The Semblance of Success in Nudging Consumers to Pay Down Credit Card Debt*

Benedict Guttman-Kenney† Paul Adams Stefan Hunt
David Laibson Neil Stewart Jesse Leary

Abstract

We test an active choice nudge on credit cardholders through a field experiment and a survey experiment. This nudge aims to prevent credit card payments being anchored to the minimum. Our field experiment shows the nudge reduces enrollment in Autopaying the minimum by 74% and increases enrollment in an active choice Autopay option by 73%. However, the nudge does not reduce credit card debt because of offsetting responses by cardholders. Actively chosen Autopay amounts are only slightly higher than the minimum. The nudge reduces both Autopay enrollment and non-Autopay payments. Our results are explained by cardholders experiencing frequently-binding liquidity constraints.

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†Guttman-Kenney: Corresponding author (benedict@chicagobooth.edu), University of Chicago, Booth School of Business. Adams: Innovations for Poverty Action & London School of Economics (pauldun-canadams@gmail.com). Hunt: Keystone Strategy (stefanhunt@gmail.com). Laibson: Harvard University & NBER (dlaibson@harvard.edu). Stewart: Warwick Business School, University of Warwick (neil.stewart@wbs.ac.uk). Leary: Amazon (learyjesse@gmail.com).

I Introduction

Credit card payments are often at or near the minimum due: 25% are in the UK (FCA, 2016a) and 29% are 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 debt if they only pay the minimum (e.g., Adams et al., 2022) and informational disclosures or nudges to address this 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 significantly reduce credit card debt. Third, credit cardholders that expect they will be highly liquidity constrained are likely to prefer to make low payments on their credit card debt and, if so, may not voluntarily reduce their credit card debt.

We research this topic by conducting 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 would reduce credit card debt. In line with anchoring, our nudge is effective at changing choices away from the minimum. However, in line with liquidity constraints, the nudge is ultimately ineffective as it does not change credit card debt. We observe credit cardholders make offsetting responses to the nudge. We find credit cardholders experience frequently-binding liquidity constraints.

One institutional mechanism that facilitates low credit card payments is the account feature called 'Autopay' in the US or 'Direct Debit' in the UK. Autopay is a common financial technology used for payments on 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 the (ex-ante) 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 20-38% of US cards (CFPB, 2021), with growing use over time.[1] Cardholders can make supplemental, non-Autopay ('manual') payments (e.g., online, phone) to top up Autopay payments.

Persistent minimum payments and high credit card interest costs are concentrated among cardholders who have enrolled in Autopay Min. Using a regulatory definition of 'persistent credit card debt' – making nine or more minimum payments in a year on interest-bearing cards – 75% of cardholders in persistent credit card debt 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 subject to anchoring? We report the results of a survey experiment (N = 7,938) testing the anchoring theory with a treatment removing the visibility of the minimum payment on a hypothetical credit card online payment screen. This survey experiment conceptually replicates earlier research on anchoring (e.g., Stewart, 2009). We find that de-anchoring minimum payments increases hypothetical credit card payments by 12 percentage points. We find credit cardholders enrolled in Autopay Min appear to be subject to anchoring to the minimum payment similarly to cardholders not enrolled in Autopay. Our survey experiment provides only suggestive evidence, because our design studies *hypothetical* credit card payment decisions.

We attempt to exploit anchoring effects with a pre-registered field experiment (N = 40,708) that tests a nudge designed to increase credit card repayments on real credit card

[1] As noted in CFPB (2021), US estimates are more uncertain because they are based on consumer self-reports with different surveys yielding substantially different estimates.

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 changes in behavior). This field experiment was 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 accumulation of substantial amounts of 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 datasets.

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 proximate effect sizes this second lender withdrew before fieldwork was complete. We call this reduction in Autopay Min enrollment a *proximate* effect, because it is measured on an enrollment outcome, but the key target variable of economic interest is the future quantity of credit card debt that builds up.

We follow cardholders over at least seven months and find that our de-anchoring nudge does *not* change future 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 but consumers are no more likely to pay the full balance. These effects are persistent over time. We refer to these policy variables as distal outcomes, because they are not observed at the time that the enrollment web page is visited and the initial Autopay enrollment decision is made, and because these outcomes are causally downstream from the enrollment decision. Such null results on distal outcomes are critical policy inputs (Abadie, 2020) especially in contrast to the large measured impact on proximate enrollment outcomes. While our de-anchoring nudge harnesses psychological insights to change enrollment choices, it does not change distal economic outcomes of interest to policymakers.

Our results demonstrate the importance of measuring the distal effects of nudges. If a policymaker were to only observe the proximate effect 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%. In contrast, the distal effects reveal the nudge was ineffective at reducing debt. 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 proximate effects of our de-anchoring nudge in our field experiment to be undone so that the distal effects are not statistically

[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).

significant. We find three offsetting consumer responses to the de-anchoring nudge. 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.

Finally, our results provide an example of how psychological factors interact with economic constraints. Liquidity constraints best explain why consumers do not reduce their credit card debt. In our survey experiment, we find anchoring effects are attenuated by financial distress. For a selected subsample of our field experiment, we observe daily liquid cash balances from bank account data linked to our credit card data. We use these linked data to construct a new measure of dynamic liquidity constraints: the *minimum* liquid cash balances in the last ninety days. This dynamic liquidity constraint measure reveals constraints bind for approximately 50% of consumers in our linked data, compared with just 10% using a traditional static measure of liquidity constraints. Our new measure correlates with subsequent credit card repayment decisions. Consumers with small, positive minimum liquid balances (before card opening) were discontinuously more likely to repay approximately 20 percentage points more of their credit card debt seven cycles later than those with small, negative minimum liquid balances.

The paper proceeds as follows. Section II explains our survey experiment (II.A) and its results (II.B). This first de-anchoring experiment motivates the field experiment that follows. Section III explains our field experiment design (III.A) and implementation (III.B), and provides theoretical motivations (III.C). Section IV describes the data (IV.A), empirical methodology (IV.B), and summary statistics (IV.C) from the field experiment. Section V presents the results of the field experiment: proximate effects on Autopay enrollment (V.A) and distal effects on key economic outcomes (V.B). Section VI contains analysis exploring the mechanisms behind the results: three consumer responses that offset the proximate effects (VI.A), heterogeneous effects (VI.B), and the role of liquidity constraints (VI.C). Section VII concludes.

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), and Medina and Negrin (2022).

II.A Survey Experiment on Anchoring: Design

We conduct a survey experiment that manipulated anchoring effects with UK credit cardholders.[3] Because we survey actual UK cardholders and have administrative linkages to their actual credit card accounts, we know their actual enrollment status with respect to Autopay. Accordingly, we study the effects of the anchoring manipulation for different actual Autopay subsamples.[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 (after removing responses from respondents who were inactive on their credit card or in the pilot). 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

[3] Survey participation was incentivized by inviting participants to 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 observed in these respondents' administrative data.

and Zhao, 2020; Sakaguchi et al., 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$^{\text{th}}$ (£532.60) and 75$^{\text{th}}$ percentiles (£3,217.36) of the overall distribution of actual statement balances. We also randomized balance amounts as there is a wide heterogeneity in credit card balances and it was possible anchoring effects would vary with the amount due.

Respondents were also randomized across control and treatment groups. The control group's screen design (Internet Appendix Figure A1) reflects the options cardholders would observe in their own 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 bucketed 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.[5]

Prior research had not examined anchoring effects by Autopay enrollment. 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

[5] 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.

(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). We interpret this as suggesting 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 expected the results to extrapolate from the lab to the field and to undermine their profitability.

III Field Experiment

Our field experiment varies how Autopay enrollment options are presented to UK consumers who have just opened a new credit card account (whereas in the survey experiment above, we varied how manual payment options were presented on monthly bills). In this section we explain our field experiment: the nudge's design (III.A), our experiment's implementation (III.B), and additional theoretical motivations (III.C).

III.A Nudge Design

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 $max\{£5,\ 1\%\ \text{statement balance}\ +\ \text{interest}\ +\ \text{fees}\}$.[6] If a cardholder is only paying the minimum, then (i) their repayment is effectively only servicing debt interest payments (with interest rates near 20% typical), and (ii) debt reduction only happens at all if new spending is

[6] 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 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.

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).[7] 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: $max\{\text{Autopay Fix £},\ \text{Minimum Payment Due}\}$. 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 were made and only the minimum was 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 was shown to the control group in Panel A). Removing the Autopay Min option increases the salience of the alternative, Au-

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

topay 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 was 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 unintended effect of increasing consumer spending due to the increased credit limit availability arising from debt paydown (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.[8] 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.

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

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 these field trials. Two lenders were both willing and technically able to participate within the timelines necessary to inform FCA policymaking. Before putting the nudge into the field it went through the FCA's Institutional Review Board's governance reviews and also reviews 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).[9] Once allocated to control or treatment the consumer would view 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 was conducted on two UK lenders. The main lender was 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 was 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.

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

The second lender stopped the experiment after one week of fieldwork due to the lender's concern over the large size of the proximate effects on Autopay choices. 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 Section C. The rest of this paper is based on the field experiment conducted with the main lender unless explicitly stated.

III.C Theoretical Motivations

Autopay Min may be appealing because of a combination of mutually compatible economic and psychological factors. In this section we discuss how these theoretical motivations informed the design of our field experiment.

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 by the Autopay Full option. This choice architecture 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 has an unintended effect anchoring to the scenarios presented, which reduces payments for some consumers.

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.

There is evidence that cardholders 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 proximate effects on choices and were 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., 2007). 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). 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 to make additional manual payments to reduce debt, however, they may procrastinate and not make these (O'Donoghue and Rabin, 1999). There is also 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 Liquidity Constraints

Liquidity constraints would be a standard economic explanation for consumers enrolling in Autopay Min. Liquidity constraints 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). A high likelihood of a liquidity constraint – if anticipated by the cardholder – may weaken the effectiveness of our intervention. Consumers who anticipate that they are likely to be liquidity constrained in the future 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

Subsection IV.A explains the data we collected, subsection IV.B the empirical methodology for evaluating its effects on consumers, and subsection IV.C shows summary statistics.

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 microdata 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 was made automatically 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 data on bank accounts (checking/current accounts and savings accounts) for the subset of cardholders who hold these accounts 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 were enrolled in our experiment. 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. 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. We observe 3,753 cardholders with these data which is 9.2% of those in our experiment. 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 (Internet Appendix Table B12).

IV.B Empirical Methodology

Before analyzing data, we pre-registered our methodology. Our pre-registration designates primary outcomes, describes regression specifications, and provides thresholds for statistical significance. We document these pre-registered methods in this section.[10]

We structure our overall analysis in three parts: primary, secondary, and tertiary analyses. This structure limits the potential issues 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 primary outcomes are:

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

[10] Also on AEA Registry 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.

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.

These 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. Our remaining four primary outcomes (7-10) are constructed using credit file data to measure the impact across the portfolio of credit cards held by the cardholder. All these primary outcomes are bounded between zero and one: with outcomes 1-3 being binary. Our measures of debt (spending and costs) are 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).

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 B1 and C1).

The pre-registered secondary analysis considers a broader set of outcomes and empirical approaches to understand our results and their robustness. We measure the *proximate* effects of the nudge on Autopay enrollment and evaluate *distal* effects using the pounds (£) amounts of credit card debt and repayments. 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 was unclear what the appropriate functional form would be.

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

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. 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 were 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 were from the month preceding card origination. For outcomes constructed from credit file data up to eleven dummies for lags of outcomes were included as controls (X_i') for months preceding the start of the experiment. 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 required 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) which, on average, is 195 days after card opening. 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 (in our data the second statement is issued mean 43 and median 42 days from card opening) when the cardholder has had at least one month to view the control or treatment screens and use their card and, if used, their card will have 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 $\left(TREATMENT_i \times CYCLE_\tau\right)$ 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 show summary statistics for the control group after seven statement cycles in Table 1. 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.

In line with the motivation for our experiment, the cardholders in our control group are often only paying 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.

The mean credit card statement balance after cycle seven is £2,164 and £1,963 after payments. These cardholders often hold other credit cards in their portfolio as observed in credit file data: their mean credit card portfolio statement balances (summed across cards held) 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.

Allocation to the treatment group is balanced, on average, across measures (Internet Appendix Table B2). However, we do observe some small differences. The likelihood of being in the treatment group slightly varies with credit card credit limit. Investigation revealed that the 'live' randomization code used by the lender was not completely random: 526 more consumers (0.65%) were allocated to control than 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 without a selection bias through conditioning on covariates. Conditioning on observables using our pre-registered controls does not change our overall results or their implications when compared to results from unconditional means.[11]

V Experimental Results

This section presents our experimental results showing the proximate (Section V.A) and distal effects (Section V.B) of our nudge.

V.A Proximate Effects on Autopay Enrollment

The first effect we examine is the proximate 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 proximate 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 the following round numbers (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).[12] This is a statistically significant increase relative to 0.5% in the control group but we interpret it as being economically small.

[11]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.

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

Initial choices of Autopay Fix amounts are persistent over time (Internet Appendix Figure B1). 88.3% of those in the treatment group who were 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 was £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 2 – 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 B3). Estimates cycle-by-cycle (δ_τ in Equation 1) are displayed in Figure 6 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 presented in Section III.C.

V.B Distal 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 3 (unconditional means in Internet Appendix, Table B4).

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 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 4 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. As a robustness check we find consistent results examining the cumulative number of minimum payments (Tables 4 and Internet Appendix, Figure B4 and Table B5).

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).

We look at how this translates to the share of a cardholder's credit card portfolio where payments are made only equal to the minimum (constructed from credit file data). This reveals an average treatment effect a third of the size of that for the card for which the treatment was targeted. This smaller overall effect across the credit card portfolio is due to consumers holding multiple cards – only one of which was 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 for significance.

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 was evaluated ex-post after being 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 was 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 5, 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 5, 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). By doing so we can say that, 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 B6). 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 (Table 4, Internet Appendix, Figure B4, and Table B5). Our null average treatment effects on debt (robust to secondary outcomes in Table 4 and Internet Appendix Table B5) in spite of a seemingly large, proximate change in enrollment and paying only the minimum payment is surprising. How can it be that the treatment is not, on average, reducing debt if one in five more consumers are enrolled in Autopay Fix (and are not increasing spending)?

