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

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Abstract

We show a nudge causing large proximate effects on consumer choices did not translate into distal effects on real economic outcomes. Our field experiment finds a credit card nudge – shrouding the Autopay option to pay only the minimum due and increasing the salience of an alternative Autopay option to automatically amortize debt faster – has large proximate effects on Autopay enrollment but no distal effects on reducing debt. This contrast between proximate and distal outcomes is explained by liquidity constraints and three offsetting consumer responses: choosing 'low' Autopay amounts, lower manual payments, and lower Autopay enrollment increasing missed payments.

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

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I Introduction

How choices are presented to consumers can change their financial decisions (Sunstein and Thaler, 2008). Financial firms' incentives are to present choices steering consumers towards decisions maximizing firm profits but these can be sub-optimal for consumers.[1] Consumer financial protection regulators can attempt to improve consumer outcomes using 'non-price interventions' (Bernheim and Taubinsky, 2018) / 'nudges' (Sunstein and Thaler, 2008) changing *how* choices are presented *without* restricting choices or significantly changing incentives.

We conduct a field experiment testing a nudge that changed how credit cardholders are presented with automatic payment (known as 'Autopay' in the US or 'Direct Debit' in the UK) enrollment options. This nudge has large, proximate effects on consumer choices (e.g. enrollments) but null, distal effects on real economic outcomes (e.g. credit card debt). We explain contrast between proximate and distal effects occurs because consumers experience frequently-binding liquidity constraints and make offsetting responses to the nudge.

Consumers may enroll in Autopay for convenience: providing insurance against forgetting to pay a bill. Yet Autopay means credit cardholders no longer need to actively decide each month how much to pay and may become inattentive to their debt & procrastinate on paying it down. Autopay is used by 42% UK cards (FCA, 2016a) and 20-38% US cards (CFPB, 2021) with growing use over time.[2] Our study can be more broadly insightful since Autopay is a common financial technology used for payments on non-financial (e.g. cell phones) and financial (e.g. autos, mortgages) products.[3] Some FinTech products such as buy now, pay

[1]A large empirical literature shows consumers making mistaken household financial decisions: decisions inconsistent with traditional economic models of optimizing behavior as reviewed in Campbell (2006); Campbell et al. (2011); Campbell (2016); Beshears et al. (2018); Gomes et al. (2021).

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

[3]Sexton (2015) finds Autopay increases electricity consumption.

later (Guttman-Kenney et al., 2023) require users to enroll in Autopay (CFPB, 2022).

Firms are incentivized to enroll consumers in 'Autopay Min': Autopay set to pay only, exactly the minimum each month. Approximately one in four UK credit cards (FCA, 2016a) and three in ten US credit cards (Keys and Wang, 2019) only pay at or near the minimum. But persistent minimum payments and high interest costs are concentrated Autopay Min enrollees. By a regulatory definition of persistent credit card debt – making 9+ minimum payments in a year on interest-bearing cards – 75% have Autopay Min (FCA, 2016a). Consumers who switch into Autopay Min pay more in credit card interest than they save in reduced late payment fees. The 20% of UK credit cards enrolled in Autopay Min account for 43% of total interest and fees across all UK credit cards (Sakaguchi et al., 2022).

Our nudge varies how Autopay enrollment options are displayed to consumers opening a new credit card. Enrolling in Autopay is an opt-in choice architecture. Cardholders are presented with three Autopay enrollment options: 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'). With Autopay Fix, their automatic payment is maximum of their fixed amount and the minimum due that month. These three Autopay options are standard in the UK and US.

Our nudge removes the explicit appearance of the Autopay Min option, but this remains a feasible option for consumers. We shroud the Autopay Min option given evidence credit cardholders do not make well-informed decisions: automatically, repeatedly only paying exactly the minimum by default is unlikely to be optimal (e.g. Laibson et al., 2007; Sakaguchi et al., 2022), consumers anchor payments to the minimum (e.g. Stewart, 2009; Keys and Wang, 2019), significantly underestimate long only paying the minimum will take to amortize debt (e.g. Adams et al., 2022), and overconsume due to naïve present bias.[4]

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). Autopay Fix enables consumers to make an enhanced active choice (e.g. Keller et al., 2011) on how much they want to automatically pay each month by default. An active choice approach can be optimal in domains such as ours (e.g. Carroll et al., 2009) where large heterogeneity in consumer circumstances makes it hard to set an appropriate default repayment and when consumers are likely to procrastinate on paying down debt.

Our pre-registered field experiment is an ex-ante test of a potential nudge that the UK consumer financial protection regulator – the Financial Conduct Authority (FCA) – was considering implementing, given its concerns over consumers persistently holding credit card debt (FCA, 2014, 2016b). We conduct the experiment on 40,708 UK credit cards newly issued by one lender. A second lender withdrew from the experiment before fieldwork was complete. We measure outcomes in credit card and credit file administrative datasets.

Our nudge shrouding the Autopay Min option causes a large, proximate effect on Autopay choices: increasing initial Autopay Fix enrollment by 21 percentage points (72%). The nudge causes the likelihood of only paying exactly the minimum falls by seven percentage points. These effects are persistent over time. However, we do not find effects 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. If the only effect of the policy had been changing the composition of Autopay enrollments we estimate this to translate into reducing debt by approximately 4.5%. While our active choice nudge successfully harnesses psychological insights to change enrollment choices, it cannot

[4] Meier and Sprenger (2010); Heidhues and Kőszegi (2010, 2015); Kuchler and Pagel (2021).

change real economic outcomes. Our study is an example, in an economically important domain, how policies academics, financial firms, consumer organizations, and regulators expect to change consumer behavior can struggle to do so.

Why does the nudge have no distal effects on debt reduction, despite its large, proximate effects on choices? Three offsetting consumer responses explain why this does not occur. First, the nudge results in consumers selecting Autopay Fix amounts that are 'too low': binding at or just above the minimum. Second, the nudge lowers enrollment in any Autopay (i.e. Autopay Min, Autopay Fix, or Autopay Full). With fewer consumers enrolled in Autopay, more miss payments and so their debt does not reduce. Finally, we observe substitution by those enrolled in Autopay. Automatic payments increase but this is offset by lower manual payments. Without this manual payment substitution we estimate this group's debt would decline by approximately 2.9%. We interpret this as revealing credit cardholders enrolled in Autopay are more attentive than they first appeared: cardholders take action counteracting the nudge.

Our results demonstrate how important it is to measure the distal effects of nudges because these may significantly differ from effects on proximate choices. Our study advances a broader debate on the effects of nudges (e.g. Thaler, 2017; Laibson, 2020; Chater and Loewenstein, 2022). DellaVigna and Linos (2022)'s meta-study systematically documented the heterogeneous effects of nudges and shows academic publication bias overestimates their average effectiveness.[5] Across financial domains nudges can shift enrollments but consumers may also counteract these. For example, Choukmane (2021) finds the long-run effects of automatic enrollment defaults on savings are smaller than short-run contribution increases

[5]DellaVigna and 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%).

found in the earlier, seminal academic literature (e.g. Madrian and Shea, 2001; Thaler and Benartzi, 2004). Some 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).

Liquidity constraints explain why consumers do 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. Our new dynamic liquidity constraint measure reveals constraints bind for approximately 50% of consumers in our linked data, compared with just 10% using a traditional static measure. Our new measure correlates with subsequent credit card repayment decisions. Consumers with small, positive minimum liquid balances (before card opening) were discontinuously more likely to repay approximately 20 percentage points more of their credit card debt seven cycles later than those with small, negative minimum liquid balances. This new measure can be broadly applied to understand heterogeneous consumption & investment decisions.

The paper proceeds as follows. Section II explains our experiment's design (II.A.), theoretical motivation (II.B.), and implementation (II.C.). 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

Our field experiment varies how Autopay enrollment options are presented to UK consumers at credit card opening. In this section we explain our experiment: the nudge's design (II.A.), the nudge's theoretical motivations (II.B.), and our experiment's implementation (II.C.).

II.A. Nudge Design

Credit cardholders have broad discretion in how much to repay each month (in contrast to fixed term loans): paying any amount between the minimum due and the full balance fulfils their contractual obligations. The minimum payment due is typically calculated by $max\{\pounds 5,\ 1\%\ \text{statement balance}\ +\ \text{interest}\ +\ \text{fees}\}$.[6] If a cardholder is only paying the minimum, then (i) their repayment is effectively only servicing debt interest payments (with interest rates near 20% typical) rather than substantially paying down capital, and (ii) debt reduction only happens if pay down of debt principal exceeds new spending.[7]

When a consumer opens a new credit card online they are typically presented with the option to opt-in to enroll in Autopay. If a consumer decides to opt-in, they are normally presented with three Autopay options: Autopay Full, Autopay Fix, and Autopay Min. These are shown to the control group (Figure 1, Panel A) and are also typically presented on US credit cards.[8] At this stage consumers can still decide against enrolling in any type of

[6]This is a typical and most common construction, but there are some exceptions. Some UK credit cards have higher percentages of outstanding balances in their minimum payment rules. Some UK credit card brands have a minimum of £25 rather than £5. Some UK credit cards also include another clause for max 2.5% (or a different fraction) of balance. Some UK credit cards issued before 2011 have minimum payment rules which may not pay off debt even if the cardholder paid the minimum and spent no more on their card.

[7]This credit card amortization structure is somewhat similar to interest-only (or reverse) mortgages. Such mortgages have a fixed end point requiring the consumer 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.

[8]The largest US credit card lenders (e.g. American Express, Chase, Citi, Capital One, Discover, US

Autopay by not completing the enrollment process. They could also return and complete the enrollment later.

While Autopay Min is a common repayment option, cardholders also have the option to enroll in an alternative Autopay option that would repay debt faster: 'Autopay Fix'. Autopay Fix is calculated by: $max\{\text{Autopay Fix £},\ \text{Minimum Payment Due}\}$. While the minimum payment – and therefore Autopay Min – typically declines with balances, a fixed payment functions as a ratchet sticking to the higher payment amount. 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 (starting around £25 and reducing to £5). However, by fixing to paying £25 each month, it dramatically reduces to pay off debt in 5 years and 1 month, saving over £750 in interest costs. Choosing even a slightly higher fixed payment amounts can greatly reduce amortization times and borrowing costs. For example, with a fixed payment of £50 each month, pays off debt in 2 years and interest costs become only £191 (compared to £509 if paying a fixed amount of £25).[9]

The treatment webpage (Figure 1, Panel B) is a new, bold 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 in 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.

The treatment aims to work by first increasing Autopay Fix enrollment which, relative to Autopay Min, is expected to increase automatic payments (and significantly shorten the hypothetical repayment schedule) which, in turn, increases payments above the minimum

Bank, & Wells Fargo) present these Autopay options.

[9]All scenarios assume 18.9% APR and no further card spending.

and, assuming spending is unchanged, reduces debt and interest costs.[10]

While there no longer is an explicit Autopay Min option in the treatment, consumers can choose an equivalent option by setting an Autopay Fix of £5 (or less). These two options are equivalent as the minimum payment is calculated as $max\{£5,$ 1% 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.[11] This equivalence is not highlighted to consumers and we do not expect them to be aware of this or work this out. We explain this to show that the treatment does not restrict consumer choice of an Autopay option to pay the minimum – and so the treatment is a nudge rather than a restriction (the Autopay Min option is just no longer explicitly labelled on the website). If a consumer in either the control or treatment group phones the lender's call center they could still enroll in an explicit Autopay Min if they ask to do so. 30 days after card opening consumers in both the control or treatment groups have identical control group screens containing explicit Autopay Min options if they want to change their Autopay enrollment.

[10]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 and Souleles, 2002; Agarwal et al., 2017)

[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. Theoretical Motivations

Consumers may enroll in Autopay Min and repeatedly pay only the minimum because of a combination of economic & psychological factors. We discuss how these theoretical motivations inform our nudge design.

Present Bias

The nudge is designed to help counter present bias (Laibson, 1997). Theoretical models without present bias struggle to explain observed levels of credit card debt (Laibson et al., 2007). If naïve, present biased consumers are over-consuming, this generates welfare losses and therefore provides a rationale for nudging consumers to repay more (e.g. Heidhues and Kőszegi, 2010, 2015; Allcott et al., 2022). Empirical literature finds present biased consumers hold more credit card debt (Meier and Sprenger, 2010) and struggle to stick to their plans to pay it down (Kuchler and Pagel, 2021). A present biased consumer may enroll in Autopay Min with the intention to make additional manual payments to reduce debt, however, they may procrastinate and not do so (O'Donoghue and Rabin, 1999).

The nudge increases the salience of the Autopay Fix option that provides a way for consumers today to decide their long-run intentions for how much they want to repay each month in the future. Consumers are choosing a repayment plan for the future: setting a higher Autopay Fix amount only results in a payment when a consumer's first payment comes due – typically one to two months after card opening. Sophisticated agents may view Autopay Fix as a commitment device to repay debt (potentially one they would not consider when the Autopay Min option is presented alongside it). Naïve agents may be overoptimistic in their ability to repay and set a high Autopay Fix. Psychological frictions push against dynamically-inconsistent consumers exerting effort to later reduce their Autopay.

Financial Illiteracy

Our expectation was consumers' decisions to enroll in Autopay Min and persistently paying only the minimum are generally unlikely to reflect well-informed choices by financially literate consumers. By no longer explicitly showing the Autopay Min option it avoids unsophisticated consumers easily choosing the Autopay Min option.

There is clear evidence of credit cardholders making non-optimal repayment choices (e.g. Gathergood et al., 2019b,a) with credit card lenders structuring products & marketing to exploit a lack of sophistication (e.g. Ru and Schoar, 2020). Approximately half of credit cardholders in one UK survey incorrectly thought the minimum payment is the amount most people repaid: it is not - approximately a quarter do (FCA, 2016b). Studies across countries show cardholders have mistaken beliefs: significantly overestimating the speed (and underestimating the interest costs) paying only the minimum payment reduces debt (e.g. Lusardi and Tufano, 2015; Seira et al., 2017; Adams et al., 2022) Among a survey of UK Autopay Min enrollees 96% of respondents underestimate the time it would take to fully repay a debt if the cardholder made only the minimum required payment (Adams et al., 2022). Informational disclosures to credit cardholders to address financial illiteracy are ineffective at changing consumer behavior across the US (Agarwal et al., 2015), Mexico (Seira et al., 2017), and the UK (Adams et al., 2022).[12] Given the ineffectiveness of such disclosures, our nudge tests a more intrusive intervention.

Inertia & Limited Attention

Our nudge is targeted at new card originations to be a preventative measure against inert consumers persistently carrying high credit card debt. We nudge Autopay enrollment at card origination because these initial decisions are sticky over time (e.g. Sakaguchi et al., 2022;

[12] 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 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).

Wang, 2023) – even with later nudges to do so (Adams et al., 2022). An explanation for sticky enrollment is consumers with limited attention default into enrolling into Autopay Min, do not get around to updating their initially-selected Autopay choice, and neglect to make larger non-Autopay payments reducing debt that they would otherwise make (Sakaguchi et al., 2022). Targeting behavior at origination at a time when consumers are most attentive is expected to be more likely to succeed than changing existing cardholder behaviors that first requires attracting consumer attention and then also prompts them to act.[13] Our nudge aims to harness inertia 'for good' by getting consumers to initially enroll in an Autopay Fix (or Autopay Full) to be on a path to automatically repay more of their credit card debt.

