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

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October 21, 2022

Abstract

We show how a nudge causing large proximate effects on consumer choices did not translate into distal effects on real economic outcomes. Our field experiment tests the effects of a credit card nudge. The nudge shrouds the Autopay option to automatically pay only the minimum due and increases the salience of an alternative Autopay option to automatically amortize debt faster. Despite the nudge causing large (21 percentage points) proximate effects on Autopay enrollment, it has no distal effects on reducing debt. This is due to offsetting consumer responses to the nudge including choosing 'low' Autopay amounts binding at or near the minimum, and lower Autopay enrollment increasing missed payments. Accounting for the dynamics of liquidity constraints explains why consumers do not reduce their debt. Consumers frequently face liquidity constraints that correlate with lower subsequent credit card payments.

Keywords: Autopay, consumer finance, consumer financial protection regulation, credit cards, FinTech, household debt, household finance, liquidity constraints, nudges.

JEL Codes: G51, D04, D90, D18, G28, L51.

*1st version: July 2018. We are grateful to the institutions we worked with for their cooperation, without which this research would not have been possible. We are extremely appreciative of feedback from Alex Chesterfield, Ben Keys, Brianna Middlewood, Christopher Palmer, C. Yiwei Zhang, Dan Bartels, Daniel Egan, Deniz Aydin, Elizabeth Linos, Eric Johnson, John Gathergood, Laura Smart, Kellen Mrkva, Michael Grubb, Michaela Pagel, Neale Mahoney, Phil Armour, Robert Metcalfe, Sam Hirshman, Taha Choukhmane, Tania Van Den Brande & Tony Cookson, and participants at ACR, AFE, ASSA, CFPB, Boulder Consumer Financial Decision Making, #EconTwitter Virtual Economics + Finance Research, FDIC, INFORMS Marketing Science, Leiden University, London School of Economics, Miami Behavioral Finance, NBER Behavioral Public Economics Bootcamp, NEST, NIBS, RAND BeFi, Russell Sage Foundation Behavioral Economics Summer Institute, Wharton & University of Chicago conferences and seminars. We thank the many Financial Conduct Authority (FCA) staff who supported this research – especially Brian Corr, Cheryl Ng, Hayley Fletcher, Jesse Leary, Karen Croxson, Kate Collyer, Lucy Hayes, and Mary Starks. David Laibson acknowledges support from the Pershing Square Foundation fund for the Foundations of Human Behavior Initiative at Harvard University. Neil Stewart's research was supported by Economic and Social Research Council ES/K002201/1, ES/P008976/1, ES/N018192/1 and the Leverhulme Trust RP2012-V-022 grants. These funding sources provided financial support to David Laibson and Neil Stewart but were not involved in any other aspects of the research. The views in this paper should not be interpreted as reflecting the views of the FCA or Competition and Markets Authority (CMA) – they are solely the responsibility of the authors. During this research project, Paul Adams, Benedict Guttman-Kenney & Stefan Hunt were FCA employees, David Laibson & Neil Stewart were unpaid academic advisors to the FCA. This study was approved by the FCA's IRB, lenders' internal governance and is registered on the AEA Registry AEARCTR-0009326.

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I Introduction

Credit cards are a large, economically important market: In the UK and US, the majority of adults hold at least one card and there is over one trillion dollars in outstanding balances.[1] Approximately one in four UK credit cards (FCA 2016a) and three in ten US credit cards (Keys & Wang 2019) only pay at or near the minimum. If a cardholder is only paying the minimum, then (i) their repayment is effectively only servicing debt interest payments rather than substantially paying down capital and (ii) debt reduction only happens if pay down of debt principal exceeds new spending. A pattern of repeatedly only paying at or just above the minimum can be costly given revolving card balances incur interest rates of near 20% (Ausubel 1991, Sakaguchi et al. 2022). And repayment schedules from only paying the minimum can be decades long - over 18 years for a typical balance (Adams et al. 2022).[2] Beyond interest costs, policymakers may be concerned that naïve, present focused consumers are over-consuming, which can potentially generate welfare losses (e.g. Meier & Sprenger 2010, Heidhues & Kőszegi 2010, 2015, Kuchler & Pagel 2021, Allcott et al. 2022).[3]

What policy tools are available to encourage consumers to increase payments and pay down credit card debt? One approach is to mandate information disclosure on the costs of persistently holding credit card debt and how much more consumers need to pay to reduce their debt. Such disclosures have been found to be ineffective in the US (Agarwal et al. 2015), Mexico (Seira et al. 2017), and the UK (Adams et al. 2022).[4] This is even when

[1] https://www.bankofengland.co.uk/statistics/money-and-credit/2020/february-2020, https://www.fca.org.uk/publication/research/financial-lives-survey-2020-appendix-a.pdf, https://www.federalreserve.gov/publications/files/2020-report-economic-well-being-us-households-202105.pdf, https://www.newyorkfed.org/microeconomics/hhdc.html.

[2] This amortization structure is somewhat similar to interest-only (or reverse) mortgages. Such mortgages have a fixed end point requiring the customer to decide to repay or refinance at the end of their term and may have large potential gains to refinancing if interest rates change. Whereas credit cards are open ended agreements without a salient event to prompt similar action and with relatively little variation in interest rates over the business cycle.

[3] There are many prior works documenting financial illiteracy and behavioral mistakes (e.g. anchoring, balance-matching heuristic, exponential growth bias) in the credit card market. For examples, see Ausubel (1991), Stewart (2009), Stango & Zinman (2009), Lusardi & Tufano (2015), Seira et al. (2017), Keys & Wang (2019), Gathergood et al. (2019a,b), Adams et al. (2022), Sakaguchi et al. (2022).

[4] Information on borrowing costs might have been thought to be useful to inattentive consumers as interest costs can accumulate to sizeable amounts without a salient event to make consumers aware of and

such information goes beyond factual disclosures to include behaviorally-informed language explicitly encouraging debt repayment (Adams et al. 2022). An alternative, more intrusive, paternalistic approach is to require higher minimum payments (e.g. Keys & Wang 2019) as doing so forces consumers to pay more. However, policymakers may be reluctant to use such an approach as it involves difficult trade-offs where the benefits may be outweighed by the costs (Campbell 2016) e.g. forcing consumers facing temporary liquidity constraints into arrears. A third, potentially attractive policy solution are "nudges" (or "non-price interventions", Bernheim & Taubinsky 2018): defined as "choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives" (Sunstein & Thaler 2008, Balz et al. 2014). In our credit card domain, nudges could encourage higher credit card repayments and reduce debt without the costs of causing consumers facing temporary liquidity constraints into arrears.

We test a nudge that changes the way in which credit card payment options are presented to credit cardholders. Our main finding is that this nudge causes large proximate effects on consumer choices that do not translate into distal effects on real economic outcomes. We tested this new nudge through a field experiment (Harrison & List 2004) on newly-issued credit cards in the UK. Our study finds a large, proximate effect on initial consumer enrollment but a null, distal effect on reducing credit card debt. We show that offsetting consumer responses and liquidity constraints can explain this contrast between proximate and distal outcomes.

Our experiment is targeted at new UK credit cardholders enrolled in automatic payments – known as 'Autopay' in the US or 'Direct Debit' in the UK. Consumers may enroll in Autopay for convenience: providing insurance against forgetting to pay a bill (e.g. Gathergood et al. 2021, Sakaguchi et al. 2022). However, Autopay means consumers no longer need to engage each month in making a decision on how much to pay and as a result may become inattentive and sub-optimally increase their consumption (e.g. Sexton 2015, Fuentealba

attentive to their borrowing costs. The non-salient, accumulation of interest costs contrasts with the salient fees and alerts triggered when consumers miss payments (Gathergood et al. 2021, Sakaguchi et al. 2022).

et al. 2021). Persistent minimum payments and high interest costs are especially concentrated among credit cardholders enrolled in 'Autopay Min' to automatically pay only the minimum each month (Adams et al. 2022, Sakaguchi et al. 2022). By one regulatory definition of credit cardholders in persistent debt – making 9+ minimum payments in a year on interest-bearing cards – 75% had Autopay Min (FCA 2016a). Autopay options are used by 42% UK cards (FCA 2016a) and 20-38% US cards (CFPB 2021) with growing use over time in both countries.[5] The Autopay Min option may only be used by 20% of active UK credit cards (Sakaguchi et al. 2022), however, cardholders with Autopay Min often repeatedly pay exactly only the minimum due, so barely pay down their credit card debt and incur high interest costs. Cards enrolled in Autopay Min account for 43% of total interest and fees across all UK credit cards (Sakaguchi et al. 2022).

Our nudge varies the way that Autopay options are displayed to consumers when opening a new credit card. Autopay choices typically allow consumers to enroll into automatically paying the full amount owed ('Autopay Full'), only the minimum due each month ('Autopay Min'), or a fixed amount of their choice ('Autopay Fix'). In the case of Autopay Fix each month a payment is taken for the maximum of this fixed amount and the minimum due. Our nudge removes the explicit appearance of the Autopay Min option, but this remains a feasible payment option for consumers. By shrouding the Autopay Min option we increase the salience of the Autopay Fix option which would automatically amortize debt faster – assuming no other changes in behavior.

Our pre-registered field experiment was designed as an ex-ante test to inform a potential consumer protection regulation that the UK financial regulator – the Financial Conduct Authority (FCA) – was considering implementing, given its concerns over consumers persistently holding credit card debt (FCA 2014, 2016b). We targeted new credit cardholders to try to be a preventative measure against consumers persistently carrying high credit card debt balances since attracting consumer attention and changing consumer behavior ex-post

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

is challenging (e.g. Adams et al. 2022). We conduct the experiment on 40,708 UK credit cards newly issued by one lender. A second lender pulled out of the field experiment before fieldwork was complete.

We find our treatment shrouding the Autopay Min option caused a large, proximate effect on Autopay choices: increasing initial Autopay Fix enrollment by 21 percentage points (72%). The likelihood of only paying exactly the minimum falls by seven percentage points in the treatment group. These effects are persistent over time. However, we do not find an average treatment effect on the distal outcomes of broader economic importance. We observe null effects, on average, on debt as well as spending, total payments, and borrowing costs after seven completed credit card cycles.

Why did the nudge have no distal effects on debt reduction, despite its large, proximate effects on choices? Three offsetting consumer responses explain this. First, we find the treatment results in consumers selecting Autopay Fix amounts that are 'too low': binding at or just above the minimum due. Second, the treatment lowers enrollment in any Autopay (i.e. Autopay Min, Autopay Fix, or Autopay Full). With fewer consumers being enrolled in Autopay, more miss payments and so their debt does not reduce. Finally, we also observe substitution. Automatic payments increase but this is offset by lower manual payments by those enrolled in any Autopay. We interpret this as revealing credit cardholders using Autopay are less inert and nudge-able than they first appeared: cardholders take action to counteract the nudge.

In addition to these offsetting consumer responses, we also show the role liquidity constraints play in explaining why consumers did not reduce their credit card debt. For a selected subsample, 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 measure shows it is common for consumers to experience binding liquidity constraints. Our new dynamic liquidity constraint measure (minimum liquid cash balances) reveals constraints bind for approximately 50% of consumers in our linked data, compared with just 10% using a traditional static measure. Our new dynamic liquidity constraint 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, on average, of their credit card debt seven cycles later than those with small, negative minimum liquid balances (also before card opening). There is a large prior literature studying liquidity constraints and showing its macroeconomic importance for producing more accurate models of consumption that goes back to Hayashi (1985). We advance this by providing micro evidence demonstrating how a combination of richer, linked data and accounting for the dynamics of liquidity constraints can help to better understand consumer behavior such as credit card repayment choices.

Our results demonstrate how important it is to measure the distal effects of nudges as these may significantly differ from proximate choices due to offsetting consumer responses to nudges. Our study fits into a broader debate on the effects of nudges (e.g. Thaler 2017, Laibson 2020, Chater & Loewenstein 2022). DellaVigna & Linos (2022)'s meta-study has systematically documented the effects of nudges. This found heterogeneous effect sizes across domains and revealed nudges are often a less effective policy tool than would had been previously inferred due to the publication bias of significant results published in early academic studies.[6] Nudges are still a valuable addition to a policymaker's armory given even a small benefit can prove cost-effective to implement (e.g. Benartzi et al. 2017, DellaVigna & Linos 2022). There are some parallels between our credit card study and recent research in retirement savings (e.g. Choukmane 2021) which finds automatic enrollment defaults to be less effective at increasing saving relative to the successes from large, positive effects on enrollment documented in the early literature (e.g. Madrian & Shea 2001, Thaler & Benartzi 2004). Nudges' effectiveness may also depend on the array of outcomes considered. Some

[6]DellaVigna & Linos (2022) show the average effect among academic published studies of nudges is 8.7 pp (33.4%) whereas the average effects from the population of studies from large Behavioral Insights Teams are far smaller: 1.4 pp (8%).

nudges may still appear effective when broader effects on consumers are observed (e.g. Chetty et al. 2014, Beshears et al. 2022), whereas some nudges may have adverse side effects (e.g. Medina 2021).

Finally, our paper provides an example of the value of regulators conduct ex-ante tests of potential regulations to inform potential policymaking. By testing potential regulations ex-ante, regulators can avoid imposing costly policies that are only found to be ineffective ex-post (e.g. as occurred with the 2009 US CARD Act disclosures Agarwal et al. 2015, Keys & Wang 2019). More broadly, our study shows how even policies that academics, financial firms, consumer organizations, and regulators expect to change consumer behavior, can struggle to do so in the real-world due to the reactions and constraints of consumers.

The paper proceeds as follows. Section II explains our experiment's design (II.A.) and implementation (II.B.). Section III covers the data (III.A.), empirical methodology (III.B.), and summary statistics (III.C.). Section IV presents the results of the experiment: proximate effects on Autopay enrollment (IV.A.), distal effects on real economic outcomes (IV.B.). Section V contains analysis exploring the mechanisms behind the results: offsetting consumer responses (V.A.), heterogeneous effects (V.B.), and liquidity constraints (V.C.). Finally, section VI offers a brief concluding discussion.

II The Experiment

In this section we explain our experiment: the nudge's design (II.A.) and how we implemented an experiment to test the nudge's effects (II.B.).

II.A. Nudge Design

Credit cardholders have a large amount of discretion in how much to repay each month (in contrast to fixed term loans): choosing to pay any amount between the minimum due and the full balance fulfils their contractual obligations. A common choice (FCA 2016a, Keys &

Wang 2019) is to only pay the contractual minimum payment due: this is typically calculated by $max\{£5,\ 1\%\ \text{statement balance}\ +\ \text{interest}\ +\ \text{fees}\}$.[7]

We designed a field experiment that varies the 'choice architecture' (Sunstein & Thaler 2008, Balz et al. 2014) of how Autopay payment options are presented to consumers at credit card opening. When a consumer takes out a new credit card online they typically have the option to opt-in to enroll in Autopay. If they decide to do so they are normally presented with three Autopay options: Autopay Full, Autopay Fix, and Autopay Min, as shown to the control group and displayed in Figure 1, Panel A. At this stage they can still decide against enrolling in any type of Autopay by not completing the enrollment process. They could also return and complete the enrollment later.

