Do Nudges Reduce Borrowing and Consumer Confusion in the Credit Card Market?*

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Abstract

We test the effects of two nudges designed to reduce credit card indebtedness; our study uses field experiments on 183,441 UK credit cardholders. All of the nudges have two components: (i) they explain the negative consequences of making only the minimum required payment and (ii) they recommend and logistically facilitate payments in excess of the minimum required payment. Our first experiment studies nudges that appear on monthly credit card statements. Our second experiment studies letters and email nudges (apart from monthly statements), which are sent to cardholders who have chosen to make automatic minimum payments. Both interventions generate no effect on economic outcomes by the end of our half-year observation period. In a follow-up survey, we find 96% of survey respondents in the control group underestimate the repayment time generated by making only required minimum payments. The nudges substantially reduce the magnitude of this optimistic bias, but the bias remains overwhelmingly common.

JEL Codes: D04, D14, D18, G20, G28, G51.

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I Introduction

The 2009 CARD Act required US credit card lenders to use monthly statements to report (i) the time it would take to repay the cardholder's balance if the cardholder only paid the contractual minimum every month and (ii) the fixed monthly payment that would pay off the balance in three years and the interest costs and savings from such a repayment schedule compared to only paying the minimum (both (i) and (ii) assume the cardholder made no new purchases). These disclosures were implemented with neutral language and had little, if any, effect on credit card repayments (Agarwal et al. 2015, Keys & Wang 2019). Given such ineffectiveness, consumer financial protection regulators could instead use more prescriptive 'nudges' (Sunstein & Thaler 2008, Campbell 2016) that make explicit recommendations to credit cardholders to reduce their debt.

The current paper presents two field experiments (i.e., Randomised Controlled Trials, or RCTs) that test the effects of explicitly encouraging credit cardholders to pay more than the required minimum payment, specifically by both reporting how long it will take cardholders to pay off their balances if they make the minimum payment and by specifically recommending that the cardholders set up fixed automatic monthly payments that will pay off the balances more quickly. Like the less prescriptive nudges in the CARD Act, our more forceful nudges also fail to change cardholders' economic outcomes. After the RCTs were completed, we conducted a survey finding overwhelming consumer confusion among respondents in how long only repaying the contractual minimum would take to repay the cardholder's balance. Our survey revealed that our nudges only partly correct consumer underestimation of repayment horizons and that partial correction did not have a measured effect on cardholder behavior. Our two experiments were designed to directly inform potential policymaking; they were run by the UK financial regulator – the Financial Conduct Authority (FCA) – in collaboration with UK credit card lenders.

Our overarching aim was to use behaviorally-informed interventions to 'nudge' (Sunstein & Thaler 2008) cardholders to repay more than the contractual minimum, and, by implication, to reduce their long-run credit card debt. Paying only the contractual minimum reduces credit card debt by only 1% per month (if no new charges are made on the card) and results in high long-run carrying costs. Cardholders that perpetually carry high levels of debt are profitable for lenders, who therefore do not have incentives to educate/nudge cardholders in ways that might lead the cardholders to lower their debt levels (Financial Conduct Authority 2015*a*). This potentially motivates financial services regulators to require lenders to provide

information to borrowers about the economic cost of credit card debt.

Our first experiment – 'statement nudges' – tests the effects of more aggressive versions of the disclosures required on US credit card statements under the CARD Act. Our RCT methodology (using a sample of 29,683 UK cardholders at one lender) complements the difference-in-difference methodologies used in prior studies of the US CARD Act (Agarwal et al. 2015, Keys & Wang 2019). Like the informational disclosures required by the CARD Act, our nudges display (on the first page of each statement) the years it would take to pay off debt under alternative payment schemes. However, while the CARD Act displays information with neutral language, we use language that specifically recommends that consumers pay more than the minimum payment; in addition, we tell cardholders how to (easily) set up automatic fixed payments that exceed the minimum payment. Moreover, we use graphical displays of information, rather than the tabular form required by the CARD Act. In a further treatment, we test the effect of adding information on the interest costs of borrowing. We find precise zero effects on outcomes measured after 'statement nudges' have been added to six months of credit card statements. This result aligns with earlier research on the lack of efficacy of the disclosure components of the CARD Act (Agarwal et al. 2015, Keys & Wang 2019). Our nudges, which include actionable recommendations, go well beyond disclosure, but we still find zero effect on average economic outcomes (e.g., credit card debt).

Our second field experiment – 'automatic minimum payment nudges' – tests the effectiveness of nudges directed at a sample of cardholders that had set up automatic minimum payments (in the U.K. this is referred to as 'Direct Debit' of the contractual minimum). Cardholders with automatic minimum payments were chosen for this study because it is common for households to stick with this arrangement over the long-run (Financial Conduct Authority 2016) and thereby incur high interest costs (Sakaguchi et al. 2021). For our treatment groups, we graphically display personalized information on the amount of time it would take to pay off debt if no new charges were incurred and only minimum payments were made (typically decades) and contrast this benchmark with three alternative scenarios showing the payment amounts needed to pay off debt in one, two, or three years. As in our first field experiment, our language specifically recommends taking action to stop making minimum payments and instead set up automatic fixed payments that exceed the minimum payment (and we explain how to easily do this). In a further treatment we test the effect of adding information on the interest costs of making the minimum payment to the nudge. We also randomly vary sending an additional reminder.

The second field experiment was implemented with three lenders totalling 153,758 UK credit cards enrolled in automatic minimum payments. For two lenders we sent the nudges by letter and for one lender via email – these communications were all separate from the

monthly credit card statements. Cardholders appear inattentive to our 'automatic minimum payment nudges'. On average, 1-2 in 100 cardholders initially (within a couple of months) respond to the nudge by enrolling in automatic fixed payments; these magnitudes match related results reported for US data by Keys & Wang (2019). We interpret these magnitudes as small in absolute terms but large in relative terms since almost no one in our control group switches to automatic fixed payments. Our results are consistent across lenders, across emails or letters, and whether or not interest payment information was included.

While the 'automatic minimum payment nudges' initially appear to increase average payments and reduce credit card debt such gains are not sustained over time with null effects after nine completed statement cycles. We see no additional spending on the card which could account for this combination of outcomes. Instead, it appears the nudges bring forward the timing of manual payments (made in addition to cardholders' automatic payments) rather than changing the cumulative amount of payments and outstanding debt. One treatment adding a reminder has some evidence of debt reduction but it is not replicated across lenders and is not robust across outcome measures.

We find little heterogeneity in our results for the 'automatic minimum payment nudges' across variables that regulators or lenders could readily use to target communications: credit scores and past credit card behaviors. We find some weak evidence that cardholders with balance transfer debt (debts that had been transferred from one card to another) appear most likely to respond. This is an interesting finding as balance transfer debts typically have 0% interest rates: therefore the cardholders who would potentially benefit most from the nudge through interest savings – those without balance transfers – appear to be the least impacted by the nudge.

We designed a survey for cardholders of one lender in the 'automatic minimum payment nudge' experiment. Our survey is designed to help understand the reasons why cardholders enrolled in automatic minimum payments make repeated minimum payments and why the treatments did not change long-run economic outcomes. The response rate to the survey was low and thus we treat the precise estimates with caution. The survey implies that cardholders have mistaken beliefs – being confused about the implications of credit card minimum payments. In a hypothetical credit card repayment scenario, 96% of respondents underestimate how long only paying the minimum would take to fully clear debt. Underestimates are often substantial: although the correct answer is 18 years and 9 months, 38% of respondents report full repayment will take less than three years and 65% report full repayment will take less than five years. Our finding that consumers have mistaken beliefs with little understanding of credit card minimum payments is consistent with Soll et al. (2013) and contemporaneous research by Seira et al. (2017) in Mexico. Such dramatic underestimation

of amortization times closely relates to prior evidence showing a lack of understanding of other credit card features – especially underestimating the costs of credit card borrowing due to linearizing exponential functions (Stango & Zinman 2009, 2011) – but it is also related, more broadly, to low levels of financial literacy (Lusardi & Mitchell 2014).

One potential explanation for our field evidence is that consumers did not understand or remember the treatments. Our survey evidence helps to rule out this explanation. Although 93% of respondents who received the nudges still underestimate how long only paying the minimum will take to repay debt, the treatment significantly reduces how biased respondents' underestimates are. For example, treatment increases the average expected amortization time from 70.6 to 84.5 months (a 20% increase) and reduces the fraction who incorrectly expect amortization to be under three and five years by 6 and 9 percentage points respectively. The treatment letters were sent over a year before the survey and thus such effects appear persistent. We interpret these results as consumers understanding and remembering our nudges but with these being insufficiently powerful to prompt changes in borrowing behavior.

Our results contribute to a growing literature studying consumer responsiveness to nudges on financial behavior. Our results are broadly consistent with the lack of consumer responses to new credit card information in the US (e.g. Agarwal et al. 2015, Keys & Wang 2019) and Mexico (Seira et al. 2017). The inability of information to change existing behavior is also found in much simpler financial decisions such as cash savings (Adams et al. 2021) and those with higher financial stakes such as mortgage refinancing (Keys et al. 2016). While such informational interventions initially appear attractive to regulators, ex-ante tests such as studied here enable sophisticated regulators to avoid implementing policies that impose costs on firms without achieving their desired aims of yielding offsetting consumer protection benefits. [1]

In a broad set of domains, a variety of recent studies (e.g. Jachimowicz et al. 2019, DellaVigna & Linos 2020) have shown nudges to have smaller effects than previously documented. Even small effects of nudges can still be net beneficial if they outweigh the relatively low costs of firms to implement - which is typically a one-time fixed cost with limited variable costs (Benartzi et al. 2017, DellaVigna & Linos 2020). However, there is a growing view that behaviorally-informed nudges are insufficient to achieve policy goals (e.g. Campbell 2016, Loewenstein & Chater 2017, Laibson 2020). Indeed Guttman-Kenney et al. (2021) shows in a field experiment on credit cardholders that even more intrusive nudge interventions using

[1] Such an approach is in line with the discussion in Soll et al. (2013) that, given uncertainty in what actually changes credit cardholder behavior, it is desirable to test a variety of potential approaches through field experiments before deciding whether to impose new regulations.

choice architecture that is highly effective at changing proximate choices (e.g. automatic payment enrollment) is unsuccessful at changing more distal, real, economic outcomes (such as debt) due to offsetting consumer responses.

The paper proceeds as follows. Section II briefly explains the first experiment on 'statement nudges': experimental motivation (II.A), design (II.B), and results (II.C). Section III introduces the second experiment on 'automatic payment nudges' explaining automatic minimum payments (III.A) and the nudge experiment's design (III.B). Sections IV and V explain the data and empirical methodology for analysing the nudge. Section VI presents the results of the automatic payment nudge: its proximate (VI.A), long-term (VI.B), and heterogeneous (VI.C) effects. Section VII offers survey evidence to understand the automatic minimum payment nudge results. Section VII.A describes the survey's design while VII.B tests consumer understanding of minimum payments and VII.C the treatment effects of nudges at improving understanding, VII.D examines the reasons consumers report using automatic payments and VII.E their self-reported financial distress & stated preferences. Finally, section VIII offers a brief concluding discussion.

