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 significantly increases credit card payments. This finding contrasts with prior evidence from highly-replicated online experiments that finds large increases in hypothetical payments from shrouding the minimum payment. My results demonstrate that the minimum payment does not act as an anchor that lowers payments. Instead, credit cardholders frequently lack liquid cash and target paying at least the minimum payment, which acts as a reference point.

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1 Introduction

One of the foundational biases in Tversky and Kahneman (1974) is the "anchoring and adjustment heuristic," whereby consumers faced with an uncertain choice rely on salient but irrelevant numerical information and adjust too little away from it. In the fifty years since, there is broad evidence of anchoring across domains (Schley and Weingarten, 2025). Anchoring has first order implications, as it suggests that preferences are unstable or inconsistent.

An economically important domain in which anchoring has been influential is credit cards. A large body of research—with publications across the fields of economics, finance, management, marketing, and psychology—attributes anchoring as an important determinant of consumer credit card payment choices.[1] Online experiments, the results of which have been repeatedly replicated, find that shrouding the credit card minimum payment causes a large increase in hypothetical credit card payments, leading to the conclusion that the minimum payment acts as a psychological anchor.[2] Further evidence in support of credit card anchoring has been provided in credit card administrative data by Keys and Wang (2019) and Medina and Negrin (2022), who both examine consumer responses to changes in minimum payments and find that these responses are inconsistent with mere liquidity constraints. An important implication of this research is that consumers would pay more on their credit cards without anchoring.[3]

In this paper, I show that the results of online experiments do not extrapolate to the field, and my evidence is inconsistent with consumers anchoring to the credit card minimum payment. I study a field experiment on 6,714 consumers with a FinTech lender that provides credit cards in the United States, primarily to consumers with below-prime credit scores. Consumers with low credit scores are those who are most likely to pay at or near the minimum payment amount (Keys and Wang, 2019) and have the highest interest rates (Consumer Financial Protection Bureau, 2025; Guttman-Kenney and Shahidinejad, 2025; Drechsler et al., 2025). These cardholders also appear to rely more on heuristics that may not minimize their interest costs (Keys and Wang, 2019; Medina and Negrin, 2022; Agarwal, Presbitero, Silva and Wix, 2026). Given the combination of these factors, consumers with low credit scores have the highest potential benefits from paying more to reduce their interest costs.

For six credit card statement cycles, the experiment varies the in-app choice architecture to

[1] Thaler and Sunstein (2008) write in their book *Nudge*, that a credit card minimum payment "can serve as an anchor, and as a nudge that this minimum payment is an appropriate amount." A series of academic papers attribute credit card payment choices to anchoring: Stewart (2009); Navarro-Martinez et al. (2011); Jiang and Dunn (2013); Salisbury (2014); McHugh and Ranyard (2016); Guttman-Kenney et al. (2018); Keys and Wang (2019); Salisbury and Zhao (2020); Hendy et al. (2021); Medina and Negrin (2022); Sakaguchi et al. (2022); Katz et al. (2024); Ortiz et al. (2024); Vihriälä (2025); Durán-Micco et al. (2026), and Incekara-Hafalir et al. (2026).

[2] Navarro-Martinez et al. (2011); Jiang and Dunn (2013); Salisbury (2014); McHugh and Ranyard (2016); Guttman-Kenney et al. (2018); Salisbury and Zhao (2020); Hendy et al. (2021); Sakaguchi et al. (2022); Ortiz et al. (2024); Durán-Micco et al. (2026), and Incekara-Hafalir et al. (2026). Appendix Figure A1 is an example of such online experimental results. Online experiments are sometimes referred to as lab or survey experiments, and in the taxonomy of Harrison and List (2004) as artefactual or framed field experiments.

[3] Keys and Wang (2019) 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."

test two nudges to shroud the minimum payment amount, and also an option to pay 50% of the statement balance. My main data source is administrative credit card data from the lender. I supplement this with clickstream data on payment choices, household financial transaction data on liquid cash, and consumer credit reporting data, all of which are merged.

I find that neither nudge significantly increases credit card payments. After six statements, the effects on payments (as a percentage of the statement balance) are -1.26 (95% C.I. -3.67, 1.15) and -0.54 (95% C.I. -2.87, 1.79) percentage points for the two nudges, respectively. Pooling data across cycles to increase power also finds the two nudges to have insignificant effects: -0.67 (95% C.I. -2.26, 0.92) and 0.29 (95% C.I. -1.28, 1.86) percentage points. My results are robust to other outcomes and regression specifications. In fact, after six statements, payments in dollars significantly decrease, and revolving debt significantly increases. All of these results are inconsistent with anchoring and contrast sharply with online experiments: For example, shrouding the minimum payment increases hypothetical payments by 12.4 (95% C.I. 10.8, 14.0) percentage points in Guttman-Kenney et al. (2023). I also find that adding a 50% payment option has the unintended effect of temporarily making consumers less likely to pay their credit card balance in full.

My research contributes to debates on research methods and the challenges of scaling interventions (List, 2022). List (2001) shows how hypothetical responses can be biased, leading to a gap between intentions and actions. Online experiments are widely used across the social sciences (Rauf et al., 2025), and Gandhi et al. (2025) conclude that hypothetical studies are generally consistent in direction with field experiments but vary widely in magnitude.[4] My study provides a case where a highly-replicated online experimental result does not hold up in a field setting that captures the intertemporal trade-offs consumers face when making financial decisions.

These results have direct policy implications. Persistently revolving credit card debt is costly and common (Grodzicki and Koulayev, 2021). To reduce credit card debt, regulators in the United Kingdom have considered requiring lenders to shroud minimum payments (Financial Conduct Authority, 2021), and similar proposals have also been discussed in the United States citing the academic evidence on credit card anchoring (e.g., Tescher and Stone, 2022; Gdalman et al., 2023; Krouse and Richardson, 2025). My study provides new field experimental evidence that such a policy to shroud the minimum payment is expected to be ineffective. Given this policy interest, documenting null results is important, especially in the broader context of publication bias and a lack of self-correction in the academic literature (e.g., Abadie, 2020; Brodeur et al., 2023; Ankel-Peters et al., 2025).

The paper contributes to the behavioral economics and finance literatures. Nudges are less effective than they were originally thought to be (DellaVigna and Linos, 2022; Choukhmane, 2025; Choi et al., 2024). My results are consistent with the closest prior work on credit card nudges, Guttman-Kenney et al. (2025), but go beyond this in four respects. First, I test shrouding the min-

[4]Such issues do not only apply to online experiments. Gordon et al. (2023) document how, even with rich data and sophisticated machine learning methods, they cannot accurately estimate the causal effects of adverts from field experiments. The use of Large Language Models (LLMs) by survey respondents may also reduce the quality of online panels (Westwood, 2025), although this cannot explain the online studies in my domain as these predate LLMs.

imum payment amount on manual payments each month, whereas Guttman-Kenney et al. (2025) test the shrouding of the option at card opening to enroll in autopay to the minimum. The majority of the value of credit card payments are made manually, even among those enrolled in autopay (Consumer Financial Protection Bureau, 2025; Guttman-Kenney et al., 2025), and anchoring is expected to be most important for this manual channel where payment choices are actively made each month. Second, I show that even without the offsetting responses in Guttman-Kenney et al. (2025), consumers undo the nudge.[5] Third, I also test a new intermediate payment option: 50% of the statement balance. Fourth, and most importantly, the data and research design allow me to pin down the economics and psychology behind credit card payment decisions.

My results have important implications for macroeconomic models. I provide evidence using administrative data consistent with Lee and Maxted (2026), showing that hand-to-mouth consumers are not at their borrowing limit, in contrast to how consumers are modeled in heterogeneous-agent consumption models (e.g. Kaplan and Violante, 2022). The empirical support for Lee and Maxted (2026)'s modeling approach has broad implications, as it affects how households respond to fiscal and monetary policy.

My empirical results inform theory regarding why consumers choose to pay only the minimum payment, or amounts just above it. Consistent with earlier work (Keys and Wang, 2019; Medina and Negrin, 2022), my analysis also rules out a purely traditional liquidity constraints explanation. Instead, consumers frequently lack liquid cash: 93% have a minimum liquid cash balance of less than $501 during the ninety days before the experiment, following the approach in Guttman-Kenney et al. (2025).

An important new result in this paper is that examining clickstream data on consumers' initial in-app payment choices reveals that, when the minimum is shrouded, consumers are significantly more likely to initially select lower payment amounts—in particular amounts less than the minimum. This is the opposite of the anchoring explanation proposed by the earlier literature, which suggests that shrouding the minimum payment amount should increase credit card payments. Forcing consumers to make an active choice (e.g., Carroll et al., 2009; Keller et al., 2011) does not change economic outcomes; instead, consumers immediately exert effort to reveal the minimum payment, hacking the active choice design.

The psychology of consumers' targeting specific payment amounts helps to understand my results. If the minimum payment amount acts as an anchor—an arbitrary and irrelevant focal value (Tversky and Kahneman, 1974; Ariely et al., 2003)—then shrouding this amount should change payment choices.[6] However, if the amount acts as a target, a relevant reference point that

[5]Some of the offsetting effects of the nudge 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 nudges in this paper. 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 the 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. By setting an autopay, a consumer may become inattentive to their debt (Sakaguchi et al., 2022).

[6]In Tversky and Kahneman (1974), one anchor is randomly determined by spinning a wheel of fortune. In Ariely et al. (2003), one anchor involves asking consumers to think of the last two digits of their Social Security Number, digits that

consumers aim for (e.g., Heath et al., 1999; Allen et al., 2017; O'Donoghue and Sprenger, 2018; Bartels et al., 2024), shifting choices may be harder. There is growing evidence that consumers have reference-dependent preferences in high-stakes financial decisions.[7] My evidence is consistent with the minimum payment amount acting as a target rather than an anchor: there is a discontinuity in the economic payoffs associated with paying at least this amount (e.g., avoiding late fees), making it a natural amount for consumers to target. Kleven (2016) discusses how kinks or notches in economic incentives can also be reference points, and Dean et al. (2026) provides theory on reference points providing information for consumers facing uncertainty, both of which appear consistent with my interpretation.

My work further suggests that in the credit card domain, there are two competing reference points: the minimum payment amount and the statement balance. While the welfare effects of single reference points have been studied (e.g., Reck and Seibold, 2026), there is no clear theory or empirical evidence on how consumers act under multiple reference points (O'Donoghue and Sprenger, 2018). My evidence appears consistent with consumers strongly targeting paying at least the minimum payment and, if possible, paying the statement balance, where there is another discontinuity in economic payoffs, as paying this amount avoids incurring any interest costs. Intermediate amounts—such as 50% of the statement balance, and the three-year payment scenarios tested in Agarwal et al. (2015) and Keys and Wang (2016)—can only slightly detract from these other amounts, with no economically important overall impact. This targeting interpretation helps to understand the findings of Keys and Wang (2019), Medina and Negrin (2022), Castellanos et al. (2026), and Allen et al. (2026), where consumers who do not appear to be constrained to only pay the minimum still respond to higher minimum payments. When minimum payments are higher, this reduces the distance to the statement balance amount, which may make this higher payment amount appear as a more achievable psychological goal for consumers to aim to target, similar to marathon runners' target times (Allen et al., 2017).

This paper also contributes to the household finance literature on consumer financial protection and credit cards (Agarwal et al., 2015, 2023; Agarwal, Qian and Zou, 2026; Seira et al., 2017; Hirshman and Sussman, 2022; Nelson, 2025; Matcham, 2025; Bord et al., 2026; Brown et al., 2026; Castellanos et al., 2026). Consumer behavior in the credit card domain is difficult to shift: information and nudges 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, Mao, Sussman, Mahoney and Min, 2025). Even when nudges in this domain show some success, effects attenuate toward zero over time (e.g., Adams et al., 2022; Han and Yin, 2026),

are randomly assigned. In both of these examples, the anchors are irrelevant to the choices studied, yet the anchors do affect consumer choices, which is inconsistent with classical economic choice models.

[7] For example, Genesove and Mayer (2001), Andersen et al. (2022), and Badarinza et al. (2025) show evidence in the housing market, Gianinazzi (2022) in mortgage refinancing decisions, in investment and consumption decisions (Pagel, 2017; Meyer and Pagel, 2022). Some consumers target round-numbered monthly payment amounts in disaster recovery loans (Collier and Ellis, 2024), mortgages (Ng, 2025), and auto loans (Argyle et al., 2020; Mateen et al., 2025; Momeni, 2024; Katcher et al., 2024). As shown in Argyle et al. (2020), such targeting behavior is inconsistent with classic economic models.

there may be offsetting responses (e.g., Guttman-Kenney et al., 2025), or unintentional harmful effects elsewhere in the financial portfolio (e.g., Medina, 2021). My contribution is to understand the economics and psychology of why consumers commonly pay at or just above the minimum, reconciling why payment behavior is difficult to shift using nudges but can shift with increases in minimum payments, in a way that is inconsistent with pure liquidity constraints. Given the combination of such preferences with frequently managing low liquid cash balances, it is understandable why it is hard to shift consumers to pay much more than the minimum, where there is a discontinuity in the economic incentive.

Finally, what are the implications of this work for anchoring more broadly? My work shows that researchers should be cautious before describing consumer behavior as anchoring. Anchoring effects appear robust in no- or low-stakes online experiments (Schley and Weingarten, 2025), whereas in economic markets with real incentives, initial eye-catching anchoring results, such as in Ariely et al. (2003), appear to diminish upon closer inspection (Fudenberg et al., 2012; Maniadis et al., 2014; Alevy et al., 2015), or be explained by alternative theories to anchoring (Guo, 2024; Bartels et al., 2024). These findings suggest that a richer understanding of consumer preferences and the psychology behind them may help to better understand otherwise puzzling economic behaviors.

The paper proceeds as follows. Section 2 explains the design of the experiment, the data, and the empirical methodology. Section 3 presents my results. Section 4 examines the heterogeneity in my results. Section 5 evaluates whether the evidence is consistent with anchoring or with targeting payment amounts and discusses how this can reconcile findings in the literature. 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. Nationally, 76% of credit card accounts are enrolled in mobile apps, with this method growing over time and becoming more widely used than online portals in 2024, and 57% of payments are made manually—i.e., non-autopay channels such as via app or online (Consumer Financial Protection Bureau, 2025).