While Autopay Min was a common repayment option, cardholders also had the option to enroll in an alternative Autopay option that would repay debt faster 'Autopay Fix'. Autopay Fix is calculated by: $max\{\text{Autopay Fix £},\ \ \text{Minimum Payment Due}\}$. While the minimum payment – and therefore Autopay Min – typically declines with balances, a fixed payment sticks to the higher amount in a ratchet-like way smoothing payments over time. For example, a typical credit card balance of £1,000 would take 18 years and 6 months to pay off if only the minimum was paid each month (which would start around £25 and reduces to £5). However, by fixing to paying £25 each month it could be dramatically reduced to 5 years and 1 month, saving over £750 in interest costs.

Further, over and above having a fixed payment which does not reduce with the balance, choosing a slightly higher payment also greatly reduces amortization times and borrowing costs. For example, a balance of £1,000 is paid off in 5 years and 1 month with an interest cost of £509 with a monthly fixed payment of £25, but with a monthly payment of £50 the balance is paid off in 2 years costing £191 in interest (assuming 18.9% APR and no further

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

card spending).

The treatment webpage, shown in Figure 1, Panel B, is a nudge shrouding the ability to automatically pay only the minimum. This is done by removing the explicit appearance of the Autopay Min option shown to the control group (Panel A). Doing so increases the salience of the alternative, Autopay Fix option – that would automatically reduce debt (ceteris paribus). This treatment has never been tested before. Our decision to shroud the Autopay Min option was informed by prior research showing the minimum payment amount can act as a bad nudge anchoring consumers to repay low amounts (e.g. Sunstein & Thaler 2008, Stewart 2009, Keys & Wang 2019, Guttman-Kenney et al. 2018).

The treatment aims to work by first increasing Autopay Fix enrollment which, relative to Autopay Min, would be expected to increase automatic payments (and significantly shorten the hypothetical repayment schedule) which, in turn, increases payments above the minimum and, assuming spending is unchanged, reduces debt and interest costs.[8] This treatment was designed to be a preventative measure against consumers persistently carrying high credit card debt balances. Given cardholders rarely revise their Autopay after their initial choice, even with nudges to do so (Adams et al. 2022), we hoped our treatment would harness this inertia 'for good' to automatically reduce credit card debt.

Without an explicit Autopay Min option consumers are forced to make an enhanced active choice – considering how much they can afford to regularly pay each month.[9] We purposefully designed our treatment to avoid anchoring consumers to follow a particular alternative payment schedule, we just wanted them to avoid easily enrolling to automatically pay only the minimum. We took this approach given prior literature (Agarwal et al. 2015, Keys & Wang 2019) studying the US CARD Act disclosures showed providing consumers with credit card repayment scenarios can result in an unintended anchoring effect

[8] It could possibly also yield second order effects of increased consumer spending from increased credit limit availability, given the findings of consumer responses to credit limit increases (e.g. Gross & Souleles 2002, Agarwal et al. 2017, Aydin 2022)

[9] Keller et al. (2011) reviews the active choice literature.

e.g. reducing payments for some consumers.[10]

While there is no longer an explicit Autopay Min option, consumers can effectively choose this option if they set an Autopay Fix of £5 (or less). The two options are equivalent because the minimum payment is calculated as $max\{£5,\ 1\%$ statement balance + interest + 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 (two worked examples of this are in the footnote).[11] This equivalence is not highlighted to consumers and we do not expect them to be aware of this or work this out, nor is there a strong reason why they should. We explain this here 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 – instead such an option is just no longer explicitly labelled on the website. If a consumer in either the control or treatment group phoned the lender's call center they could still set-up an explicit Autopay Min if they asked to do so but consumers were not directed to this option as part of the experiment. After 30 days post card opening consumers in both control or treatment would view identical control group screens containing explicit Autopay Min options if they returned to the pages to enroll in or change their Autopay choices.

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.

[10]Kuchler & Pagel (2021) show that FinTech users who set target debt repayment plans often fail to stick to these due to naïve present bias.

[11]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).

II.B. Experiment Implementation

We test the nudge through a randomized controlled trial (RCT) tested in the field. All UK credit card lenders were invited by the FCA to voluntarily participate in these field trials.[12] 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 governance reviews at the FCA and also at both lenders.

We implemented the experiment on new credit cards. This is because as part of a general trend of increasing use of Autopay over time, Autopay Min is a payment method increasingly used by new card openings.[13] 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 selected the option confirming that they wanted to enroll for Autopay, they were included in the experiment. Inclusion in the experiment is irrespective of whether the Autopay enrollment process is completed after reaching this screen. At this point consumers were directed to either control or treatment (the nudge) group screens based on random assignment.[14] Once allocated to control or treatment the consumer would view these same screens if they returned to the pages to enroll in or change their Autopay choices within 30 days.

We conducted a field experiment on 40,708 credit cards newly issued by a large UK lender between February and May 2017.[15] We also ran the experiment with a second lender. This second lender stopped the experiment after one week of fieldwork due to concern over the

[12] See Adams et al. (2022) for other field trials we ran contemporaneously testing whether information and informational nudges on monthly statements as well as standalone emails or letters to cardholders already enrolled in Autopay Min could change behavior ex-post. These had zero or small proximate effects on choices and were ineffective at changing real economic outcomes Adams et al. (2022). See Guttman-Kenney et al. (2018) for an example where no firms were willing and able to participate in a field experiment and framed field experiment (along with earlier lab evidence in Sakaguchi et al. 2022) was used as a second best alternative to inform policymaking.

[13] https://www.ukfinance.org.uk/system/files/Summary-UK-Payment-Markets-2018.pdf

[14] 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.

[15] We wanted at least 20,000 cards in each of control and treatment group. The final figure was slightly higher as for logistical reasons new cards were included until the end of May 2017.

large size of the proximate effects on Autopay choices. The experiment was not restarted and the pre-agreed target sample size was not reached. The achieved sample size of 1,531 cards is therefore a result of p-hacking and is also insufficiently powered to distinguish between null results and real effects. Had we known this lender would have pulled-out we would not have run the experiment with the second lender. This shows the practical challenges of running such experiments. For completeness results from the second lender are in the Online Appendix (see footnote for details).[16] The rest of this paper is based on the experiment conducted with the first lender unless explicitly stated otherwise.

III Data & Methodology

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

III.A. Data

Our data was gathered by the UK financial regulator (FCA) using its statutory powers. From the two credit card lenders in the experiment we collected 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.[17] Each individual payment made against these statements is observed including the date, amount, and whether the payment was made

[16] Second lender's unconditional means are presented in Tables A13, A14, and A15 for balance checks, primary outcomes, Autopay enrollments respectively. Regression estimates are presented in Tables A16 and A17 for primary outcomes and Autopay enrollments respectively. Finally, cycle-by-cycle regression estimates are shown in Figures A10 and A11 for primary outcome measures of any minimum payment and statement balance net of payments (% statement balance).

[17] For the second lender we observe twelve completed statement cycles.

automatically or manually.

Credit files were gathered for all the individuals in the experiment enabling us to observe effects across the portfolio of credit cards held by a consumer. These provide monthly, product-level data on credit use 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 made a minimum payment. UK credit files contain payments data for all credit cards a consumer holds – this is higher quality than US credit files where only a selected subset of credit cards report payments data (Guttman-Kenney & Shahidinejad 2022). 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. The lender microdata and credit files are linked using an anonymous key created for this project. All analysis was 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 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.[18] These cardholders are more likely to be younger, with higher incomes and

[18]The choice of threshold used produces similar sample sizes. For example, requiring average account credits and debits are both £500 results in 3,552 cardholders compared to a threshold of £100 that results

credit scores, fewer credit cards and lower credit card debts (Online Appendix Table A12).

III.B. Empirical Methodology

Following best practice in conducting field experiments (Harrison & List 2004) we pre-registered our empirical methodology before analyzing data. Our pre-registration outlined the structure of analysis including the primary outcomes, regression specifications, and thresholds for evaluating statistical significance which we now document here.[19]

We structured 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 outcomes upon which the nudge's effectiveness was evaluated. The primary outcomes are:

1. **Any minimum payment:** Binary outcome for target card. Defined as only paying exactly the minimum due (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 due.

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

in 3,831 cardholders.

[19] 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.

of statement balance.

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

7. **Share of credit card portfolio only paying minimum:** Outcome ranging from zero to one. Defined as the proportion of credit cards paying exactly the minimum due (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 due.

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 & 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 regarded 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.[20] 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.[21]

The pre-registered secondary analysis considers a broader set of outcomes and empirical approaches to check the robustness of the primary results and understand the mechanisms driving the results in greater detail. For example, we measure the proximate effects of the nudge on Autopay enrollment and evaluate long-run effects measuring the amounts of credit card debt and repayments in levels (£). Conducting secondary analysis depended on the results from the primary outcomes. Finally, we designed and implemented 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

[20]Significance at 0.005 aligns with Bayes factors of 14+ which is often considered as substantial evidence for a hypothesis.

[21]The minimum detectable effect sizes for primary and selected secondary outcomes are shown in the Online Appendix Tables A1 and A2 respectively.

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.

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

Our regression includes a constant (α), a vector of time-invariant control variables (X_i') constructed using information on the new credit card opened and cardholder data from before the start of the experiment. 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.[22] 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, median 11 days from card opening) to on-board the card onto

[22]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 - 245 days.

a particular billing cycle and as a result this first statement has a zero payment due that makes it uninteresting (but we show results for completeness). Therefore a customer's first full statement is statement two (in our data the second statement is issued mean 43, 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 aggregating 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 $TREATMENT_i$ as shown in Equation 2 where our single static parameter of interest is δ.

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

III.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. Cardholders have a wide range of interest rates, credit scores, credit card credit limits, ages, and incomes.

In line with the motivation of our experiment the cardholders in our control group are often only paying the minimum due. 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 £2,165 and £1,962 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 (Online Appendix Table A3). However, we do see 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 (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 and their implications when compared against results from unconditional means.[23]

IV Experimental Results

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

IV.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 2, Panel A shows the treatment caused 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 2, 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.

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

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 (this is 4.8% of the subset enrolled in Autopay Fix) set an Autopay Fix of £5 or less. This is a statistically significant increase relative to 0.5% in the control group but we interpret as being economically small. 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 made are persistent over time.[24] 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 average 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 a mean increase in Autopay Full enrollment of 1.2 pp. This 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.

[24] Online Appendix Figure A1 shows Figure 2 for each cycle.

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 increased 16.7 pp, Autopay Full increased by 0.6 pp (the latter being only significant at the 5% not the 0.5% level), and any Autopay enrollment declining by 4.4 pp.[25] Estimates cycle-by-cycle (δ_τ in Equation 1) are displayed in Figure 5 for Autopay Fix enrollment, Online Appendix Figure A2 for Autopay Full and Autopay Min, and Online Appendix Figure A3 for any Autopay. Over time, the small initial effect on Autopay Full enrollment attenuates to being statistically insignificant from zero. The Autopay Fix and Autopay minimum also attenuate but effects remain large. Effects of the treatment on any Autopay enrollment change relatively little between cycles two and eight.

IV.B. Distal Effects on Long-Term Real Economic Outcomes

We examine the effects on our ten primary outcomes using our pre-registered regression specification. All estimates of outcomes are seven statement cycles after card-opening (δ_7 in Equation 1) and are shown in Table 3 (Online Appendix, Table A4 shows unconditional means).

We find a large and persistent effect of the treatment making cardholders less likely to only pay exactly the minimum (as a result of either, or both, Autopay and manual payment). The treatment causes a significant reduction in the likelihood of only paying exactly the minimum of 7.1 pp (95% confidence interval of 6.2-7.9 pp). Figure 3 presents this treatment effect over time showing the effect reduces from -10.9 pp in the second cycle to stabilizing near -7 pp by the sixth cycle. As a robustness check we examine the cumulative number of minimum payments and results are consistent in line with the treatment effect each month being additive.[26]

[25] Online Appendix Table A5 shows t-tests of unconditional means.

[26] The number of cumulative minimum payments are increasing fairly linearly for each new statement

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

We look at how this effect 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 would be in the trial – so the effects are limited to the card in the trial.

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 missed payments on the target card of 0.38 percentage points (95% confidence interval 0.02-0.75 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, missing payments, and outstanding debt when aggregating across the portfolio of credit cards held.

Our treatment does not appear to be reducing credit card debt at or before the seventh statement cycle (Figure 4, 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 4, Panel B). The same conclusions are reached looking across the portfolio of credit card debt in pounds (Online Appendix Figure A5).

As the cycle-by-cycle estimates on our primary measure of credit card debt (statement balance net of payments as a percent of statement balance) are persistently, slightly (but

cycle from statement cycle five onwards (cycle-by-cycle regression estimates in Online Appendix, Figure A4 and estimates from the seventh cycle in Table 5 with unconditional means in Online Appendix, Table A6).

statistically insignificantly) below zero, we check the robustness of this result in tertiary analysis by aggregating 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 (given 95% confidence intervals) as shown in Table 4. 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 (given 95% confidence interval).

Similarly, even with this pooling exercise, we find no significant effects on 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 further robustness check, we examine the cumulative number of full payments and results are consistent with stable estimates across cycles each being precisely-estimated nulls (cycle-by-cycle regression estimates in Online Appendix, Figure A4 and results from the seventh cycle in Table 5 and unconditional means in Online Appendix, Table A6).

Our null average treatment effects on debt in spite of a seemingly large, proximate change in enrollment and paying only the minimum payment is surprising. It is robust to a variety of secondary outcomes displayed in Table 5.[27] 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 spending no more on their card?

V 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 V.A.), examining heterogeneity in results (Section V.B.), and the role of liquidity constraints (Section V.C.).

[27]Unconditional means displayed in Online Appendix, Table A6.

V.A. Offsetting Consumer Responses

We find three offsetting consumer responses to the nudge make it ineffective.

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 caused a 16.7 pp increase in Autopay Fix enrollment by statement seven (the purple coefficients in Figure 5), the treatment effect on enrollment with Autopay Fix *exceeding* the minimum amount due is half the size (the pink coefficients in Figure 5): 8.6 percentage points which is a 34% increase on the control group mean (Online Appendix, Table A5). The regression estimates at cycle seven are also shown 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 (Online Appendix, Figure A6 and Table A5).

When we examine the distribution of Autopay Fix amounts chosen by the treatment group (Figure 6) 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 due (Panels B and D).[28] Pooling across all seven cycles, we find that for 48% of Autopay Fix enrollees in the treatment group, the cumulative Autopay Fix amount is £100 or less in excess of the minimum. At the other extreme, it is only over £500, for 13%. Such amounts can be evaluated relative to the mean cumulative value of repayments across these cycles in the control group: £1,277 (Table 5). 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 average £46 per month (£320

[28]We do not show the control group as the treatment causes large changes in Autopay Fix enrollment and so these two groups are not directly comparable.

cumulative across cycles 1-7).

The low Autopay Fix amounts chosen explain why the effect on debt reduction would be substantially smaller than the effect on enrollment, however, it alone does not explain why there's a null average effect. To explain the null average effect requires some negative offsetting effects.

Lower Enrollment In Any Autopay

The second offsetting effect is that the nudge initially causes one in twenty fewer consumers to be in enrolled in any type of Autopay (Table 2 shows regression estimates at cycle 7).[29]

This lower enrollment explains a slight increase in arrears (Table 2) since if enrolled in Autopay it is only possible to miss a payment if they have insufficient funds in your checking account whereas it is much easier for un-enrolled consumers to forget to make payments. 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) as displayed in Table A9: increasing missed payments by 0.4 pp with a 95% confidence interval of 0.19 to 0.62 pp.