II Experiment 1: Statement Nudges

Our first experiment is motivated by US statement disclosures. We conduct an RCT field experiment with one UK lender to test refined designs intended to nudging cardholders to repay their debt faster. In this section we explain the motivation, design and results of this first experiment.

II.A. Experimental Motivation

The 2009 CARD Act required US credit card lenders to provide information to borrowers in their monthly credit card statements.[2] The Act prescriptively requires the information be displayed in a 'Schumer Box': a table displaying the interest costs of credit card borrowing if the cardholder only makes the contractual minimum payment with comparisons to costs and the payment amount to repay debt in three years (these calculations assume no further spending on the card). These disclosures use neutral language. Difference-in-difference studies (Agarwal et al. 2015, Keys & Wang 2019) have shown this CARD Act disclosure causes slight bunching at the three year scenario but overall had little, if any, change in repayment behavior - with an upper bound of $57-62 million per year (trivial given the size

[2]Credit Card Accountability Responsibility and Disclosure (CARD) Act of 2009

of the US credit card market) but their confidence intervals indicate there may have been no effect.

Soll et al. (2013) concludes that alternative designs may be more effective replacements for the existing CARD Act disclosures, but would benefit from testing a variety of treatments through field experiments – something not possible in the US without a change in legislation. Our UK context provides an environment to test alternative information on statements without such US legal constraints. There are no existing requirements for UK credit card lenders to display any Schumer-box style statement disclosures and lenders did not voluntarily do so. The UK financial regulator (the FCA) wanted to test whether statement nudges would be effective in a UK setting, to inform potential policymaking.

II.B. Experimental Design & Methodology

We use a randomized controlled trial (RCT) field experiment to test statement nudges – this methodology complements the difference-in-difference methodologies used in prior studies of the US CARD Act. Given the ineffectiveness of US CARD Act statement disclosures and the UK and US credit card markets being similar we expected there to be a low chance statement disclosures would prove effective in the UK. Therefore we did not exactly replicate the CARD Act statement disclosures in our experiment and instead designed nudges encouraging consumers to make particular choices.

Our field experiment with one UK credit card lender trialled two treatments derived from the US CARD Act statement disclosures.[3] The treatments were nudges displayed on the front page of cardholders' statement for six statement cycles. Unlike the CARD Act they used graphical rather than tabular information in order to make it more accessible and to provide a visual cue of how much longer it would take to repay debt if a cardholder only pays the minimum payment. The graphic includes a range of repayment scenarios: minimum payment repayment duration compared to the payment amount required to clear debt in one, two, and three years. Multiple, short amortization scenarios were included to counter the unintended effect of the CARD Act's single three year repayment scenario leading some cardholders to reduce their payments to this amount - possibly due to cardholders interpreting the single scenario as a recommendation (Agarwal et al. 2015, Hershfield & Roese 2015, Keys & Wang 2019). While the CARD Act displayed information using neutral language, we purposefully use language encouraging consumers to repay their debt faster and highlighting a method of automatic payments they can easily enroll in to help them to do so.

[3]The lender participated voluntarily and the FCA's statutory powers enabled the data sharing required to analyze this experiment.

Our first treatment 'Time to Repay' (Figure 1, Panel A) shows the duration of time to repay debt under different payment scenarios. The second treatment 'Time + Cost to Repay' (Figure 1, Panel B) adds cost information to the scenarios as making the financial stakes salient could increase cardholders' responsiveness.[4] We calculate scenarios based on balances closest to a cardholder's actual credit card balance but infrastructure constraints prevented them being personalised to each cardholder's precise balance. The control group received their normal statements without these disclosures.

Despite these alterations we anticipated that this intervention would have little, if any, effect on borrowing behavior given the prior US evidence of CARD Act statement disclosures found economically small effects (Agarwal et al. 2015, Keys & Wang 2019).

One key reason for our pessimism was because many cardholders do not open or read their statements (as previously noted in Agarwal et al. (2015)). To increase the chances of detecting an effect of the disclosures, the sample for this trial excluded cardholders enrolled in automatic payments (known as 'autopay' in the US and 'Direct Debits' in the UK) as we believed these to be the least likely to read statements.

Our data and empirical methodology are pre-registered and identical to that used in Experiment 2 and we explain these in detail in Sections IV and V. We randomly allocate credit card accounts to the control and treatment groups. After applying basic exclusion criteria (see footnote for details) 29,683 cardholders were in this experiment with balance in covariates between control and treatment groups (Online Appendix Table A1).[5] Summary statistics are displayed in Table 1. Control and treatment groups are balanced on observable, pre-trial credit card characteristics except for a small imbalance on credit score in one treatment. We include pre-registered controls in our regressions (see Section V for details) to recover balance and improve precision of our estimates. Regression results with controls are consistent with those comparing unconditional means. We evaluate the trial using ten, pre-registered primary outcomes after six statement cycles.

II.C. Experimental Results

After six statement cycles both statement nudges have precise zero effects across our ten primary outcomes. These results are shown in Table 2 (Online Appendix Table A6 shows

[4]Raynard & Craig (1993) finds adding cost information can help improve consumers to estimate loan durations. More generally, Chetty et al. (2009) shows increasing the salience of taxes results in larger behavioral responses.

[5]Before randomization we excluded cards in arrears, forbearance, dormant, open for less than six months, enrolled in automatic payments, having repaid balance in full in the last three statement cycles or with balances below £50. Exclusion criteria for cards which were closed, entered arrears or become dormant were reapplied after randomization and before treatment.

consistent results using t-tests from unconditional means). Such null results are consistent with the economically small effects of the CARD Act statement disclosures previously found (Agarwal et al. 2015, Keys & Wang 2019).

We found no effects of the intervention changing the likelihood of paying off debt in full, making minimum payments, missing payments, costs of borrowing, transactional use of cards or outstanding debt net of payments. Our tests were well-powered such that we can be confident that either there was no effect of either treatment or any effects are so small that they would not be economically meaningful. As our results were unambiguous we did not pursue more detailed analysis of this experiment except for a couple of simple robustness checks which also found precise zero effects (Online Appendix Table A5).

Unfortunately, the lender was unable to record whether consumers had opened their statements so we cannot distinguish between whether consumers did not read the statements or that they read them but the information had no impact on their behavior. Other authors have hypothesized that the former factor – failure to even read their statement – is a substantial part of the explanation. Agarwal et al. (2015) posit that providing similar information outside of statements may prove to be effective. The rest of this paper focuses on our second experiment that tests standalone, behaviorally-informed information ('automatic minimum payment nudges').

III Experiment 2: Automatic Minimum Payment Nudges

Our second field experiment are RCTs with three UK credit card lenders testing standalone, behaviorally-informed information 'nudges' (Sunstein & Thaler 2008) designed to encourage a group of cardholders who commonly and persistently pay only the minimum payments to change their repayment behavior.

III.A. What are Automatic Minimum Payments?

UK cardholders have the option to enroll into automatic payments – these are known as 'autopay' in the US and 'Direct Debits' in the UK – set up to pay their credit card bill each month by attempting to take a payment directly from the cardholder's bank ('current' or 'checking') account subject to it having sufficient funds available. Automatic payments act as insurance against forgetting to make a payment – something that may otherwise result in incurring a late fee or adverse credit file impact (Agarwal et al. 2008, Medina 2021, Gathergood et al. 2020, Sakaguchi et al. 2021). Cardholders with automatic minimum payments can make additional manual – typically online – payments, however, such payments

are infrequent.[6] Automatic payments are used across a broad range of domains beyond credit cards (e.g. mortgages, rent, mobile phone and utility bills).

There is no requirement for cardholders to enroll in automatic payments but if they want to do so there are three options available. The most popular type of automatic payment option selected by cardholders are for 'automatic minimum payment' which are set to cover the contractual minimum each payment cycle. Other options are 'automatic full payment' to pay their balance in full each month and 'automatic fixed payment' which pay the higher of a fixed pound amount – of cardholder's choosing – or the contractual minimum.

The automatic minimum payment option may appear an attractive option to cardholders as it ensures they are meeting their contractual payment obligations. The contractual minimum payment due on UK credit cards is typically calculated as the sum of interest and fees and 1% of outstanding balance.[7] If this sum is less than £5 then the contractual minimum is £5.

However, by automatically making payments each month a cardholder may become inattentive and not regularly actively engage with their credit card debt and the interest costs they are incurring (Ausubel 1991, Sakaguchi et al. 2021). Interest costs are much less salient than other costly events such as missing a payment which result in fees and alerts and frequently prompt changes in consumer behavior (Gathergood et al. 2020). And given that only 1% of the balance is paid down by the minimum payment, cardholders are barely reducing their debt balance.

It is common for cardholders enrolled in automatic minimum payment to persistently only pay the contractual minimum with sizeable interest costs accumulating.[8] Of all UK cardholders making nine or more minimum payments in a year, 75% had automatic minimum payments (Financial Conduct Authority 2016). 43% of total interest and fees across UK credit cards is held by those on automatic minimum payments (Sakaguchi et al. 2021). Such borrowing patterns are profitable for lenders who therefore do not have incentives to provide information to cardholders to make more informed borrowing decisions that may result in lower debt (Financial Conduct Authority 2015*a*). Such findings motivated the UK financial regulator (FCA) to test nudges as potential policies to encourage cardholders using automatic minimum payments to pay more than the minimum.

[6]There are processing time lags in UK automated payments which means that if a manual payment is made shortly before an automated payment is already due to go through it would not replace it.

[7]As with US credit cards (Agarwal et al. 2015, Keys & Wang 2019), UK credit cards have a grace period where no interest is charged on new purchases until the statement's payment due date. After this grace period any balances incur interest costs typically calculated as the annual interest rate / 365 x average daily balance x days since last statement. 18.9% was the average interest rate at the time of our study.

[8]Patterns of persistent credit card borrowing are also common in the USA (Keys & Wang 2019, Grodzicki & Koulayev 2021) but the relationship with automatic minimum payment enrollment has not been studied.

In general, the welfare effects of credit card borrowing are ambiguous, however, such patterns of repeatedly only paying the contractual minimum with high interest costs accumulating (typical APRs are around 20%) are unlikely to be optimal as they imply implausibly high discount rates in standard economic models. A pattern of repeated minimum payments may be consistent with present focus and procrastinating on paying down debt (Laibson 1997, Meier & Sprenger 2010, Heidhues & Kőszegi 2010, Kuchler & Pagel 2021).

Persistently carrying credit card debt is recorded on credit files and thus can also adversely limit the amount and price consumers are able to borrow at now or in the future (especially if an economic shock results in them defaulting on their credit card debt).[9] Aside from the financial costs, survey measures of struggling to repay debts have been found to have strong negative relationships with psychological well-being (Gathergood 2012, Richardson et al. 2013, Gathergood & Guttman-Kenney 2016).

