100% of observations in windows that incrementally include an additional one percentage point of observations, and this is calculated separately for each of the three groups: C, T1, and T2. Each regression includes an intercept, indicators for each of T1 and T2, a linear term for the Proximity, and interactions between the Proximity term and the T1 indicator and also separately with the T2 indicator. Heteroskedastic-robust standard errors are in parenthesis.

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Supplemental Appendix Accompanying *"Targeting Higher Credit Card Payments"*

A. Pre-Analysis Plan (March 20, 2025)

A1. Research Question

Does nudging credit card payment options increase credit card payments? A large body of lab experimental evidence exists showing that a nudge shrouding the credit card minimum payment option increases hypothetical manual payments.[16] This new field experiment provides an important test of whether this finding extrapolates to the field. I test two nudges that help to understand the credit card payment amounts that consumers target. Prior empirical studies have shown, across countries, other efforts to nudge consumers to reduce their debt have been ineffective.[17]

A2. Experimental Sample

The experiment studies a card lender's customers in the USA. Consumers can only hold one credit card with the lender, so therefore studying results at the credit card account-level also correspond to the consumer-level. The sample includes consumers who have had at least one statement issued, a credit limit of over $40, a non-zero statement balance and have never been over 35 days past due. We also exclude 2,337 consumers who were incorrectly randomized to treatments in December 2024. At least 1,100 consumers are randomly selected for each of the two treatment groups. The control group consists of the remaining consumers and will contain at least 1,100 consumers.

A3. Experimental Design

The experiment varies the presentation of the credit card manual payment options a consumer is presented with on the lender's mobile app. This experiment was approved by

[16] Stewart (2009), Navarro-Martinez et al. (2011), Jiang and Dunn (2013), Salisbury (2014), Salisbury and Zhao (2020), Hendy et al. (2021), Sakaguchi et al. (2022), Guttman-Kenney et al. (2023). Empirical evidence from historical data consistent with "anchoring" or "targeting" to the minimum payment has also been shown in Keys and Wang (2019), Medina and Negrin (2022), and Vihriälä (2025). Thaler and Sunstein (2008) write a credit card's minimum payment "can serve as an anchor, and as a nudge that this minimum payment is an appropriate amount".

[17] Agarwal et al. (2015), Seira et al. (2017), Keys and Wang (2019), Adams et al. (2022).

Rice University's Institutional Review Board (IRB-FY2024-399 on 21st June 2024). The sample will be randomly split across three groups:

1. 68% of consumers (4,674) are in the control group (C). This has these payment options: current balance, statement balance, 50% of statement balance, minimum payment, and a free text box to choose any payment amount. These are the status quo options customers experience in the absence of the experiment.

2. 16% of consumers (1,103) are in Treatment 1 (T1). This does not show consumers the minimum payment option. It has these payment options: current balance, statement balance, 50% of statement balance, and a free text box to choose any payment amount.

3. 16% of consumers (1,128) are in Treatment 2 (T2). This does not show consumers the minimum payment option and it also does not show consumers the 50% of statement balance option. It has these payment options: current balance, statement balance, and a free text box to choose any payment amount.

Importantly, in all the groups (control, T1, and T2), if a consumer attempts to pay an amount less than the minimum payment then a warning comes up showing the minimum payment amount. This enables the consumer to revise their initial payment choice to prevent the consumer accidentally paying less than the minimum. Sakaguchi et al. (2022) shows in a lab experiment how such prompts appear effective at preventing such choices.

The motivation behind T1 is that the minimum payment amount acts as a psychological default (see cites in the preceding footnote), potentially reducing some consumer payment choices to exactly this amount or amounts just above it. By shrouding this option, it may increase payments by increasing the salience of the statement balance for consumers to use as a target and enables consumers to make an active choice without being distorted by the presence of the minimum.[18]

The motivation behind T2 is that the 50% of statement balance option can also act as a target, potentially reducing some consumer payment choices to exactly 50%, or amounts close to it, that would otherwise be higher. Prior research has indicated more payment options may unintentionally reduce payments for some consumers.[19] By shrouding the 50% option, along with the minimum payment option, it may increase payments by increasing the salience of the statement balance for consumers to use as a target (see cites in the preceding footnote).

[18] Bartels et al. (2024) and Schwartz (2025).

[19] Agarwal et al. (2015), Hershfield and Roese (2015), and Keys and Wang (2016).

Comparing C to T1 shows the effect of shrouding the minimum payment option, to target the higher payment options. Comparing C to T2 shows the effect of shrouding both the minimum payment and 50% of statement balance options, to target the statement balance as a higher payment option. Comparing T1 to T2 shows the marginal effect of including the 50% of statement balance option.

A4. Data

Outcomes are evaluated using the credit card lender's anonymized administrative data. The includes data on each credit card statement and each credit card payment a consumer makes.

Data Preparation

I will winsorize balances, debt, payments, and spending variables, when measured in dollars, at their 99th percentiles. This is done as these variables are highly non-normal in their distributions, with a mass at zero and fat right tails to their distributions. I will check for balance between the control and treatment groups using covariates observed before the experiment's start to ensure the randomization was successfully implemented and report t-statistics comparing differences in these. I do not expect the treatments to affect card closure rates. However, if they did it affects how to deal with missing data. Therefore, I plan to examine card closing rates. Assuming there is no significant difference that is economically meaningful, more than 10%, I proceed with studying outcomes across observed card payment cycles.

A5. Experiment Dates

The experiment was planned to start from 11th September 2024 but, due to operational considerations at the lender it was delayed and was scheduled to go live on 12th December 2024, however, the randomization was incorrectly done between treatments and therefore the experiment is scheduled to re-launch after correct randomization in late March 2025. The experiment is planned to end after at least three completed credit card payment cycles. Payments against the third credit card statement are expected to be due by July 2025.

I intend to report outcomes using data after at least three completed statement cycles. If after three completed statement cycles, the experiment is having a positive impact on consumer outcomes, it will be continued for another three completed statement cycles, and primary results will be evaluated after six completed statement cycles. If the experiment is having no effect on consumer outcomes after three completed statement cycles, the experiment will end with primary outcomes evaluated at this time. If there is a severe negative impact, the lender may stop the experiment prior to three completed statement cycles.

A6. Experimental Analysis

Descriptive Analysis

I will show the cumulative distribution function (CDF) of payments for control and treatment groups. This is designed to examine how the nudge affects the distribution of payments, and whether it changes the bunching of payments around the minimum and the 50% statement balance options, as would be consistent with consumers targeting these payment amounts.

1. Distribution of payments (% statement balance).

2. Distribution of excess payments (%). Excess payments are calculated by dividing payments by balances, both excess of the minimum payment amount. The numerator is payments less the minimum payment amount, and the denominator is statement balance less the minimum payment amount.

Significance Testing

I will primarily use t-tests to evaluate significance, using the latest payment cycle of data (at least 3 payment cycles) to evaluate the effects of the nudges. I will use 5% as a threshold for statistical significance. I will also report Minimum Detectable Effects (MDE) for primary outcomes assuming 80%, with significance thresholds of 0.5%, 1%, and 5% (5% is the threshold for significance used for analysis).

Regression Specification

To understand the dynamics of the treatment effects, I will use the dynamic regression specification shown below that uses data across all completed statement cycles during the experiment:

$$Y_{i,t} = \alpha + \sum_{\tau} \left[\delta_{\tau}^{T1} \left(D_{\tau} \times T1_{i} \right) + \delta_{\tau}^{T2} \left(D_{\tau} \times T2_{i} \right) \right] + \varepsilon_{i,t}$$

In this regression, $Y_{i,t}$ denotes outcome Y for consumer i at time t, and $\varepsilon_{i,t}$ is the error term. The term α is a constant. The term $T1_i$ is an indicator for Treatment 1 ($T1$) and the term $T2_i$ is an indicator for Treatment 2 ($T2$). The term D_τ is an indicator for time τ. I focus on the coefficients δ_{τ}^{T1} and δ_{τ}^{T2} from the latest period observed, but will also show results from earlier periods to enable readers to see the dynamics of the effects, and will also show results from a static regression that pools data across cycles as a robustness exercise. Standard errors are clustered at consumer level in both regressions.

$$Y_{i,t} = \alpha + \delta^{T1} T1_i + \delta^{T2} T2_i + \varepsilon_{i,t}$$

A7. Experimental Outcomes

Primary Outcome

1. Payment (%): Continuous outcome defined as the sum of payments ($) divided by the statement balance ($). Payment (%) is given a value of one if statement balance is zero. If Payment (%) is greater than one, it is winsorized at a value of one.