Without an explicit Autopay Min option consumers with limited attention are forced to make an enhanced active choice (Keller et al., 2011; Beshears et al., 2021) – considering how much they can afford to regularly pay each month. The nudge makes it difficult for inattentive consumers to default into automatically paying only the minimum. We purposefully design our nudge to not specify a default Autopay choice. A lack of an explicit default is an attractive policy option given a broad heterogeneity in consumers' socio-economic circumstances & preferences (e.g. Carroll et al., 2009).[14]

Anchoring

The salience of the minimum payment amount can act as a 'bad' nudge anchoring consumers to repay lower amounts than they otherwise would (e.g. Stewart, 2009; Keys and

[13]Consumer inaction is common across household financial decisions – even simple decisions with low frictions such as cash savings (e.g. Adams et al., 2021) and decisions with high financial stakes such as mortgage choices (e.g. Keys et al., 2016).

[14]This is especially due to information asymmetry meaning the policymaker has significant uncertainty at the time of credit card opening in how a consumer will use their card – making it difficult to calculate the optimal defaults for heterogeneous consumers. In the domain of retirement savings Carroll et al. (2009) discuss how a default asset allocation may be optimal but it may be preferable to set contribution rates by active choice given heterogeneity in optimal savings rates. See Keller et al. (2011) and Cronqvist and Thaler (2004) for comparisons of defaults and active choices in retirement savings.

Wang, 2019).[15] Lab evidence consistently shows across studies & countries how anchoring makes consumers significantly more likely to pay exactly the minimum payment, less likely to pay in full, and changes the distribution of payments to be more dispersed away from the minimum, and makes them no more likely to pay less than the minimum.[16]

Our nudge removes the minimum payment as an anchor during Autopay enrollment. We purposefully do not include an alternative recommended Autopay Fix amount as we do not want to replace one anchor with another: we want consumers to make active choices. This decision is informed by US studies (Agarwal et al., 2015; Hershfield and Roese, 2015; Keys and Wang, 2019) which find providing consumers with credit card repayment scenarios has an unintended anchoring effect: reducing payments for some consumers.

Liquidity Constraints

Given the evidence of a variety of behavioral factors (e.g. anchoring, financial illiteracy, limited attention, naïvete, present bias), these appeared the most likely explanation for consumers enrolling in Autopay Min and repeatedly only paying the minimum.

An alternative explanation is that consumers are subject to liquidity constraints on their decisions. If a consumer is experiencing binding liquidity constraints then – irrespective of whether they are subject to behavioral factors – our nudge would be ineffective. This is because the consumer does not have the money to pay down more of their credit card debt (although this may also be deep-rooted in their struggle to manage their finances). The nudge is designed to enable behavioral consumers not facing liquidity constraints to set a path to automatically reduce their debt *without* forcing consumers experiencing temporary liquidity constraints into arrears.

[15] Sunstein and Thaler (2008) write "Credit cards minimum payment...can serve as anchor and as a nudge that this payment is an appropriate amount."

[16] Stewart (2009); Navarro-Martinez et al. (2011); McHugh and Ranyard (2016); Guttman-Kenney et al. (2018); Sakaguchi et al. (2022)

II.C. Experiment Implementation

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

We implement the experiment on new credit cards. When a consumer is applying for a new credit card online and has been accepted by a lender they have the option to set-up Autopay on this new card. If a consumer selects the option confirming that they want to enroll for Autopay, they are 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 are directed to either control or treatment (the nudge) group screens based on random assignment.[17] Once allocated to control or treatment the consumer would view their allocated screen if they returned to the pages within 30 days.

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

Our field experiment has 40,708 credit cards newly issued by a large UK lender between February and May 2017.[18] We also conduct the experiment with a second lender. The second

[17] 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.

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

lender stopped the experiment after one week of fieldwork due to concern over the large size of the proximate effects on Autopay choices. The second experiment was not restarted and the pre-agreed target sample size was not reached. The second experiment's achieved sample size of 1,531 cards is insufficiently powered to distinguish between null results and real effects. Had we known this second lender would have pulled-out we would not have run the experiment with them. This shows the practical challenges of running RCTs. For completeness results from the second lender are in the Internet Appendix.[19] The rest of this paper is based on the experiment conducted with the first lender unless explicitly stated.

Complementary Polices Tested

The experiment in this paper was part of a series of research testing nudges to reduce credit card debt to inform FCA policymaking. In Adams et al. (2022) we conduct field trials across three lenders testing whether personalized, informational nudges explicitly encouraging debt repayment via standalone emails or letters to credit cardholders already enrolled in Autopay Min could change behavior ex-post. These had zero or small proximate effects on choices and were ineffective at changing real economic outcomes.

What about consumers not enrolled in Autopay? In Adams et al. (2022) we find adding nudges to monthly statements had precisely zero effects on consumers not enrolled in Autopay. No firms were willing or able to participate in a field experiment testing a nudge remove the anchoring effect of the minimum on manual payments and therefore the best feasible alternative was to conduct a framed field experiment matching survey responses with administrative data (Guttman-Kenney et al., 2018) that built on our earlier lab evidence

[19]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).

(Sakaguchi et al., 2022) to inform potential policymaking. Both studies find evidence of anchoring indicating an active choice treatment would change proximate and distal choices.

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 is gathered by the UK financial regulator (FCA) using its statutory powers. From the two credit card lenders in the experiment we collect detailed microdata covering every credit card in the experiment. We observe data recorded at card origination (e.g. opening date, interest rates, initial credit limit) and across all statements (e.g. statement balances, transactions) to December 2017. A completed statement cycle is one where the payment due date for a credit card statement has passed. For the main lender in our experiment we observe seven completed statement cycles for effectively all cards (99.9%) and up to eleven for the cards opened earliest in the experiment.[20] Each individual payment made against these statements is observed including the date, amount, and whether the payment was made automatically or manually.

Credit files are gathered for all the individuals in the experiment enabling us to observe effects across a consumer's portfolio of credit cards. These provide monthly, product-level data showing credit limits, balances, payments and arrears from card opening to the end of 2017. For credit cards we observe statement balances (i.e. before repayments), repayments,

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

balances after repayments (i.e. debt), and indicators for whether a card only paid the minimum. UK credit files contain payments data for all credit cards – this is higher quality than US credit files where only a selected subset of credit cards report repayments data (Guttman-Kenney and Shahidinejad, 2023). We observe credit risk scores and income estimates (where available) at two points-in-time: the month before the card was opened and nine months afterwards. The lender microdata and credit files are linked using an anonymous key created for this project. All analysis is conducted on anonymized data.

We also observe data on bank accounts (checking/current accounts and savings accounts) for the subset of cardholders who hold these accounts with the credit card lender in our experiment. The bank account data report end of day balances each day up to a year before (or when the account was opened) the experiment started and up to June 2017 - a month after the last cards were enrolled in our experiment. We keep 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.[21] These cardholders are more likely to be younger, with higher incomes and credit scores, fewer credit cards and lower credit card debts (Internet Appendix Table A12).

[21]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 in 3,831 cardholders.

III.B. Empirical Methodology

Following best practice in conducting field experiments, we pre-registered our empirical methodology before analyzing data. Our pre-registration outlines the structure of analysis including the primary outcomes, regression specifications, and thresholds for evaluating statistical significance which we now document here.[22]

We structure our overall analysis in three parts: primary, secondary, and tertiary analyses. This structure limits the potential issues for data mining or p-hacking. The primary analysis focuses on ten primary, real economic outcomes upon which the nudge's effectiveness is evaluated. The primary outcomes are:

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

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

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

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

[22] 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.

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

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

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

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

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

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

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 regard a p value of 0.005 as the threshold for statistical significance but also highlight where results are 'suggestively significant' at the 0.01 and 0.05 levels.[23] 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.[24]

The pre-registered secondary analysis considers a broader set of outcomes and empirical approaches to understand our results and their robustness. We measure the *proximate* effects of the nudge on Autopay enrollment and evaluate long-run effects using the amounts of credit card debt and repayments (£). Conducting secondary analysis depends on the primary analysis's results. We design and implement tertiary analysis after examining the data.

We are able to causally identify the effects of the treatment on consumers in our field experiment since we are randomizing whether a consumer receives the control or treatment. The average treatment effect is the policy parameter of interest as the treatment was a

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

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

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

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

Our regression includes a constant (α), a vector of time-invariant control variables (X_i') constructed using information on the new credit card opened and cardholder data from before the start of the experiment. The controls (X_i') are: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These were all from the time of card origination except for the variables constructed from credit file data (Credit Score, DTI Ratio and Any Mortgage Debt), which were from the month preceding card origination. For outcomes constructed from credit file data up to eleven dummies for lags of outcomes were included as controls (X_i') for months preceding the start of the experiment. We also include time fixed effects: we control for both the statement cycle (γ_t) and year-month ($\gamma_{m(i,t)}$) because statement cycles do not perfectly align with calendar months and new credit cards have different opening dates - entering the experiment until the required sample size was achieved. Standard errors are clustered at the consumer-level.

For our primary analysis we focus on the outcomes from the last cycle where the panel is balanced: the seventh completed statement cycle (δ_7) which, on average, is 195 days after card opening.[25] 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 a particular billing cycle and so this first statement has a zero payment due that makes it uninteresting (we show for completeness). A consumer's first full statement is statement two (in our data the second statement is issued mean 43, median 42 days from card opening) when the cardholder has had at least one month to view the control or treatment screens and use their card and, if used, their card will have a non-zero payment due.

In tertiary analysis we check the robustness of selected results by pooling across all statement cycles to provide more statistical power. We modify Equation 1 replacing the dynamic $\left(TREATMENT_i \times CYCLE'_\tau\right)$ with static $TREATMENT_i$ 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 with a wide range of interest rates, credit scores, credit card credit limits, ages, and incomes.

[25] 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.

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

The mean credit card statement balance after cycle seven is £2,165 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.[26] 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 or their implications when compared to results from unconditional means.[27]

IV Experimental Results

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

[26]Internet Appendix Table A3

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

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 causes large, significant initial effects in Autopay enrollment choices. The treatment raises the fraction of cardholders enrolling in Autopay for a fixed amount (Autopay Fix) by 20.9 percentage points: a 72% increase on the control group mean. For comparison, Figure 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.

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.[28] 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

[28]Internet Appendix Figure A1 shows Figure 2 for each cycle.

in the treatment group enrolled in Autopay Fix at cycle 2 the mean Autopay amount was £96.85 (median £80) compared to £104.60 (median £100) at cycle 7: this indicates that cardholders who enroll in Autopay Fix later on are choosing slightly higher Autopay Fix amounts than the initial group.

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

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

We estimate these treatment effects on Autopay enrollment more precisely using our pre-registered regression specification and find statistically significant changes in enrollment. The regression coefficients after seven statement cycles (δ_7 in Equation 1) – presented in Table 2 – are in line with initial changes in enrollment: Autopay Min enrollment decreased 21.7 pp, Autopay Fix enrollment increases 16.7 pp, Autopay Full increases 0.6 pp (the latter being only significant at the 5% not the 0.5% level), and any Autopay enrollment declines 4.4 pp.[29] Estimates cycle-by-cycle (δ_τ in Equation 1) are displayed in Figure 5 for Autopay Fix enrollment, Internet Appendix Figure A2 for Autopay Full and Autopay Min, and Internet 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

[29]Internet Appendix Table A5 shows t-tests of unconditional means.

and Autopay Min also attenuate but effects remain large. Effects of the treatment on any Autopay enrollment change relatively little between cycles two and eight.

The observed changes in Autopay enrollments – the nudge making consumers more likely to choose full, less likely to choose minimum, & changing the distribution of Autopay amounts – are consistent with a variety of biases (e.g. anchoring, limited attention, financial illiteracy) distorting the control group's choices. These changes closely match our framed field experimental results testing a nudge to remove the anchoring effect on manual payments (Guttman-Kenney et al., 2018): making consumers hypothetically more likely to pay in full, less likely to pay the minimum, & changing distribution of payments to be more dispersed away from the minimum. In re-analyzed data from (Guttman-Kenney et al., 2018) there is also consistent evidence that consumers enrolled in Autopay are subject to anchoring.[30]

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. These estimates are seven statement cycles after card-opening (δ_7 in Equation 1) and are shown in Table 3 (unconditional means in Internet Appendix, Table A4).

We find a large and persistent effect of the nudge making cardholders less likely to only pay exactly the minimum. The nudge causes a significant reduction in the likelihood of only paying exactly the minimum of 7.1 pp (95% confidence interval of 6.2-7.9 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.[31]

[30]In a hypothetical credit card repayment experiment consumers enrolled in Autopay Min commonly select to pay exactly the minimum (as they do in real life). When we shroud the minimum payment amount, their hypothetical repayments become de-anchored from the minimum.

[31]As a robustness check we find consistent results examining the cumulative number of minimum payments. The cumulative number of minimum payments increases linearly for each new statement cycle from

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. Also some cards have no balance due and therefore no minimum payment and no payments taken (we regard such cases as a full payment).

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

We observe precisely-estimated null effects on average treatment effects on other primary outcomes for the target card in the experiment: the likelihood of paying debt in full, debt net of payments, borrowing costs, and purchases. The exception is an increase in 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 reduce 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) or across the portfolio of credit card debt (Internet Appendix Figure A5).

As the cycle-by-cycle estimates on our primary measure of credit card debt are persis-

statement cycle five: cycle-by-cycle regression estimates in Internet Appendix, Figure A4 and estimates from the seventh cycle in Table 5 with unconditional means in Internet Appendix, Table A6.

tently, slightly (but statistically insignificantly) below zero, we check the robustness of this result in tertiary analysis by pooling across all statement cycles to provide more statistical power (Equation 2). By doing so we can say that, if the treatment has any average effect on debt, the average effect on the target card is at most a 1.1 percentage point reduction 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.

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 robustness check, we examine the cumulative number of full payments and results are consistent with stable, precisely-estimated null effects across cycles.[32] Our null average treatment effects on debt (robust to secondary outcomes) in spite of a seemingly large, proximate change in enrollment and paying only the minimum payment is surprising.[33] How can it be that the treatment is not, on average, reducing debt if one in five more consumers are enrolled in Autopay Fix (and are not increasing spending)?

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.).

[32]Cycle-by-cycle regression estimates in Internet Appendix, Figure A4 and results from the seventh cycle in Table 5 and unconditional means in Internet Appendix, Table A6.

[33]Table 5, Internet Appendix Table A6

V.A. Offsetting Consumer Responses

If the only change were compositional, changing Autopay enrollment (without any other changes), the proximate effects on Autopay enrollment may have been expected to lead to a distal effect of reducing debt by approximately £90 (4.5%).[34] We find three offsetting consumer responses to the nudge make it ineffective: producing a precise zero effect on debt.

Autopay Fix Amounts 'Too Low'

Many cardholders are responding to the nudge by setting an Autopay Fix that is 'too low': often binding at or just above the minimum due. While the treatment causes a 16.7 pp increase in Autopay Fix enrollment by statement seven (the purple coefficients in Figure 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.[35] The regression estimates are in Table 2.

As credit card balances accumulate over the first few months of card ownership, the minimum amount due rises, causing the minimum payment amount to exceed many of the fixed payments. After seven statement cycles, the proportion of consumers in the treatment group with an Autopay Fix exceeding the minimum payment amount is 66% - noticeably down from 78% in the second cycle.[36]

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 (Panels

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

[35] Internet Appendix, Table A5.

[36] Internet Appendix, Figure A6 and Table A5.

B and D).[37] 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 cumulative across cycles 1-7).