III.B. Experimental Design

We designed 'automatic minimum payment nudges': standalone communications providing behaviorally-informed information to existing cardholders enrolled in automatic minimum payments. Our designs were informed by qualitative consumer testing and feedback from lenders and consumer organisations to ensure people would understand their content. Without our nudges, these cardholders do not receive information on the implications of their repeated minimum payments and thus their payment choices could reflect a lack of awareness. [10]

We test two treatments: 'Time to Repay' – shown in Figure 2 – and 'Time + Cost to Repay' – shown in Figure 3 – with the control group receiving no new communication. The key salient feature of both of these nudges is a large, personalized graphic designed to attract attention to prompt cardholders to actively engage with their credit card debt. The graphic shows how long it would take to repay their outstanding credit card debt if the cardholder continued to pay only the minimum payment and contrasts this with three scenarios for the fixed payment amounts required to repay debt in one, two, or three years. This graphic is colourful and large to grab attention: consumers in our qualitative research described the information displayed as 'shocking'.

[9] UK credit files record the amount owed on credit cards both before and after payments. They also have fields to record if the cardholder has only paid the minimum.

[10] Cardholders are sent a communication after six consecutive minimum payments but there are no detailed requirements for what information this communication should detail. It does not have to (and in the examples we saw did not) show the implications of minimum payments, costs incurred, or provide any specific guidance to the cardholder. Given the patterns of persistent minimum payments observed in the market this status quo communication does not appear to be a salient event.

The 'Time + Cost to Repay' treatment adds information on the projected interest costs under the scenarios presented in the first treatment (only minimum payments, pay off in one, two, or three years). Cardholders may become especially attentive to being prompted into action once they become aware of the interest costs they are incurring as a result of their repeated minimum payments.

Communications in both treatments are personalized using calculations from the credit card's previous statement. We do not display the minimum payment amount in pounds to prevent consumers anchoring to this value (Stewart 2009). We also took this decision to prevent cardholders being confused by how much their contractual minimum payment is that month as it may be different from the minimum payment used for the scenarios.

The nudges also highlight one easy mechanism cardholders can use to achieve their repayment goals is by switching their automatic minimum payment to an automatic fixed payment option.[11] This offers a relatively hassle-free way to permanently increase payments as these cardholders already have automatic payments set-up. Cardholders may not have been aware of automatic fixed payment as a repayment option.

Debt can be amortized far quicker by setting automatic payments to a fixed amount rather than the minimum payment. This is because while the contractual minimum payment (and automatic minimum payment) typically declines with balances, a fixed payment stays the same. For example, a typical credit card balance of £1,000 would take 18 years and 6 months to pay off if only the minimum was paid each month (which would start around £25 and then reduce to £5). However, by fixing to £25 each month it could be dramatically reduced to 5 years and 1 month saving over £750 in interest costs. Therefore, holding all else constant, we would expect higher automatic payments to yield lower debt and borrowing costs. We are agnostic about automatic fixed payment enrollment per se but consider this a potentially convenient mechanism to achieve changes in borrowing behavior with real economic consequences.

We tested our automatic payment nudges across three UK lenders via large RCTs conducted between 2016 and 2018 on 153,758 credit cards enrolled in automatic minimum payments and open for at least six months and had not recently repaid their debt in full (see footnote for more details on sample selection).[12] By testing across multiple lenders we can evaluate whether findings were likely to be firm-idiosyncratic or generalizable if applied

[11] Additional details of the practical steps for how to change automatic payments were provided on the letters and with links in the emails - these were redacted from Figures 1 and 2 to preserve lender confidentiality.

[12] Before randomization we excluded cards in arrears, forbearance, dormant, open for less than six months, not enrolled in automatic minimum payments, having repaid balance in full in the last three statement cycles or with balances below £50. Exclusion criteria for cards which were closed, entered arrears or became dormant were reapplied after randomization and before treatment.

marketwide. Before putting the trial into the field the research was subject to institutional reviews at both the FCA and each of the lenders involved. Lenders participated voluntarily and the FCA's statutory powers enabled the data sharing required to analyze this experiment.

We varied the mechanisms of sending communications adding reminders and using letters and emails. With lenders 1 and 2 cardholders received the nudges via letters. With lenders 1 and 2 we tested the effectiveness of sending reminder letters by randomizing treatment groups into and without reminders. We kept the design of the reminders consistent with the original communications (i.e. if you were in the 'Time to Repay' treatment for the first communication, this is also the design you received for the reminder) but adjusted the drafting to refer to the earlier communication sent and re-calculated personalized scenarios using more recent statement balances. Reminders are useful as cardholders may want to take action after the first communication, not get around to it but the reminder prompts them to do so. With Lender 3 communications were sent via emails without any reminders. To increase readability on mobile devices the time to repay graphics for these emails are vertical rather than horizontal. It is useful to examine the effect of emails, as they are cheaper than letters as there are not postage or printing costs. If emails achieve similar effect sizes to letters they would offer a more cost effective policy with lower compliance costs for firms.

IV Data

We analyze our experiments using administrative data on credit cards and cardholders' credit files. Data were gathered by the FCA using its statutory powers.

Credit card data includes details from each card's origination (e.g. opening dates, initial credit limits) and each monthly statement (e.g. statement balances, interest costs, automatic payment enrollment) including up to 11 pre-experimental statements and at least nine statements after Experiment 2 started (six statements for Experiment 1: statement nudges). We observe every payment made against these statements including the date, amount and whether it was made via automatic or manual means.

Credit files from a UK credit bureau enable us to monitor effects of the experiment on the portfolio of credit cards held. This is important as it is common to have multiple credit cards with different providers meaning effects on one card may be offset by changing behaviors on other cards (Gathergood et al. 2019*a*,*b*). Credit files contain monthly data at the credit product level showing balances, payments, credit limits and delinquency status. We also observe flags for whether a cardholder paid exactly the minimum payment. For two points-in-time – the month before the trial started and nine months afterwards – we observe

credit risk scores and income estimates produced by the credit bureau (where available).

Table 1 displays summary statistics on the 153,758 cards in the automatic minimum payment nudge experiment. Just over three-quarters of these cardholders only paid the contractual minimum in the month preceding the trial's start. This is consistent across the three lenders in spite of other differences in characteristics.

It is very common for these cardholders (who are enrolled in the automatic minimum payment scheme at the start of our study period) to repeatedly only make minimum payments. Across the three lenders 31%, 35% and 54% of cardholders only paid the minimum for every one of the twelve months leading up to the start of the trial. The average number of minimum payments in the last twelve months is high across the lenders: 8.2, 8.9 and 10.3. Only a minority of cardholders repaid their credit card statement in full in any cycle during the last twelve months – although the proportions noticeably vary across lenders: 34%, 26% and 17%.

We also designed a short survey after analyzing results to help interpret our findings. We describe the survey along with its results in section VII.

V Empirical Methodology

We pre-registered our experiments at the FCA before analyzing data. Our pre-registration outlined the structure for analysis including the outcomes, regression specifications, and thresholds to judge statistical significance. These were identical for both experiments except experiment 1 was evaluated after six statement cycles whereas experiment 2 was after nine statement cycles: the latest point-in-time observed across all three lenders. We examined interim data after pre-registration but before nine statements to inform potential policymaking.

We structured our analysis in three parts: primary, secondary and tertiary. This structure limits the potential issues for data mining or p-hacking (Simmons et al. 2011). Primary analysis is designed to evaluate the effects of the experiments. Secondary analysis checks the robustness of the primary results and understand the mechanisms in greater detail. Conducting secondary analysis was contingent on the results from the primary outcomes. Finally, tertiary analysis was designed after examining the data.

The primary analysis focuses on ten economic outcomes measuring the effects on: paying only the minimum payment, paying the full balance, any missed payment and outstanding debt as a percent of statement balance (to normalize to deal with 'fat tails' to credit card balances) for both the card in the trial and cardholders' portfolio of credit cards derived from

credit files.[13] The final two primary outcomes were the cost of borrowing and purchases for cards both as a percent of statement balance and were only observable for cards in the trial. We analyze automatic payment choices as outcomes in secondary analysis as the main mechanism the treatment was expected to work through. As automatic minimum payment choices are sticky, rarely changing over time, if we observe changes in these it acts as proxy measure of cardholder attentiveness with the nudge.

We construct an unbalanced panel with one observation for each credit card (i) for each statement cycle (t) observed. This panel is unbalanced as some cards are closed during the trial. Our primary analysis is conducted separately lender-by-lender. We gain more precise estimates of the average treatment effects (ATEs) through focusing on OLS regression specified in Equation 1. We also compare unconditional means of outcomes between control and treatment groups.

$$Y_{i,t} = \alpha + \sum_{k=1}^{K} \delta_{k,t} \left(TREATMENT_{k,i} \times CYCLE'_t \right) + X'_i\beta + \gamma_t + \gamma_m + \varepsilon_{i,t} \tag{1}$$

Equation 1 includes a constant (α) and a vector of control variables (X'_i) using information on the cardholder before the experiments began. The following time-invariant controls were constructed for each card using data from the month preceding the experiment's start: 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 and Any Credit Card Secondary Cardholder and up to 11 lags of outcomes preceding the start of the trial. Fixed effects for the statement cycles (γ_t) and calendar months (γ_m) are included.

$TREATMENT_{k,i}$ is a dummy for each treatment ($k \in (1, K)$) conducted with each lender. The treatments are: 'Time to Repay', 'Time + Cost to Repay' with the control group being the omitted category. When analyzing the effect of the reminders for lenders 1 and 2 we focus on post-reminder statement cycles with treatment groups: 'Time to Repay', 'Time + Cost to Repay', 'Reminder - Time to Repay', 'Reminder - Time + Cost to Repay' with the control group being the omitted category. $CYCLE'_t$ is a vector of dummies for each statement cycle.

Our parameter of interest is $\delta_{k,t}$. This shows the average treatment effect of treatment k, t statement cycles since the start of the experiment. We hypothesized that treatment effects will vary over time but we did not impose a functional form on these as the appropriate

[13] Outcomes as a percent of statement balance are bounded between zero and one.

functional form was unclear. Standard errors were clustered at the card-level.

For Lender 3 there were some technical issues that delayed putting Experiment 2 into the field. This resulted in the exclusion criteria being re-applied several months after randomisation and also excluding consumers who would be expected to repay their debt in less than three years. As a result not all consumers in the treatment groups received the nudge: 73.82% did. 100% of consumers in the control group did not receive the email (as originally intended). We account for this by modifying Equation 1 using an instrumental variables approach whereby assignment to a particular treatment group is an instrument for whether an individual actually received that treatment email. The main implication of this is that our estimates are less precisely estimated for this lender.

In line with Benjamin et al. (2018), we regard in our pre-registered analysis a p-value of 0.005 as the threshold for statistical significance but also highlight where results are 'suggestively significant' at the more traditional 0.05 and 0.01 levels. This approach reduces false positive rates and aligns hypothesis testing with Bayes factors of 14+ considered to be substantial evidence for a hypothesis. This approach is similar to applying a Bonferroni or familywise error corrections to testing multiple hypotheses at the traditional 0.05 significance level.