The lender in the experiment is Arro, a FinTech that provides credit cards in the United States. Its customer base is primarily consumers with below-prime credit scores. 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 who 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 shown 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. But 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 total 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). Such prompts appear as standard across all major credit card lenders in the United States and other countries to prevent accidentally paying less than the minimum, enabling consumers to revise their initial payment choice. Sakaguchi et al. (2022) show in an online experiment how such prompts appear effective at preventing such choices, and Mrkva et al. (2026) documents how prompts are more broadly useful for consumers.

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) regarding 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 to 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. Shrouding the minimum payment without an alternative option may be socially optimal if there is a high degree of heterogeneity in consumers' socio-economic circumstances and preferences (Carroll et al., 2009).

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, which would otherwise be higher. Prior research on 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 involve 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 T1 to those in group C shows the effect of shrouding the minimum payment option, to target the higher payment options. Comparing outcomes in group T2 to those in group C 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 T1 to those in group T2 shows the effect of adding a 50% statement balance option, while holding constant the shrouding of the minimum payment 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 clickstream 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 savings account held by consumers, which I use to 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 the covariates (credit score, APR, card tenure, and credit limits) are balanced 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, a typical VantageScore credit score near 560 with consumers generally in the subprime or near-prime categories, and 41% of consumers being enrolled in autopay. In my data, 92% of the total value of payments is 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. Clearly, this data is not nationally-representative, however, we can still learn about consumer behavior from such rich but selected data sources, see Indarte et al. (2026) as one example. Nationally, approximately a quarter of consumers with credit cards have below-prime credit scores. Among these consumers, their total cost of credit—interest plus fees, as a percentage of statement balance—is typically 25% or higher. This is because consumers with lower credit scores have higher interest rates, lower repayment rates, and are more likely to incur late fees than those with higher credit scores (Consumer Financial Protection Bureau, 2025). Consumers with lower credit scores are those most likely to pay at or near the minimum payment in Keys and Wang (2019), who estimate no anchoring effects for consumers with superprime credit scores whereas for all credit score groups lower than superprime, they find similarly-sized estimates of anchoring. Therefore, the lack of superprime consumers in my study is not important for evaluating anchoring, and one might expect similar results for prime consumers as for near-prime consumers.

The main limitation of this data source is that the credit cards used 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 is paying the full statement balance amount (or more), follows a very similar pattern to market-wide data across lenders in the United States (Keys and Wang, 2016; Guttman-Kenney and Shahidinejad, 2025; Consumer Financial Protection Bureau, 2025). In addition, Guttman-Kenney et al. (2018) find 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., 2026; 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 from 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 the outcome Y for consumer i at time t. Because consumers are restricted to holding one credit card with the lender, card account-level data correspond directly 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 treatment 1 group ($T1$), and the term $T2_i$ is an indicator for the consumers in treatment 2 group ($T2$). The term D_τ is an indicator for event time τ, where $\tau = 1$ is the first full statement cycle after the launch of the experiment. 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. I still also report results for the original pre-registered specification.[8] 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.[9] This vector contains linear controls for age, credit score, card tenure, the pre-experiment values of 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).[10] 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:

[8] There are some different views on PAPs. For example Abrams et al. (2026) shows registrations are often too vague without PAPs, whereas Banerjee et al. (2020) suggest avoiding too strict adherence to PAPs.

[9] Goldsmith-Pinkham et al. (2024) highlight how multiple treatments with controls may, in theory, produce biased regression 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.

[10] 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.

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

I follow my PAP in clustering standard errors at the consumer level in my regressions.[11]

My primary outcome in my PAP is payments (%), defined as the sum of payments ($) divided by the statement balance ($). If payments (%) is greater than one, it is winsorized at a value of one, and it is assigned a value of one if the 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 are substantially larger. For example, in Guttman-Kenney et al. (2023), a nudge shrouding minimum payments causes an increase in hypothetical payments, as a percentage of the statement balance, of 12.4 percentage points.

I also present results for eleven secondary outcomes specified in my PAP. These consist of measuring payments in dollars, aspects of credit card behavior—spending in dollars and as a percentage 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. 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 and six completed statement cycles in Figure 1. Panel A of Figure 1 shows the distribution of payments as

[11] This may be conservative given how Abadie et al. (2023) explain that there is no 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."

a percentage of the statement balance after three completed statement cycles, and Panel B after six. Panel C shows, conditional on any payment being made, the distribution of excess payments after three completed statement cycles, and Panel D after six. Excess payments are the amounts paid in excess of the minimum payment amount and are normalized as a percentage of the statement balance less the minimum payment amount. In panels C and D of 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. Panels A and B of Figure 1 show a clear discontinuous increase in the likelihood of payments being at 50% of the statement balance, compared to just below this amount, for the control group C and T1, both of which display the 50% payment option. There is no discontinuity at 50% for T2, which does not display the 50% payment option to consumers. This descriptive evidence suggests that the 50% payment amount acts as a salient target for consumers when it is presented to them. The second small difference, noticeable in Panels C and D, 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 percentage 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 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 significantly less likely to pay exactly the minimum, with a 28% (4.6 percentage point) decrease on a control mean of 16.2%. Under T2, zero consumers choose 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 shown in column (6) of this table, making consumers 3.4 percentage points more likely to pay exactly 50% of the statement balance, without having any significant effects on the other consumer outcomes. These are robust after six cycles (Appendix Table A2).

3.2 Regression Analysis of Primary Outcome

Table 3 shows the results of the 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 displays the estimates from different regression specifications, starting with my regression that includes a constant and no controls in column (1), which follows my PAP for completeness. The results in this column are not robust to the inclusion of time fixed effects, as shown in column (2). Column (3) presents the results of the regression specified in Equation 1, which includes both time fixed effects and pre-experiment consumer controls. 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), which 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 while 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 not significantly different from zero, they are positive, which may suggest that there could be a small positive effect, but without sufficient statistical power to detect it. 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, as shown in columns (4) and (8) of Panel A of this table, where the 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 no significant positive effects on payments arising from either treatment, even for one cycle. Notably, even though the estimates for 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 the statistical 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, confirming that T1 does not increase payments. The dynamics of the other regression specifications are shown in Panel A of Appendix Figures A3, A4, and A5, with the only notable result being that when including consumer fixed effects (Panel A of Appendix Figure A5), 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. This attenuates to become 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 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 when pooling across cycles while 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, as shown in column 4 of Panel B 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, as shown in Panel A of Appendix Figure A5.

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 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 A3 shows results for secondary outcomes across the different regression specifications pooled across cycles, and Appendix Tables A4 and A5 do so for the dynamic estimates after three and six cycles, respectively.[12]

Table 4 and Figure 3 demonstrate that the results of my primary analysis are robust to examining payments in dollars, rather than normalizing them as a percentage of the statement balance. They are also robust to examining statement balance or revolving debt (statement balance less payments made against this balance, bounded at zero), with no significant reductions found. In fact, after six statements, shrouding the minimum payment amount causes payments in dollars

[12] Appendix Figures A3, A4, and A5 present estimates across cycles for the dynamic regression models without controls, with time fixed effects, and with fixed effects for both consumers and time.

to significantly decrease by \$9.3 (s.e. \$3.3), a 12% decline from the baseline mean, and revolving debt significantly increases by \$10.8 (s.e. \$5.3), a 7% increase from the baseline mean, as shown in panels A and E of Figure 3, respectively (see Appendix Table A5 for estimates). Adding the 50% option significantly increases statement balance by \$9.6 (s.e. \$4.5), representing a 4.1% increase over the control mean of \$231 after three cycles. This does not persist by cycle six (\$2.1, s.e. \$6.3, Appendix Table A5), 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, which 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 in subsequent 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 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 relative to 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, are 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. I find that adding the 50% payment option, comparing T1 relative to 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.

Relative to the control group, T1 and T2 make consumers no more likely to pay their statement balance in full. 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 by 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.

Panel A of Figure 4 shows precise zero effects of any of the treatments on dollar payment amounts slightly below the minimum. 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 the treatments leading consumers to round their payments up to the next whole number rather than choosing exactly the minimum payment. This makes sense, as it requires less effort than entering the exact minimum payment amount to the cent in the app.

There is no consistent evidence that these treatments lead consumers to make payments that are much larger than the minimum payment. Panel A of Figure 4 shows that 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 $\$21 < x \leq \$22$. Panel B of Figure 4 reveals that T1 makes consumers more likely to pay 50% of the 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% of the 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, as shown in Panel C of Figure 4.

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. 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 A7 shows that the distribution of payments in excess of the minimum across different cycles persists only up to cycle two. These results are consistent with Appendix Figure A6, which shows that 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 T1 (vs T2), 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 of $45\% < x \leq 55\%$, and more likely to pay in the range of $35\% < x \leq 45\%$. 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 A8 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 of Figure 4 by the significant negative coefficients at $11 more than the minimum, which is often a payment of $22 or two times the common minimum payment of $11. When the minimum payment is shrouded, consumers are 3.61 (s.e. 0.62) percentage points significantly more likely to instead choose to pay small, round-numbered amounts, as is also shown in Appendix Table A8.

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 my experiment, as it reveals the psychology that underpins consumers' payment choices. Consumers appear to rely on the heuristic of multiples of the minimum payment when this amount is salient, as demonstrated by Medina and Negrin (2022), which contributes to the results in Keys and Wang (2019). This is also documented in the control group of my study. But when the minimum payment amount is not salient, consumers heuristically choose round numbers, as in the treatment groups of my study (such round numbers are also common choices in other credit card data, Sakaguchi et al., 2026). Therefore, the overall effect of shrouding the minimum payment in other domains may be expected to depend on whether the dollar amount derived from using a heuristic of paying multiples of the minimum payment amount—or other heuristics such as rounding up or adding a small round number to the minimum payment amount—is greater than the dollar amount of small round numbers that consumers would otherwise choose.

3.5 Initial Payment Choices

I observe clickstream data that records each payment choice a consumer makes in the app, including their initial payment choice for each statement cycle and any revisions to it. Observing initial payment choices is especially important for evaluating anchoring. Anchoring relies on consumers placing too much weight on an initial piece of (irrelevant) information and insufficiently adjusting from this. In my treatments, the minimum payment is not an initial piece of information shown to consumers. If anchoring is lowering payments, as proposed in Stewart (2009) and Keys and Wang (2016), the distribution of initial payments should be shifted so that more payments are significantly higher than the minimum or near-minimum amounts otherwise chosen, resulting in a smoother distribution with less bunching, as observed in online experiments (e.g., Guttman-Kenney et al., 2023).

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. The initial payment choices of consumers can be especially informative for understanding consumers' ultimate decisions because they show how much a consumer wants to pay, without any distortion from the minimum payment amount. This is useful as it can help to understand in 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 during each statement cycle of 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 more likely to initially choose a lower payment amount, measured by adapting my primary outcome to be the *initial* payment choice (as a percentage 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 initial choices 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 the 50% payment option makes them 11.06 (s.e. 1.38) percentage points less likely to do so. One possibility is that consumers are so strongly anchored to paying the minimum that shrouding this amount is insufficient to overcome it. That explanation is inconsistent with the psychological notion of anchoring on irrelevant information. Instead, it may be more accurate to describe this possibility as consumers having developed a habit of paying only the minimum. Consumers may be hacking the nudge due to habituation to observing the minimum payment. Therefore, it is possible that a group of consumers outside the experiment, who have no prior experience with any credit cards or minimum payments, may respond differently.

The full distribution of payment choices shows how consumers initially want to pay less than the minimum payment amount but then revise their choices. Appendix Figure A9 shows CDFs of payment choices pooled across cycles. The solid lines denote consumers' first choices in a statement cycle, while the dashed lines denote their last choices in a statement cycle. The colors of the lines denote the control, T1, and T2, which is consistent with earlier figures. This figure shows that the initial payment choice is less than the minimum payment amount. A consistent pattern is shown defining payments in alternative ways in Appendix Figure A9.

It is common for consumers to first choose amounts that are zero, close to zero, or simply do not make a choice at all (which I classify as zero). T1 and T2 make consumers much more likely to make such very low payment choices (Panel A of Appendix Figure A9). While the control group shows a large spike at exactly \$11, a common minimum payment amount, this spike 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 Appendix Figure A9, 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 C corresponds to consumers initially choosing zero or near-zero payment amounts while having a minimum payment due of \$11. Panel E of Appendix Figure A9 shows the full distribution of payment amounts as a percentage 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 pay under 50% of the statement balance. In this figure, I classify consumers who attempt to click through without making any selection as zeros. The results are robust to excluding such zero or no-choice clicks, which may be warranted if these clicks contain accidental choices rather than purposeful attempts by consumers to hack the active choice nudge to reveal the minimum payment amount, as shown in Panel B of Table 5 and Panels B, D, and F of Appendix Figure A9. The effects are smaller in cycle zero (reflecting how some consumers have already made payments and therefore do not view the treatment that cycle) and are then stable across cycles one to six, suggesting that consumers quickly learn to hack the nudge (Appendix Figure A11).

These effects can be clearly seen in regression form in Panel B of Figure 5, which uses the initial payment choices (% statement balance) as outcomes instead of the 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 A10). 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.

Consumers revise their initial low choices, and across all three panels, there is a limited but 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 Figure 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 increases of 0.4 (s.e. 0.11) and 0.85 (s.e. 0.11) in the average total number of payment choices made per statement cycle, rising from 3.8 in the control group. Conversely, adding a 50% payment option reduces the number of choices made (Panel A of Table 5).

One potential explanation for consumers choosing low payments is a lack of financial sophistication, such as consumers not understanding exponential discounting (Stango and Zinman, 2009), which 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). If the cause of consumers choosing low credit card payment amounts is not understanding exponential discounting, then providing information to educate or debias consumers would be the natural solution. Yet, research consistently shows that disclosing information or providing informational nudges to consumers on credit card repayment times (or the costs of borrowing) does not significantly change their payment choices (Agarwal et al., 2015; Seira et al., 2017; Adams et al., 2022; Allen et al., 2026), even if it reduces confusion (Adams et al., 2022). Therefore, while a lack of financial sophistication may contribute to low payment choices, it cannot be the sole reason.