There were no statistically significant differences in the types of consumers who were more likely to drop out as a result of the treatment (see footnote for details).[30]

The effect on arrears is solely on missing a single payment: precise zeros are estimated on missing two or three payments (Online Appendix, Table A9).[31] The treatment does not

[29] Online Appendix Figure A3 displays cycle-by-cycle regression estimates and unconditional means Figure fig:add while Table A5 shows t-tests of these at cycle 7.

[30] We conducted OLS regressions shown in Online Appendix Table A8 with one observation per card. We predicted 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, number of credit cards with debt credit file. While most of these were significant predictors of Autopay enrollment none of them were when interacted with the treatment and so do not explain this drop-out.

[31] Examining the time series of cumulative number of missed payments (Online Appendix, Figure A4) shows the treatment effect is stable after the third cycle.

lead to consumers being classified as being in more severe arrears which is often defined as being two, three (or more) 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 (Online Appendix, Table A9). It also does not appear in our primary outcome for the share of credit card portfolio missing payment (Table 3 and 4) - this is because temporary arrears are not reported in credit files.

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 spiral into a terminal state of financial distress that they cannot recover from.

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.

Figure 7 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, effect lowering manual payments (the estimates after seven cycles are shown in Table 5). We also find the treatment is more likely to cause consumers to be 1.3 pp more likely to make both an automatic and manual payment in the same cycle (Table 5).

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.[32] 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.[33]

[32]The percentages of different cuts 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) (Online Appendix Table A6); 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. Online Appendix Table A10 shows 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.

[33]54% for those enrolled in Autopay Fix or Min in the control group at cycle seven.

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 those same 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 as opposed to paying down debt (in line with (Fuentealba et al. 2021, Gathergood et al. 2021, Sakaguchi et al. 2022)).[34]

However, comparing automatic and manual payments is conflating two effects: a change in composition (Autopay enrollment) 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 understand these forces, we decompose Equation 1 by whether the consumer was enrolled in Autopay at cycle seven ($AUTOPAY_{7,i}$) as shown in Equation 3. This is a decomposition by an endogenous variable and so our estimates will suffer from bias and are

[34]In another domain, Autopay enrollment increases electricity consumption – a finding attributed to Autopay reducing the price salience (Sexton 2015).

not causal.

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

We examine the cumulative value of payments, in total and split by automatic and manual payments, by the seventh cycle across these subgroups. Figure 8 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 those enrolled in Autopay: automatic payments increase, manual payments decrease, and thus overall payments for this group are unchanged. 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 its 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 cautiously 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 is offset by lower Autopay enrollment (a composition effect) and, even among cardholders who are enrolled, they substitute higher automatic payments for lower manual payments (a substitution effect).

V.B. Heterogeneous Effects

In response to presentation feedback we performed a 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 who might have benefited from the policy. The potential gains for the most vulnerable consumers may be highest given their limited financial resources, however, the nudge may be most effective for least vulnerable consumers who can afford to pay more but do not do so for other reasons (e.g. inertia).

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 four 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 (Online Appendix Table A11).

We interpret the results of our heterogeneity as not producing clear effects. Our results are presented in Figure 9 (with estimates in Online Appendix Table A11). None of the heterogeneous groups show an effect that is statistically significant at our 0.5% threshold for significance. There are no clear effects by income. By credit score we see 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 pp whereas all other quartiles have insignificant effects. The second least vulnerable 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 pp while all other quartiles have insignificant effects.

V.C. Liquidity Constraints

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 explanation to examine 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 (Online Appendix Table A12).

In addition to being a selected subsample, we do not have sufficient power to estimate treatment effects for this group.[35] Instead, we present descriptive analysis that we consider informative for updating a Bayesian reader's priors.

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. Our second measure examines a consumer's minimum liquid balance 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 these dynamics. It records the number of intra-month 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). We use £100 as a threshold as

[35] If we had sufficient power we could evaluate a potential attractive feature of nudging as opposed to alternative, hard, paternalistic policies such as increased minimum payment requirements that the former does not force consumers who cannot afford to pay more into arrears.

not all transactions can be paid for with credit cards and therefore consumers may find it necessary to hold a positive liquid balance. This informs how volatile a consumer's finances are.

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 & Tufano 2015) and behavioral factors such as naive present focus leading to impulsive, overconsumption (e.g. Heidhues & Kőszegi 2015).

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 Online Appendix Table A18). 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 balances 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 zero or negative liquid cash balances. We also see this distribution has very fat tails (and so the mean is not well-estimated) but is stable over time with median balances near £400.

Our second dynamic measure (Panel B) reveals clear sorting of consumers into two types (distribution summarized in Online Appendix Table A18). One group of consumers has a zero or negative minimum liquid cash balance. However, 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 balances is effectively zero (£4.76) and the 75$^{\text{th}}$ percentile £142.39. This second measure reveals how liquidity constraints commonly bind for approximately half of consumers - whereas if we had only measured a point-in-time liquid balance (Panel A) it would significantly understate liquidity constraints.

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

Relationship Between Liquidity Constraints & 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 much clearer relationship when we use our measure of minimum liquid cash balances over 90 days. Here we see consumers with positive minimum liquid balances (before card opening) discontinuously repaid approximately 20pp 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 of this distribution contained in our primary outcomes: payments of 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 decrease in likelihood of missing a payment.[36] Paying only the minimum becomes less likely among less liquidity constrained consumers, however,

[36]See Online Appendix Figure A9. The relationship with Autopay choices, shown in Online Appendix Figure A8,is less clear except for a discontinuous increase in Autopay Full enrollment.

there is a less clear discontinuity around zero.

Finally, Panel F 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 not reducing their debt due to frequently experiencing binding liquidity constraints. These liquidity constraints indicate why we observe no distal effects to the nudge: consumers are often making offsetting responses to address their constraints. These constraints *may* also mean other interventions fail.

Our research may also help to understand why some consumers may appear to simultaneously co-hold high-interest debt and low-interest liquid cash: because consumers' liquidity constraints bind over relatively short time periods. We interpret our empirical results as appearing most in line with Telyukova (2013)'s structural model based on US survey data that showed the importance of liquidity constraints in understanding credit card repayment behaviors.

VI Concluding Discussion

We have shown how a nudge has large proximate effects on consumer choices but no distal effects on real outcomes. We found that changing the menu of Autopay options at credit card sign up has large proximate effects on the Autopay consumers select but no distal effects on their level of credit card debt. This highlights how important it is to evaluate nudges, along with other public policy interventions, on their distal effects on real economic outcomes.[37] Otherwise policymakers could introduce policies that appear effective but are actually ineffective and costly. Conducting ex-ante tests of potential policies enables policymakers to

[37]Subsequent work by Saccardo et al. (2022) in other domains provides further evidence of this contrast in the effectiveness of nudges depending on whether these are examined on their proximate and distal effects.

find this out before such costs are imposed on firms and consumers.

Our paper shows the challenge of using nudges due to offsetting consumer responses. With nudges less effective than previously thought there is a role to focus on examining the trade-offs of hard, paternalistic interventions (e.g. Campbell 2016, Loewenstein & Chater 2017, Laibson 2020, Chater & Loewenstein 2022). For example, in the case of credit cards, requiring higher minimum payments may harm some liquidity-constrained consumers but help unconstrained consumers who are making behavioral mistakes, creating net effects that are ambiguous.

Finally, our work highlights the importance of using granular, linked liquid savings data with credit data to consider the dynamics of liquidity constraints for understanding credit card repayment behavior. A static, point-in-time measure would imply 10% of consumers experience binding liquidity constraints in our linked data. However, once dynamics are considered, liquidity constraints actually bind for half of consumers in our linked data.

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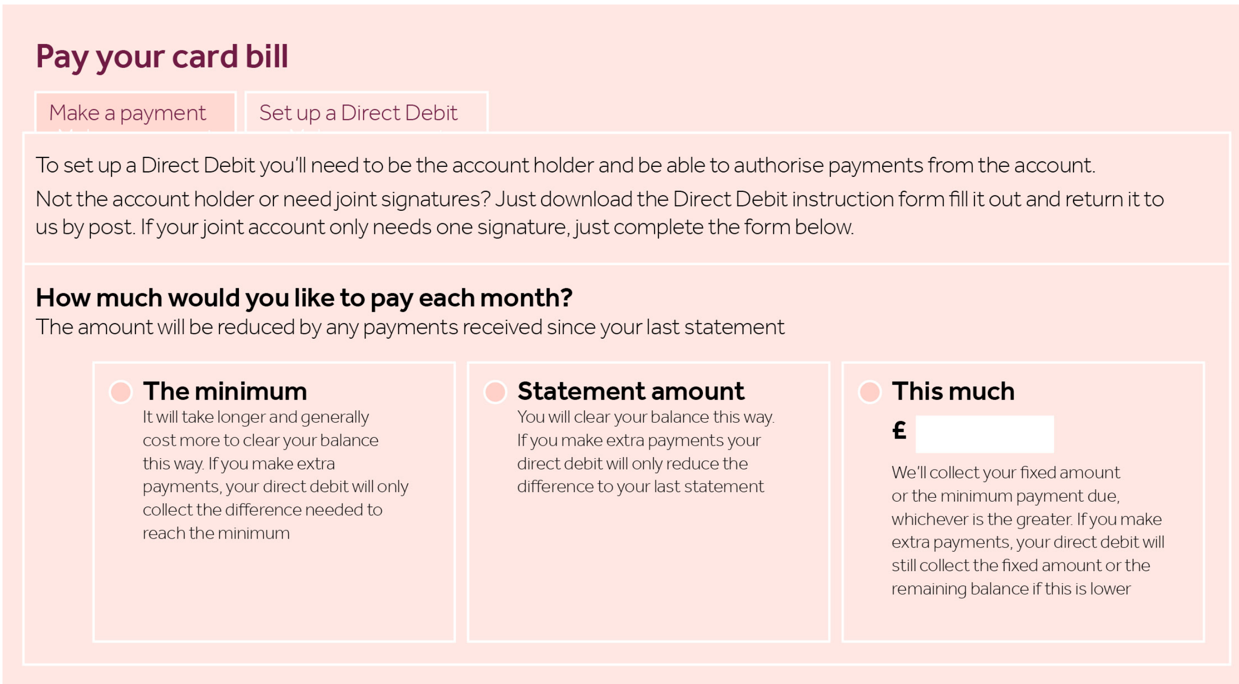
VIII Figures & Tables

Figure 1: Automatic payment choice architecture presented to control (panel A) and treatment (panel B) groups

A: Control



B: Treatment

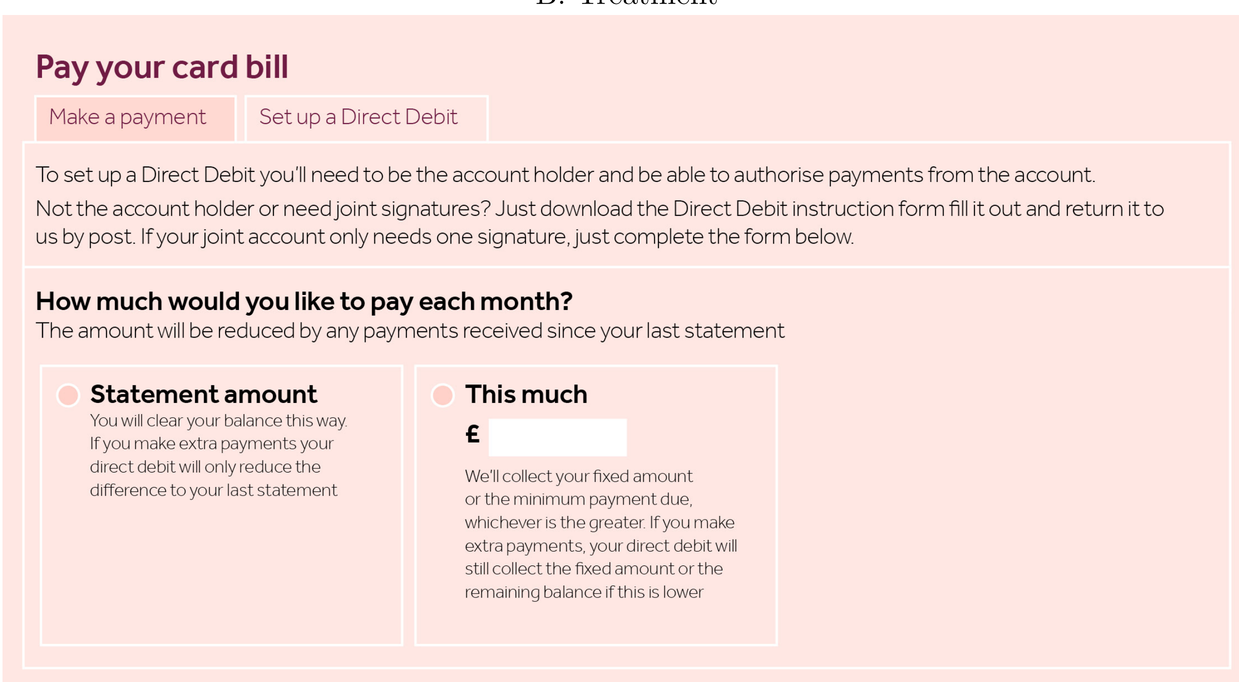
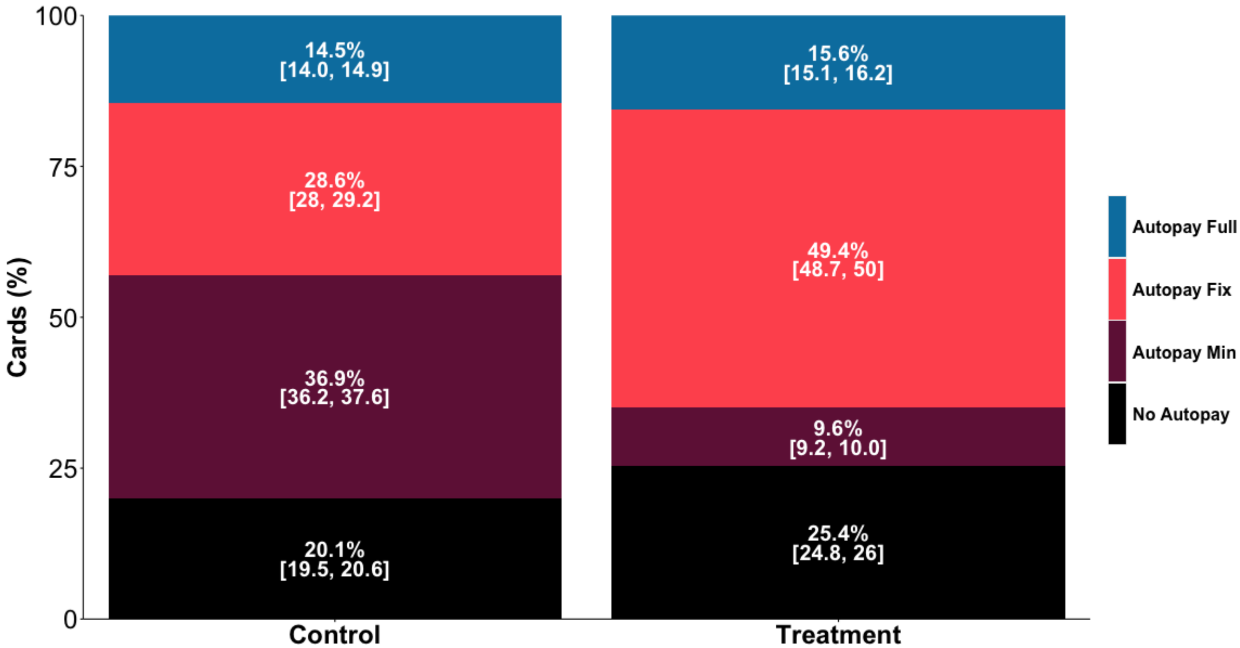
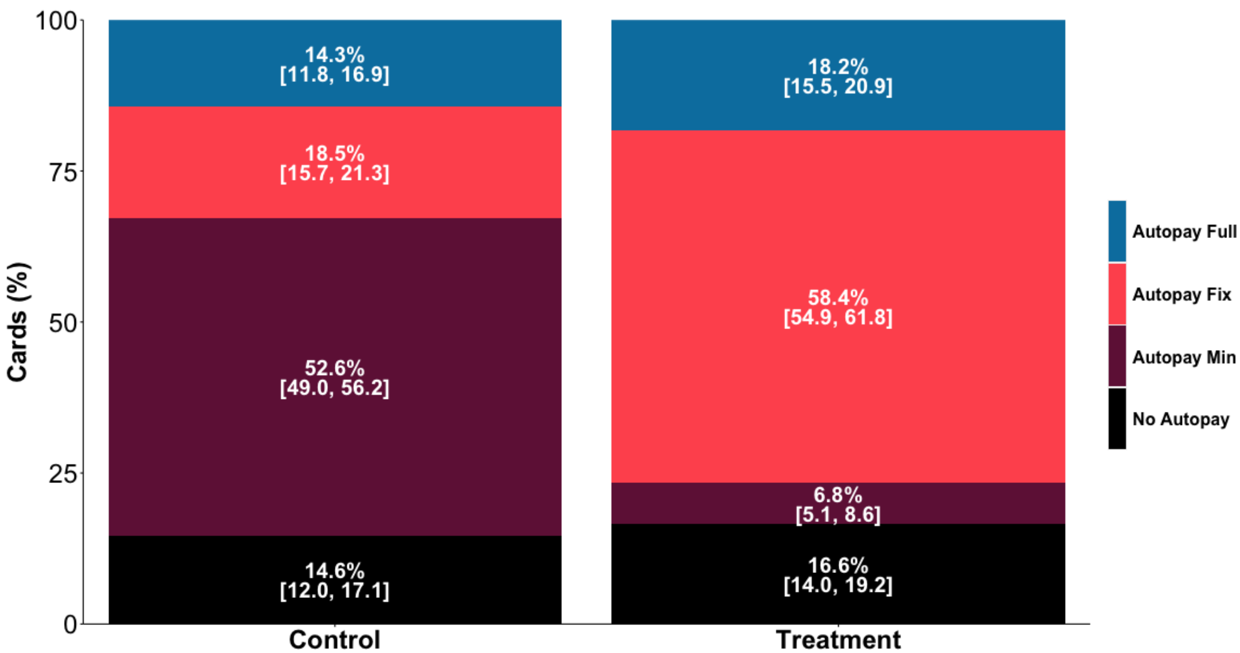