VI Results

In this section we examine the treatment effects of the automatic payment nudge: its proximate effects (subsection A), long-term effects (subsection B) and heterogeneous effects (subsection C).

VI.A. Proximate Effects

We examine the proximate effects of the nudge on automatic payment enrollment – a proxy for consumer attention to the treatment. We show initial effects from two rather than one statement post-nudge since operational lags can mean a requested change mid-statement cycle may not take effect until the following statement. Most requested changes occur in the first few days after the nudge was sent. We were able to listen to some phone calls made to lenders' call centres from consumers in the treatment groups who switched to automatic fixed payments after receiving the nudges. The language used in these calls was consistent with the qualitative research used to design the nudges where cardholders described the graphical disclosure as impactful and surprising.

We find clear effects that are of similar magnitudes across the 'Time to Repay' and

'Time + Cost to Repay' treatments and precisely replicated across all three lenders. The treatment initially reduces automatic minimum payment enrollment by 0.9 to 2.0 percentage points two statement cycles after the nudges were sent (Figure 4, Panel A). Automatic fixed payment enrollment increases by 1.1 to 1.6 percentage points (Figure 4, Panel B). The cost information added in the 'Time + Cost to Repay' treatment has no additional effect. As can be seen in these figures, there is no discernible difference in estimates from lenders 1 and 2 who sent letters compared to Lender 3 who sent emails.

How should the sizes of such proximate effects be interpreted? The effects may appear 'small' since the treatments are causing 1 to 2 in a 100 cardholders to change their automatic minimum payment enrollment. An alternative interpretation is that, as this is a group of highly inert consumers where almost no one is naturally changing to automatic fixed payments. In relative terms, the impact of the interventions are not as small as it first appears: on average causing increased take up of automatic fixed payments by around 61-67% at Lender 1, 171-186% at Lender 2 and 19-26% at Lender 3 relative to each of their respective control groups.[14] These results are slightly more encouraging when set against the low marginal costs of sending the disclosures – particularly if they are sent via emails rather than letters. So even relatively small effects could be net beneficial overall.

What is the initial overall effect on all minimum payments, both automatic, as described above, and manual? This may be larger than the initial effect on automatic minimum payment enrollment since cardholders may also respond to the treatment by making manual payments in addition to their automatic payments. The effect on making exactly the minimum payment is slightly larger but broadly similar in magnitude to the effects on automatic payment choices. Figure 4, Panel C shows the treatments initially reduce the likelihood of making exactly the minimum payment by between 1.1 and 2.3 percentage points. There is no statistically significant difference across the two treatments or three lenders.

VI.B. Long-term Effects

We now examine the effects on our primary outcomes after nine completed payment cycles. Figures 5 and 6 present the estimated effects on our primary outcomes and we start with considering the long-term treatment effect on minimum payments. Long-term effects are estimated after nine cycles. Online Appendices provide tables of estimates (regression estimates in Table A2) with results consistent to alternative outcomes (Table A8) and t-tests of unconditional means (Table A7).

[14] In the control groups for Lenders 1, 2 and 3 respectively, 1.8%, 0.7% & 6.2% of consumers in our experiment have naturally switched from automatic minimum payments to automatic fixed payments from the time of randomization to the second statement cycle post-treatment.

Figure 5, Panel A shows by the ninth statement the treatment effect on minimum payments reduces towards zero for some of the lender-treatment combinations. However, differences are not statistically significantly different from the effects after two statement cycles (Figure 4, Panel C). The 'Time to Repay' treatment appears effective for Lender 1 and Lender 3 with a smaller estimate at Lender 2 that is insignificant. The 'Time + Cost to Repay' treatment is no better than the control group. Sending reminder nudges appears to help to reduce the number of cardholders making minimum payments at Lender 2 but does not significantly do so at Lender 1 (Figure 5, Panel A).

Examining effects on minimum payments across the portfolio of credit cards observed in credit files (Figure 6, Panel A) produces noisier estimates. The coefficients are generally negative and slightly smaller and not always statistically significantly different from zero. The reminders appear the most effective treatment.

Neither 'Time to Repay' or 'Time + Cost to Repay' treatments have statistically significant effects on our other primary outcomes (Figure 5 and Figure 6). We find precisely estimated, zero effects on borrowing costs, missed payments, full payments, spending, average payments or outstanding debt net of payments. These results are robust to alternative secondary outcomes measures.

Having observed these results we decided to conduct tertiary analysis pooling observations across Lender 1 and Lender 2 to increase the precision of our estimates. We estimate this using Equation 1 but adding in a firm fixed effect (see footnote for specification).[15] We did not pool Lender 3 given an IV rather than OLS method is used for estimating effects, the communications were sent via emails rather than letters and their portfolio of cards were noticeably different to the other two lenders (Table 1).

Figure 7 shows the initial effects of the nudges on minimum payments in the second statement cycles are fairly constant over time for the 'Time to Repay' treatment but decline to insignificance for the 'Time + Cost to Repay' treatment.

This pooled analysis of Lender 1 and Lender 2 reveals how both the treatments reduced debt net of repayments during the early statement cycles (Figure 9, Panel A). The reduction peaked at £36.88 for 'Time to Repay' treatment and £59.45 for 'Time + Cost to Repay' treatment after four statement cycles. This temporary debt reduction equated to a reduction of 1.2% and 2.0% relative to the average debt in the control group.

Such a debt reduction was temporary. Why? Part of the driver for this temporary reduction in debt is a small proportion of people (under 0.5%) react to the nudges by making a manual payment by the first two statement cycles. Such manual payments are a one-off

[15] We estimate: $Y_{i,t} = \alpha + \sum_{k=1}^{K} \delta_{k,t} \left(TREATMENT_{k,i} \times CYCLE'_t\right) + X'_i\beta + \gamma_f + \gamma_t + \gamma_m + \varepsilon_{i,t}$ that pools data from Lender 1 and Lender 2 where γ_f is firm fixed effect.

reaction and not maintained over time: Figure 9, Panel B shows the cumulative value of manual payments plateaus. We interpret this as the nudges bringing forward the timing of manual payments rather than increasing the cumulative amount of manual payments.

One treatment with a reminder has some real, long-lasting effects. The 'Reminder - Time to Repay' treatment does appear to reduce credit card debt by 1.2 percentage points (1.6%) and increase the likelihood of repaying debt in full by 1.1 percentage points (6.9%) at Lender 2. However, we caveat these results as these findings were not replicated at Lender 1. Also, these effects are not visible in credit file outcomes aggregating the portfolio all credit cards by a cardholder – this indicates portfolio rebalancing of payments rather than increasing payments to reduce credit card debt.

VI.C. Heterogeneous Effects

It is possible the average effects of the nudges mask much larger effects for a subset of cardholders who are attentive. If so, it may be an even more targeted approach – sending the nudges to a subset of automatic minimum payment cardholders could be more efficient.

In response to feedback from reviewers we examined such a possibility of heterogeneous treatment effects conducting tertiary analysis pooling Lender 1 and Lender 2 as done previously. We examine and report heterogeneous effects across four variables: credit scores, whether the cardholder had balance-transfer debt in the month preceding the experiment, the number of full payments and the number of minimum payments in the twelve months preceding the experiment.

Credit score is chosen as the covariate from Equation 1 that best predicts minimum payments. Statistically significant reductions (4.3 percentage points) in minimum payments are concentrated for the 'Time to Repay' treatment in the fourth decile of credit score, however, there is some indication – though statistically insignificant – of larger effects among the lowest credit scores compared to higher deciles (estimates in Online Appendix Tables A3 and A4). There is some evidence of statistically significant debt reduction among the lowest and highest credit score deciles but at no other deciles. There is no evidence of the 'Time + Cost to Repay' treatment being effective.

The numbers of minimum and full payments are chosen as forms of payment behavior consistently observable across lenders that regulators could potentially apply rules to – unlike other demographic factors.

There are no clear patterns in effects by the number of minimum or full payments in the last twelve months (estimates in Online Appendix Tables A3 and A4). If anything, responses are slightly larger among those with fewer minimum payments or more full payments i.e. the

opposite of those the nudge is most aimed to respond: cardholders persistently only paying the minimum and not paying in full.

We segment cardholders into two groups with different exposures to interest costs by taking advantage of the fact that some cardholders made balance transfers. Balance transfers are where cardholders incur 0% interest on debts transferred onto a card for a promotional fixed period after paying an initial up-front fee. Cardholders with balance transfers are examined as they often have high credit card balances not incurring interest although their non-balance transfer debt may still incur interest and 0% deals are temporary and contingent on not missing repayments. They have higher credit scores and often cycling debt from one product to another rather than paying it off during the interest-free period (Financial Conduct Authority 2015*b*, Guttman-Kenney et al. 2018, January 8).

We find suggestive evidence of the disclosures potentially mostly affect cardholders with balance transfer credit card debt. We pool Lender 1 and Lender 2 to increase precision as only 11% have balance transfer debt whereas 52% of cardholders at Lender 3 do. Figure 10, Panel A shows the 'Time to Repay' treatment causes a 3.0 percentage point decrease in paying the minimum compared to 0.8 for those without balance transfers for those at Lenders 1 + 2 pooled. The effect of the 'Time + Cost to Repay' treatment is similar: 2.7 percentage point decrease for those with balance transfer debt compared to 0.2 for those without (Lenders 1 + 2 pooled). We infer from this that adding cost information does not make cardholders who are incurring interest (i.e. those without balance transfer debt) more likely to respond. The estimates for Lender 3 are consistent with this conclusion, however, the confidence intervals are wide for the group without balance transfer debt so we are less certain of this.

We observe no statistically significant effects on debt for cardholders with or without balance transfer debt for any of the lenders (Figure 10, Panel B). For the group with balance transfer debt our confidence intervals are wide – approximately £300: and thus we cannot conclude whether there is an effect for this group or not. We can precisely rule out effects on cardholders without balance transfer debt for the pooled Lenders 1 + 2.

VII Survey Evidence

Given the results of our nudge experiment we designed a survey (explained in section VII.A) to test consumer understanding of minimum payments (VII.B), testing the treatment effects of the nudges at improving understanding of minimum payments (VII.C), the reasons for automatic minimum payment use (VII.D) and self-reported financial distress and stated preferences (VII.E).

VII.A. Survey Design

Our survey was designed as tertiary analysis to help interpret the results of Experiment 2. The survey was sent via email from lender 2 to cardholders in the automatic minimum payment nudge field trial. Participation was incentivized through a prize draw and the invitation mentioned the role of the lender and the FCA, in seeking views and experiences of using credit cards with an email subject line 'Win £500! Help make credit cards better'.[16] We ran a small pilot to refine the survey format and do not use those responses for analysis. The full survey questionnaire can be found in Online Appendix ('B: Survey Questionnaire'). This survey was conducted over a year after Experiment 2 began and achieved 1,713 responses which is a 2.9% response rate.