Lower Enrollment In Any Autopay

The second offsetting effect is that the nudge causes a 4.3 pp (5.6%) significant decline in enrolled in any type of Autopay (Table 2).[38] This lower enrollment explains an unintended slight average increase in missed payments (Table 2). If enrolled in Autopay a consumer would only miss a payment if they have insufficient funds in their checking account whereas it is much easier for consumers not enrolled to forget to make a payment. While this increase is not statistically significant at our 0.5% significance threshold when examining any particular statement cycle, it is clearly significant when conducting a joint significance test pooling data across all statement cycles (while still clustering at the consumer-level): increasing missed payments by 0.4 pp with a 95% confidence interval of 0.19 to 0.62 pp.[39] There were no statistically significant differences in the types of consumers who were more likely to not have any Autopay enrollment as a result of the treatment.[40]

[37]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.

[38]Internet Appendix Figure A3 displays cycle-by-cycle regression estimates and unconditional means Figure A1 while Table A5 shows t-tests of these at cycle 7.

[39]Internet Appendix Table A9

[40]We conducted OLS regressions shown in Internet 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

The effect on missed payments is solely on temporarily being a single payment behind: precise zeros are estimated on being two or three payments behind.[41] The treatment does not lead to consumers being in more severe arrears which industry define as being 2+ or 3+ payments behind: these are all null results even when pooling observations across cycles to increase power to account for the low incidence of such severe arrears.[42] Only more severe arrears get reported in their credit file (i.e. missing a payment by 1 day would not be reported, but by 31 days would be reported). This explains why we do not observe increased missed payments in our primary outcome for the share of credit card portfolio missing payment (Tables 3 and 4) constructed from credit files. Given that there is no difference in severe arrears on the card in the experiment and also no difference in severe arrears across the portfolio of cards in credit files, we infer that severe arrears on others cards was unaffected.

We interpret this result as indicating that not having an Autopay means consumers forget to make a payment which has a temporary impact, most notably incurring a late payment fee (in line with Gathergood et al., 2021; Sakaguchi et al., 2022) and not reducing debt, rather than causing consumers to enter a debt spiral of severe financial distress.

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

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 of Autopay enrollment.

[41] Internet Appendix, Table A9. Examining the time series of cumulative number of missed payments (Internet Appendix, Figure A4) shows the treatment effect is stable after the third cycle.

[42] Internet Appendix, Table A9

insignificant, negative effect on manual payments (the estimates after seven cycles are shown in Table 5). We find the treatment causes consumers to be 1.3 pp more likely to make both an automatic and manual payment in the same cycle (Table 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.[43] 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.[44]

In months where manual payments are made by those enrolled in Autopay in the control group, the mean value of the manual payment is £377, with a median value of £105. Automatic payments in such months average £105 with a median of £55 and are similar in months where consumers are not making manual payments. Most manual payments by those enrolled in Autopay do not clear a consumer's debt – just 17.9% do so in the control group. 65% of manual payments are for round number values whose digit to the left of the decimal is a zero or five. These numbers found to prominently appear in manual payments appear with far less frequency in total payments: 48%. Such patterns of large, manual payments at round numbers may be consistent with cardholders experiencing adjustment costs (e.g. the psychological cost of logging into online banking to make a manual payment and working out how much to pay) to making a payment above the minimum or having reference-dependent preferences for round numbers (e.g. Sakaguchi et al., 2020). Consumers appear to use Autopay as insurance against forgetting to make a payment (in line with Gathergood et al.,

[43]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) (Internet 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. Internet 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.

[44]54% for those enrolled in Autopay Fix or Min in the control group at cycle seven.

2021; Fuentealba et al., 2021; Sakaguchi et al., 2022) as opposed to paying down debt.

However, comparing automatic and manual payments is conflating two effects: a change in Autopay enrollment composition and a change in Autopay amount. Conditional on being enrolled in Autopay, one would expect automatic payments to be higher in the treatment than the control, since Autopay Fix is greater than or equal to Autopay Min. Yet automatic payments will be lower in the treatment group because fewer consumers enroll in Autopay than in the control group. For the same reason we may expect manual payments to be higher in the treatment group, however, this is ambiguous as it depends on whether cardholders are forgetting to make any payments or substituting between automatic and manual payments.

To help disentagle these, we decompose Equation 1 by whether the consumer was enrolled in any Autopay (i.e. Autopay Min, Fix, or Full) at cycle seven ($AUTOPAY_{7,i}$) as shown in Equation 3. This is a decomposition by an endogenous variable and so our estimates will suffer from bias and are not causal.

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

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

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

V.B. Heterogeneous Effects

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

We examine three groups of consumer vulnerability: credit score, income, unsecured debt-to-income (DTI) ratio. These were chosen as groups that are observable to us (and lenders) and relevant to regulators as they are used as inputs for assessing new credit cardholders' ability to pay their debt. We split these groups into quartiles as it was not clear whether effects would be monotonic. We estimate Equation 1 separately for each quartile of each group. To keep the number of results manageable we only examine heterogeneous effects by our primary outcome of debt (statement balance net of payments as a percent of statement balance). In the control group, there is relatively little difference in this outcome measure across quartiles of income but noticeably more across quartiles of credit score and DTI - especially comparing the top and bottom quartiles (Internet Appendix Table A11).

Our heterogeneity analysis does not produce clear effects. Our results are presented in Figure 9 (with estimates in Internet Appendix Table A11). None of the heterogeneous groups show an effect that is statistically significant at our 0.5% threshold. There are no clear effects by income. By credit score we 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 with insignificant effects for other quartiles.

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 potential explanation is whether liquidity constraints prevented them from doing so.

We explore this by constructing new measures of liquidity constraints from our linked bank account data. Unfortunately, we only observe these linked data for a selected subset of cardholders who also bank with their credit card provider. Based on observed socio-economic characteristics (e.g. income, credit score), we would expect this sample to be less liquidity constrained than those we do not observe linked data for (Internet Appendix Table A12).

In addition to being a selected subsample, we do not have sufficient power to estimate treatment effects for this group.[45] Despite such limitations, these data represent an advancement on research into credit card payments decisions where liquid savings data is unobserved (e.g. Keys and Wang, 2019; Medina and Negrin, 2022). We therefore 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. These go beyond measures used in prior literature using transaction data which do not examine heterogeneity by the *minimum* balance reached but instead focus on different moments: the mean or median balance (e.g. Agarwal and Qian, 2014; Gelman et al., 2014; Olafsson and Pagel, 2018; Baker, 2018).[46] Our second measure examines a consumer's minimum liquid balances over the last 90 days before card opening (along with other time

[45] If we had sufficient power we would evaluate the nudge's heterogeneous effects by liquidity constraints.

[46] Agarwal and Qian (2014) segments by the mean value of checking account balance. Gelman et al. (2014) segments by the mean value of checking and savings accounts balances (normalized by the daily average spending of each consumer). (Olafsson and Pagel, 2018) segments by the mean and median values of cash and available liquidity (normalized by the daily average spending of each consumer to provide measures of 'consumption days'). Baker (2018) segments by the mean of liquid assets / income, illiquid assets / income, total assets / income, debt / (debt + assets), and debt / income.

horizons). This accounts for how consumers' finances are dynamic and thus one point-in-time does not reflect how constraints can bind at different points-in-time for different consumers depending on the timing of their incomes and expenditures.

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

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

Summarizing Liquidity Constraints

We show the distribution of these three measures of liquidity constraints in the left hand side panels of Figure 10 (summarized in Internet Appendix Table 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 balance above zero being much more likely than those below. This kink may reflect there being a discontinuous increase in costs from becoming overdrawn on checking accounts and precautionary rationale to keep a small amount of buffer stock savings. By this measure approximately 10% experience a binding liquidity constraint of having a zero or negative liquid cash balance. We also see this distribution has very fat tails (and so the mean is not well-estimated) but is stable over time with a median balance near £400.

Our second dynamic measure (Panel B) reveals clear sorting of consumers into two types (distribution summarized in Internet Appendix Table A18). One group of consumers has a zero or negative minimum liquid cash balance. There is a lot of bunching with another group of consumers just managing to keep positive, but small, liquid cash balances. A longer time window for calculating minimum liquid balances results in a slight steepening of the CDF around zero. Using a 90 day window the median minimum balance is effectively zero (£4.76) and the 75$^{\text{th}}$ percentile £142.39. This second measure reveals liquidity constraints commonly bind for approximately 50% of consumers: far higher than the 10% a point-in-time liquid balance measure (Panel A) would indicate.

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

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 clearer relationship when we use our measure of minimum liquid cash balances over 90 days. Consumers with positive minimum liquid balances (before card opening) discontinuously repaid approximately 20 pp more, on average, of their credit card debt seven cycles later than those with zero or small negative liquid balances. Given the bimodal distribution to repayments we also examine the other moments: payments at the minimum, full, and less than minimum. The discontinuity in average repayments is driven by discontinuous increases in the likelihood of paying in full and decrease in likelihood of missing a payment.[47] Paying only the minimum becomes less likely among less liquidity constrained consumers, however, there is a less clear discontinuity around zero. Panel F also shows a clear relationship: consumers who have more days with low liquid cash balances (pre-card opening), repay less credit card debt seven cycles later.

We interpret our results as helping to understand why these consumers are less nudge-able than they first appeared from their Autopay choices and inert minimum payment behavior. Consumers appear to be making 'low' credit card payments and offsetting the nudge to not reducing their debt due to frequently experiencing binding liquidity constraints. These liquidity constraints *may* also mean other interventions ultimately fail to change real outcomes.

Finally, our results provide micro evidence to understand why some consumers simultaneously co-hold high-interest debt and low-interest liquid cash: because consumers have a need for liquidity with constraints binding over relatively short time periods – this is most in line with Telyukova (2013)'s structural model that used survey data.

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

VI Concluding Discussion

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

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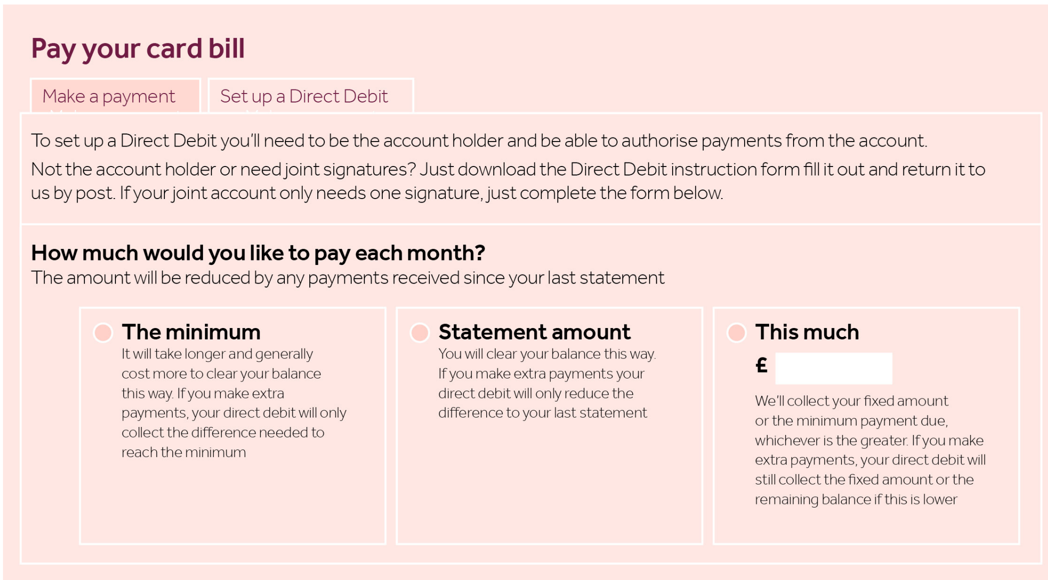
VIII Figures & Tables

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

A: Control



B: Treatment

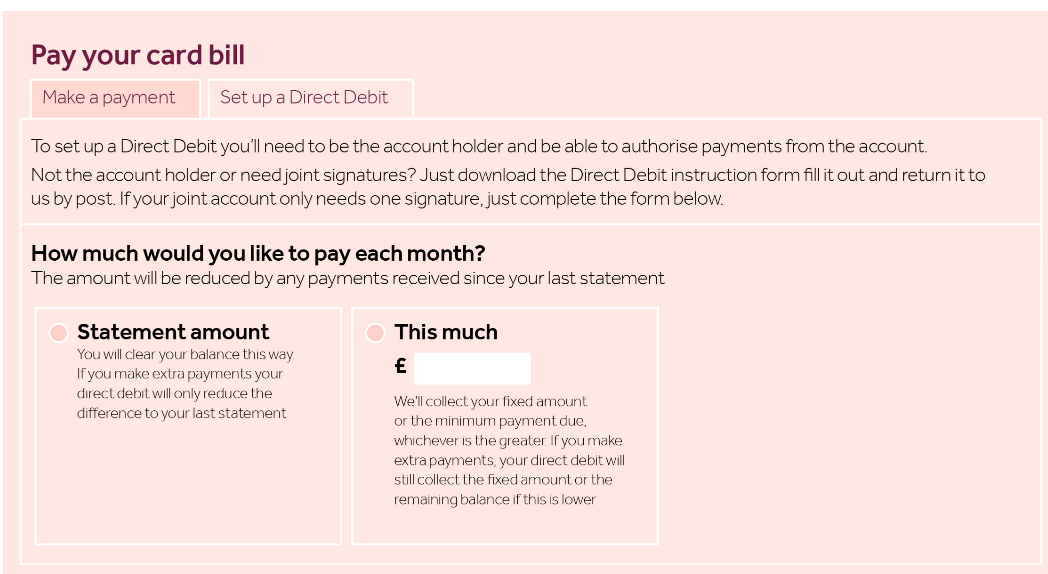
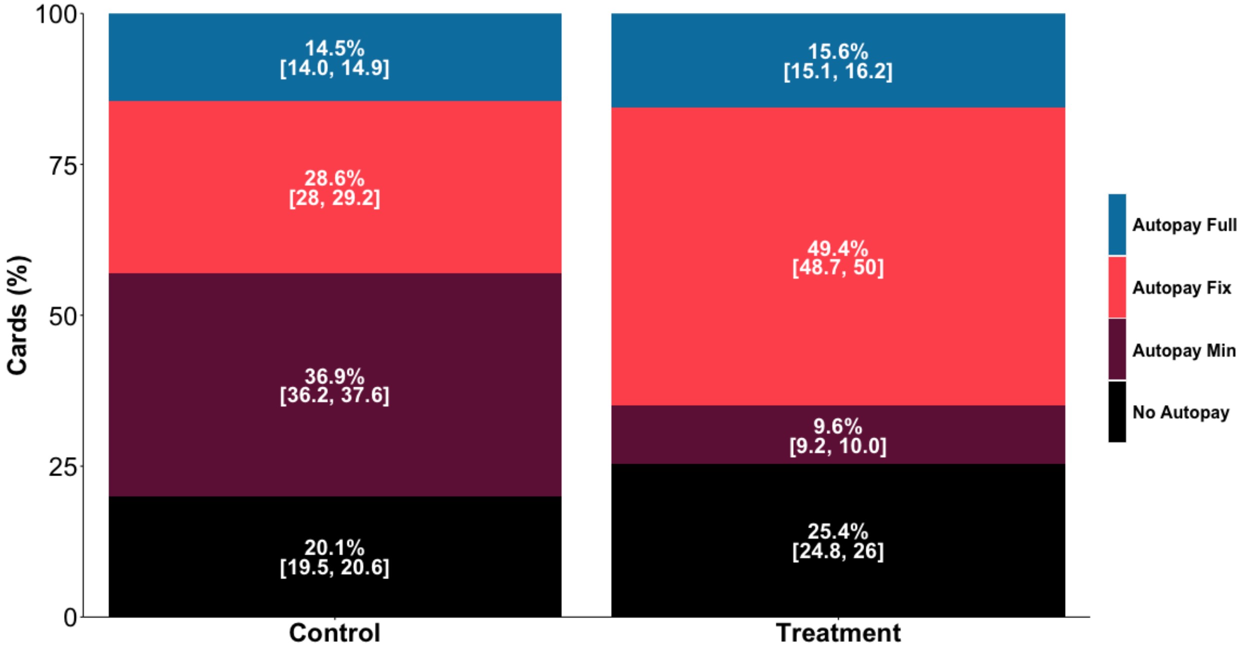
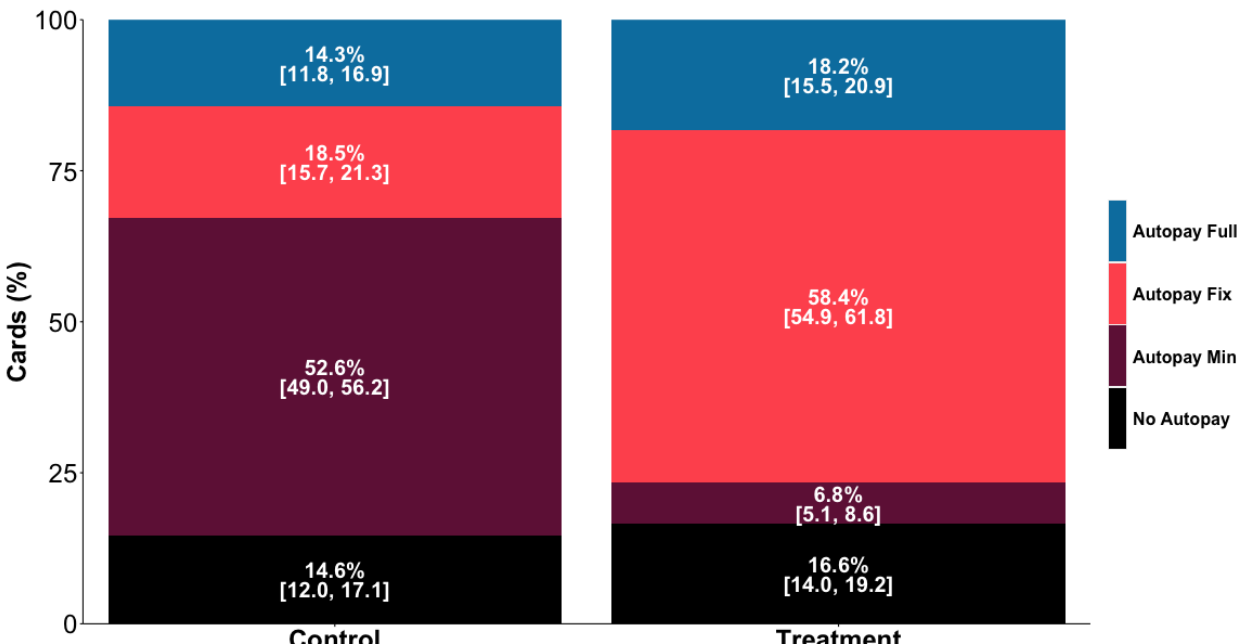