Figure 2: Automatic payment enrollment for control and treatment groups after two statements, split by lender

B: MAIN LENDER (N = 40,708)

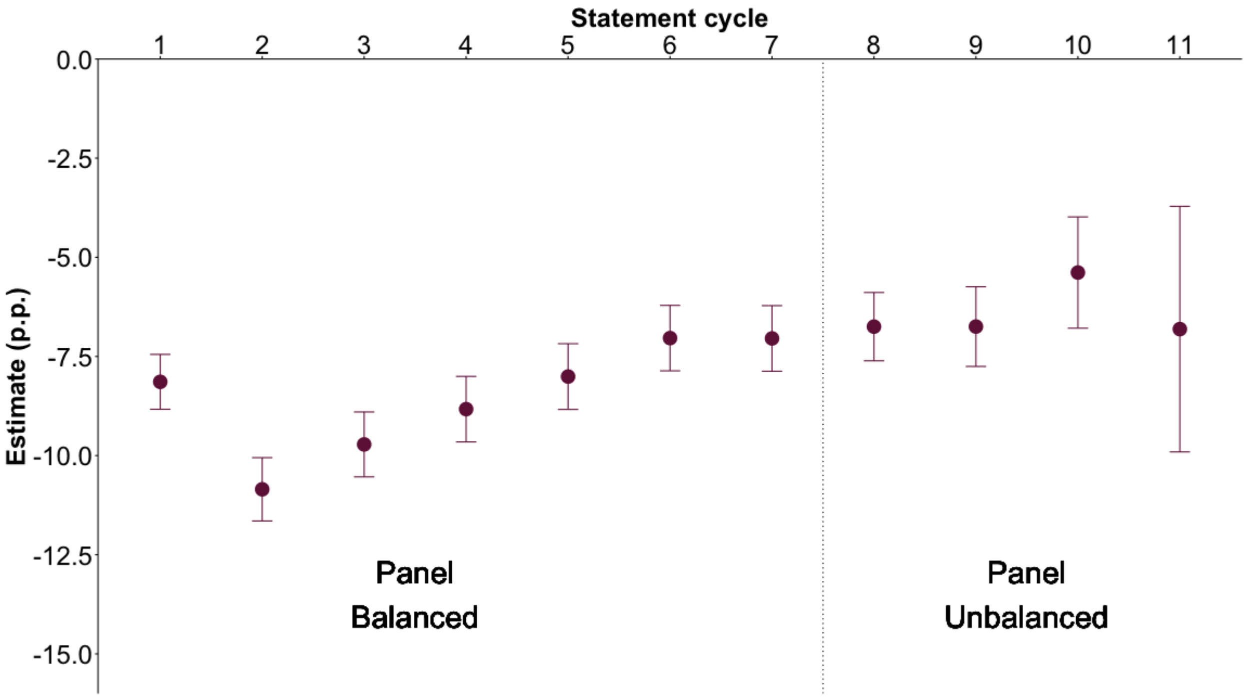


B: SECOND LENDER (N = 1,531)



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

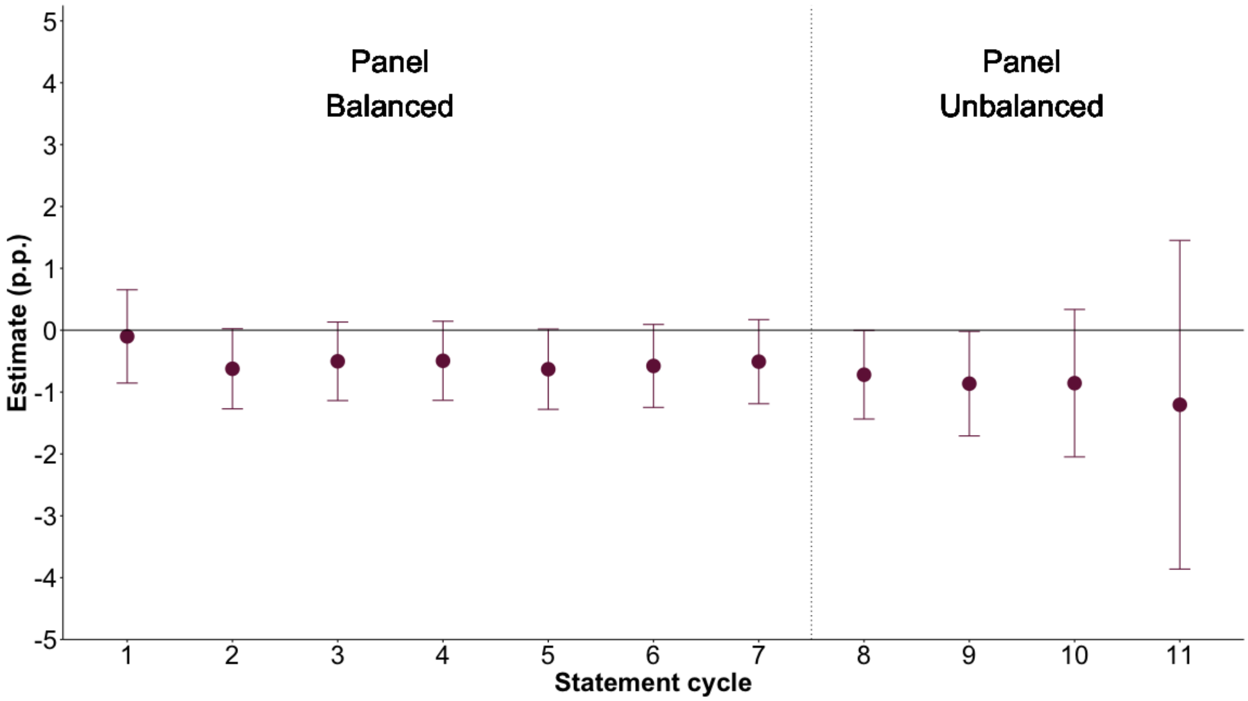
Figure 3: Average treatment effects on making only a minimum payment across 1-11 statement cycles



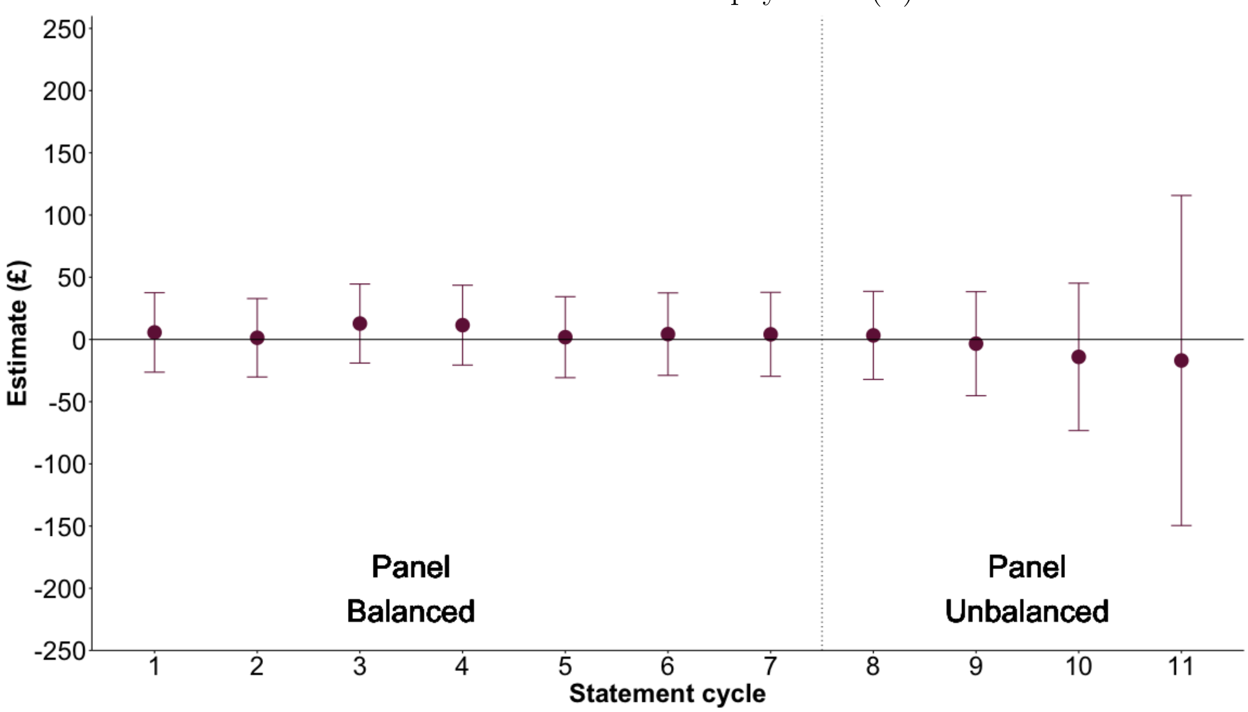
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure 4: Average treatment effects on credit card debt across 1-11 statement cycles

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

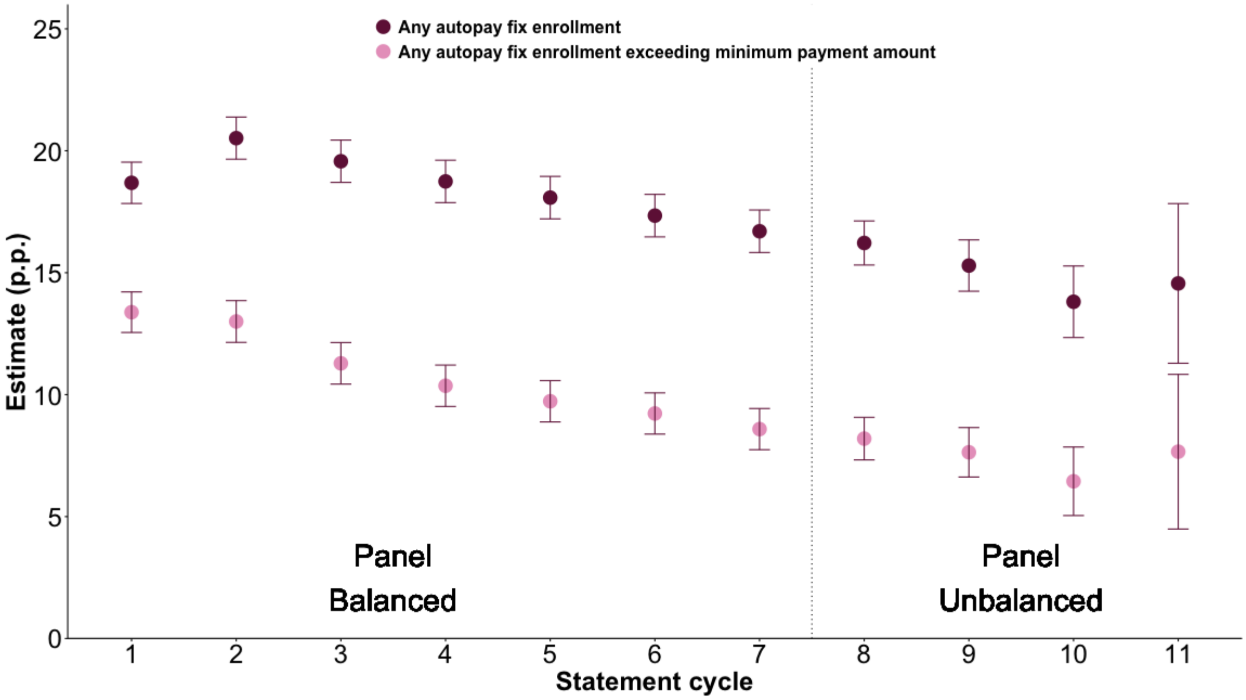


B: Statement balance net of payments (£)



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.