VI Mechanisms

Having completed the primary analysis, we now conduct tertiary analysis to understand the mechanisms behind our results. This section provides analysis showing offsetting consumer responses to the nudge (Section VI.A), examining heterogeneity in results (Section VI.B), and the role of liquidity constraints (Section VI.C).

VI.A Offsetting Consumer Responses

If the only change were compositional – changing Autopay enrollment but *assuming no other changes* – the proximate effects on Autopay enrollment may have been expected to lead to a distal effect of reducing debt by approximately 4.5%.[13] Indeed, the fact that the second lender withdrew after only observing proximate effects is evidence of our null distal effects being unexpected. We find three offsetting consumer responses on the target card to the nudge make it ineffective: producing a precise zero effect on debt.

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

Many cardholders are responding to the nudge by setting an Autopay Fix that is 'too low': often 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 6), the treatment effect on enrollment with Autopay Fix *exceeding* the minimum amount due is half the size (the pink coefficients in Figure 6): 8.6 percentage points which is a 34% increase on the control group mean (Internet Appendix, Table B3). The regression estimates are in Table 2.

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 B3).

When we examine the distribution of Autopay Fix amounts chosen by the treatment group (Figure 7) we find 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

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

the minimum. At the other extreme, it is only over £500, for 13%. Such amounts can be evaluated relative to the mean cumulative value of repayments across these cycles in the control group: £1,277 (Table 4). 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 offsetting effect is that the nudge causes a 4.3 pp (5.6%) significant decline in enrollment in any type of Autopay (Table 2 and Internet Appendix Figures B3, B1 and Table B3). This lower enrollment explains an unintended slight average increase in the likelihood of missed payments (Table 2). If enrolled in Autopay a consumer would only miss a payment if they have insufficient funds in their checking account whereas it is much easier for consumers not enrolled to 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 B9). There were no statistically significant differences in the types of consumers who were more likely to not have any Autopay enrollment as a result of the treatment.[14]

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

[14]We conduct OLS regressions shown in Internet Appendix Table B8 with one observation per card. We predict 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 were significant predictors of Autopay enrollment, none were when interacted with the treatment and so do not explain this decline in Autopay enrollment.

B9 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 B9). 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 3 and Internet Appendix Table B6). 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 was unaffected.

We interpret this result as indicating that not having an 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 consumers to enter a debt spiral of severe financial distress. While lower enrollment in Autopay was not an intended effect of the nudge, it is not resulting in consumers being more indebted. 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 often be considered a worse economic outcome. For example, if a nudge lowers enrollment in 401(k)s 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 now examine substitution between the two as another potential offsetting effect and find consumers do substitute and so are not as inert or inattentive as they initially appear. Figure 8 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 (the estimates after seven cycles are shown in Table 4). We find the treatment causes consumers to be 1.3 pp more likely to make both an automatic and manual payment in the same cycle (Table 4) – this is in spite of fewer consumers enrolling in Autopay.

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 B10). 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).

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. These numbers found to prominently appear in manual payments appear with far less frequency in total payments: 48%. 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). 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. Survey responses in our earlier working paper (Adams et al. (2018b)) are aligned with this explanation. The most common reasons 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 being able to manually adjusting payments each month.

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. For the same reason we may expect manual payments to be higher in the treatment group, however, this is ambiguous as it depends on whether cardholders are forgetting to make any payments or substituting between automatic and manual payments.

To help disentangle these, we decompose Equation 1 by whether the consumer was 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 will suffer from bias and are not causal.

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

We examine the cumulative value of payments, in total and split by automatic and manual payments, by the seventh cycle across these subgroups. Figure 9, Panel A shows the causal estimates across all cards using Equation 1 and then Panels B and C respectively show non-causal estimates for cards enrolled and not enrolled in Autopay using Equation 3.

Panel B indicates 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, they would have reduced average debt by approximately 2.9%. Panel C has 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 was 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 effect 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 was the parameter of interest, it can still be informative to understand whether there were 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 were chosen as groups 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. We split these groups into quartiles as it was 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 relatively little difference in this outcome measure across quartiles of income but noticeably more across quartiles of credit score and DTI - especially comparing the top and bottom quartiles.

Our heterogeneity analysis does not produce clear effects (See Internet Appendix Figure B8 and Table B11 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 was 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 was 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 Liquidity Constraints

VI.C.1 Measuring Liquidity Constraints

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 whether liquidity constraints prevented them from doing so.

We explore this by constructing new measures of liquidity constraints 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 we do not observe linked data for (Internet Appendix Table B12).

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 constraints. We present descriptive analysis that we consider informative for updating a Bayesian reader's priors. Despite such limitations, these data represent an advancement on research into 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 liquidity constraints. 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. 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 constraints. 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 (e.g., Agarwal and Qian, 2014; Gelman et al., 2014; Olafsson and Pagel, 2018; Baker, 2018).[15] 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 are dynamic and thus one point-in-time does not reflect how constraints can bind 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

[15] Agarwal and Qian (2014) segments by the mean value of checking account balance. Gelman et al. (2014) segments by the mean value of checking and savings accounts balances (normalized by the daily average spending of each consumer). (Olafsson and Pagel, 2018) 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) segments by the mean of liquid assets / income, illiquid assets / income, total assets / income, debt / (debt + assets), and debt / income.

and therefore consumers may find it necessary to hold a positive liquid balance.

While we term 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).

VI.C.2 Summarizing Liquidity Constraints

We show the distribution of these three measures of liquidity constraints in the left hand side panels of Figure 10 (Summarized in Internet Appendix Table B13). 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 a binding liquidity constraint of having a zero or negative liquid cash balance. 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 B13). 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 liquidity constraints commonly bind 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.

VI.C.3 Relationship Between Liquidity Constraints 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 B10). The relationship with Autopay choices is less clear except for a discontinuous increase in Autopay Full enrollment (Internet Appendix Figure B9). 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.

We interpret our results as helping 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 experiencing binding liquidity constraints. 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. It indicates consumers have a need for liquidity with constraints binding over relatively short time periods. This explanation appears most in line with Telyukova (2013)'s structural model. The liquidity constraints we observe *may* also mean other interventions ultimately fail to change real outcomes.

Further examination of our survey experiment of hypothetical credit card repayments provides supportive evidence of this explanation. We now allow for heterogeneity in the de-anchoring treatment effect of manual payments 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 group responses into three groups of financial distress: no distress (the omitted category), some distress, and high distress (see footnote for full question).[16] 52% of respondents report

[16] 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

no distress, 38% some distress, and 11% high distress.

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

Respondents in our survey experiment self-reporting high financial distress are significantly more likely to hypothetically pay lower amounts (Table 5 and Internet Appendix Figure A3). 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. We interpret this as anchoring effects are attenuated by financial distress.

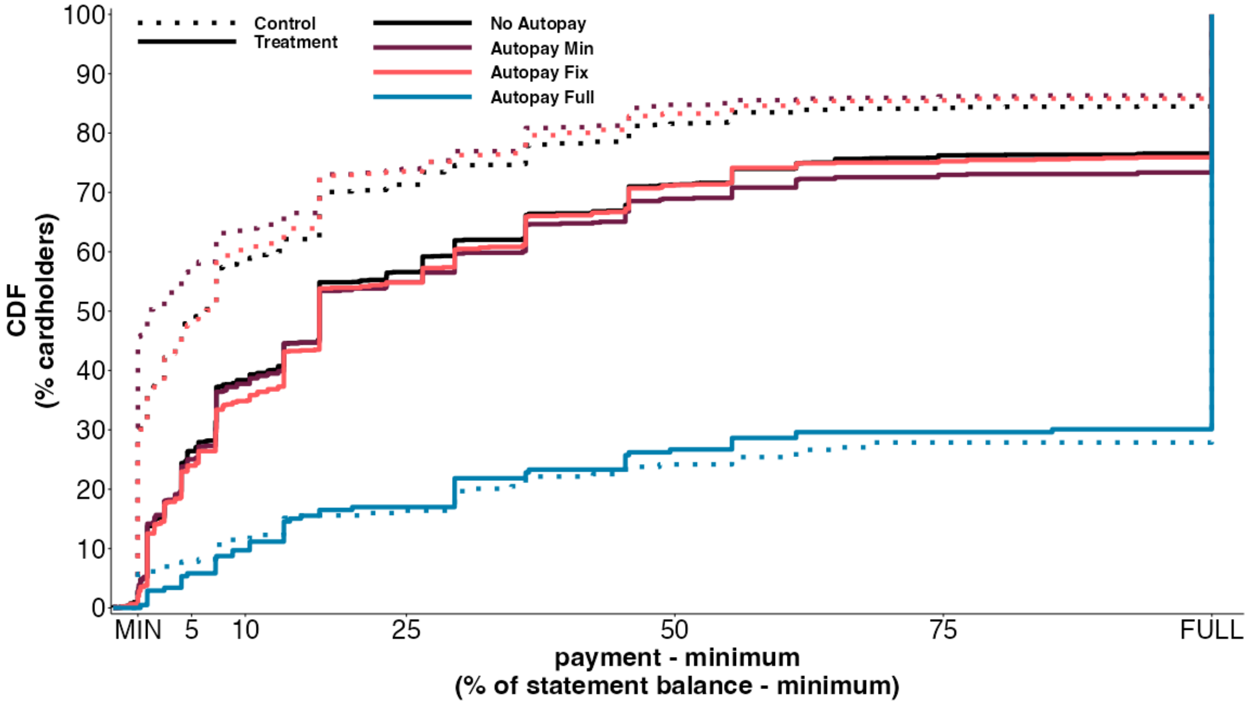
VII Concluding Discussion

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

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'.

VIII Figures & Tables

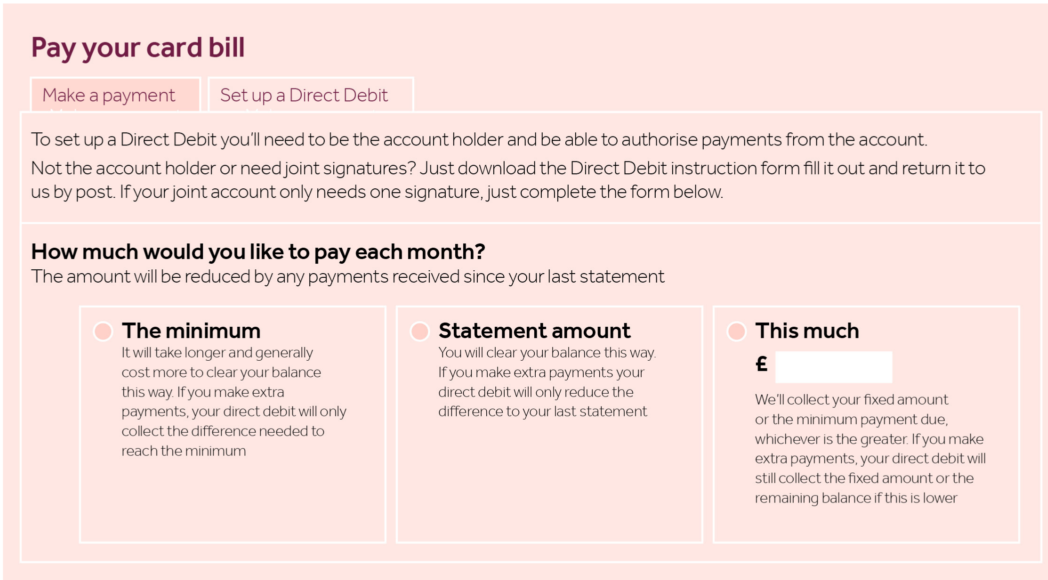
Figure 1: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount, shown by Autopay enrollment