Secondary Outcomes

2. Payments ($): Continuous outcome.
3. Statement Balance ($): Continuous outcome.
4. Spending ($): Continuous outcome defined as the sum of purchases ($).
5. Spending (%): Continuous outcome defined as the sum of purchases ($) divided by the statement balance ($). Spending (%) is given a value of one if statement balance is zero. If Spending (%) is greater than one, it is winsorized at a value of one.
6. Debt ($): Continuous outcome measuring revolving credit card debt. This is defined as the statement balance ($) less payments ($). Debt is given a value of zero if payments exceed the statement balance.
7. Credit Limit ($): Integer outcome.
8. Miss Pay: Binary outcome. Takes a value of one if the consumer's payments are zero or less than the minimum payment (and the minimum payment is non-zero, and the payments are less than the statement balance).
9. Pay Min: Binary outcome. Takes a value of one if the consumer's payments are only exactly the minimum payment (and the minimum payment is non-zero, and the payments are less than the statement balance).
10. Pay 50%: Binary outcome. Takes a value of one if the consumer's payments are only exactly the 50% of the statement balance (and the minimum payment is non-zero).

11. Pay Full: Binary outcome. Takes a value of one if the consumer's payments are for the full statement balance or more (or if a zero minimum payment is due, including cases of zero statement balances).

12. Pay Current: Binary outcome. Takes a value of one if the consumer's payments are for the current balance or more (if current balance is non-zero).

Power Calculations

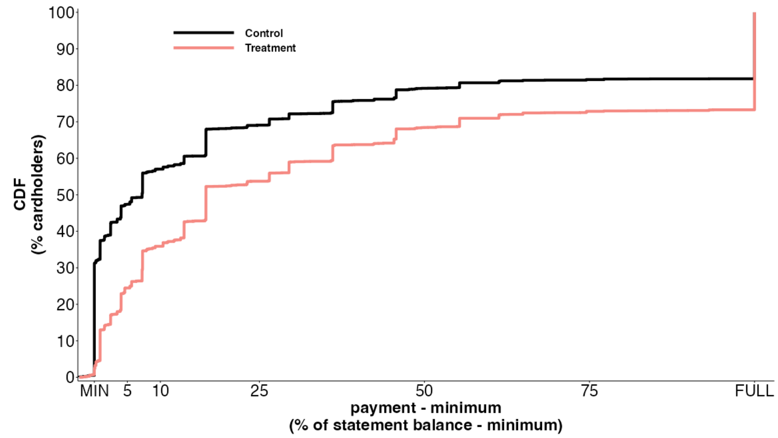
Based on data from June 2024, a sample of 2,200 consumers evenly split between a control and a treatment group would have sufficient power to detect a change in payments of 0.045, given a baseline mean of 0.235 and a standard deviation of 0.372. It would have sufficient power to detect a change in revolving debt of $26, given a baseline mean of $214 and a standard deviation of $221. This appears sufficiently powerful given the effect sizes of nudges shrouding minimum payments that I have previously tested in lab experiments on hypothetical credit card payment scenarios. In Guttman-Kenney (2024), the nudge has a 0.124 increase in payments (% statement balance) on a mean of 0.378. In Sakaguchi et al. (2022), the nudge has a 0.127 increase in payments (% statement balance) on a mean of 0.385.

Consumer Credit Reporting Data

Ideally, I would want to examine the effects of the treatments across the portfolio of credit cards held by a consumer. This would enable me to evaluate whether effects such as increasing payments on the credit card in the experiment are offset by consumers reducing payments on other credit cards. See Guttman-Kenney et al. (2025) for an example of such an approach when studying a different nudge treatment using UK consumer credit reporting data. Unfortunately, as documented in Guttman-Kenney and Shahidinejad (2025), consumer credit reporting data (reviewed in Gibbs et al., 2025) in the USA does not include credit card payments information for any of the six largest US credit card lenders, and only 24% of credit cardholders have this information across all their cards held. This means researchers cannot accurately distinguish between revolving debt and new credit card spending. Another issue is, even if consumer credit reporting data included credit card payments data, it would be challenging to have sufficient statistical power to detect offsetting effects. This is because the credit card lender's business model offers low credit limit cards, and the size of payment amounts may be too small to precisely estimate whether higher payments on the card in the experiment have been offset by lower payments on other cards held.

B. Supplementary Figures And Tables

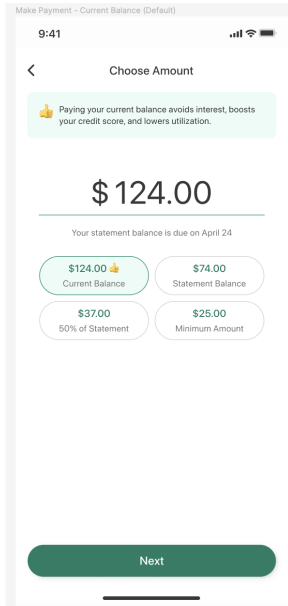
Figure A1: Example Of Effects Of Shrouding The Minimum Payment In A Hypothetical Credit Card Payment Experiment



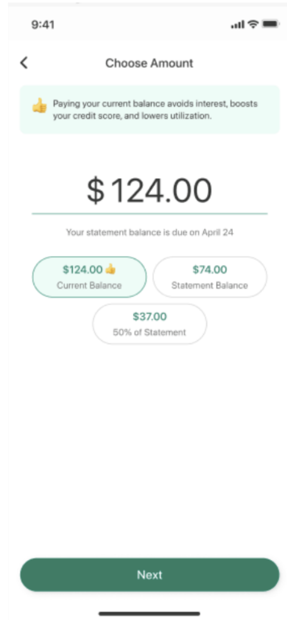
Notes: This figure is reprinted from Guttman-Kenney (2024). This figure shows the results of an online experiment testing the effect of shrouding the minimum payment amount on a sample of $N = 7,938$ consumers who have credit cards in the U.K. The figure displays CDFs of the payment amount less the minimum payment amount (as a percent of statement balance less than the minimum payment amount). Payments of exactly the minimum are denoted by 'MIN' and payments of the full amount are denoted by 'FULL'. The black line is the control group where the minimum payment amount is displayed. The orange line is the treatment group where the minimum payment amount is shrouded. The average effect on payments is to significantly increase average hypothetical credit card payments as a percent of the statement balance by 12.4 (s.e. 0.8) percentage points.

Figure A2: Credit Card Payment App Choice Architectures

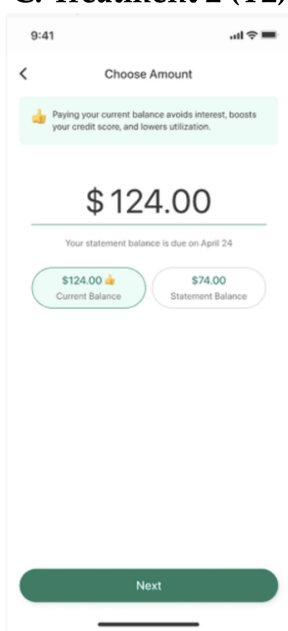
A. Control (C)



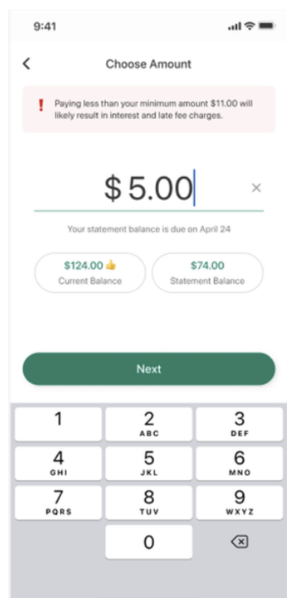
B. Treatment 1 (T1)