Figure 2: Autopay enrollment for control and treatment groups after two statements, split by lender

B: MAIN LENDER (N = 40,708)

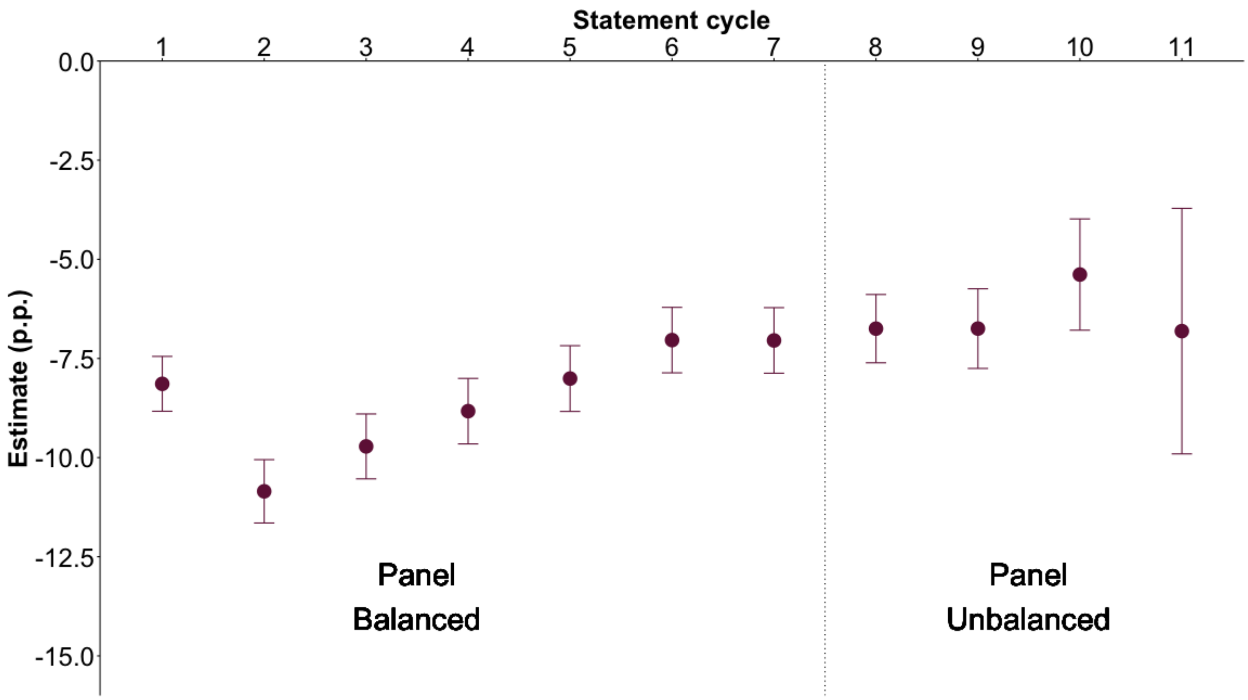


B: SECOND LENDER (N = 1,531)



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

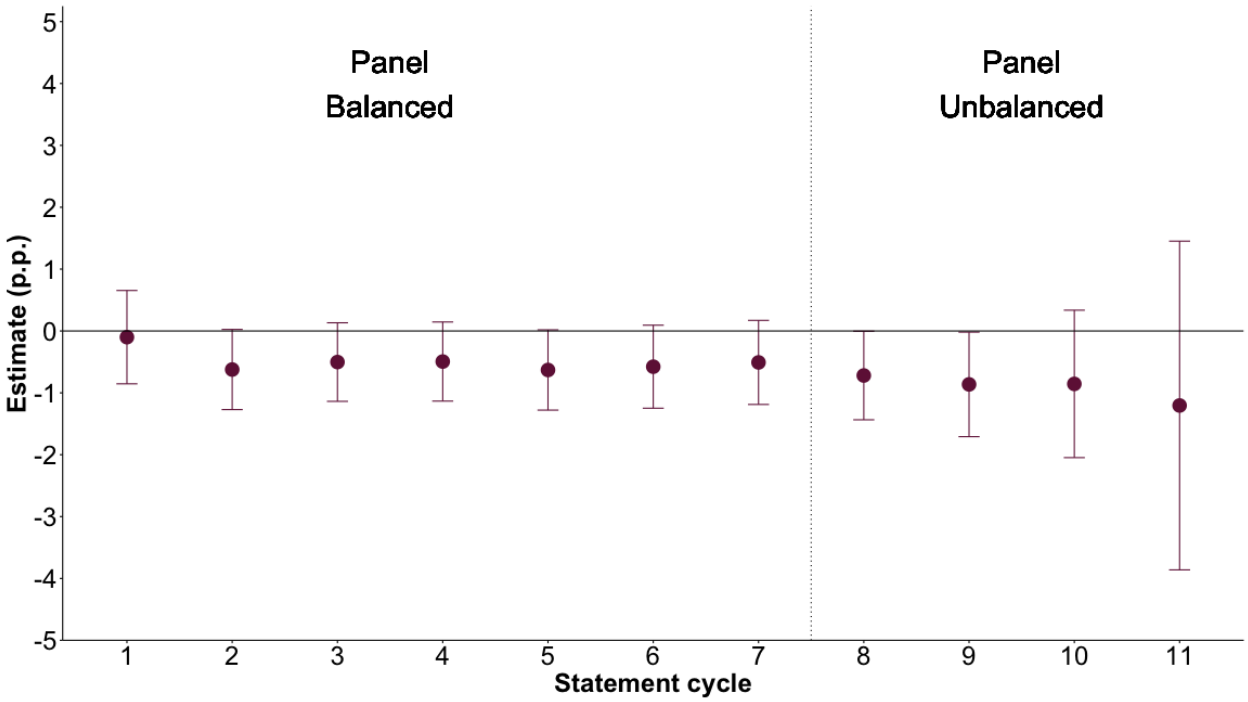
Figure 3: Average treatment effects on making only a minimum payment across 1-11 statement cycles



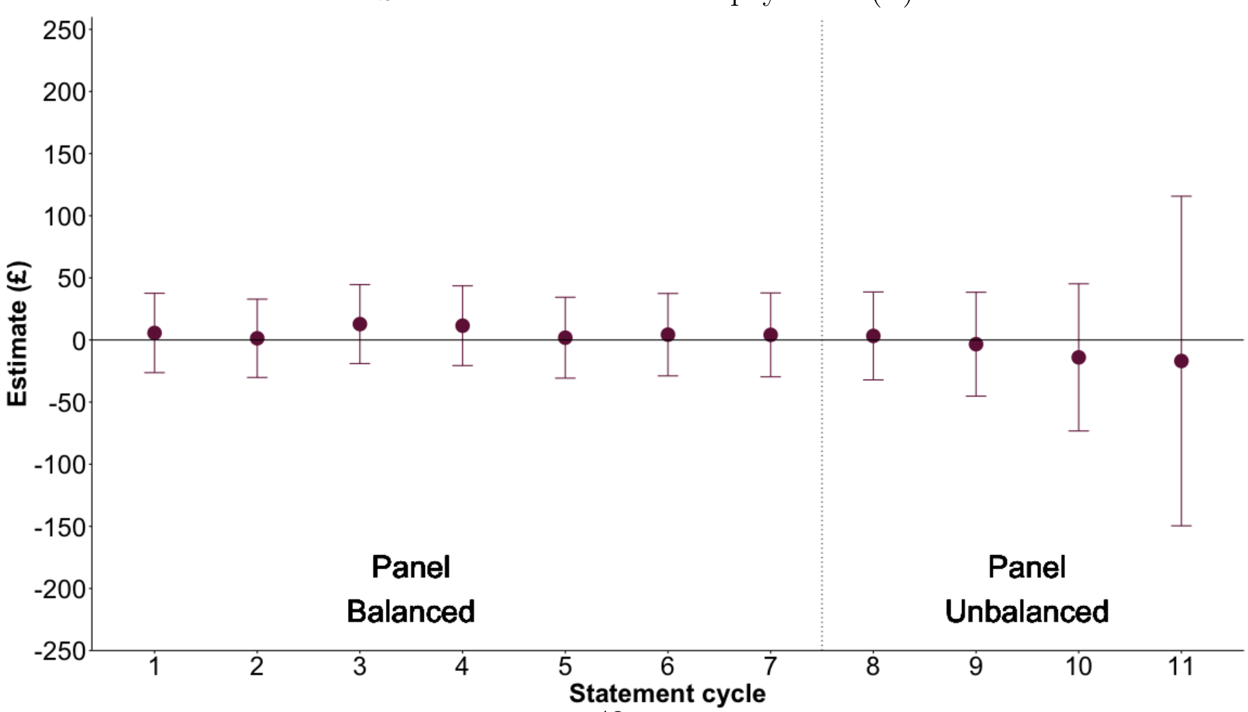
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

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

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

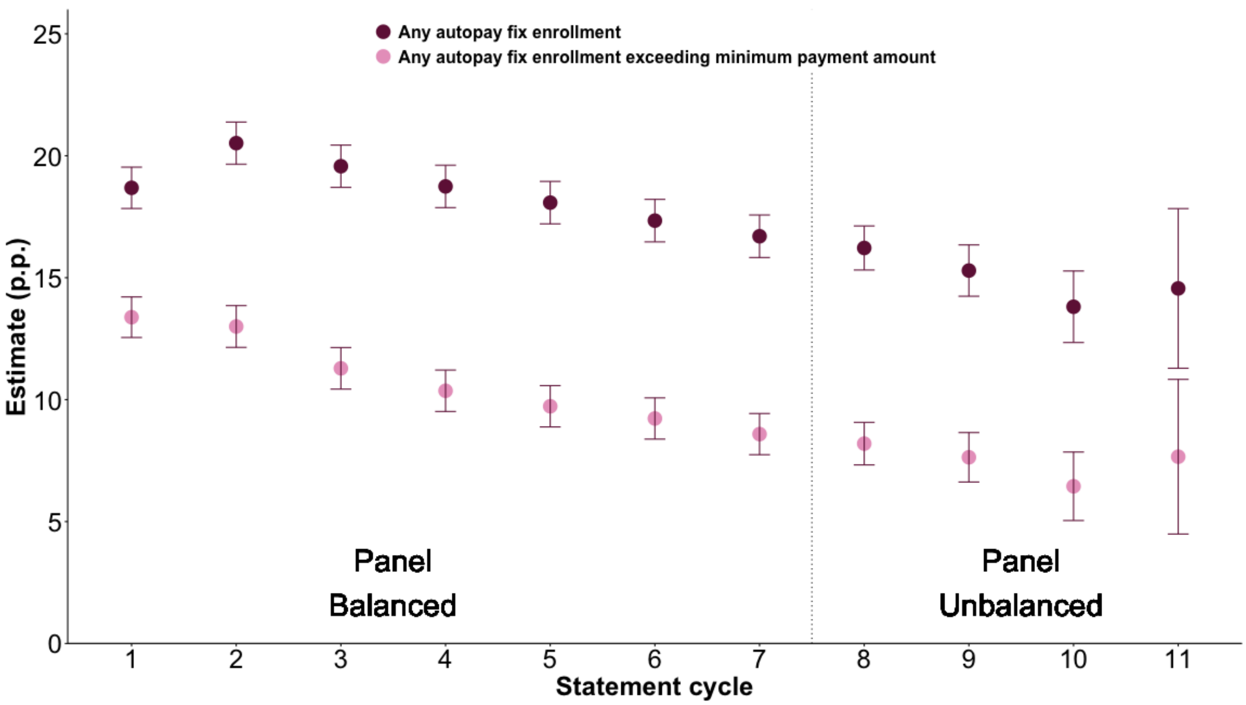


B: Statement balance net of payments (£)



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.