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

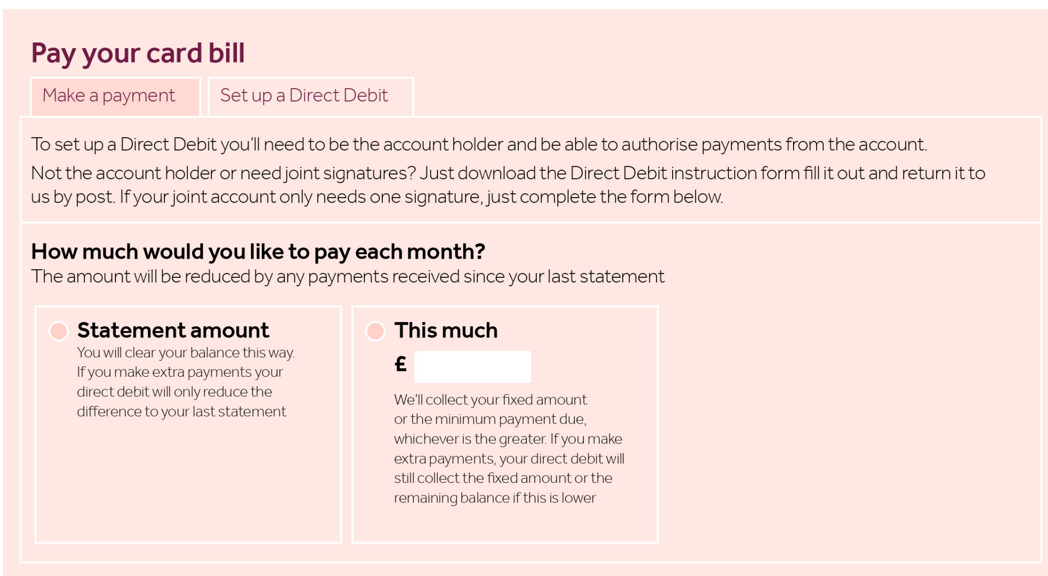
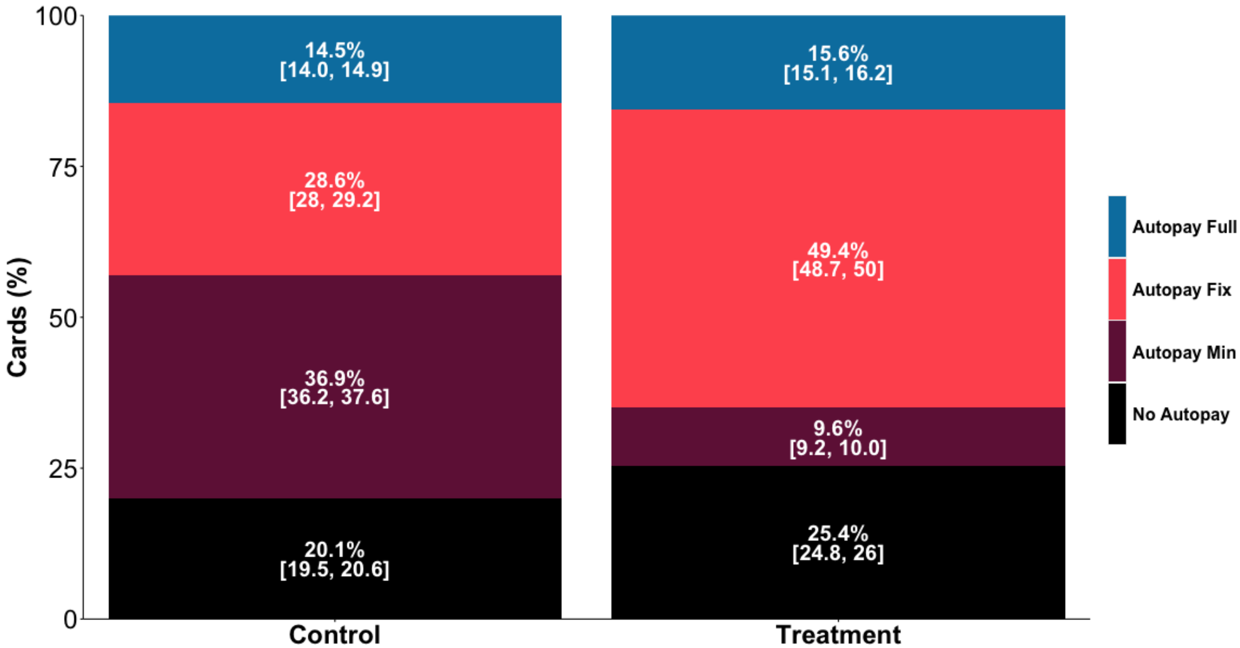
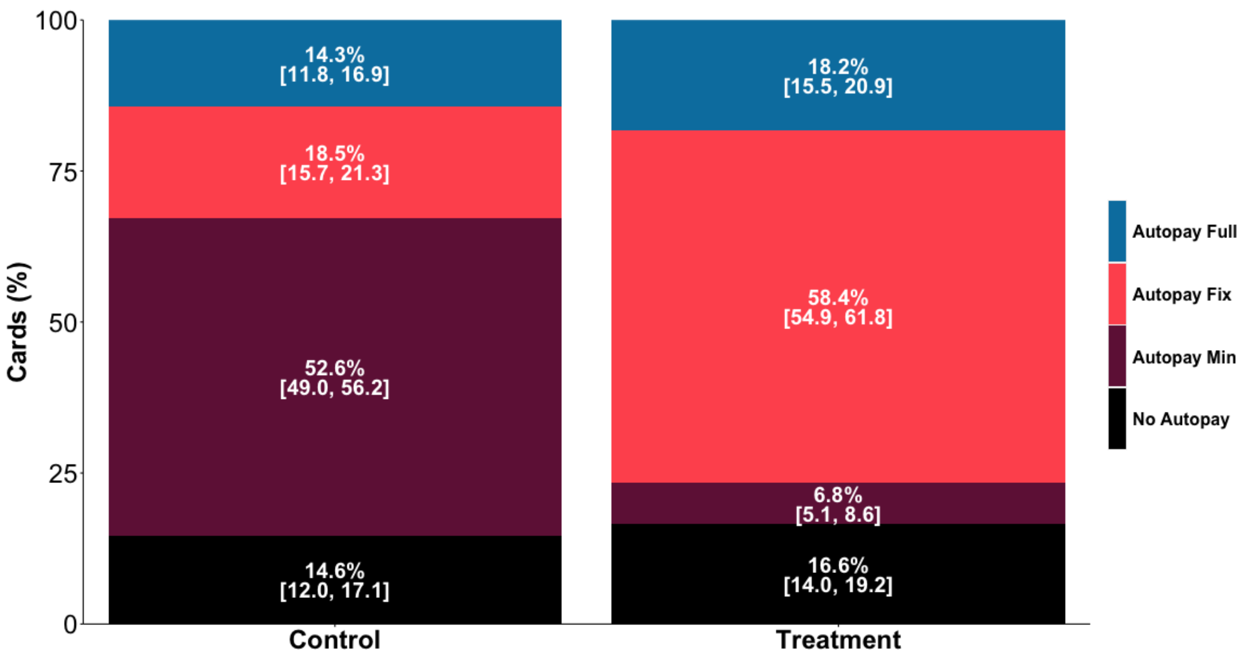


Figure 3: Autopay enrollment for control and treatment groups after two statements, split by lender

B: MAIN LENDER (N = 40,708)

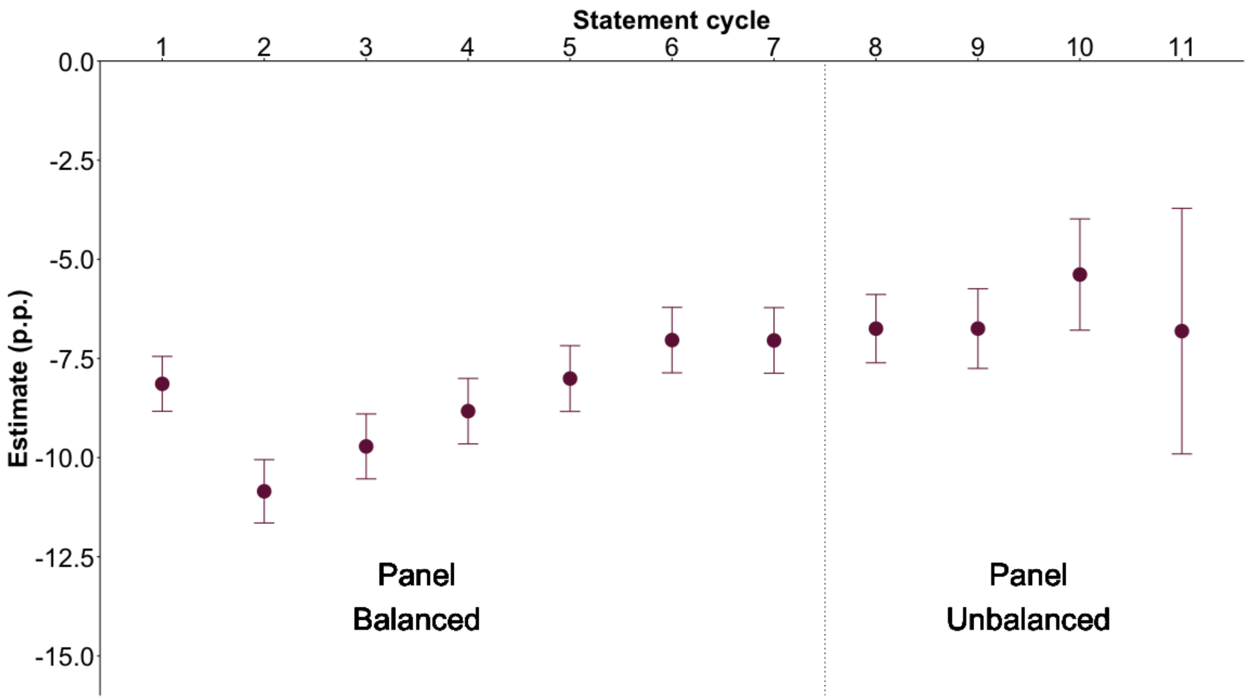


B: SECOND LENDER (N = 1,531)



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

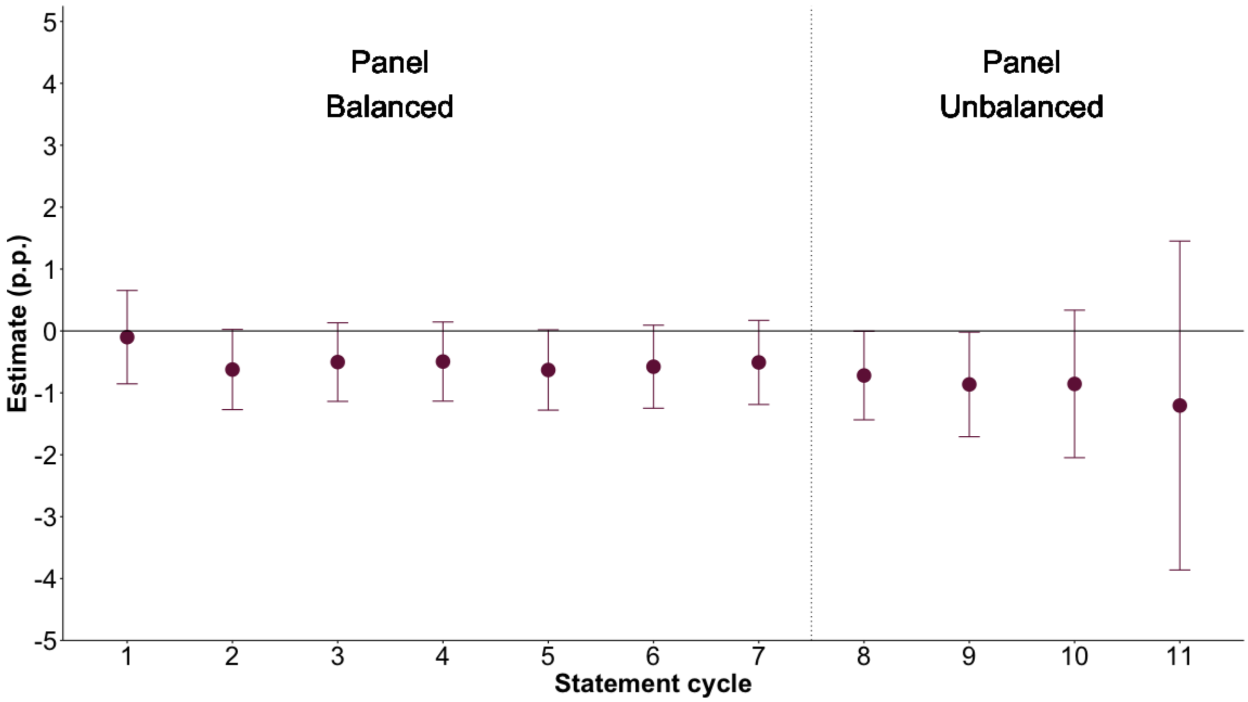
Figure 4: Average treatment effects on making only a minimum payment across 1-11 statement cycles



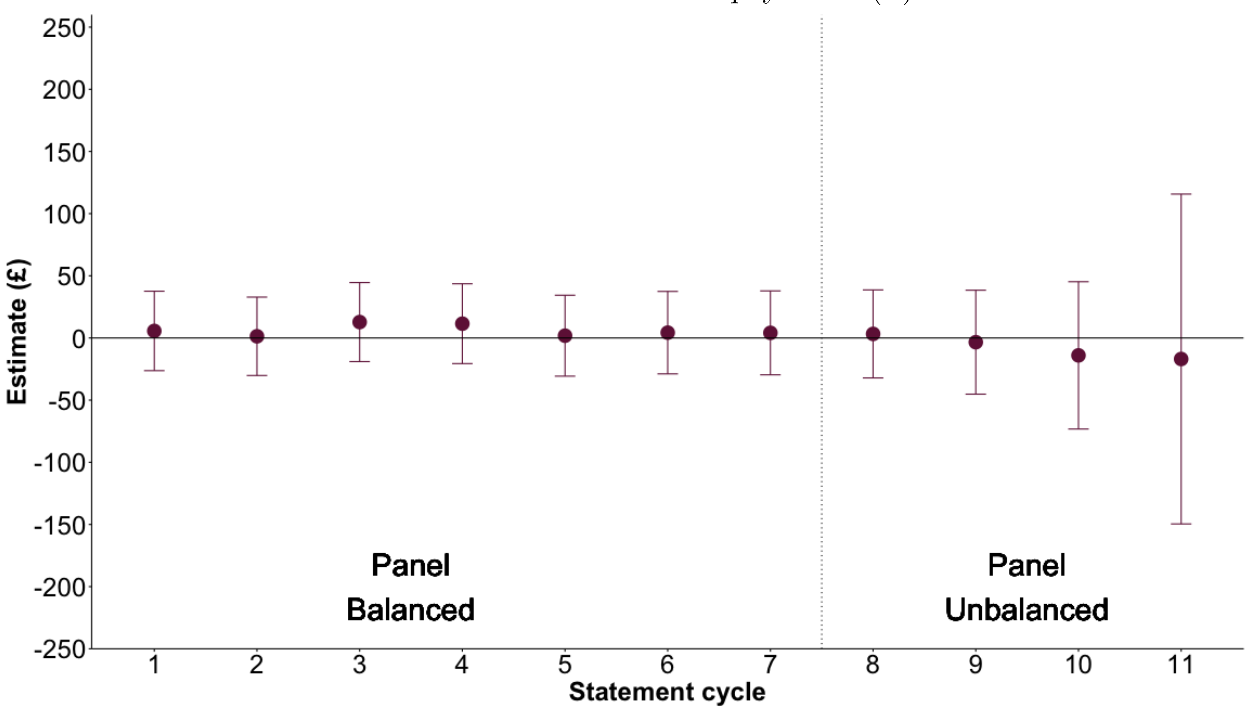
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure 5: Average treatment effects on credit card debt across 1-11 statement cycles