C. Treatment 2 (T2)



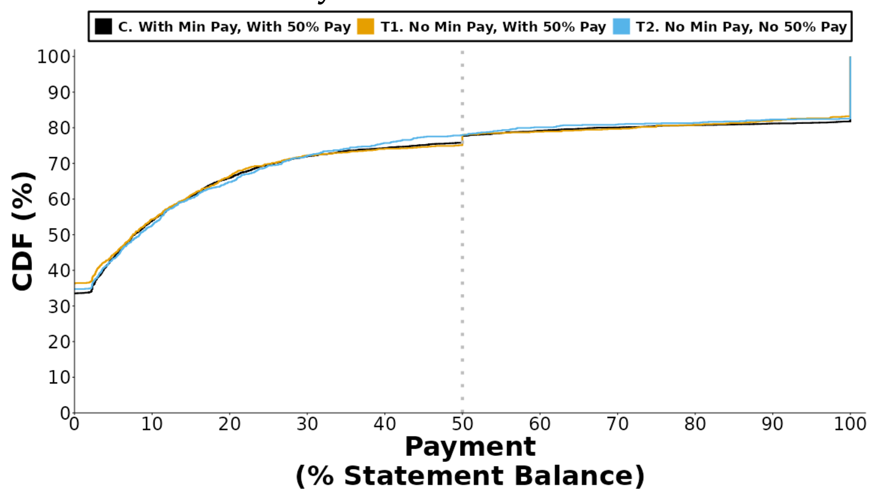
D. Prompt



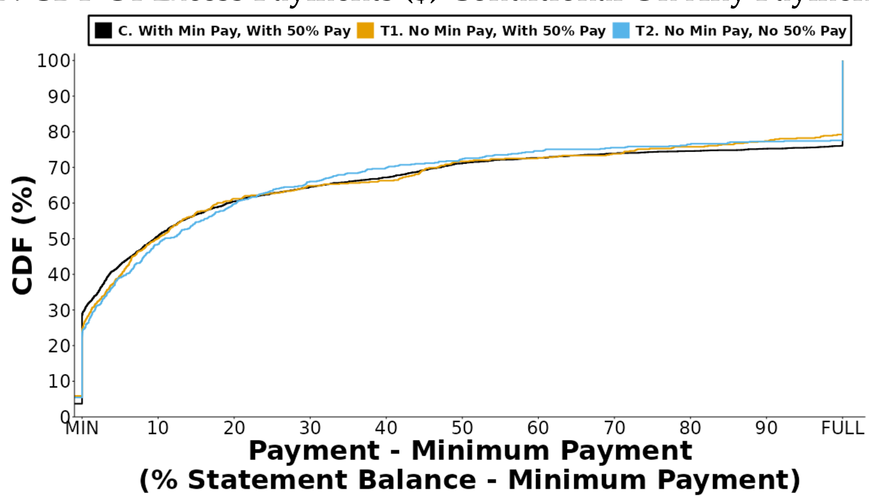
Notes: The minimum payment warning prompt in Panel D only appears if a consumer enters an amount less than the minimum payment amount. Panel D displays this warning prompt for the Treatment 2 (T2) payment options, such a warning prompt also appears for the Control (C) group and for the Treatment 1 (T1) group.

Figure A3: Distribution Of Credit Card Payments, After Six Statements

A. CDF Of Payments (% Statement Balance)

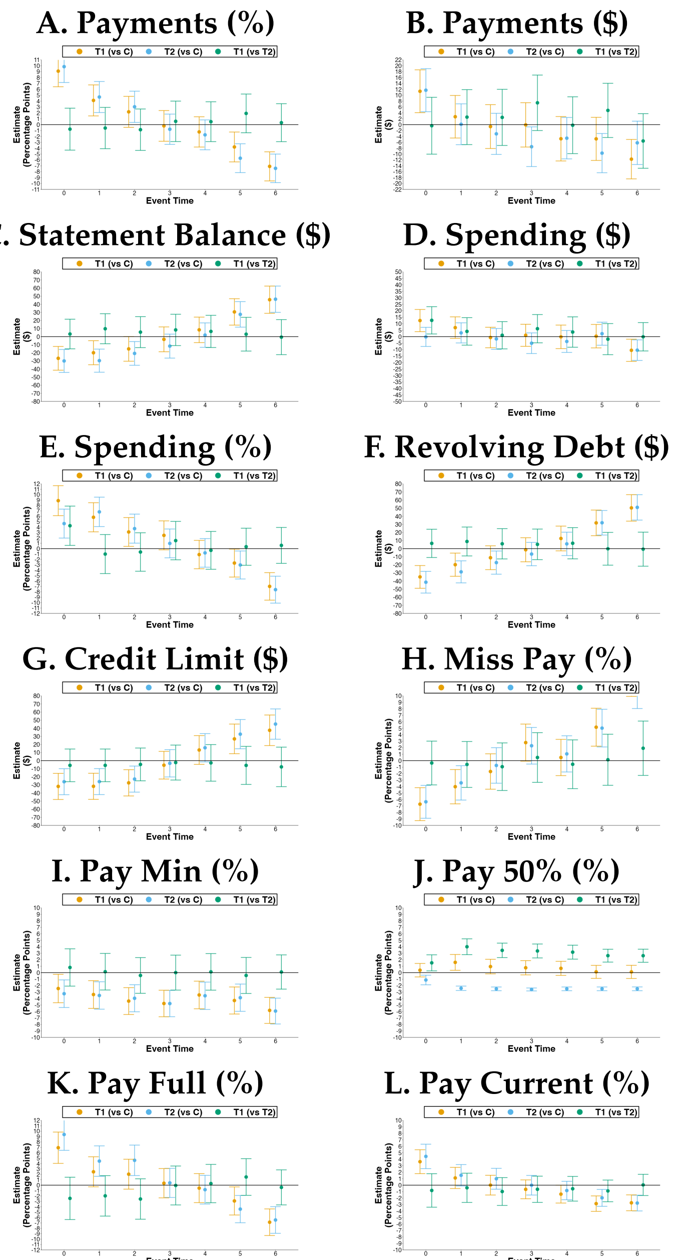


B. CDF Of Excess Payments ($) Conditional On Any Payment



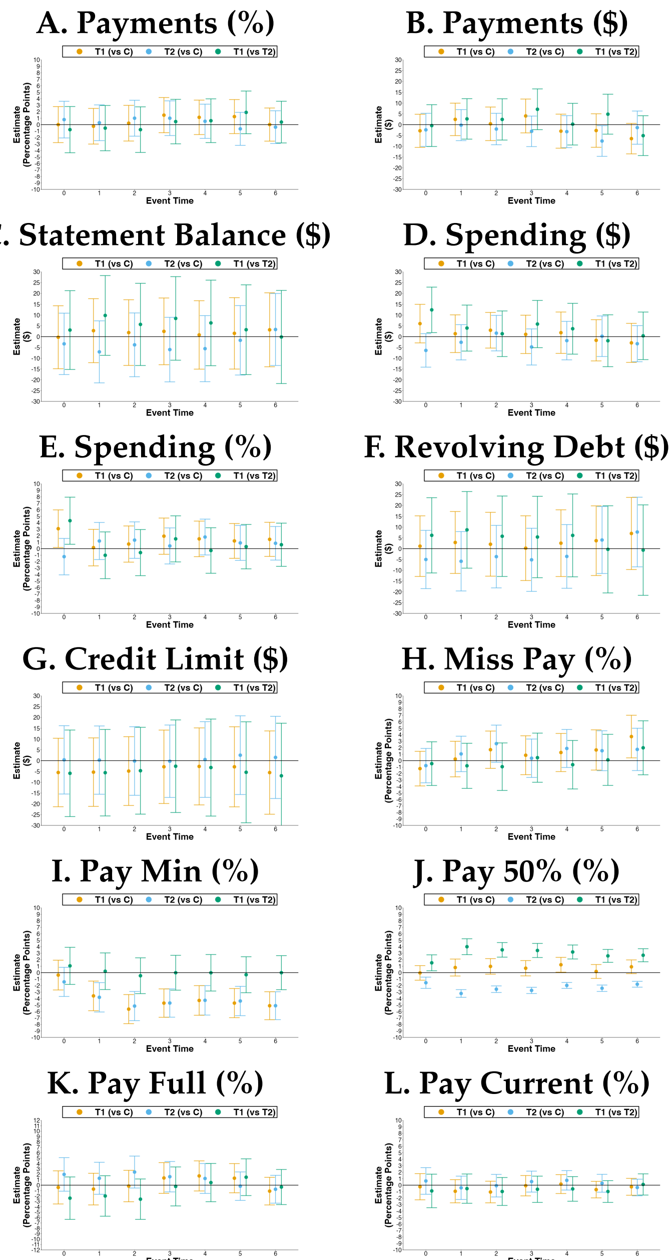
Notes: These panels show the CDFs of payments. Panel A shows the CDF of payments as a percent of the statement balance. The dotted vertical line in Panel A denotes 50% of the statement balance amount. Panel B shows the CDF of excess payments, defined as the payments less the minimum payment (as a percent of the statement balance less minimum payment) conditional on consumers making any payment. In panel B, MIN denotes consumers that make exactly the minimum payment and FULL denotes those that pay their statement balance in full (or more). In both panels, each color shows the CDF for the control group (C) in black, treatment group T1 (that shrouds the minimum payment option) in orange, and treatment group T2 (that shrouds both the minimum payment option and also the 50% payment option) in blue. In both panels, the outcome is measured after six completed statement cycles. Panel A contains 6,389 consumers observed in cycle six. Panel B the subset of the consumers in Panel A that make any payment in the that sixth cycle, which is 3,983 consumers.

Figure A4: Dynamic Effects On Primary And Secondary Outcomes, No Time F.E.