3.6 Autopay

Autopay enrollment does not explain my results. One potential reason for the null effects could be that if consumers pay via autopay, they may not even view the manual payment screen, and thus this may attenuate a positive effect on the subset of consumers not enrolled in autopay. I have already controlled 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 is made manually (the importance of manual payments for those enrolled in autopay is shown in Guttman-Kenney et al., 2025). Both autopay and non-autopay enrollees are sensitive to the shrouding of minimum payment amounts in online experiments (Guttman-Kenney, 2024).

Nevertheless, I can more rigorously evaluate this concern by excluding 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 enrollees pre-experiment, I find no significant increase in payments, as shown in Appendix Table A6. Among consumers without autopay, T1 actually significantly reduces payments in dollars after six cycles, however, there is no significant change in payments when measured as a percentage 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, but consumers often hold other credit cards with different 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, as I also 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. Instead, I 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 A7 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 might increase payments on other cards they hold. While this is possible, it is extremely unlikely to occur given prior research findings, and it 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 (e.g., Beshears and Kosowsky, 2020; Laibson, 2020; Guttman-Kenney et al., 2025), which, in my case, would be the card in the experiment. If the nudge increases payments on the card in the experiment (which it did not), this would generally be expected to either leave payments on other cards unchanged (for example, if consumers are not liquidity constrained and somewhat inattentive) or for the effect to be 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 make 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.[13] In both Adams et al. (2022) and Guttman-Kenney et al. (2025), credit card autopay nudges have no effect on increasing payments for the cards in the experiments and also have no effect on increasing payments across the portfolio of cards held by a consumer.

[13]The lack of actual payments information in U.S. credit reporting data means that I cannot estimate consumer use of the balance-matching heuristic.

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). 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 that 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 do 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 based on 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 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 the data. First, based on consumers' pre-experiment payment behavior on the card used 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 simple 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. A more rigorous way to evaluate this is to follow Keys and Wang (2019) in isolating a group of consumers who do not appear to be constrained: near-minimum payers. Consumers who pay just slightly more than the minimum, and therefore cannot have a binding constraint at the minimum payment amount, may be most susceptible to the nudge if their choices are driven by non-standard preferences.

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 having not paid in full for at least 50% of the six statement cycles leading up to the start of the experiment, and for consumers with less than six statement cycles, I calculate with the available data. My 18% near-minimum payer share 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 observed. To examine effects on these heterogeneous groups, I pool data across cycles to increase power.

If anchoring were the explanation here, I would expect payments for near-minimum payers to increase for both T1 and T2. That is not what I find in Appendix Table A9. The estimates for this near-minimum payment group are less precisely estimated than my main analysis, so they should be regarded with more caution. I find no evidence that shrouding the minimum payment significantly increases payments for these near-minimum payers, with the effect of T1 of 2.89 (95% C.I. -0.50, 6.28) and the effect of T2 of -0.56 (95% C.I. -3.58, 2.46) percentage points. There is also no significant effect on payments for the non-near-minimum consumers, which may be expected as it includes consumers who only pay the minimum, potentially due to being constrained and therefore will not pay more, as well as consumers who already pay in full and also will not pay more. Results are similar when checking their robustness to an outcome of payments, measured in dollars, where there is no evidence of shrouding increasing payments (Panel B of Appendix Table A9).

4.2 Credit Card Utilization

One common way to evaluate liquidity constraints is to determine whether credit cardholders' credit card balances are at (or close to) their credit limit (e.g. Gross and Souleles, 2002). Prior work (e.g. Agarwal et al., 2018; Aydin, 2022; Yin, 2026; Agarwal et al., 2025) has consistently shown that lender-driven credit card limit increases cause cardholders to increase their borrowing and spending. Importantly, these papers show that such responses occur for consumers who, before the limit increase, had balances far below their limit, and so were unconstrained in their borrowing. I allocate consumers into heterogeneous groups by their pre-experiment utilization, with full results in Appendix Table A10 which I briefly summarize.

I split consumers into tertiles based on their pre-experiment credit card utilization, calculated as the statement balance divided by the credit card limit. There are no significant effects of T1 for any tertile. For T2, I find one significant increase in payments for the lowest tertile of credit card utilization, increasing payments by 3.7 (95% C.I. 0.5, 6.8) percentage points. It is also not robust to measuring payments in dollars, where the estimate for this same group is insignificant: $-\$1.5$ (95% C.I. $-\$9.6$, $\$6.6$), relative to the control mean of \$80.5. When I examine heterogeneous treatment effects by tertiles of portfolio utilization rates, observed in linked Equifax credit reporting data, I find no significant effects of T1 or T2 at increasing payments, even for the least-utilized tertile. The fact that one significant increase is observed for low-utilized cards rather than for low-utilized consumers is weakly suggestive of consumers' payment choices not being fully coordinated across cards, potentially consistent with mental accounting (Thaler, 1999; Gelman and Roussanov, 2024; Katz, 2026).

Consistent with Lee and Maxted (2026), consumers in my experiment frequently have available credit based on their portfolio of credit cards. 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, a standard threshold for judging a consumer as being effectively constrained (Gibbs et al., 2025). This means that consumers are generally not experiencing a binding borrowing constraint.

My findings have important implications for macroeconomic models. Lee and Maxted (2026) document an inconsistency between the modeling approach used in heterogeneous-agent consumption models (e.g. Kaplan and Violante, 2022) and the data on consumer credit card use. Specifically, Lee and Maxted (2026) use data from the Survey of Consumer Finances (SCF) to show that consumers with approximately zero liquid assets frequently revolve intermediate levels of credit card debt and are therefore not at their borrowing limit (closely-related to this, Olafsson and Pagel, 2018 document hand-to-mouth behavior for consumers without binding liquidity constraints). This is inconsistent with hand-to-mouth consumers being at their borrowing limit in heterogeneous-agent consumption models. In the SCF, liquid assets are only observed at a single point-in-time and there is known underreporting of credit card debt (Zinman, 2009). Therefore, it is important to evaluate whether such data limitations affect their results. I avoid these issues by using granular administrative data on daily liquid assets and credit cards, and find evidence consistent with Lee and Maxted (2026). The empirical support for the theory of Lee and Maxted (2026) has broad implications, as it relies on present bias, which is well-documented in the credit card literature (e.g. Meier and Sprenger, 2010; Heidhues and Kőszegi, 2015; Kuchler and Pagel, 2021; Laibson et al., 2026), instead of hard borrowing constraints. This fundamentally affects how households respond to fiscal and monetary policy.

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 down more of their credit card debt, keeping cash that earns little to no interest while paying interest rates of 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. Morduch and Schneider (2017) write that "the mismatch of timing between income and spending is where families feel the most pain." I also show a more traditional point-in-time liquid cash balance. The choice of what constitutes a "high" amount of liquid cash available in such measures 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 (Gelman, 2022), including overconsuming due to naive present bias (e.g. Meier and Sprenger, 2010; Heidhues and Kőszegi, 2015; Kuchler and Pagel, 2021; Laibson et al., 2026; Lee and Maxted, 2026) 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% have point-in-time liquid cash balances of at least \$501. 63% have a minimum liquid cash balance of \$1 or less (i.e., negative if overdrawn).

Despite a very different sample and time period, these findings are consistent with Guttman-Kenney et al. (2025), who study consumers enrolling in autopay for a large U.K. bank that provides credit cards across the breadth of the credit card market, where only 11% have at least £1,001. It also appears consistent with Gathergood and Olafsson (2026), who document how co-holding cash and overdrafting in Iceland is substantially less common when using daily rather than monthly data. This suggests that such patterns of frequently holding limited liquid cash may reflect a more general phenomenon among consumers who revolve credit card debt. This is also consistent with the financial diaries study of Morduch and Schneider (2017) and Bhutta et al. (2023)'s analysis, which show how few consumers hold more than a few months of liquid assets, and broader research on earnings instability, which increases consumption risk (e.g., Ganong et al., 2025), and expenditure instability, with consumers frequently experiencing large expense shocks (Fulford and Low, 2026).

I find no evidence that there is a subgroup of consumers categorized 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 on payments (%), with similar results examining effects on payments in dollars (Appendix Table A11. Consumers with "low" minimum liquid cash balances do not increase their payments, which is expected since these consumers do not have the 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 does 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 and so a 95% confidence interval would contain economically meaningful positive and negative effects. 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 for the difference between the high and low minimum liquid cash groups being −3.4 to 14.7 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 in 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 criterion is met in their domain, and this may be sufficient to establish whether it is worthwhile to replicate this field experiment or to make policy.

5 Distinguishing Anchoring from Targeting

Putting these results together, it is clear that neither of the treatments that shroud the minimum payment amount has any substantial, lasting effects on consumers, beyond making them less likely to pay only exactly the minimum. This is in sharp contrast to the evidence from the effects of hypothetical payments in online experiments, which predict large increases in payments, attributed to anchoring to the minimum payment amount. I also find little substantive effect from 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. Examining initial payment choices provides evidence inconsistent with anchoring, as consumers initially choose amounts less than the minimum. 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 payoffs associated with paying the minimum or other amounts, so the economic trade-offs remain unchanged. The experiment varies the psychological salience of payment options and, therefore, can provide a test of anchoring.

The recent marketing literature of Bartels et al. (2024) and Herzog et al. (2025) distinguishes between anchors and targets and also provides an empirical test–Value Proximity Analysis (VPA)– for whether behavior around a focal value is best characterized as psychological anchoring or instead as targeting i.e., reference-dependent preferences. The idea behind this test is to examine the distribution of observations around a focal value, F. VPA plots the proportion of observations that are at or above a focal value, 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% of observations above the focal value 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 above the focal value, as consumers exert effort to achieve at least this value, and slope down as more observations are included. Anchors have a symmetric distribution around a focal value, whereas targets have an asymmetric distribution.

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

$$\begin{aligned} P(Y_w \geq F) =& \alpha + \beta^{T1}\, T1_w + \beta^{T2}\, T2_w + \gamma Proximity_w + \theta^{T1}\, (Proximity_w \times T1) \\ &+ \theta^{T2}\, (Proximity_w \times T2) + \varepsilon_w \end{aligned} \tag{3}$$

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, $Proximity_w$, to estimate how the relationship linearly varies with the proximity window, and it also includes interactions with T1 and T2 to allow for the slope to differ between C, T1, and T2. I use heteroskedasticity-robust standard errors. I run this regression separately for each outcome, $P(Y_w \geq F)$, where the outcome is the fraction of payments in a proximity window that are at or above a threshold, F. The three thresholds that I test to determine whether they act as an anchor or a target for consumers are: 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 three 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, there would be a negative slope for targets or high anchors, and a positive slope for low anchors. These first two predictions can only be established when there are no changes in economic payoffs that coincide with the focal value being tested. The third prediction can be tested: Under anchoring, both the intercept and the slope would significantly vary with the treatments that make particular amounts less salient, whereas if these are targets, the slope would not change significantly.

5.2 Value Proximity Analysis (VPA) Results

Table 7 shows the VPA regression results, and these are consistent with the underlying VPA data presented in Appendix Figure A12. My VPA results are consistent with the minimum payment amount acting as a strong target rather than as an anchor. 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 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 reference point for consumers to target paying, rather than an irrelevant anchor, makes sense given the strong economic incentives to pay at least this amount, most directly by avoiding late fees but also potentially avoiding a negative delinquency mark on their credit report (if they do not make up delinquent payments within thirty days), preserving their reputation with their lender, and maintaining access to their credit card for spending. It is also consistent with qualitative evidence in Katz et al. (2024), where consumers self-report paying at least the minimum. Consistent with my earlier evidence on initial payment choices, this evidence suggests that the asymmetric bunching of payments at or just above the minimum payment is driven by a discontinuity in the economic payoffs due to the minimum payment, which leads to payments, that would otherwise be lower, bunching at these amounts, rather than due to anchoring. Such a pattern is observationally similar to how discontinuous notches in other domains, such as tax schedules, distort incentives that lead to bunching in the distribution of choices (Kleven, 2016). As Kleven (2016) notes, "an issue that has received relatively little attention in the literature is that kinks and notches may represent reference points," and therefore my paper contributes to this.

The results from column 2 of Table 7 examine the VPA for 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 treatment, 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 intercept relative to the T1 intercept of 82.1% ($\alpha + \beta^{T1}$), and an insignificant flattening of the slope by 0.08 (s.e. 0.06) relative to the T1 slope of -0.63 ($\gamma + \theta^{T1}$) 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. If anything, it is reducing rather than increasing payments in the short-term, so it 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 VPA for the statement balance amount. The intercept is near 90%, and there is a significant negative slope with proximity. 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 the groups C, T1, or T2, it does become more prominent with the shrouding of alternative payment options in T1 and T2.

The statement balance amount may act as a second reference point to target rather than as an anchor, consistent with Schwartz (2025). These results may be consistent with consumers experiencing competing payment amounts, with consumers targeting at least to pay the minimum payment amount, and aiming to pay the statement balance.

5.3 Reconciling the Literature

Consumers with frequently low liquid cash balances targeting to pay at least the minimum payment amount on their credit cards can explain the results of my experiment. Such reference-dependent preferences can help researchers understand why information and nudges are unable to meaningfully change credit card payments (Agarwal et al., 2015; Keys and Wang, 2016; Seira et al., 2017; Adams et al., 2022; Guttman-Kenney et al., 2025; Batista, Mao and Sussman, 2025). Developing this idea further by considering the role of multiple reference points (O'Donoghue and Sprenger, 2018; Reck and Seibold, 2026) can also help to understand why intermediate payment amounts, such as the 50% payment amount tested in this paper and other payment amounts, including those disclosed in the CARD Act, are ineffective at changing behavior (Agarwal et al., 2015; Keys and Wang, 2016; Adams et al., 2022). This is because such intermediate amounts struggle to compete with the economically important minimum and full payment amounts, which have discontinuities in economic payoffs. The idea that the minimum and full payment amounts are two relevant reference points for consumers to target is consistent with Dean et al. (2026)'s theory of reference points as information, who write: "One of the advantages of thinking about reference dependence from an informational perspective is that it naturally allows for multiple reference points to play a role." Their theory predicts that "reference effects are most pronounced when value uncertainty is high and the reference point well-known," features that appear to accord with a consumer's decision of how much to pay on their credit card.