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

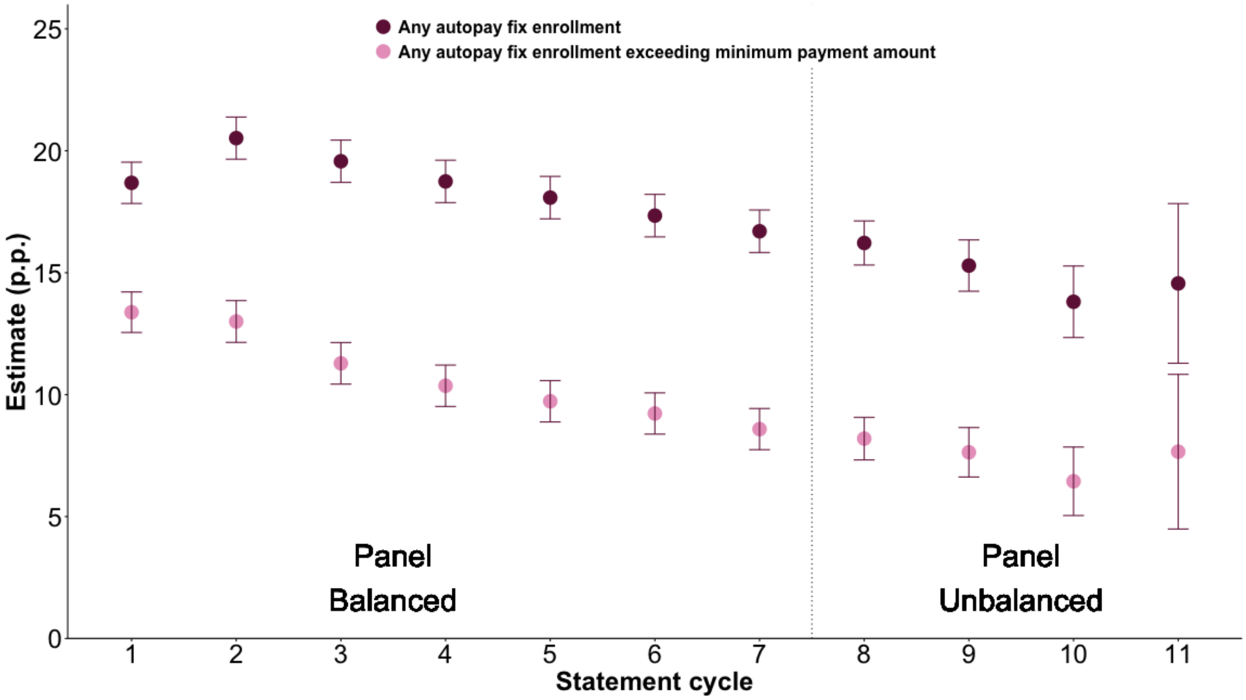


B: Statement balance net of payments (£)



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.

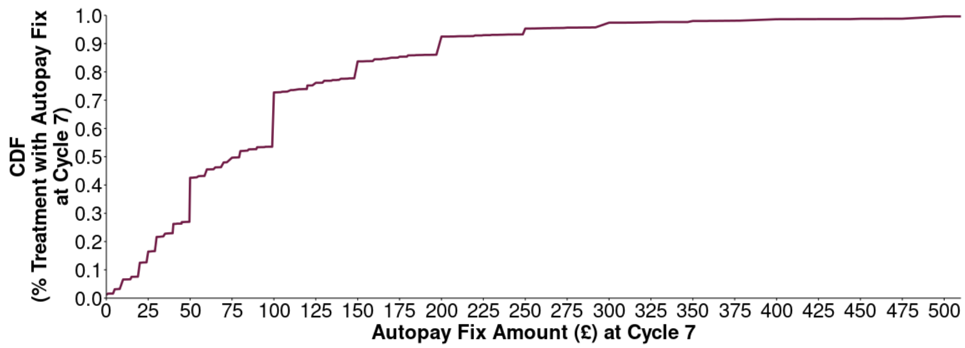
Figure 6: Average treatment effects on Autopay Fix enrollment (purple) and Autopay Fix enrollment not binding at minimum payment (pink) across 1-11 statement cycles



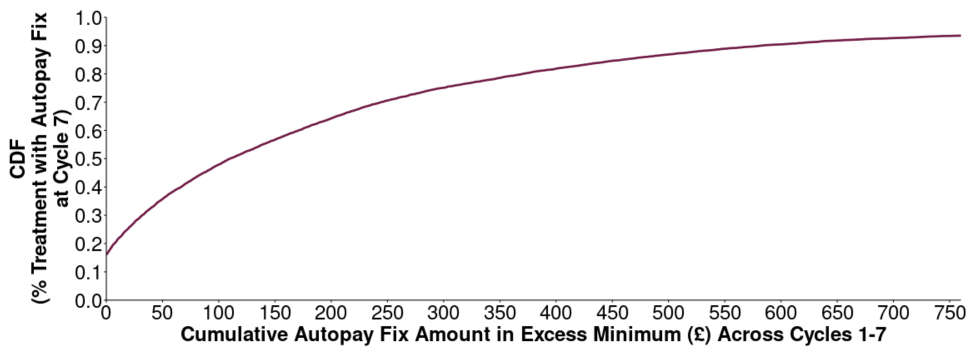
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

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

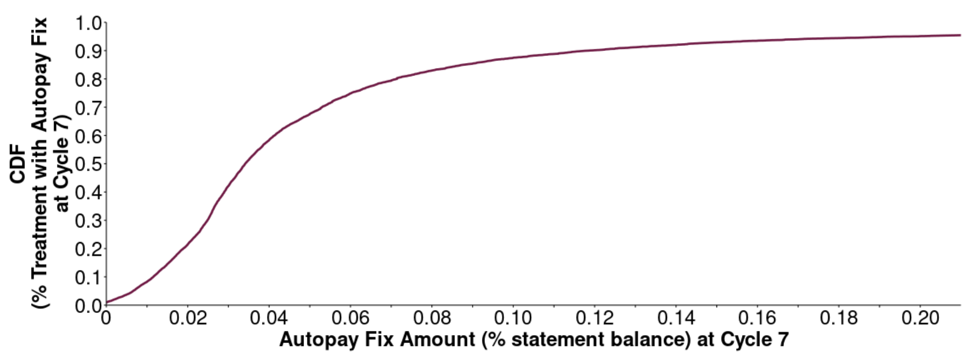
A: Autopay fix amount (£) at cycle 7



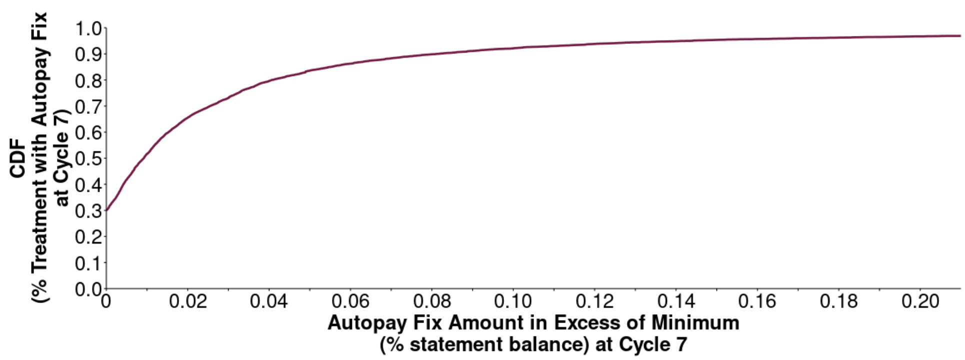
B: Cumulative autopay fix amount in excess of minimum (£) across cycles 1-7



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



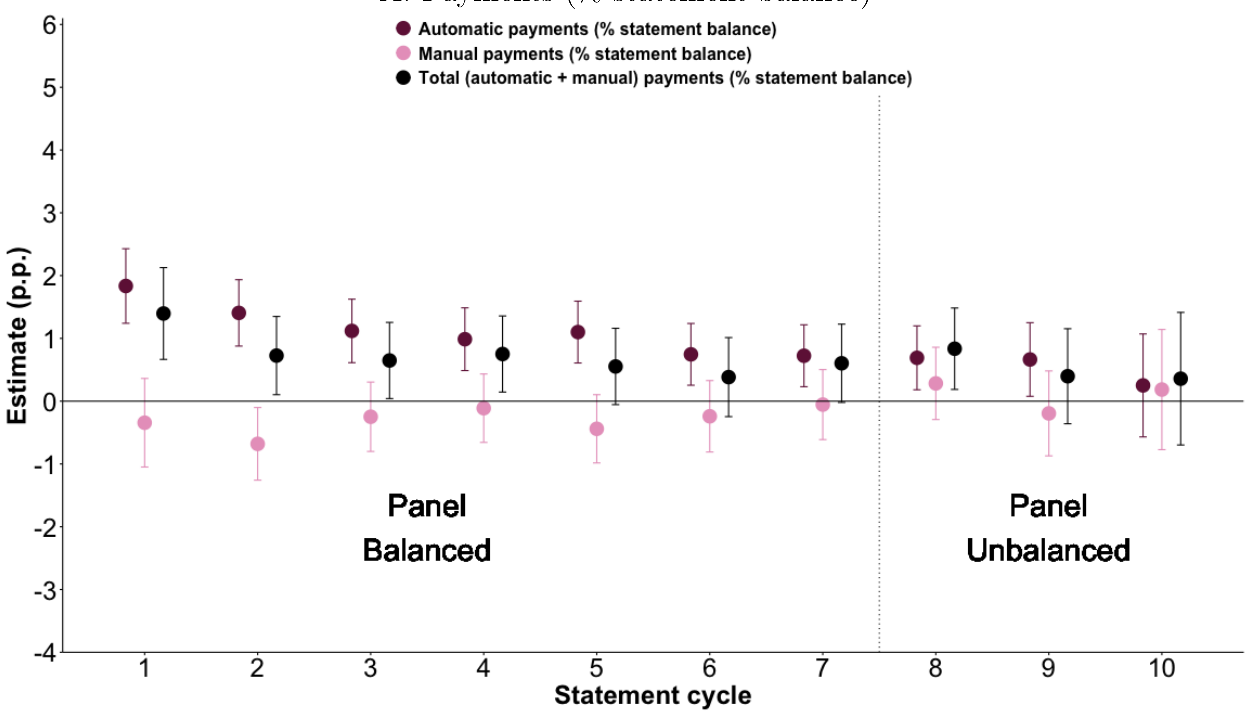
D: Autopay fix amount in excess of minimum (% statement balance) at cycle 7



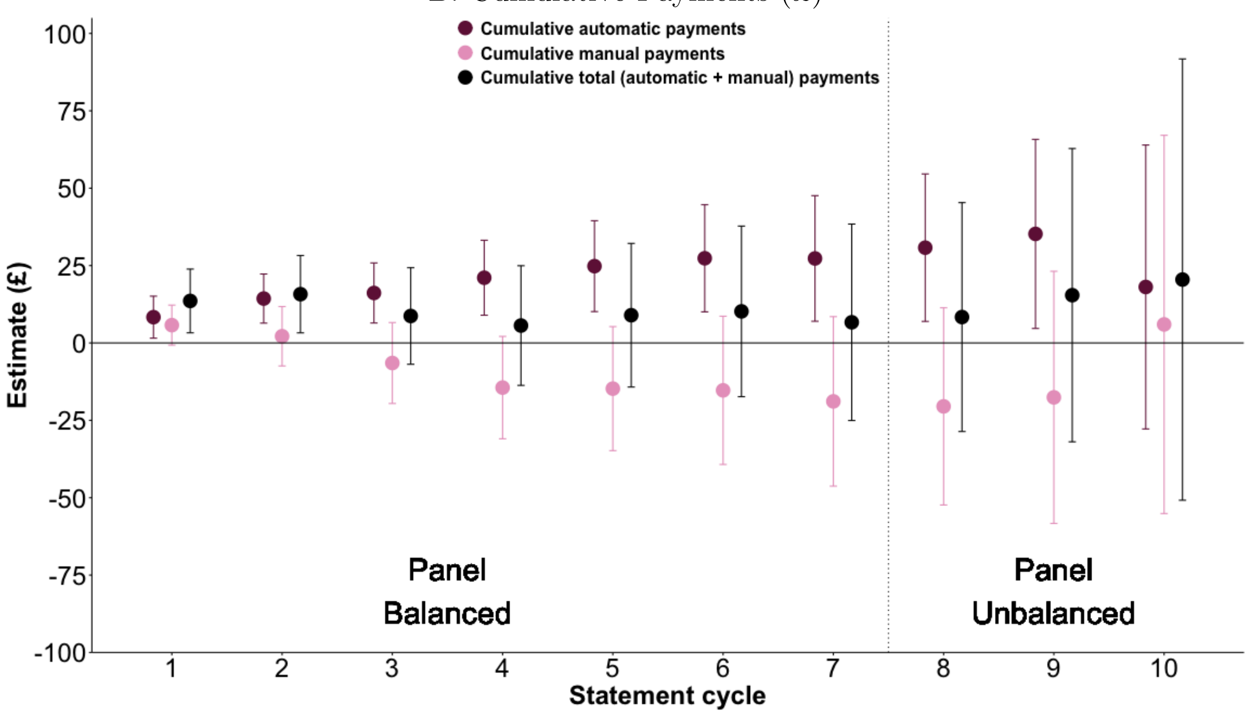
Notes: X-axes of CDFs are right-censored to ease presentation.