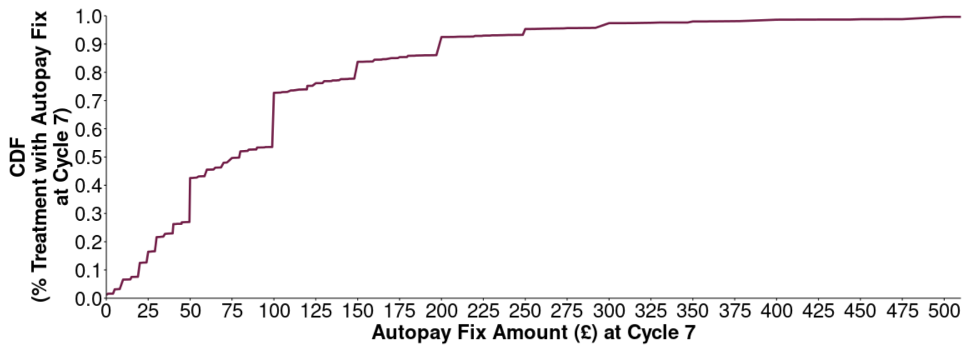
Figure 5: Average treatment effects on Autopay Fix enrollment (purple) and Autopay Fix enrollment not binding at minimum payment (pink) across 1-11 statement cycles



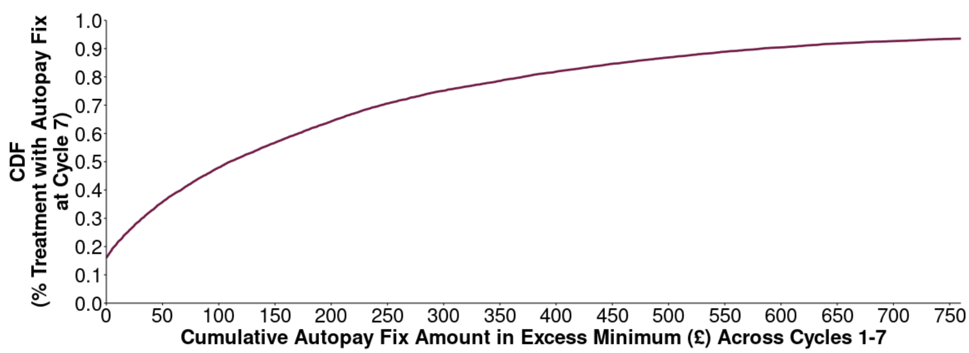
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

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

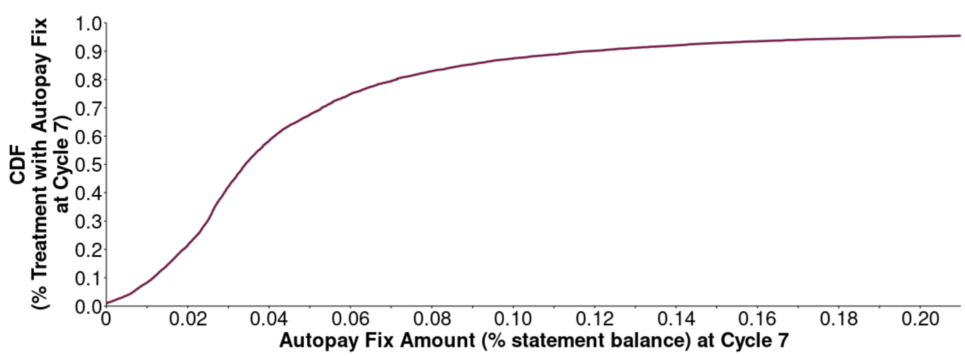
A: Autopay fix amount (£) at cycle 7



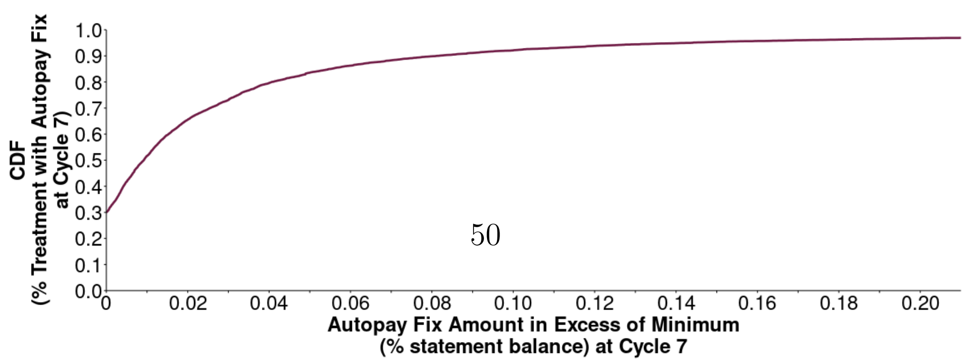
B: Cumulative autopay fix amount in excess of minimum (£) across cycles 1-7



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



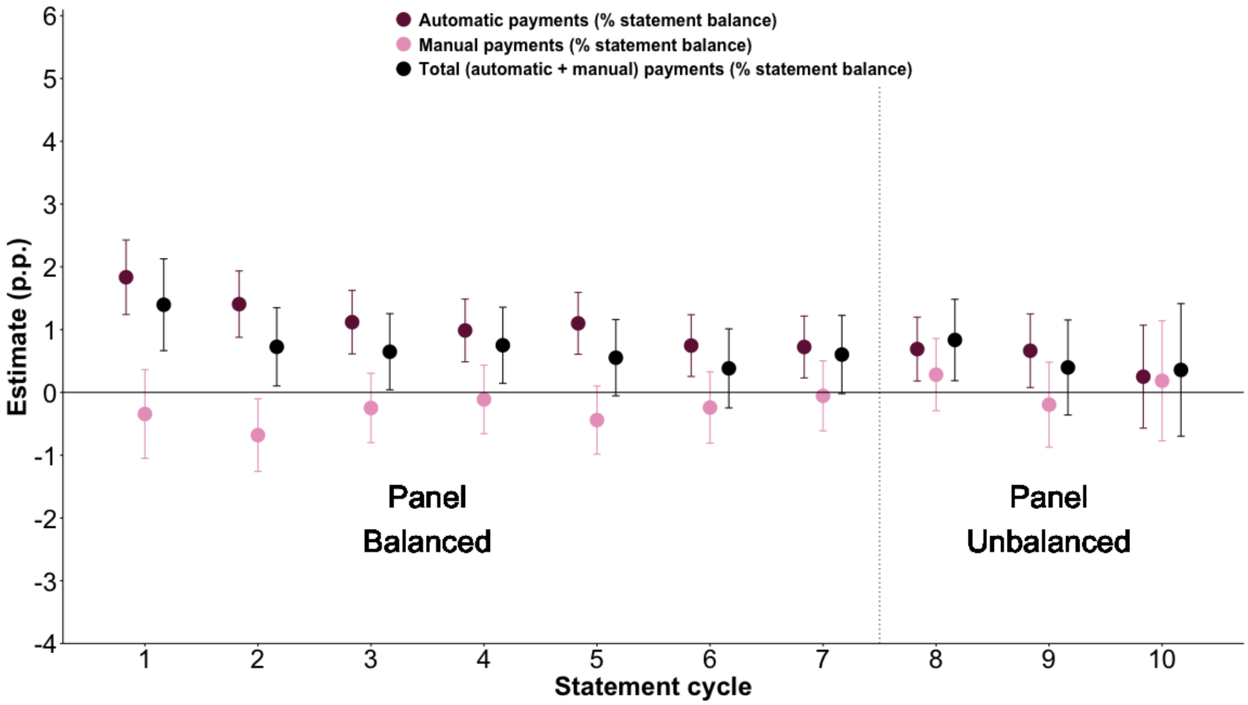
D: Autopay fix amount in excess of minimum (% statement balance) at cycle 7



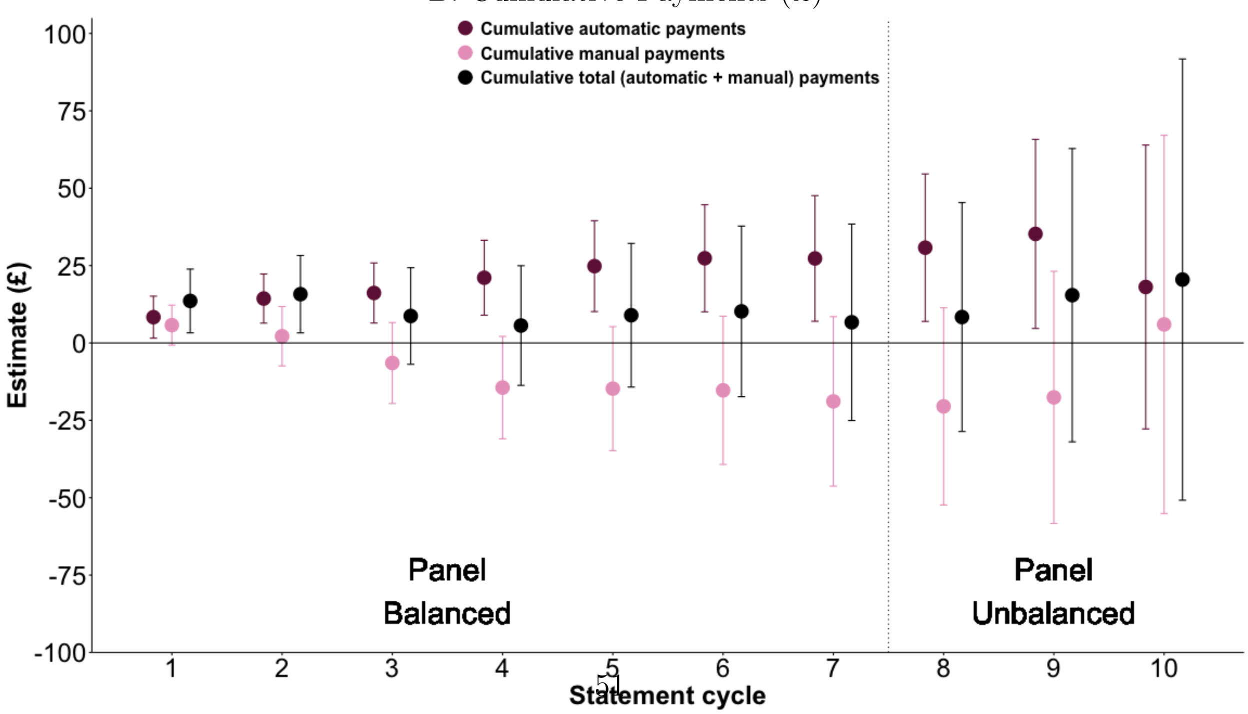
Notes: X-axes of CDFs are right-censored to ease presentation.

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

A: Payments (% statement balance)

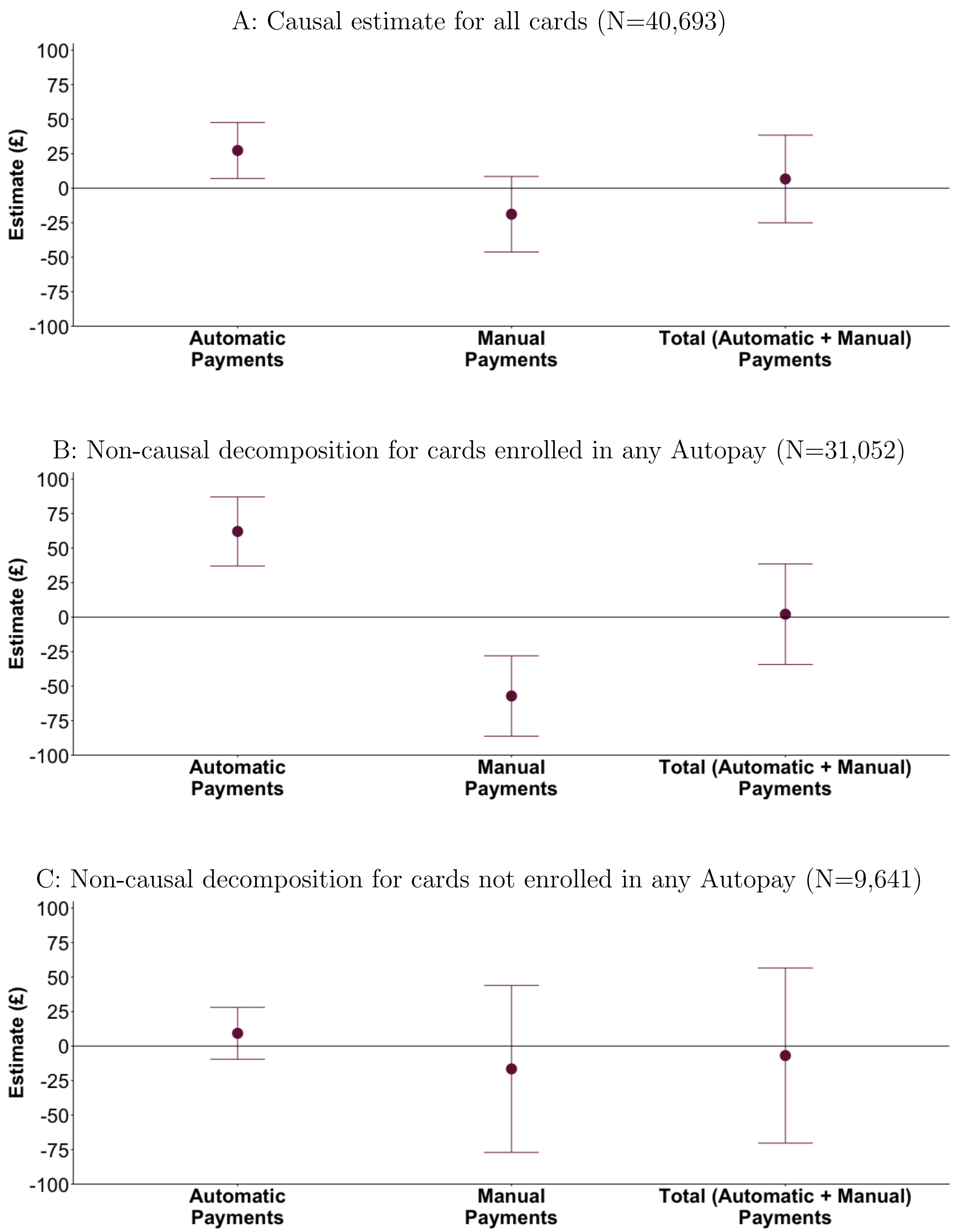


B: Cumulative Payments (£)



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

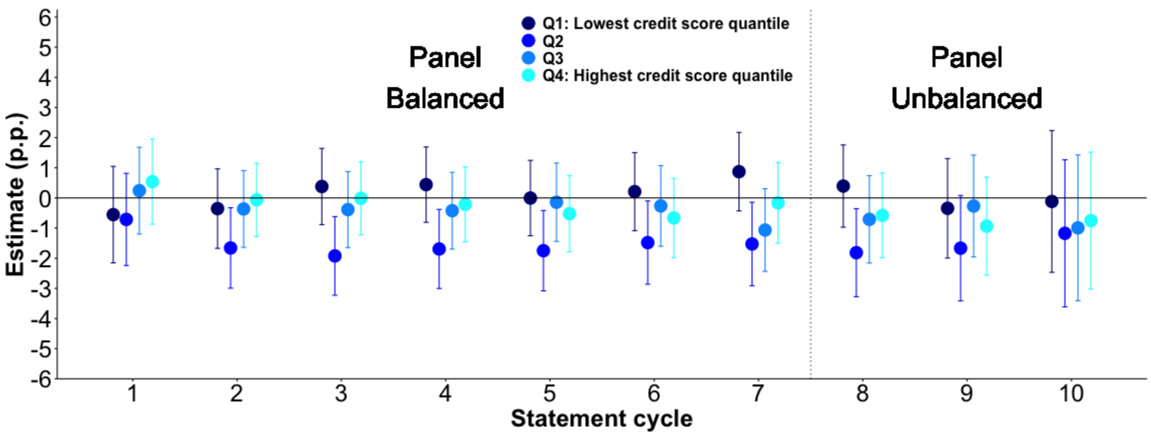
Figure 8: Estimates on cumulative payments decomposed by any Autopay enrollment after seven statement cycles



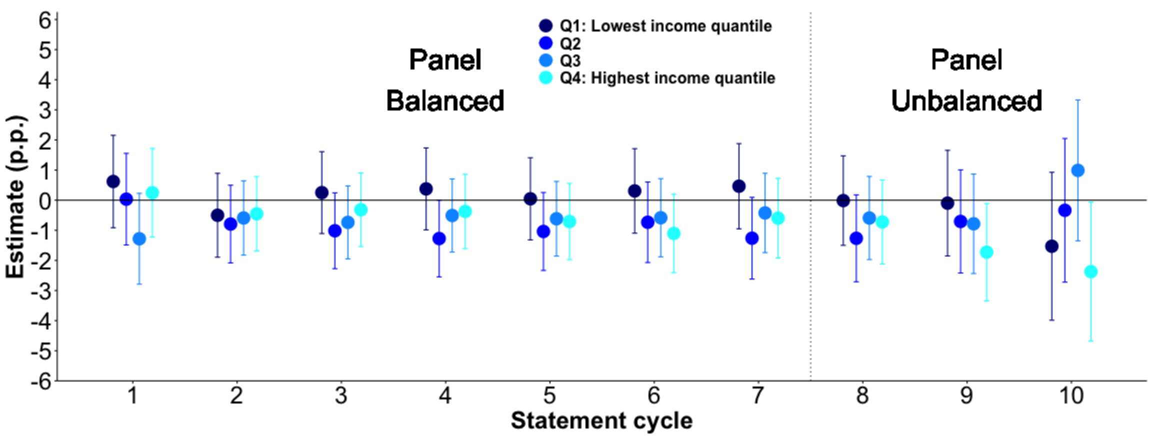
Notes: Panel A is causal estimated treatment effects from coefficients (δ_7) in OLS regression specified in Equation 1. Panels B and C show non-causal estimates (δ_7) from OLS regression specified in Equation 3. Standard errors clustered at consumer-level. Error bars are 95% confidence intervals.