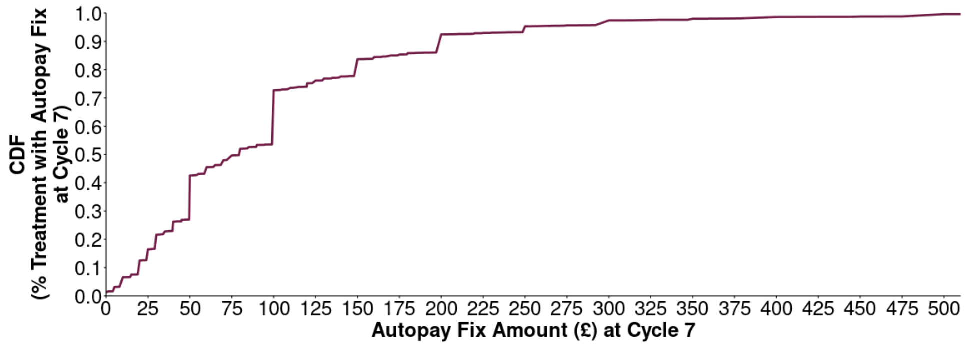
Figure 5: Average treatment effects on automatic fixed enrollment (purple) and enrollment not binding at minimum payment (pink) across 1-11 statement cycles



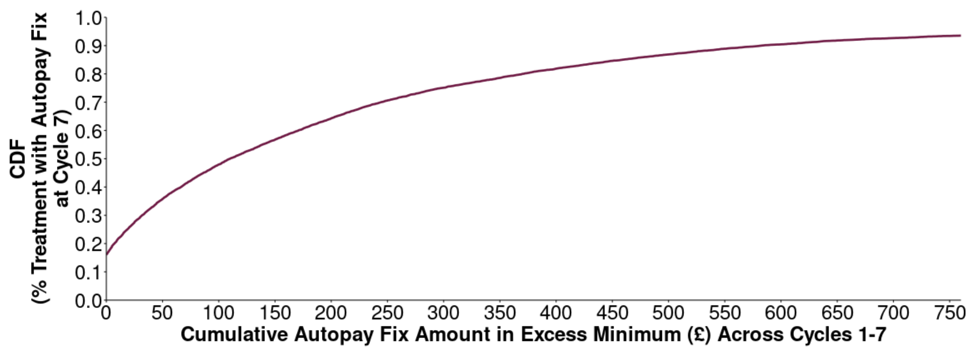
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure 6: CDF of autopay fix payment amounts for those enrolled in autopay fix in the treatment group after seven statements

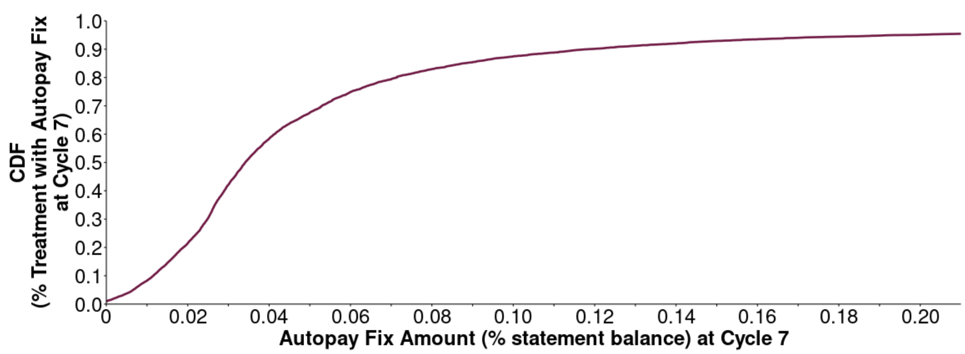
A: Autopay fix amount (£) at cycle 7



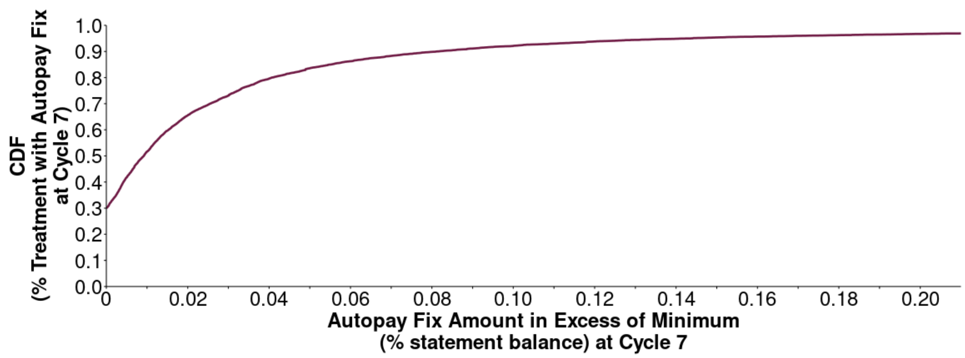
B: Cumulative autopay fix amount in excess of minimum (£) across cycles 1-7



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

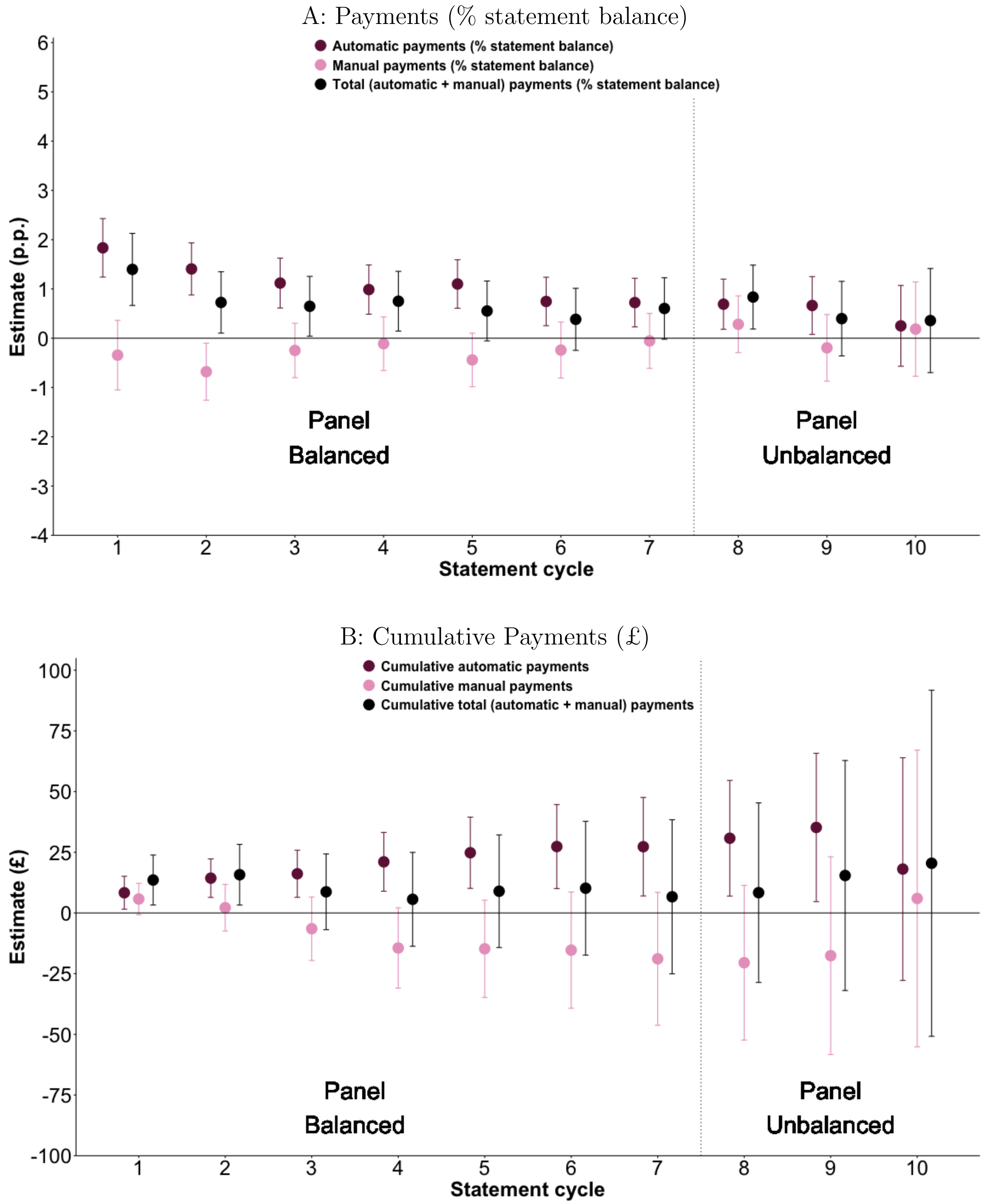


D: Autopay fix amount in excess of minimum (% statement balance) at cycle 7



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

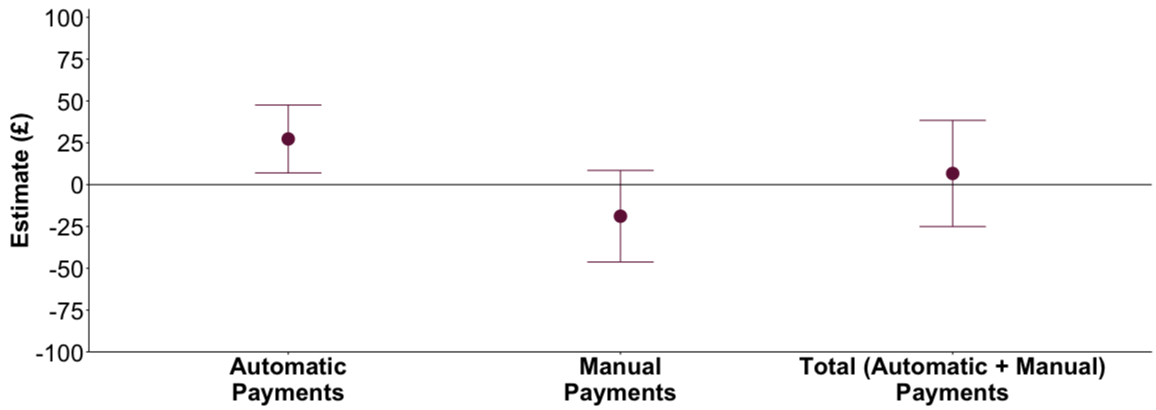
Figure 7: Average treatment effects on automatic, manual and total (automatic + manual) payments across 1-10 statement cycles



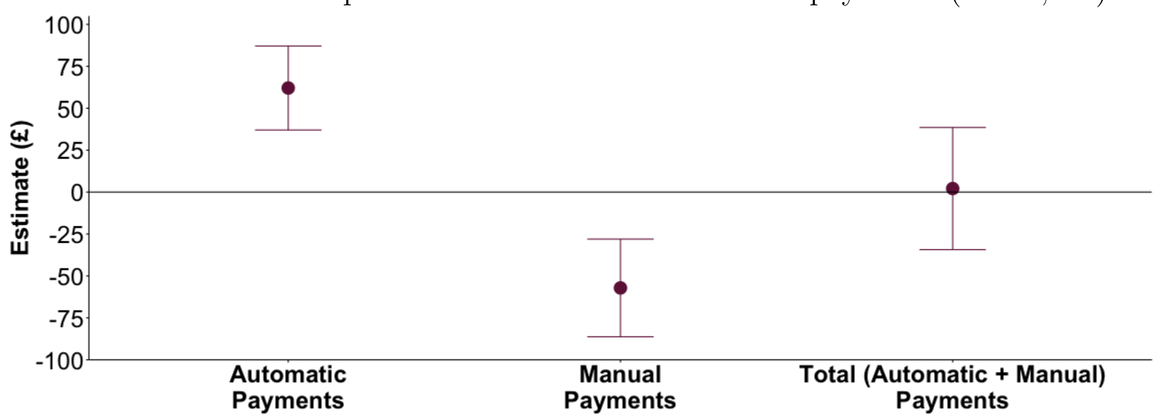
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals on Panel B are extremely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

Figure 8: Estimates on cumulative payments decomposed by automatic payment enrollment after seven statement cycles

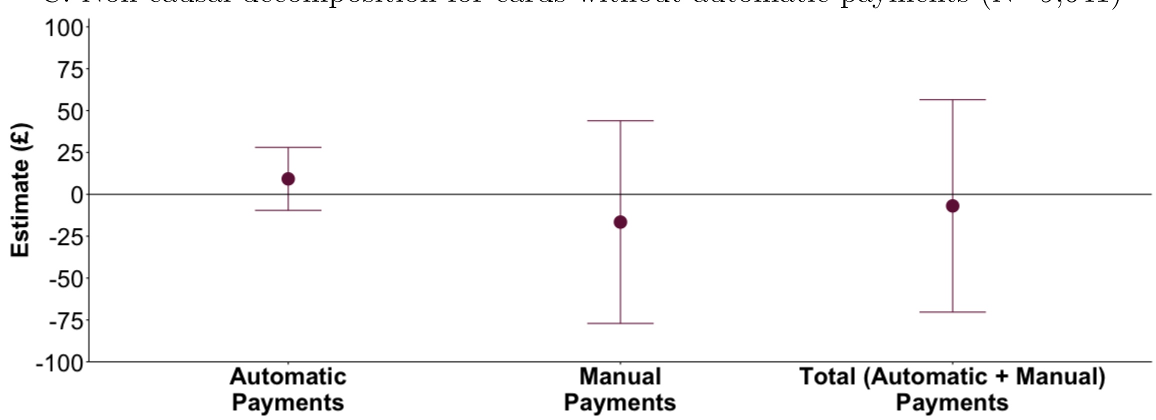
A: Causal estimate for all cards (N=40,693)



B: Non-causal decomposition for cards with automatic payments (N=31,052)



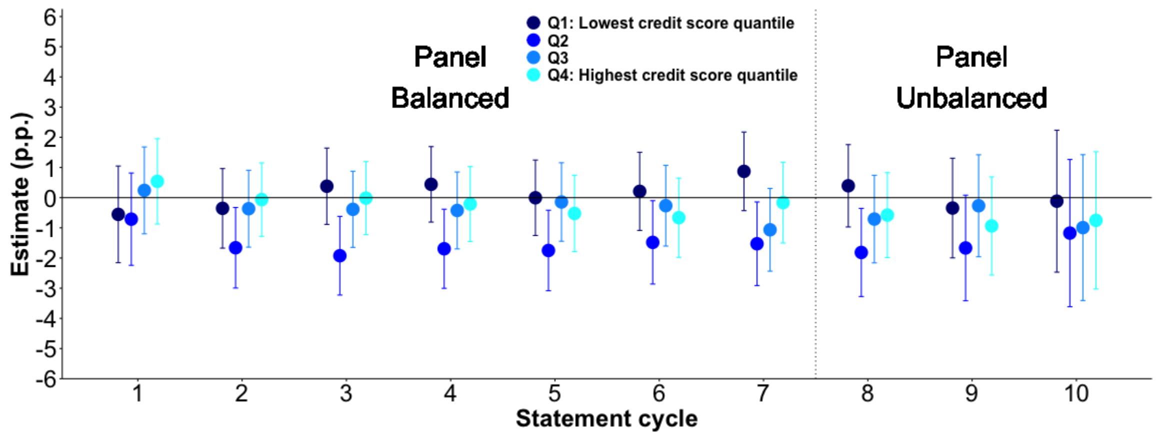
C: Non-causal decomposition for cards without automatic payments (N=9,641)



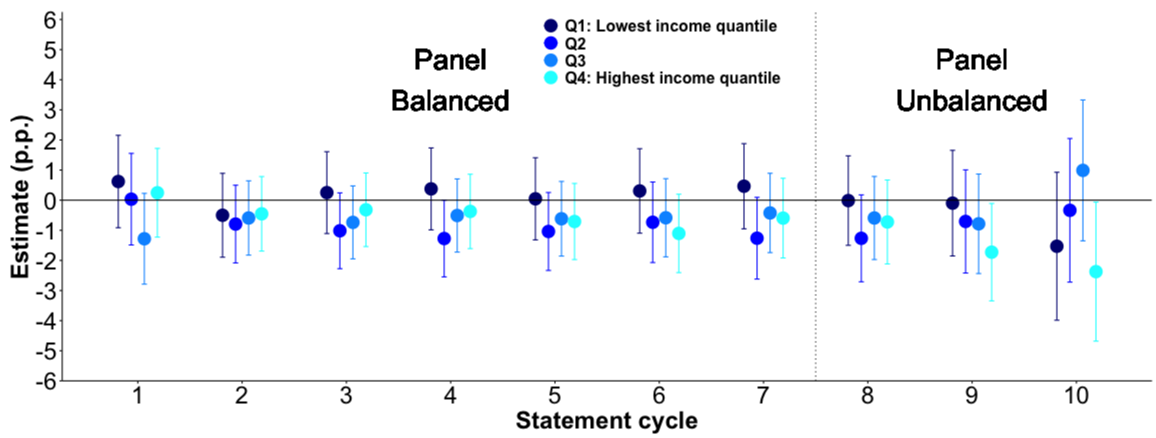
Notes: Panel A is causal estimated treatment effects from coefficients (δ_7) in OLS regression specified in Equation 1. Panels B and C show non-causal estimates (δ_7) from OLS regression specified in Equation 3. Standard errors clustered at consumer-level. Error bars are 95% confidence intervals.