This preference-based explanation can also help to understand the results of Keys and Wang (2019), Medina and Negrin (2022), Castellanos et al. (2026), and Allen et al. (2026), which find that when minimum payments increase, some consumers pay more than the new higher minimum. These papers study the responses of "unconstrained" consumers, a group for whom the minimum payment is not binding, so a higher minimum does not mechanically force them to pay more. Yet, some of these unconstrained consumers still respond to higher minimums by paying more. Through which psychological channel might this occur? When minimum payments are higher, this reduces the distance to the full payment, which may make it appear as a more achievable psychological goal for consumers to target (Heath et al., 1999), similar to marathon runners' target times (Allen et al., 2017), pulling payments toward this higher amount.

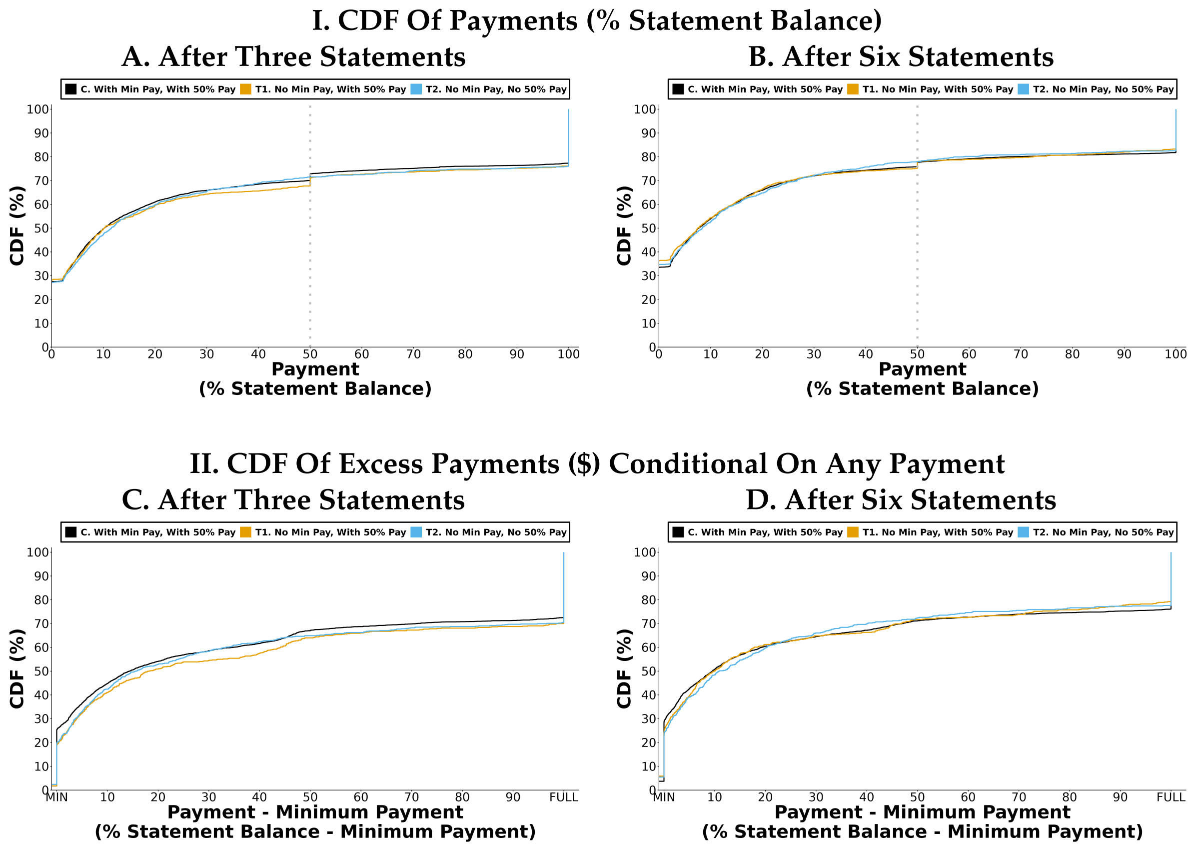
6 Conclusions

A large body of work across the fields of economics, finance, management, marketing, and psychology documents the importance of credit card anchoring in explaining 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 option does not increase payments. Instead of increasing low payments, this 50% option 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. When the minimum payment is shrouded, consumers initially choose amounts below the minimum, which is the opposite of what anchoring predicts, and in sharp contrast to the prior literature. Consumers are not at their borrowing constraints but do frequently have low liquid cash balances, leaving them with limited cash to pay more on their credit cards. Targeting to pay at least the credit card minimum payment amount appears to be how consumers manage their finances.

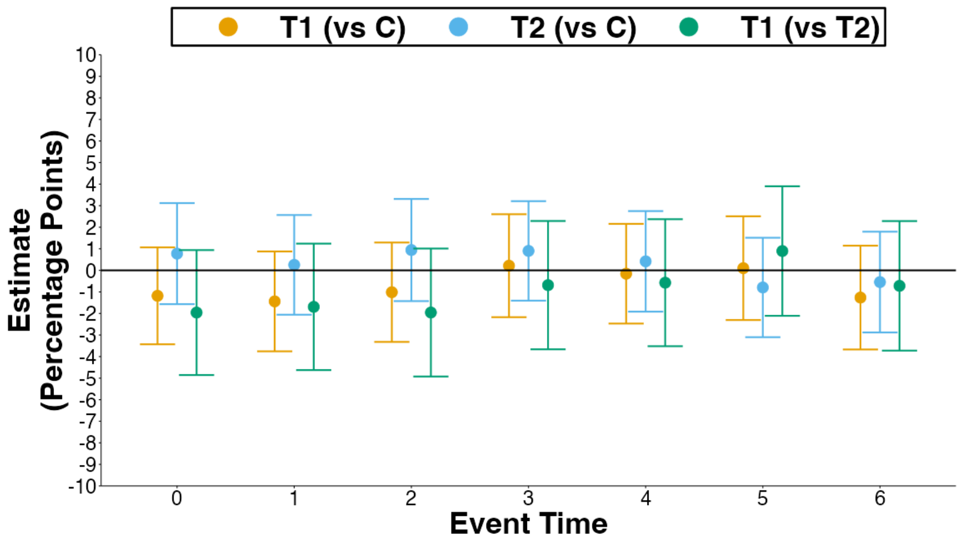
7 Figures & Tables

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



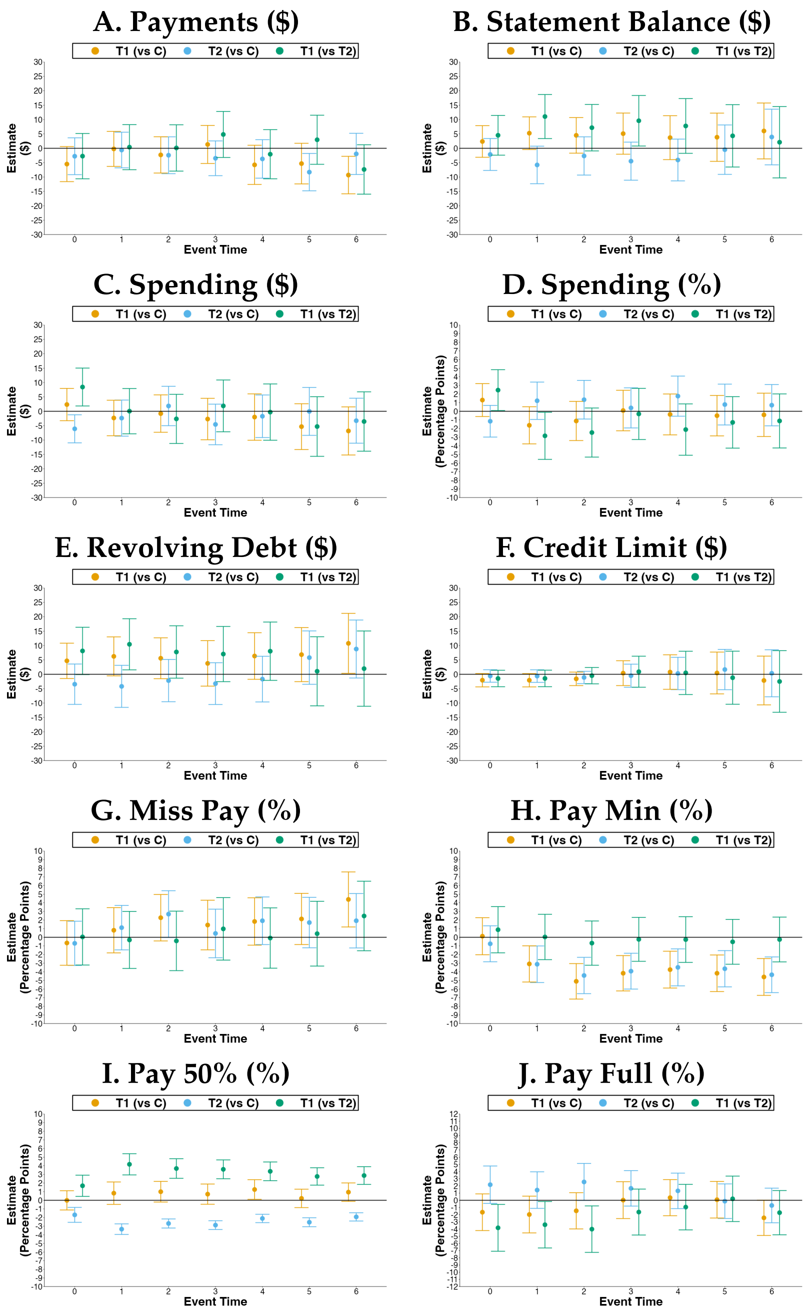
Notes: These panels show the CDFs of payments. Panels A and B show the CDF of payments as a percent of the statement balance. The dotted vertical line in Panels A and B denote 50% of the statement balance amount. Panels C and D show the CDF of excess payments, defined as the payments less the minimum payment (as a percent of the statement balance less minimum payment) conditional on consumers making any payment. In panels C and D, MIN denotes consumers that make exactly the minimum payment and FULL denotes those that pay their statement balance in full (or more). In all panels, each color shows the CDF for the control group 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 panels A and C, the outcome is measured after three completed statement cycles, and in panels B and D it is measured after six completed statement cycles. Panel A contains 6,693 consumers observed in cycle three, and Panel C is the subset of the consumers that make any payment in the third cycle, which is 4,592 consumers. Panel B contains 6,389 consumers observed in cycle six, and Panel D is the subset of the consumers that make any payment in the sixth cycle, which is 3,983 consumers.

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



Notes: The outcome in this figure 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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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).

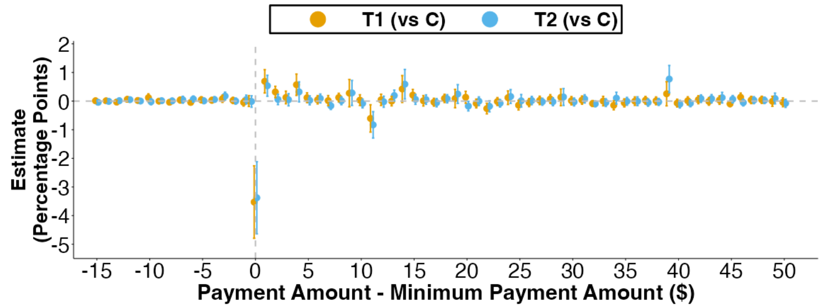
Figure 3: Dynamic Effects On Secondary Outcomes



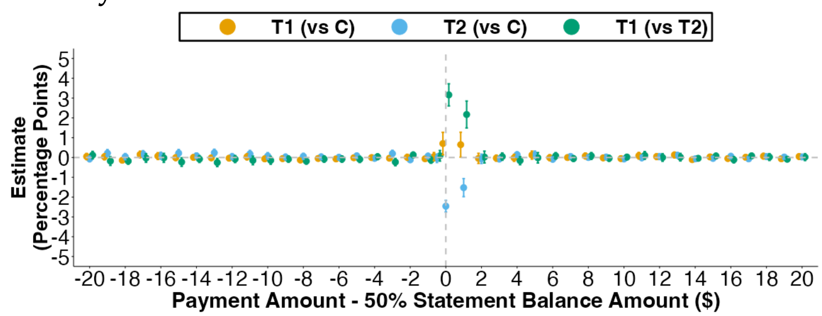
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from 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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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

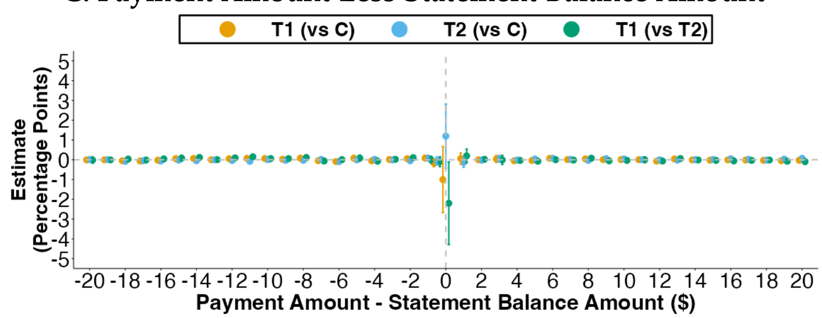
A. Payment Amount Less Minimum Payment Amount