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

A: Payments (% statement balance)

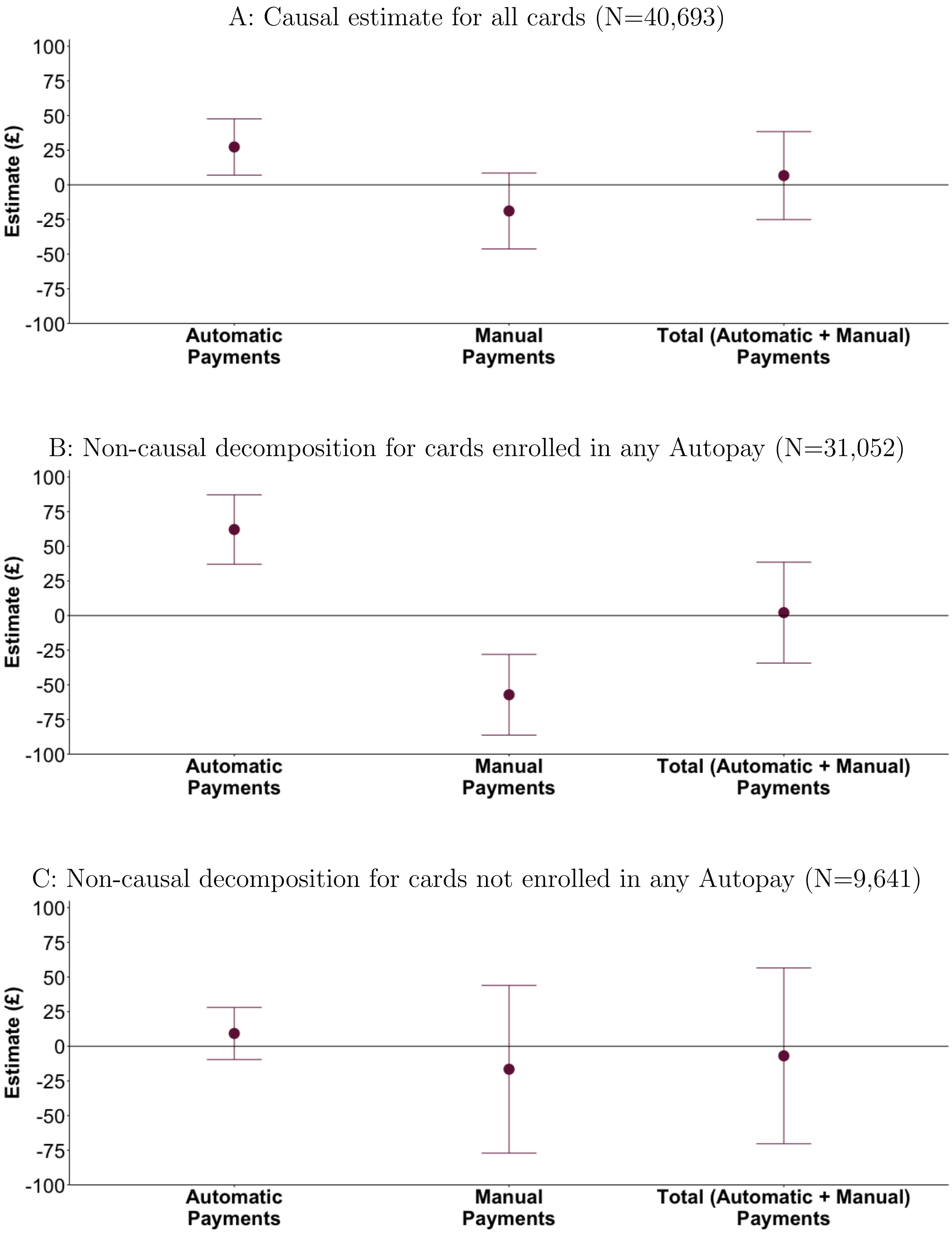


B: Cumulative Payments (£)



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. 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

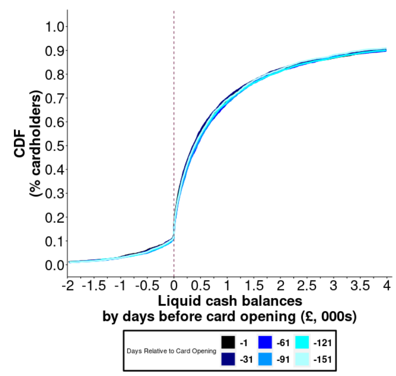


Notes: Panel A is causal estimated treatment effects from coefficients (δ_7) in OLS regression specified in Equation 1. Panels B and C show non-causal estimates (δ_7) from OLS regression specified in Equation 3. Standard errors clustered at consumer-level. Error bars are 95% confidence intervals.

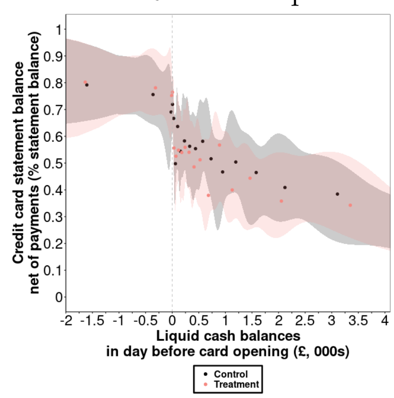
Figure 10: CDFs of liquidity constraints measured before card opening (left hand side panels) and their non-parametric relationships with credit card debt (statement balance net of payments as a % of statement balance) at statement cycle 7, by treatment group (right hand side panels)

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

A CDF

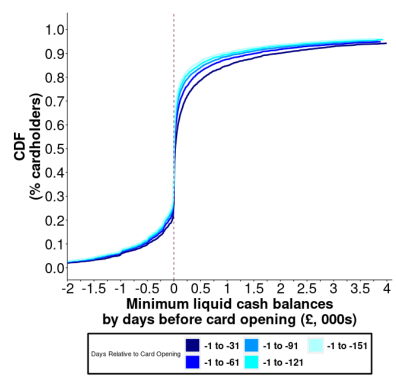


D. Relationship

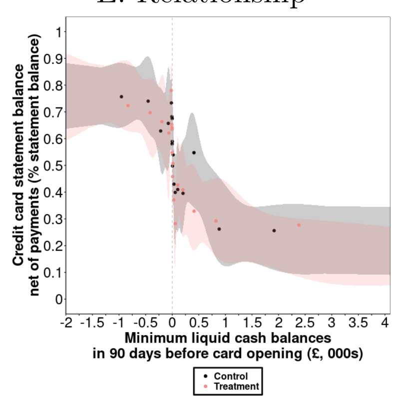


II: Minimum liquid cash balance reached during 90 days before card opening (£)

B. CDF

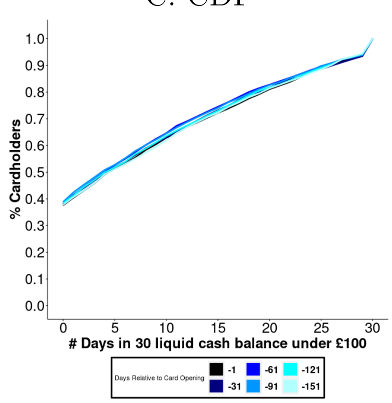


E. Relationship

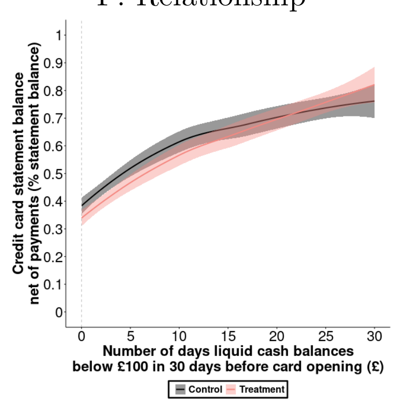


III: Number of days liquid cash balance below £100 in 30 days before card opening (# days)

C. CDF



F. Relationship



Notes: N = 3,753 consumers. Liquidity constraints 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: 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 2: 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	-0.0437∗∗∗	[-0.0517,	0.0000	0.7811
	(0.0041)	-0.0356]		
Autopay full	0.0065∗	[0.0009,	0.0217	0.1309
	(0.0028)	0.0120]		
Autopay fix	0.1670∗∗∗	[0.1583,	0.0000	0.2955
	(0.0045)	0.1757]		
Autopay min	-0.2172∗∗∗	[-0.2251,	0.0000	0.3547
	(0.0041)	-0.2092]		
Autopay fix exceeding	0.0859∗∗∗	[0.0774,	0.0000	0.2523
minimum payment amount	(0.0043)	0.0943]		

*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 3: 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.*

Table 4: 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 5: 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	0.4829***	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.*

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X Internet Appendix

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

A. Survey Experiment

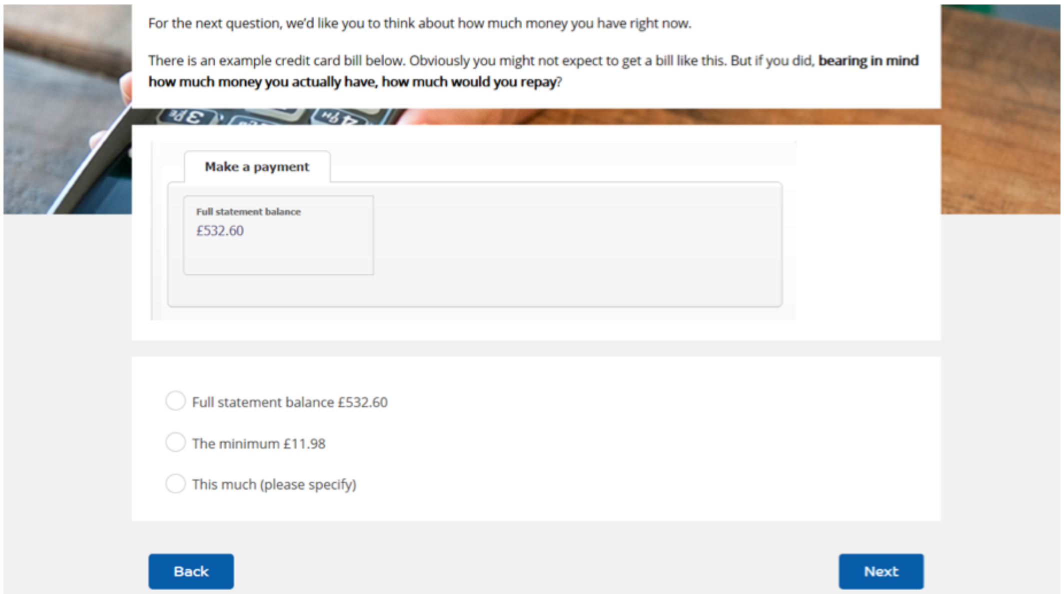
B. Field Experiment: Main Lender

C. Field Experiment: Second Lender

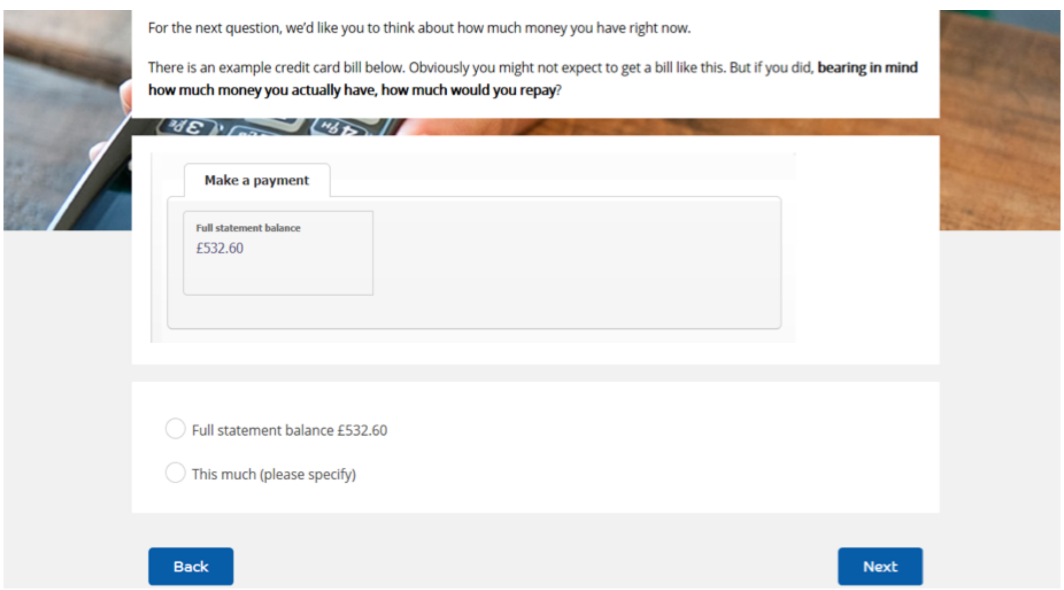
A. Survey Experiment

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

A: Control

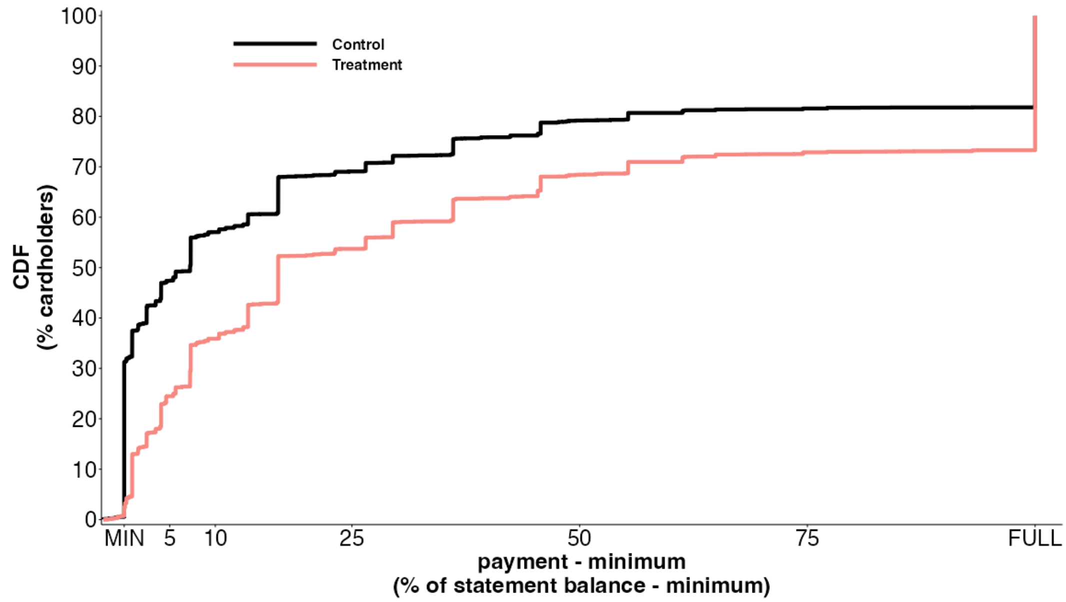


B: Treatment



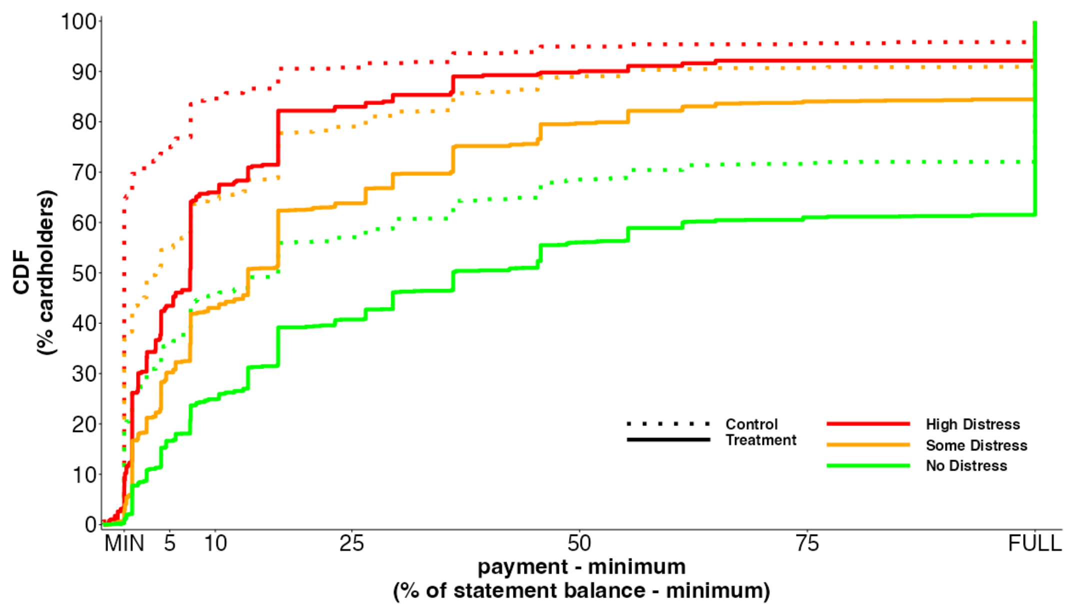
Notes: Survey experiment where treatment shrouds minimum payment amount. Consumers have to decide how much to pay on a hypothetical credit card balance. Consumers were randomized into (i) control or treatment and (ii) a low or high statement balance scenarios. Example is shown for low statement balance 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.