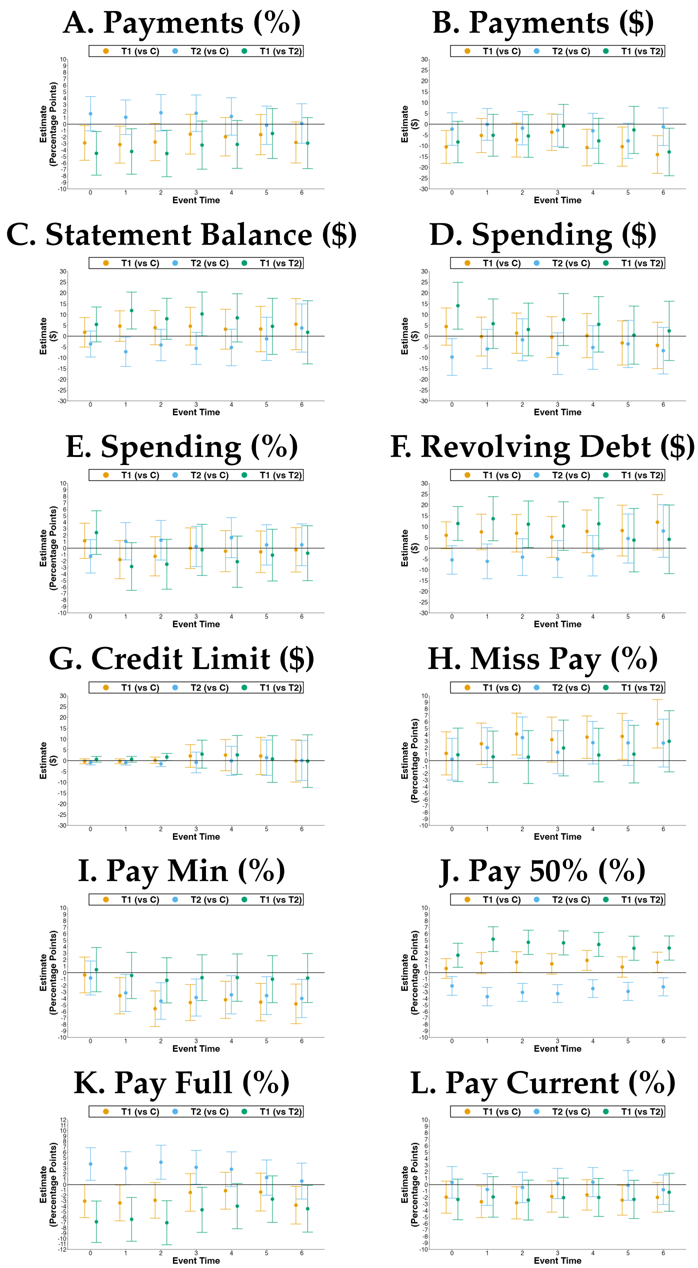
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6).

Figure A5: Dynamic Effects On Primary And Secondary Outcomes, With Time F.E.



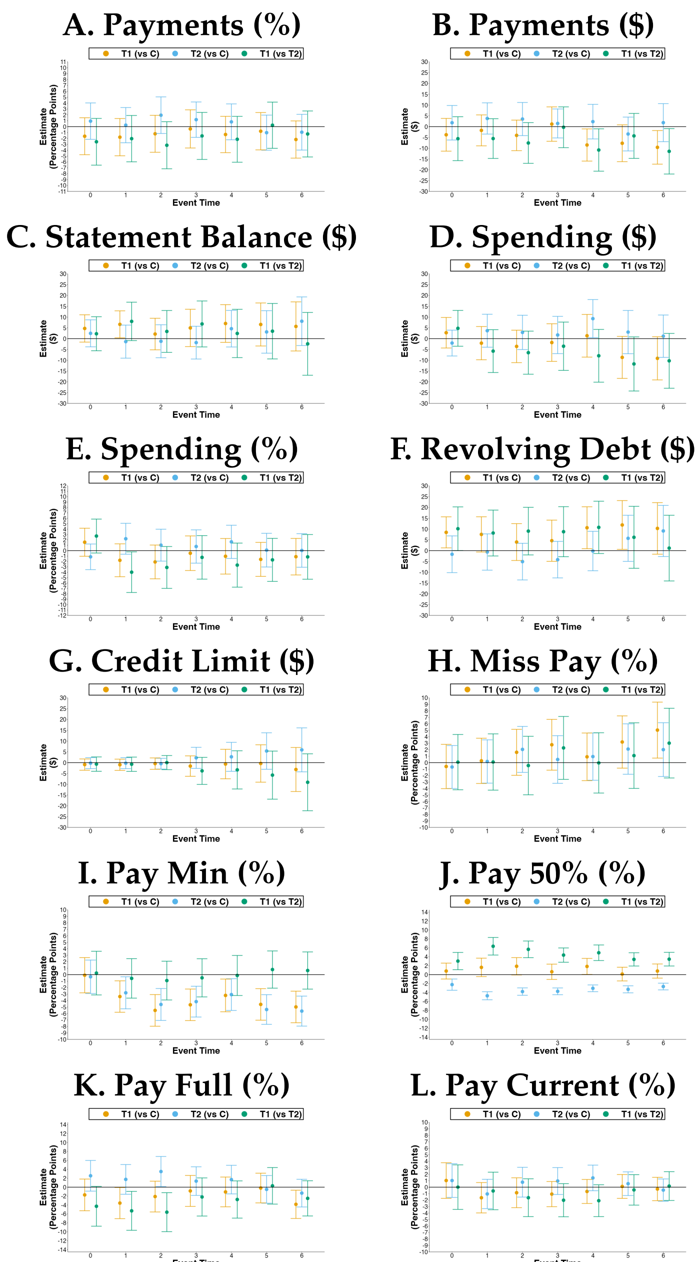
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. All regressions also include time fixed effects for each statement end date. The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6).

Figure A6: Dynamic Effects On Primary And Secondary Outcomes, With Time F.E. And Consumer F.E.



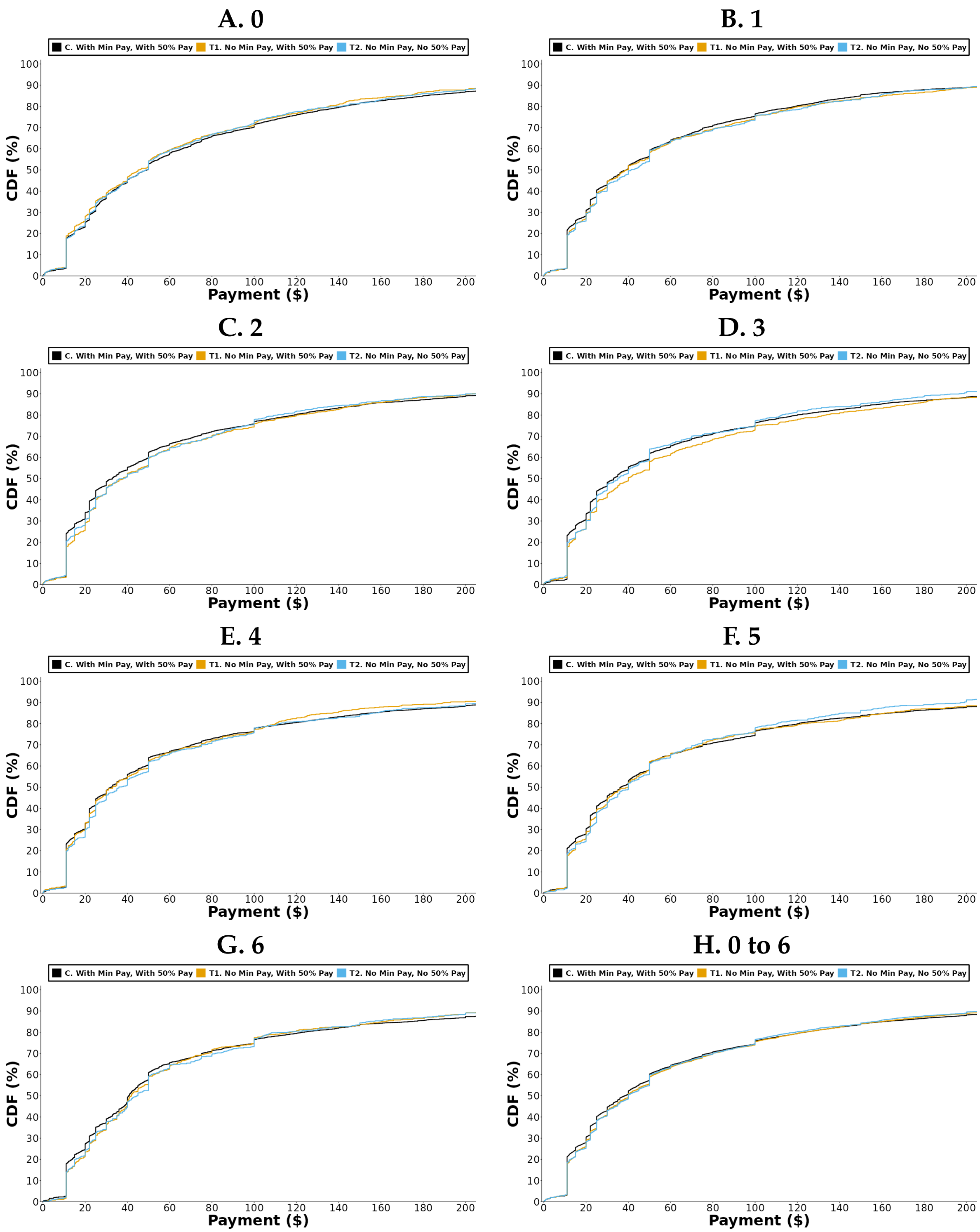
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. All regressions also include time fixed effects for each statement end date and also fixed effects for each consumer. With these consumer fixed effects, the omitted category for the interactions are those on the statement cycle before the experiment started. The regressions use data on 6,714 consumers and $N = 53{,}233$ *observations (statement cycles -1 to 6).*