Figure 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

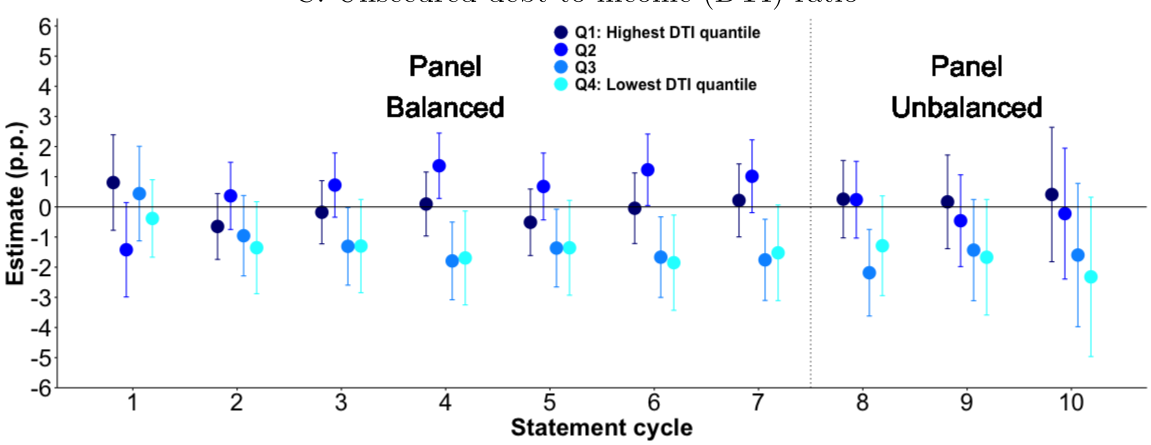
A: Credit Score



B: Income



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

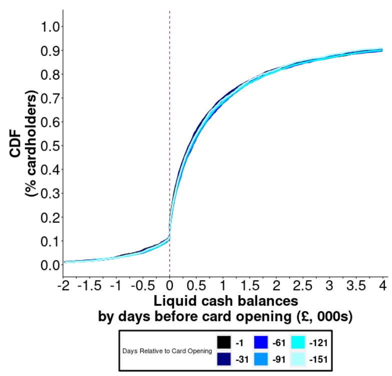


Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Heterogeneous variables calculated from credit file data in month preceding credit card opening (& trial start). Cycle 11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

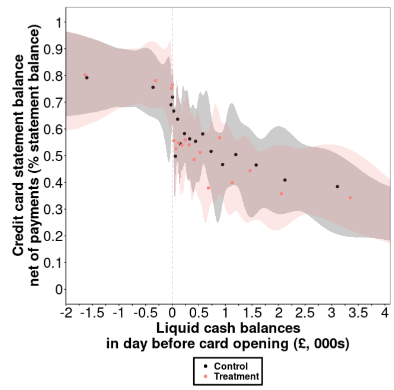
Figure 10: CDFs of liquidity constraints measured before card opening (left hand side panels) and their non-parametric relationships with credit card debt (statement balance net of payments as a % of statement balance) at statement cycle 7, by treatment group (right hand side panels)

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

A CDF

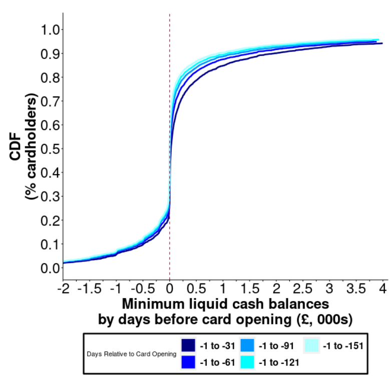


D. Relationship

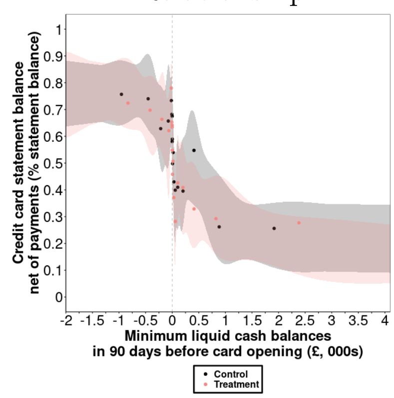


II: Minimum liquid cash balance reached during 90 days before card opening (£)

B. CDF

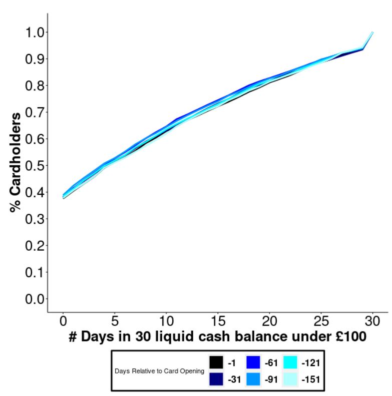


E. Relationship

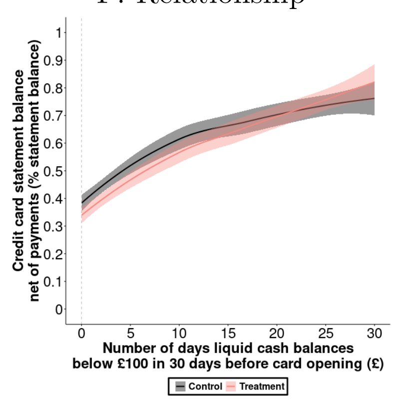


III: Number of days liquid cash balance below £100 in 30 days before card opening (# days)

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 Autopay enrollment outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any autopay	-0.0437*** (0.0041)	[-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***	[-0.0787,	0.0000	0.3012
	(0.0042)	-0.0622]		
Any full payment	0.0040	[-0.0032,	0.2747	0.2397
	(0.0037)	0.0112]		
Any missed payment	0.0038*	[0.0002,	0.0409	0.0369
	(0.0019)	0.0075]		
Statement balance net of payments	-0.0051	[-0.0119,	0.1428	0.6936
(% statement balance)	(0.0035)	0.0017]		
Costs	-0.0003	[-0.0015,	0.6782	0.0111
(% statement balance)	(0.0006)	0.0010]		
Transactions	0.0025	[-0.0036,	0.4199	0.2007
(% statement balance)	(0.0031)	0.0087]		
Share of credit card portfolio	-0.0264***	[-0.0317,	0.0000	0.2012
only paying minimum	(0.0027)	-0.0210]		
Share of credit card portfolio	0.0011	[-0.0054,	0.7340	0.4414
making full payment	(0.0033)	0.0076]		
Share of credit card portfolio	-0.0000	[-0.0025,	0.9701	0.0236
missing payment	(0.0013)	0.0024]		
Credit card portfolio balances	-0.0053	[-0.0115,	0.0896	0.6954
net of payments (% statement balances)	(0.0031)	0.0008]		

*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 Internet Appendix

Internet 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)

Outcome	Significance Thresholds 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)

Outcome	Significance Thresholds 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 Autopay enrollment after seven statement cycles

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

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

Table 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
Any missed payment	0.0040*** (0.0011)	[0.0019, 0.0062]	0.0002	0.0297
Arrears 1+ payments behind	0.0031*** (0.0010)	[0.0011, 0.0051]	0.0024	0.0267
Arrears 2+ payments behind	0.0004 (0.0007)	[-0.0009, 0.0018]	0.5476	0.0110
Arrears 3+ payments behind	0.0002 (0.0005)	[-0.0009, 0.0012]	0.7677	0.0071
Share of credit card portfolio missing payment	0.0004 (0.0007)	[-0.0009, 0.0017]	0.5400	0.0144

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

Table 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 (000s)	0.0034***
	(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 Balances (000s)	−0.0025
	(0.0011)
Credit Card Portfolio Statement Balances Net of Payments (000s)	0.0044***
	(0.0012)
Number Credit Cards Portfolio	−0.0152***
	(0.0010)
Number Credit Cards Portfolio With Debt	−0.0112***
	(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 Autopay enrollment after seven statement cycles

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

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

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

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

Table A17: Second Lender: Average treatment effects for Autopay enrollment outcomes after seven statement cycles

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

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

Table 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

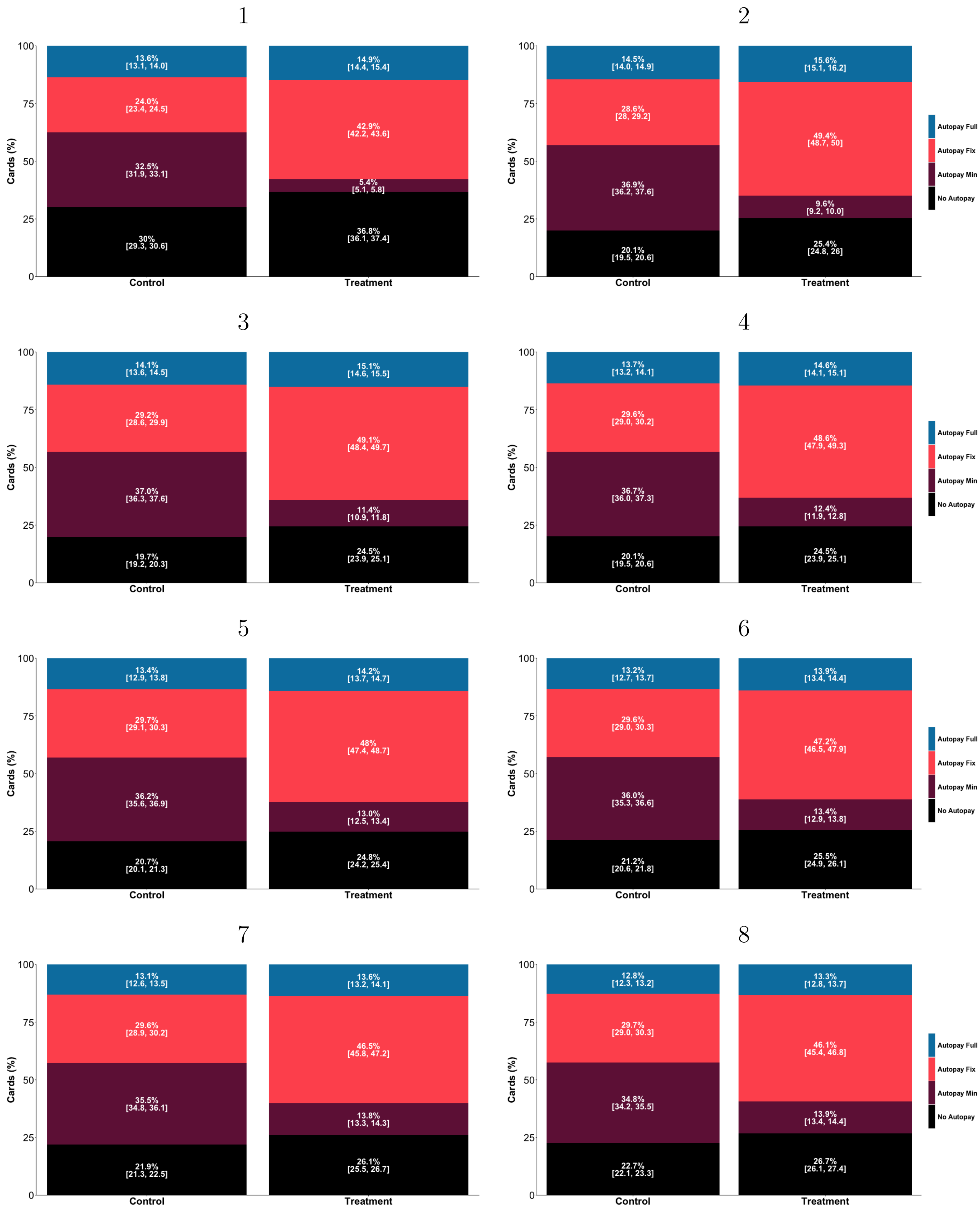
Notes: N = 3,753 consumers. Liquid cash balance is sum of end of day current/checking account and cash saving accounts balances.

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.

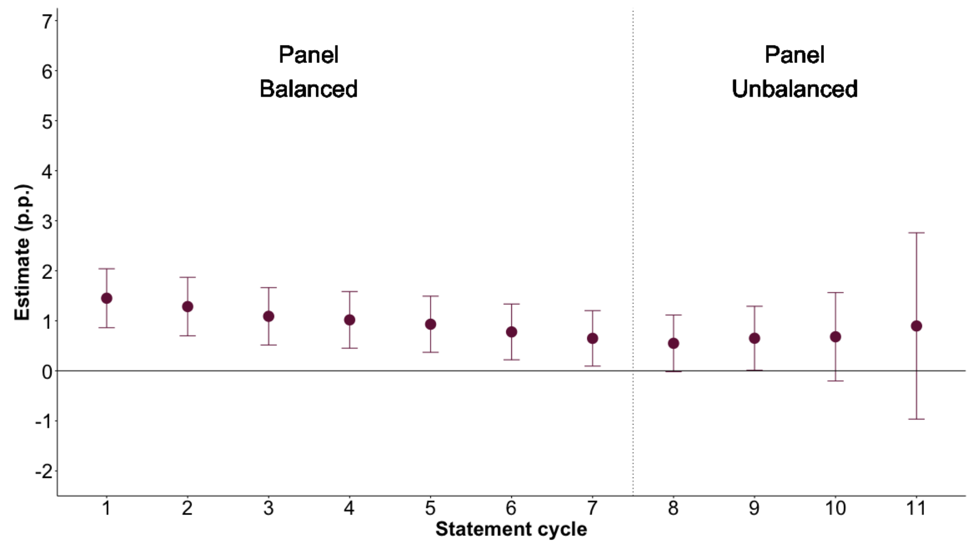
Figure A1: Autopay enrollment for control and treatment groups split by statement cycles one to eight



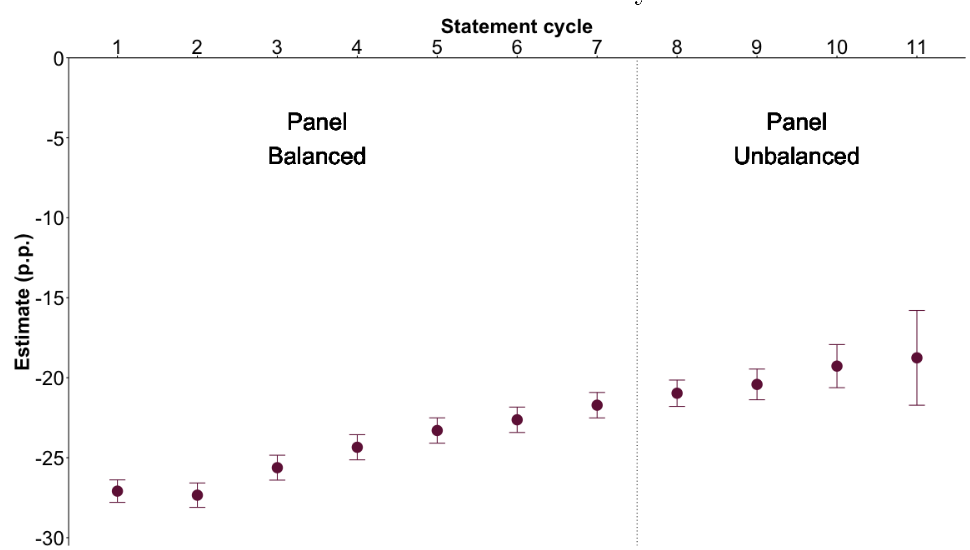
Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ]. Cycle 1 is before all treated cards have had 30 days to experience the treatment. Not all cards are observed in cycle 8.