B. Payment Amount Less 50% Statement Balance Amount



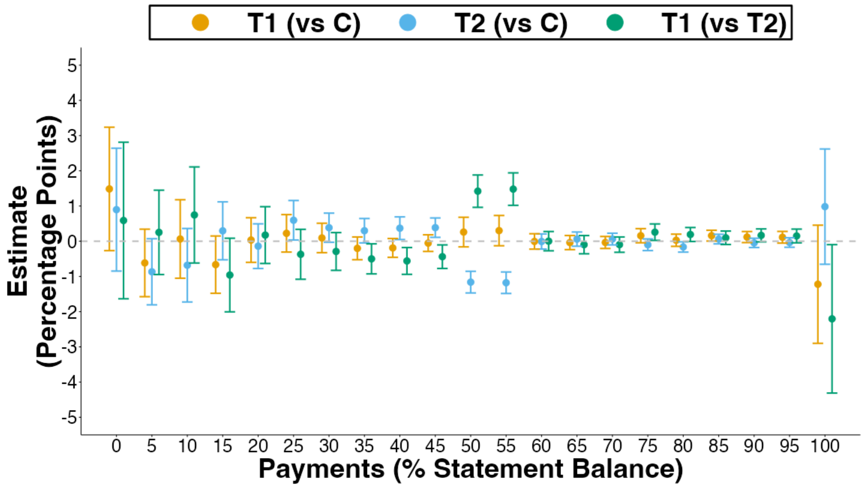
C. Payment Amount Less Statement Balance Amount



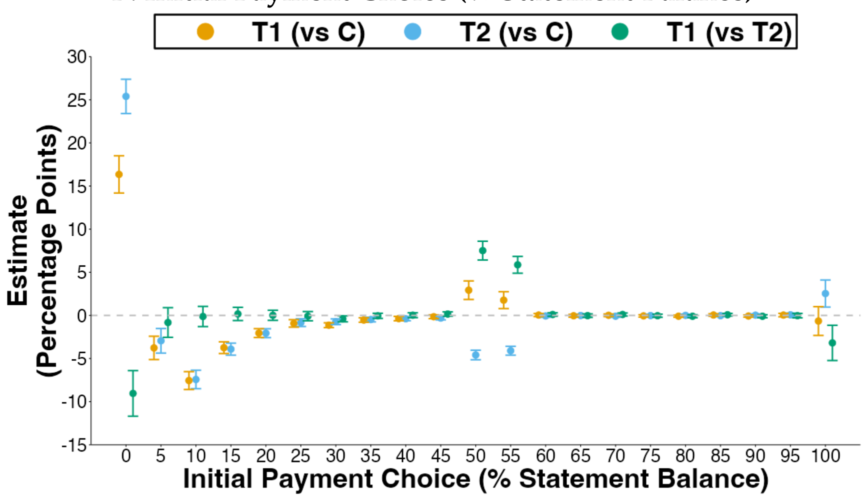
Notes: This figure shows the estimates on treatment indicators, showing the average effects pooled across statements, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression with treatment indicators for each of T1 and T2, 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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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

A. Payments (% Statement Balance)



B. Initial Payment Choice (% Statement Balance)



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 with separate indicators for T1 and T2, with the control group as the omitted category. In green are the estimates from a T1 indicator, 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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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.

Table 1: Pre-Experiment Summary Statistics

Outcome	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]	[0.850]
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	101.30	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

Outcome	C Mean (1)	T1 Mean (2)	T2 Mean (3)	T1 – C (4)	T2 – C (5)	T1 – T2 (6)
Payments (%)	32.92	34.40	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)
				[0.000]	[0.000]	[0.995]
Pay 50% (%)	2.72	3.35	0	0.63	−2.72***	3.35***
				(0.60)	(0.24)	(0.55)
				[0.293]	[0.000]	[0.000]
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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

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

A: Dynamic Estimates After Three and Six Statements

	Dynamic (δ_3^k)				Dynamic (δ_6^k)			
Comparison	(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)			
Comparison	(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, the pre-experiment values of 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). Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

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

Outcome	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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table 5: Regression Estimates On Initial Payment Choices

A: Initial Payment Choices

Outcome	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean
Initial Payment Choice (% Statement Balance)	-0.90	-4.33^{***}	3.43^{***}	26.83%
	(0.87)	(0.81)	(1.07)	
Pr (Initial Payment Choice < Minimum Payment)	19.44^{***}	30.50^{***}	-11.06^{***}	33.74%
	(1.16)	(1.02)	(1.38)	
Number of Payment Choices	0.40^{***}	0.85^{***}	-0.46^{***}	3.82
	(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

Outcome	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean
Initial Payment Choice (% Statement Balance)	-0.22	-1.82^{*}	1.60	30.19%
	(0.86)	(0.83)	(1.07)	
Pr (Initial Payment Choice < Minimum Payment)	15.59^{***}	23.57^{***}	-7.98^{***}	21.73%
	(1.08)	(1.04)	(1.38)	
Number of Payment Choices	0.10	0.35^{***}	-0.25^{*}	3.01
	(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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

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

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean	Consumers
Liquid Cash < $1	1.32	1.01	0.31	25.15%	1,025
	(2.28)	(2.22)	(2.97)		
Liquid Cash $1+	−1.36	0.20	−1.56	36.33%	5,689
	(0.88)	(0.86)	(1.10)		
Liquid Cash < $501	−0.46	0.70	−1.16	31.37%	3,796
	(1.06)	(1.06)	(1.34)		
Liquid Cash $501+	−0.80	−0.45	−0.35	38.78%	2,918
	(1.3)	(1.23)	(1.60)		
Liquid Cash < $1,001	−1.11	0.42	−1.53	32.11%	4,726
	(0.95)	(0.95)	(1.21)		
Liquid Cash $1,001+	0.25	−0.22	0.47	40.60%	1,988
	(1.58)	(1.50)	(1.96)		
Minimum Liquid Cash < $1	−1.47	0.68	−2.15	32.10%	4,230
	(1.02)	(1.03)	(1.31)		
Minimum Liquid Cash $1+	−0.25	−0.15	−0.09	38.99%	2,484
	(1.37)	(1.33)	(1.70)		
Minimum Liquid Cash < $501	−0.88	0.61	−1.49	33.64%	6,252
	(0.83)	(0.83)	(1.06)		
Minimum Liquid Cash $501+	1.50	0.43	1.07	48.04%	462
	(3.33)	(3.34)	(4.07)		
Minimum Liquid Cash < $1,001	−0.95	0.21	−1.16	33.94%	6,422
	(0.82)	(0.82)	(1.04)		
Minimum Liquid Cash $1,001+	1.77	5.87	−4.10	49.24%	292
	(4.46)	(4.55)	(5.77)		
Time F.E.	X	X	X		
Consumer Controls	X	X	X		

*Notes: The outcome in all regressions is the primary outcome: payments (% statement balance). 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 consumers 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 consumers 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. 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, the pre-experiment values of 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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

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 × T1	−0.005	−0.044	−0.012
	(0.041)	(0.057)	(0.052)
Proximity × T2	0.008	0.078	−0.016
	(0.042)	(0.062)	(0.050)
Observations	300	300	300
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. Heteroskedasticity-robust standard errors are in parenthesis. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

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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.[14] 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.[15]

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 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.

[14] 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."

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

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.[16]

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.[17] 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).

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

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

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

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

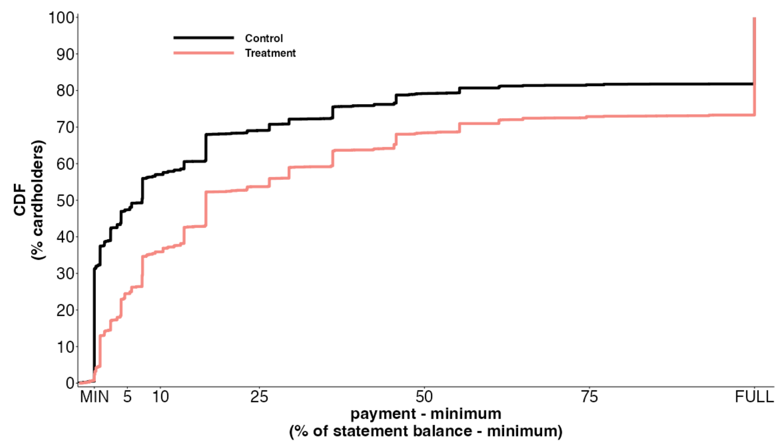
Based on data from June 2024, a sample of 2,200 consumers evenly split between a control and a treatment group would have sufficient power to detect a change in payments of 0.045, given a baseline mean of 0.235 and a standard deviation of 0.372. It would have sufficient power to detect a change in revolving 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

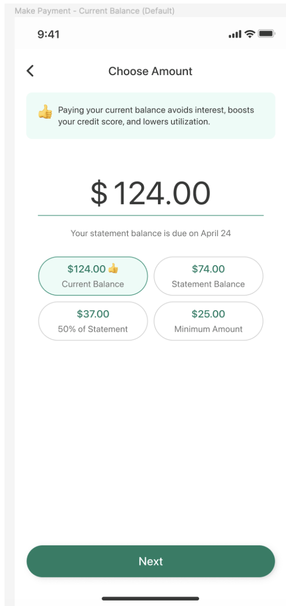
Figure A1: Example Of Effects Of Shrouding The Minimum Payment In A Hypothetical Credit Card Payment Experiment



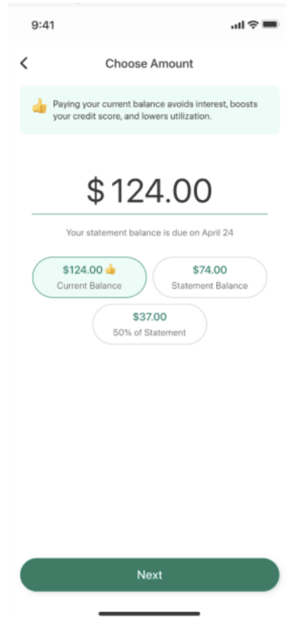
Notes: This figure is reprinted from Guttman-Kenney (2024), and shows the results of an online experiment testing the effect of shrouding the minimum payment amount on a sample of $N = 7,938$ consumers who have credit cards in the U.K. The figure displays the CDFs of the payment amount less the minimum 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

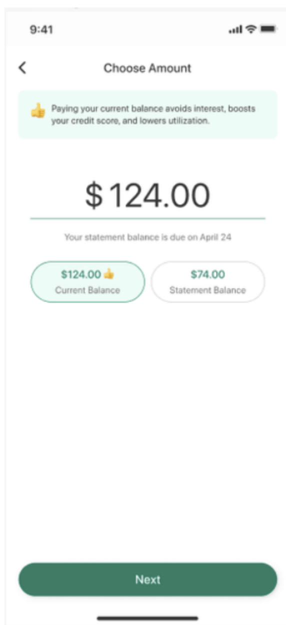
A. Control (C)



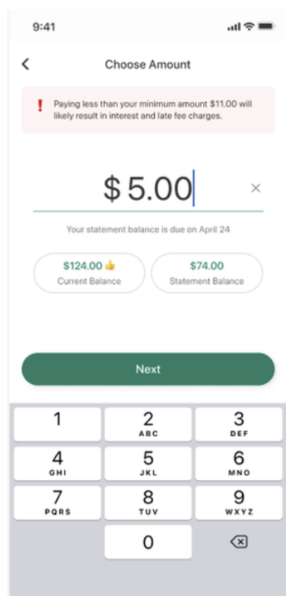
B. Treatment 1 (T1)