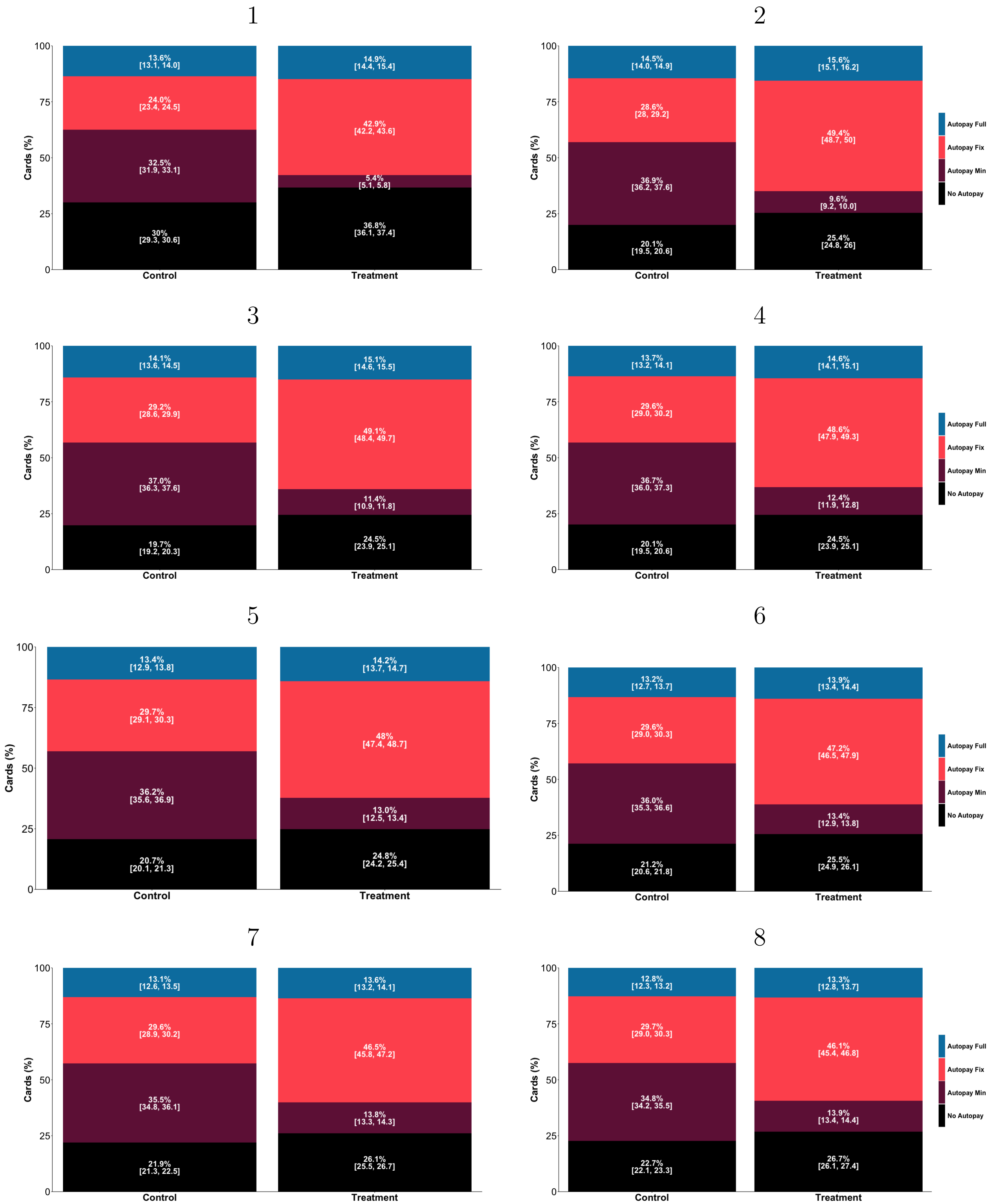
Table A1: Average treatment effects on hypothetical credit card payments from survey experiment where treatment shrouds minimum payment amount

	(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.*

B. Field Experiment: Main Lender

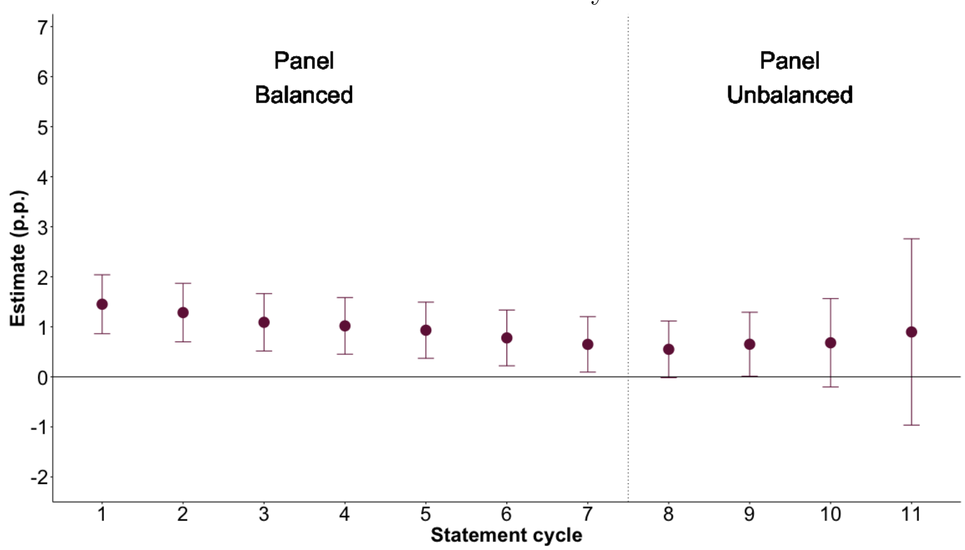
Figure B1: Autopay enrollment for control and treatment groups, by statement cycles one to eight



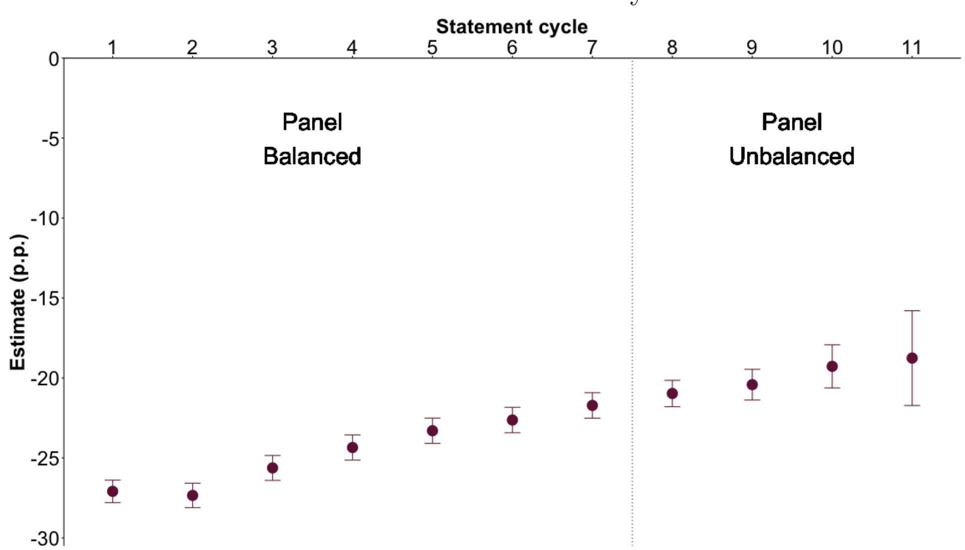
Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ]. Cycle 1 is before all treated cards have had 30 days to experience the treatment. Not all cards are observed in cycle 8.