Figure 9: Heterogeneous treatment effects by quartiles of (A) credit score, (B) income and (C) unsecured debt-to-income (DTI) ratio on credit card debt (statement balance net of payments % statement balance) across 1-10 statement cycles

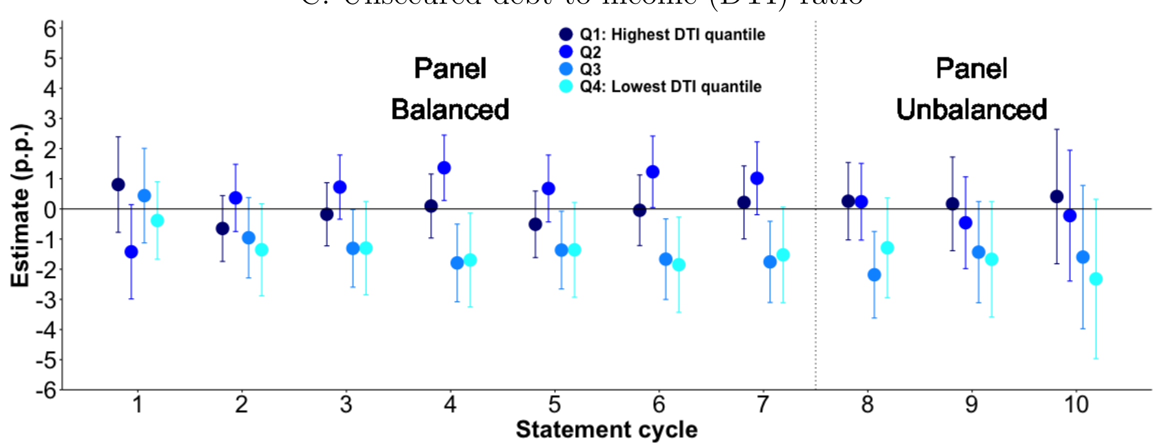
A: Credit Score



B: Income



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

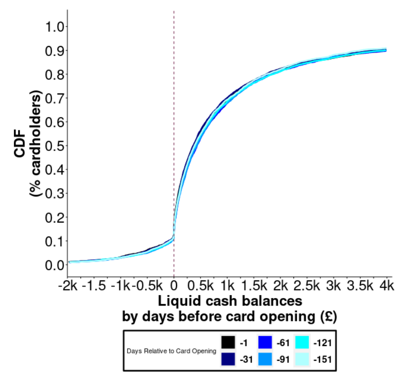


Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Heterogeneous variables calculated from credit file data in month preceding credit card opening (& trial start). Cycle 11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

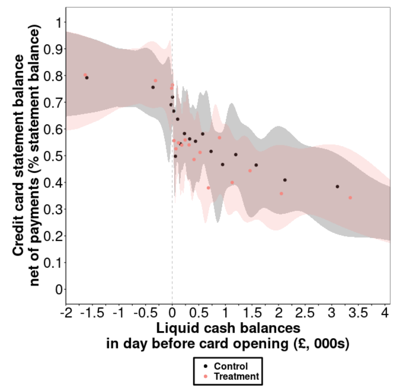
Figure 10: CDFs of liquidity constraints measured before card opening (left hand side panels) and their non-parametric relationships with credit card debt (statement balance net of payments as a % of statement balance) at statement cycle 7, by treatment group (right hand side panels)

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

A CDF

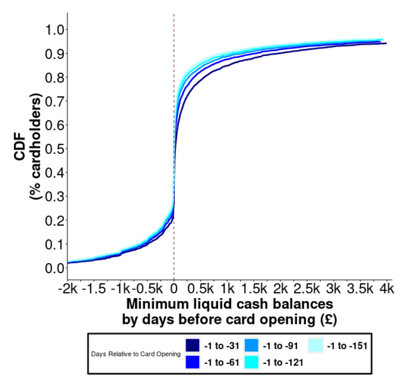


D. Relationship

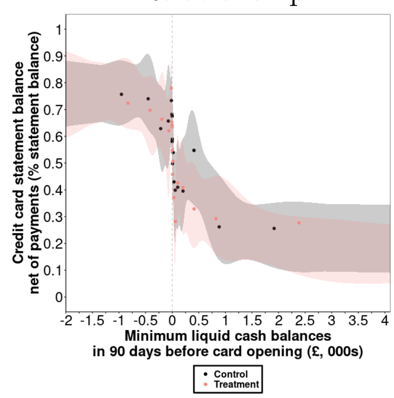


II: Minimum liquid cash balance reached during 90 days before card opening (£)

B. CDF

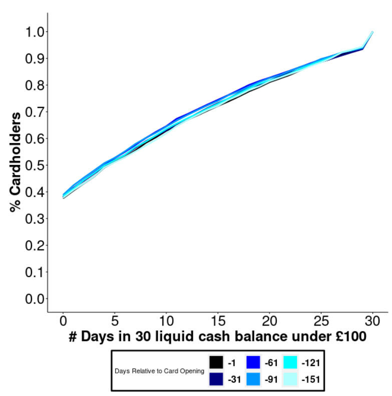


E. Relationship

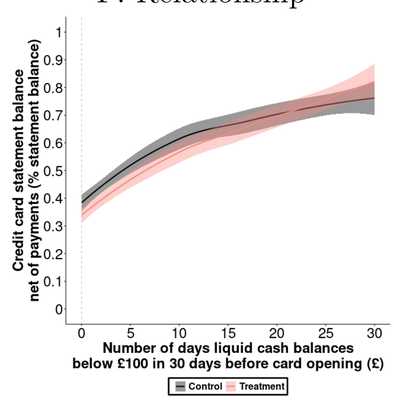


III: Number of days liquid cash balance below £100 in 30 days before card opening (# days)

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

Table 2: Average treatment effects for automatic payment enrollment outcomes after seven statement cycles

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

*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 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 5: 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.*

IX Online Appendix

Accompanying "The Semblance of Success in Nudging Consumers to Pay Down Credit Card Debt" by Benedict Guttman-Kenney, Paul Adams, Stefan Hunt, David Laibson and Neil Stewart

Table A1: 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 A2: 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 A3: 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 A4: 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 A5: Unconditional mean comparison of treatment effects for automatic payment 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 A6: 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 A7: 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 A8: 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)

$^{***}p < 0.001$; $^{**}p < 0.005$; $^{*}p < 0.01$

*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 A9: 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
Arrears 1+ payments behind	0.0040*** (0.0011)	[0.0019, 0.0062]	0.0002	0.0297
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

*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 A10: 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 A11: 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 A12: 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 A13: 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 A14: 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 A15: Second Lender: Unconditional mean comparison of treatment effects for automatic payment 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 A16: 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∗∗∗	[-0.1962,	0.0000	0.3160
	(0.0215)	-0.1119]		
Any full payment	0.0223	[-0.0207,	0.3092	0.2503
	(0.0219)	0.0653]		
Any missed payment	0.0089	[-0.0244,	0.6011	0.1176
	(0.0170)	0.0421]		
Statement balance net of payments	-0.0351	[-0.0753,	0.0874	0.6753
(% statement balance)	(0.0205)	0.0051]		
Costs	-0.0089∗	[-0.0168,	0.0276	0.0391
(% statement balance)	(0.0040)	-0.0010]		
Transactions	0.0122	[-0.0241,	0.5113	0.2245
(% statement balance)	(0.0185)	0.0485]		
Share of credit card portfolio	-0.0814∗∗∗	[-0.1080,	0.0000	0.2016
only paying minimum	(0.0136)	-0.0549]		
Share of credit card portfolio	0.0089	[-0.0278,	0.6342	0.3455
making full payment	(0.0187)	0.0456]		
Share of credit card portfolio	0.0120	[-0.0123,	0.3315	0.0904
missing payment	(0.0124)	0.0363]		
Credit card portfolio balances	-0.0274	[-0.0627,	0.1276	0.7281
net of payments (% statement balances)	(0.0180)	0.0078]		

*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 A17: Second Lender: Average treatment effects for automatic payment 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 A18: 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

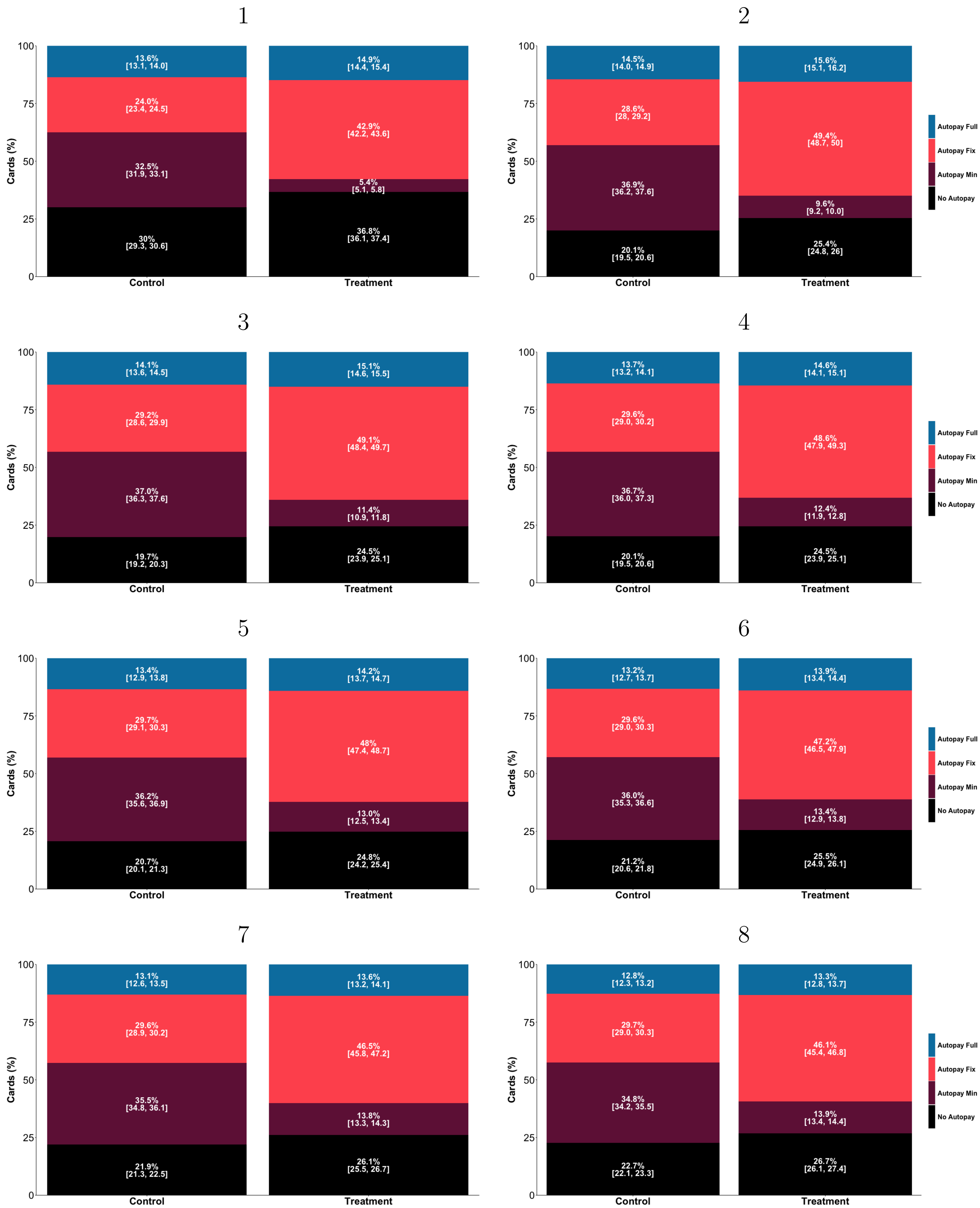
Notes: N = 3,753 consumers. Liquid cash balance is sum of end of day current/checking account and cash saving accounts balances.

Table A19: Summary statistics on minimum liquid cash balances over windows preceeding credit card opening

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

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

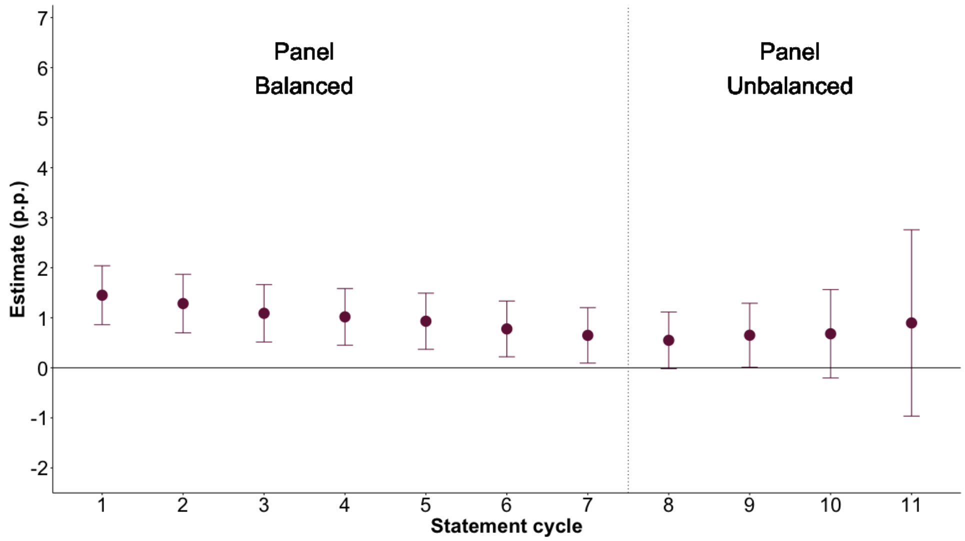
Figure A1: Automatic payment enrollment for control and treatment groups split by statement cycles one to eight



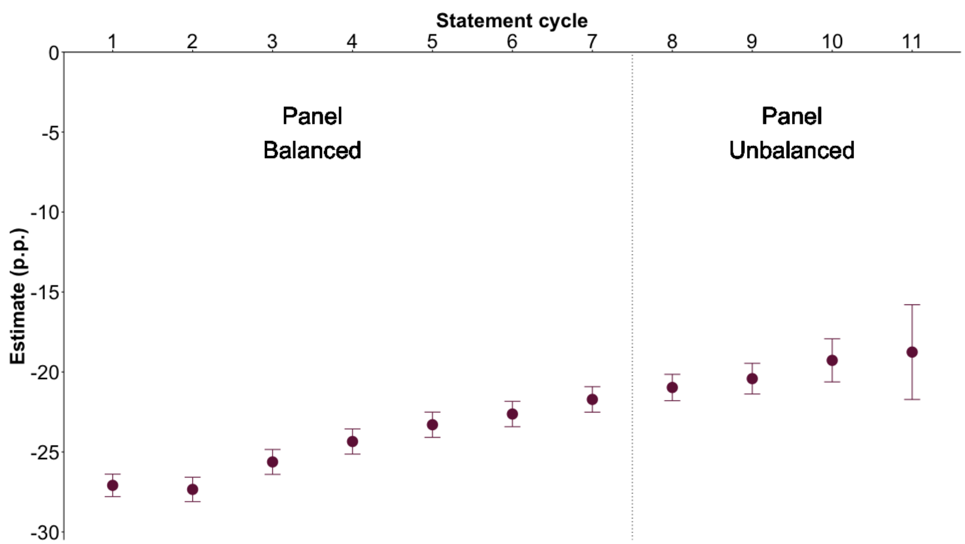
Numbers display percentage of cards enrolled in each type of automatic payment. 95% confidence intervals in [ ]. Cycle 1 is before all treated cards have had 30 days to experience the treatment. Not all cards are observed in cycle 8.