Figure A7: Dynamic Effects On Primary And Secondary Outcomes, With Time F.E. And Consumer Controls, For Subsample Without autopay



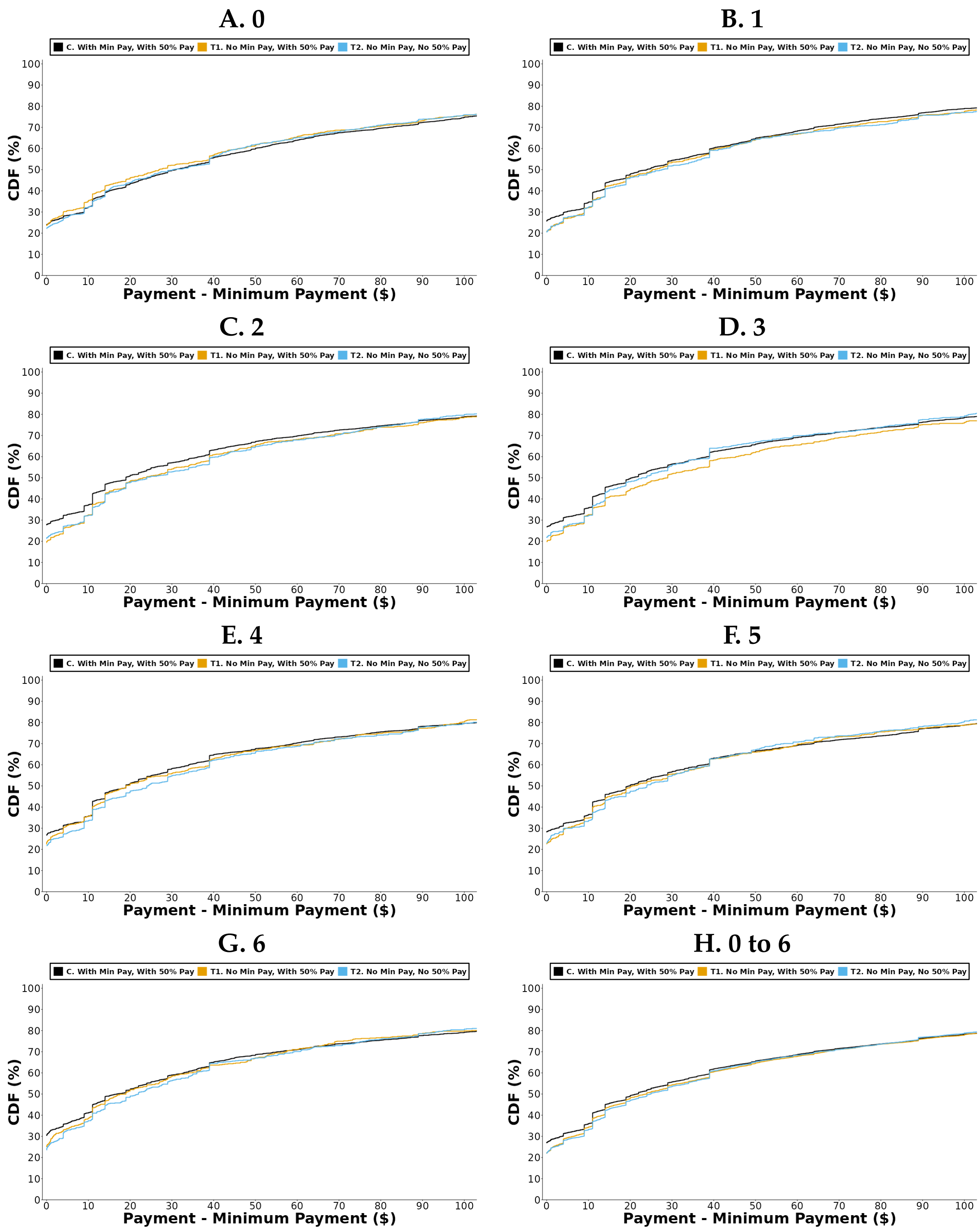
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. All regressions also include time fixed effects for each statement end date and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). The regressions use data on 3,939 consumers and $N = 27{,}206$ observations (statement cycles 0 to 6).

Figure A8: Distributions Of Payments ($) By Statement



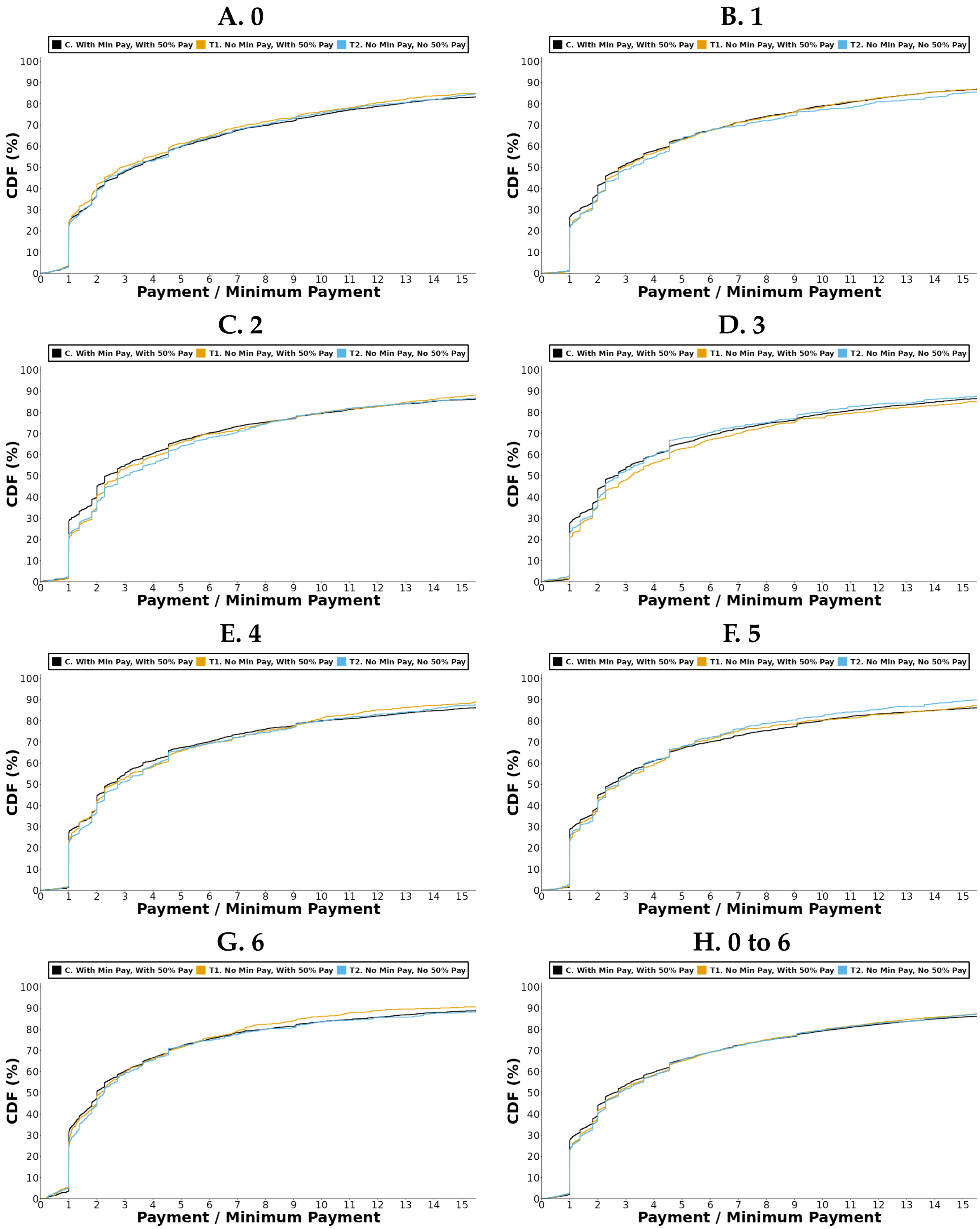
Notes: These panels show the CDFs of payments ($) across statement cycles. Panel H shows aggregating data across all cycles 0 to 6. The CDFs only includes consumers that make any payments in that cycle. In all panels, each color shows the CDF for the control group (C) in black, treatment group T1 (that shrouds the minimum payment option) in orange, and treatment group T2 (that shrouds both the minimum payment option and also the 50% payment option) in blue.