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

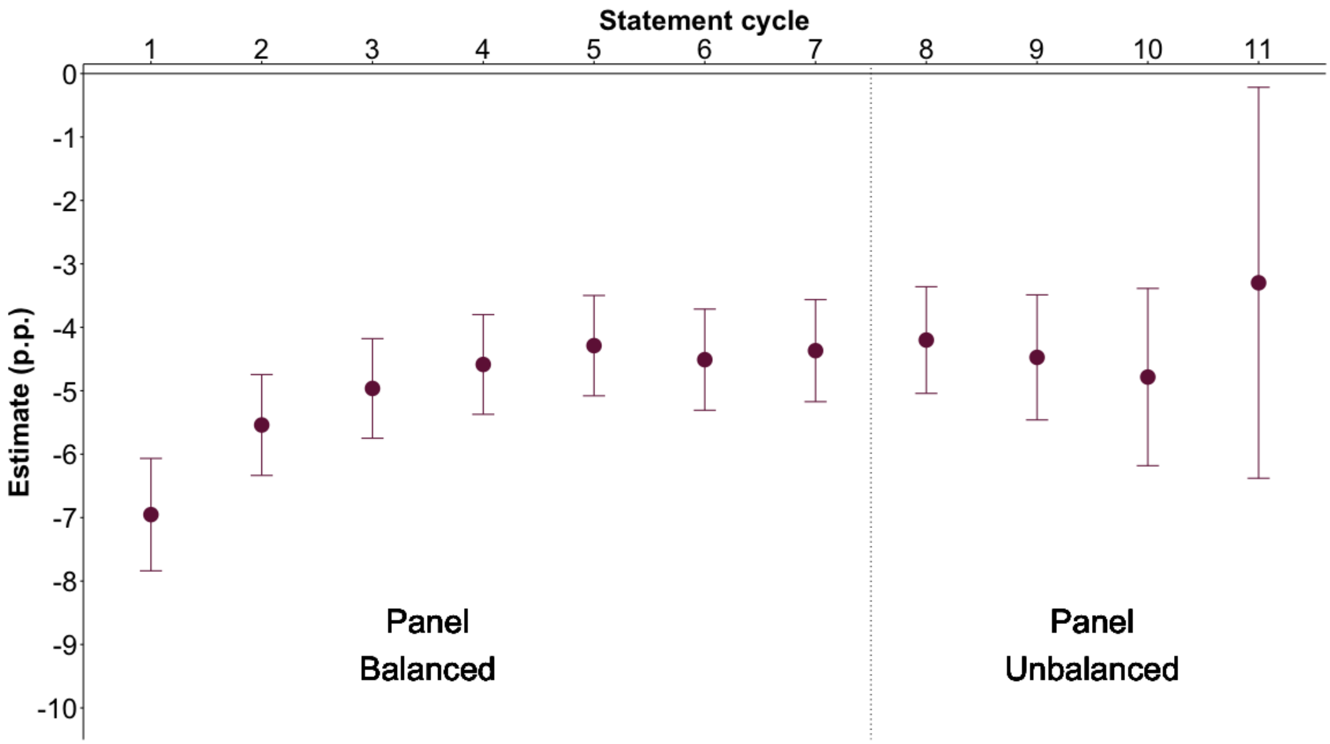


B: Automatic Minimum Payment



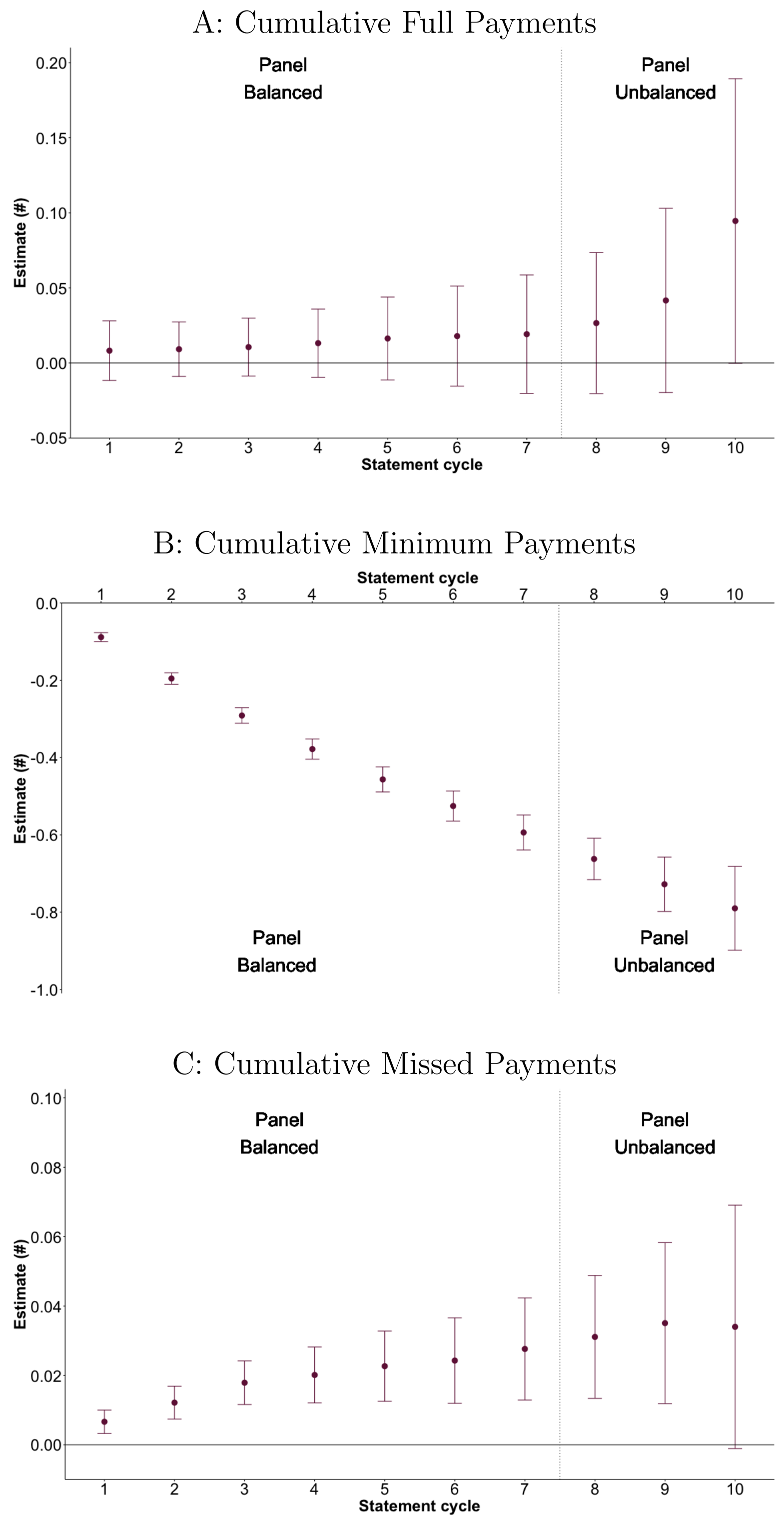
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

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.

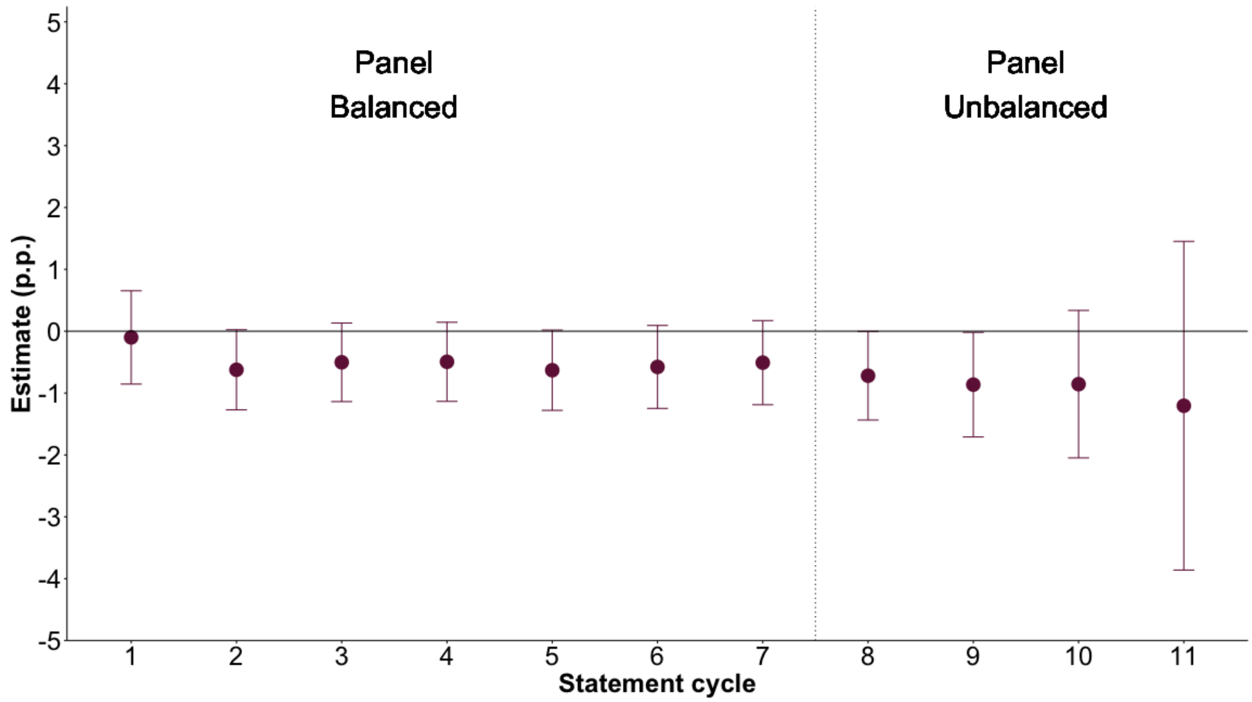
Figure B4: Treatment effects on cumulative number of full, minimum and missed payments across 1-10 statement cycles



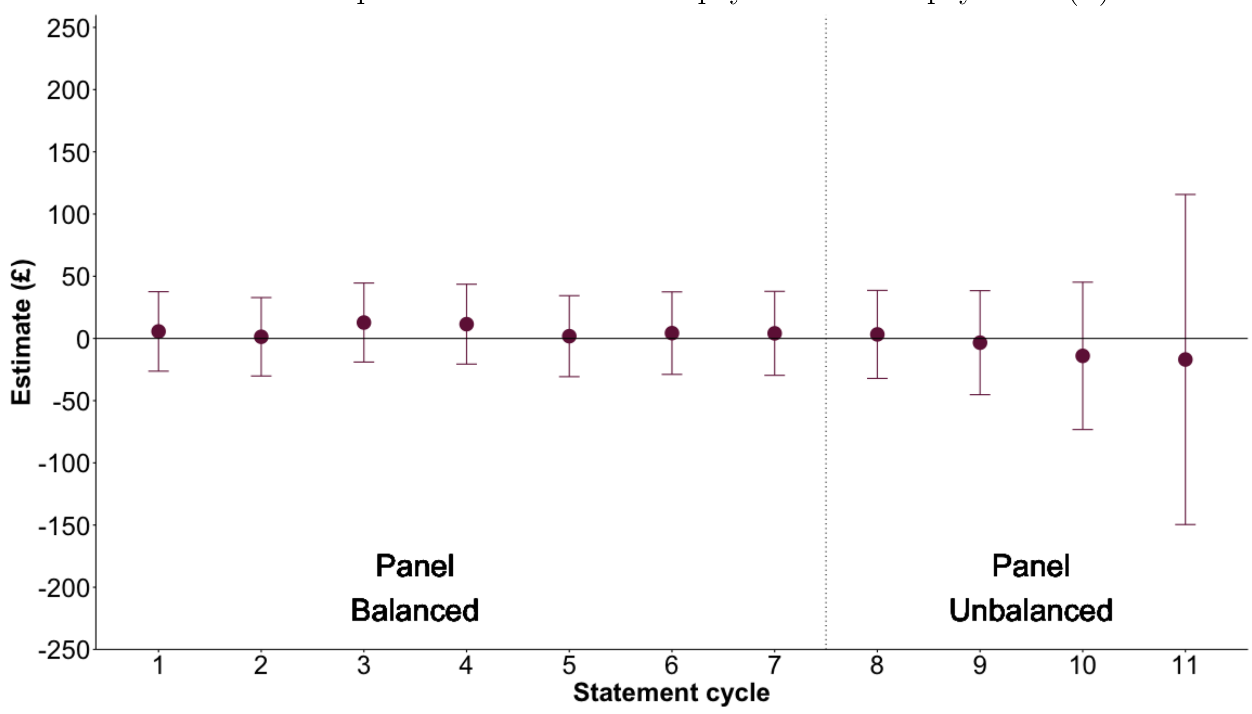
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large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

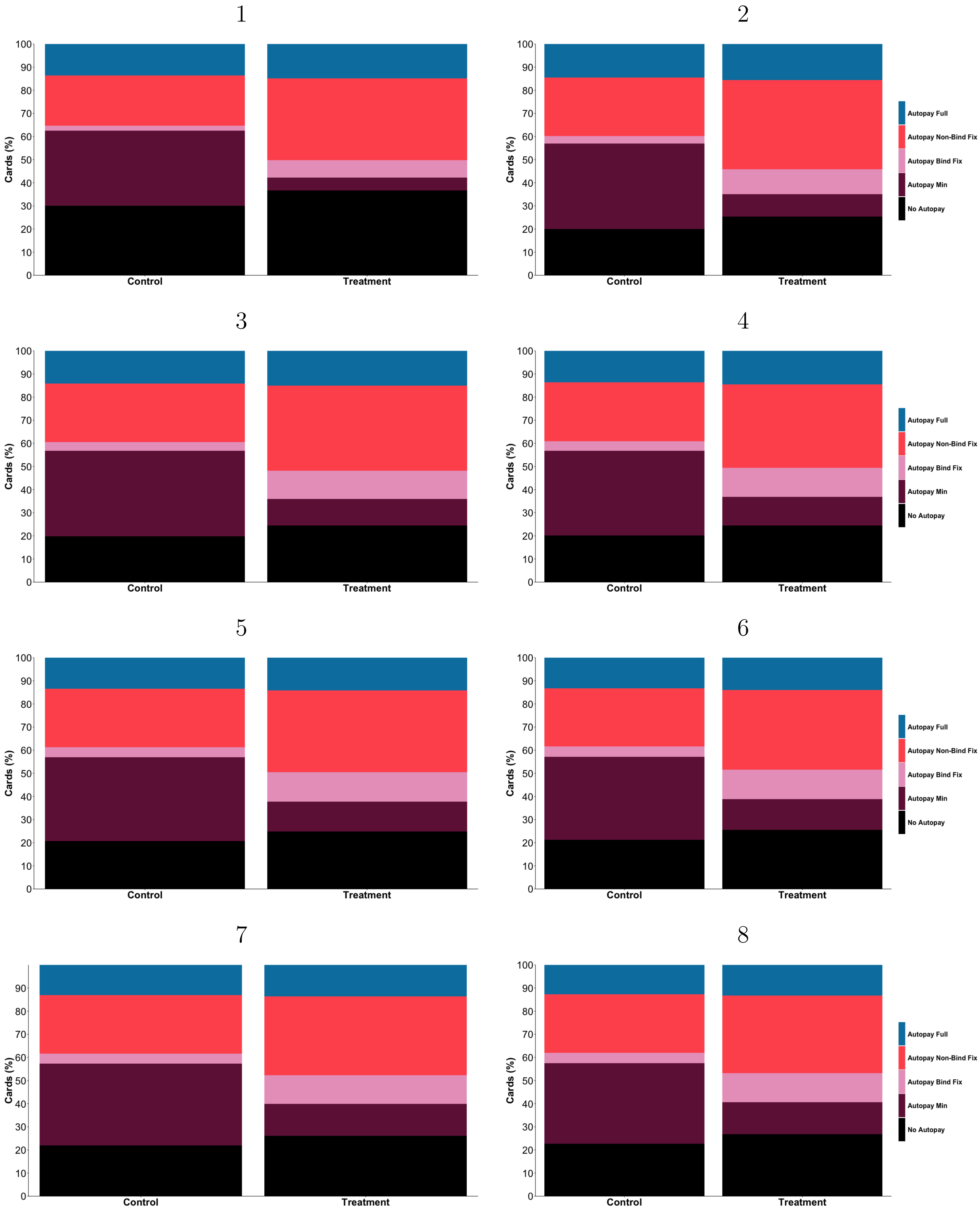


B: Credit card portfolio balances net of payments net of payments (£)



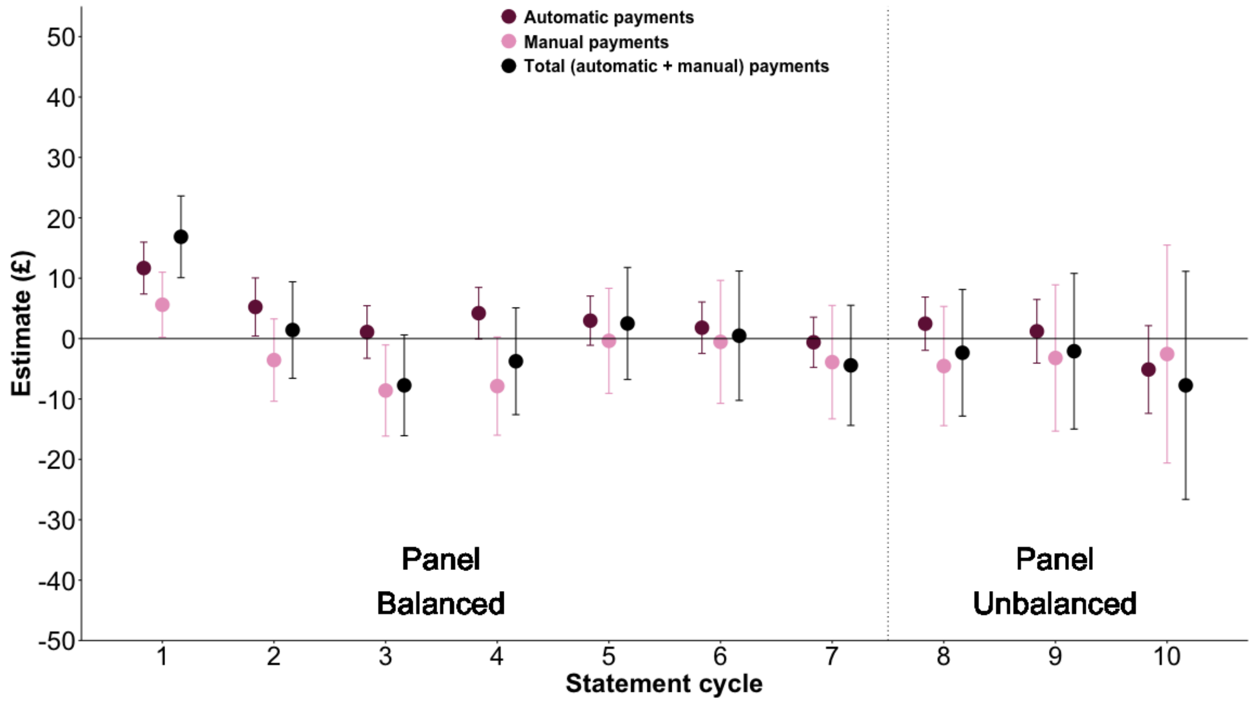
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