Figure A2: Average treatment effects on automatic full (panel A) and minimum (panel B) payment enrollments across 1-11 statement cycles

A: Automatic Full Payment

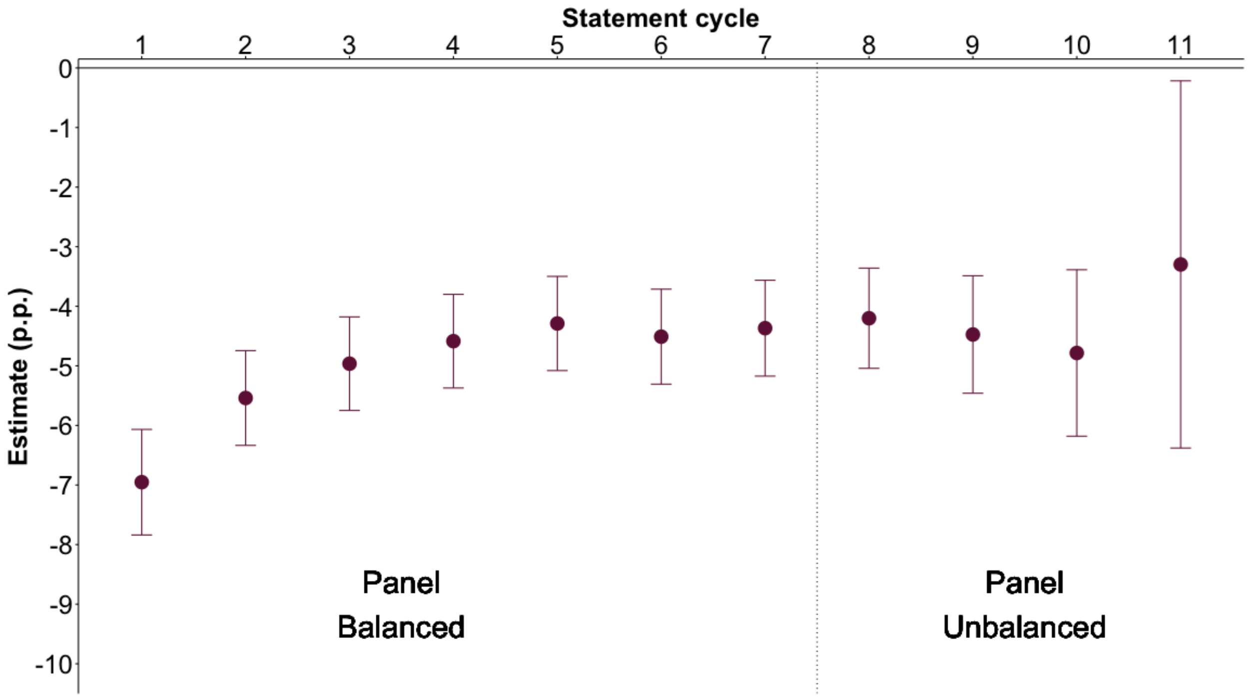


B: Automatic Minimum Payment



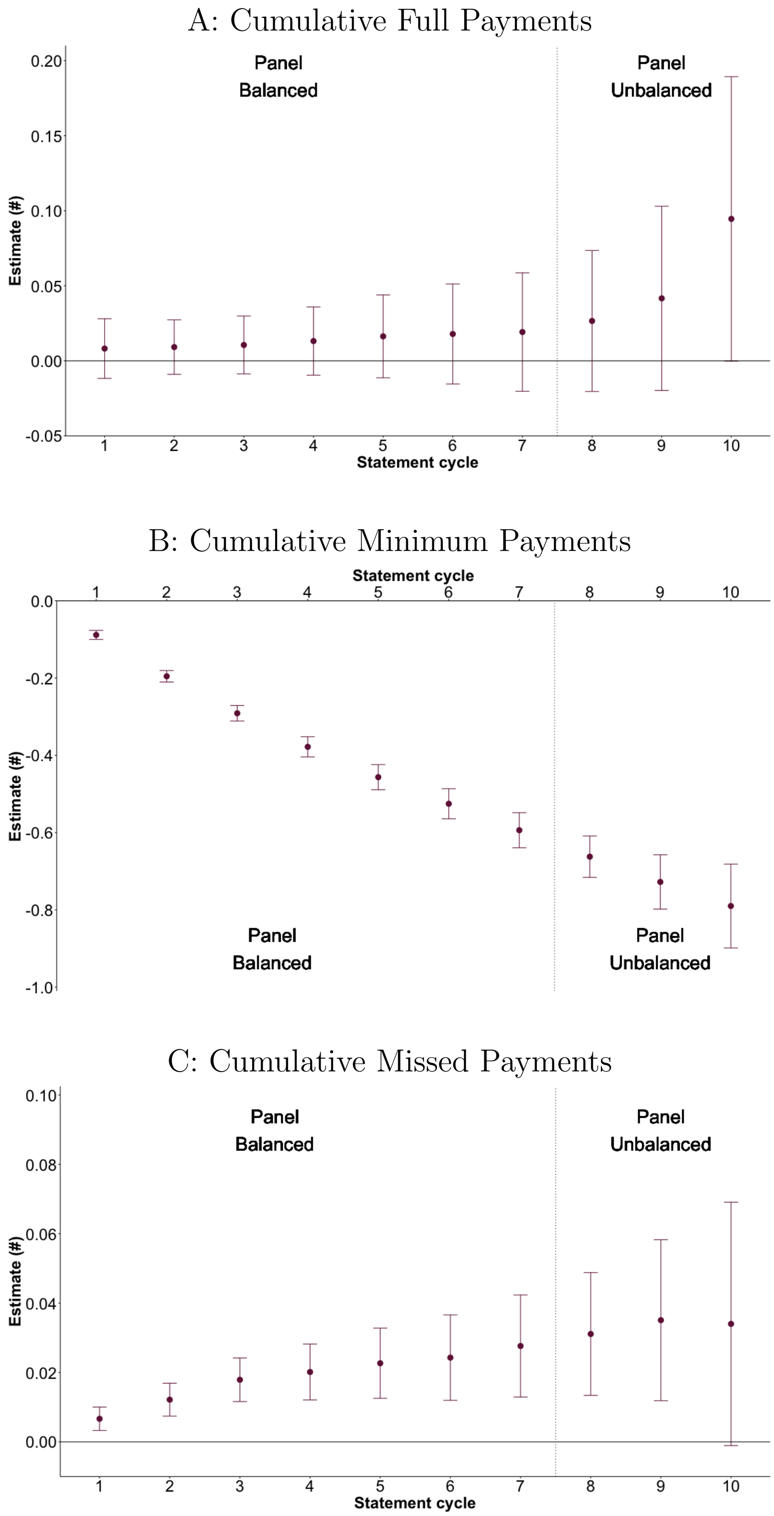
Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure A3: Average treatment effects on any automatic payment enrollment across 1-11 statement cycles



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.

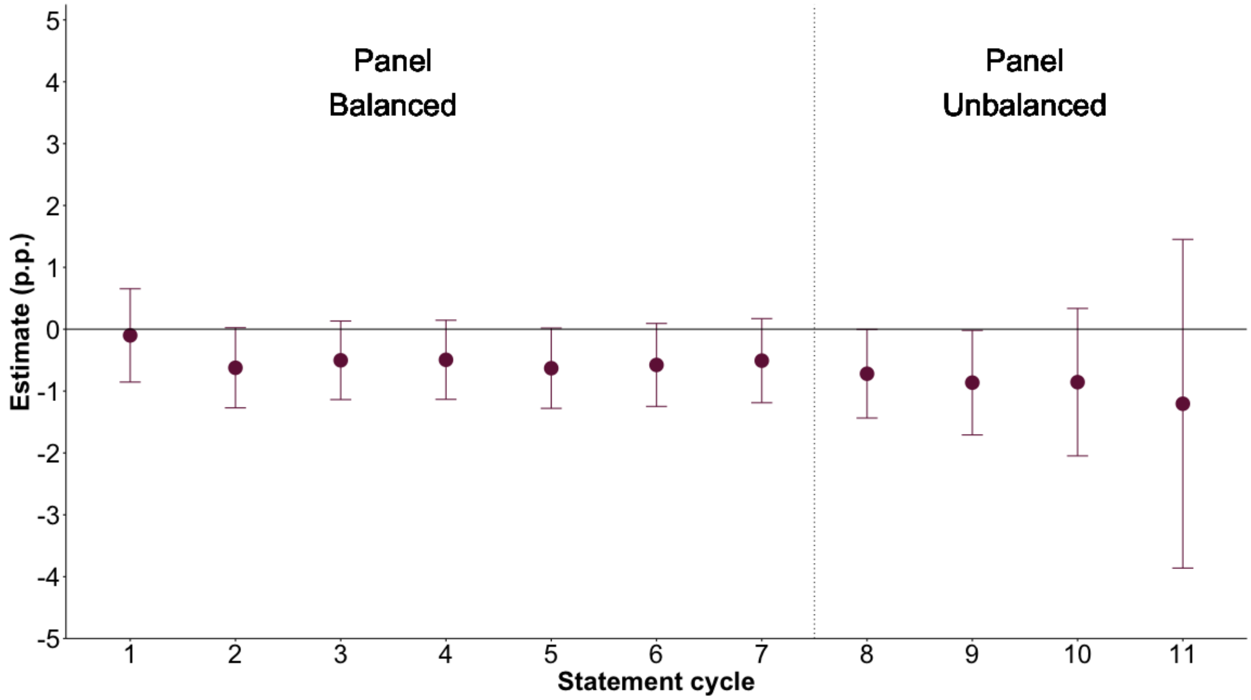
Figure A4: Treatment effects on cumulative number of full, minimum and missed payments across 1-10 statement cycles



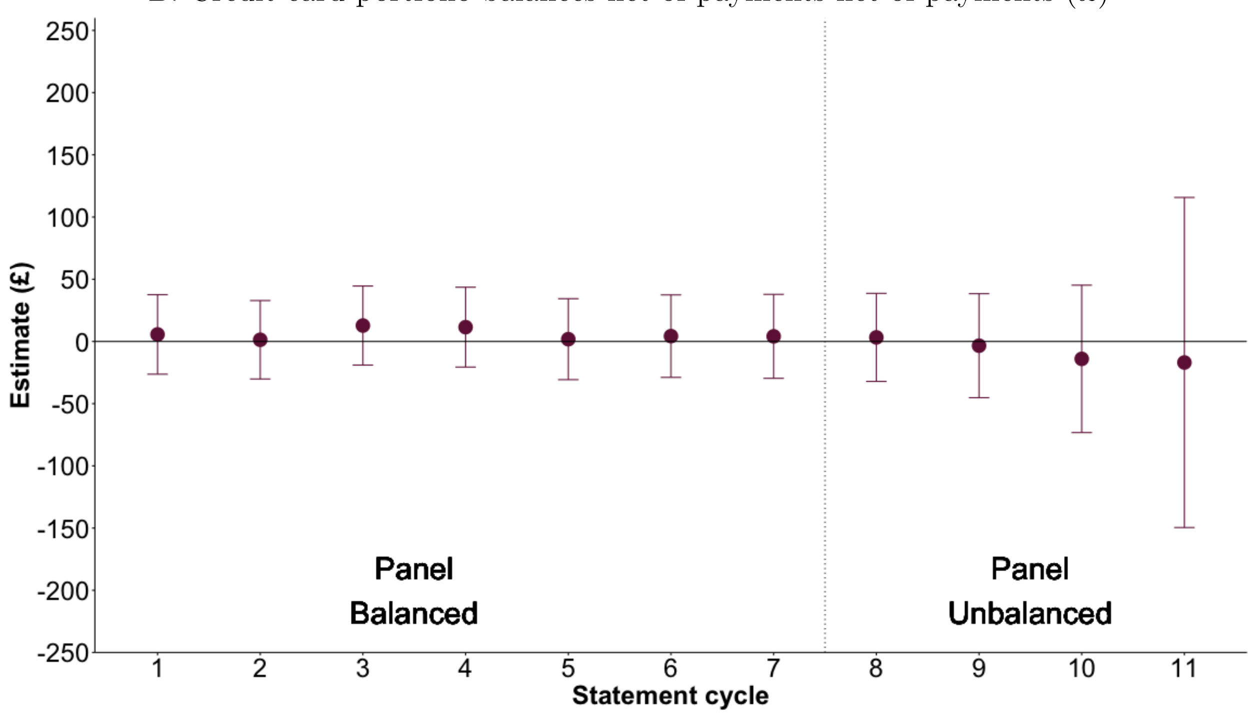
Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that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

Figure A5: Average treatment effects on credit card portfolio debt across 1-11 statement cycles

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

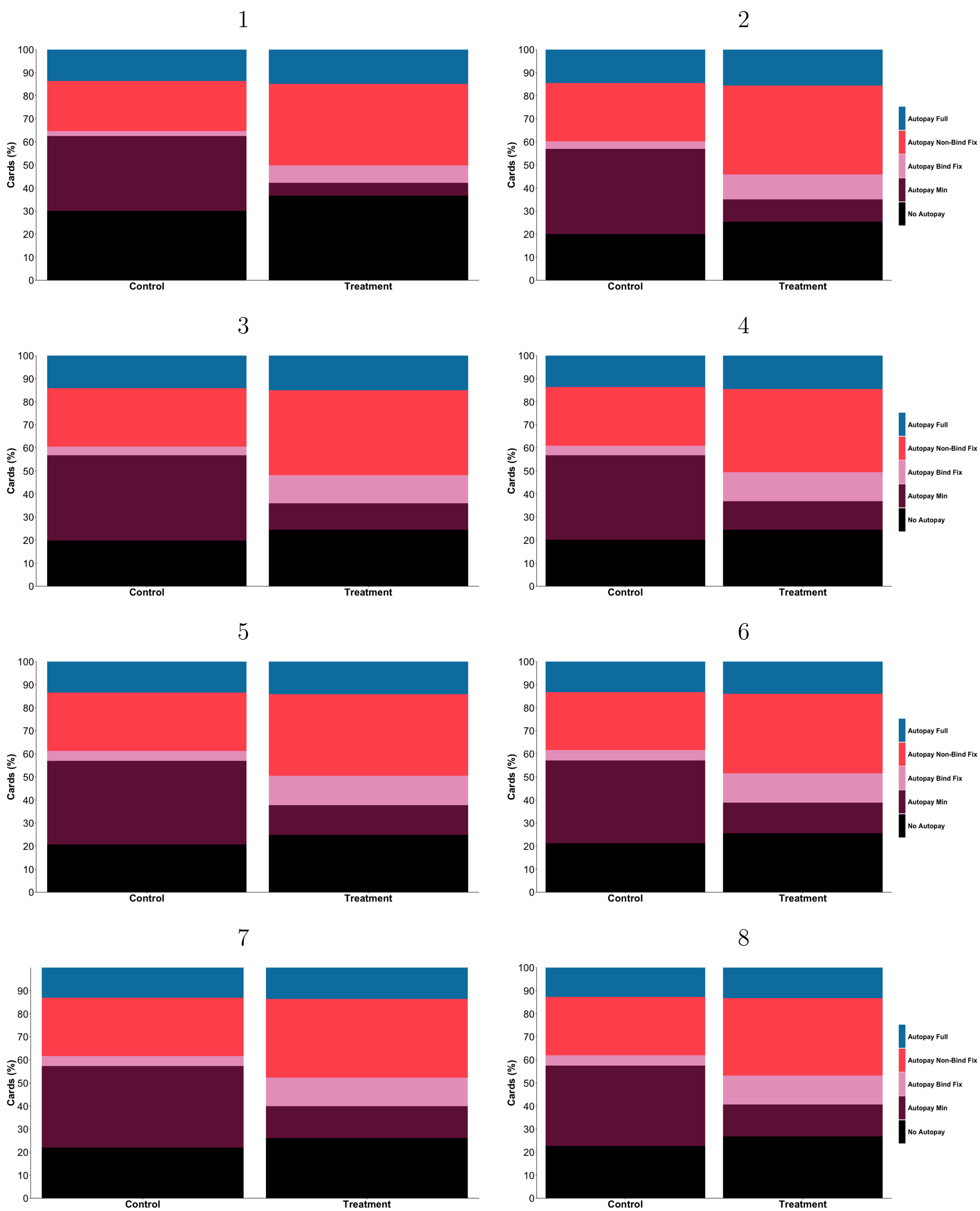


B: Credit card portfolio balances net of payments net of payments (£)