Figure A9: Distributions Of Payments Minus Minimum Payment ($) By Statement



Notes: These panels show the CDFs of payments minus the minimum payment ($) across statement cycles. Panel H shows aggregating data across all cycles 0 to 6. The CDFs only includes consumers that make any payments in that cycle. In all panels, each color shows the CDF for the control group (C) in black, treatment group T1 (that shrouds the minimum payment option) in orange, and treatment group T2 (that shrouds both the minimum payment option and also the 50% payment option) in blue.

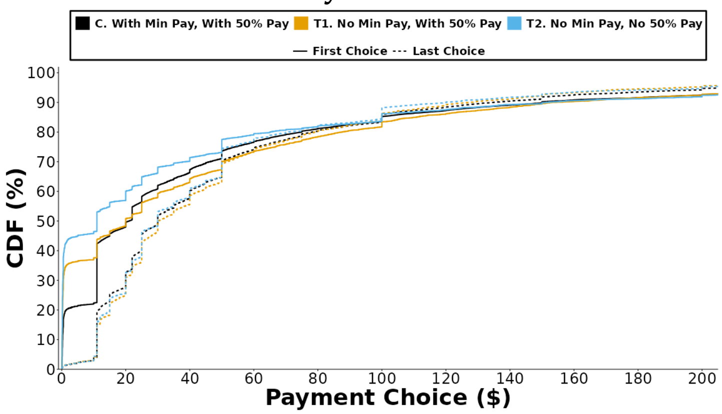
Figure A10: Distributions Of Ratio Of Payments To Minimum Payment By Statement



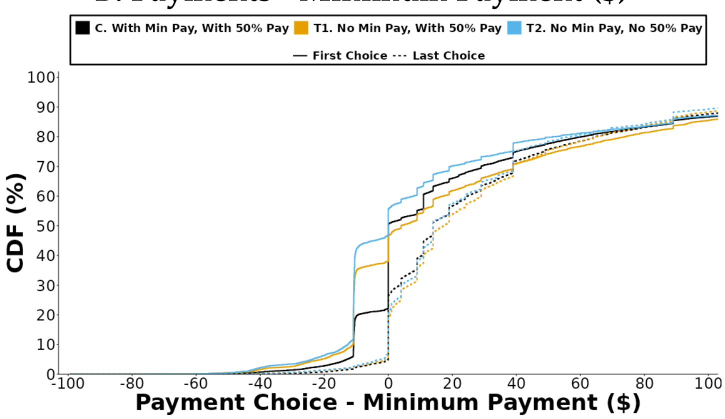
Notes: These panels show the CDFs of the ratio of payments to the minimum payment across statement cycles. Panel H shows aggregating data across all cycles 0 to 6. The CDFs only includes consumers that make any payments in that cycle and have a non-zero statement balance and a non-zero minimum payment. In all panels, each color shows the CDF for the control group (C) in black, treatment group T1 (that shrouds the minimum payment option) in orange, and treatment group T2 (that shrouds both the minimum payment option and also the 50% payment option) in blue.

Figure A11: CDFs Of First And Last Payment Choices, Excluding Blank Choices And Choices Of Zero Dollars

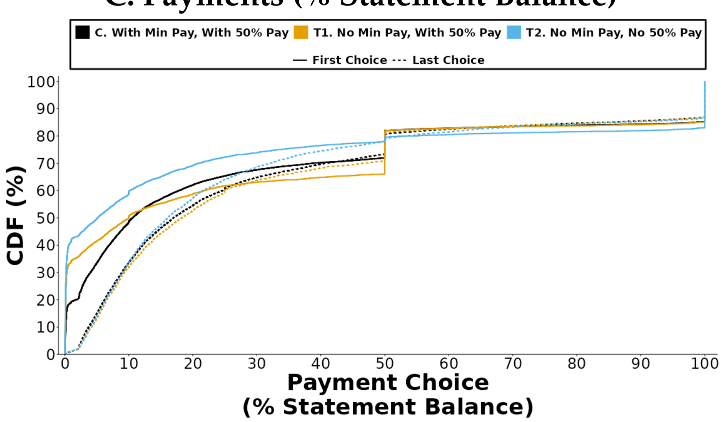
A. Payments ($)



B. Payments - Minimum Payment ($)

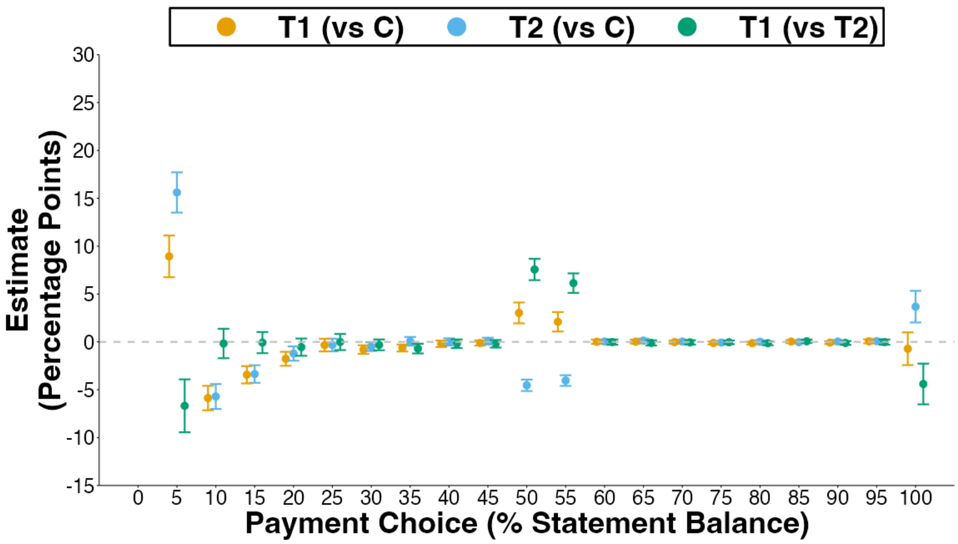


C. Payments (% Statement Balance)



Notes: This includes consumers who make at least one payment and have a non-zero statement balance and non-zero minimum payment due in that cycle. This excludes blank choices and choices of zero dollars. Each panel shows CDFs calculated a different way, Panel A shows CDFs for payments in dollars, Panel B shows for excess payments (i.e., payments less the minimum payment), and Panel C shows for payments as a percentage of the statement balance. The colors show the CDFs for consumers in the control group, C in black, and the two treatment groups, T1 in orange and T2 in blue. The solid lines show the CDF of consumers' first payment choices in a payment cycle and the dashed lines show the CDF of consumers' last payment choices in a payment cycle. All CDFs use data pooled across cycles zero to six.

Figure A12: Effects On Payment Distribution Of Initial Payment Choice (% Statement Balance), Excluding Blank Choices And Choices Of Zero Dollars



Notes: This figure shows the estimates on treatment indicators, showing the average effects pooled across statements, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 interaction, with the T2 group as the omitted category and data for group C is not used in these regressions. All regressions also include time fixed effects for each statement end date and also a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). The regressions use data on 5,635 consumers and $N = 25{,}820$ observations, the initial payment choices of consumers in the app each cycle (statement cycles 0 to 6). This excludes blank choices and choices of zero dollars. The estimates for each x-axis level show results for a different binary outcome that takes a value of one if a consumer paid 5 percentage points up to that amount, and zero otherwise. The estimates corresponding to the x-axis value of one are if a consumer paid in full.

Table A1: Regression Estimates On Attrition

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)
3	-0.23	-0.12	-0.12
	(0.14)	(0.18)	(0.18)
6	-0.09	0.00	-0.09
	(0.64)	(0.70)	(0.83)
Time F.E.	X	X	X
Consumer Controls	X	X	X

Notes: This table shows estimates in percentage points for the effects on attrition as of the third cycle (denoted by row labeled 3), and attrition as of the sixth cycle (denoted by row labeled 6) estimated from separate regressions. All of the estimates are from treatment indicators, showing the average effects. All regressions include time fixed effects for each statement end date and a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). Heteroskedastic-robust standard errors are shown in parenthesis. The regressions use data on 6,714 consumers with one observation per consumer.