C. Treatment 2 (T2)

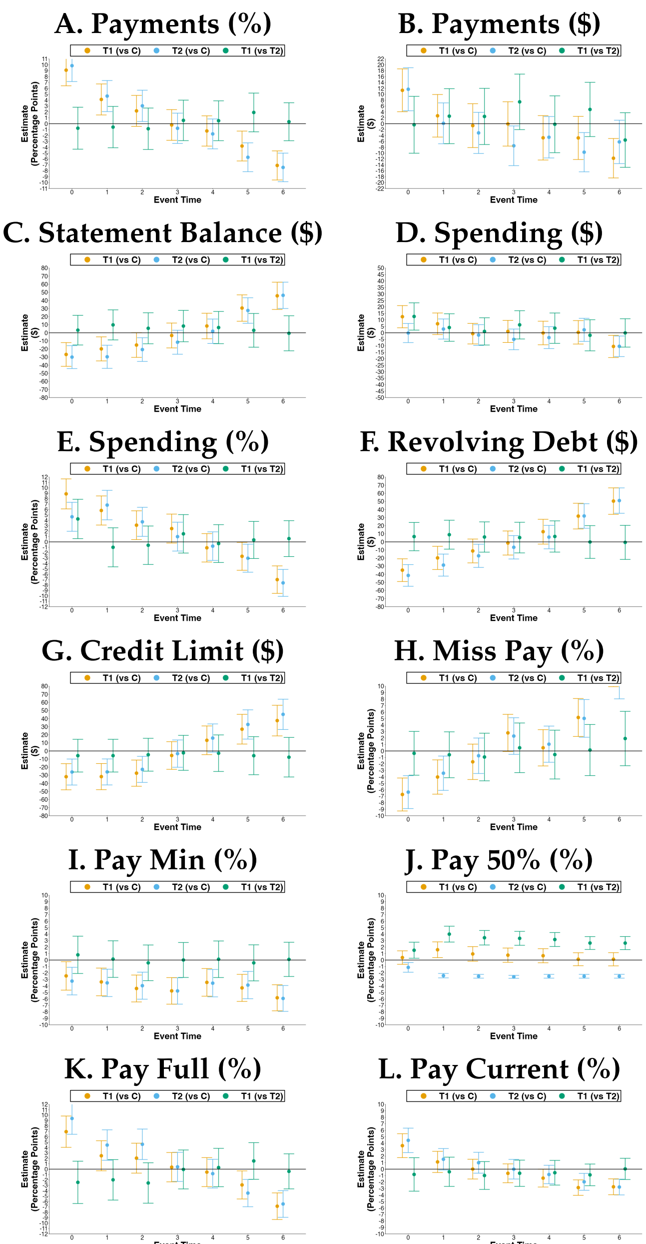


D. Prompt



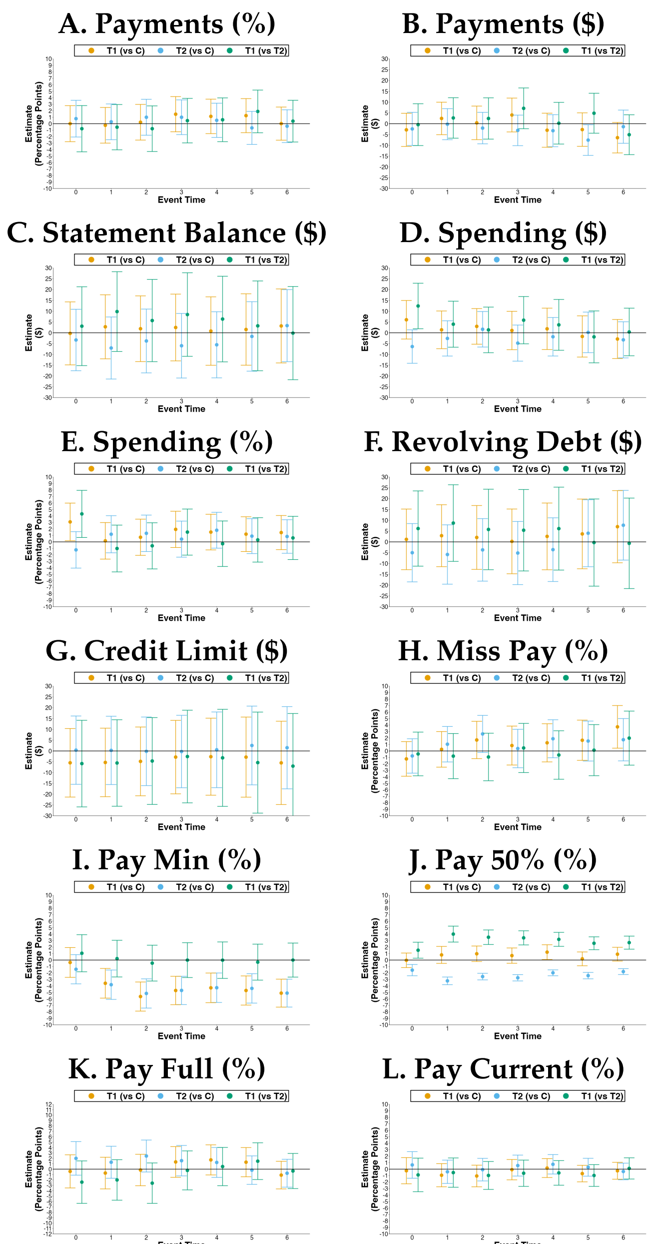
Notes: These screenshots show an example where the consumer has provisionally selected in the app to pay the current balance amount of $124.00, however, for clarity there are no options pre-selected as a default. The minimum payment warning prompt in Panel D only appears if a consumer enters an amount 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. In Panel D, there is a typo in this mock-up: the minimum payment amount should be $25 to be consistent with Panel A.

Figure A3: Dynamic Effects On Primary And Secondary Outcomes, No Time F.E.



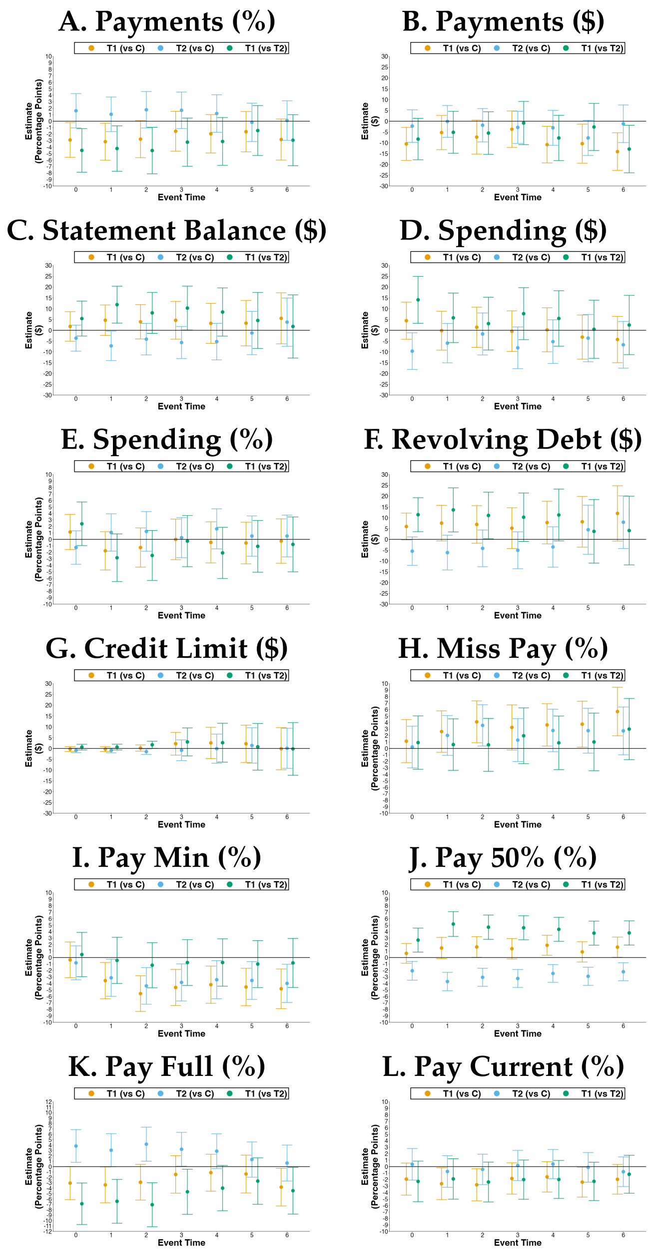
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from 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 A4: Dynamic Effects On Primary And Secondary Outcomes, With Time F.E.



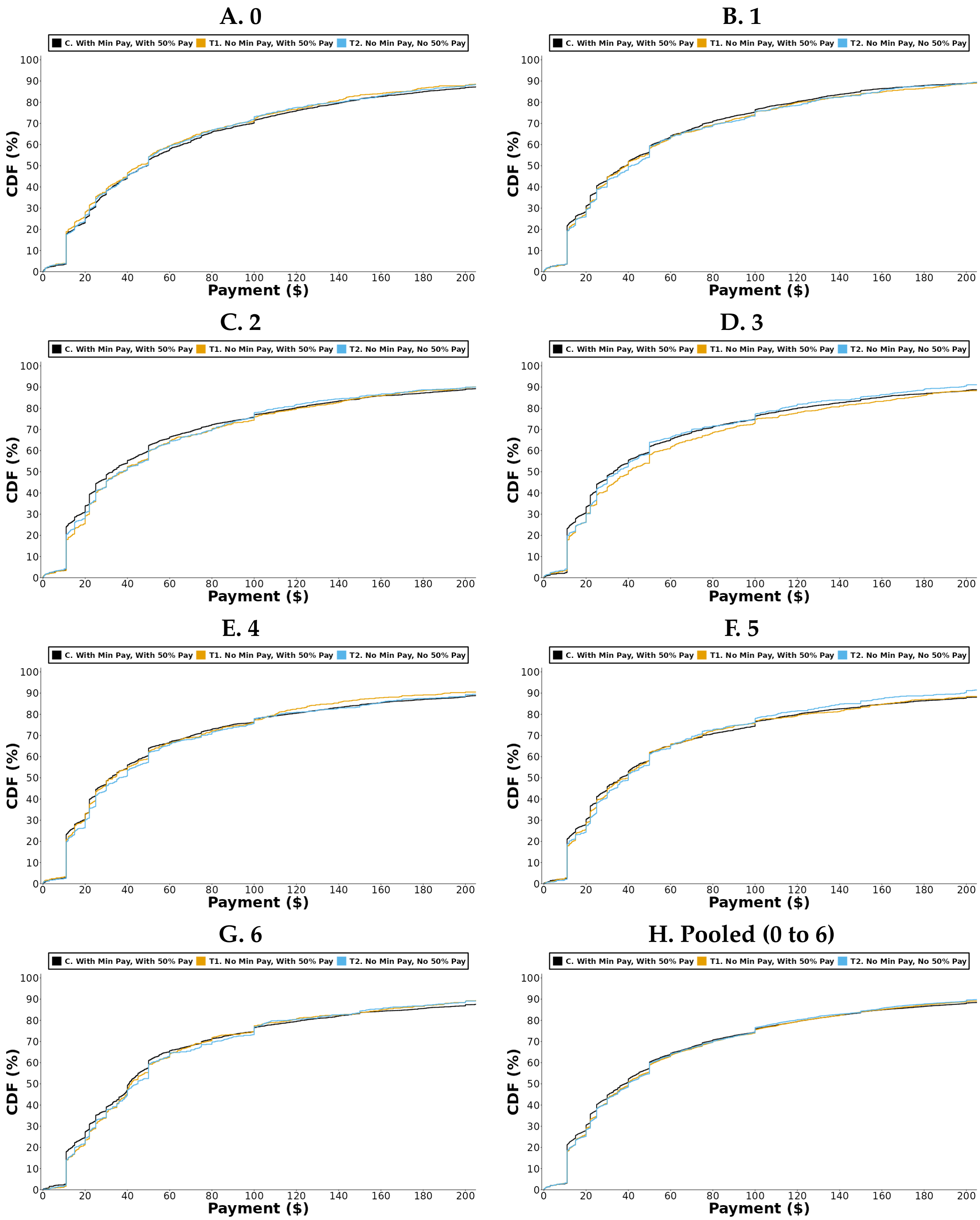
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from 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 A5: Dynamic Effects On Primary And Secondary Outcomes, With Time F.E. And Consumer F.E.



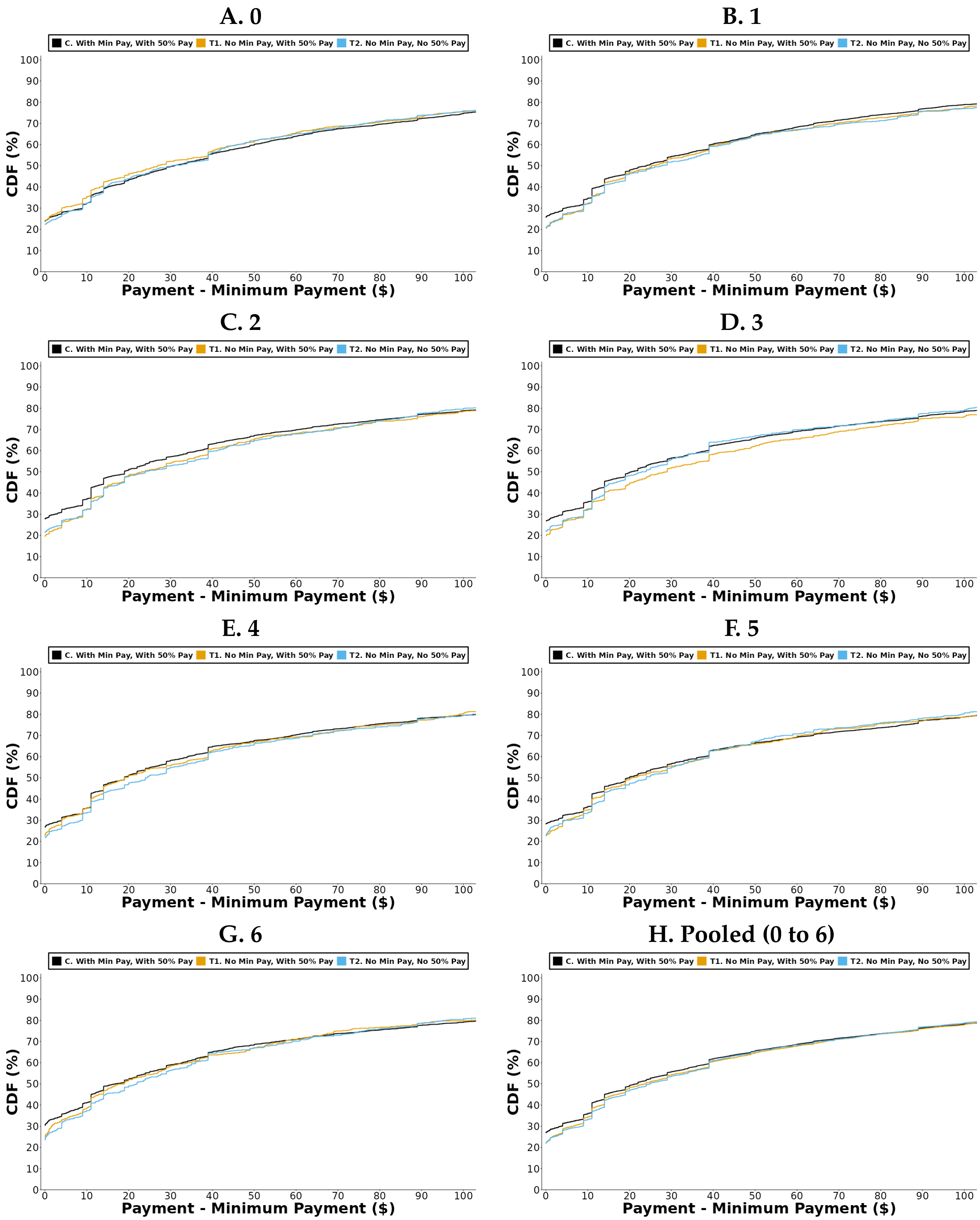
Notes: Each panel shows results for a different outcome. This figure shows the estimates from interactions between treatment indicators and time indicators, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from 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 A6: Distributions Of Payments ($) By Statement