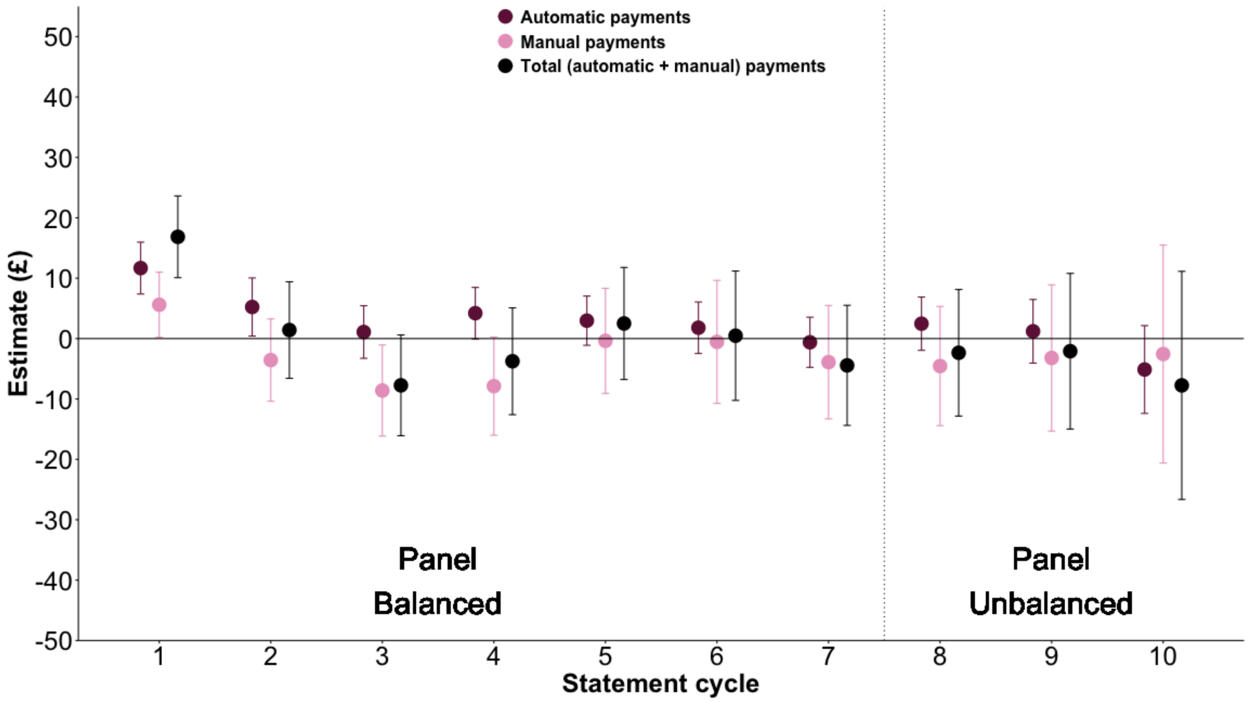
Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure A6: Automatic payment 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



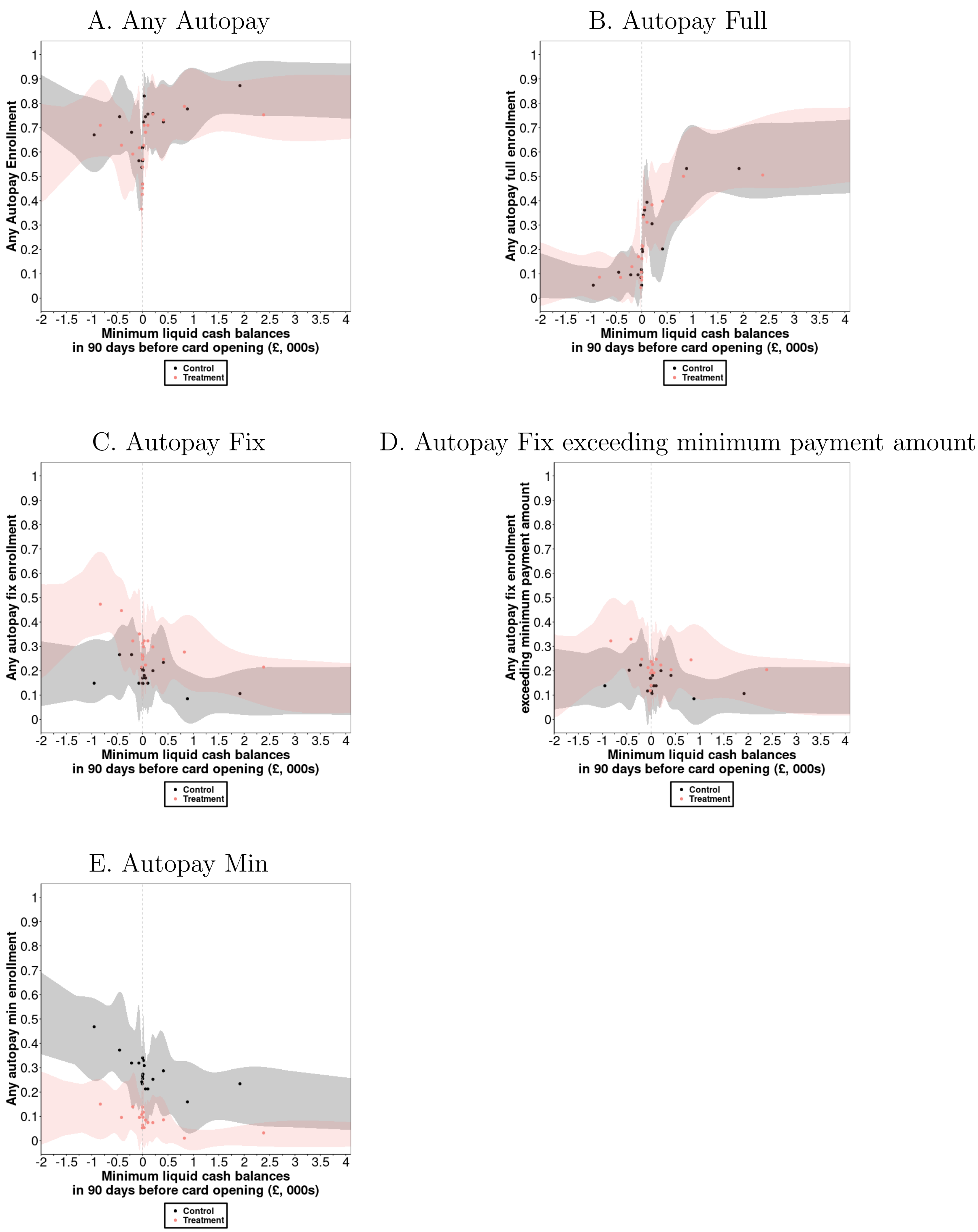
Numbers display percentage of cards enrolled in each type of automatic payment. 95% confidence intervals in [ ].

Figure A7: Average treatment effects on automatic, manual and total (automatic + manual) payments across 1-10 statement cycles



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.

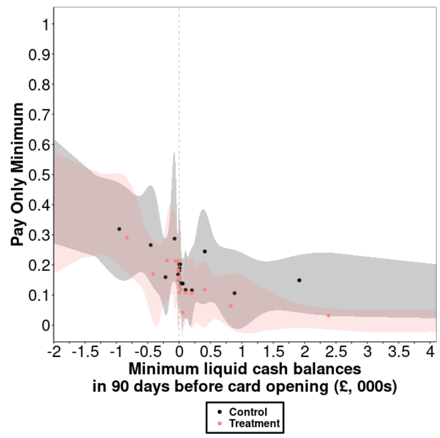
Figure A8: Non-parametric relationship between minimum liquid cash balance during 90 days before card opening with credit card Autopay enrollments at statement cycle 7, by treatment group



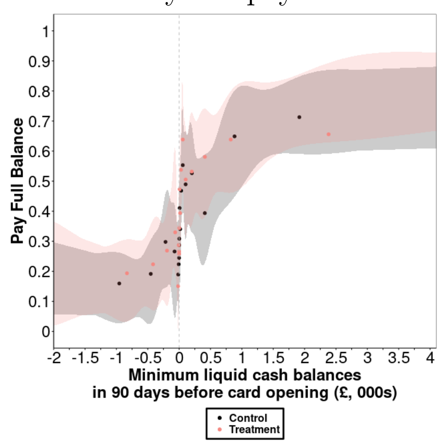
Notes: N = 3,753 consumers. Liquidity constraints are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.

Figure A9: 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

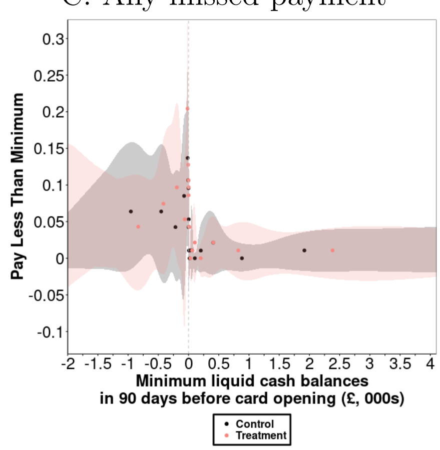
A. Any minimum payment



B. Any full payment

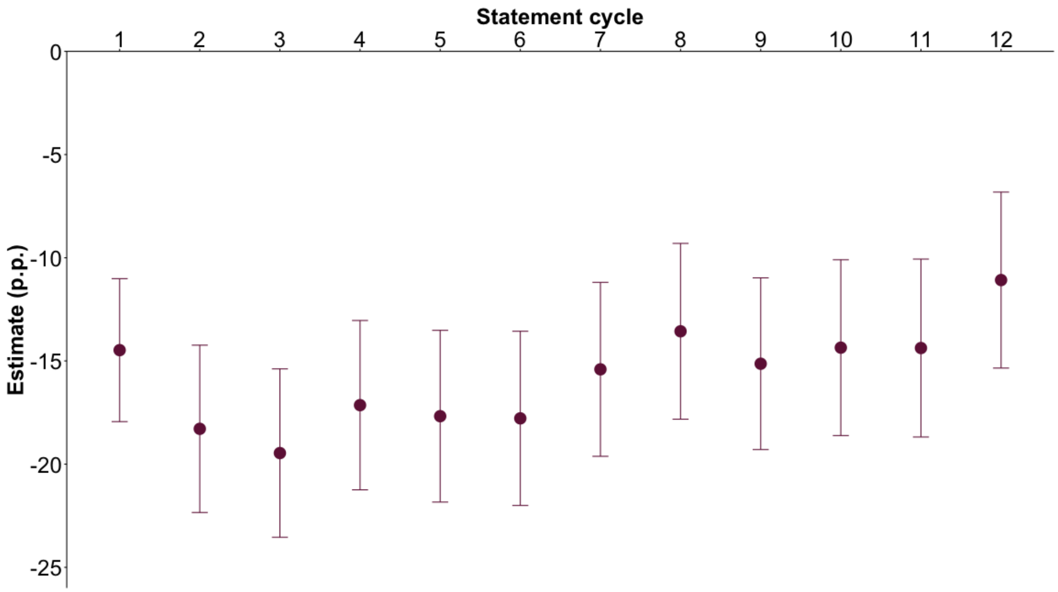


C. Any missed payment



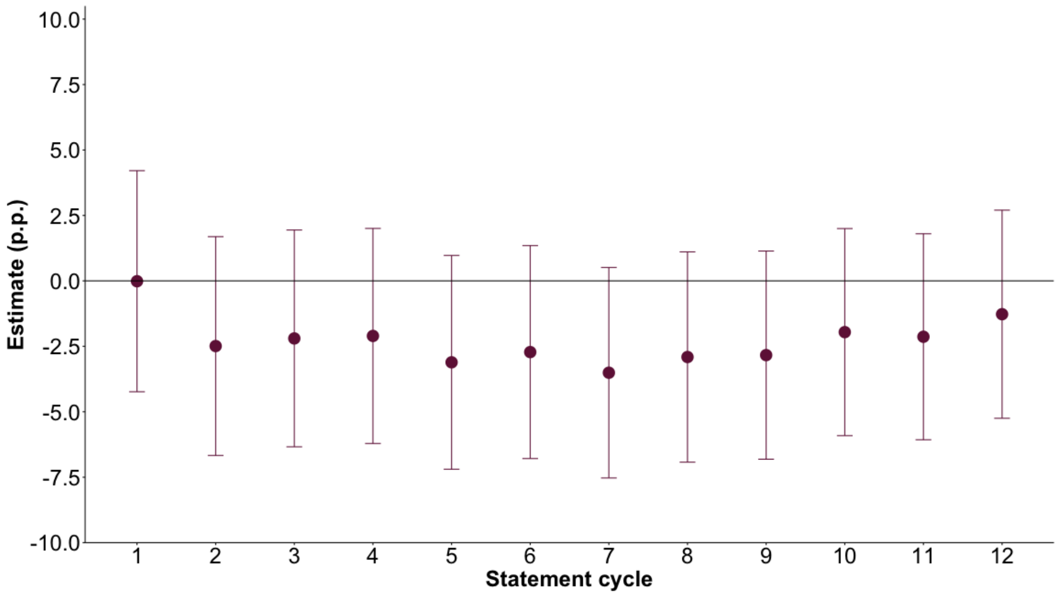
Notes: N = 3,753 consumers. Liquidity constraints are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.

Figure A10: SECOND LENDER - Average treatment effects on making only a minimum payment across 1-12 statement cycles



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 A11: SECOND LENDER - Average treatment effects on credit card debt across 1-12 statement cycles
where credit card debt is measured by primary outcome measure: statement balance net of payments (% statement balance)



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.