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

A: Automatic Full Payment

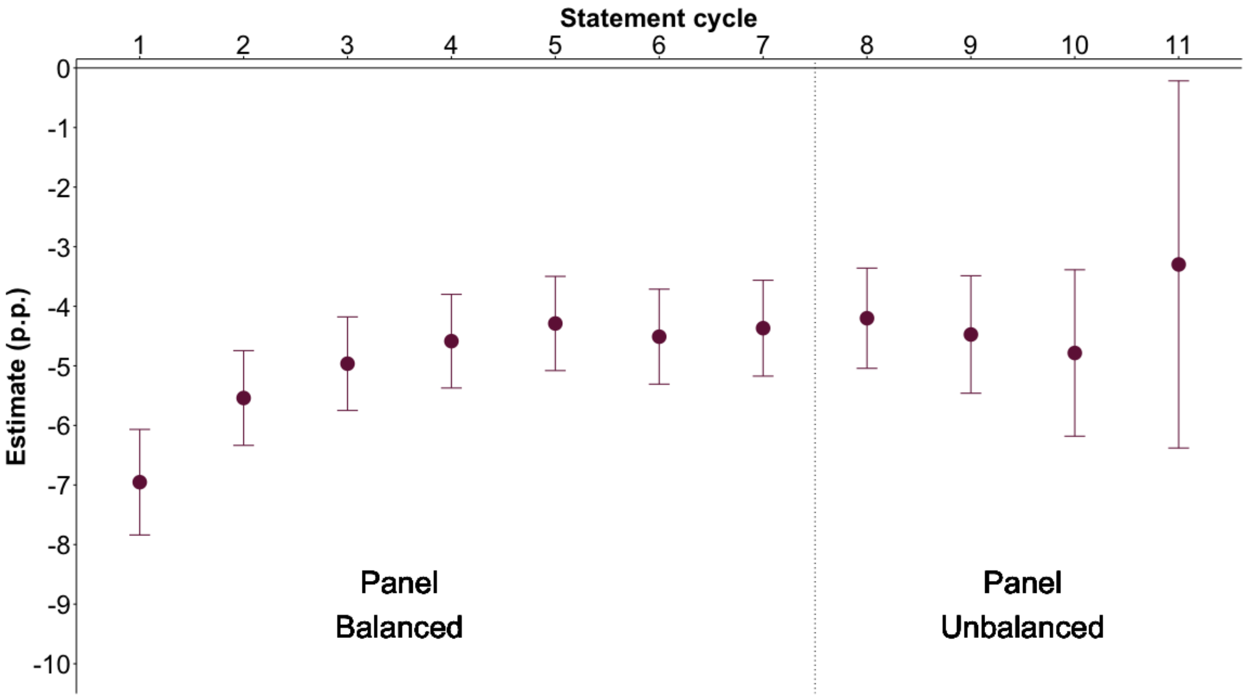


B: Automatic Minimum Payment



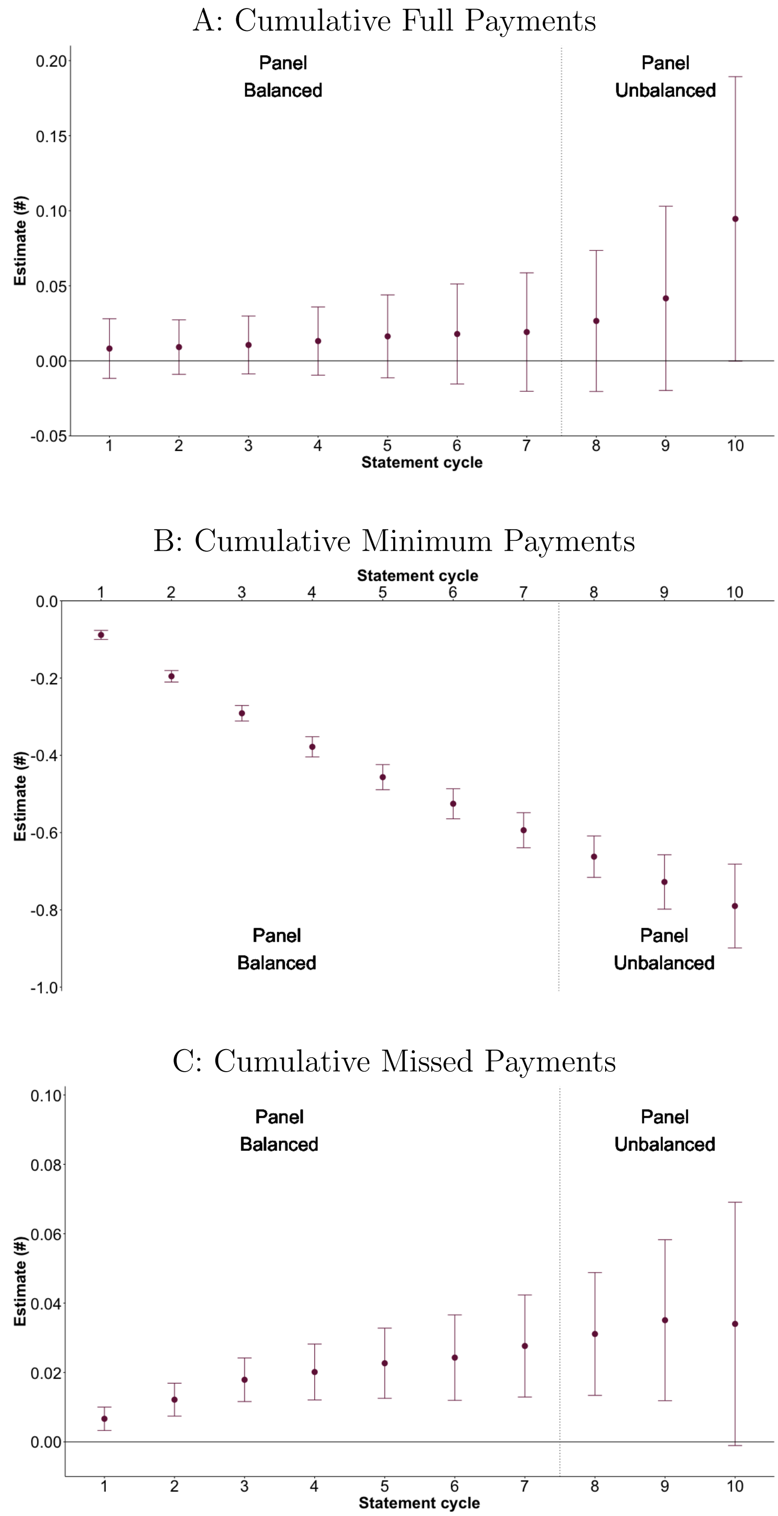
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

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



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

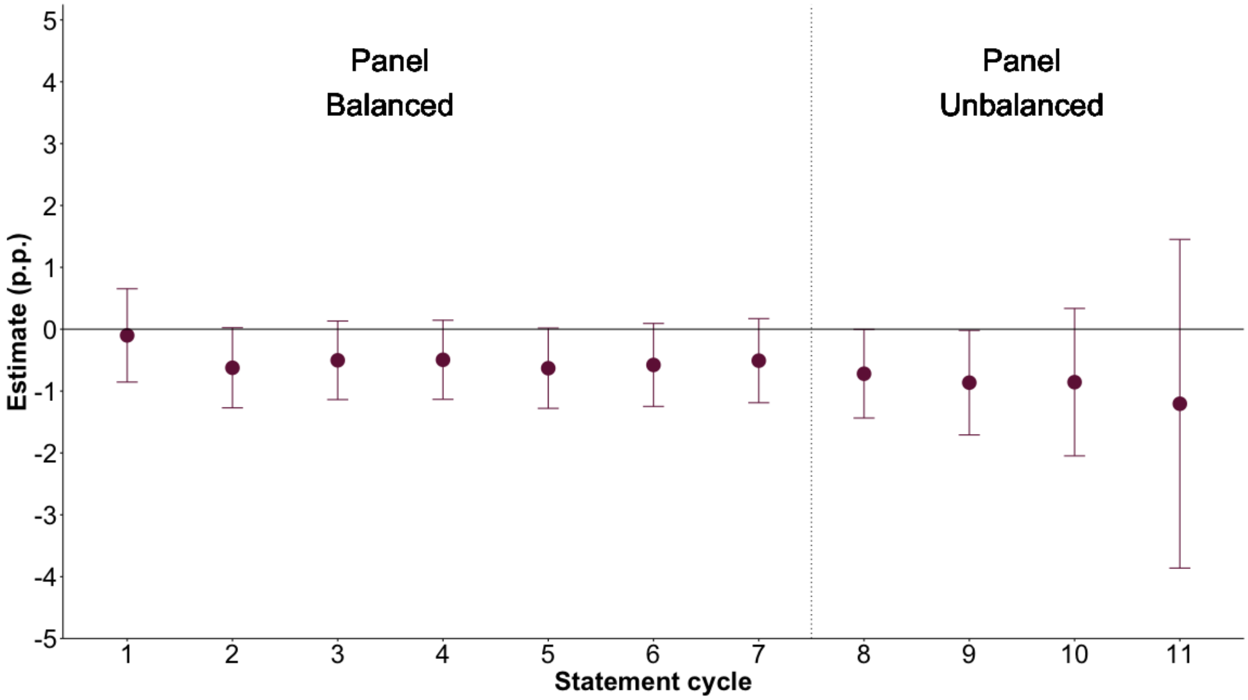
Figure A4: Treatment effects on cumulative number of full, minimum and missed payments across 1-10 statement cycles



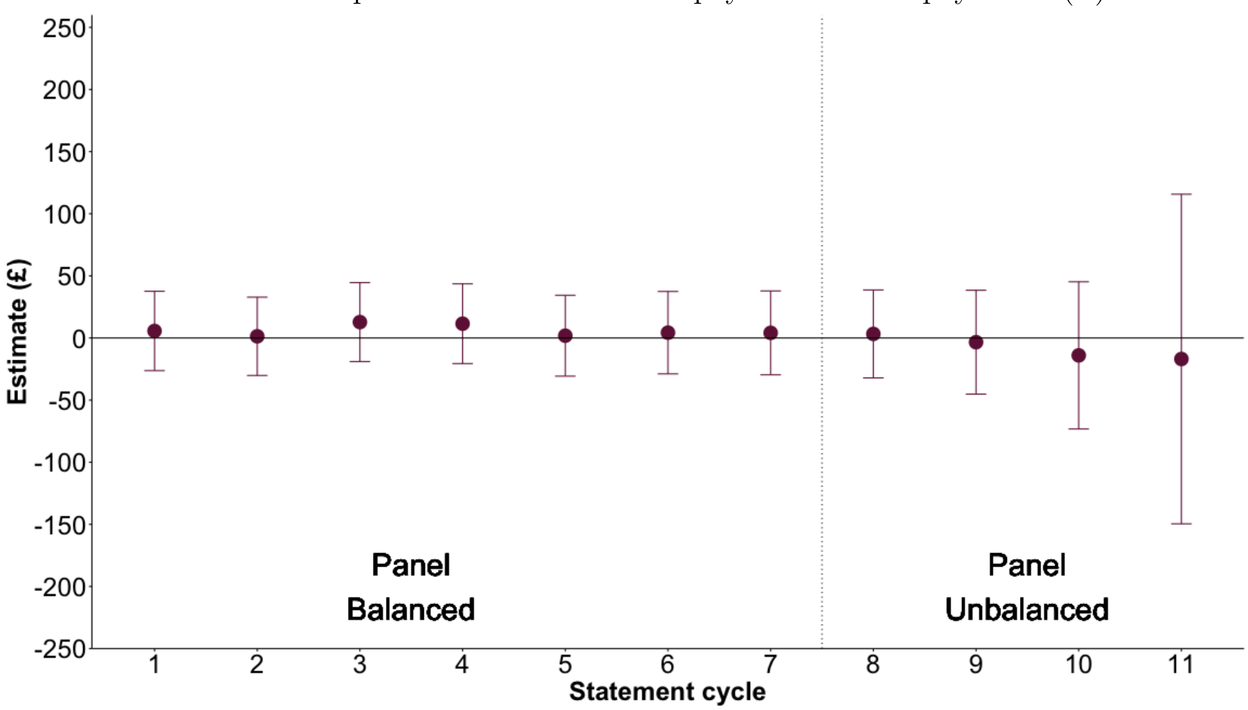
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large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