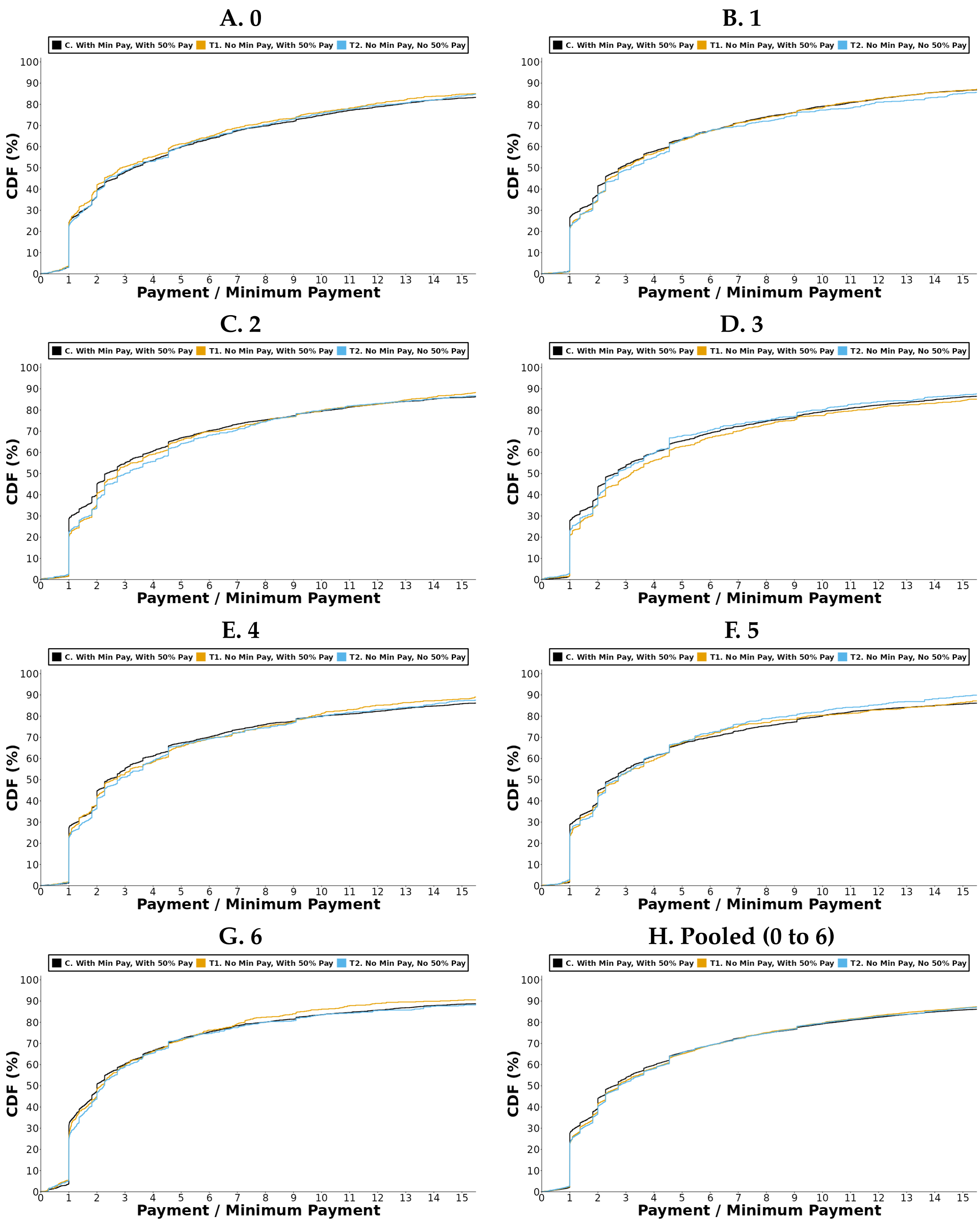
Notes: These panels show the CDFs of payments ($) across statement cycles. Panel H shows aggregating data across all cycles 0 to 6. The CDFs only includes consumers that make any payments in that cycle. In all panels, each color shows the CDF for the control group (C) in black, treatment group T1 (that 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 A7: 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 A8: Distributions Of Ratio Of Payments To Minimum Payment By Statement

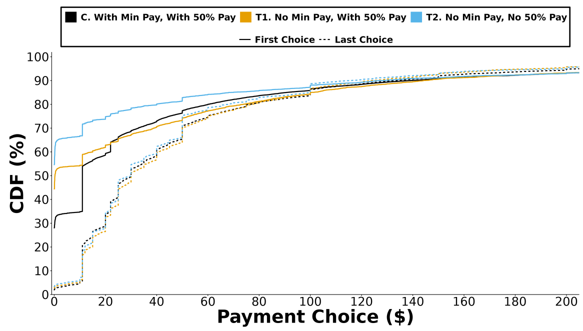


Notes: These panels show the CDFs of the ratio of payments to the minimum payment across statement cycles. Panel H shows aggregating data across all cycles 0 to 6. The CDFs only includes consumers that make any payments in that cycle and have a non-zero statement balance and a non-zero minimum payment. 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.

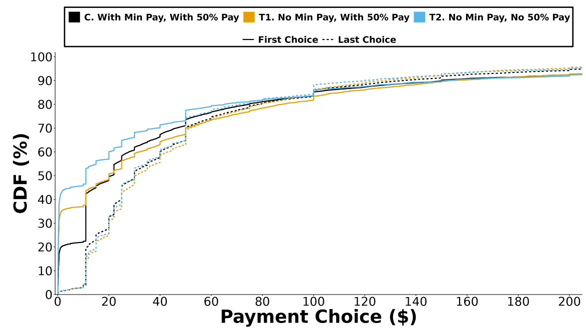
Figure A9: CDFs Of First And Last Payment Choices, With and Without Blank Choices And Choices Of Zero Dollars

I: Payments ($)

A: All Data

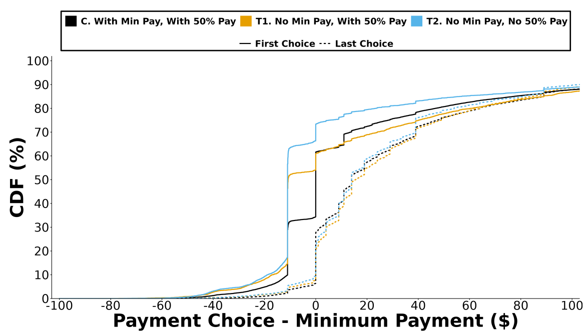


B: Excluding Blank Choices And Choices Of Zero Dollars

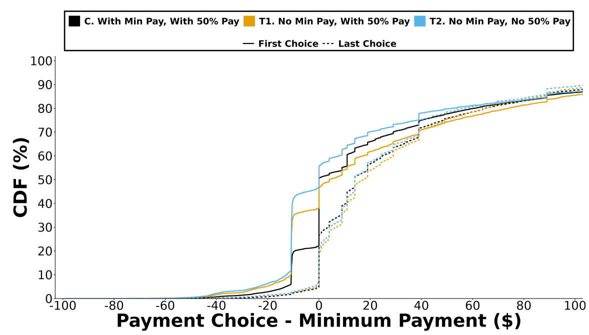


II: Payments − Minimum Payment ($)

C: All Data

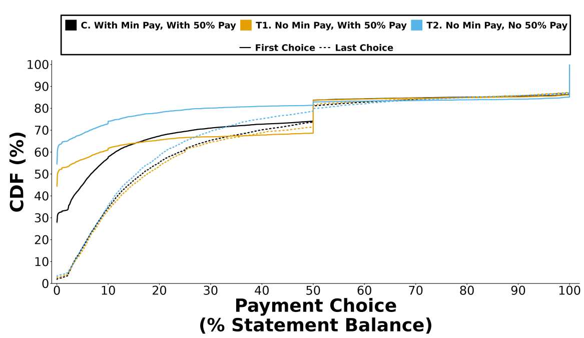


D: Excluding Blank Choices And Choices Of Zero Dollars

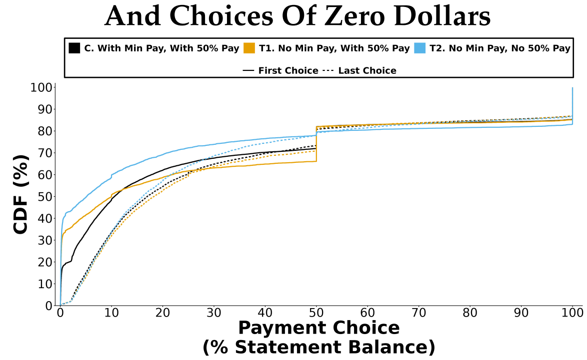


III: Payments (% Statement Balance)

E: All Data

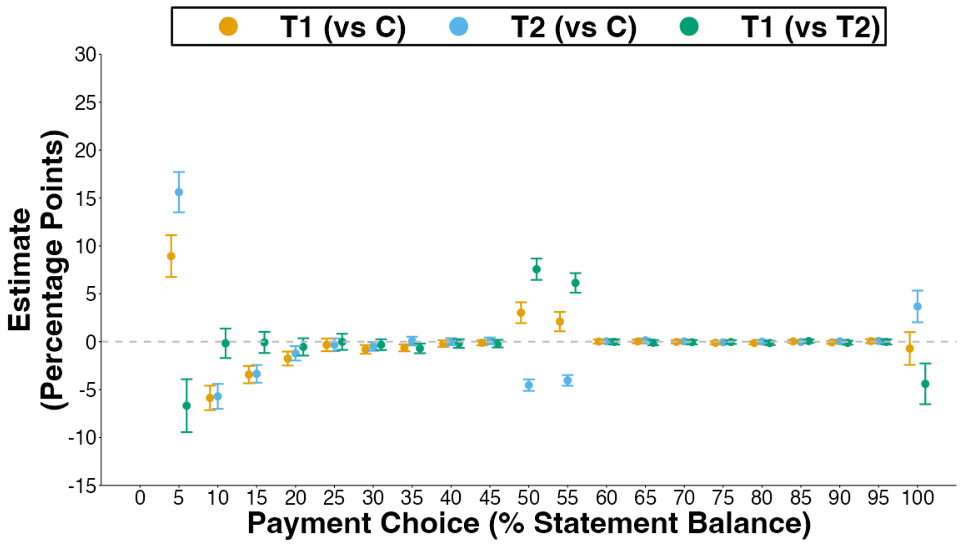


F: Excluding Blank Choices And Choices Of Zero Dollars



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. Panels A, C, and E isolate first and last choices in a cycle using data for all choices whereas B, D, and, F exclude blank choices and choices of zero dollars to isolate first and last choices in a cycle. Each panel shows CDFs calculated a different way, Panel I shows CDFs for payments in dollars, Panel II shows for excess payments (i.e., payments less the minimum payment), and Panel III 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.

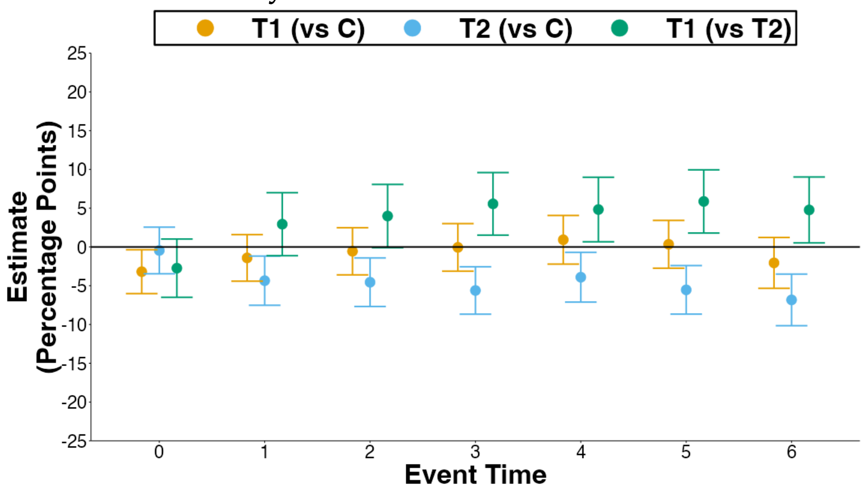
Figure A10: Effects On Payment Distribution Of Initial Payment Choice (% Statement Balance), Excluding Blank Choices And Choices Of Zero Dollars



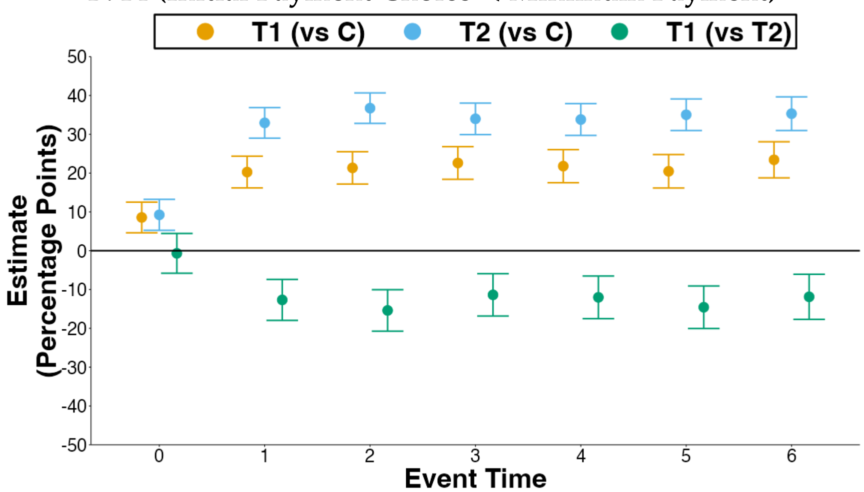
Notes: This figure shows the estimates on treatment indicators, showing the average effects pooled across statements, with 95% confidence intervals based on clustering standard errors at the consumer-level. In orange and blue are respectively the estimates from a regression where both T1 and T2 are each separately interacted with time indicators, with the control group as the omitted category. In green are the estimates from a T1 indicator, 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, the pre-experiment values of 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.

Figure A11: Dynamic Effects On Initial Payment Choices

A. Initial Payment Choice (% Statement Balance)

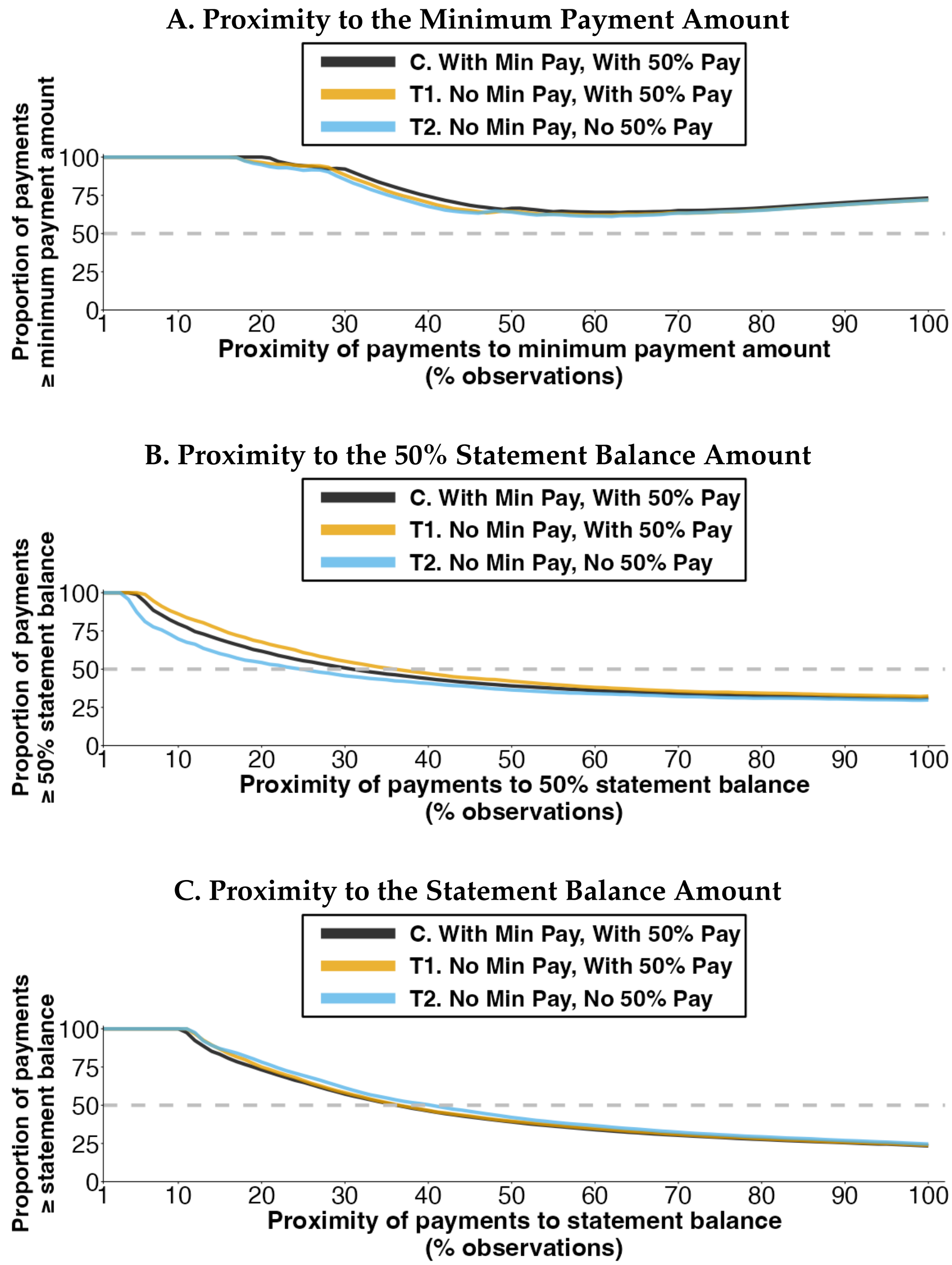


B. Pr (Initial Payment Choice $<$ Minimum Payment)



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 with separate indicators for T1 and T2, 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 a vector of pre-experiment consumer controls. The vector of consumer controls are linear controls for age, credit score, card tenure, the pre-experiment values of 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 5,644 consumers and $N = 25{,}928$ observations, the initial payment choices of consumers in the app each cycle (statement cycles 0 to 6). The units are percentage points.

Figure A12: 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 A1: Regression Estimates On Attrition, After Three and Six Statements

	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean
3	−0.23	−0.12	−0.12	0.37%
	(0.14)	(0.18)	(0.18)	
6	−0.09	0.00	−0.09	5.00%
	(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, the pre-experiment values of 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, the pre-experiment values of 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). Heteroskedasticity-robust standard errors are shown in parenthesis. The regressions use data on 6,714 consumers with one observation per consumer. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A2: T-Tests, After Six Statements

Outcome	C Mean (1)	T1 Mean (2)	T2 Mean (3)	T1 – C (4)	T2 – C (5)	T1 – T2 (6)
Payments (%)	27.75	27.52	27.19	−0.23	−0.56	0.33
				(1.31)	(1.29)	(1.65)
				[0.864]	[0.664]	[0.839]
Payments	58.41	51.53	57.07	−6.89	−1.34	−5.55
				(3.62)	(3.95)	(4.73)
				[0.057]	[0.734]	[0.241]
Statement Balance	278.41	282.85	283.45	4.44	5.04	−0.60
				(8.81)	(8.52)	(11.05)
				[0.614]	[0.554]	[0.957]
Spending	65.25	62.05	62.15	−3.20	−3.10	−0.10
				(4.59)	(4.28)	(5.59)
				[0.486]	[0.468]	[0.986]
Spending (%)	27.32	28.59	27.99	1.27	0.66	0.61
				(1.34)	(1.32)	(1.70)
				[0.346]	[0.616]	[0.721]
Revolving Debt	230.91	239.63	240.26	8.72	9.35	−0.63
				(8.59)	(8.25)	(10.74)
				[0.310]	[0.257]	[0.953]
Credit Limit	366.11	362.14	369.84	−3.97	3.73	−7.70
				(9.86)	(9.76)	(12.47)
				[0.687]	[0.702]	[0.537]
Miss Pay (%)	35.97	39.83	37.91	3.86*	1.94	1.92
				(1.69)	(1.67)	(2.14)
				[0.023]	[0.246]	[0.371]
Pay Min (%)	15.62	10.56	10.46	−5.06***	−5.16***	0.10
				(1.11)	(1.10)	(1.35)
				[0.000]	[0.000]	[0.940]
Pay 50% (%)	1.85	2.71	0.10	0.86	−1.76***	2.62***
				(0.55)	(0.23)	(0.52)
				[0.116]	[0.000]	[0.000]
Pay Full (%)	18.24	16.96	17.37	−1.28	−0.87	−0.41
				(1.31)	(1.31)	(1.66)
				[0.328]	[0.509]	[0.803]
Pay Current Balance (%)	4.06	3.78	3.74	−0.28	−0.31	0.04
				(0.67)	(0.66)	(0.84)
				[0.678]	[0.636]	[0.965]

*Notes: This table uses data for the consumers that are observed after six completed statement cycles. Columns (1), (2), and (3) respectively show the means for the control group C of 4,315 consumers, the Treatment 1 (T1) group of 1,042 consumers, and the Treatment 2 (T2) group of 1,032 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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A3: Regression Estimates On Secondary Outcomes, Pooled Across Statements

	T1 (vs C)		T2 (vs C)		T1 (vs T2)	
Outcome	(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, the pre-experiment values of 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, the pre-experiment values of 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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

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

		T1 (vs C)			T2 (vs C)			T1 (vs T2)	
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Payments	−0.07	4.03	−3.72	−7.50*	−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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A5: Regression Estimates On Secondary Outcomes, After Six Statements

	T1 (vs C)		T2 (vs C)		T1 (vs T2)	
Outcome	(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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A6: Regression Estimates For Subsample Without Autopay, After Six Statements

	T1 (vs C)		T2 (vs C)		T1 (vs T2)	
Outcome	(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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A7: Regression Estimates On The Portfolio Of Credit Card Statement Balances, Pooled Across Statements

Outcome	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean
Portfolio Statement Balances	36.55	6.60	29.94	\$3,681
	(51.85)	(53.41)	(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). Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A8: Regression Estimates On Heuristic Payment Amounts, Pooled Across Statements

Outcome	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean
Pay Multiples Of Minimum	-0.76^{***}	-0.94^{***}	0.18	3.48%
	(0.25)	(0.23)	(0.29)	
Pay Round Number	2.51^{***}	3.61^{***}	-1.10	9.58%
	(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 ∈ {\$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). Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A9: Heterogeneous Treatment Effects On Payments By Pre-Experiment Payment Behavior, Pooled Across Statements

A: Payments (% Statement Balance)

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean	Consumers
Near-Minimum	2.89	−0.56	3.45	22.18%	1,226
	(1.73)	(1.54)	(2.07)		
Not-Near-Minimum	−1.44	0.28	−1.72	37.42%	5,488
	(0.92)	(0.92)	(1.17)		
Time F.E.	X	X	X		
Consumer Controls	X	X	X		

B: Payments ($)

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean	Consumers
Near-Minimum	0.43	−2.41	2.84	$36.76	1,226
	(2.56)	(2.39)	(3.00)		
Not-Near-Minimum	−4.05	−3.79	−0.26	$69.24	5,488
	(2.56)	(2.53)	(3.23)		
Time F.E.	X	X	X		
Consumer Controls	X	X	X		

*Notes: Each panel shows estimates for a different outcome. In Panel A the outcome is payments as a percent of the statement balance, with the units in percentage point. In Panel B the outcome is payments in dollars, which are also the units. This table shows the estimates from treatment indicators, showing the heterogeneous effects pooled across statement cycles 0 to 6. Each row of this table shows estimates for a different 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 greater than or equal to 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). Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A10: Heterogeneous Treatment Effects On Payments By Tertiles Of Pre-Experiment Credit Card Utilization, Pooled Across Statements

A: Payments (% Statement Balance)

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean	Consumers
Q1 Card Utilization (< 60%)	−1.89	3.70*	−5.59**	52.23%	2,238
	(1.65)	(1.60)	(2.08)		
Q2 Card Utilization (60−91%)	1.57	−1.19	2.76	30.08%	2,238
	(1.40)	(1.31)	(1.72)		
Q3 Card Utilization (91%+)	−1.45	−1.30	−0.15	21.38%	2,238
	(1.15)	(1.17)	(1.44)		
Q1 Portfolio Utilization (< 12%)	−1.68	0.37	−2.05	33.24%	2,238
	(1.43)	(1.47)	(1.86)		
Q2 Portfolio Utilization (12−45%)	−2.31	0.94	−3.25	33.72%	2,238
	(1.43)	(1.40)	(1.83)		
Q3 Portfolio Utilization (45%+)	1.66	−0.96	2.62	36.85%	2,238
	(1.45)	(1.36)	(1.78)		
Time F.E.	X	X	X		
Consumer Controls	X	X	X		

B: Payments ($)

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean	Consumers
Q1 Card Utilization (< 60%)	−8.30*	−1.53	−6.78	$80.49	2,238
	(4.15)	(4.14)	(5.30)		
Q2 Card Utilization (60−91%)	0.01	−5.73	5.74	$58.93	2,238
	(3.31)	(2.99)	(3.90)		
Q3 Card Utilization (91%+)	−2.06	−0.13	−1.93	$50.19	2,238
	(3.12)	(3.33)	(4.02)		
Q1 Portfolio Utilization (< 12%)	−2.93	2.13	−5.06	$64.13	2,238
	(3.83)	(4.07)	(5.00)		
Q2 Portfolio Utilization (12−45%)	−4.57	−1.15	−3.43	$60.13	2,238
	(3.26)	(3.24)	(4.26)		
Q3 Portfolio Utilization (45%+)	−0.96	−7.82*	6.86	$65.48	2,238
	(3.95)	(3.61)	(4.81)		
Time F.E.	X	X	X		
Consumer Controls	X	X	X		

*Notes: Each panel shows estimates for a different outcome. In Panel A the outcome is payments as a percent of the statement balance, with the units in percentage point. In Panel B the outcome is payments in dollars, which are also the units. 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. 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. All regressions include time fixed effects for each statement end date and a vector of pre-experiment consumer controls (see the notes accompanying other tables for a list). In all of these regressions, standard errors are clustered at the consumer-level and are shown in parenthesis. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*

Table A11: Heterogeneous Treatment Effects On Payments ($) By Pre-Experiment Liquid Cash Balances, Pooled Across Statements

Sample	T1 (vs C) (1)	T2 (vs C) (2)	T1 (vs T2) (3)	Control Mean	Consumers
Liquid Cash < $1	−2.54	4.58	−7.13	$43.44	1,025
	(4.48)	(5.38)	(6.58)		
Liquid Cash $1+	−5.17*	−4.92*	−0.24	$66.85	5,689
	(2.42)	(2.29)	(2.99)		
Liquid Cash < $501	−3.44	−0.67	−2.77	$58.73	3,796
	(2.69)	(2.76)	(3.47)		
Liquid Cash $501+	−3.28	−5.15	1.86	$69.07	2,918
	(3.51)	(3.30)	(4.30)		
Liquid Cash < $1,001	−3.55	−1.94	−1.61	$60.45	4,726
	(2.49)	(2.49)	(3.18)		
Liquid Cash $1,001+	−1.86	−4.53	2.67	$69.94	1,988
	(4.24)	(3.90)	(5.10)		
Minimum Liquid Cash < $1	−4.37	−1.92	−2.45	$59.70	4,230
	(2.54)	(2.67)	(3.32)		
Minimum Liquid Cash $1+	−3.69	−6.22	2.53	$69.44	2,484
	(4.03)	(3.49)	(4.78)		
Minimum Liquid Cash < $501	−2.66	−2.59	−0.06	$62.05	6,252
	(2.26)	(2.18)	(2.84)		
Minimum Liquid Cash $501+	−15.50	1.52	−17.02	$79.79	462
	(8.53)	(8.83)	(9.97)		
Minimum Liquid Cash <$1,001	−3.49	−3.34	−0.16	$62.41	6,422
	(2.22)	(2.16)	(2.79)		
Minimum Liquid Cash $1,001+	−18.52	3.25	−21.77	$81.69	292
	(10.03)	(10.85)	(12.49)		
Time F.E.	X	X	X		
Consumer Controls	X	X	X		

*Notes: The outcome in all regressions is the outcome: payments ($). 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 consumers 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 consumers 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. 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, the pre-experiment values of 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. Statistical significance denoted at * 5%, ** 1%, and *** 0.5% levels.*