Table A2: Regression Estimates On Secondary Outcomes, Pooled Across Statements

	T1 (vs C)		T2 (vs C)		T1 (vs T2)	
	(1)	(2)	(3)	(4)	(5)	(6)
Payments	-3.83	-8.85	-3.31	-2.73	-0.52	-6.12
	(2.19)	(3.43)	(2.13)	(3.12)	(2.74)	(4.15)
Statement Balance	4.39	3.85	-2.29	-3.40	6.68	7.25
	(2.42)	(3.63)	(2.35)	(3.08)	(3.04)	(4.21)
Spending	-2.49	-0.25	-2.32	-5.87	-0.17	5.61
	(2.13)	(3.93)	(2.10)	(4.11)	(2.70)	(5.12)
Spending (%)	-0.38	-0.46	0.72	0.56	-1.10	-1.02
	(0.71)	(1.29)	(0.71)	(1.25)	(0.9)	(1.61)
Revolving Debt	6.29	7.61	-0.10	-1.81	6.39	9.42
	(2.78)	(3.95)	(2.72)	(3.64)	(3.50)	(4.77)
Credit Limit	-0.89	0.91	-0.09	-0.41	-0.80	1.32
	(1.72)	(2.26)	(1.64)	(2.11)	(2.17)	(2.80)
Miss Pay (%)	1.72	3.43	1.29	2.18	0.43	1.26
	(0.90)	(1.34)	(0.90)	(1.30)	(1.15)	(1.66)
Pay Min (%)	-3.53	-3.94	-3.38	-3.29	-0.15	-0.65
	(0.65)	(1.17)	(0.64)	(1.18)	(0.80)	(1.49)
Pay 50% (%)	0.70	1.35	-2.46	-2.80	3.16	4.15
	(0.29)	(0.62)	(0.15)	(0.67)	(0.28)	(0.81)
Pay Full (%)	-1.00	-2.43	1.20	2.75	-2.20	-5.18
	(0.85)	(1.42)	(0.82)	(1.30)	(1.07)	(1.72)
Pay Current Balance (%)	-0.67	-2.17	0.26	-0.16	-0.93	-2.01
	(0.40)	(1.01)	(0.42)	(0.97)	(0.52)	(1.26)
Time F.E.	X	X	X	X	X	X
Consumer Controls	X		X		X	
Consumer F.E.		X		X		X

Notes: Each row of this table shows estimates for a different outcome. All of the estimates are from treatment indicators, showing the average effects pooled across statement cycles 0 to 6. All columns include time fixed effects for each statement end date in the regressions. Columns (1), (3), and (5) include in the regressions a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). Columns (2), (4), and (6) include an extra observation per consumer for the statement cycle before the experiment to enable including consumer fixed effects in the regressions. In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions in columns (1), (3), and (5) use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6), and the regressions in columns (2), (4), and (6) contain $N = 53{,}233$ observations (statement cycles -1 to 6). The units shown are in dollars, or for outcomes with (%) are in percentage points.

Table A3: Regression Estimates On Secondary Outcomes, After Three Statements

	T1 (vs C)			T2 (vs C)			T1 (vs T2)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Payments	-0.07	4.03	-3.72	-7.5	-3.09	-2.87	7.42	7.12	-0.85
	(3.84)	(4.00)	(4.32)	(3.44)	(3.61)	(3.81)	(4.81)	(4.82)	(5.10)
Statement Balance	-3.34	2.45	4.61	-11.67	-6.00	-5.67	8.33	8.45	10.28
	(7.84)	(7.89)	(4.45)	(7.55)	(7.63)	(3.79)	(9.89)	(9.87)	(5.19)
Spending	1.07	1.07	-0.42	-5.07	-4.77	-8.11	6.14	5.84	7.69
	(4.35)	(4.53)	(4.80)	(4.06)	(4.27)	(4.92)	(5.57)	(5.58)	(6.14)
Spending (%)	2.46	1.93	-0.01	0.97	0.42	0.26	1.49	1.51	-0.27
	(1.37)	(1.43)	(1.60)	(1.36)	(1.42)	(1.58)	(1.82)	(1.81)	(2.02)
Revolving Debt	-1.39	0.23	5.18	-6.69	-5.17	-5.06	5.30	5.39	10.24
	(7.58)	(7.65)	(4.82)	(7.36)	(7.46)	(4.36)	(9.65)	(9.62)	(5.73)
Credit Limit	-5.61	-2.84	2.18	-3.28	-0.26	-0.84	-2.34	-2.58	3.02
	(8.72)	(8.68)	(2.68)	(8.61)	(8.56)	(2.45)	(11.02)	(10.94)	(3.31)
Miss Pay (%)	2.79	0.84	3.24	2.30	0.38	1.29	0.49	0.46	1.95
	(1.46)	(1.53)	(1.78)	(1.44)	(1.51)	(1.69)	(1.96)	(1.94)	(2.20)
Pay Min (%)	-4.76	-4.70	-4.62	-4.77	-4.69	-3.84	0.01	-0.01	-0.78
	(1.05)	(1.12)	(1.42)	(1.04)	(1.11)	(1.46)	(1.38)	(1.37)	(1.80)
Pay 50% (%)	0.75	0.68	1.36	-2.59	-2.74	-3.23	3.35	3.43	4.59
	(0.56)	(0.60)	(0.8)	(0.12)	(0.25)	(0.71)	(0.55)	(0.56)	(0.95)
Pay Full (%)	0.36	1.33	-1.45	0.42	1.57	3.22	-0.06	-0.24	-4.66
	(1.39)	(1.45)	(1.77)	(1.38)	(1.44)	(1.60)	(1.84)	(1.84)	(2.13)
Pay Current Balance (%)	-0.65	-0.08	-1.83	-0.01	0.55	0.18	-0.64	-0.63	-2.01
	(0.74)	(0.80)	(1.23)	(0.77)	(0.83)	(1.19)	(1.03)	(1.04)	(1.55)
Time F.E.		X	X		X	X		X	X
Consumer F.E.			X			X			X

Notes: Each row of this table shows estimates for a different outcome. All of the estimates are from interactions between treatment indicators and an indicator for the third statement cycle in the experiment. All of these regressions also include interactions between treatments and the other statement cycles. All columns except (1), (4), and (7) include time fixed effects for each statement end date in the regressions. Columns (3), (6), and (9) include an extra observation per consumer for the statement cycle before the experiment to enable including consumer fixed effects in the regressions. In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6), except for the models with consumer fixed effects that contain 53,233 observations (statement cycles -1 to 6). The units shown are in dollars, or for outcomes with (%) are in percentage points.

Table A4: Regression Estimates On Secondary Outcomes, After Six Statements

	T1 (vs C)		T2 (vs C)		T1 (vs T2)	
	(1)	(2)	(3)	(4)	(5)	(6)
Payments	-9.31	-14.08	-1.95	-1.19	-7.36	-12.89
	(3.32)	(4.45)	(3.65)	(4.44)	(4.38)	(5.62)
Statement Balance	6.03	5.54	3.92	3.77	2.11	1.78
	(4.96)	(6.04)	(4.94)	(5.69)	(6.33)	(7.46)
Spending	-6.83	-4.28	-3.27	-6.73	-3.56	2.45
	(4.27)	(5.48)	(3.99)	(5.51)	(5.27)	(7.00)
Spending (%)	-0.41	-0.26	0.71	0.52	-1.12	-0.78
	(1.28)	(1.75)	(1.22)	(1.64)	(1.6)	(2.16)
Revolving Debt	10.76	12.02	8.79	7.94	1.97	4.09
	(5.32)	(6.53)	(5.14)	(6.22)	(6.68)	(8.12)
Credit Limit	-2.15	-0.14	0.35	0.08	-2.50	-0.22
	(4.33)	(4.97)	(4.17)	(4.74)	(5.47)	(6.22)
Miss Pay (%)	4.39	5.70	1.92	2.71	2.47	2.99
	(1.63)	(1.91)	(1.61)	(1.88)	(2.06)	(2.41)
Pay Min (%)	-4.60	-4.82	-4.34	-3.99	-0.26	-0.84
	(1.09)	(1.56)	(1.06)	(1.50)	(1.32)	(1.93)
Pay 50% (%)	0.94	1.60	-1.93	-2.20	2.87	3.80
	(0.54)	(0.80)	(0.24)	(0.71)	(0.52)	(0.95)
Pay Full (%)	-2.44	-3.80	-0.72	0.66	-1.72	-4.46
	(1.25)	(1.78)	(1.23)	(1.70)	(1.57)	(2.21)
Pay Current Balance (%)	-0.48	-1.96	-0.33	-0.79	-0.16	-1.18
	(0.66)	(1.17)	(0.66)	(1.17)	(0.84)	(1.49)
Time F.E.	X	X	X	X	X	X
Consumer Controls	X		X		X	
Consumer F.E.		X		X		X

Notes: Each row of this table shows estimates for a different outcome. All of the estimates are from interactions between treatment indicators and an indicator for the sixth statement cycle in the experiment. All of these regressions also include interactions between treatments and the other statement cycles. All columns include time fixed effects for each statement end date in the regressions. Columns (1), (3), and (5) include in the regressions a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). Columns (2), (4), and (6) include an extra observation per consumer for the statement cycle before the experiment to enable including consumer fixed effects in the regressions. In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions use data on 6,714 consumers and $N = 46{,}519$ observations (statement cycles 0 to 6), except for the models with consumer fixed effects that contain 53,233 observations (statement cycles -1 to 6). The units shown are in dollars, or for outcomes with (%) are in percentage points.