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

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

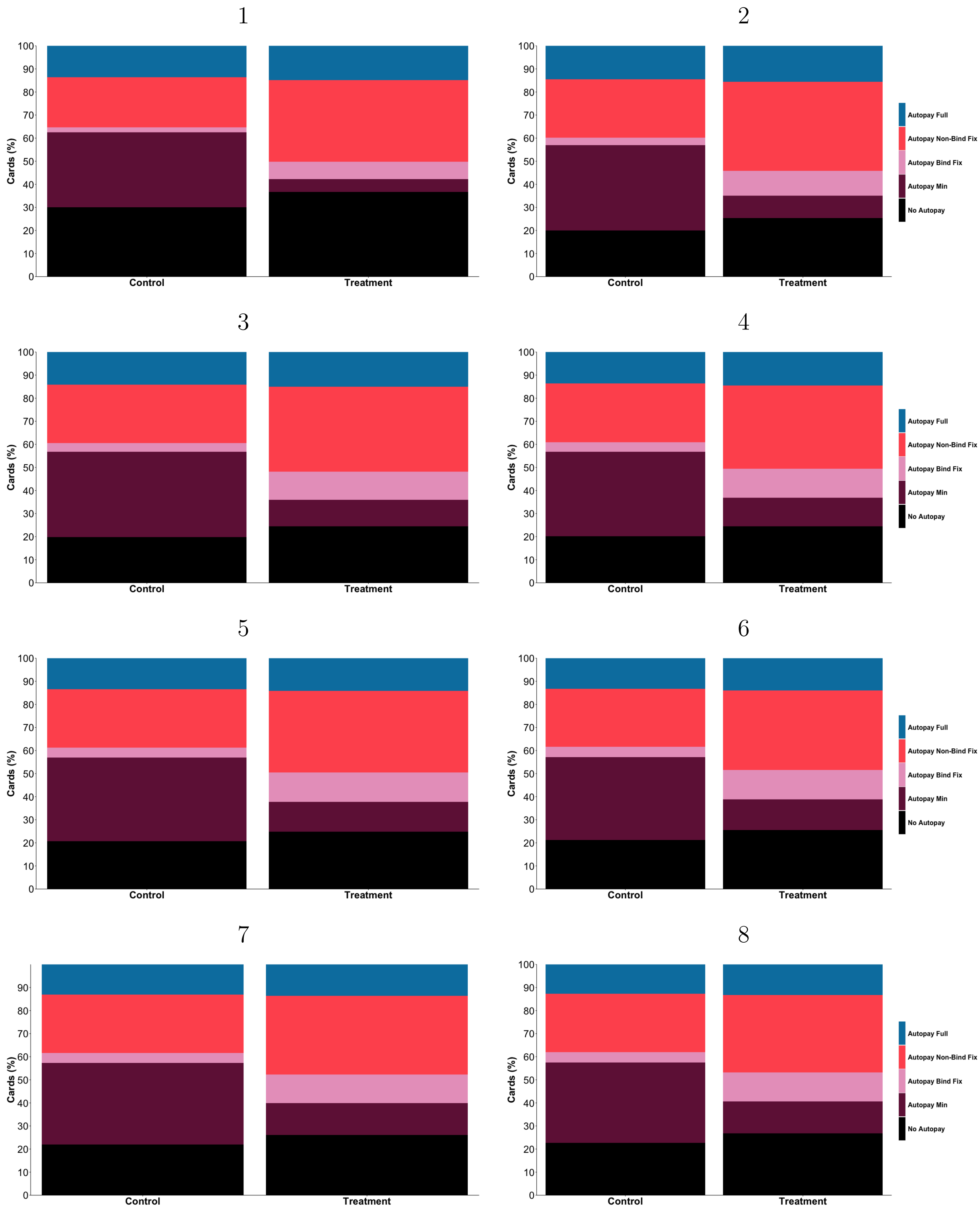


B: Credit card portfolio balances net of payments net of payments (£)



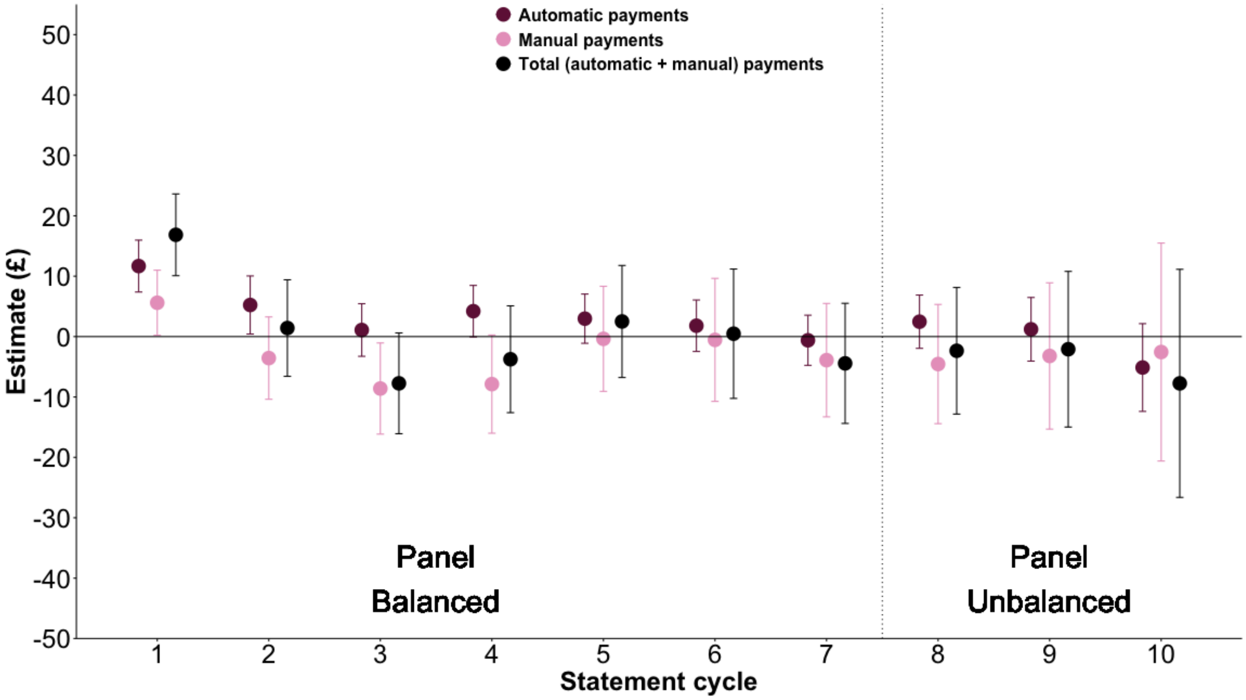
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals.

Figure A6: Autopay enrollment - splitting out automatic fixed payments into those that do and do not bind at the minimum payment amount - for control and treatment groups split by statement cycles one to eight



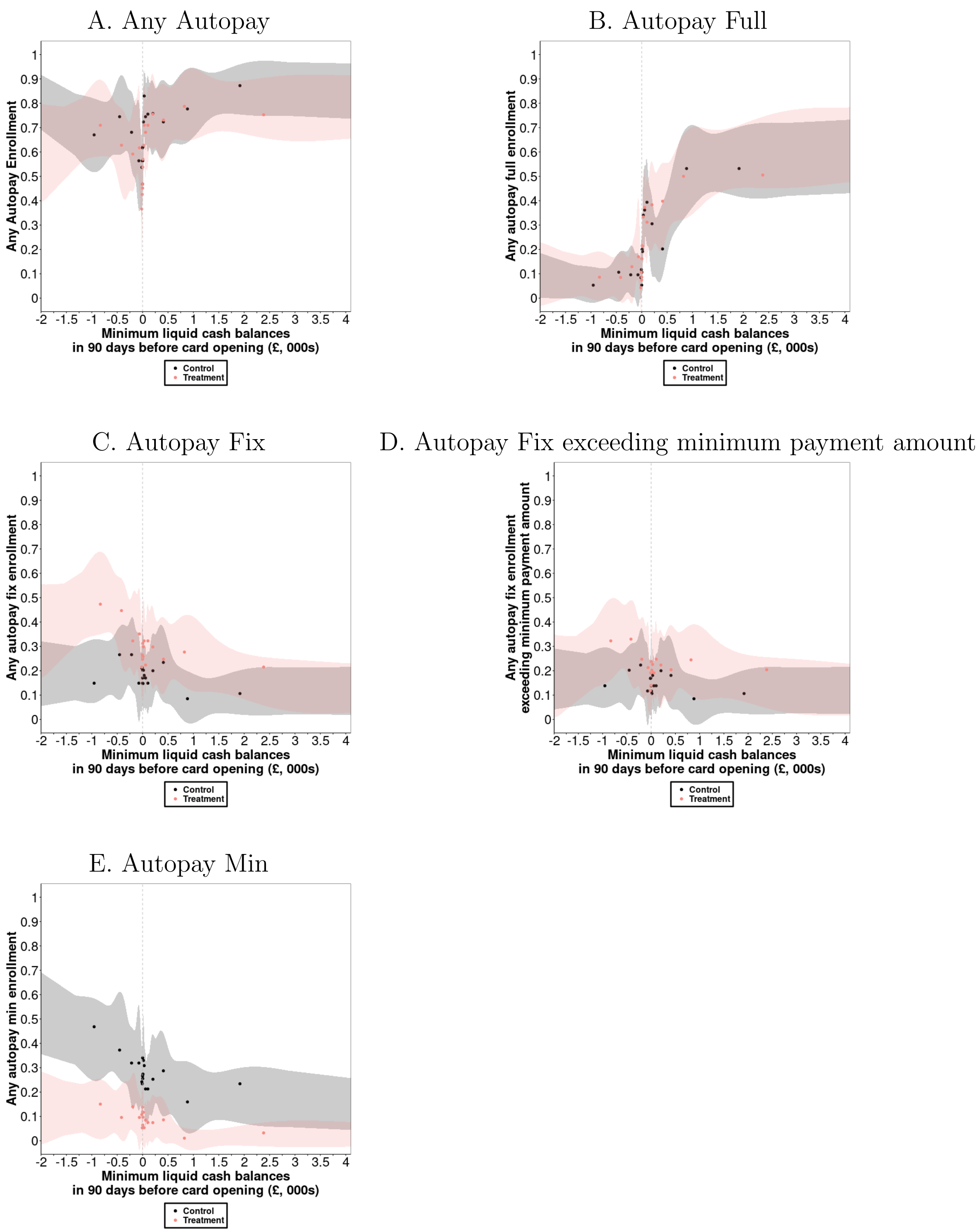
Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ].

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



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

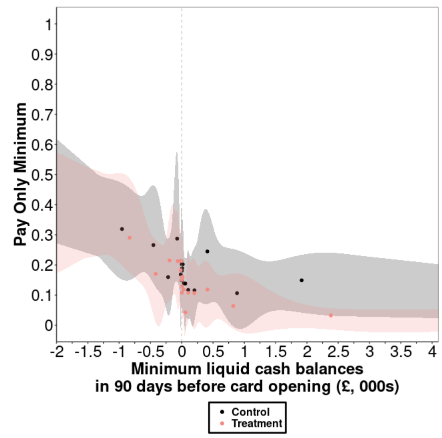
Figure A8: Non-parametric relationship between minimum liquid cash balance during 90 days before card opening with credit card Autopay enrollment at statement cycle 7, by treatment group



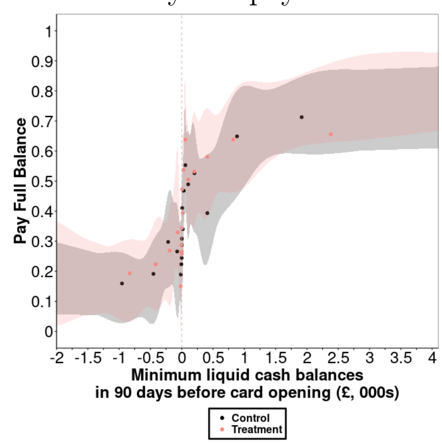
Notes: N = 3,753 consumers. Liquidity constraints are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.

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

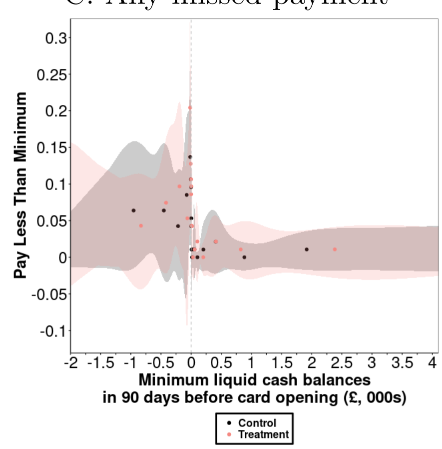
A. Any minimum payment



B. Any full payment

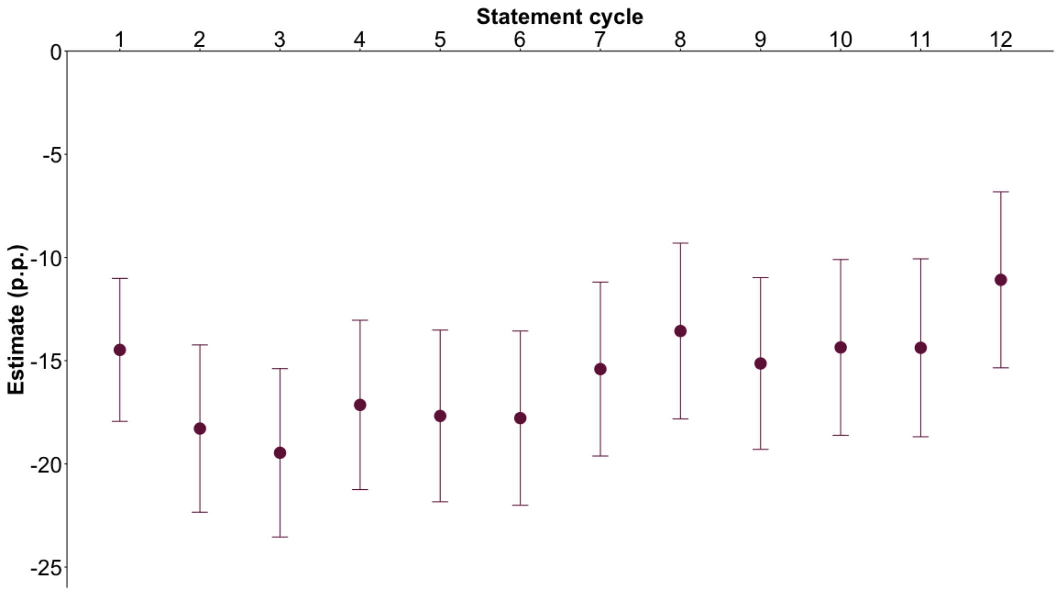


C. Any missed payment



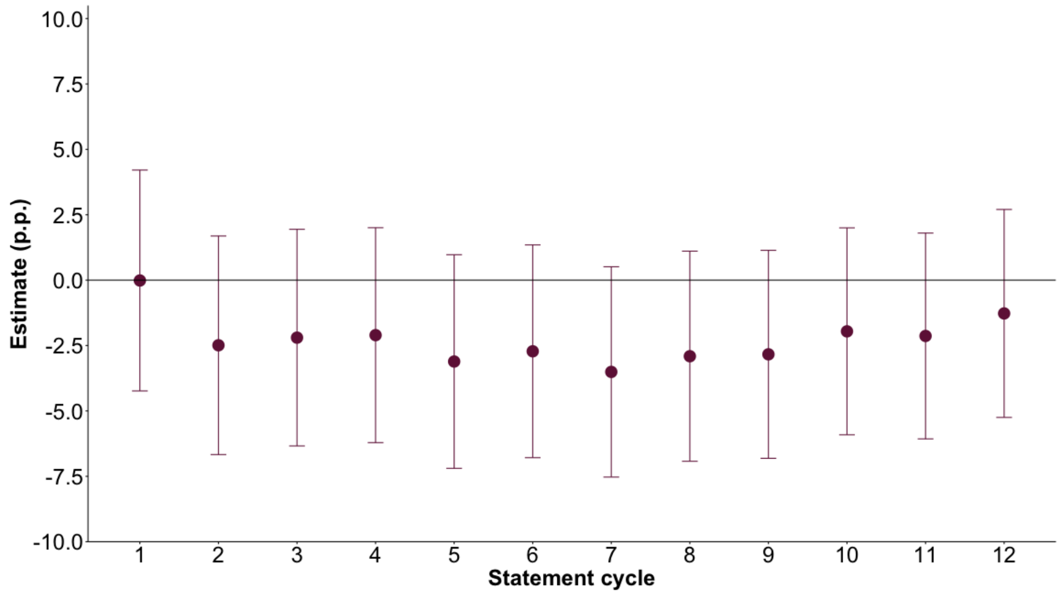
Notes: N = 3,753 consumers. Liquidity constraints are measured before credit card opening. Panels are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes are censored to ease presentation given a fat tail to the distribution of these variables.

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



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 A11: Second Lender - Average treatment effects on credit card debt across 1-12 statement cycles



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