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



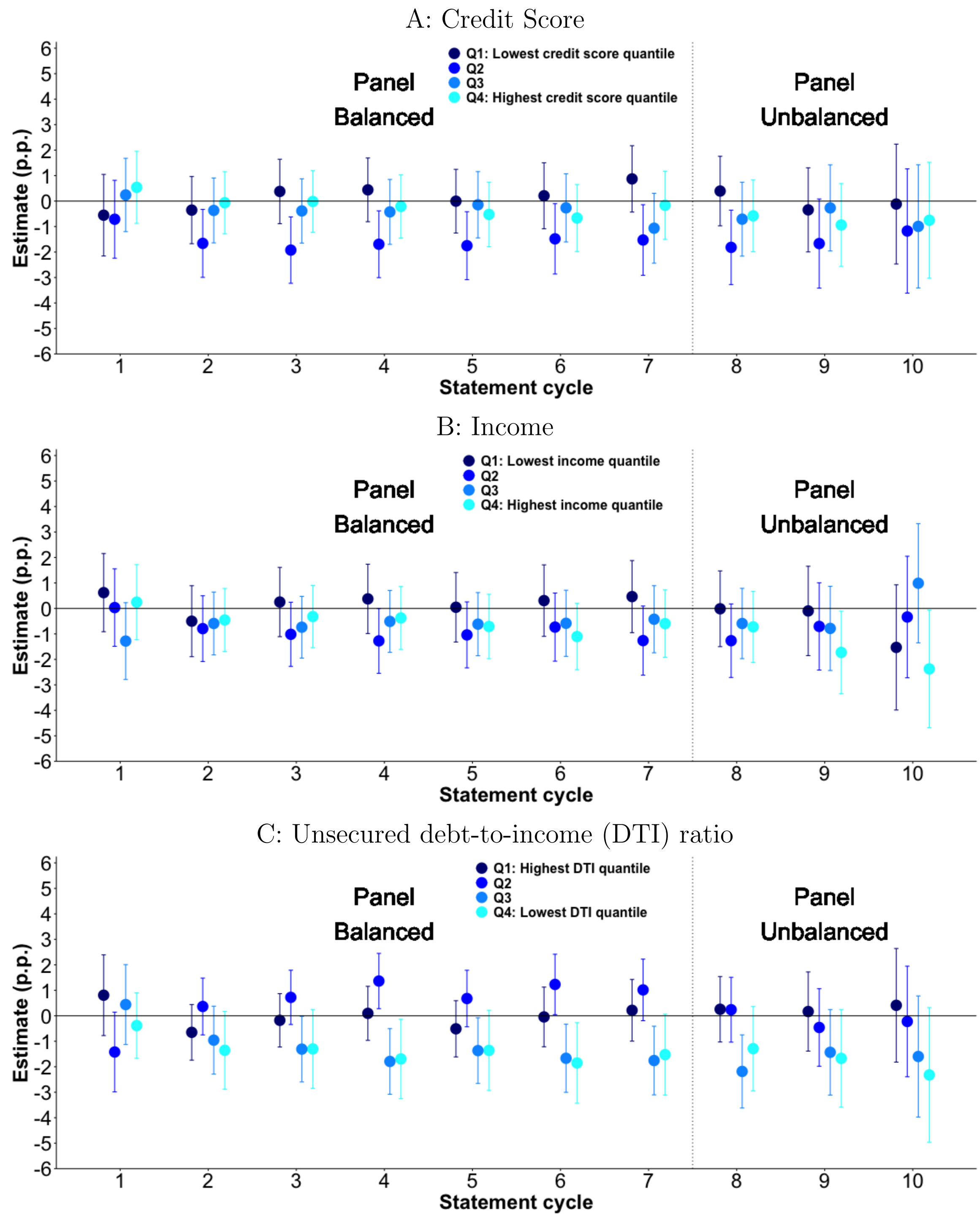
Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ].

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.

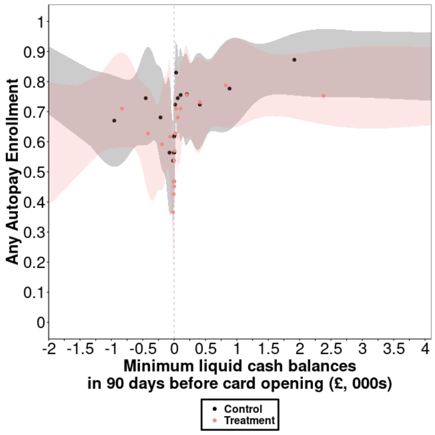
Figure B8: Heterogeneous treatment effects by quartiles of (A) credit score, (B) income and (C) unsecured debt-to-income (DTI) ratio on credit card debt (statement balance net of payments % statement balance) across 1-10 statement cycles



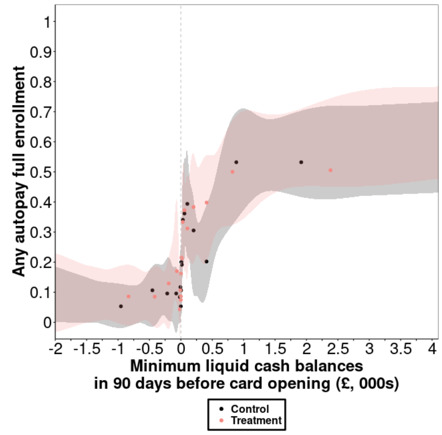
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Heterogeneous variables calculated from credit file data in month preceding credit card opening (& trial start). Cycle 11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

Figure B9: 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

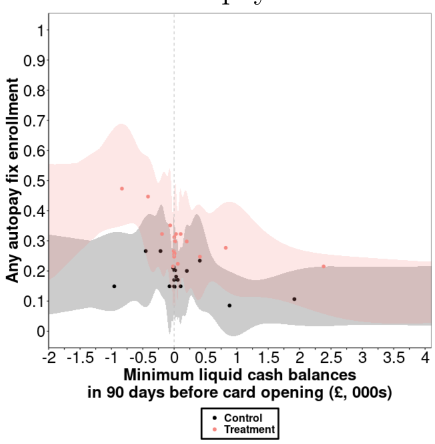
A. Any Autopay



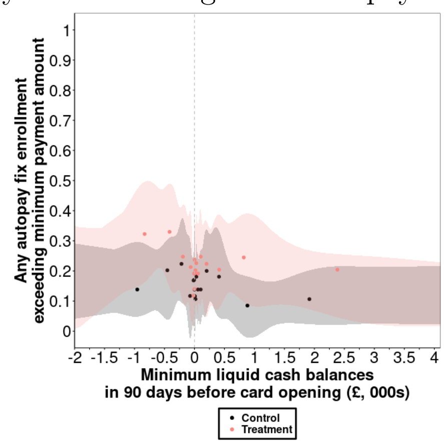
B. Autopay Full



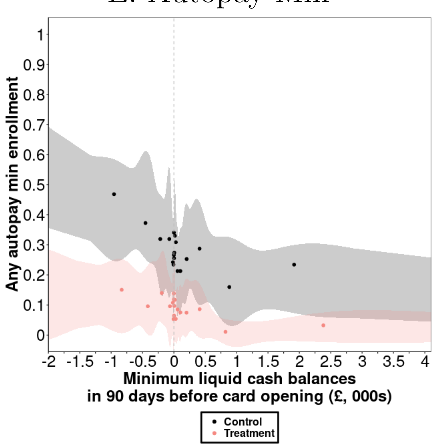
C. Autopay Fix



D. Autopay Fix exceeding minimum payment amount



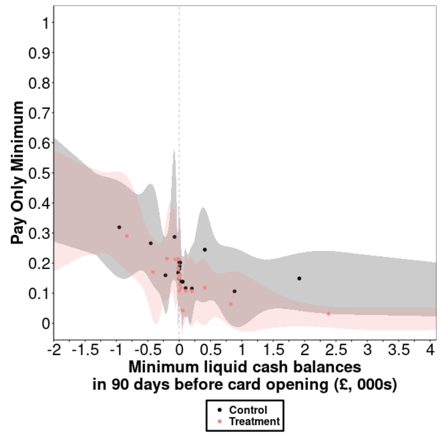
E. Autopay Min



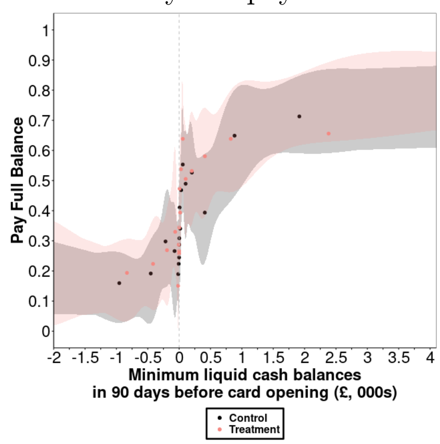
Notes: N = 3,753 consumers. Liquidity constraints are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.

Figure B10: 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

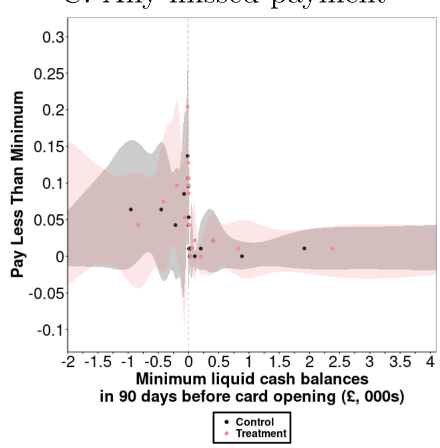
A. Any minimum payment



B. Any full payment



C. Any missed payment



Notes: N = 3,753 consumers. Liquidity constraints 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.

Table B1: 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 B2: 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 B3: 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 B4: 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 B5: 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 B6: 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 B7: 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 B8: 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 B9: 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 B10: 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 B11: 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.*

Table B12: 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 B13: 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.

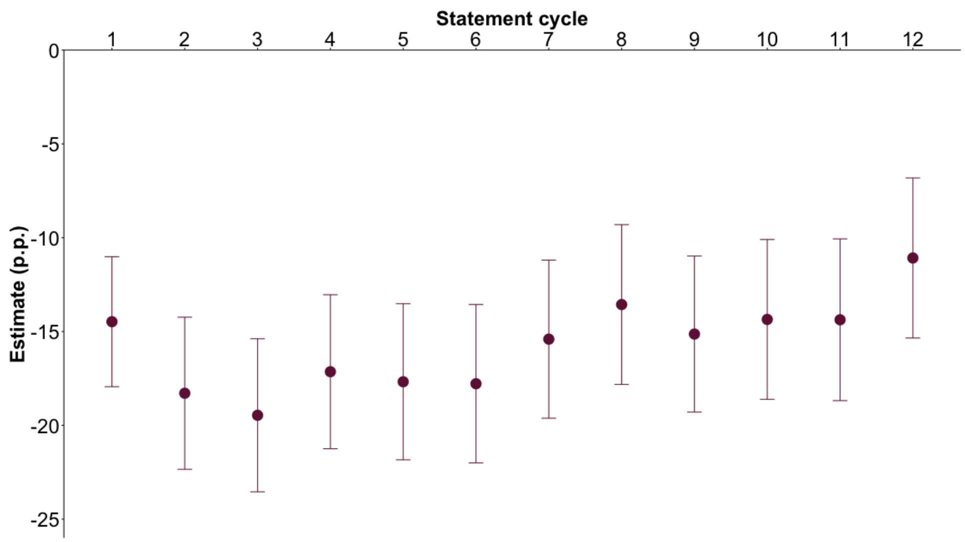
Table B14: Summary statistics on minimum liquid cash balances over windows preceeding credit card opening

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.

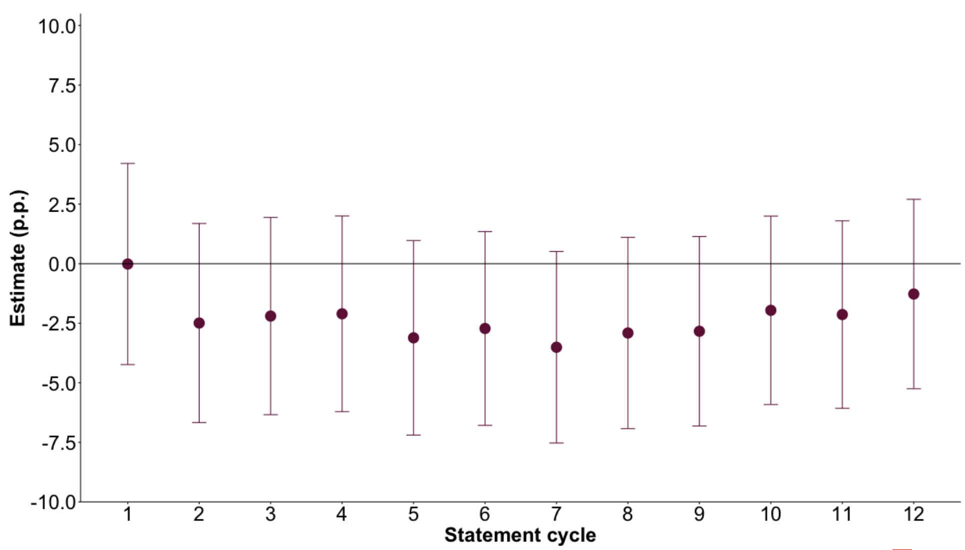
C. Field Experiment: Second Lender

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.*