Table A5: Regression Estimates For Subsample Without Autopay, After Six Statements

	T1 (vs C)		T2 (vs C)		T1 (vs T2)	
	(1)	(2)	(3)	(4)	(5)	(6)
Payments (%)	-2.18	-2.97	-0.94	-0.24	-1.24	-2.73
	(1.61)	(2.08)	(1.55)	(2.02)	(1.99)	(2.58)
Payments	-9.58	-10.48	1.85	3.84	-11.44	-14.31
	(3.97)	(5.08)	(4.50)	(5.37)	(5.36)	(6.51)
Statement Balance	5.66	3.30	8.07	8.87	-2.41	-5.57
	(5.8)	(6.93)	(5.75)	(6.78)	(7.42)	(8.82)
Spending	-9.12	-7.76	1.15	0.10	-10.27	-7.86
	(5.10)	(6.94)	(5.00)	(6.69)	(6.48)	(8.65)
Spending (%)	-1.12	-0.42	0.03	-0.39	-1.15	-0.03
	(1.73)	(2.37)	(1.58)	(2.15)	(2.11)	(2.89)
Revolving Debt	10.31	9.85	9.13	7.11	1.18	2.74
	(6.09)	(7.46)	(6.04)	(7.43)	(7.76)	(9.47)
Credit Limit	-3.17	-2.24	5.93	6.46	-9.09	-8.70
	(5.2)	(5.78)	(5.20)	(5.89)	(6.73)	(7.50)
Miss Pay (%)	5.03	5.38	2.01	2.77	3.02	2.61
	(2.21)	(2.55)	(2.12)	(2.42)	(2.75)	(3.16)
Pay Min (%)	-4.99	-5.17	-5.63	-6.05	0.64	0.88
	(1.24)	(1.93)	(1.18)	(1.77)	(1.46)	(2.33)
Pay 50% (%)	0.81	2.22	-2.66	-3.05	3.47	5.26
	(0.81)	(1.15)	(0.38)	(1.08)	(0.78)	(1.41)
Pay Full (%)	-3.84	-3.81	-1.33	0.43	-2.51	-4.24
	(1.62)	(2.35)	(1.58)	(2.22)	(2.01)	(2.89)
Pay Current Balance (%)	-0.29	-0.96	-0.45	-0.42	0.16	-0.54
	(0.92)	(1.53)	(0.88)	(1.52)	(1.13)	(1.94)
Time F.E.	X	X	X	X	X	X
Consumer Controls	X		X		X	
Consumer F.E.		X		X		X

Notes: This table uses data for the subsample of consumers not enrolled in autopay in the month before the launch of the experiment. Each row of this table shows estimates for a different outcome. All of the estimates are from interactions between treatment indicators and an indicator for the sixth statement cycle in the experiment. All of these regressions also include interactions between treatments and the other statement cycles. All columns include time fixed effects for each statement end date in the regressions. Columns (1), (3), and (5) include in the regressions a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). Columns (2), (4), and (6) include an extra observation per consumer for the statement cycle before the experiment to enable including consumer fixed effects in the regressions. In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions use data on 3,939 consumers and $N = 27{,}206$ observations (statement cycles 0 to 6), except for the models with consumer fixed effects that contain $N = 31{,}145$ observations (statement cycles -1 to 6). The units shown are in dollars, or for outcomes with (%) are in percentage points.

Table A6: Regression Estimates On The Portfolio Of Credit Card Statement Balances, Pooled Across Months

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)
Portfolio Statement Balances	36.55 (51.85)	6.60 (53.41)	29.94 (66.98)
Time F.E.	X	X	X
Consumer Controls	X	X	X

Notes: This table shows estimates in dollars for the effects on the portfolio of credit card statement balances, measured in credit reporting data. All of the estimates are from treatment indicators, showing the average effects pooled across months. All regressions include time fixed effects for each credit reporting month and a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions use data on 6,712 consumers and $N = 46{,}280$ observations (months 0 to 6).

Table A7: Regression Estimates On Payments, Pooled Across Statements

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)
Pay Multiples Of Minimum	-0.76	-0.94	0.18
	(0.25)	(0.23)	(0.29)
Pay Round Number	2.51	3.61	-1.10
	(0.62)	(0.62)	(0.80)
Time F.E.	X	X	X
Consumer Controls	X	X	X

Notes: This table shows the estimates from treatment indicators, showing the average effects, in percentage points, pooled across statement cycles 0 to 6. Each row of this table shows estimates for a different outcome. Pay Multiples Of Minimum is a binary outcome that takes a value of one if the consumer pays 2, 3, 4, or 5 times the minimum payment amount (after dollar rounding). Pay Round Number is a binary outcome that takes a value of one if the consumer pays exactly a round number $\in$ *{$15, $20,...,$45, $50}. These outcomes only take a value of one for the cases where the minimum payment is positive and the consumer pays more than the minimum and less than the full balance. All regressions include time fixed effects for each statement end date and a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The regressions use data on 6,714 consumers and N = 46,519 observations (statement cycles 0 to 6).*

Table A8: Heterogeneous Treatment Effects On Payments (%) By Pre-Trial Payment Behavior, Pooled Across Statements

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Consumers
Near Minimum	2.89	-0.56	3.45	1,226
	(1.73)	(1.54)	(2.07)	
Not Near Minimum	-1.44	0.28	-1.72	5,488
	(0.92)	(0.92)	(1.17)	
Time F.E.	X	X	X	
Consumer Controls	X	X	X	

Notes: This table shows the estimates from treatment indicators, showing the heterogeneous effects, in percentage points, pooled across statement cycles 0 to 6. Each row of this table shows estimates for a different subsample, with the fourth column showing the number of consumers in each subsample. Near Minimum is the 18.3% sample of consumers who paid more than the minimum but less than or equal to $50 more than the minimum (but not paid in full) for 50%+ of the six cycles before the start of the experiment (for cards opened less than six cycles before the experiment, we calculate this based on cycles that occur). Not Near Minimum is the sample of all other consumers: 81.7%. The outcome in both regressions is the primary outcome: payments (% statement balance). All regressions include time fixed effects for each statement end date and a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. The Near Minimum regression uses data on 1,226 consumers and N = 8,502 observations (statement cycles 0 to 6). The Not Near Minimum regression uses data on 5,488 consumers and N = 38,017 observations (statement cycles 0 to 6).

Table A9: Heterogeneous Treatment Effects On Payments (%) By Tertiles Of Pre-Trial Credit Card Utilization, Pooled Across Statements

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Consumers
Q1 Card Utilization (<60%)	-1.89	3.70	-5.59	2,238
	(1.65)	(1.60)	(2.08)	
Q2 Card Utilization (60-91%)	1.57	-1.19	2.76	2,238
	(1.40)	(1.31)	(1.72)	
Q3 Card Utilization (91%+)	-1.45	-1.30	-0.15	2,238
	(1.15)	(1.17)	(1.44)	
Q1 Portfolio Utilization (<12%)	-1.68	0.37	-2.05	2,238
	(1.43)	(1.47)	(1.86)	
Q2 Portfolio Utilization (12-45%)	-2.31	0.94	-3.25	2,238
	(1.43)	(1.40)	(1.83)	
Q3 Portfolio Utilization (45%+)	1.66	-0.96	2.62	2,238
	(1.45)	(1.36)	(1.78)	
Time F.E.	X	X	X	
Consumer Controls	X	X	X	

Notes: This table shows the estimates from treatment indicators, showing the heterogeneous effects, in percentage points, pooled across statement cycles 0 to 6. Each row of this table shows estimates for a different subsample, with the fourth column showing the number of consumers in each subsample. The rows with 'Card Utilization' segment consumers by tertiles of their credit card utilization (credit card statement balance divided by credit card limit) calculated only using the card in the experiment based on the statement before the experiment went into the field. The rows with 'Portfolio Utilization' segment consumers by tertiles of their portfolio credit card utilization (sum of credit card statement balances divided by sum of credit card limits) using Equifax credit reporting data which uses the portfolio of credit cards held by a consumer in the month before the the experiment went into the field. Q1 denotes the lowest tertile, Q2 is the intermediate tertile, and Q3 is the highest tertile. The outcome in all regressions is the primary outcome: payments (% statement balance). All regressions include time fixed effects for each statement end date and a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score card tenure, all of the primary and secondary outcomes, the number of open credit cards held in their portfolio, total credit card portfolio balances, and total credit card portfolio credit limits, and fixed effects for each of the following: the card's interest rate, credit limit, all of the primary and secondary outcomes, and the type of autopay the consumer was enrolled in (autopay to the minimum, autopay to the full amount, autopay to a fixed amount, or no autopay). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis.