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THE SEMBLANCE OF SUCCESS IN NUDGING CONSUMERS TO PAY DOWN CREDIT CARD DEBT

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ABSTRACT

We run a field experiment and a survey experiment to study an active choice nudge. Our nudge is designed to reduce the anchoring of credit card payments to the minimum payment. In our field experiment, the nudge reduces enrollment in Autopaying the minimum from 36.9% to 9.6%. However, the nudge does not reduce credit card debt after seven payment cycles. Nudged cardholders tend to choose Autopay amounts that are only slightly higher than the minimum payment. The nudge lowers Autopay enrollment resulting in increasing missed payments. Finally, the nudge reduces manual payments by cardholders enrolled in Autopay.

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I Introduction

Credit card payments are often at or near the minimum due: 25% of payments in the UK (FCA, 2016a) and 29% in the US (Keys and Wang, 2019). Three mutually compatible reasons may explain the appeal of low credit card payments, including minimum payments. First, most credit cardholders underestimate how long it will take to pay off credit card debt if they only pay the minimum (e.g., Adams et al., 2022). Informational disclosures or nudges to address this bias are ineffective at significantly reducing credit card debt (e.g., Agarwal et al., 2015; Seira et al., 2017; Adams et al., 2022). Second, some credit cardholders anchor payments to the minimum (e.g., Stewart, 2009; Keys and Wang, 2019), which suggests a policy removing the anchoring effect ('de-anchoring') could reduce credit card debt. Third, credit cardholders with limited liquidity (whether this is the result of rational or behavioral mechanisms) are likely to choose to make low payments on their credit card debt.

We conduct a survey experiment and a field experiment on UK credit cardholders. We test whether an active choice nudge to de-anchor credit card payments from the minimum payment reduces credit card debt. In line with anchoring, our nudge is effective at shifting choices away from the minimum. However, in line with limited liquidity, the nudge is ineffective in changing the amount of credit card debt. We find that credit cardholders responses to the nudge make it ineffective. We also show that credit card payment behavior is strongly correlated with a new dynamic measure of liquid cash balances.

A mechanism facilitating low credit card payments is the FinTech feature called 'Autopay' in the US or 'Direct Debit' in the UK. Autopay is a common payment mechanism used across non-financial (e.g., cell phones) and financial (e.g., autos, mortgages) products. Some FinTech credit products, such as 'buy now, pay later' in the US, require users to enroll in Autopay (CFPB, 2022). For credit cards, enrolling in Autopay is an opt-in choice. Cardholders choosing to enroll in Autopay are presented with three options: automatically paying exactly the minimum amount due each month ('Autopay Min'), automatically paying a fixed amount each month ('Autopay Fix') where the automatic payment is the maximum of a fixed amount and the minimum due that month, and automatically paying the full balance due on the statement each month ('Autopay Full'). These three Autopay options are standard in the UK and US. Autopay is used by 42% of UK cards (FCA, 2016a) and 20 to 38% of US cards (CFPB, 2021), with growing use over time.[1] Cardholders enrolled in Autopay can also make supplemental, non-Autopay ('manual') payments (e.g., online, phone).

Persistent minimum payments and high credit card interest costs are concentrated among cardholders enrolled in Autopay Min. 75% of consumers in 'persistent credit card debt'

[1] US estimates are more uncertain because they are based on consumer self-reports (CFPB, 2021).

(using a regulatory definition of making nine or more minimum payments in a year on interest-bearing cards) are enrolled in Autopay Min (FCA, 2016a). Consumers who switch into Autopay Min pay more in credit card interest than they save in reduced late payment fees (Sakaguchi et al., 2022). The 20% of UK credit cards enrolled in Autopay Min account for 43% of total interest and fees across all UK credit cards (Sakaguchi et al., 2022).

Are credit cardholders enrolled in Autopay Min subject to anchoring? We advance prior research by studying anchoring effects by Autopay enrollment observed in linked administrative data. We conduct a survey experiment (N = 7,938) testing a treatment removing the visibility of the minimum payment on a hypothetical credit card online payment screen. De-anchoring minimum payments increases hypothetical credit card payments by 12 percentage points. Credit cardholders enrolled in Autopay Min appear to be subject to anchoring to the minimum payment similarly to cardholders not enrolled in Autopay.

We attempt to exploit anchoring effects with a pre-registered field experiment (N = 40,708) testing a nudge designed to increase credit card repayments on real credit card accounts. Our nudge has never been tested before. For consumers in the nudge treated group, we remove the minimum payment as a visible and salient anchor for cardholders enrolling in Autopay at card opening. We do so by removing the explicit appearance of the Autopay Min option for the nudged treated group. Autopay Fix and Autopay Full remain visible options for both control and treatment groups. Autopay Min remains a feasible choice for consumers if they actively chose a low Autopay Fix amount that binds at the minimum. By shrouding the Autopay Min option we increase the salience of the Autopay Fix option which enables an active choice and would automatically amortize debt faster (assuming no other behavioral changes).

This field experiment is an ex-ante test of a potential nudge that the UK consumer financial protection regulator – the Financial Conduct Authority (FCA) – was considering implementing, in light of regulatory concerns about the substantial amounts of UK credit card debt (FCA, 2014, 2016b). This field experiment is conducted on cardholders who have self-selected to come to the Autopay enrollment web page as these are the policy-relevant population. We measure outcomes in credit card and credit file administrative data.

This de-anchoring nudge reduces Autopay Min enrollment from 36.9 percent of the control group to 9.6 percent in the nudged treatment group: a 74% decline. The nudge increases Autopay Fix enrollment by 73%. We also conduct a field experiment of the same de-anchoring nudge with a second lender but after observing similarly large treatment effects on Autopay enrollment this second lender withdrew before fieldwork was complete.

We follow cardholders over at least seven months and find that our de-anchoring nudge does *not* change credit card debt. We observe null effects, on average, on credit card debt as well as spending, total payments, and borrowing costs after seven completed credit card cycles on the specific card in the trial and across a consumers' entire portfolio of credit cards. It causes the likelihood of only paying exactly the minimum to fall by seven percentage points (23%) but consumers are no more likely to pay the full balance. These effects are persistent over time. Such null results are critical policy inputs (Abadie, 2020) especially when the null effects on real outcomes contrast to the large effects on Autopay enrollment outcomes. While our de-anchoring nudge harnesses psychological insights to change enrollment choices, it does not change economic outcomes of ultimate importance to policymakers.

Our results demonstrate the importance of considering how the effects of nudges are evaluated. If a policymaker only observes the effects of the nudge on the composition of Autopay enrollments, it may appear effective: we estimate it would be expected to translate into reducing debt by approximately 4.5%. Whereas examining the effects on debt reveals the nudge is ultimately ineffective. Our study contributes to a broader debate on the effects of nudges (e.g., Thaler, 2017; Laibson, 2020; Chater and Loewenstein, 2022). DellaVigna and Linos (2022)'s meta-study documents the heterogeneous effects of nudges and provides evidence for publication bias.[2] Across financial domains nudges can shift enrollments but consumers may also subtly counteract these effects. For example, Choukmane (2021) finds the long-run effects of automatic enrollment defaults on savings are smaller than short-run contribution increases found in the earlier, academic literature (e.g., Madrian and Shea, 2001; Thaler and Benartzi, 2004). Some nudges are still highly effective even when potential countervailing effects are measured (e.g., Chetty et al., 2014; Beshears et al., 2022), whereas some nudges may have adverse side effects (e.g., Medina, 2021).

We investigate the mechanisms that cause the enrollment effects of our de-anchoring nudge in our field experiment to be undone so that the effects on economic outcomes are not statistically significant. We find three factors explain why the de-anchoring nudge is ineffective. First, nudged cardholders set up fixed Autopay amounts that are only modestly higher than the minimum payment due, and in the long-run, essentially no higher than the minimum payment because the minimum payment rises mechanically as card balances rise over time. Second, nudged cardholders are less likely to enroll in Autopay, causing more missed payments relative to the cardholders who are not nudged. Third, nudged cardholders enrolled in Autopay make lower manual payments.

Limited liquidity can partially explain why consumers do not reduce their credit card debt. For a selected subsample of our field experiment, we observe daily liquid cash balances

[2] DellaVigna and Linos (2022) show the average effect among academic published studies of nudges is 8.7 pp (33.4% increase in take up) whereas the average effects from the population of studies from Behavioral Insights Teams are smaller: 1.4 pp (8% increase).

from bank account data linked to our credit card data. In the UK, it is common for checking accounts to have an overdraft line of credit facility, so liquid cash balances can be negative. We use these linked data to construct a new dynamic measure of liquid cash balances: the *minimum* liquid cash balances in the last ninety days. This dynamic liquidity measure reveals that approximately 50% of consumers in our linked data have negative liquid cash balances at some point in the last 90 days, compared with just 10% using a traditional static measure of liquid cash balances. Our new measure strongly predicts subsequent credit card repayment decisions. Consumers with small, positive minimum liquid cash balances (before card opening) repaid approximately 20 percentage points more of their credit card debt seven cycles later than those with small, zero or negative minimum liquid cash balances.

II Survey Experiment on Anchoring

Social scientists have documented that consumer choices are influenced by anchoring effects (e.g., Mussweiler et al., 2000). Sunstein and Thaler (2008) write "Credit cards minimum payment...can serve as anchor and as a nudge that this payment is an appropriate amount." This conjecture has been supported by a series of empirical studies (e.g., Stewart, 2009; Keys and Wang, 2019; Medina and Negrin, 2022).

II.A Survey Experiment on Anchoring: Design

We conduct a survey experiment testing whether UK credit cardholders enrolled in Autopay anchor to the minimum payment.[3] Prior research does not examine anchoring effects by Autopay enrollment (e.g., Autopay enrollment is unobserved in Keys and Wang, 2019 and Medina and Negrin, 2022). We observe Autopay enrollment in administrative data linked to survey responses.[4] This survey experiment was not pre-registered.

Survey respondents were shown an online credit card payment screen, asked to imagine this was their actual bill and, considering their actual financial situation, report how much they would hypothetically pay. The survey generated 7,938 responses and these are linked to administrative data on credit card behaviors.[5] This is a relatively large and externally valid sample compared to prior studies in this domain that use platforms such as MTurk (e.g., Stewart, 2009; Navarro-Martinez et al., 2011; Salisbury and Zhao, 2020; Sakaguchi et al.,

[3]Participation is incentivized through a prize draw with two £500 and fifteen £100 Amazon gift vouchers.

[4]Our earlier working paper (Guttman-Kenney et al., 2018) contains more analysis for respondents not enrolled in Autopay. This includes comparing hypothetical responses to actual credit card behavior in these respondents' administrative data.

[5]We remove respondents who were inactive on their credit card or in the survey experiment's pilot.

2022). Our survey response rate is 6.7% which is low on an absolute basis, but comparable to other surveys such as the FRBNY's Survey of Consumer Expectations which has 3,853 respondents and a response rate of 6% (Armantier et al., 2017).

Respondents to the survey experiment were randomized across two statement balance amounts: the 25^{th} (£532.60) and 75^{th} percentiles (£3,217.36) of the overall distribution of actual statement balances. We also randomized balance amounts given a wide heterogeneity in credit card balances and it is possible anchoring effects would vary with balances.

Respondents were also randomized across control and treatment groups. The control group's screen design (Internet Appendix Figure A1) shows the options cardholders observe in their online manual payment screen: an option to pay in full, an option to pay the minimum amount due, and an option to pay a specific amount they can choose. For the control group, the statement balance amount and minimum payment amount are both presented.

The treatment group's screen (Internet Appendix Figure A1) does not show the minimum amount due or have a radio button with which they can pay the minimum. This removes one of the two passive options: the minimum amount due has been removed, leaving full payment as the only passive option. If the respondent does not want to make the full payment, they are forced to make an active choice (Carroll et al., 2009) of how much to pay, which is de-anchored from the minimum.

In both control and treatment groups, if a respondent entered an amount less than the minimum amount due, a prompt appeared that showed the minimum amount due and asked the respondent to re-enter their payment amount. After being prompted once, the respondent was allowed to choose to pay an amount less than the minimum amount due. This sequence of prompts replicates the actual online experience of cardholders.

II.B Survey Experiment on Anchoring: Results

In our survey experiment, we find evidence of anchoring to the minimum payment – conceptually replicating prior lab studies (e.g., Stewart, 2009; Navarro-Martinez et al., 2011; Salisbury and Zhao, 2020; Sakaguchi et al., 2022). Figure 1 shows the distribution of hypothetical repayment choices in our experiment as measured by 'payment - minimum (% of statement balance - minimum)' to normalize payment amounts relative to the minimum across balance scenarios. These are grouped by Autopay enrollment status in respondents' actual credit card administrative data. The solid lines show the control groups and the dotted lines show the de-anchored treatment groups.

The de-anchoring treatment makes respondents significantly less likely to pay exactly the minimum payment, more likely to pay in full, disperses the distribution of payments away from being anchored at or near to the minimum, and makes respondents no more likely to pay less than the minimum (Internet Appendix Figure A2 and Table A1). These survey experiment results are consistent with anchoring effects found in administrative data by Keys and Wang (2019) and Medina and Negrin (2022) who examine how credit cardholders' repayments change in response to lenders changing their minimum payment formulae.[6]

We find our de-anchoring treatment has no statistically significant effect on respondents enrolled in Autopay Full: with a treatment effect on payments of -1.4 pp of the statement balance (95% C.I. with a 95% C.I. of -7.5 to 4.8 pp). For other respondents, the de-anchored choices in the treatment are significantly different from the anchored choices in the control. The largest de-anchoring treatment effect on payments is for respondents enrolled in Autopay Min: 17.3 pp estimate with a 95% C.I. of 13.8 to 20.9 pp. Effects are similar for Autopay Fix enrollees (14.6 pp treatment effect on payments with a 95% C.I. of 11.5 to 17.6 pp), and those with No Autopay enrollment (11.7 pp treatment effect with a 95% C.I. of 9.5 to 13.8 pp). This indicates credit cardholders enrolled in Autopay Min appear to be subject to anchoring to the minimum payment similarly to cardholders not enrolled in Autopay. Despite regulatory pressure, no UK lender was willing or able to test our treatment de-anchoring manual payments in a field experiment (and no prior literature does so either). From this resistance, we infer that lenders expect the lab results to extrapolate to the field.

III Field Experiment

III.A Nudge Design

In our survey experiment we varied how manual payment options are presented. In our field experiment, we vary how Autopay enrollment options are presented to UK consumers who have just opened a new credit card account. Credit cardholders have broad discretion in how much to repay each month (in contrast to fixed term loans); paying any amount between the minimum due and the full balance fulfills their contractual obligations. The minimum payment due is typically calculated by $max\{£5,\ 1\%\ \text{statement balance} + \text{interest} + \text{fees}\}$.[7] If a cardholder is only paying the minimum, then (i) their repayment is effectively

[6] In both of these studies the minimum payment is a visible anchor before and after the formulae changes. This means such studies may under-estimate anchoring effects if a consumer remains well-anchored to the minimum. Keys and Wang (2019) write "at least 22% of near-minimum payers (and 9% of all accounts) respond to the formula changes in a manner consistent with anchoring as opposed to liquidity constraints alone". The anchoring effect would ultimately only be revealed if a field experiment shrouded the minimum payment in a way tested in our survey experiment and prior lab studies.

[7] This is a typical and most common construction, but there are some exceptions. Some UK credit cards have higher percentages of outstanding balances in their minimum payment rules. Some UK credit card brands have a minimum of £25 rather than £5. Some UK credit cards also include another clause for max

only servicing debt interest payments (with interest rates near 20% typical), and (ii) debt reduction only happens at all if new spending is less than 1% of the statement balance. Even with *no* new spending, debt paydown is only 1% of the statement balance per month if a cardholder only pays the minimum. This credit card amortization structure is somewhat similar to interest-only (or reverse) mortgages although one important difference is that credit cards are open ended agreements.

When a consumer opens a new credit card online they are typically presented with the option to enroll in Autopay. If a consumer decides to opt-in, they are normally presented with three Autopay options: Autopay Full, Autopay Fix, and Autopay Min. These options are shown to our control group (Figure 2, Panel A).[8] At this stage consumers can still decide against enrolling in any type of Autopay by not completing the enrollment process. They could also return and complete the Autopay enrollment later.

While Autopay Min is a common repayment option, cardholders also have the option to enroll in an alternative Autopay option that would repay debt faster: 'Autopay Fix'. Autopay Fix is calculated by: $max\{$Autopay Fix £, Minimum Payment Due$\}$. By contrast, the minimum payment – and therefore Autopay Min – typically declines with balances. For example, a typical credit card balance of £1,000 (assuming 18.9% APR and no further card spending) would take 18 years and 6 months to pay off if no new purchases are made and only the minimum is paid each month (starting around £25 and falling to £5). However, by fixing payment to £25 each month, the debt pay-off horizon falls to 5 years and 1 month, saving over £750 in interest costs. Choosing slightly higher fixed payment amounts sharply decreases amortization times and borrowing costs. For example, with a fixed payment of £50 each month, the debt pay-off horizon falls to 2 years and interest costs become only £191 (compared to £509 if paying a fixed amount of £25).

The treatment webpage (Figure 2, Panel B) is a nudge that shrouds the option to automatically make only the minimum payment each month. This is done by removing the explicit appearance of the Autopay Min option (which is shown to the control group in Panel A). Removing the Autopay Min option increases the salience of the alternative, Autopay Fix and the Autopay Full options. This intervention has never been tested before.

Because few consumers can pay their credit card debt in full each month, the treatment is designed to work by increasing Autopay Fix enrollment which, relative to Autopay Min, is expected to increase automatic payments and reduce debt and interest costs. It could possibly also yield an effect of increasing consumer spending via debt paydown increasing

2.5% (or a different fraction) of balance. Some UK credit cards issued before 2011 have minimum payment rules which may not pay off debt even if the cardholder paid the minimum and spent no more on their card.

[8]The largest US credit card lenders (e.g., American Express, Chase, Citi, Capital One, Discover, US Bank, and Wells Fargo) offer these Autopay options.

credit limit availability (e.g., Gross and Souleles, 2002; Agarwal et al., 2017).

While there is no longer an explicit Autopay Min option in the treatment arm, cardholders can choose an operationally equivalent option by setting an Autopay Fix of £5 (or less). These two options are equivalent as the minimum payment is calculated as $max\{£5,\ 1\%\ \text{statement balance}\ +\ \text{interest}\ +\ \text{fees}\}$ and so is greater than or equal to £5 by construction. This means that when the minimum payment due in a particular month is more than £5, the Autopay attempted to be taken will adjust accordingly, regardless of whether a consumer has an Autopay Fix amount of £5 or an Autopay Min.[9] This equivalence is not highlighted to consumers and we do not expect them to be aware of this or work this out. We explain this to show that the treatment does not restrict consumer choice of an Autopay option to pay the minimum – and so the treatment is a nudge rather than a restriction (the Autopay Min option is no longer explicitly labelled on the website). If a consumer in either the control or treatment group phones the lender's call center they could still enroll in an explicit Autopay Min if they ask to do so. Thirty days after card opening, cardholders in both the control and treatment groups have identical (control group) screens containing explicit Autopay Min enrollment options. This is relevant if a cardholder comes back to the Autopay launch page to change their Autopay enrollment status.

III.B Experiment Implementation

We test the nudge through a randomized controlled trial (RCT) tested in the field on UK credit cardholders. The FCA invited all UK credit card lenders to voluntarily participate in a field trial. Two lenders were willing and technically able to participate within the timelines necessary to inform FCA policymaking. Before putting the nudge into the field it went through reviews at the FCA's Institutional Review Board's governance and at both lenders.

We implement the experiment on new credit cards. When a consumer is applying for a new credit card online and has been accepted by a lender they have the option to set-up Autopay on this new card. If a consumer selects the option confirming that they want to enroll in Autopay, they are included in the experiment. Inclusion in the experiment is irrespective of whether the Autopay enrollment process is completed after reaching the Autopay enrollment screen. At this point consumers are randomly assigned to either control or treatment (the nudge).[10] Once allocated to control or treatment the consumer would view

[9]Example 1: If a consumer had a £5 minimum payment due then £5 would be attempted to be taken if the consumer is enrolled in Autopay Min. If a consumer had an Autopay Fix amount of £5 then £5 would be attempted. Example 2: If a consumer had a £10 minimum payment due then £10 would be attempted to be taken if the consumer is enrolled in Autopay Min. If a consumer is enrolled in Autopay Fix amount of £5 then £10 would be attempted (as the minimum is higher than the fixed amount).

[10]Since we did not know who new applicants were going to be in advance of their application, this

the same assigned screen if they returned to the Autopay landing page within thirty days.

We carried out qualitative consumer testing to ensure consumers would understand how to navigate the treatment, conducted an ethical review to consider the potential for unintended consumer harm, and sought feedback from all UK credit card providers and large consumer organizations. Lenders did not report any consumer complaints to us regarding the lack of an explicit Autopay Min option.

Our field experiment is conducted on two UK lenders. The main lender is a large UK firm and our experiment included 40,708 credit cards newly issued by them between February and May 2017. We wanted at least 20,000 cards in each of control and treatment group. The final achieved number is slightly higher as for logistical reasons new cards were included until the end of May 2017. We also conducted the experiment with a second lender. The second lender stopped the experiment after one week of fieldwork due to the lender's concern over the large treatment effects on Autopay enrollments. The second lender's experiment was not restarted and the pre-agreed target sample size was not reached. The second lender's experiment's achieved sample size of 1,531 cards is insufficiently powered to distinguish between null results and imprecisely estimated non-null effects. Had we known this second lender would pull-out we would not have run the experiment with them. For completeness, results from the second lender are in Internet Appendix C. The rest of this paper is based on the field experiment with the main lender unless explicitly stated.

III.C Theoretical Motivations

Autopay Min may be appealing because of multiple mutually compatible economic and psychological factors. We discuss how these inform our field experiment's design.

III.C.1 Anchoring

The nudge tested in our field experiment removes the minimum payment as an anchor during Autopay enrollment. We purposefully do not include an alternative recommended Autopay Fix amount because we do not want to replace the minimum payment anchor with another anchor (other than the Autopay Full anchor). We want consumers to make active choices Carroll et al. (2009) or be anchored to the Autopay Full option. This design choice is motivated by US studies (Agarwal et al., 2015; Hershfield and Roese, 2015; Keys and Wang, 2019) which find that providing consumers with credit card repayment scenarios can unintentionally reduce payments for some consumers.

randomization had to be done live during the application process instead of in advance. This was carried out through a random number generator JAVA script created by the lender.

III.C.2 Financial Literacy

We hypothesized that some consumers' decision to enroll in Autopay Min reflects an imperfect understanding of the costs associated with this option.

Cardholders often make non-optimal repayment choices (e.g., Gathergood et al., 2019a,b) with prior literature showing that credit card lenders structure products and marketing to exploit a lack of sophistication (e.g., Gabaix and Laibson, 2006; Ru and Schoar, 2020). Approximately half of credit cardholders in one UK survey incorrectly thought the minimum payment is the amount most people repaid, when in fact only a quarter do (FCA, 2016b). Studies across countries show cardholders significantly overestimate the speed at which debt is cleared (and by implication underestimating the interest costs) if only the minimum payment is made (e.g., Lusardi and Tufano, 2015; Seira et al., 2017; Adams et al., 2022).

Among a survey of UK Autopay Min enrollees 96% of respondents underestimate the time it would take to fully repay a debt if the cardholder made only the minimum required payment (Adams et al., 2022). Informational disclosures to credit cardholders to address financial illiteracy are ineffective at changing consumer behavior across the US (Agarwal et al., 2015), Mexico (Seira et al., 2017), and the UK (Adams et al., 2022).

In Adams et al. (2022) we conduct field trials across three lenders testing whether personalized, informational nudges explicitly encouraging debt repayment via standalone emails or letters to credit cardholders already enrolled in Autopay Min could change behavior. These interventions had zero or small effects on Autopay enrollement and are ineffective at reducing debt. Given the ineffectiveness of disclosures and informational nudges, our nudge in this paper tests a more intrusive intervention. Our nudge is designed as a policy that can be applied at low-cost to apply at scale (primarily involving a one-time IT compliance cost), in contrast to more costly policies attempting to increase financial literacy.

III.C.3 Inertia and Limited Attention

Consumers may enroll in Autopay for convenience: providing insurance against forgetting to pay a bill. Yet Autopay means credit cardholders no longer need to actively decide each month how much to pay and may become inattentive to their debt and procrastinate on paying it down (e.g., Sakaguchi et al., 2022).

Our nudge is targeted at new card originations to be a preventative measure against inert consumers persistently carrying high credit card debt. We nudge Autopay enrollment at card origination because these initial Autopay decisions are sticky (e.g., Sakaguchi et al., 2022; Adams et al., 2022; Wang, 2023). Sticky Autopay enrollments may arise from limited attention (Sakaguchi et al., 2022). Indeed this is consistent with another domain; Sexton

(2015) argues that enrollment into Autopay (Full) for utility bills, reduces price salience and results in 'overconsumption' of electricity.

Targeting behavior at the time of card origination is expected to be more likely to succeed than trying to change habitual cardholder behavior. Consumer inertia is common across household financial domains, including simple decisions such as cash savings (e.g., Adams et al., 2021) and high stakes decisions such as mortgage origination and refinancing (e.g., Andersen et al., 2020). Our nudge attempts to harness inertia by getting consumers to initially enroll in an Autopay Fix (or Autopay Full). Psychological frictions push against consumers exerting effort to frequently change their Autopay choice.

Without an explicit Autopay Min option consumers with limited attention may be forced to make an active choice (e.g., Carroll et al., 2009; Keller et al., 2011) – calculating how much they can afford to regularly pay each month. The nudge makes it difficult for inattentive consumers to default into automatically paying only the minimum. We purposefully design our nudge to not specify a default Autopay choice (other than Autopay Full). A lack of a low-payment default may be socially optimal if there is a high degree of heterogeneity in consumers' socio-economic circumstances and preferences (e.g., Carroll et al., 2009). This is especially likely if there is information asymmetry – making it impractical to implement an optimized individual policy default for heterogeneous consumers. In the domain of retirement savings, Carroll et al. (2009) discuss how a default asset allocation may be optimal but it may be preferable to set contribution rates by active choice given heterogeneity in optimal savings rates. Keller et al. (2011) and Cronqvist and Thaler (2004) present more discussion of comparisons of defaults and active choices in retirement savings.

III.C.4 Present Bias

Present bias (Laibson, 1997; O'Donoghue and Rabin, 1999) may also contribute to low credit card repayments. Theoretical models without present bias struggle to simultaneously explain observed levels of credit card debt and wealth formation (Laibson et al., 2023). If naïve, present biased consumers are over-consuming, this generates welfare losses and therefore provides a rationale for nudging consumers to repay more (e.g., Heidhues and Kőszegi, 2010, 2015; Allcott et al., 2022). The empirical literature finds present biased consumers hold more credit card debt (Meier and Sprenger, 2010) and generally fail to stick to their plans to pay it down (Kuchler and Pagel, 2021).

A present biased consumer may enroll in Autopay Min with the intention of making additional manual payments to reduce debt, however, they may not follow through (O'Donoghue and Rabin, 1999). There is evidence (e.g., Kuchler and Pagel, 2021) that consumers want to repay their debt more quickly than they do. For example, the average respondent enrolled in Autopay Min self-reports wanting to repay their credit card debt in three years, which is substantially faster than the six years they expect it to take, and the eighteen years it would actually take at Autopay Min (Adams et al., 2018a).

III.C.5 Limited Liquidity

Limited liquidity would be a standard economic explanation for consumers enrolling in Autopay Min. Limited liquidity may arise for either classical reasons (e.g., a relatively high exponential discount rate or an adverse income/spending shock) or behavioral reasons (e.g., present bias). Limited liquidity may weaken the effectiveness of our intervention. Consumers who anticipate that they are likely to have low levels of future liquidity may want the flexibility that arises from a low automatic payment. Such consumers may replace Autopay Min with a low fixed automatic payment.

IV Data and Methodology

IV.A Data

Our data is gathered by the UK financial regulator (FCA) using its statutory powers. From the two credit card lenders in the experiment we collect detailed data covering every credit card in the experiment. We observe data recorded at card origination (e.g., opening date, interest rates, initial credit limit) and across all statements (e.g., statement balances, transactions) to December 2017. A completed statement cycle is one where the payment due date for a credit card statement has passed. For the main lender in our experiment we observe seven completed statement cycles for effectively all cards (99.9%) and up to eleven for the cards opened earliest in the experiment. For the second lender we observe twelve completed statement cycles. Each individual payment made against these statements is observed including the date, amount, and whether the payment is made via Autopay or manually.

Credit files are gathered for all the individuals in the experiment enabling us to observe effects across a consumer's debt portfolio. Credit files provide monthly, product-level data showing credit limits, balances, payments, and arrears from card opening to the end of 2017. For credit cards, we observe statement balances (i.e. before repayments), repayments, balances after repayments (i.e. debt), and indicators for whether a card only paid the minimum. UK credit files contain payments data for all credit cards – this is higher quality than US credit files where only a selected subset of credit cards report repayments data (Guttman-Kenney and Shahidinejad, 2023). We observe credit risk scores and income estimates (where available) at two points-in-time: the month before the card was opened and nine months afterwards. These data mean that if the treatment caused a large increase to payments to credit cards in the experiment that caused financial distress elsewhere in their portfolio we could observe it. The lender microdata and credit files are linked using an anonymous key created for this project. All analysis is conducted on anonymized data.

We also observe bank account data (checking/current accounts and savings accounts) for the subset of cardholders who hold these with the credit card lender in our experiment. The bank account data report end of day balances each day up to a year before (or when the account was opened) the experiment started and up to June 2017 - a month after the last cards are enrolled in our experiment. After restricting these data to cardholders who appear to be actively using this bank as their primary bank account, we observe 3,753 cardholders or 9.2% of our field experiment (Additional details in Internet Appendix D).

IV.B Empirical Methodology

Before analyzing data, we pre-registered our methodology. Our pre-registration designates primary outcomes, regression specifications, and thresholds for statistical significance.[11] We structure our analysis in three parts: primary, secondary, and tertiary analyses. This structure limits the potential for data mining or p-hacking. The primary analysis focuses on ten primary, real economic outcomes upon which the nudge's effectiveness is evaluated.

The first six primary outcomes (1-6) measure the impact on the credit card in the experiment ('target card') - constructed from microdata collected from the lender. All these primary outcomes are bounded between zero and one. Outcomes 1, 2, and 3 are binary: (1) any minimum payment, (2) any full payment, (3) any missed payment. Outcome (4) is a measure of credit card debt: statement balance net of payments (% statement balance) We examine multiple moments because credit card payments have a non-normal, bimodal distribution (e.g., Keys and Wang, 2019) with the tails being economically important. Outcome (5) is a measure of borrowing costs (combining interest and fees): Costs (% statement balance). Outcome (6) is a measure of consumption: Transactions (% statement balance). Our measures of debt, spending, and costs are all normalized by statement balances in order to deal with fat tailed credit card balances. Normalizing our measures of debt by credit card statement balance is not ideal as it means our outcome is a ratio of two endogenous components. To address this our secondary analysis also shows the numerator and denominator in levels separately (and having completed the analysis we find the results are consistent).

Primary outcomes 7 to 10 are analogous to primary outcomes 1 to 4 but constructed

[11] Available at AEARCTR-0009326. The pre-registration jointly covered the field experiments in Adams et al. (2022) – the only differences being Adams et al. (2022) had different exclusion criteria given it was conducted on existing rather than new credit cards and also had different treatments.

using credit file data to assess the impact across a consumer's portfolio of credit cards. These primary outcomes are: (7) Share of credit card portfolio only paying minimum, (8) Share of credit card portfolio making full payment, (9) Share of credit card portfolio missing payment, (10) Credit card portfolio balances net of payments (% statement balances). See Online Appendix B for more details on primary outcome definitions.

Following Benjamin et al. (2018) we regard a p value of 0.005 as the threshold for statistical significance but also highlight where results are 'suggestively significant' at the 0.01 and 0.05 levels. 0.005 significance aligns with 14+ Bayes factors: often considered substantial evidence for a hypothesis. This approach is analogous to applying Bonferroni or familywise error corrections to ten outcomes evaluated at 0.05 significance levels. Given the precision of our estimates, alternative corrections would not affect our results or conclusions. For our primary outcomes, we have sufficient power to differentiate null effects from economically meaningful ones to inform potential policymaking (the minimum detectable effect sizes are in Internet Appendix Tables B2 and C1).

The pre-registered secondary analysis considers a broader set of outcomes and empirical approaches to understand our results and their robustness. This secondary analysis measures the effects of the nudge on Autopay enrollment and uses the pounds (£) amounts of credit card debt and repayments as robustness checks of our primary outcomes. Conducting secondary analysis depends on the primary analysis's results. We design and implement tertiary analysis after examining the data.

We are able to causally identify the effects of the treatment on consumers in our field experiment since we are randomizing whether a consumer receives the control or treatment. The average treatment effect is the policy parameter of interest as the treatment was a potential regulatory policy which was being considered to be applied across the UK credit card market. Equation 1 shows the OLS regression specification used to derive average treatment effects. To estimate this we construct an unbalanced panel with one observation for each consumer's (i) credit card statement cycle (t) observed. This panel is unbalanced as some cards are opened earlier than others. In this specification δ_τ shows the average treatment effect $\tau \in \{1, 2, ..., T\}$ cycles since the start of the experiment. We hypothesized that treatment effects will vary over time but we did not impose a functional form because it is unclear what the appropriate functional form would be.

$$Y_{i,t} = \alpha + \sum_{\tau=1}^{T} \delta_\tau \left(TREATMENT_i \times CYCLE_\tau \right) + X_i'\beta + \gamma_{m(i,t)} + \gamma_t + \varepsilon_{i,t} \qquad (1)$$

Our regression includes a constant (α), a vector of time-invariant control variables (X_i') constructed using information on the new credit card opened and cardholder data from before the start of the experiment (controls listed in footnote).[12] We also include time fixed effects: we control for both the statement cycle (γ_t) and year-month ($\gamma_{m(i,t)}$) because statement cycles do not perfectly align with calendar months and new credit cards have different opening dates - entering the experiment until the pre-registered sample size was achieved. Standard errors are clustered at the consumer-level.

For our primary analysis we focus on the outcomes from the last cycle where the panel is balanced: the seventh completed statement cycle (δ_7). The seventh statement cycle is complete when its due date has passed: this is mean 195 and median 196 days from card opening with a range of 175 to 245 days. This seventh statement cycle should be thought of as six genuine statement cycles as a new card's first statement is typically less than a month (in our data the first statement is issued mean 12 and median 11 days from card opening) to on-board the card onto a particular billing cycle and so this first statement has a zero payment due that makes it uninteresting (we show for completeness). A consumer's first full statement is statement two (the second statement is issued mean 43 and median 42 days from card opening) when the cardholder has at least one month to view the control or treatment screens and to use their card (and, if used, has a non-zero payment due).

In tertiary analysis we check the robustness of selected results by pooling across all statement cycles to provide more statistical power. We modify Equation 1 replacing the dynamic $\Big(TREATMENT_i \times CYCLE_\tau\Big)$ with static $TREATMENT_i$ shown in Equation 2 where our single static parameter of interest is δ.

$$Y_{i,t} = \alpha + \delta\, TREATMENT_i + X_i'\beta + \gamma_{m(i,t)} + \gamma_t + \varepsilon_{i,t} \tag{2}$$

IV.C Summary Statistics

As the experiment is conducted on newly-opened cards we describe summary statistics for the control group after seven statement cycles (see Internet Appendix Table B1). We observe a diversity of credit cardholders in our data with a wide range of interest rates, credit scores, credit card credit limits, ages, and incomes. The mean credit card statement balance after cycle seven is £2,164 and £1,963 after payments. Cardholders often hold other credit cards in their portfolio: their mean credit card portfolio statement balances (summed across cards

[12] The controls (X_i') are: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from credit file data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. For outcomes constructed from credit file data up to eleven dummies for lags of outcomes are included as controls (X_i') for months preceding the start of the experiment.

held in credit file data) is £3,917 and £3,432 after payments. Credit card portfolio balances both before and after payments are higher than consumers' mean income of £2,437.

In line with the motivation for our experiment, the cardholders in our control group are often only paying exactly the minimum. 30% make the minimum payment in the seventh statement cycle. 19% pay the minimum six or more times in the first seven cycles: by comparison 18% had paid in full six or more times.

Allocation to the treatment group is balanced, on average, across measures (Internet Appendix Table B3). However, we do observe the likelihood of being in the treatment group slightly varies with credit card limit. Investigation revealed that the 'live' randomization code used by the lender was not completely random: 526 more consumers (0.65%) are allocated to control than to treatment. As consumers applying for credit cards were unaware of (and unable to manipulate) their likelihood of being allocated treatment, we can recover balance between treatment and control through conditioning on covariates. Conditioning on observables using our pre-registered controls does not change our results.[13]

V Experimental Results

V.A Effects on Autopay Enrollment

The first effect we examine is the mechanism the treatment is designed to work through: changing Autopay enrollment choices by the time of their second credit card statement. Autopay enrollments are secondary outcomes.

Figure 3, Panel A shows the treatment causes large, significant initial effects in Autopay enrollment choices. The treatment raises the fraction of cardholders enrolling in Autopay for a fixed amount (Autopay Fix) by 20.9 percentage points: a 72% increase on the control group mean. For comparison, Figure 3, Panel B displays these enrollment effects are even larger for the second lender who stopped the field experiment early: increasing Autopay Fix enrollment by 40 pp (216%). Subsequent results are all based on the main lender.

The Autopay Fix amounts consumers initially choose are frequently round numbers. 62% of Autopay Fix amounts are for (in descending order of frequency): £100, £50, £200, £150, £20, £30, or £25. Very few consumers select Autopay Fix amounts of £5 or less that are mechanically identical to Autopay Min: 2.4% of the treatment group set an Autopay Fix of £5 or less (4.8% of Autopay Fix enrollees).[14] This is a statistically significant increase

[13]We also did a robustness check using non-parametric controls for each credit card credit limit value instead of our pre-registered a linear control and it made no difference.

[14]Effectively no cardholders enroll in an Autopay Fix set exactly equal to £5 in either control (0.06%) or treatment (0.07%) groups.

relative to 0.5% in the control group but we interpret it as being economically small.

Initial choices of Autopay Fix amounts are persistent over time (Internet Appendix Figure B1). 88.3% of those in the treatment group who are enrolled in Autopay Fix at their second credit card statement remain enrolled in Autopay Fix at their seventh statement (7.0% have no Autopay, 4.4% Autopay Min, and 0.3% Autopay Full). Of those, 97% have it set for the same Autopay Fix amount, and, on average, the difference in amount is trivial: £0.78. Among all cardholders in the treatment group enrolled in Autopay Fix at cycle 2, the mean Autopay amount is £96.85 (median £80) compared to £104.60 (median £100) at cycle 7: this indicates that cardholders who enroll in Autopay Fix later on are choosing slightly higher Autopay Fix amounts than the initial group.

Almost all of the mass of increased Autopay Fix enrollment is redistributed from cardholders enrolling in Autopay Min in the control group. The treatment reduces the fraction of cardholders enrolling in Autopay Min by 27.3 pp: a 74% decrease on the control group mean. Autopay Min are not entirely eliminated as it was possible for individuals in the treatment group to sign-up for these through other ways (e.g., telephoning the call center).

The treatment also causes an increase in Autopay Full enrollment of 1.2 pp. This effect size can be interpreted relative to a control mean Autopay Full enrollment of 14.5%. The treatment also causes a decline in any Autopay enrollment (Autopay Full, Autopay Fix, or Autopay Min) of 5.1 pp from the control mean of 80.2 pp.

We estimate these treatment effects on Autopay enrollment more precisely using our pre-registered regression specification and find statistically significant changes in enrollment. The regression coefficients after seven statement cycles (δ_7 in Equation 1) – presented in Table 1 – are in line with initial changes in enrollment: Autopay Min enrollment decreased 21.7 pp, Autopay Fix enrollment increases 16.7 pp, Autopay Full increases 0.6 pp (the latter being only significant at the 5% not the 0.5% level), and any Autopay enrollment declines 4.4 pp (unconditional means in Internet Appendix Table B4). Estimates cycle-by-cycle (δ_τ in Equation 1) are displayed in purple in Figure 4 for Autopay Fix enrollment (Internet Appendix Figure B2 for Autopay Full and Autopay Min, and Internet Appendix Figure B3 for any Autopay). The small, initial effect on Autopay Full enrollment attenuates over time and becomes statistically insignificant from zero. The Autopay Fix and Autopay Min also attenuate but effects remain large. As initial Autopay choices in the treatment group are highly persistent, this attenuation is primarily driven by some in control group 'catching-up' and switching from Autopay Min towards Autopay Fix or Autopay Full. Effects of the treatment on any Autopay enrollment change relatively little between cycles two and eight.

The observed changes in Autopay enrollments – the nudge making consumers more likely to choose full, less likely to choose minimum, and changing the distribution of Autopay amounts – are consistent with the minimum payment amount distorting the control group's choices. These changes closely match our survey experimental results in Section III.C.

V.B Effects on Long-Term Real Economic Outcomes

We examine the effects on our ten primary outcomes using our pre-registered regression specification. These estimates are seven statement cycles after card-opening (δ_7 in Equation 1) and are shown in Table 2 (unconditional means in Internet Appendix Table B5).

We find a large and persistent effect of the nudge making cardholders less likely to only pay exactly the minimum. The nudge causes a significant 23% reduction in the likelihood of only paying exactly the minimum of 7.1 pp (95% confidence interval of 6.2 to 7.9 pp). Figure 5 presents this treatment effect over time showing the effect is -10.9 pp in the second cycle and stabilizes near -7 pp by the sixth cycle (Internet Appendix Figure B4, Tables B7 and B6 show consistent results examining the cumulative number of minimum payments).

This effect on making only minimum payments is smaller than the effect on Autopay Min enrollment shown in the previous subsection. This is because cardholders enrolled in Autopay Min can also make additional manual payments to pay more than the minimum. Also some cards have no balance due and therefore no minimum payment and no payments taken (we regard such cases as a full payment).

How does this translate to the share of a cardholder's credit card portfolio where payments are made only equal to the minimum (constructed from credit file data)? There is an average treatment effect a third of the size of that for the card for which the treatment is targeted. This smaller overall effect across the credit card portfolio is due to consumers holding multiple cards – only one of which is directly affected by the nudge.

We observe precisely-estimated null effects on average treatment effects on other primary outcomes for the target card in the experiment: the likelihood of paying debt in full, debt net of payments, borrowing costs, and purchases. The exception is an increase in the likelihood of missed payments on the target card of 0.38 percentage points (95% confidence interval 0.02 to 0.75 pp) that is statistically significant at the 5% level but not at our 0.5% threshold.

There are precisely-estimated null effects on average treatment effects across our other credit file outcomes: the likelihood of paying in full, the likelihood of missing payments, and outstanding debt when aggregating across the portfolio of credit cards held. There is no evidence of the treatment affecting other cards held, although we caveat that in an RCT as an ex-ante test of a potential policy we cannot rule out the possibility of general equilibrium effects if this policy applied to all of a consumers' cards. The lack of negative spillovers on a consumer's portfolio is important as one reason for testing the nudge to inform potential policymaking is to evaluate whether any debt reduction for the card in the experiment is partially or fully crowded by greater indebtedness or financial distress elsewhere.

Our treatment does not reduce credit card debt at or before the seventh statement cycle (Figure 6, Panel A). As a robustness check as part of our secondary analysis we look at debt in pounds and also find no statistically significant effect (Figure 6, Panel B) or across the portfolio of credit card debt (Internet Appendix Figure B5).

As the cycle-by-cycle estimates on our primary measure of credit card debt are stable over time but persistently, slightly, but statistically insignificantly, below zero, we check the robustness of this result in tertiary analysis by pooling across all statement cycles to provide more statistical power (Equation 2). If the treatment has any average effect on debt, the average effect on the target card is at most a 1.1 percentage point reduction (Internet Appendix Table B8). Even with this pooling there is no statistically significant effect on credit card debt across the portfolio of cards held: at most a 0.79 pp reduction.

Similarly, even with this pooling exercise, we find no significant effects on the likelihood of repayment in full on the target card. At most it increases by 0.1 pp: which we interpret as a trivially small amount. As a robustness check, we examine the cumulative number of full payments and results are consistent with stable, precisely-estimated null effects across cycles (Internet Appendix Figure B4, Tables B7 and B6). Our null average treatment effects on debt (robust to secondary outcomes in Internet Appendix Tables B7 and B6) in spite of a seemingly large changes in enrollment and reduction in paying only the minimum payment is surprising. Why does the treatment not, on average, reduce debt if one in five more consumers are enrolled in Autopay Fix (and are not increasing spending)?

VI Mechanisms

VI.A Factors Explaining Nudge Ineffectiveness

Having completed the primary and secondary analysis, we now conduct tertiary analysis to understand the mechanisms behind our results. If the only changes are compositional – changing Autopay enrollment but *assuming no other changes* – the effects on Autopay enrollment may have been expected to lead to a effect of reducing debt by approximately 4.5%.[15] Indeed, the fact that the second lender withdrew after only observing effects on enrollment is evidence of our null effects on later real outcomes being unexpected. We find three consumer responses on the target card make the nudge ineffective at reducing debt.

[15] Calculated using the mean debt net of payments in cycle 7 for cardholders in the control group for each Autopay enrollment type and weighting these by the treatment group's Autopay enrollments shares.

VI.A.1 Autopay Fix Amounts 'Too Low'

Cardholders often respond to the nudge by setting an Autopay Fix that is 'too low': binding at or just above the minimum due. While the treatment causes a 16.7 pp increase in Autopay Fix enrollment by statement seven (the purple coefficients in Figure 4), the treatment effect on enrollment with Autopay Fix *exceeding* the minimum amount due is still large but half the size (the pink coefficients in Figure 4): 8.6 percentage points which is a 34% increase on the control group mean (Internet Appendix Table B4). See Table 1 for regression estimates.

As credit card balances accumulate over the first few months of card ownership, the minimum amount due rises, causing the minimum payment amount to exceed many of the fixed payments. After seven statement cycles, the proportion of consumers in the treatment group with an Autopay Fix exceeding the minimum payment amount is 66% - noticeably down from 78% in the second cycle (Internet Appendix Figure B6 and Table B4).

Examining the distribution of Autopay Fix amounts chosen by the treatment group (Figure 7) shows they are often 'low', commonly round number pound amounts such as £50 or £100 (Panel A) that are small amounts in excess of the minimum (Panels B and D). We do not show the Autopay Fix for the control group as the treatment causes large changes in Autopay Fix enrollment and so the Autopay Fix groups are not directly comparable. Pooling across all seven cycles, we find that for 48% of Autopay Fix enrollees in the treatment group, the cumulative Autopay Fix amount is £100 or less in excess of the minimum. At the other extreme, it is only over £500, for 13%. We evaluate these relative to the mean cumulative value of repayments across these cycles in the control group: £1,277. We interpret that the additional payments from Autopay Fix over the minimum are typically 'low' in absolute levels, however, they are large increases relative to the extremely low minimum payment due which averages £46 per month (£320 cumulative across cycles 1-7).

VI.A.2 Lower Enrollment In Any Autopay

The second factor is that the nudge causes a 4.3 pp (5.6%) significant decline in enrollment in any type of Autopay (Table 1 and Internet Appendix Figures B1 , B3, and Table B4). This lower enrollment explains an unintended slight average increase in the likelihood of missed payments (Table 1). If enrolled in Autopay a consumer would only miss a payment if they have insufficient funds in their checking account whereas consumers not enrolled may easily forget to make a payment. While this increase is not statistically significant at our 0.5% significance threshold when examining any particular statement cycle, it is clearly significant when conducting a joint significance test pooling data across all statement cycles (while still clustering at the consumer-level). We find the nudge increases the probability of missed payments by 0.4 pp with a 95% confidence interval of 0.19 to 0.62 pp (Internet Appendix Table B11). There are no statistically significant differences in the types of consumers the treatment made more likely to not have any Autopay enrollment.[16]

The effect on missed payments is solely on temporarily being a single payment behind: precise zeros are estimated on being two or three payments behind (Internet Appendix Table B11 and Figure B4). The treatment does not lead to consumers being in more severe arrears which the industry defines as being 2+ or 3+ payments behind: these are all null results even when pooling observations across cycles to increase power to account for the low incidence of such severe arrears (Internet Appendix Table B11). Only more severe arrears get reported in their credit file (i.e. missing a payment by 1 day would not be reported, but by 31 days would be reported). This explains why we do not observe increased missed payments in our primary outcome measuring this in credit files (Table 2 and Internet Appendix Table B8). Given that there is no difference in severe arrears on the card in the experiment and also no difference in severe arrears across the portfolio of cards in credit files, we infer that severe arrears on others cards is unaffected.

This result indicates that having no Autopay means consumers forget to make a payment which has a temporary impact, most notably incurring a late payment fee (in line with Gathergood et al., 2021; Sakaguchi et al., 2022) and not reducing debt, rather than causing a debt spiral or severe distress. While lower enrollment in Autopay is not an intended effect of the nudge, it is not increasing consumer indebtedness. This is consistent with consumers being more attentive to their debt if not enrolled in Autopay (Sakaguchi et al., 2022). This is different to other domains where lower enrollment may be a worse economic outcome. For example, if a nudge lowers 401(k) enrollment then consumers can be missing out on 'free money' from employer-matched contributions and under-save for retirement.

VI.A.3 Manual Payments Substitution

Cardholders can make manual payments instead of or in addition to automatic payments. We find substitution between the two as another potential offsetting effect. Figure 8 (and Figure 9 Panel A) shows that although there is a positive and significant treatment effect increasing automatic payments, the effect on overall payments is lower due to a negative, but statistically insignificant, negative effect on manual payments. We find the treatment causes

[16] OLS regressions shown in Internet Appendix Table B10 with one observation per card predicting a binary outcome for whether the cardholder had no Autopay enrollment on Female, Age, Income, log credit limit, subprime, purchases rate, any balance transfer, credit score, any mortgage debt, value of credit card statement balances in credit files, value of credit card statement balances net of payments in credit files, number of credit cards in credit file, and the number of credit cards with debt credit file. While most of these are significant predictors of Autopay enrollment, none are when interacted with the treatment and so do not explain this decline in Autopay enrollment.

consumers to be 1.3 pp more likely to make both an automatic and manual payment in the same cycle in spite of lower Autopay enrollment. More details are in Internet Appendix Table B7.

Manual payments are infrequent but large. Just 8.5% of those enrolled in any Autopay option in the control group also made a manual payment in the seventh cycle. The percentages of different subsamples of the control group that made both a manual and automatic payment in the seventh cycle are: 6.7% of all consumers (i.e. with and without Autopay enrollment in the control group); 9.2% of consumers enrolled in Autopay Fix or Min; 12.7% for consumers enrolled in Autopay Fix; 6.3% of consumers enrolled in Autopay Min. Cardholders making both a manual and automatic payment have little differences from other cardholders except being slightly younger and being more likely to not hold mortgage debt (Internet Appendix Table B12). However, manual payments account for 45% of the total cumulative value of payments made across cycles 1-7 by those in the control group enrolled in Autopay at cycle seven (54% for those enrolled in Autopay Fix or Min).

Consumers appear to use Autopay as insurance against forgetting to make a payment (in line with Gathergood et al., 2021; Fuentealba et al., 2021; Sakaguchi et al., 2022) as opposed to paying down debt.[17] In months where manual payments are made by those enrolled in Autopay in the control group, the mean value of the manual payment is £377, with a median value of £105. Automatic payments in such months average £105 with a median of £55 and are similar in months where consumers are not making manual payments. Most manual payments by those enrolled in Autopay do not clear a consumer's debt – just 17.9% do so in the control group. 65% of manual payments are for round number values whose digit to the left of the decimal is a zero or five.[18] These round numbers found to prominently appear in manual payments appear with far less frequency in total payments: 48%.

Comparing automatic and manual payments is conflating two effects: a change in Autopay enrollment composition and a change in Autopay amount. Conditional on being enrolled in Autopay, one would expect automatic payments to be higher in the treatment than the control, since Autopay Fix is greater than or equal to Autopay Min. Yet automatic payments will be lower in the treatment group because fewer consumers enroll in Autopay than in the control group. Similarly we may expect manual payments to be higher in the treatment

[17] Survey responses in our earlier working paper (Adams et al., 2018b) are aligned with this explanation. The most common reasons survey respondents enrolled in Autopay provide for using Autopay is to prevent incurring a late fee or to prevent a negative credit score impact, while the most common reason respondents not enrolled in Autopay provide is they prefer the control of manually adjusting payments each month.

[18] Such patterns of large, manual payments at round numbers may be consistent with cardholders experiencing adjustment costs (e.g., the psychological cost of logging into online banking to make a manual payment and working out how much to pay) to making a payment above the minimum or having reference-dependent preferences for round numbers (e.g., Sakaguchi et al., 2020).

group, however, this is ambiguous as cardholders may be forgetting to make any payments rather than substituting automatic for manual payments. We disentangle this by decomposing Equation 1 by whether the consumer is enrolled in any Autopay (i.e. Autopay Min, Fix, or Full) at cycle seven ($AUTOPAY_{7,i}$) shown in Equation 3. This is a decomposition by an endogenous variable and so our estimates are not causal and may be biased.

$$Y_{i,t} = \alpha + \sum_{\tau=1}^{T} \delta_\tau \left(TREATMENT_i \times CYCLE_\tau \right) + X_i'\beta + \gamma_{m(i,t)} + \gamma_t + \varepsilon_{i,t} \qquad (3)$$
$$if\ AUTOPAY_{7,i} = g, \quad g \in \{0, 1\}$$

We examine the cumulative value of payments, in total and split by automatic and manual payments, by the seventh cycle across these subgroups in Figure 9. Panel B shows evidence of substitution among consumers enrolled in Autopay: automatic payments increase by £62, manual payments decrease by £57, and so overall payments for this group are unchanged (£2). If all the increased automatic payments had passed through, without offsetting manual payments, average debt would have reduced by approximately 2.9%. Panel C shows zero estimates on automatic, manual, and total payments for those not enrolled in Autopay: this indicates the treatment's main effect on this group is likely shifting this group's size rather than changing its payment amounts differentially to what one would expect from a cardholder in the control group who is not enrolled in Autopay.

As this decomposition is non-causal we interpret this evidence as suggesting the treatment is changing *how* cardholders make payments rather than the *amount* of payments they make. The treatment's effectiveness at changing the composition of Autopay enrollment is offset by consumers choosing low Autopay amounts often binding at or near the minimum, an unintended effect of lower Autopay enrollment increasing arrears and, even among cardholders who are enrolled in Autopay, they appear to substitute higher automatic payments for lower manual payments. This offsetting consumer response shows consumers are less inert than they initially appeared.

VI.B Heterogeneous Effects

In response to presentation feedback we performed tertiary analysis exploring heterogeneity in effects on debt paydown. While for policymaking the average treatment effect is the parameter of interest, it can still be informative to understand whether there are subgroups experiencing heterogeneous effects. The potential gains for the most vulnerable consumers may be highest given their limited financial resources or unsophistication, however, the nudge may be most effective for least vulnerable consumers who may be more sophisticated or who can afford to pay more but do not do so for other reasons (e.g., limited attention).

We examine three groups of consumer vulnerability: credit score, income, unsecured debt-to-income (DTI) ratio. These groups are chosen as variables that are observable to us (and lenders) and relevant to regulators as they are used as inputs for assessing new credit cardholders' ability to pay their debt. These variables are measured from the month preceding card origination. We split these groups into quartiles as it is not clear whether effects would be monotonic. We estimate Equation 1 separately for each quartile of each group. To keep the number of results manageable we only examine heterogeneous effects by our primary outcome of debt (statement balance net of payments as a percent of statement balance). In the control group, there is little difference in this outcome across quartiles of income but noticeably more across quartiles of credit score and DTI.

Our heterogeneity analysis does not produce clear effects (See Internet Appendix Figure B8 and Table B13 for more details). None of the heterogeneous groups show an effect that is statistically significant at our 0.5% threshold. There are no clear effects by income. By credit score we find the second most vulnerable quartile experienced a reduction in debt that is significant at the 5% threshold with a 95% confidence interval of -2.9 to -0.0 pp whereas all other quartiles have insignificant effects. The second least vulnerable quartile by DTI also has a reduction in debt that is significant at the 5% threshold with a 95% confidence interval of -3.1 to -0.0 pp with insignificant effects for other quartiles.

VI.C Relationship with Liquid Cash Balances

Having documented the effects of the nudge and investigated the factors explaining our null result, we wanted to understand *why* consumers are not paying more on their credit card. The most natural potential explanation is that many consumers have limited liquid cash balances available, which prevents or disincentivizes them from repaying credit card debt. While we may term these liquidity constraints, we caveat that limited liquid cash balances is an observable financial outcome that may arise for many reasons such as financial illiteracy (e.g., Lusardi and Tufano, 2015) and behavioral factors such as naïve present bias leading to impulsive overconsumption (e.g., Heidhues and Kőszegi, 2015).

We explore this in tertiary analysis by constructing three measures of liquidity from our linked bank account data (additional details in Internet Appendix D). We measure liquid cash balances before card opening. For each measure we show, in Figure 10, its CDF (left panels) and its relationship with credit card repayments seven cycles later (right panels). Liquid cash balances are the end-of-day balance in bank accounts by aggregating all liquid cash held across checking and non-checking, instantly-accessible cash savings accounts. In the UK, checking accounts often have an overdraft line of credit facility, so liquid cash measures can have negative balances. Based on observed socio-economic characteristics, we expect this selected sample with linked data to be less liquidity constrained than those unobserved (Internet Appendix Table D1). We do not have sufficient power to estimate heterogeneous treatment effects by liquid cash balances but describing these data represents an advance on prior credit card research where liquid cash is unobserved (e.g., Keys and Wang, 2019; Medina and Negrin, 2022).

Our first measure shows approximately 10% of linked cardholders have a zero or negative liquid cash balance available at a point-in-time before card opening (Figure 10, Panel A). Figure 10, Panel D shows consumers who had small, positive liquid cash balances (before card opening) repaid more of their credit card debt, on average, seven cycles later than those with zero or small negative liquid cash balances.

Our other two measures are innovative as they consider the dynamics of liquid cash balances. Our second measure examines a consumer's minimum liquid cash balances over the last 90 days before card opening (along with other time horizons). This accounts for how consumers' finances vary over time; one point-in-time does not reflect how liquidity varies at different points-in-time for different consumers depending on the timing of their incomes and expenditures. Prior literature does not examine heterogeneity by the *minimum* balance but studies different moments: the mean or median balance (see Internet Appendix D).

This second measure shows a lot of bunching of consumers just managing to keep positive, but small, liquid cash balances (Figure 10, Panel B). Using a 90 day window the median minimum balance is effectively zero (£4.76) and the 75$^{\text{th}}$ percentile £142.39. This second measure reveals effectively zero cash balances for approximately 50% of consumers: far higher than the 10% a point-in-time liquid cash balance measure (Figure 10, Panel A) would indicate. Figure 10, Panel E shows consumers with positive minimum liquid cash balances (before card opening) discontinuously repaid approximately 20 pp more, on average, of their credit card debt seven cycles later than those with zero or small negative liquid cash balances.

Our third measure shows the volatility of a consumer's finances. It records the number of days a consumer's liquid cash balance drops below £100 in the thirty days before card opening (along with earlier points-in-time pre-card opening). Approximately 60% of consumers have a low liquid cash balance for one or more days a month. Figure 10, Panel F also shows a clear relationship: consumers who have more days with low liquid cash balances (pre-card opening) repay less credit card debt seven cycles later.

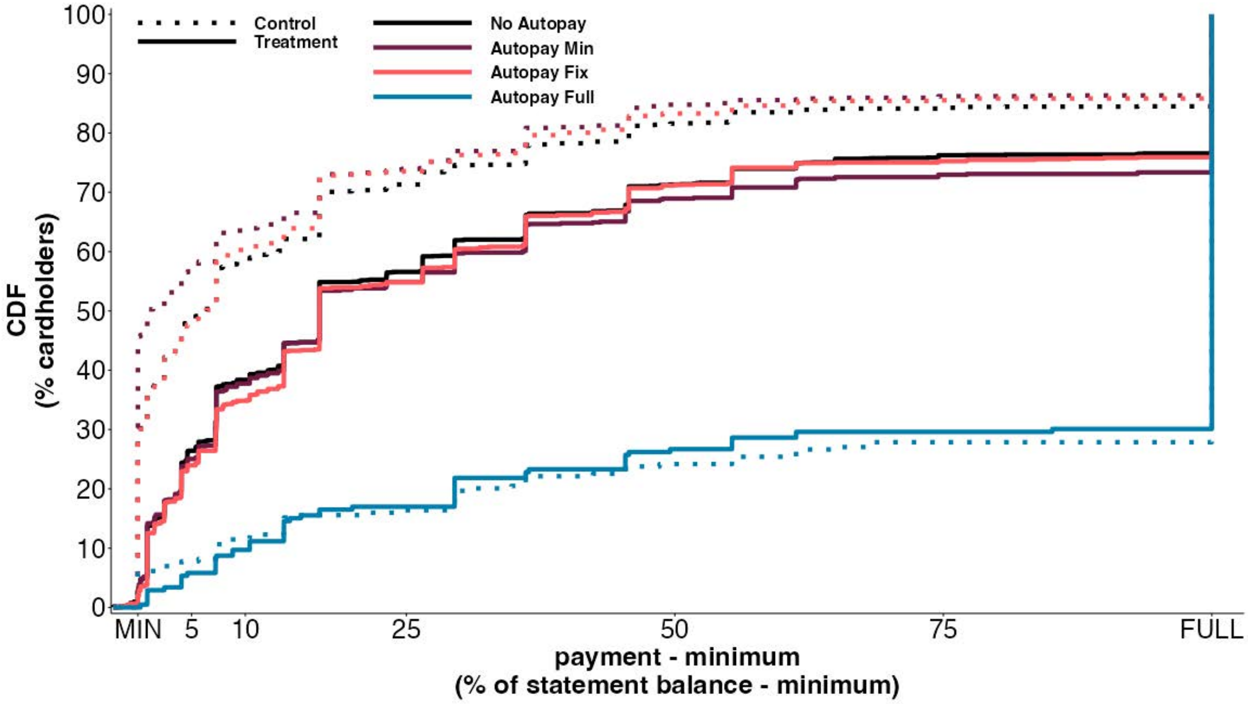
Our results help to understand why these consumers are less "nudge-able" than they first appeared from their Autopay choices and inert minimum payment behavior. Consumers appear to be making 'low' credit card payments and offsetting the nudge to not reduce their debt due to frequently holding limited liquid cash balances. Further examining our survey experiment provides supportive evidence of this. Respondents in our survey experiment self-reporting high financial distress are significantly more likely to hypothetically pay lower amounts and the effects of anchoring are significantly attenuated (details in Internet Appendix A). Such financial uncertainty may explain the lack of demand for committing to reducing their debt. This evidence may provide micro evidence to understand why some consumers simultaneously co-hold high-interest debt and low-interest liquid cash. They have a need for liquidity with limited liquid cash available over relatively short time periods. This explanation appears most in line with Telyukova (2013)'s structural model. Such limited liquidity may also mean other policies ultimately fail to change real outcomes.

VII Concluding Discussion

We show how an active choice nudge significantly changes consumer Autopay enrollment choices but has no real economic effects on reducing credit card debt. This is explained by offsetting consumer responses and consumers holding limited liquid cash balances. Our study highlights the need to evaluate nudges on their real economic effects and, where possible, do so with ex-ante tests. Otherwise consumer financial protection regulations that sound appealing – and may even change enrollment or other proximate choices – may be introduced that are costly and ineffective at changing distal real economic outcomes e.g., as is only discovered ex-post with the US CARD Act disclosures (Agarwal et al., 2015; Keys and Wang, 2019). If nudges are unable to change real economic outcomes, there is an increased need to research the trade-offs of hard, paternalistic policies (e.g., Loewenstein and Chater, 2017; Laibson, 2020; Chater and Loewenstein, 2022).

VIII Figures & Tables

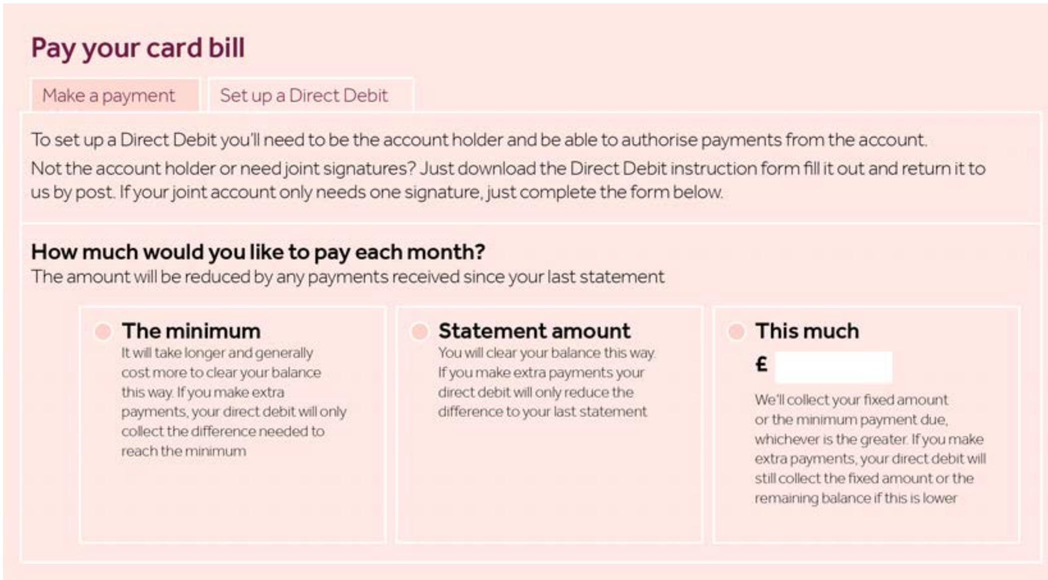
Figure 1: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount, shown by Autopay enrollment



Notes: N = 7,938. Dotted lines are control group where minimum payment amount is displayed. Solid lines are treatment group where minimum payment amount is shrouded. Color of lines show Autopay enrollment observed in administrative data.

Figure 2: Autopay enrollment choice architecture presented to control (panel A) and treatment (panel B) groups

A: Control



B: Treatment

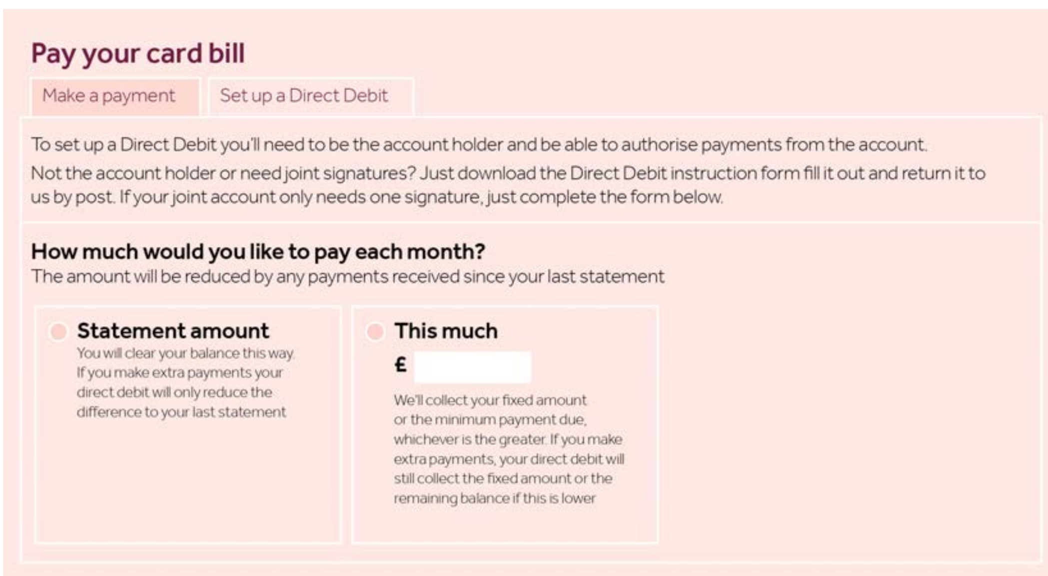
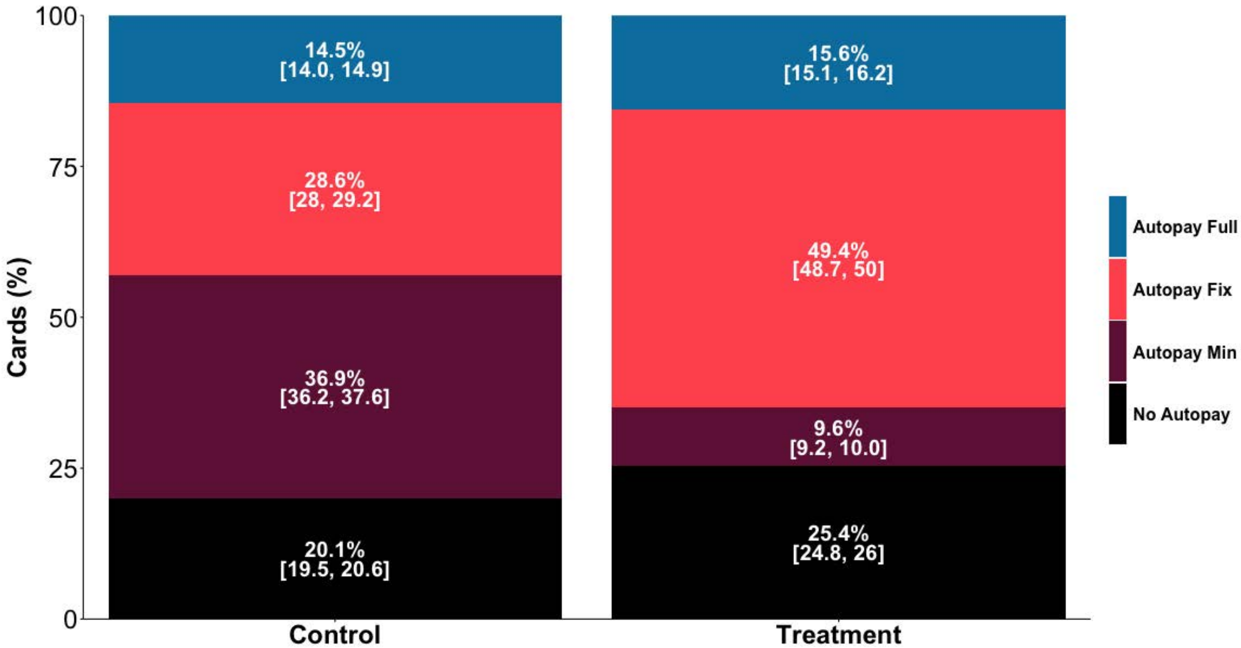
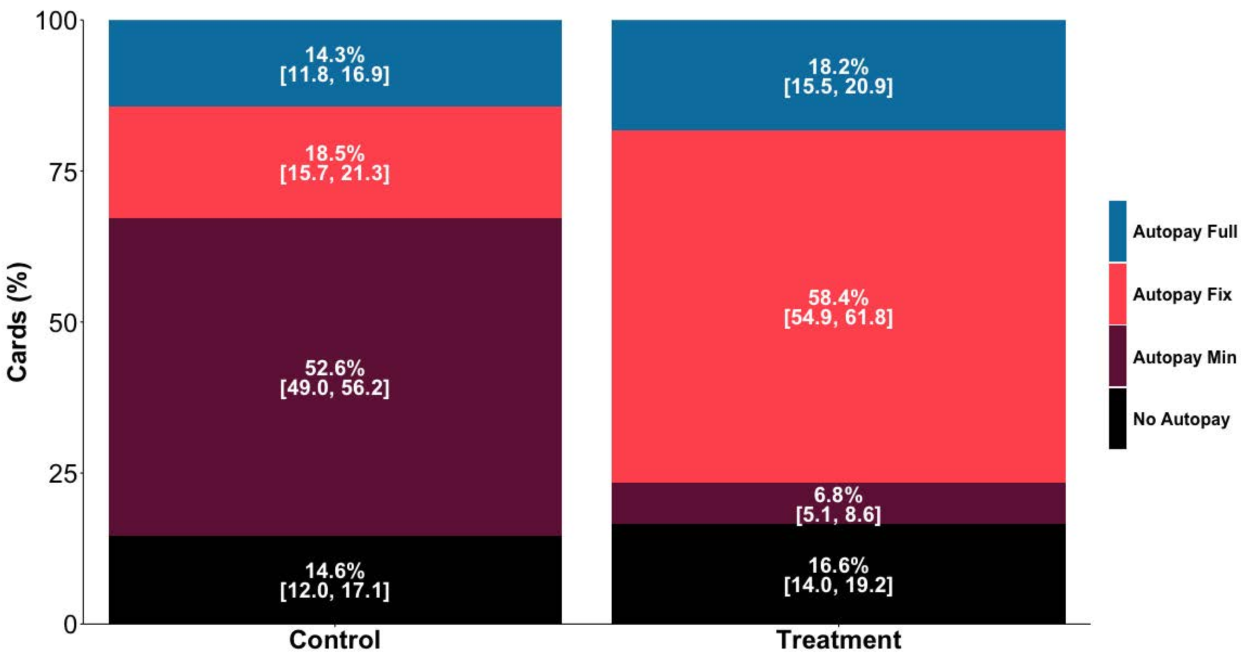


Figure 3: Autopay enrollment for control and treatment groups after two statements, split by lender

B: MAIN LENDER (N = 40,708)

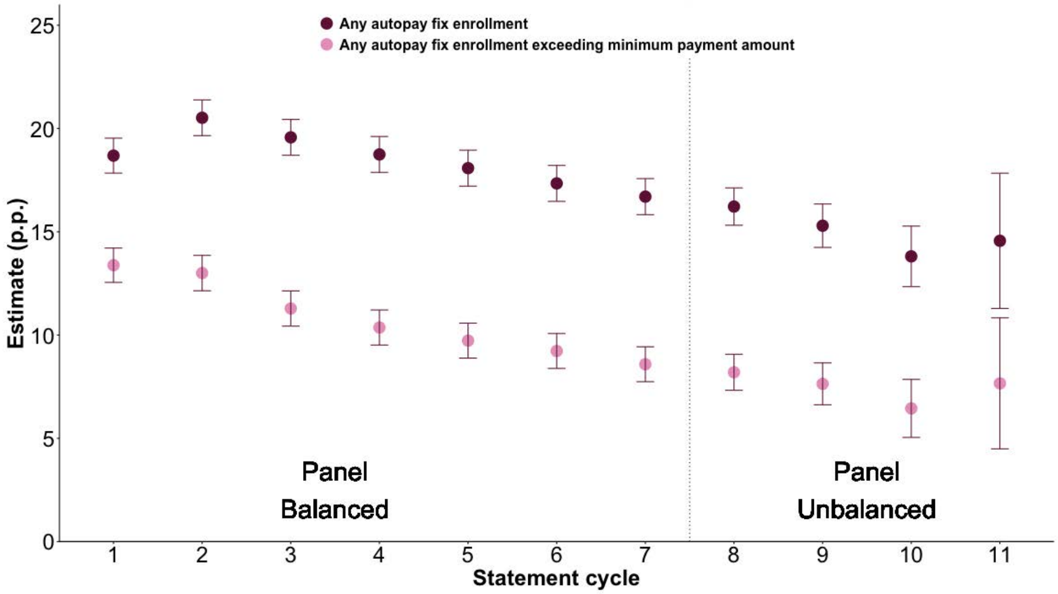


B: SECOND LENDER (N = 1,531)



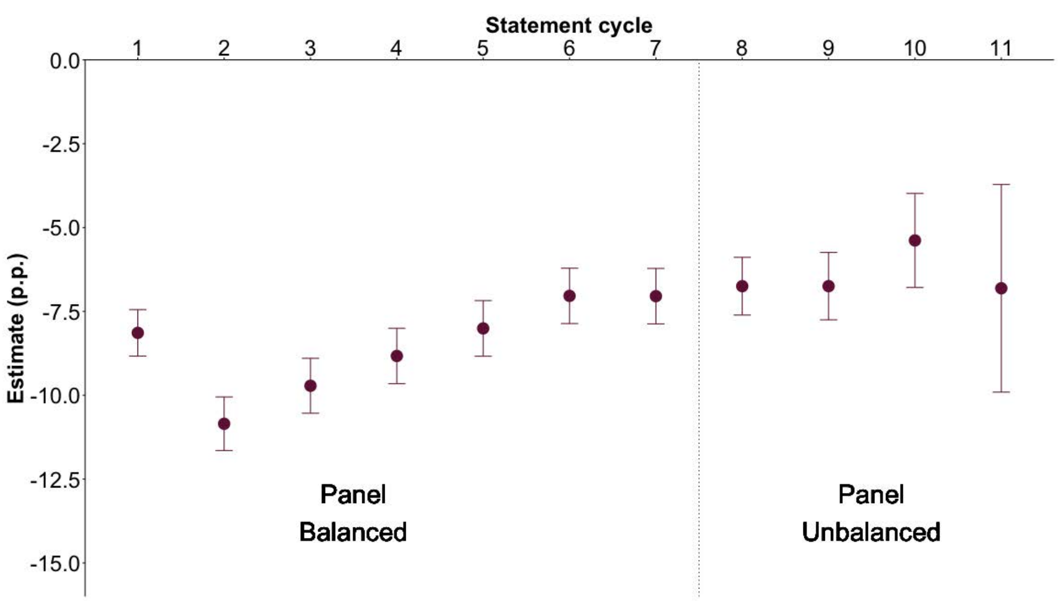
Notes: Numbers display percentage of cards enrolled in each type of Autopay by the second statement cycle. 95% confidence intervals in [ ].

Figure 4: Average treatment effects on Autopay Fix enrollment (purple) and Autopay Fix enrollment not binding at minimum payment (pink) across 1-11 statement cycles



Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

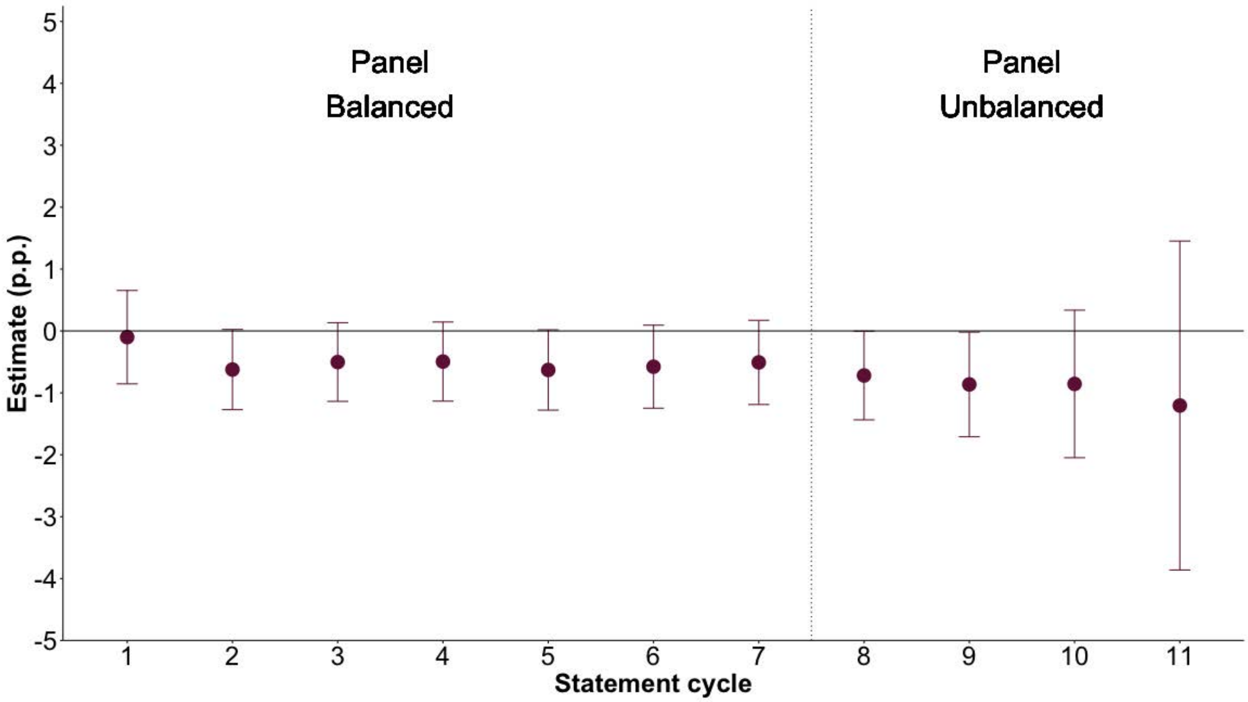
Figure 5: Average treatment effects on making only a minimum payment across 1-11 statement cycles



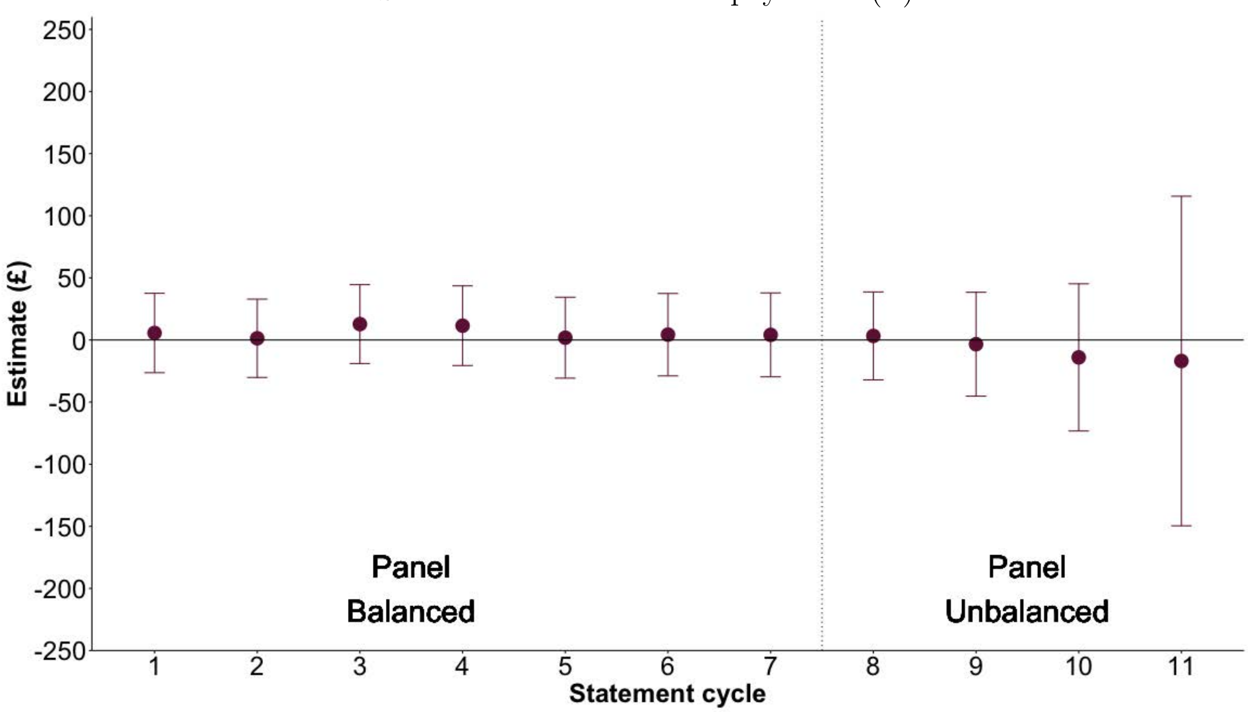
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure 6: Average treatment effects on credit card debt across 1-11 statement cycles

A: Statement balance net of payments (% statement balance)



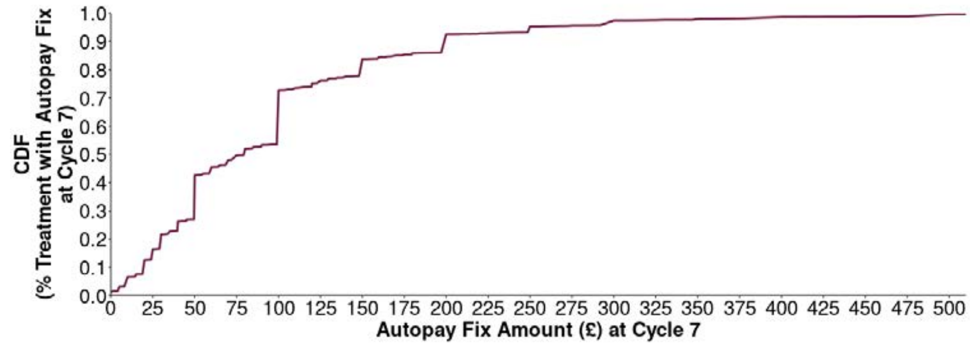
B: Statement balance net of payments (£)



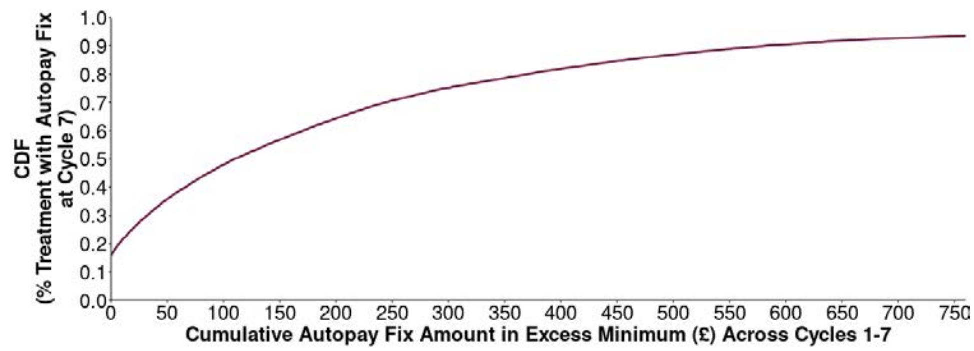
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure 7: CDF of Autopay Fix payment amounts for those enrolled in Autopay Fix in the treatment group after seven statements

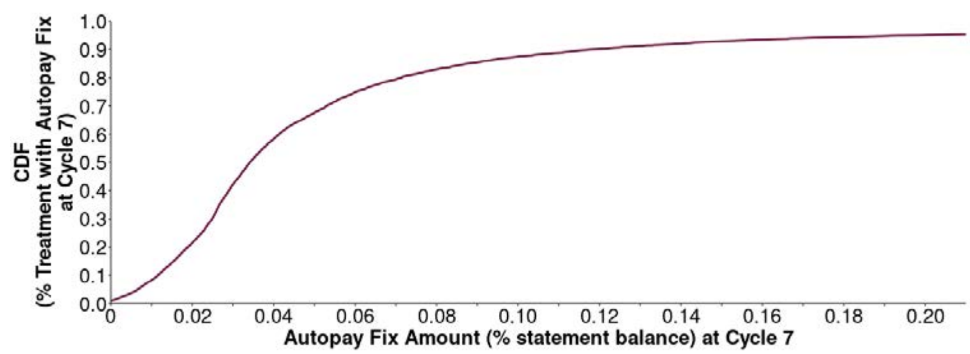
A: Autopay fix amount (£) at cycle 7



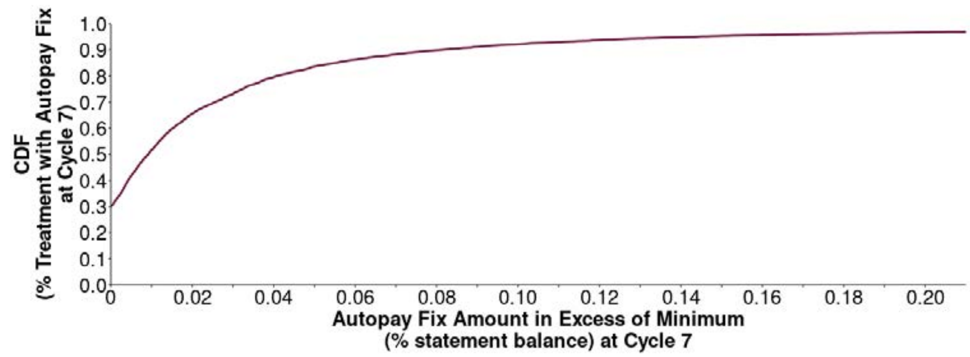
B: Cumulative autopay fix amount in excess of minimum (£) across cycles 1-7



C: Autopay fix amount (% statement balance) at cycle 7



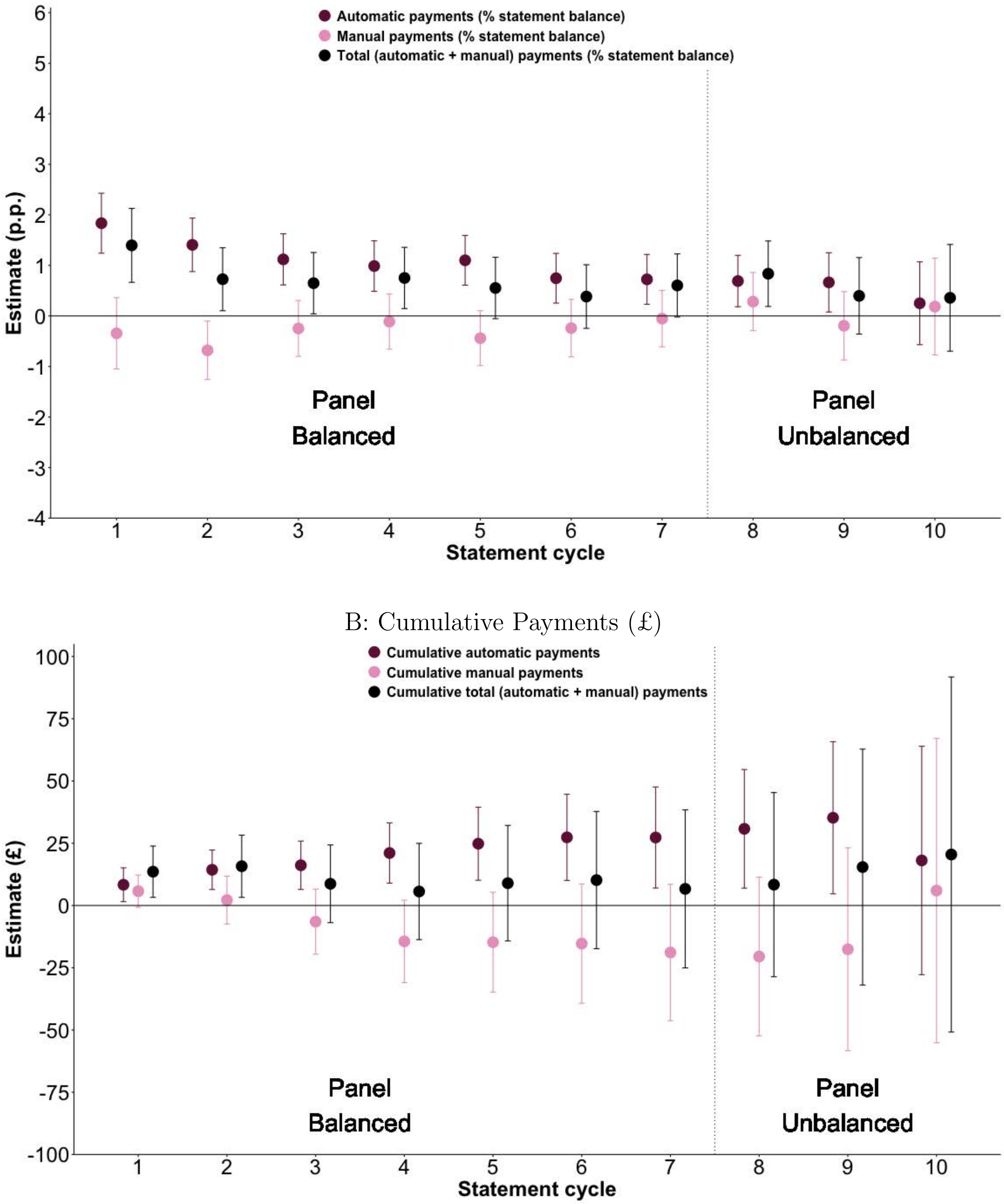
D: Autopay fix amount in excess of minimum (% statement balance) at cycle 7



Notes: X-axes of CDFs are right-censored to ease presentation.

Figure 8: Average treatment effects on automatic, manual, and total (automatic + manual) payments across 1-10 statement cycles

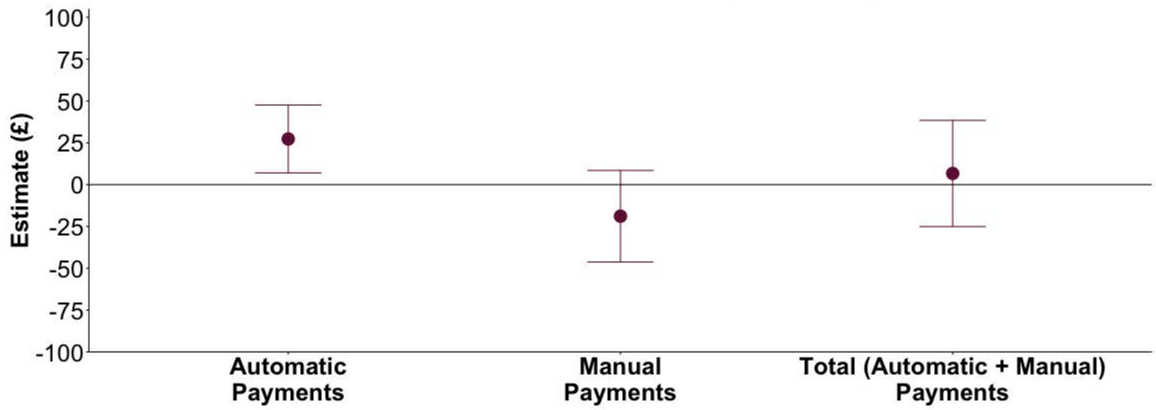
A: Payments (% statement balance)



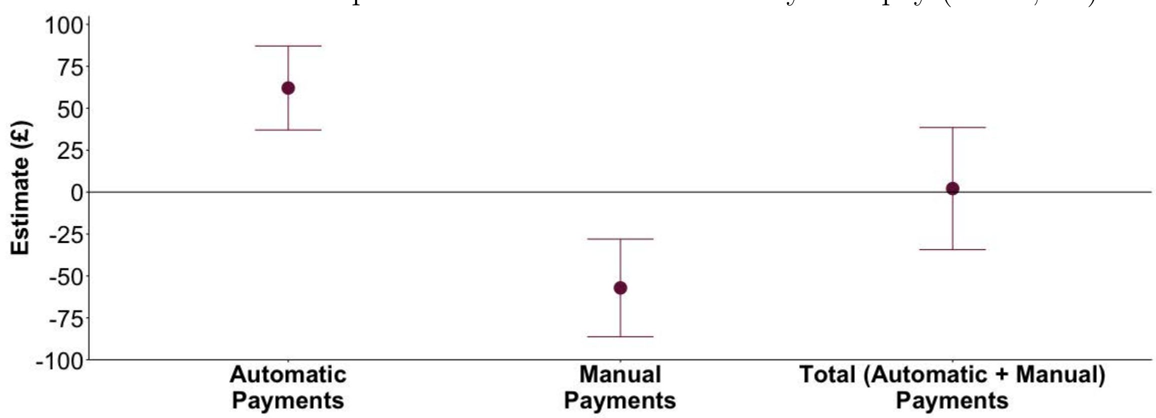
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals on Panel B are extremely large such that estimates are uninformative.

Figure 9: Estimates on cumulative payments decomposed by any Autopay enrollment after seven statement cycles

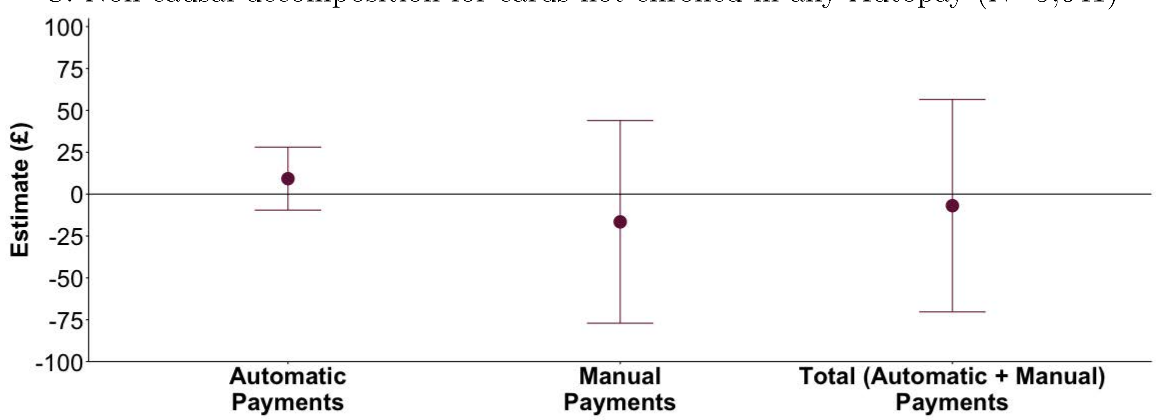
A: Causal estimate for all cards (N=40,693)



B: Non-causal decomposition for cards enrolled in any Autopay (N=31,052)



C: Non-causal decomposition for cards not enrolled in any Autopay (N=9,641)

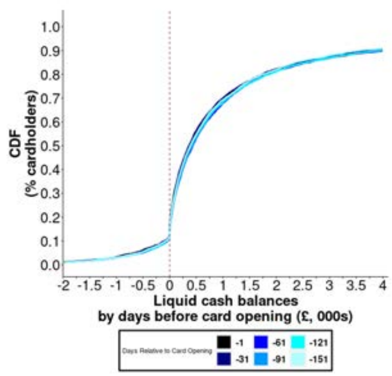


Notes: Panel A is causal estimated treatment effects from coefficients (δ_7) in OLS regression specified in Equation 1. Panels B and C show non-causal estimates (δ_7) from OLS regression specified in Equation 3. Standard errors clustered at consumer-level. Error bars are 95% confidence intervals.

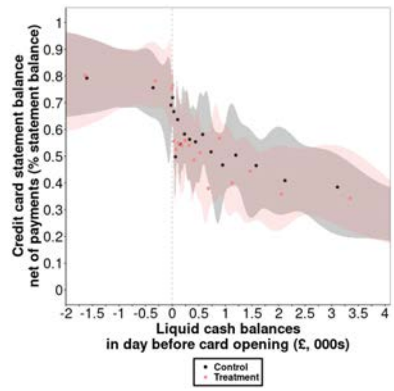
Figure 10: CDFs of liquid cash balances measured before card opening (left hand side panels) and their non-parametric relationships with credit card debt (statement balance net of payments as a % of statement balance) at statement cycle 7, by treatment group (right hand side panels)

I: Liquid cash balance at day 30 before card opening (£)

A CDF

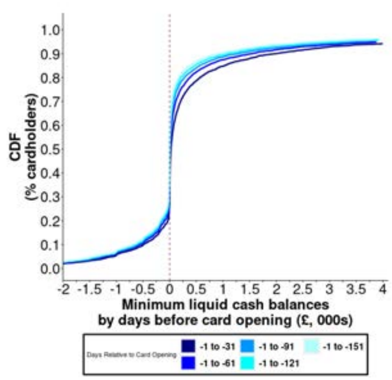


D. Relationship

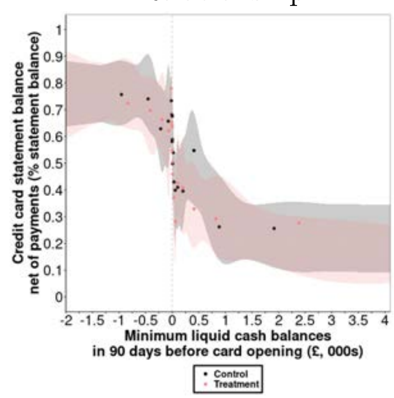


II: Minimum liquid cash balance reached during 90 days before card opening (£)

B. CDF

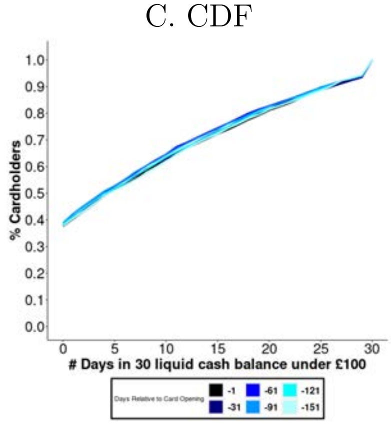


E. Relationship

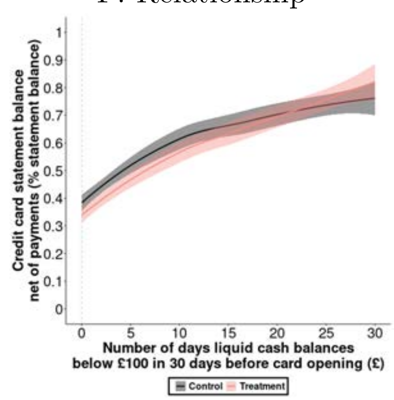


III: Number of days liquid cash balance below £100 in 30 days before card opening (# days)

C. CDF



F. Relationship



Notes: N = 3,753 consumers. Liquid cash balances are measured before credit card opening. Panels A., B. and C. are CDFs. Panel F. is loess, Panels D. and E. are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes of A, B, D, and E are censored to ease presentation given a fat tail to the distribution of these variables.

Table 1: Average treatment effects on Autopay enrollment after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any autopay	-0.0437*** (0.0041)	[-0.0517, -0.0356]	0.0000	0.7811
Autopay full	0.0065* (0.0028)	[0.0009, 0.0120]	0.0217	0.1309
Autopay fix	0.1670*** (0.0045)	[0.1583, 0.1757]	0.0000	0.2955
Autopay min	-0.2172*** (0.0041)	[-0.2251, -0.2092]	0.0000	0.3547
Autopay fix exceeding minimum payment amount	0.0859*** (0.0043)	[0.0774, 0.0943]	0.0000	0.2523

Table 2: Average treatment effects for primary outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any minimum payment	-0.0705*** (0.0042)	[-0.0787, -0.0622]	0.0000	0.3012
Any full payment	0.0040 (0.0037)	[-0.0032, 0.0112]	0.2747	0.2397
Any missed payment	0.0038* (0.0019)	[0.0002, 0.0075]	0.0409	0.0369
Statement balance net of payments (% statement balance)	-0.0051 (0.0035)	[-0.0119, 0.0017]	0.1428	0.6936
Costs (% statement balance)	-0.0003 (0.0006)	[-0.0015, 0.0010]	0.6782	0.0111
Transactions (% statement balance)	0.0025 (0.0031)	[-0.0036, 0.0087]	0.4199	0.2007
Share of credit card portfolio only paying minimum	-0.0264*** (0.0027)	[-0.0317, -0.0210]	0.0000	0.2012
Share of credit card portfolio making full payment	0.0011 (0.0033)	[-0.0054, 0.0076]	0.7340	0.4414
Share of credit card portfolio missing payment	-0.0000 (0.0013)	[-0.0025, 0.0024]	0.9701	0.0236
Credit card portfolio balances net of payments (% statement balances)	-0.0053 (0.0031)	[-0.0115, 0.0008]	0.0896	0.6954

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects after seven statement cycles. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 1 that includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.*

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Internet Appendix accompanying "*The Semblance of Success in Nudging Consumers to Pay Down Credit Card Debt*"

Internet Appendix Sections:

A. Survey Experiment

B. Field Experiment: Main Lender

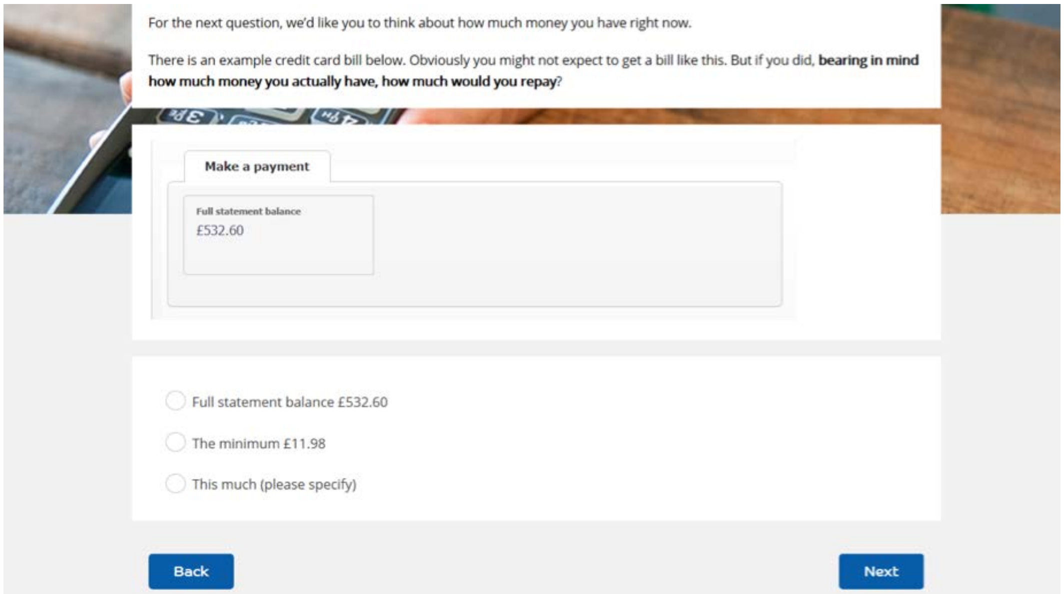
C. Field Experiment: Second Lender

D. Liquid Cash Balances

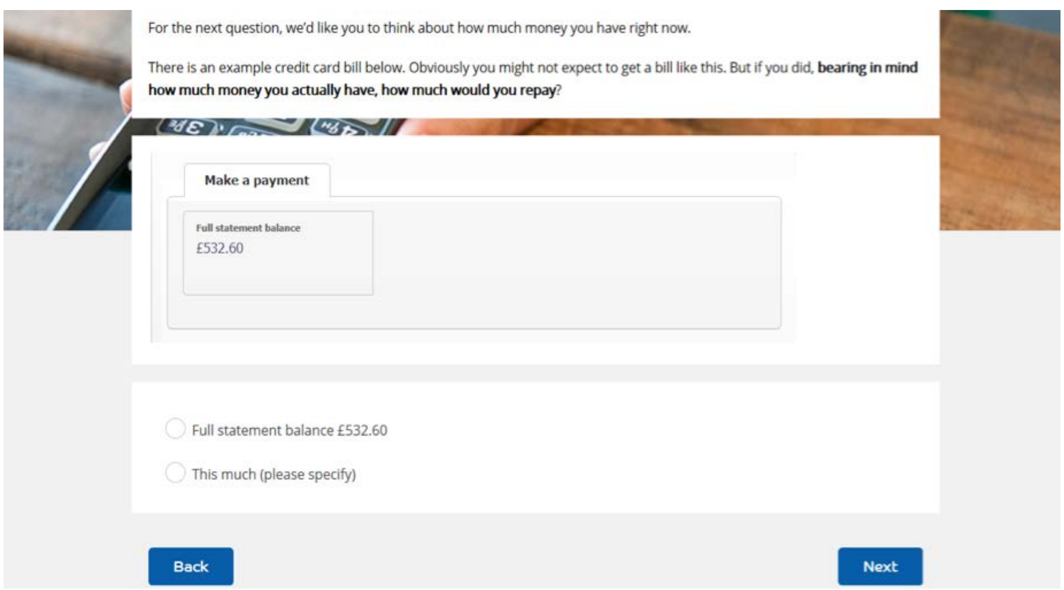
A. Survey Experiment

Figure A1: Choice architecture in survey experiment presented to control (panel A) and treatment (panel B) groups

A: Control

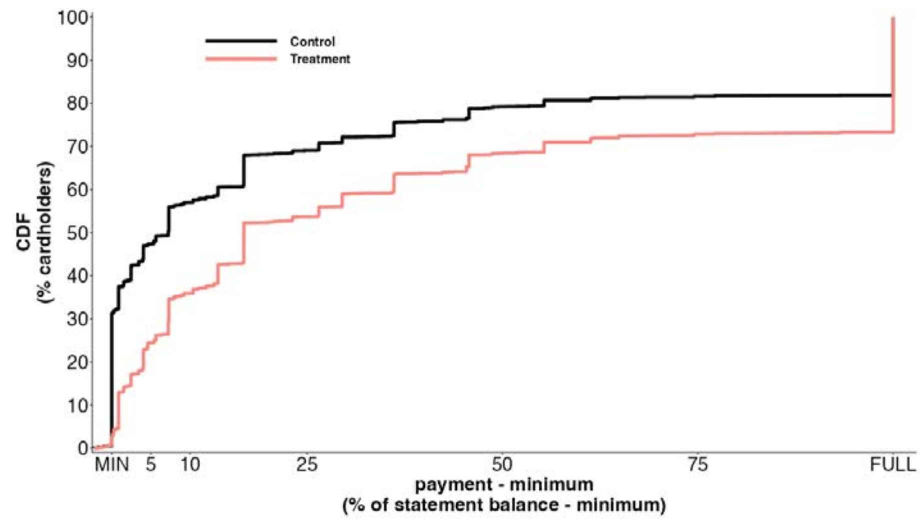


B: Treatment



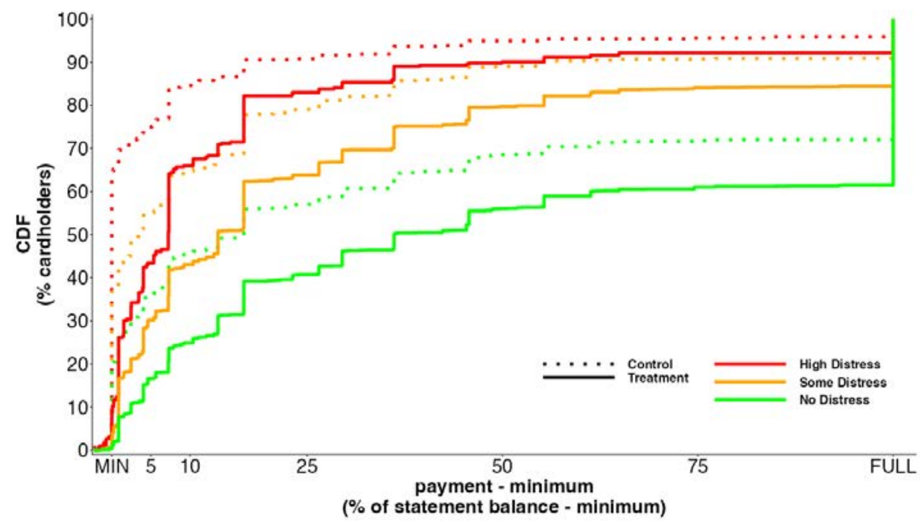
Notes: Survey experiment where treatment shrouds minimum payment amount. Consumers have to decide how much to pay on a hypothetical credit card balance. Consumers were randomized into (i) control or treatment and (ii) a low or high statement balance scenarios. Example is shown for low statement balance scenario. The high balance scenario was identical except with a statement balance amount of £3,217.36 and a minimum payment amount due of £72.38.

Figure A2: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount



Notes: N = 7,938. Black line is control group where minimum payment amount is displayed. Orange line is treatment group where minimum payment amount is shrouded.

Figure A3: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount, shown by self-reported financial distress



Notes: N = 7,938. Dotted lines are control group where minimum payment amount is displayed. Solid lines are treatment group where minimum payment amount is shrouded. Color of lines show self-reported financial distress.

Table A1: Average treatment effects on hypothetical credit card payments from survey experiment where treatment shrouds minimum payment amount

	(1) Payment (% statement balance)	(2) Any full payment	(3) Any minimum payment	(4) Any missed payment
Intercept	0.3783***	0.2408***	0.2491***	0.0000
	(0.0072)	(0.0081)	(0.0073)	(0.0010)
High Balance	-0.1951***	-0.1223***	0.1144***	0.0127***
	(0.0082)	(0.0088)	(0.0075)	(0.0019)
Treatment	0.1240***	0.0840***	-0.2947***	0.0020
	(0.0082)	(0.0092)	(0.0073)	(0.0019)
Control Mean	0.2813	0.1800	0.3060	0.0063

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N = 7,938. Table shows coefficients on high balance scenario indicator (baseline low balance), treatment effect indicator (baseline control) from OLS regressions predicting hypothetical credit card payment decision from survey experiment. Robust standard errors in parenthesis.*

Estimating heterogeneous treatment effects by self-reported financial distress

We estimate an OLS regression (with robust standard errors) shown in Equation 4. We include dummies for if the respondent (i) is randomly assigned to the high balance amount presented ($HighBalance_i$) and is randomly assigned to the de-anchoring treatment ($Treatment_i$).

We use an official UK self-reported measure of financial distress used by the Office for National Statistics. Gathergood and Guttman-Kenney (2016) shows this measure is correlated with other measures of financial distress as well as also with measures of subjective well-being. Respondents are asked how well they are keeping up with bills and commitments and we split responses into three groups indicating the severity of financial distress: no distress (the omitted category), some distress, and high distress. The survey question is: "Which of the following statements best describes how well you are keeping up with your bills and credit commitments at the moment?" Respondents can choose from the following options: "1. Keeping up with all of them without any difficulties; 2. Keeping up with all of them, but it is a struggle from time to time; 3. Keeping up with all of them, but it is a constant struggle; 4. Falling behind with some of them; 5. Having real financial problems and have fallen behind with many of them; 6. Don't have any commitments". For analysis we classify responses 1 and 6 as 'no distress'; 2 as 'some distress'; and 3, 4, and 5 as 'high distress'. 52% of respondents report no distress, 38% some distress, and 11% high distress.

$$\begin{aligned} Y_i = \alpha + \beta\ HighBalance_i + \gamma_1\ SomeDistress_i + \gamma_2\ HighDistress_i + \delta\ Treatment_i + \\ \theta_1\big(\ Treatment_i \times SomeDistress_i\big) + \theta_2\ \big(Treatment_i \times HighDistress_i\big) + \varepsilon_i \end{aligned} \tag{4}$$

The results of this estimation are in Table 4. Financially distressed respondents are more likely to pay less than the minimum, more likely to only pay exactly the minimum, and less likely to pay the full balance. The effect of the treatment de-anchoring manual payments is significantly lower for the most distressed respondents.

Table A2: Heterogeneous treatment effects by self-reported financial distress on hypothetical credit card payments from survey experiment where treatment shrouds minimum payment amount

	(1) Payment (% statement balance)	(2) Any full payment	(3) Any minimum payment	(4) Any missed payment
Intercept	0.4829***	0.3373***	0.1419***	-0.0044
	(0.0095)	(0.0110)	(0.0085)	(0.0013)
High Balance	-0.1948***	-0.1220***	0.1142***	0.0126***
	(0.0077)	(0.0088)	(0.0072)	(0.0019)
Some Distress	-0.1949***	-0.1860***	0.1640***	0.0027
	(0.0110)	(0.0120)	(0.0147)	(0.0020)
High Distress	-0.2836***	-0.2410***	0.4119***	0.0310***
	(0.0132)	(0.0131)	(0.0241)	(0.0084)
Treatment	0.1344***	0.1057***	-0.1947***	0.0016
	(0.0123)	(0.0144)	(0.0087)	(0.0016)
Treatment × Some Distress	-0.0197	-0.0444*	-0.1540***	0.0020
	(0.0164)	(0.0186)	(0.0152)	(0.0033)
Treatment × High Distress	-0.0534**	-0.0680***	-0.3766***	-0.0005
	(0.0207)	(0.0216)	(0.0262)	(0.0126)

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N = 7,938 of which 4,100 self-report no financial distress, 3,001 some financial distress, and 837 high financial distress. Table shows coefficients on high balance scenario indicator (baseline low balance), treatment effect indicator (baseline control), self-reported financial distress (baseline is no distress), and interaction treatment and financial distress from OLS regressions predicting hypothetical credit card payment decision from survey experiment. Robust standard errors in parenthesis.*

B. Field Experiment: Main Lender

Definitions of Primary Outcomes

1. **Any minimum payment:** Binary outcome for target card. Defined as only paying exactly the minimum (unless that is zero or equal to the full statement balance).

2. **Any full payment:** Binary outcome for target card. Defined as paying the full statement balance (or if no payment is due because there's a zero statement balance).

3. **Any missed payment:** Binary outcome for target card. Defined as paying zero or less than the minimum.

4. **Statement balance net of payments (% statement balance):** Continuous outcome for target card as a measure of credit card debt. Defined as the value of statement balance net of payments as a percent of the value of statement balance. This is the fraction of credit card debt remaining after payments.

5. **Costs (% statement balance):** Continuous outcome for target card a measure of the costs of borrowing. Defined as the sum of credit card interest and fees as a percentage of statement balance.

6. **Transactions (% statement balance):** Continuous outcome for target card a measure of consumption. Defined as the sum of the value of new credit card transactions that statement cycle as a percentage of statement balance.

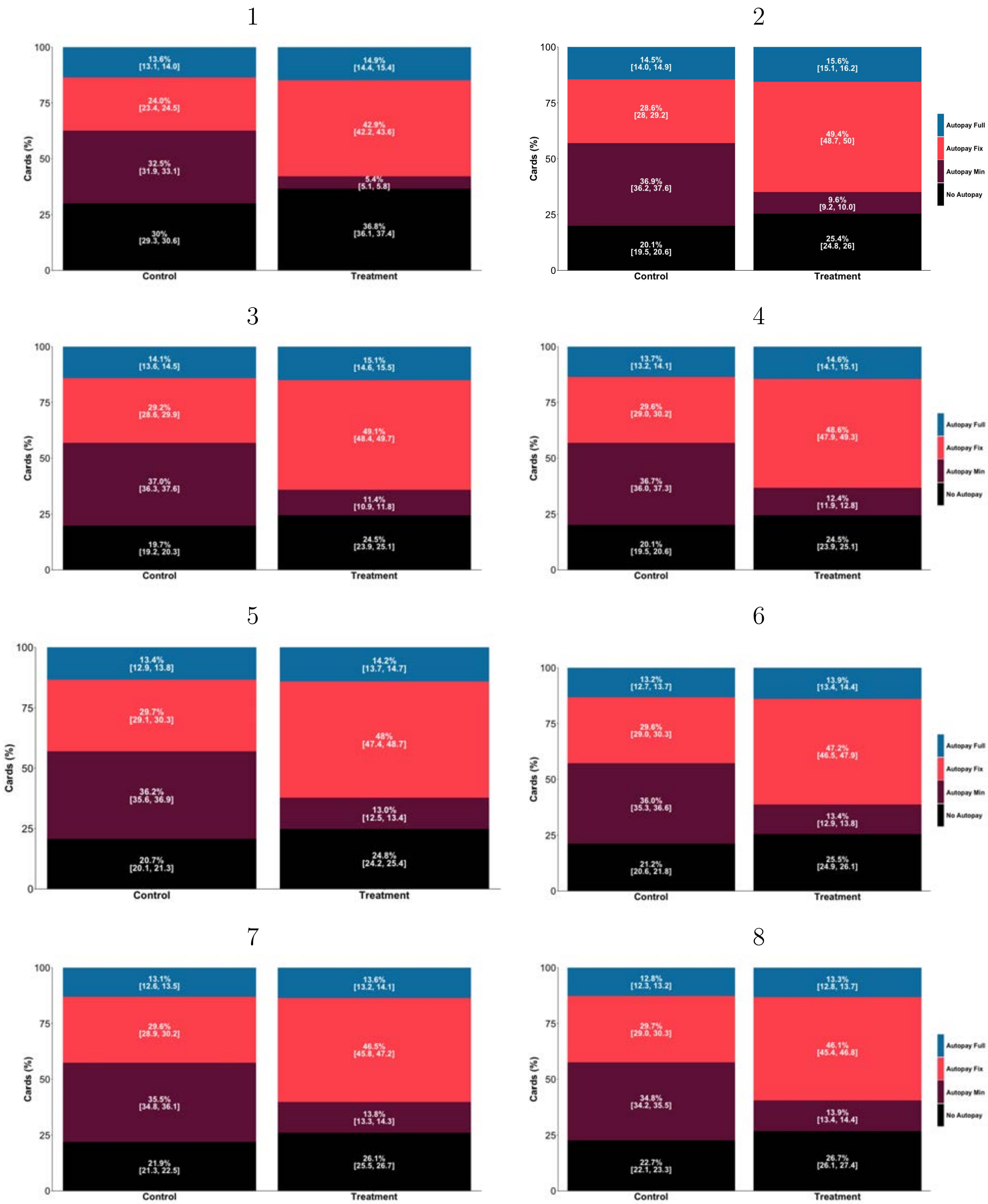
7. **Share of credit card portfolio only paying minimum:** Outcome ranging from zero to one. Defined as the proportion of credit cards paying exactly the minimum (unless that is zero or equal to the full balance).

8. **Share of credit card portfolio making full payment:** Outcome ranging from zero to one. Defined as the proportion of credit cards paying the full statement balance (or if no payment is due because there's a zero statement balance).

9. **Share of credit card portfolio missing payment:** Outcome ranging from zero to one. Defined as the proportion of credit cards paying zero or less than the minimum.

10. **Credit card portfolio balances net of payments (% statement balances):** Continuous outcome for credit card portfolio. Defined as the aggregated value of statement balances net of payments across the credit card portfolio as a percent of the aggregated value of statement balances across credit card portfolio. This is the fraction of credit card debt portfolio remaining after payments.

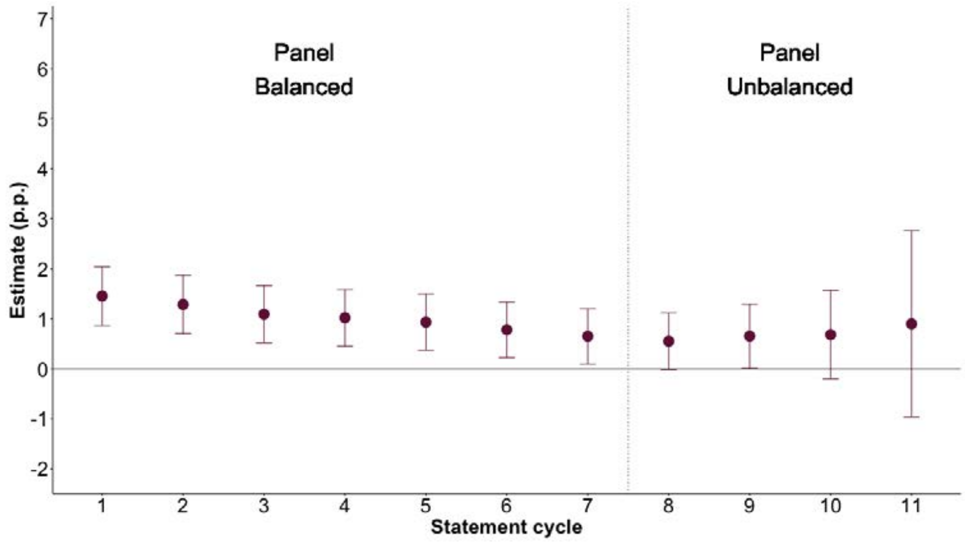
Figure B1: Autopay enrollment for control and treatment groups, by statement cycles one to eight



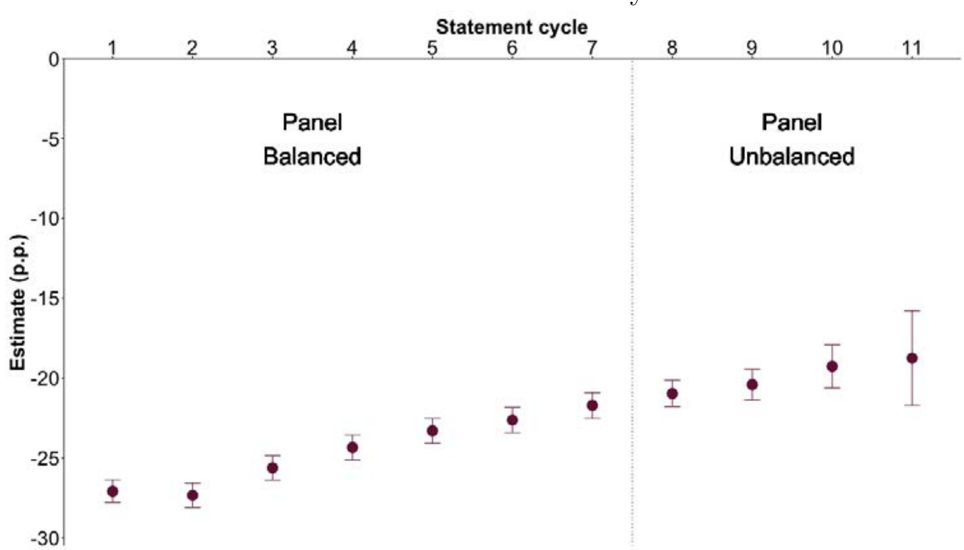
Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ]. Cycle 1 is before all treated cards have had 30 days to experience the treatment. Not all cards are observed in cycle 8.

Figure B2: Average treatment effects on automatic full (panel A) and minimum (panel B) payment enrollment across 1-11 statement cycles

A: Automatic Full Payment

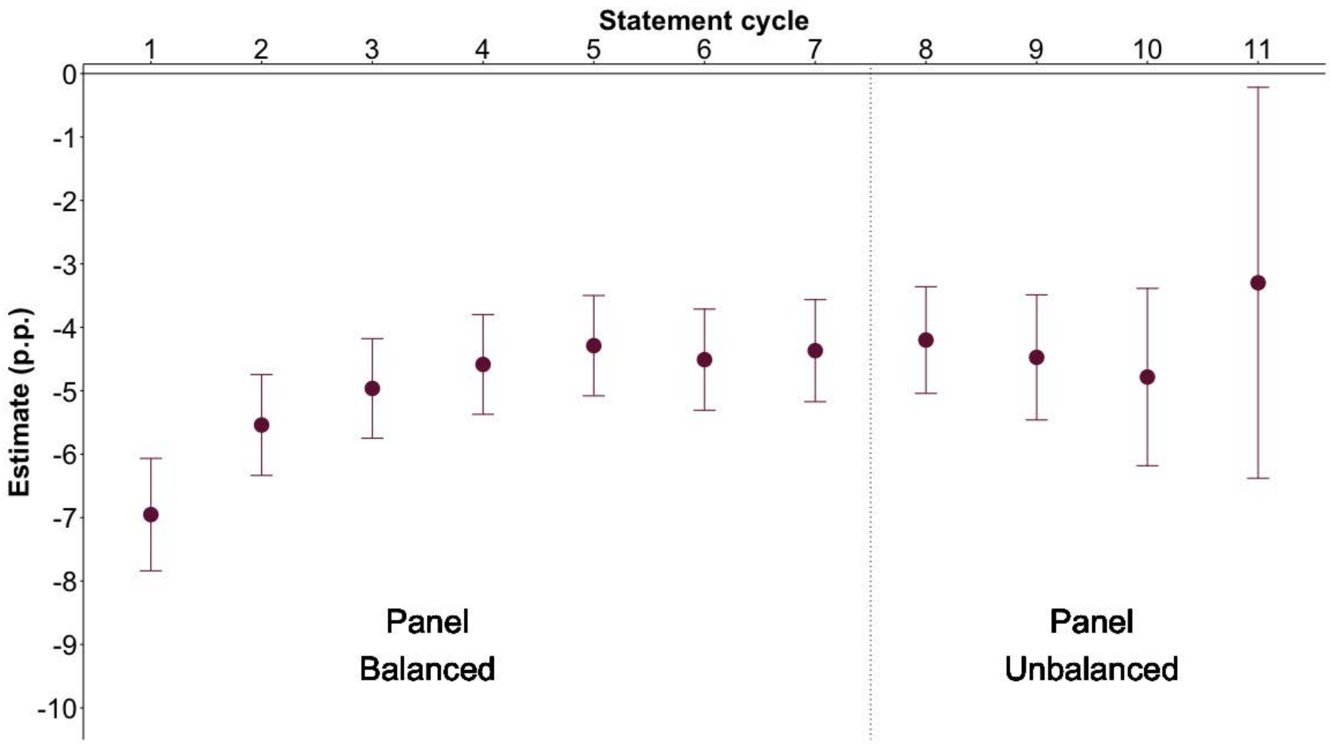


B: Automatic Minimum Payment



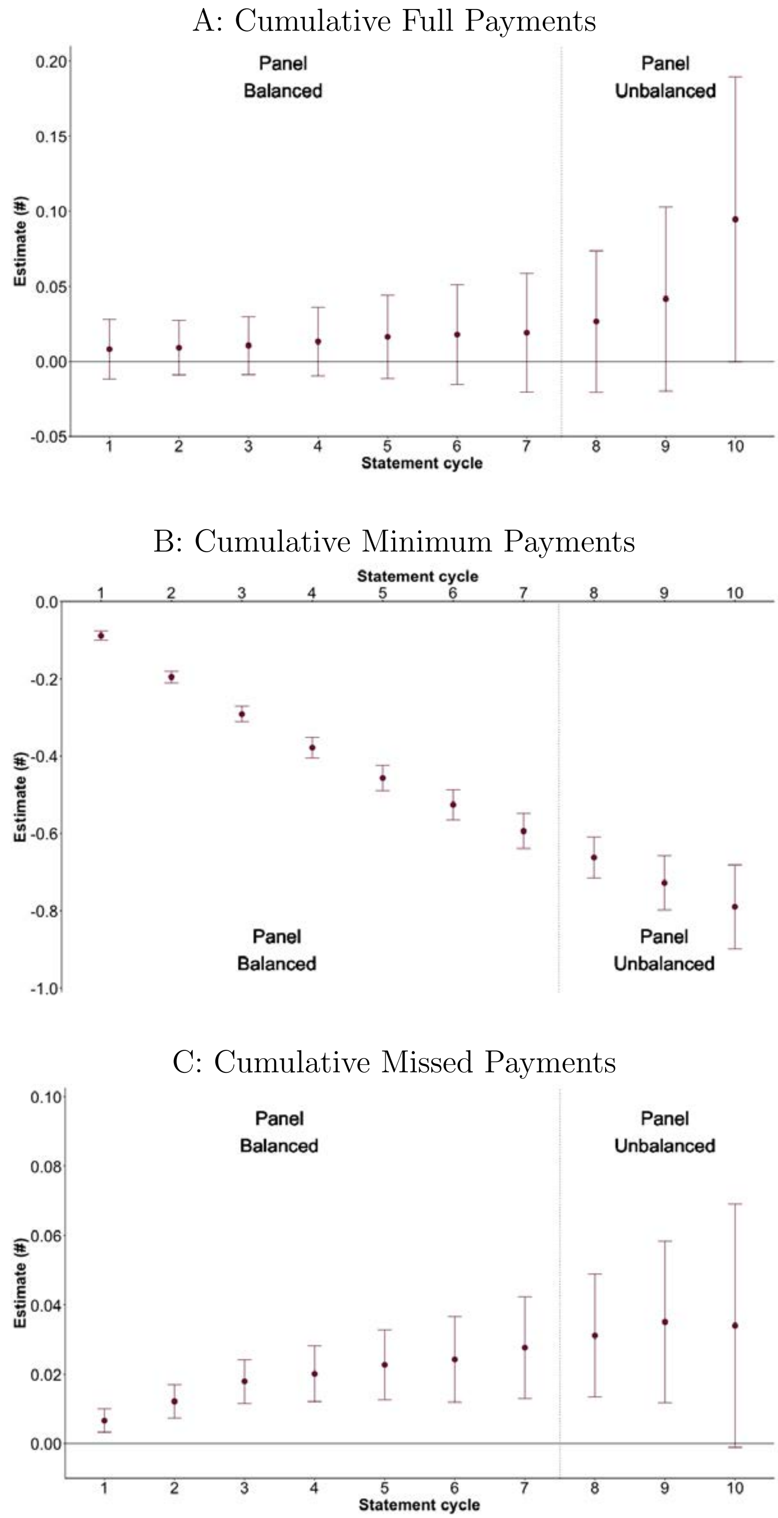
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure B3: Average treatment effects on any Autopay enrollment across 1-11 statement cycles



Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

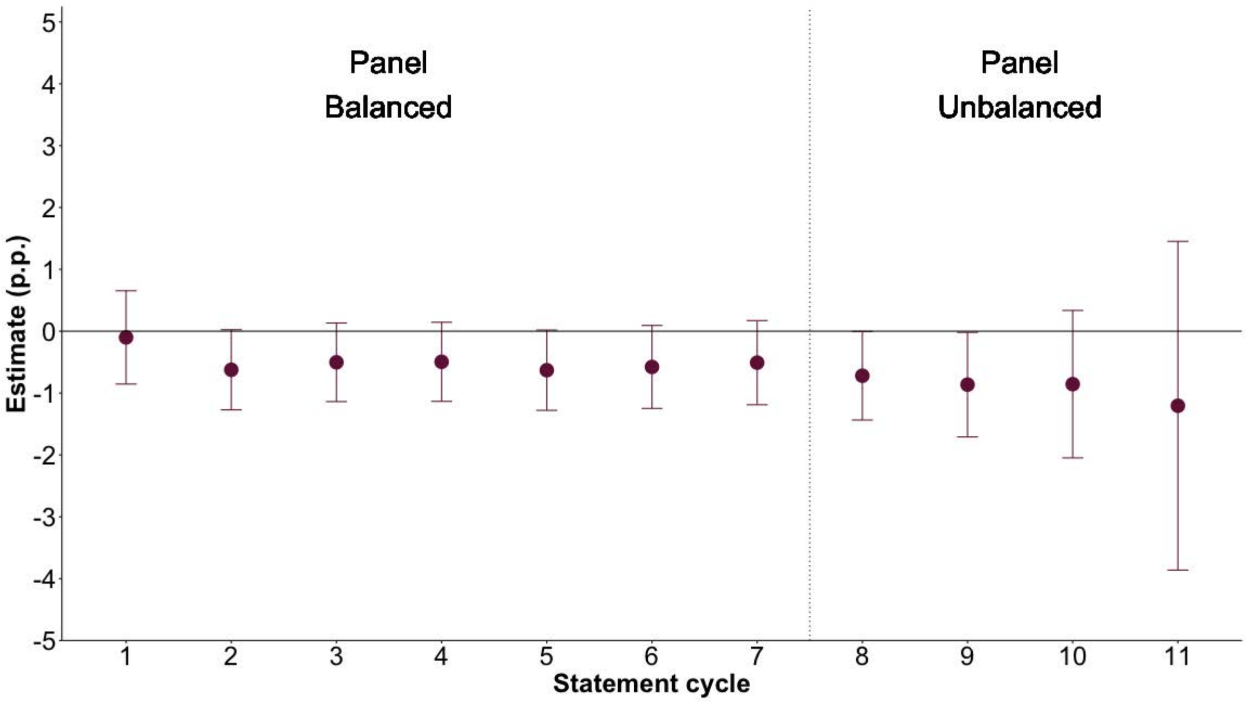
Figure B4: Treatment effects on cumulative number of full, minimum and missed payments across 1-10 statement cycles



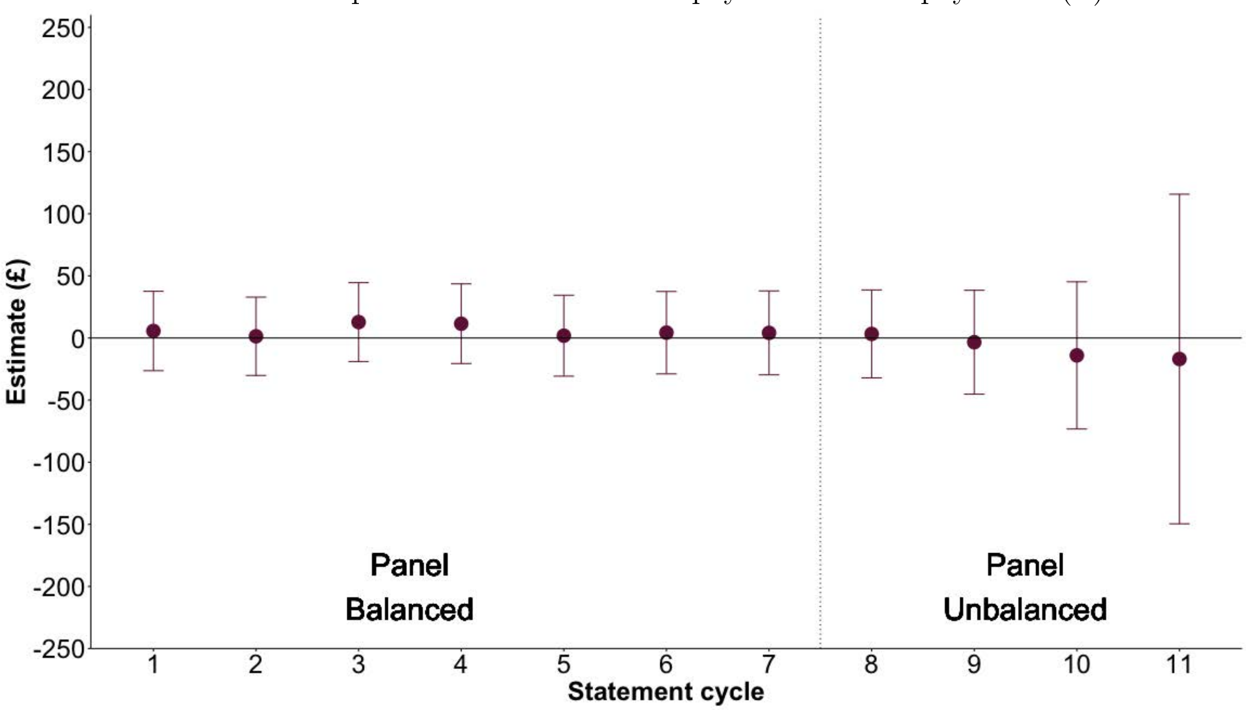
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that estimates are uninformative.

Figure B5: Average treatment effects on credit card portfolio debt across 1-11 statement cycles

A: Credit card portfolio balances net of payments (% statement balances)

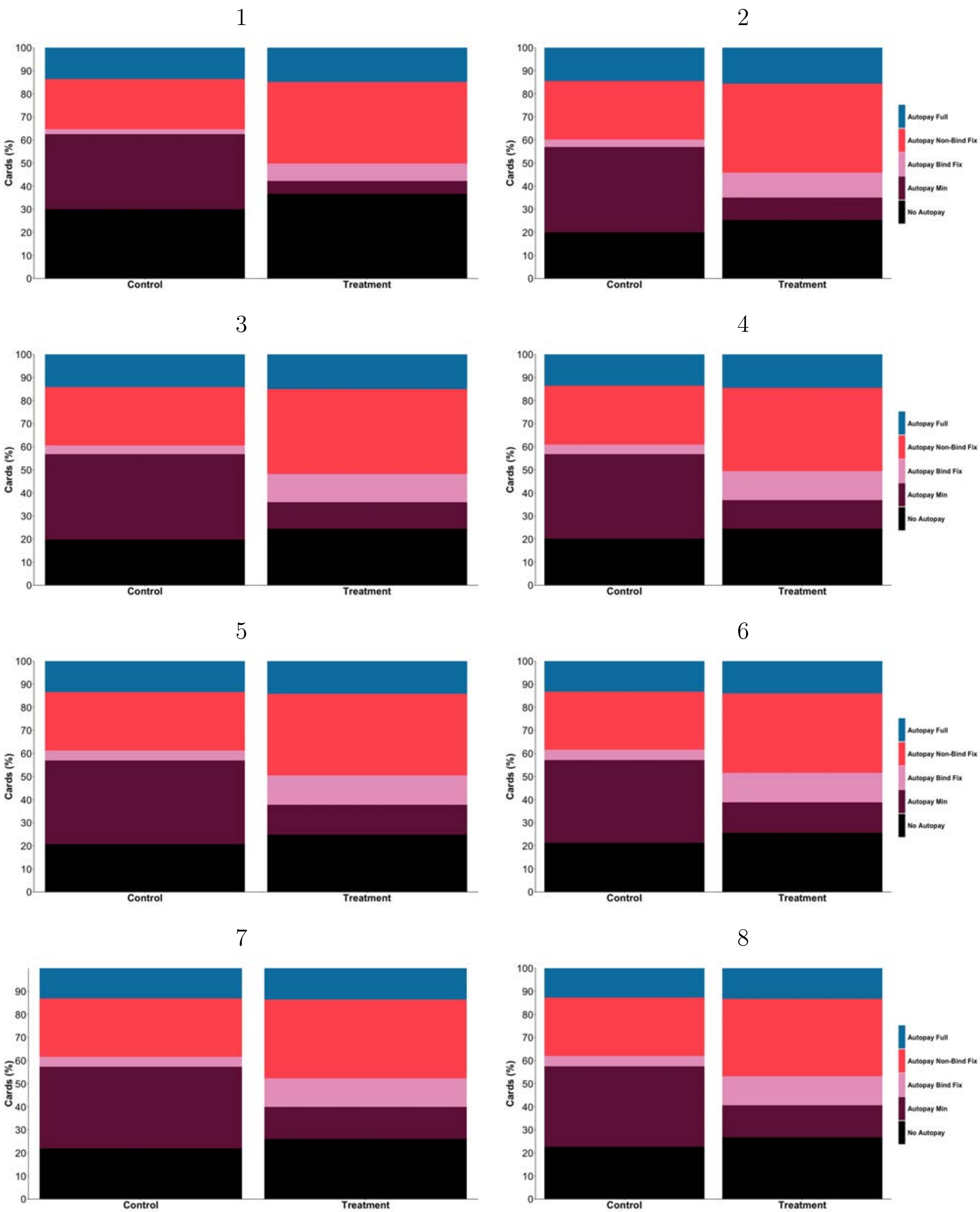


B: Credit card portfolio balances net of payments net of payments (£)



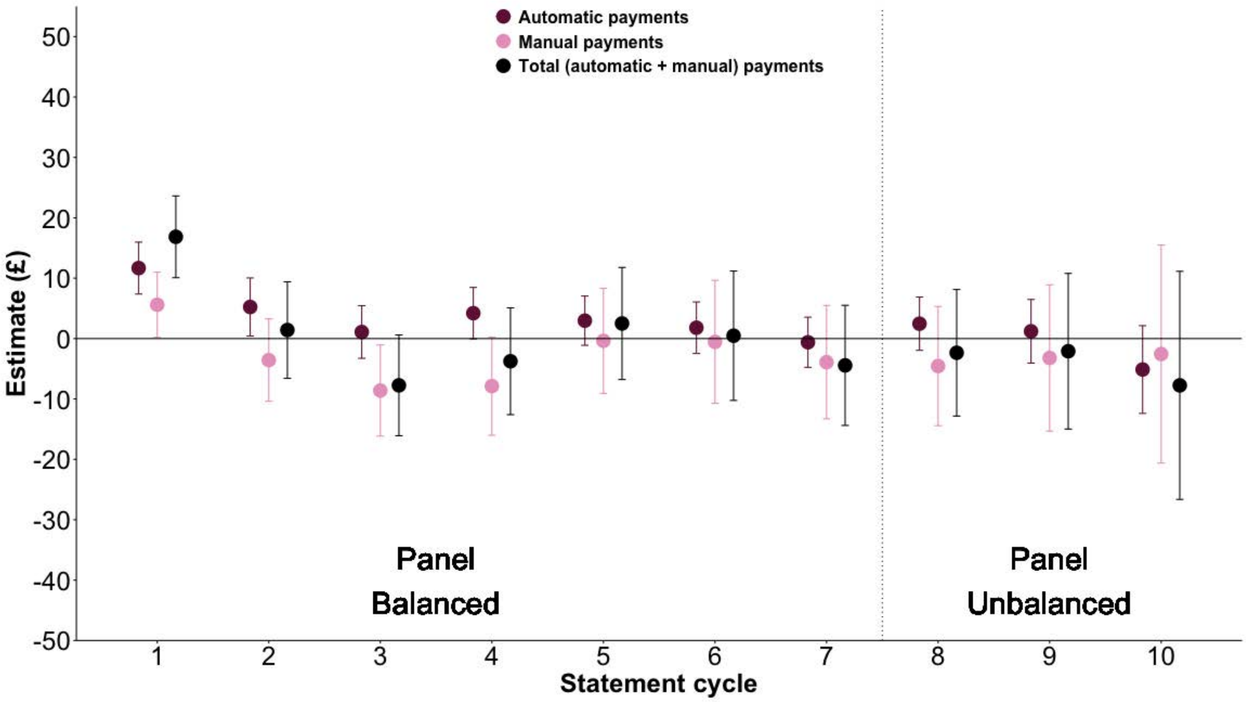
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure B6: Autopay enrollment - splitting out automatic fixed payments into those that do and do not bind at the minimum payment amount - for control and treatment groups split by statement cycles one to eight



Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ].

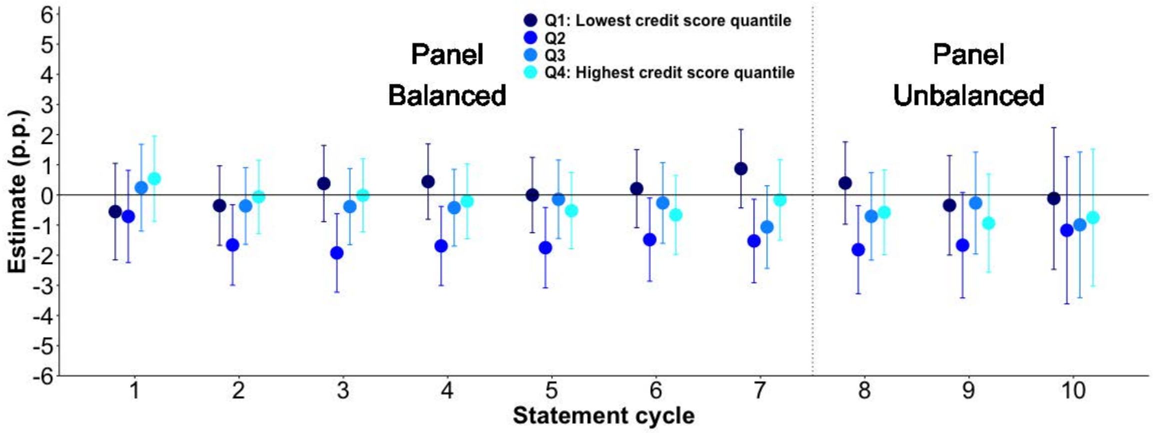
Figure B7: Average treatment effects on automatic, manual and total (automatic + manual) payments across 1-10 statement cycles



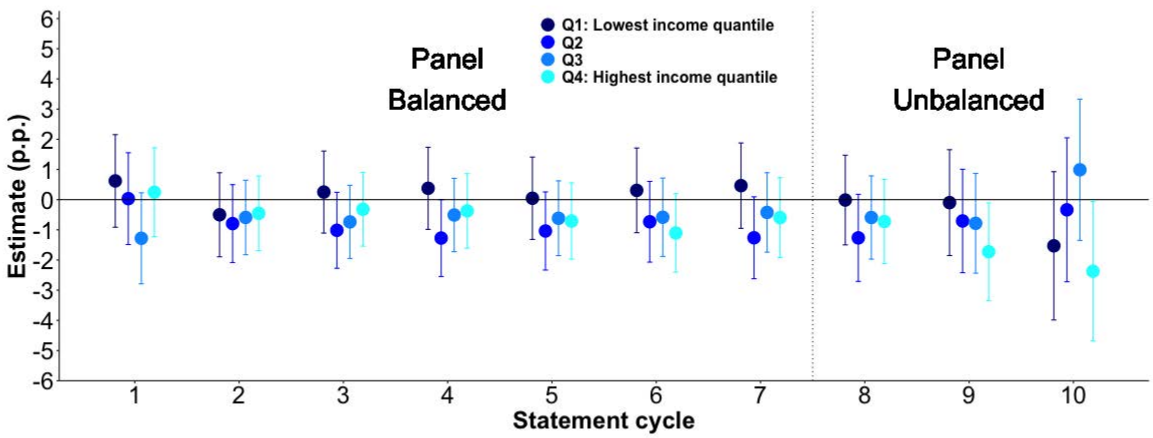
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that estimates are uninformative.

Figure B8: Heterogeneous treatment effects by quartiles of (A) credit score, (B) income and (C) unsecured debt-to-income (DTI) ratio on credit card debt (statement balance net of payments % statement balance) across 1-10 statement cycles

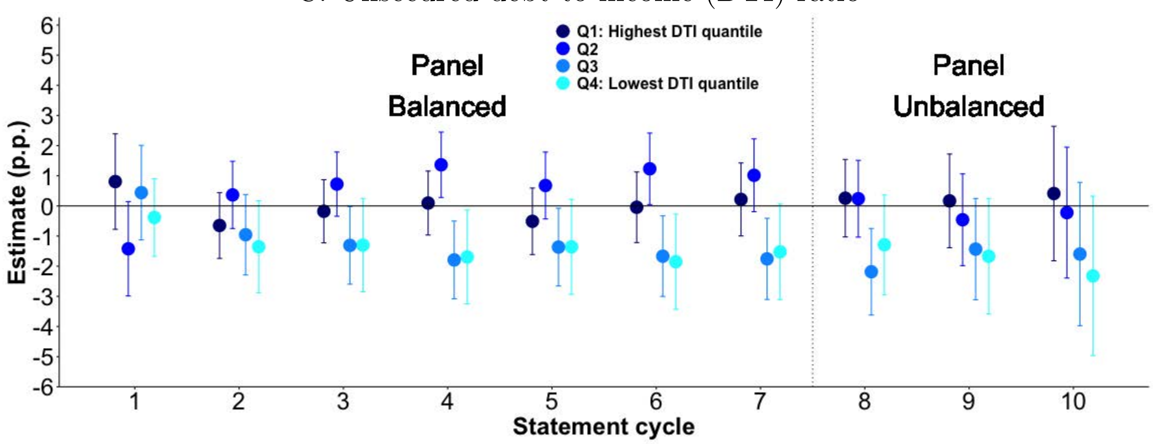
A: Credit Score



B: Income



C: Unsecured debt-to-income (DTI) ratio



Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Heterogeneous variables calculated from credit file data in month preceding credit card opening (& trial start). Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that estimates are uninformative.

Table B1: Summary statistics

Outcome	Mean	S.D.	P10	P25	P50	P75	P90
Age (years)	36.46	12.44	23	27	34	45	54
Female (% cards)	0.46	0.50	0	0	0	1	1
Credit limit (£)	4356.81	3366.08	660	1,400	3,800	6,300	9,000
Any credit score	0.99	0.12	1	1	1	1	1
Credit score (0-100)	0.65	0.07	0.560	0.610	0.660	0.700	0.740
Purchases rate (%)	22.85	6.11	18.900	18.900	18.900	29.900	34.900
Any balance transfer debt	0.43	0.50	0	0	0	1	1
Any estimated income	0.97	0.18	1	1	1	1	1
Estimated income (£)	2437.38	2155.22	899	1,321	1,880	2,816	4,336
Any autopay	0.78	0.41	0	1	1	1	1
Autopay full	0.13	0.34	0	0	0	0	1
Autopay fix	0.30	0.46	0	0	0	1	1
Autopay min	0.35	0.48	0	0	0	1	1
Statement balance (£)	2164.49	2416.30	0	373	1,290	3,274	5,437
Statement balance net of payments (£)	1962.52	2369.65	0	41	1,086	3,070	5,162
Statement balance net of payments (% statement balance)	0.69	0.41	0	0.180	0.950	0.980	0.980
Utilization	0.52	0.37	0	0.200	0.530	0.840	0.980
Any minimum payment	0.30	0.46	0	0	0	1	1
Any full payment	0.24	0.43	0	0	0	0	1
Any missed payment	0.04	0.19	0	0	0	0	0
Cumulative number times paid minimum	2.04	2.63	0	0	0	4	7
Cumulative number times paid in full	1.90	2.56	0	0	1	3	7
Cumulative number times paid less than minimum	0.19	0.76	0	0	0	0	0
6+ times paid minimum	0.19	0.39	0	0	0	0	1
6+ times paid in full	0.18	0.38	0	0	0	0	1
6+ times paid less than minimum	0.01	0.07	0	0	0	0	0
Number of credit cards	2.80	1.90	1	1	2	4	5
Number of credit cards with debt	1.52	1.36	0	1	1	2	3
Credit card portfolio statement balances (£)	3916.96	5142.72	90	626	2,284	5,143	9,734
Credit card portfolio balances net of payments (£)	3431.69	4849.58	0	255	1,851	4,597	8,830

Notes: Summary statistics are calculated for control group (N = 20,609) after 7th statement cycle.

Table B2: Minimum Detectable Effect (MDE) sizes for primary outcomes at cycle 7 across significance levels 0.005, 0.01 & 0.05 (all assuming 80% power)

	Significance Thresholds		
Outcome	0.005	0.01	0.05
Any minimum payment	0.0160	0.0150	0.0123
Any full payment	0.0155	0.0145	0.0119
Any missed payment	0.0070	0.0065	0.0053
Statement balance net of payments (% statement balance)	0.0149	0.0140	0.0114
Costs (% statement balance)	0.0023	0.0022	0.0018
Transactions (% statement balance)	0.0127	0.0119	0.0098
Share of credit card portfolio only paying minimum	0.0108	0.0101	0.0083
Share of credit card portfolio making full payment	0.0136	0.0127	0.0104
Share of credit card portfolio missing payment	0.0048	0.0045	0.0037
Credit card portfolio balances net of payments (% statement balances)	0.0141	0.0132	0.0108

Table B3: Balance comparison

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Age (years)	36.4641	36.6078	0.1437	[-0.0985, 0.3860]
Female (% cards)	0.4606	0.4612	0.0006	[-0.0091, 0.0103]
Any estimated income	0.9660	0.9630	-0.0030	[-0.0066, 0.0006]
Estimated income (£)	2437.3804	2457.5071	20.1267	[-21.9344, 62.1877]
Credit limit (£)	4356.8067	4429.0296	72.2228*	[6.3640, 138.0817]
Any credit score	0.9856	0.9834	-0.0023	[-0.0047, 0.0001]
Credit score (0-100)	0.6526	0.6538	0.0012	[-0.0003, 0.0026]
Purchases rate (%)	22.8479	22.8168	-0.0311	[-0.1496, 0.0874]
Any balance transfer offered	0.2900	0.2976	0.0076	[-0.0013, 0.0164]
Number of credit cards	2.1757	2.1917	0.0160	[-0.0204, 0.0524]
Number of credit cards with debt	0.8998	0.9135	0.0136	[-0.0080, 0.0352]
Credit card portfolio statement balances (£)	2364.9238	2439.0881	74.1643	[-0.7909, 149.1194]
Credit card portfolio balances net of payments (£)	2001.3480	2072.5311	71.1832*	[2.5927, 139.7736]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 20,617 and N (treatment) = 20,091 cards.*

Table B4: Unconditional mean comparison of treatment effects for Autopay enrollment after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Any autopay	0.7811	0.7393	-0.0418***	[-0.0501, -0.0335]
Autopay full	0.1309	0.1364	0.0056	[-0.0011, 0.0122]
Autopay fix	0.2955	0.4649	0.1694***	[0.1601, 0.1787]
Autopay min	0.3547	0.1380	-0.2167***	[-0.2248, -0.2086]
Autopay <£5 fix	0.0028	0.0146	0.0118***	[0.0100, 0.0136]
Autopay fix exceeding minimum payment amount	0.2523	0.3401	0.0878***	[0.0789, 0.0966]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 20,617 and N (treatment) = 20,091 cards.*

Table B5: Unconditional mean comparison of treatment effects for primary outcomes after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Any minimum payment	0.3012	0.2323	-0.0689***	[-0.0775, -0.0603]
Any full payment	0.2397	0.2417	0.0019	[-0.0064, 0.0102]
Any missed payment	0.0369	0.0403	0.0034	[-0.0003, 0.0071]
Statement balance net of payments (% statement balance)	0.6936	0.6910	-0.0026	[-0.0106, 0.0054]
Costs (% statement balance)	0.0111	0.0107	-0.0004	[-0.0016, 0.0009]
Transactions (% statement balance)	0.2007	0.2013	0.0006	[-0.0062, 0.0075]
Share of credit card portfolio only paying minimum	0.2012	0.1775	-0.0237***	[-0.0295, -0.0179]
Share of credit card portfolio making full payment	0.4414	0.4424	0.0011	[-0.0062, 0.0084]
Share of credit card portfolio missing payment	0.0236	0.0231	-0.0004	[-0.0030, 0.0021]
Credit card portfolio balances net of payments (% statement balances)	0.6954	0.6912	-0.0042	[-0.0118, 0.0034]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 20,617 and N (treatment) = 20,091 cards.*

Table B6: Unconditional mean comparison of treatment effects for secondary outcomes after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Cumulative number times paid in full	1.9020	1.9081	0.0061	[-0.0439, 0.0560]
Cumulative number times paid minimum	2.0444	1.4594	-0.5850***	[-0.6329, -0.5372]
Cumulative number times paid less than minimum	0.1892	0.2153	0.0261***	[0.0110, 0.0412]
Cumulative total payments (£)	1277.2667	1288.3119	11.0453	[-22.8990, 44.9895]
Cumulative automatic payments (£)	573.7899	605.2636	31.4737***	[9.6362, 53.3112]
Cumulative manual payments (£)	711.9684	693.1835	-18.7850	[-46.7112, 9.1412]
Total payments (% statement balance)	0.2271	0.2305	0.0034	[-0.0040, 0.0107]
Automatic payments (% statement balance)	0.1101	0.1164	0.0062*	[0.0007, 0.0118]
Manual payments (% statement balance)	0.1212	0.1189	-0.0023	[-0.0081, 0.0035]
Made both automatic and manual payment	0.0672	0.0797	0.0125***	[0.0074, 0.0176]
Statement balance (£)	2164.4948	2203.7629	39.2681	[-7.9750, 86.5112]
Statement balance net of payments (£)	1962.5190	2005.4041	42.8851	[-3.4588, 89.2290]
Utilization	0.5223	0.5217	-0.0006	[-0.0076, 0.0065]
Cumulative purchases (£)	3186.1868	3221.3178	35.1310	[-21.9622, 92.2242]
Credit card portfolio repayments (£)	485.7041	508.1641	22.4600*	[0.8591, 44.0608]
Credit card portfolio repayments (% statement balances)	0.2564	0.2559	-0.0005	[-0.0076, 0.0066]
Credit card portfolio statement balances (£)	3916.9554	4018.9441	101.9887*	[1.1026, 202.8748]
Credit card portfolio balances net of payments (£)	3431.6852	3510.7800	79.0948	[-15.6258, 173.8153]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 20,617 and N (treatment) = 20,091 cards.*

Table B7: Average treatment effects for secondary outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Cumulative number times paid in full	0.0192 (0.0201)	[-0.0203, 0.0586]	0.3405	1.9020
Cumulative number times paid minimum	-0.5939*** (0.0232)	[-0.6393, -0.5485]	0.0000	2.0444
Cumulative number times paid less than minimum	0.0276*** (0.0075)	[0.0129, 0.0424]	0.0002	0.1892
Cumulative total payments (£)	6.6774 (16.1915)	[-25.0579, 38.4127]	0.6800	1277.27
Cumulative automatic payments (£)	27.3038** (10.3519)	[7.0141, 47.5935]	0.0084	573.79
Cumulative manual payments (£)	-18.8732 (13.9679)	[-46.2503, 8.5039]	0.1766	711.97
Total payments (% statement balance)	0.0060 (0.0032)	[-0.0002, 0.0123]	0.0579	0.2271
Automatic payments (% statement balance)	0.0072*** (0.0025)	[0.0023, 0.0122]	0.0040	0.1101
Manual payments (% statement balance)	-0.0005 (0.0028)	[-0.0061, 0.0050]	0.8477	0.1212
Made both automatic and manual payment	0.0131*** (0.0026)	[0.0080, 0.0182]	0.0000	0.0672
Statement balance (£)	-0.3284 (17.2370)	[-34.1128, 33.4561]	0.9848	2164.49
Statement balance net of payments (£)	4.1070 (17.2164)	[-29.6371, 37.8510]	0.8115	1962.52
Utilization	0.0002 (0.0032)	[-0.0061, 0.0064]	0.9604	0.5223
Cumulative purchases (£)	-7.2306 (20.9479)	[-48.2885, 33.8273]	0.7300	3186.19
Credit card portfolio repayments (£)	9.1092 (9.3858)	[-9.2870, 27.5053]	0.3318	485.70
Credit card portfolio repayments (% statement balances)	0.0017 (0.0030)	[-0.0042, 0.0076]	0.5730	0.26
Credit card portfolio statement balances (£)	23.6451 (31.1548)	[-37.4183, 84.7085]	0.4479	3916.96
Credit card portfolio balances net of payments (£)	12.0581 (30.9206)	[-48.5463, 72.6626]	0.6966	3431.69

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects after seven statement cycles. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 1 that includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.*

Table B8: Average treatment effects for primary outcomes pooled across all statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any minimum payment	-0.0807*** (0.0033)	[-0.0871, -0.0742]	0.0000	0.2943
Any full payment	0.0041 (0.0028)	[-0.0015, 0.0096]	0.1489	0.2658
Any missed payment	0.0040*** (0.0011)	[0.0019, 0.0062]	0.0002	0.0297
Statement balance net of payments (% statement balance)	-0.0056* (0.0027)	[-0.0109, -0.0003]	0.0380	0.6692
Costs (% statement balance)	-0.0001 (0.0002)	[-0.0006, 0.0003]	0.5166	0.0109
Transactions (% statement balance)	0.0012 (0.0020)	[-0.0027, 0.0052]	0.5430	0.2918
Share of credit card portfolio only paying minimum	-0.0266*** (0.0017)	[-0.0298, -0.0233]	0.0000	0.1631
Share of credit card portfolio making full payment	0.0002 (0.0023)	[-0.0043, 0.0048]	0.9190	0.5150
Share of credit card portfolio missing payment	0.0004 (0.0007)	[-0.0009, 0.0017]	0.5400	0.0144
Credit card portfolio balances net of payments (% statement balances)	-0.0036 (0.0022)	[-0.0079, 0.0006]	0.0967	0.6245

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects pooled across statement cycles. Estimates are δ coefficients from OLS regressions as specified by Equation 2 includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.*

Table B9: Average treatment effects for secondary outcomes of balances and repayments amounts pooled across all statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Statement balance (£)	3.5857 (14.9393)	[-25.6954, 32.8667]	0.8103	2049.8420
Statement balance net of payments (£)	3.9778 (14.9169)	[-25.2594, 33.2150]	0.7897	1862.3909
Total payments (£)	-0.3921 (2.2408)	[-4.7841, 3.9999]	0.8611	187.4512
Credit card portfolio statement balances (£)	30.5985 (22.2772)	[-13.0648, 74.2618]	0.1696	3506.8973
Credit card portfolio balances net of payments (£)	24.9894 (22.0307)	[-18.1908, 68.1696]	0.2567	2961.2714
Credit card portfolio repayments (£)	4.0665 (4.3278)	[-4.4159, 12.5489]	0.3474	545.7112

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects pooled across statement cycles. Estimates are δ coefficients from OLS regressions as specified by Equation 2 includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.*

Table B10: Coefficients from OLS regressions predicting correlates of drop-out of Autopay enrollment in cycle 7, split by control (column 1) and treatment (columns 2) groups

	(1)	(2)
(Intercept)	0.4803***	0.6838***
	(0.0623)	(0.0659)
Female	0.0090	0.0148
	(0.0057)	(0.0061)
Age	−0.0021***	−0.0027***
	(0.0002)	(0.0002)
Any Income Estimate	0.0719***	0.0774***
	(0.0190)	(0.0207)
Income Estimate	−0.0081***	−0.0078***
(000s)	(0.0013)	(0.0014)
Log (Credit Limit)	−0.0251***	−0.0336***
	(0.0063)	(0.0065)
Subprime	0.0185	0.0047
	(0.0138)	(0.0144)
Purchases Rate	0.0036***	0.0036***
	(0.0008)	(0.0008)
Any Balance Transfer	−0.0068	−0.0104
	(0.0066)	(0.0071)
Credit Score	−0.1336***	−0.2409***
	(0.0333)	(0.0362)
Any Mortgage Debt	−0.0241***	−0.0373***
	(0.0063)	(0.0068)
Credit Card Portfolio Statement	−0.0058**	0.0008
Balances (000s)	(0.0020)	(0.0029)
Credit Card Portfolio Statement	0.0066**	−0.0011
Balances Net of Payments (000s)	(0.0023)	(0.0031)
Number Credit Cards Portfolio	−0.0096***	−0.0128***
	(0.0021)	(0.0023)
Number Credit Cards Portfolio	−0.0146***	−0.0138**
With Debt	(0.0041)	(0.0045)
Non-Mortgage Debt Value (000s)	0.0005	0.0013***
	(0.0003)	(0.0004)

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows coefficients from OLS regression on binary outcomes. Outcome for columns 1-2 is not being enrolled in any Autopay in cycle 7. Column 1 is estimated for the cards in the control group, column 2 is for cards in the treatment group. Predictors are calculated at card opening or from credit file data in the month preceding card opening.*

Table B11: Average treatment effects for tertiary arrears outcomes pooled across all statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any missed payment	0.0040*** (0.0011)	[0.0019, 0.0062]	0.0002	0.0297
Arrears 1+ payments behind	0.0031*** (0.0010)	[0.0011, 0.0051]	0.0024	0.0267
Arrears 2+ payments behind	0.0004 (0.0007)	[-0.0009, 0.0018]	0.5476	0.0110
Arrears 3+ payments behind	0.0002 (0.0005)	[-0.0009, 0.0012]	0.7677	0.0071
Share of credit card portfolio missing payment	0.0004 (0.0007)	[-0.0009, 0.0017]	0.5400	0.0144

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects pooled across statement cycles. Estimates are δ coefficients from OLS regressions as specified by Equation 2 includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations. The first row is our 3rd primary outcome: defined as paying zero or less than the minimum due (on the 'target' card in the experiment). The last row is our 9th primary outcome: defined as the proportion of credit cards paying zero or less than the minimum due (constructed from credit file data containing the portfolio of credit card held). All other rows show effects for non-primary outcomes for the card in the experiment: standard industry point-in-time measures for the number of payments in arrears was when payments became due.*

Table B12: Coefficients from OLS regressions predicting correlates of making both an automatic and manual payment in cycle 7 (columns 1-2) or across cycles 1-7 (columns 3-4) among subsample of cardholders enrolled in autopay min or fix at cycle 7, split by control (columns 1 and 3) and treatment (columns 2 and 4)

	(1)	(2)	(3)	(4)
Intercept	0.1984***	0.3664***	0.6083***	0.8283***
	(0.0552)	(0.0669)	(0.0898)	(0.0982)
Female	0.0074	0.0116	0.0043	0.0194
	(0.0051)	(0.0059)	(0.0081)	(0.0087)
Age	−0.0009***	−0.0019***	−0.0035***	−0.0043***
	(0.0002)	(0.0003)	(0.0004)	(0.0004)
Any Income Estimate	−0.0127	0.0030	0.0498	0.0403
	(0.0190)	(0.0220)	(0.0282)	(0.0298)
Income Estimate	0.0018	0.0015	−0.0001	0.0033
(000s)	(0.0012)	(0.0014)	(0.0021)	(0.0022)
Log (Credit Limit)	−0.0081	−0.0171*	−0.0117	−0.0300**
	(0.0054)	(0.0064)	(0.0089)	(0.0096)
Subprime	−0.0207	0.0056	0.0080	−0.0238
	(0.0131)	(0.0157)	(0.0200)	(0.0220)
Purchases Rate	0.0018	−0.0002	0.0013	0.0036*
	(0.0008)	(0.0010)	(0.0012)	(0.0014)
Any Balance Transfer	−0.0063	−0.0257***	0.0058	−0.0280**
	(0.0056)	(0.0062)	(0.0092)	(0.0098)
Credit Score	−0.0156	0.0063	−0.1174	−0.1255
	(0.0314)	(0.0348)	(0.0489)	(0.0518)
Any Mortgage Debt	−0.0132	−0.0217***	−0.0254*	−0.0346***
	(0.0056)	(0.0064)	(0.0093)	(0.0099)
Credit Card Portfolio Statement	−0.0014	0.0016	0.0039	−0.0074
Balances (000s)	(0.0018)	(0.0023)	(0.0039)	(0.0038)
Credit Card Portfolio Statement	0.0000	−0.0045	−0.0119**	−0.0019
Balances Net of Payments (000s)	(0.0020)	(0.0026)	(0.0042)	(0.0042)
Number Credit Cards Portfolio	−0.0020	−0.0040	−0.0050	−0.0053
	(0.0020)	(0.0021)	(0.0033)	(0.0035)
Number Credit Cards Portfolio	−0.0057	−0.0054	−0.0069	−0.0091
With Debt	(0.0034)	(0.0041)	(0.0057)	(0.0063)
Non-Mortgage Debt Value (000s)	−0.0004	−0.0005	−0.0008	−0.0004
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
R^2	0.0119	0.0299	0.0329	0.0593

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows coefficients from OLS regression on binary outcomes. Outcome for columns 1-2 is making both a manual and automatic payment in cycle 7. Outcome for columns 3-4 is making both a manual and automatic payment in any cycle 1-7. Predictors are calculated at card opening or from credit file data in the month preceding card opening. One observation per card using data only for cards enrolled in autopay fix or min at cycle 7. Columns (1) and (3) for control group, columns (2) and (4) for treatment group subsamples. These are run separately for control and treatment groups given different autopay enrollment.*

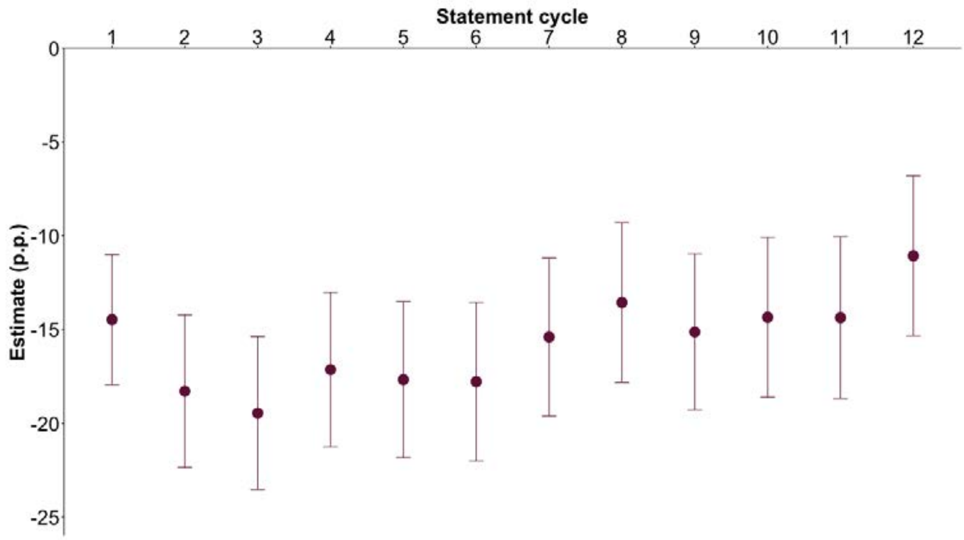
Table B13: Heterogeneous treatment effects on credit card debt (statement balance net of payments % statement balance) by quartiles of pre-trial (A) credit score, (B) income and (C) unsecured debt-to-income (DTI) ratio after seven statement cycles

	Q1: Most Vulnerable	Q2	Q3	Q4: Least Vulnerable
A. Credit Score				
Estimate, p.p.	0.0087	-0.0153*	-0.0107	-0.0016
(s.e.)	(0.0066)	(0.0071)	(0.0070)	(0.0068)
95% C.I.	[-0.0043, 0.0217]	[-0.0291, -0.0014]	[-0.0244, 0.0031]	[-0.0150, 0.0117]
P value	0.1900	0.0306	0.1278	0.8097
Control mean	0.7592	0.7226	0.6686	0.6220
B. Income				
Estimate, p.p.	0.0046	-0.0126	-0.0042	-0.0060
(s.e.)	(0.0072)	(0.0069)	(0.0067)	(0.0067)
95% C.I.	[-0.0095, 0.0188]	[-0.0262, 0.0009]	[-0.0174, 0.0089]	[-0.0192, 0.0073]
P value	0.5202	0.0681	0.5286	0.3778
Control mean	0.6793	0.7144	0.7107	0.6694
C. Unsecured Debt-to-Income (DTI)				
Estimate, p.p.	0.0022	0.0102	-0.0176*	-0.0152
(s.e.)	(0.0062)	(0.0062)	(0.0069)	(0.0081)
95% C.I.	[-0.0100, 0.0143]	[-0.0019, 0.0222]	[-0.0310, -0.0041]	[-0.0311, 0.0006]
P value	0.7275	0.0993	0.0106	0.0598
Control mean	0.8142	0.8044	0.7514	0.4027

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 2 that includes month and statement cycle fixed effects along with pre-trial controls. Each estimate is from a separate regression for subsamples by quartiles of each heterogeneous variable: credit score, estimated monthly income and unsecured debt-to-income (DTI) ratio. Heterogeneous variables are calculated from credit file data in month preceding credit card opening. Q1 (Q4) denotes the most (least) vulnerable quartiles with the lowest (highest) credit score, lowest (highest) income or highest (lowest) unsecured DTI ratio. Standard errors are clustered at consumer-level with N = 40,708 credit cards in total.*

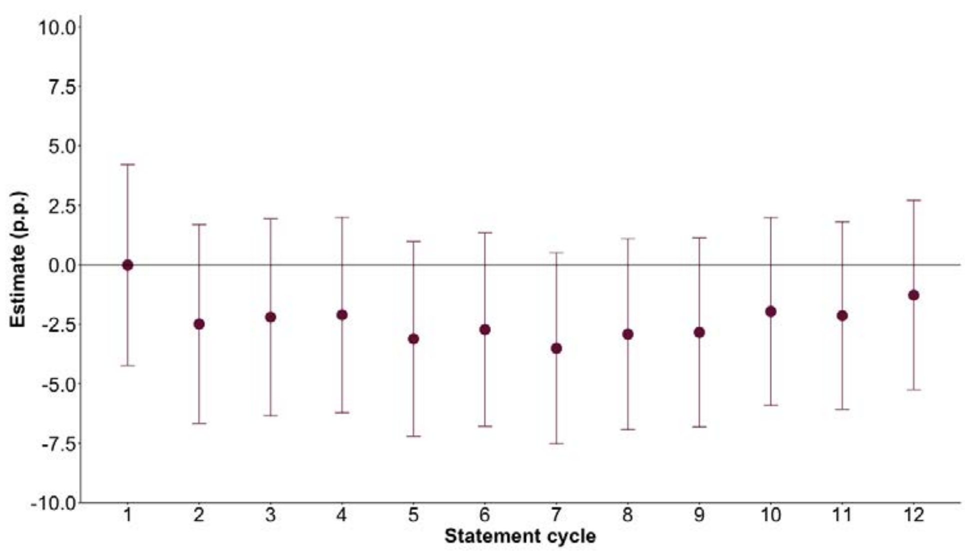
C. Field Experiment: Second Lender

Figure C1: Second Lender - Average treatment effects on making only a minimum payment across 1-12 statement cycles



Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure C2: Second Lender - Average treatment effects on credit card debt across 1-12 statement cycles



Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Credit card debt is measured by primary outcome measure: statement balance net of payments (% statement balance).

Table C1: Minimum Detectable Effect (MDE) sizes for secondary outcomes at cycle 7 across significance levels 0.005, 0.01 & 0.05 (all assuming 80% power)

	Significance Thresholds		
Outcome	0.005	0.01	0.05
Any autopay	0.0154	0.0145	0.0119
Autopay full	0.0123	0.0115	0.0095
Autopay fix	0.0176	0.0164	0.0135
Autopay min	0.0156	0.0146	0.0120
Statement balance net of payments (£)	86.2633	80.7966	66.2351
Credit card portfolio balances net of payments (£)	176.3149	165.1413	135.3790
Cumulative total payments (£)	63.2412	59.2334	48.5582
Cumulative automatic payments (£)	40.6805	38.1025	31.2355
Cumulative manual payments (£)	52.0277	48.7305	39.9481

Table C2: Second Lender: Balance comparison

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Age (years)	37.0547	36.4839	-0.5708	[-1.7761, 0.6345]
Female (% cards)	0.4774	0.5264	0.0490	[-0.0016, 0.0995]
Any estimated income	0.9248	0.9395	0.0148	[-0.0107, 0.0402]
Estimated income (£)	2073.0199	1890.8578	-182.1621*	[-349.5416, -14.7825]
Credit limit (£)	608.9603	587.3874	-21.5729	[-82.0721, 38.9263]
Any credit score	0.9863	0.9897	0.0034	[-0.0076, 0.0144]
Credit score (0-100)	0.5369	0.5406	0.0036	[-0.0057, 0.0129]
Purchases rate (%)	22.9667	23.4588	0.4920	[-0.6872, 1.6713]
Any balance transfer offered	0.1724	0.1699	-0.0025	[-0.0406, 0.0356]
Number of credit cards	2.0356	1.9974	-0.0381	[-0.1850, 0.1087]
Number of credit cards with debt	0.6389	0.6319	-0.0069	[-0.1036, 0.0897]
Credit card portfolio statement balances (£)	934.2079	872.6435	-61.5644	[-269.9267, 146.7978]
Credit card portfolio balances net of payments (£)	855.7415	803.0631	-52.6784	[-249.6079, 144.2511]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 740 and N (treatment) = 791 cards.*

Table C3: Second Lender: Unconditional mean comparison of treatment effects for Autopay enrollment after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Any autopay	0.7606	0.7117	-0.0489*	[-0.0934, -0.0044]
Autopay full	0.1081	0.1416	0.0335*	[0.0002, 0.0668]
Autopay fix	0.1860	0.4955	0.3094***	[0.2643, 0.3546]
Autopay min	0.4665	0.0746	-0.3918***	[-0.4325, -0.3512]
Autopay <£5 fix	0.0014	0.0489	0.0475***	[0.0321, 0.0630]
Autopay fix exceeding minimum payment amount	0.1614	0.3694	0.2079***	[0.1647, 0.2512]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 740 and N (treatment) = 791 cards.*

Table C4: Second Lender: Unconditional mean comparison of treatment effects for primary outcomes after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Any minimum payment	0.3160	0.1622	-0.1538***	[-0.1964, -0.1113]
Any full payment	0.2503	0.2690	0.0186	[-0.0257, 0.0630]
Any missed payment	0.1176	0.1287	0.0111	[-0.0222, 0.0443]
Statement balance net of payments (% statement balance)	0.6753	0.6440	-0.0313	[-0.0732, 0.0105]
Costs (% statement balance)	0.0391	0.0294	-0.0096*	[-0.0180, -0.0013]
Transactions (% statement balance)	0.2245	0.2330	0.0084	[-0.0287, 0.0456]
Share of credit card portfolio only paying minimum	0.2016	0.1245	-0.0771***	[-0.1051, -0.0492]
Share of credit card portfolio making full payment	0.3455	0.3556	0.0101	[-0.0287, 0.0489]
Share of credit card portfolio missing payment	0.0904	0.1021	0.0117	[-0.0132, 0.0366]
Credit card portfolio balances net of payments (% statement balances)	0.7281	0.6997	-0.0284	[-0.0667, 0.0099]

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N (control) = 740 and N (treatment) = 791 cards.*

Table C5: Second Lender: Average treatment effects for Autopay enrollment outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any autopay	-0.0512∗ (0.0214)	[-0.0932, -0.0092]	0.0169	0.7606
Autopay full	0.0308 (0.0163)	[-0.0012, 0.0628]	0.0592	0.1081
Autopay fix	0.3036∗∗∗ (0.0229)	[0.2588, 0.3484]	0.0000	0.1860
Autopay min	-0.3856∗∗∗ (0.0209)	[-0.4266, -0.3447]	0.0000	0.4665

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects from after seven statement cycles. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 1 that includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 1,531 credit cards with 19,578 observations.*

Table C6: Second Lender: Average treatment effects for primary outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any minimum payment	-0.1541*** (0.0215)	[-0.1962, -0.1119]	0.0000	0.3160
Any full payment	0.0223 (0.0219)	[-0.0207, 0.0653]	0.3092	0.2503
Any missed payment	0.0089 (0.0170)	[-0.0244, 0.0421]	0.6011	0.1176
Statement balance net of payments (% statement balance)	-0.0351 (0.0205)	[-0.0753, 0.0051]	0.0874	0.6753
Costs (% statement balance)	-0.0089* (0.0040)	[-0.0168, -0.0010]	0.0276	0.0391
Transactions (% statement balance)	0.0122 (0.0185)	[-0.0241, 0.0485]	0.5113	0.2245
Share of credit card portfolio only paying minimum	-0.0814*** (0.0136)	[-0.1080, -0.0549]	0.0000	0.2016
Share of credit card portfolio making full payment	0.0089 (0.0187)	[-0.0278, 0.0456]	0.6342	0.3455
Share of credit card portfolio missing payment	0.0120 (0.0124)	[-0.0123, 0.0363]	0.3315	0.0904
Credit card portfolio balances net of payments (% statement balances)	-0.0274 (0.0180)	[-0.0627, 0.0078]	0.1276	0.7281

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows average treatment effects from after seven statement cycles. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 1 that includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 1,531 credit cards with 19,578 observations.*

D. Liquid Cash Balances

Bank Account Data Sample Restrictions

We keep bank account data on cardholders who appear to be actively using this bank as their primary bank account for a sustained period of time meeting the following criteria: where we observe a solely-held checking account for six months to June 2017, first observed the account at least 180 days before card opening, and where the 3 month moving average of account credits average at least £250 and account debits at least £100 per month during this time. This approach is similar to that used in other research such as that using the JP Morgan Chase Institute data. For these cardholders we include their liquid cash savings from any other checking accounts held as well as non-checking cash savings accounts with instant access.

The choice of threshold used produces similar sample sizes: requiring average account credits and debits are both £500 results in 3,552 cardholders compared to a threshold of £100 that results in 3,831 cardholders. These cardholders are more likely to be younger, with higher incomes and credit scores, fewer credit cards and lower credit card debts as shown in Internet Appendix Table D1.

Measuring Liquid Cash Balances

Having documented the proximate and distal effects of the policy (along with the lack of clear heterogeneous effects) and investigated the mechanisms explaining our null result, we wanted to understand *why* consumers were not paying more on their credit card. The most natural potential explanation is that many households have limited liquid cash balances available, which prevents or disincentivizes them from repaying credit card debts.

We explore this by constructing new measures of liquidity from our linked bank account data. Unfortunately, we only observe these linked data for a selected subset of cardholders who also bank with their credit card provider. Based on observed socio-economic characteristics (e.g., income, credit score), we would expect this sample to be less liquidity constrained than those for whom we do not observe linked data (Internet Appendix Table D1).

In addition to being a selected subsample, we do not have sufficient power to estimate treatment effects for this group. If we had sufficient power we would evaluate the nudge's heterogeneous effects by liquidity. We present descriptive analysis that we consider informative for updating a Bayesian reader's priors. Despite such limitations, these data represent an advance on research on credit card payments decisions where liquid savings data is unobserved (e.g., Keys and Wang, 2019; Medina and Negrin, 2022).

We construct three measures of liquid cash balances. Our first measure is a static one. It measures 'liquid cash' as the end-of-day balance in bank accounts by aggregating all liquid cash held across checking and non-checking, instantly-accessible cash savings accounts. In the UK, it is common for checking accounts to have an overdraft line of credit facility, so liquid cash measures can have negative balances. Our first measure simply takes liquid cash balances at the day before card opening (-1) but we also show it at earlier points-in-time before card opening (-31, -61, -91, -121, -151).

Our other two measures are innovative as they consider the dynamics of liquidity. These measures go beyond measures used in prior literature using transaction data. Prior literature does not examine heterogeneity by the *minimum* balance reached but instead focus on different moments: the mean or median balance:

- Agarwal and Qian (2014, *American Economic Review*) segments by the mean value of checking account balance.

- Gelman, Kariv, Shapiro, Silverman, and Tadelis (2014, *Science*) segments by the mean value of checking and savings accounts balances (normalized by the daily average spending of each consumer).

- Olafsson and Pagel (2018, *The Review of Financial Studies*) segments by the mean and median values of cash and available liquidity (normalized by the daily average spending of each consumer to provide measures of 'consumption days').

- Baker (2018, *Journal of Political Economy*) segments by the mean of liquid assets / income, illiquid assets / income, total assets / income, debt / (debt + assets), and debt / income.

Our second measure examines a consumer's minimum liquid balances over the last 90 days before card opening (along with other time horizons). This accounts for how consumers' finances vary over time; one point-in-time does not reflect how liquidity varies at different points-in-time for different consumers depending on the timing of their incomes and expenditures.

Our third measure also accounts for dynamics. It records the number of days a consumer's liquid balance drops below £100 in the thirty days before card opening (along with earlier points-in-time pre-card opening). This measure indicates the volatility of a consumer's finances. We use £100 as a threshold as not all transactions can be paid with credit cards and therefore consumers may find it necessary to hold a positive liquid balance.

While we call these liquidity constraints, we caveat that this is an observable financial outcome that may arise for many reasons such as financial illiteracy (e.g., Lusardi and Tufano, 2015) and behavioral factors such as naïve present bias leading to impulsive overconsumption (e.g., Heidhues and Kőszegi, 2015).

Summarizing Liquid Cash Balances

We show the distribution of these three measures of liquidity in the left hand side panels of Figure 10 (Summarized in Internet Appendix Table D2). The blue lines show the robustness of these measures across alternative time horizons. Our first static measure (Panel A) shows a clear kink with liquid cash balance above zero being much more likely than those below. This kink may reflect there being a discontinuous increase in costs from becoming overdrawn on checking accounts and precautionary rationale to keep a small amount of buffer stock savings. By this measure approximately 10% experience have limited liquidity of having a zero or negative liquid cash balances available. We also observe this distribution has very fat tails (and so the mean is not well-estimated) but is stable over time with a median balance near £400.

Our second dynamic measure (Panel B) reveals clear sorting of consumers into two types (Distribution summarized in Internet Appendix Table D2). One group of consumers has a zero or negative minimum liquid cash balance. There is a lot of bunching with another group of consumers just managing to keep positive, but small, liquid cash balances. A longer time window for calculating minimum liquid balances results in a slight steepening of the CDF around zero. Using a 90 day window the median minimum balance is effectively zero (£4.76) and the 75$^{\text{th}}$ percentile £142.39. This second measure reveals effectively zero cash balances for approximately 50% of consumers: far higher than the 10% a point-in-time liquid balance measure (Panel A) would indicate.

Our final dynamic measure (Panel C) also shows sorting of consumers into three groups. One group of approximately 40% do not appear liquidity constrained: with £100 (or above) balances every day in the last month. Another group of less than 10% are always constrained: persistently having below £100 balances every day in a month. There is a third group of approximately 50% who fall in between the two: being constrained some days in a month.

Relationship Between Cash Balances and Credit Card Repayments

We show in the right hand side panels of Figure 10, the relationship between these variables and credit card payment decisions using our primary measure of credit card debt (statement balance net of payments as a fraction of statement balance). Panels D and E use binscatters by quantiles of the distribution, whereas Panel F uses loess (non-parametric smoothing) given the integer scale and high mass at both tails.

Panel D shows consumers who had small, positive liquid balances (before card opening) repaid more of their credit card debt, on average, seven cycles later than those with zero or small negative liquid balances. However, this relationship is quite noisy given how fat the distribution of liquid balances are.

Panel E shows a clearer relationship when we use our measure of minimum liquid cash balances over 90 days. Consumers with positive minimum liquid balances (before card opening) discontinuously repaid approximately 20 pp more, on average, of their credit card debt seven cycles later than those with zero or small negative liquid balances. Given the bimodal distribution to repayments we also examine the other moments: payments at the minimum, full, and less than minimum. The discontinuity in average repayments is driven by discontinuous increases in the likelihood of paying in full and decreases in the likelihood of missing a payment (Internet Appendix Figure D2). The relationship with Autopay choices is less clear except for a discontinuous increase in Autopay Full enrollment (Internet Appendix Figure D1). Paying only the minimum becomes less likely among less liquidity constrained consumers, however, there is a less clear discontinuity around zero. Panel F also shows a clear relationship: consumers who have more days with low liquid cash balances (pre-card opening) repay less credit card debt seven cycles later.

Table D1: Coefficients from OLS regression predicting correlates of observing linked liquid savings data

	(1)
(Intercept)	0.0685**
	(0.0237)
Female	0.0035
	(0.0028)
Age	−0.0007***
	(0.0001)
Any Income Estimate	−0.0155
	(0.0088)
Income Estimate	0.0034***
(000s)	(0.0007)
Log (Credit Limit)	0.0025
	(0.0026)
Subprime	−0.0470***
	(0.0070)
Purchases Rate	0.0031***
	(0.0003)
Any Balance Transfer	−0.0598***
	(0.0026)
Credit Score	0.0705***
	(0.0152)
Any Mortgage Debt	−0.0265***
	(0.0029)
Credit Card Portfolio Statement	−0.0025
Balances (000s)	(0.0011)
Credit Card Portfolio Statement	0.0044***
Balances Net of Payments (000s)	(0.0012)
Number Credit Cards Portfolio	−0.0152***
	(0.0010)
Number Credit Cards Portfolio	−0.0112***
With Debt	(0.0017)
Non-Mortgage Debt Value (000s)	−0.0011***
	(0.0002)
R^2	0.0453

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Table shows coefficients from OLS regression where binary outcome is whether observe linked liquid savings data. Predictors are calculated at card opening or from credit file data in the month preceding card opening. One observation per card.*

Table D2: Summary statistics on liquid cash balances by date preceeding credit card opening

Date	Mean	S.D.	P10	P25	P50	P75	P90
-1	2109.85	12324.35	-84.58	48.07	368.65	1,310.91	4,054.58
-31	2142.00	14616.85	-95.17	56.37	364.06	1,297.43	3,757.13
-61	2048.65	9222.26	-61.84	66.93	432.80	1,394.05	4,094.95
-91	2342.60	22005.76	-38.10	66.26	433.57	1,397.41	3,986.56
-121	2164.82	14861.37	-59.16	55.72	396.25	1,401.18	3,949.21
-151	1800.46	7761.59	-75.71	57.62	386.68	1,342.17	3,508.93

Notes: N = 3,753 consumers. Liquid cash balance is sum of end of day current/checking account and cash saving accounts balances.

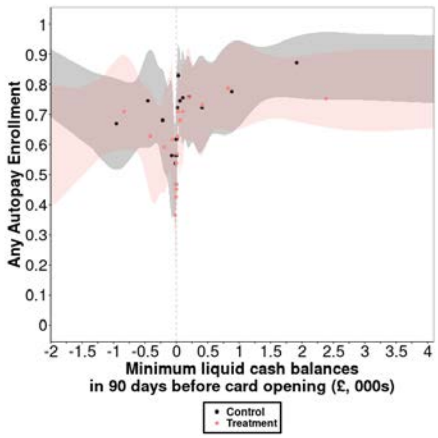
Table D3: Summary statistics on minimum liquid cash balances over windows preceeding credit card opening

Window	Mean	S.D.	P10	P25	P50	P75	P90
-1 to -31	962.86	5771.79	-487.79	-6.41	24.67	336.62	1,960.99
-1 to -61	780.91	5421.16	-552.73	-14.93	9.50	207.14	1,537.36
-1 to -91	671.38	5107.10	-597.80	-23.85	4.76	142.39	1,296.70
-1 to -121	583.06	4906.39	-629.34	-39.28	2.39	107.63	1,080.03
-1 to -151	485.62	4414.11	-687.15	-51.36	1.08	81.96	909.11

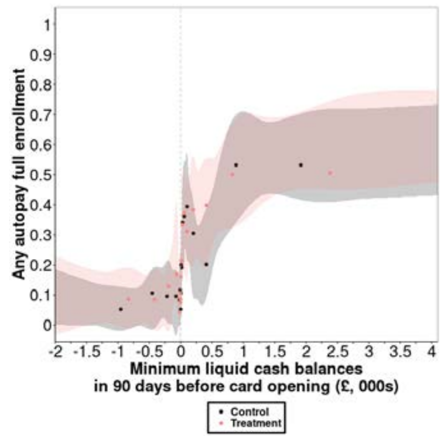
Notes: N = 3,753 consumers. Minimum liquid cash balance is minimum value of liquid cash (sum of end of day current/checking account and cash saving accounts balances) reached by a consumer over 30 to 150 day windows.

Figure D1: Non-parametric relationship between minimum liquid cash balance during 90 days before card opening with credit card Autopay enrollment at statement cycle 7, by treatment group

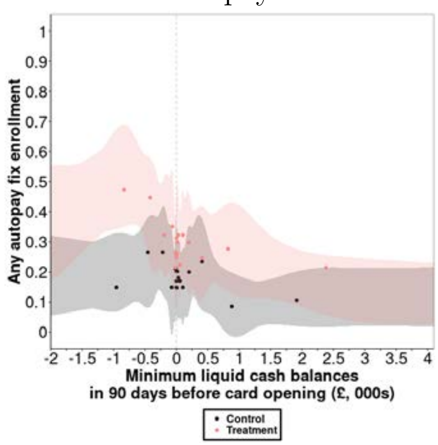
A. Any Autopay



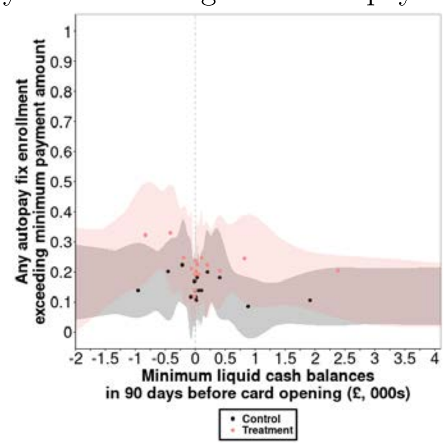
B. Autopay Full



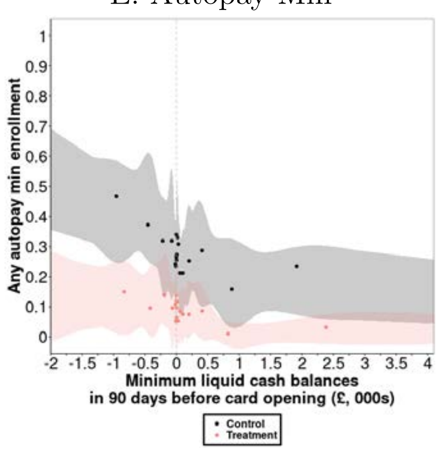
C. Autopay Fix



D. Autopay Fix exceeding minimum payment amount



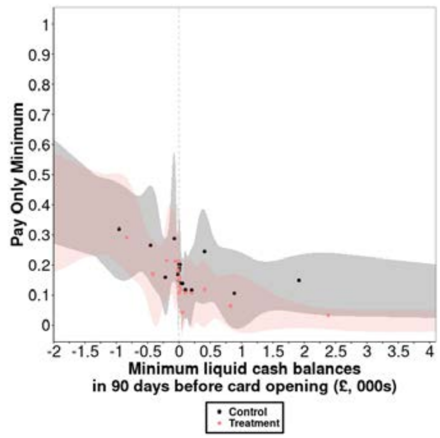
E. Autopay Min



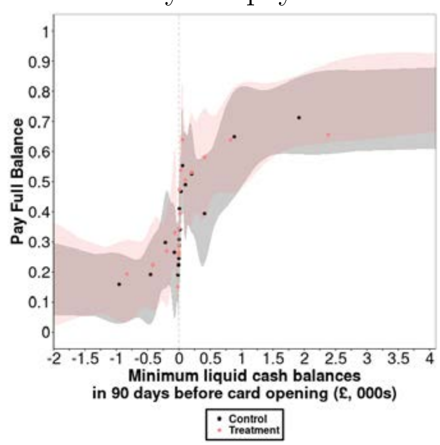
Notes: N = 3,753 consumers. Liquid cash balances are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.

Figure D2: Non-parametric relationship between minimum liquid cash balance during 90 days before card opening with credit card repayments at statement cycle 7, by treatment group

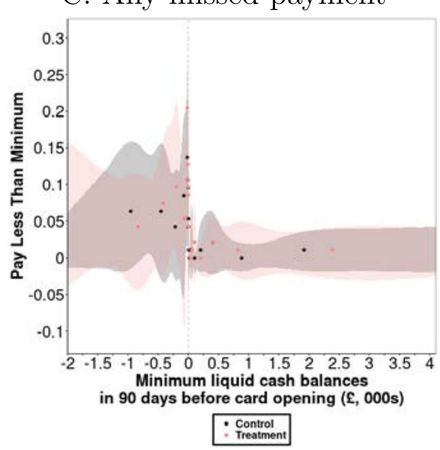
A. Any minimum payment



B. Any full payment



C. Any missed payment



Notes: N = 3,753 consumers. Liquid cash balances are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.