VII.B. Testing Consumer Understanding of Minimum Payments

We test cardholder's beliefs about minimum payments through presenting a hypothetical scenario based on a typical UK credit card statement balance (£1,029.90) and interest rate (18.9%) and asked respondents how long they expected it to take to pay if they only repay the minimum each month and spend no more on the card (question in footnote).[17] Respondents had a free text box to input their answer (in years and months) so as to not to steer them towards a particular response (Schwarz et al. 1985). The correct answer is 18 years and 9 months. This is a hard question and we do not expect respondents to get this precise number. Instead we are more interested in the distribution of responses – does it appear that many consumers' expectations are broadly consistent with actual repayment periods. For example, do they expect it to take a few (1-3) years or 10+ years?

We find evidence cardholders have mistaken beliefs – not understanding the implications of credit card minimum payments. Among those who did not receive our nudges (the control group), 95.9% of respondents underestimate how long only paying the minimum would take to clear debt with 37.9% expecting within three years. 65.4% within 5 years and 87.0% no more than 10 years.

One may expect selection into responding to the survey on unobservable characteristics. We expect respondents to be more financially sophisticated and conscientious on average

[16]The prize draw offered two £500 Amazon gift vouchers and fifteen £100 Amazon gift vouchers. Due to UK marketing research regulations entry into this prize draw was not conditional on completing the survey.

[17]Imagine a credit card statement balance of £1,029.90 with an interest rate of 18.9%. If someone only repays the minimum each month and spends no more on the card approximately how long would you expect it to take them to repay?
It doesn't matter whether the answer you give is right or wrong. We just want to find out what people understand and the question after this one lets you indicate how confident you are in your answer.

and, if so, such selection means that we are finding widespread, large confusion even among the subgroup one may expect to answer such questions most accurately.

Our results are consistent with Soll et al. (2013) and Lusardi & Tufano (2015) as well as contemporaneous research by Seira et al. (2017) in Mexico. They also closely relate to prior evidence showing a lack of understanding of other credit card features: dramatically underestimate the costs of credit card borrowing due to linearizing exponential functions (Stango & Zinman 2009, 2011).

The underestimates are large and respondents report extremely low confidence in their estimates: on average 4 out of 10 (where 1 is lowest confidence and 10 is highest confidence) with approximately a quarter stating they are not at all confident in their estimates. Such a lack of confidence indicates how many consumers lack financial literacy in the financial products they use (Lusardi & Mitchell 2014) and the challenges of overcoming this (Willis 2009). An earlier UK survey (Which? 2015) of consumers self-reporting to make minimum payments on credit cards found widespread confusions over what credit card minimum payments are: 48% regarded the minimum payment as a recommendation from their credit card provider - it is not - and 50% considering most people repaid the minimum - whereas a quarter do (Financial Conduct Authority 2016).

VII.C. Treatment Effects of Nudges on Understanding of Minimum Payments

A potential explanation for the lack of real effects of automatic minimum payment nudge is that consumers do not understand or remember the treatment. We test this hypothesis by splitting responses to our hypothetical question by control and treatment. We pool all treatments into a single group to increase power.

Responses rates are similar across treatments - an F-test finds response rates to be insignificantly different across control and treatments (F-stat 0.2109, p-value 0.8098) which is reassuring given concerns over selection on unobservables as one might have worried the treatment made cardholders more likely to respond to the survey.

Figure 11 show the distribution of responses for control compared to treatment: Panel A plots a histogram from aggregates responses to yearly bins while Panel B shows the CDF from the monthly responses. While 92.57% of the treatment group underestimate the true amortization time (18 years, 9 months) it is significantly reduction from the 95.88% of the control group who underestimate.

Across measures we find statistically significant effects of the treatments at reducing how biased respondents' underestimates are at answering this hypothetical question: results of

t-tests are displayed in Table 3. The mean duration increases by 14 months from 70.6 to 84.5: a 20% increase. Median changes are similar: increasing by 13 months from 47 to 60 months: a 28% increase. The treatment reduces the fraction (incorrectly) expecting amortization to be under three years by 6.1 percentage points (or 16%) from a baseline of 37.9%. The treatment reduces the fraction (incorrectly) expecting amortization to be under five years by 8.7 percentage points (or 13%) from a baseline of 65.4%. The treatment does not cause any statistically significant differences (p-value 0.3519) in confidence of respondents' answers with both control and treatment having mean and medians of 4 out of 10.

The treatment letters were sent over a year before the survey and thus such effects appear persistent. There are no statistically significant difference between the treatments groups with and without reminders.[18] We interpret these results as consumers understanding and remembering our nudges but with these being insufficiently powerful to prompt changes in their own borrowing behavior.

How do the answers to our hypothetical question relate to the personalized information on individuals actual minimum payment repayment times presented in the nudges? For 79.8% of respondents we have permission to anonymously link survey responses to their administrative data to examine this.[19]

As personalized scenarios change between initial and reminder letters, we focus on the treatment groups who received a single letter (no reminders) to enable us to cleanly compare to the control group: 906 observations in total. We examine this using descriptive evidence in Figure 12 and an OLS regression, with robust standard errors, specified in Equation 2 with results in Table 4.

$$HYPOTHETICAL_i = \alpha + \beta\ ACTUAL_i + \gamma\ TREAT_i + \delta\ (TREAT_i \times ACTUAL_i) + \varepsilon_i \quad (2)$$

Figure 12 shows on the x-axis the duration of time to repay their actual debt if the respondent only paid the minimum (Equation 2, variable $ACTUAL_i$): this is information they were actually provided with in the nudges (Figures 1 and 2). There are a variety of actual repayment durations because under the minimum payment formula it takes longer to amortize larger balances or with higher interest rates. On the y-axis are responses to the hypothetical question in our survey (Equation 2, variable $HYPOTHETICAL_i$): the horizontal dashed blue line shows the correct answer (18 years and 9 months). The chart

[18] P-values [95% C.I.] are: 0.5890 [-7.74,13.63], 0.7371 [-0.05,0.06], 0.8443 [-0.06,0.05], 0.4911 [-0.02,0.04] respectively for expected months, expected $<$ 3 years, expected $<$ 5 years, expected $<$ actual.

[19] There is not differential attrition between control and treatments in these consent rates. P-value 0.405 [95% C.I.: -0.0666, 0.0269].

plots all responses and loess smoothed lines of best fit.

If respondents answered the survey question with the information provided in the nudge then they would be on the gray 45 degree line. Instead we find no relationship between respondents' answers to the hypothetical question and the actual information provided in the treatment letters.[20] These actual duration of times were also calculated for the control group (who did not receive the letters) and we also observe no relationship (which is as one would expect).

The regression results (Table 4) are consistent with this. We find a statistically significant (P-value 0.0107), but noisy, average effect (γ) of the treatment among respondents increasing hypothetical time to repay close to the accurate duration but precise zero effects for its relationship (δ) with the actual duration provided. We interpret this evidence as consumers in the treatment remember the minimum payment repayment duration is long (consistent with the qualitative evidence of being shocked by it) but do not recall the precise details of information presented to them.

In other domains, providing information has been found to be effective at correcting biased beliefs and changing real behavior. For examples, Jensen (2010) finds providing information on returns to schooling increase the amount of education received, Dupas (2011) finds providing information on HIV infection risk reduces teenage pregnancy and changes reported sexual behavior, Bursztyn et al. (2020) find misperceived social norms among Saudi Arabian husbands reduce female labor force participation and, of most relevance to our study, Bertrand & Morse (2011) shows providing information on payday loan borrowing costs reduces borrowing.

VII.D. Reasons for Automatic Minimum Payment Use

We were interested in understanding the reasons consumers choose to use automatic minimum payments. Table 5 displays the survey responses for the 1,145 respondents who confirm they have an automatic minimum payment at the time of the survey. Given the low response rate and also the potential to ex-post rationalize behaviors in responses we treat the survey results – especially the point estimates – with caution.

Our survey asks cardholders why they use automatic payments with results summarized in Table 5. Cardholders were shown a list in a randomized order with multiple responses allowed and also a free text other category. The majority of respondents report using automatic payments primarily to prevent missing payments as they are worried about adverse credit score impacts (55%) or late fees (58%) from doing so. This is consistent with the

[20] We also find a zero effect on this coefficient (β) in our regression shown in Table 4.

hypotheses in prior empirical studies using administrative data (Gathergood et al. 2020, Sakaguchi et al. 2021). Some respondents report preferring to make payments in this way for a couple of other reasons: 28% simply prefer the control of making payments automatically; just under 8% say it helps them manage their unstable financial situation; and, 18% of respondents say they use them as it is easy to set up.

When prompted why they have automatic minimum payments instead of automatic fixed payments approximately 40% state they prefer the control of making automatic minimum payments and additional payments manually rather than higher automatic payments. 18% report being unaware of the fixed option: it appears that the nudges are an ineffective way to bring about enough awareness to actually prompt action in this domain.

VII.E. Self-Reported Financial Distress & Stated Preferences

Our survey includes questions to help to evaluate whether cardholders with automatic minimum payments do not pay down debt because they are liquidity constrained or simply because they do not want to pay down their debt, with the results summarized in Table 5. The low survey response rate limits the interpretation of this evidence as one may expect selection on unobservables with less financially distressed and more conscientious consumers being more likely to respond. One may also be concerned that responses to these questions bare little, if any, resemblance to their true preferences.

A potential explanation for why cardholders persistently only pay the minimum and do not repay their debt is that they are liquidity constrained. We ask "How well are you keeping up with your bills and commitments at the moment?" and find 19% of survey respondents report either facing a constant struggle to keep up with their payments or report falling behind. This is consistent with another question in the survey asking why they have an automatic minimum payment rather than an automatic fixed payment; where 20% state that they can only afford the minimum. While liquidity constraints are clearly important it does not appear to explain why many automatic minimum payment cardholders are repeatedly making minimum payments. This is in line with other studies finding patterns of credit card borrowing inconsistent with liquidity constraints (Keys & Wang 2019, Guttman-Kenney et al. 2018).

We next examine respondents' self-reported desire to reduce their debt via two questions. The first question finds a strong stated preference for debt reduction which conflicts with their revealed preferences of actually repeatedly only paying the minimum. When asked "Thinking about your financial situation in one year now. Would you most prefer to?" 71% of respondents prefer to reduce their debt compared to 27% who want to save more and

almost no one preferring to spend more. We caveat this by noting that this is a very simple survey question. The questions proxies for rather than attempts to capture the detail of how changing credit card payments would affect the rest of a household's finances.

The second question asks (using the same the hypothetical scenario described in section VII.B): "Bearing in mind how much money you actually have. If you only repay the minimum each month and spend no more on the card, approximately how long would you want it to take to repay?" On average, respondents reported wanting debt to amortize in approximately 3 years (and 2 years for the median response) if they only made minimum payments. 71% wanted only paying the minimum to result in debt amortizing faster than their own (typically vastly under-estimated) expectation of how long they thought it would take. 15% and 10% want it to be paid back in the same or longer duration than their own expectation respectively and the remaining 4% said they could not afford to pay off such debt.

VIII Concluding Discussion

The consistency in consumer responses in our UK experiments to those in other countries indicates we are learning something general about household finances. Credit cardholders allocate payments across multiple credit cards with similar ('balance-matching') heuristics across the UK (Gathergood et al. 2019*b*), US (Gathergood et al. 2019*a*), and Mexico (Ponce et al. 2017). The 'anchoring effect' (Stewart 2009) of the credit card minimum payment amount reducing payments has been replicated across the UK (Navarro-Martinez et al. 2011, Guttman-Kenney et al. 2018, Sakaguchi et al. 2021), US (Jiang & Dunn 2013, Salisbury 2014, Keys & Wang 2019, Salisbury & Zhao 2020), and Mexico (Medina & Negrin 2021).[21]

Our experiments provide empirical evidence on the challenges for consumer financial protection regulators relying on informational nudges to prompt action (Campbell et al. 2011, Campbell 2016). We contribute to a broader literature documenting consumer inattentiveness to information about financial products. Our results are broadly consistent with the relative ineffectiveness of providing credit card information in the US (e.g. Agarwal et al. 2015, Keys & Wang 2019) and Mexico (Seira et al. 2017). The inability of information to change existing behavior is also found in much simpler financial decisions such as cash savings (Adams et al. 2021) and domains with substantially larger financial stakes such as mortgage refinancing (Keys et al. 2016). The effectiveness of payday lending disclosures (Bertrand & Morse 2011, Wang & Burke 2021) appears to be an exception – possibly explained by the

[21] Consumers in the US (Ganong & Noel 2019) & Brazil (Gerard & Naritomi 2021) show similar consumption responses to unemployment insurance benefits. Jumps in spending on pay days are present across the US (Gelman et al. 2014), Iceland (Olafsson & Pagel 2018), and Brazil (Gerard & Naritomi 2021).

disclosures being applied in-store at the point of credit application rather than attempting to change behavior of inattentive existing customers of larger financial institutions. Heterogeneous effectiveness of disclosures across consumers are important considerations for regulators. For example, Kulkarni et al. (2020) finds Chilean loan disclosures only reduce reduce delinquencies for sophisticated borrowers, whereas, product standardization benefits unsophisticated borrowers.

Our second experiment shows few automatic minimum payment cardholders respond to our informational nudge: typically remaining enrolled on automatic minimum payments and continuing to repeatedly pay only the minimum. How else to reduce cycles of repeated minimum payments? When designing this experiment we considered sending nudges via email with a one-click button to reduce frictions for changing automatic payments – results from another trial found this increases response rates (Financial Conduct Authority 2016) – but decided against such an approach due to concerns that emails with links could be part of a phishing scam. Technology has since developed with push notifications on mobile apps now widely used. Push notifications' potential to reduce adjustment costs and enable consumers to re-optimize financial decisions more frequently is worth exploring.

An alternative, more intrusive, nudge is changing choice architecture at the time of automatic payment enrollment to select automatic fixed payments instead of automatic minimum payments. Guttman-Kenney et al. (2021) shows in a UK field experiment that even though such a nudge is highly effective at changing proximate choices (e.g. automatic payment enrollment) it proved unsuccessful at changing more distal, real, economic outcomes (e.g. debt) due to offsetting consumer responses.

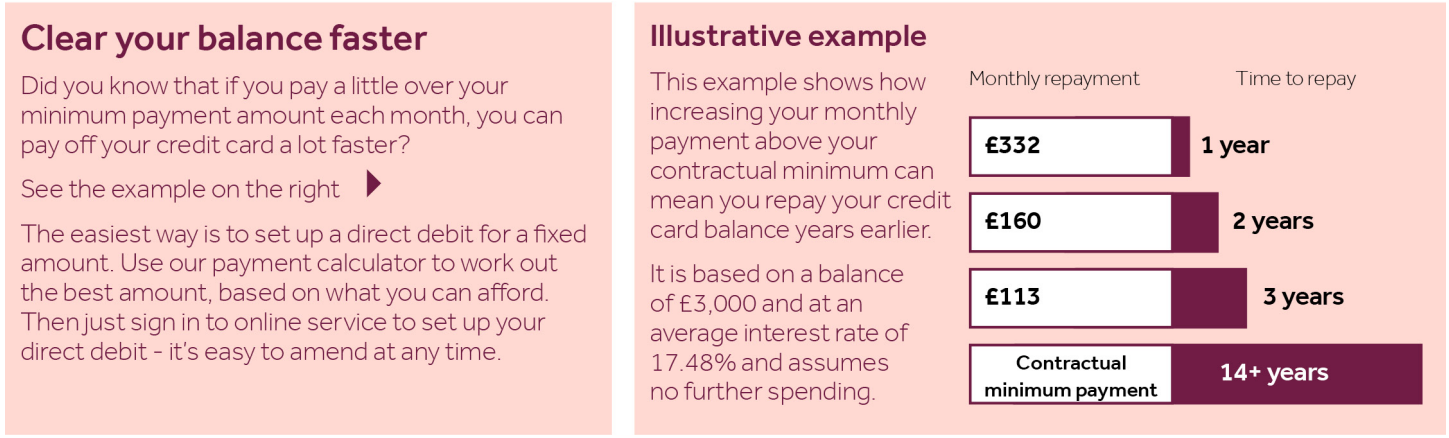
In a broad set of domains a variety of recent studies (e.g. Jachimowicz et al. 2019, DellaVigna & Linos 2020) have shown nudges to have smaller effects than previously documented. Even small effects can still be net beneficial if they cover their costs (Benartzi et al. 2017, DellaVigna & Linos 2020).[22] Yet there is a growing view that behaviorally-informed nudges are insufficient to achieve policy goals (e.g. Campbell 2016, Loewenstein & Chater 2017, Laibson 2020).

[22]For example, nudges can be helpful reminders prompting people who would otherwise forget to pay their bills (Mazar et al. 2018) or appealing to their moral to do so (Bursztyn et al. 2019).

A Figures

Figure 1: Statement nudges - design of (A) 'Time to Repay' and (B) 'Time + Cost to Repay' treatments

(A) 'Time to Repay'



(B) 'Time + Cost to Repay'

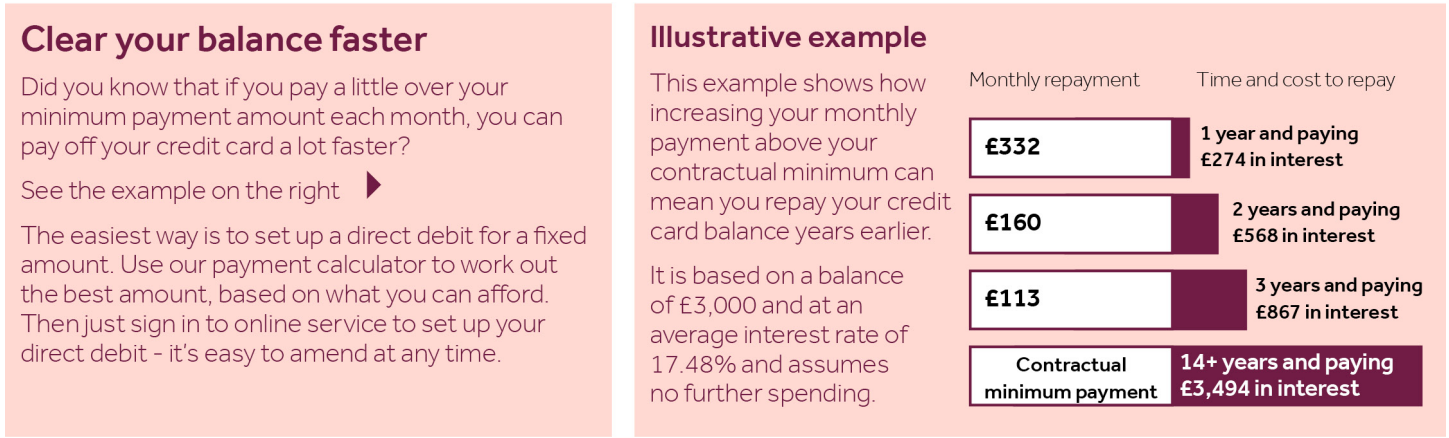
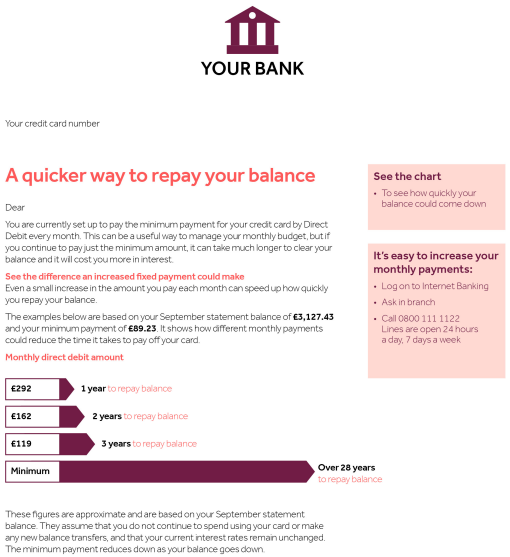
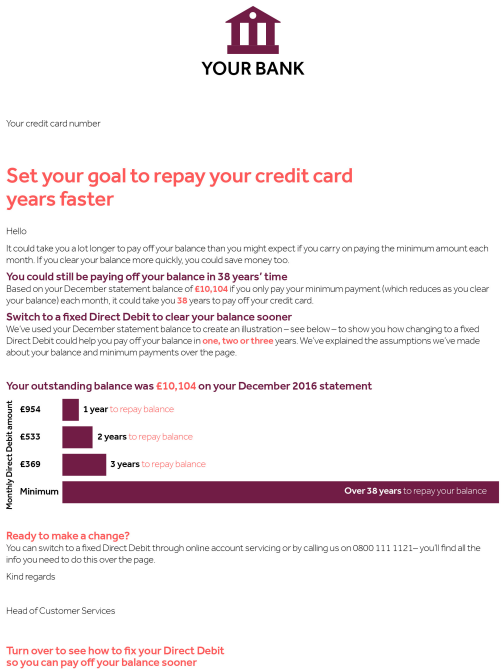


Figure 2: Automatic minimum payment nudges - design of 'Time to Repay' treatments across lenders

Lender 1



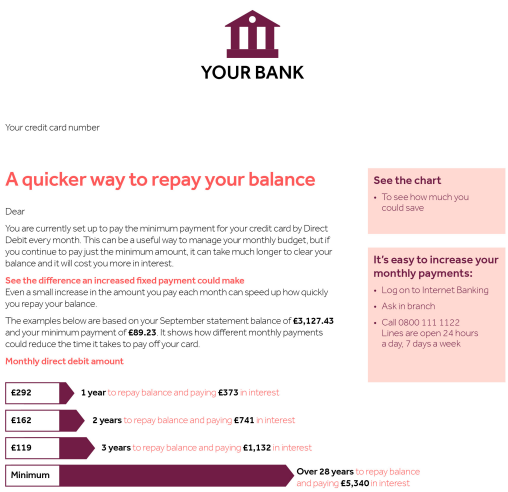
Lender 2



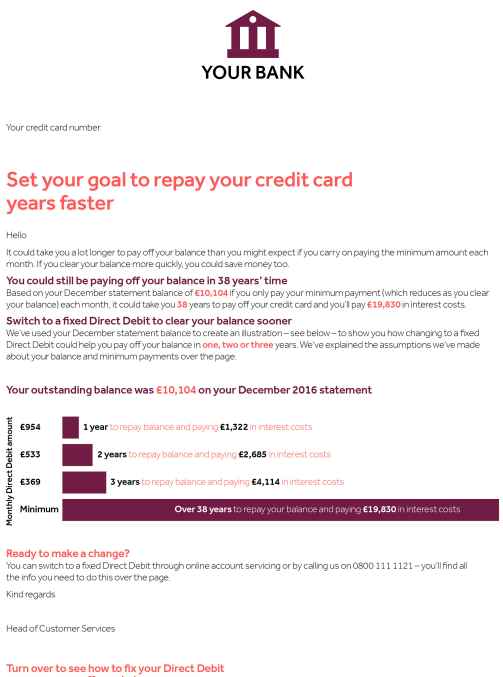
Lender 3

Figure 3: Automatic minimum payment nudges - design of 'Time + Cost to Repay' treatments across lenders

Lender 1



Lender 2



Lender 3

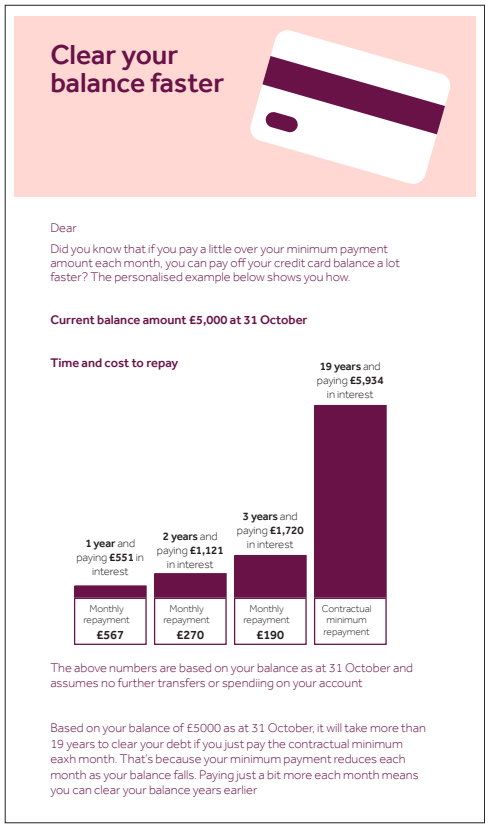
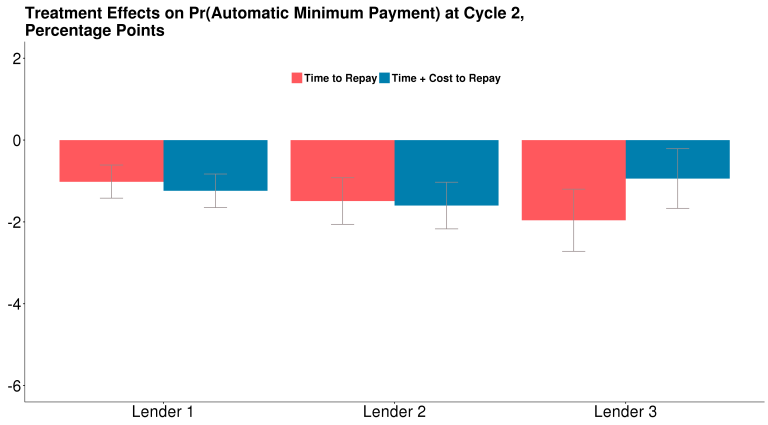
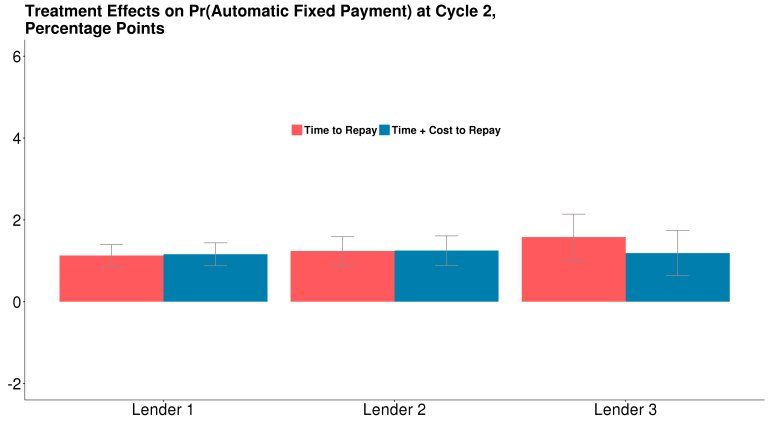


Figure 4: Automatic minimum payment nudges - treatment effects on (A) Pr(automatic minimum payment enrollment) (B) Pr(automatic fixed payment enrollment) (C) Pr(minimum payment) after two statement cycles

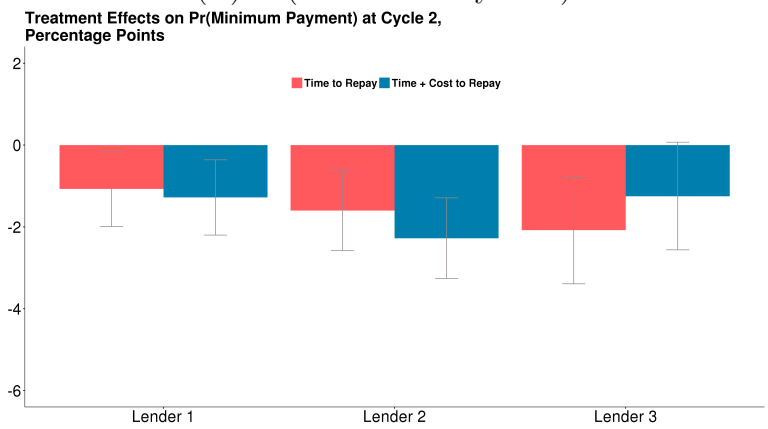
(A) Pr(Automatic Minimum Payment)



(B) Pr(Automatic Fixed Payment)



(C) Pr(Minimum Payment)



Notes: Estimates are $\delta_{k,2}$ from Equation 1 OLS regression that includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level.

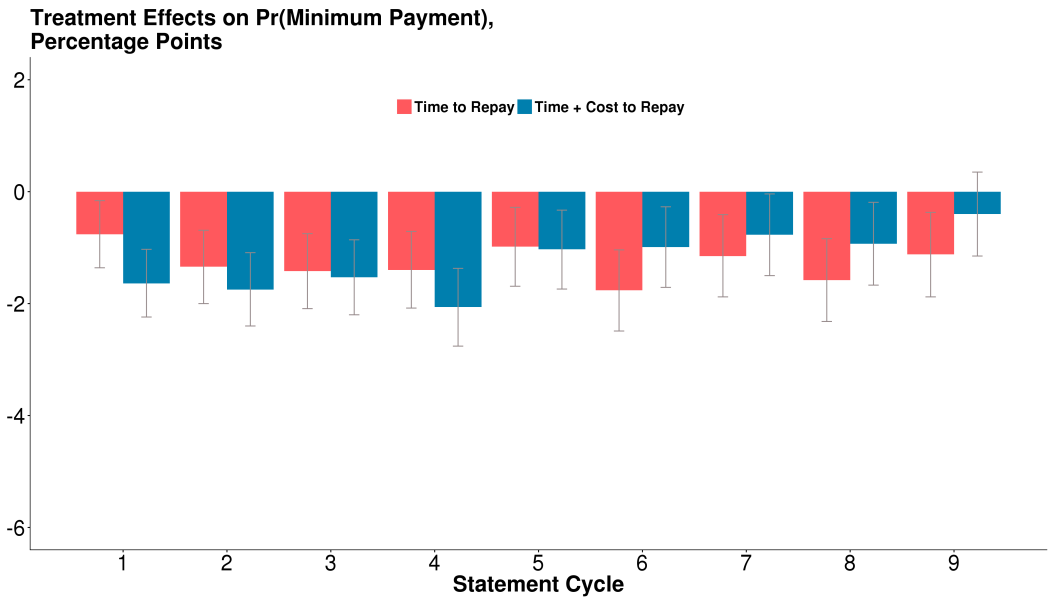
Figure 5: Automatic minimum payment nudges - treatment effects on primary credit card outcomes after nine statement cycles

Notes: Estimates are $\delta_{k,9}$ from Equation 1 OLS regression that includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level.

Figure 6: Automatic minimum payment nudges - treatment effects on primary credit reference agency (CRA) credit card outcomes after nine statement cycles

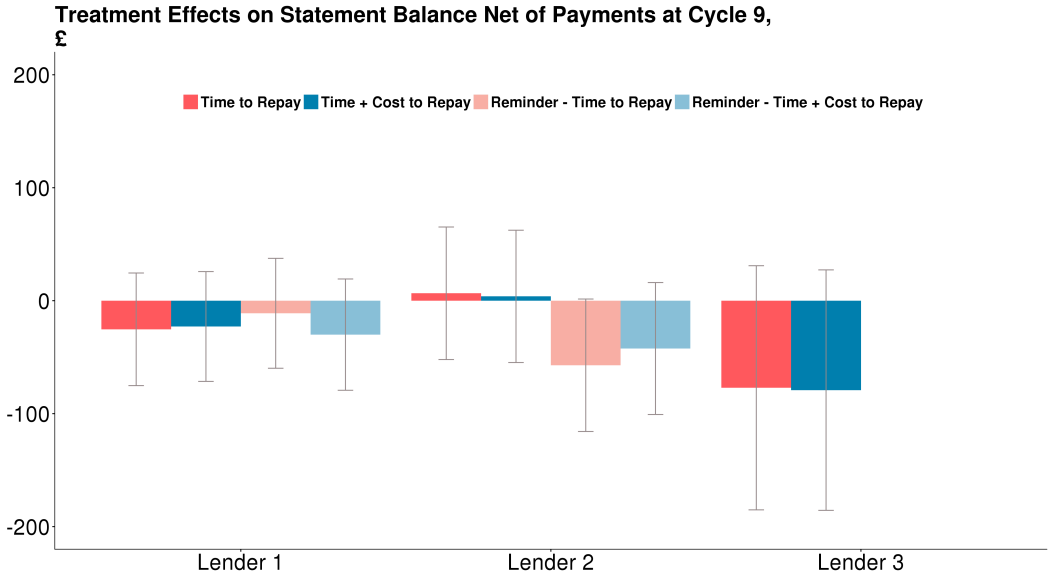
Notes: Estimates are $\delta_{k,9}$ from Equation 1 OLS regression that includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level.

Figure 7: Automatic minimum payment nudges - treatment effect on probability of only paying the minimum payment over time pooling Lender 1 and Lender 2



Notes: Estimates are $\delta_{k,t}$ from Equation 1 OLS regression (modified by pooling Lender 1 and Lender 2 and adding lender fixed effects) includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level.

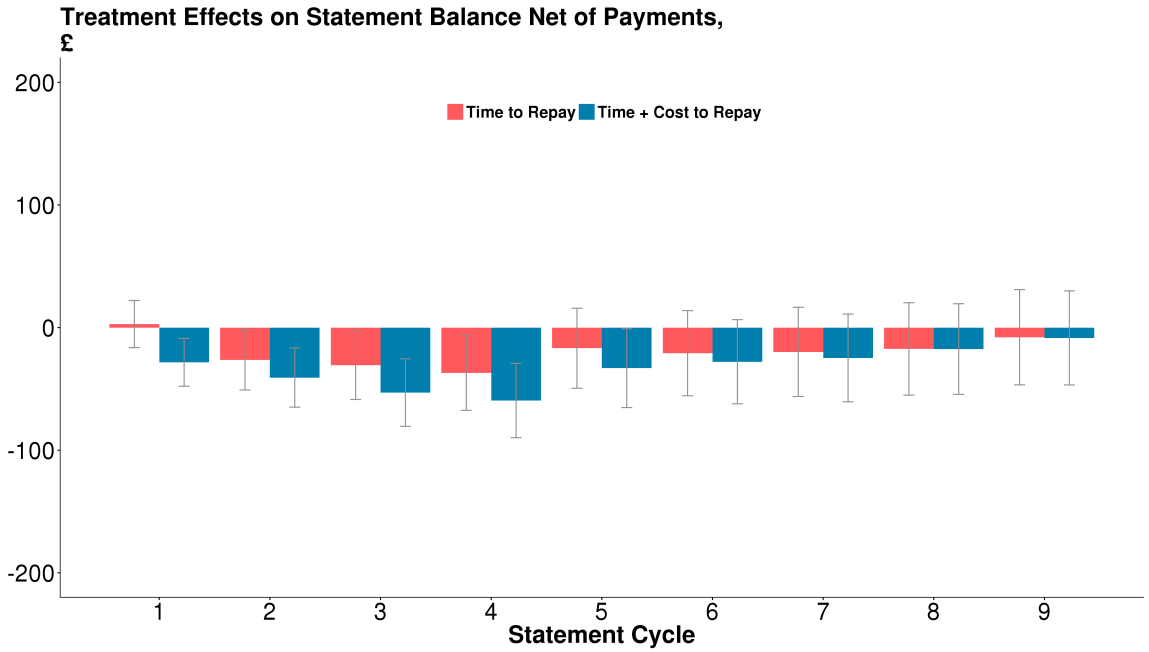
Figure 8: Automatic minimum payment nudges - treatment effect on credit card debt net of payments after nine statement cycles



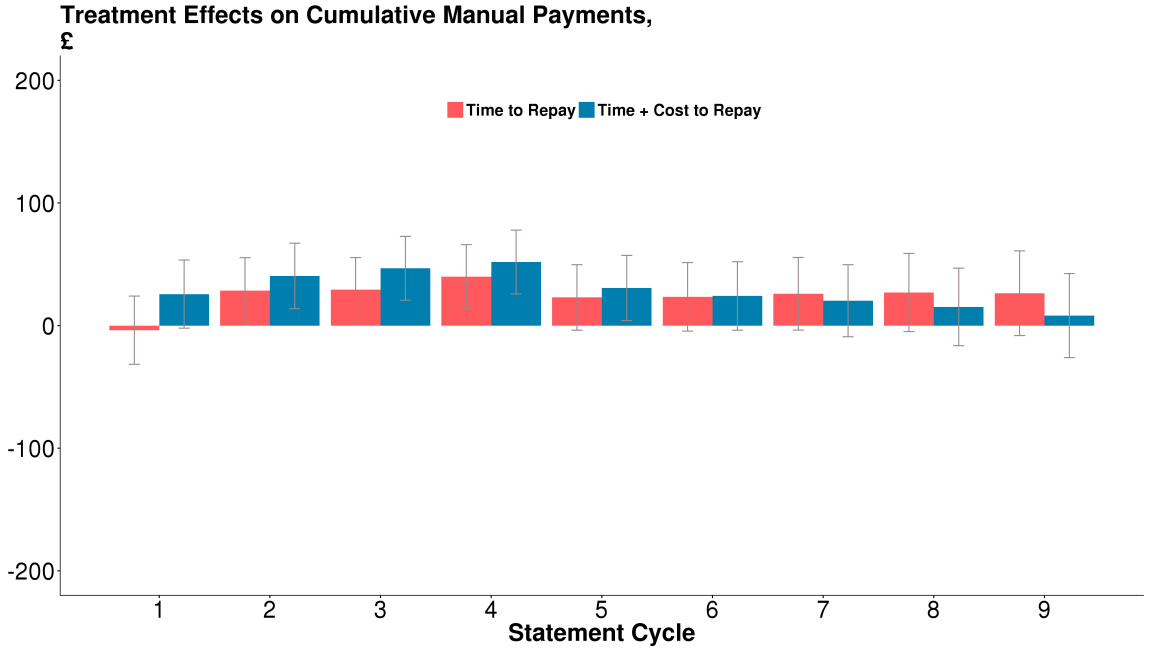
Notes: Estimates are $\delta_{k,9}$ from Equation 1 OLS regression that includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level.

Figure 9: Automatic minimum payment nudges - treatment effects over time pooling Lender 1 and Lender 2

A. Credit card debt net of payments



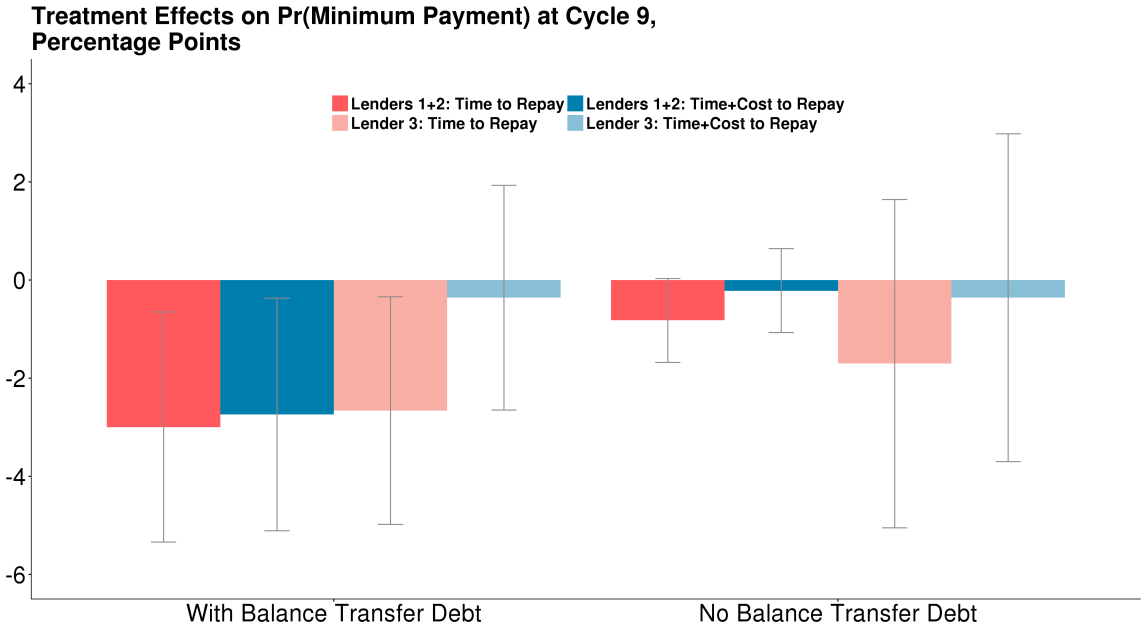
B. Cumulative manual credit card payments



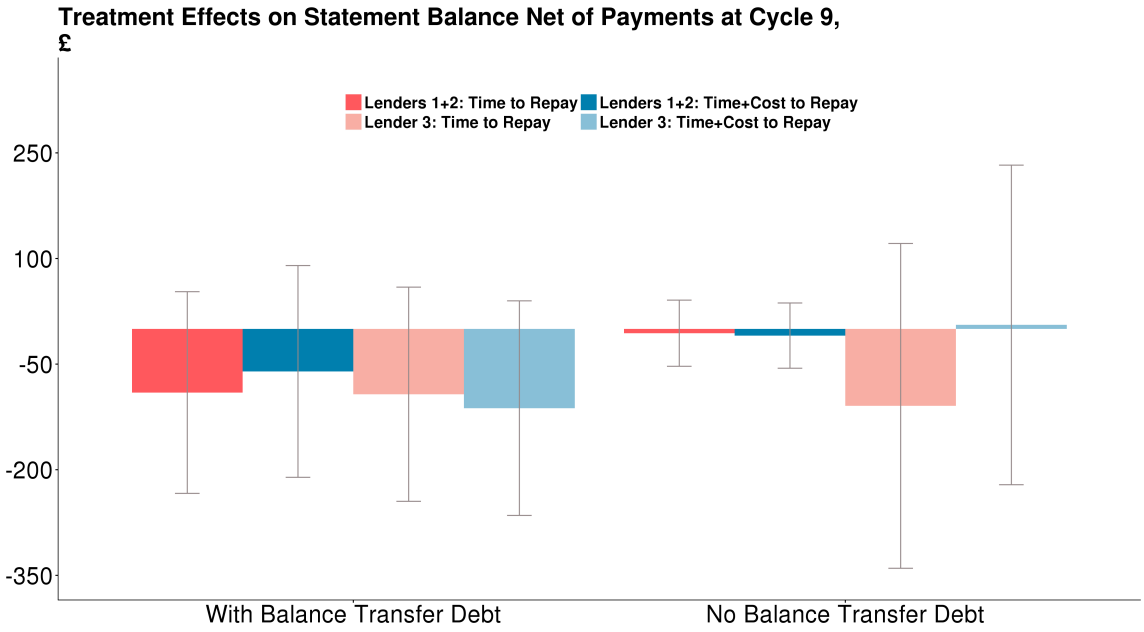
Notes: Estimates are $\delta_{k,t}$ from Equation 1 OLS regression (modified by pooling Lender 1 and Lender 2 and adding lender fixed effects) includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level.

Figure 10: Automatic minimum payment nudges - heterogeneous treatment effects on (A) Pr(minimum payment) (B) Credit card debt net of payments, by whether had balance transfer debt pooling Lender 1 and Lender 2

A: Pr(Minimum Payment)

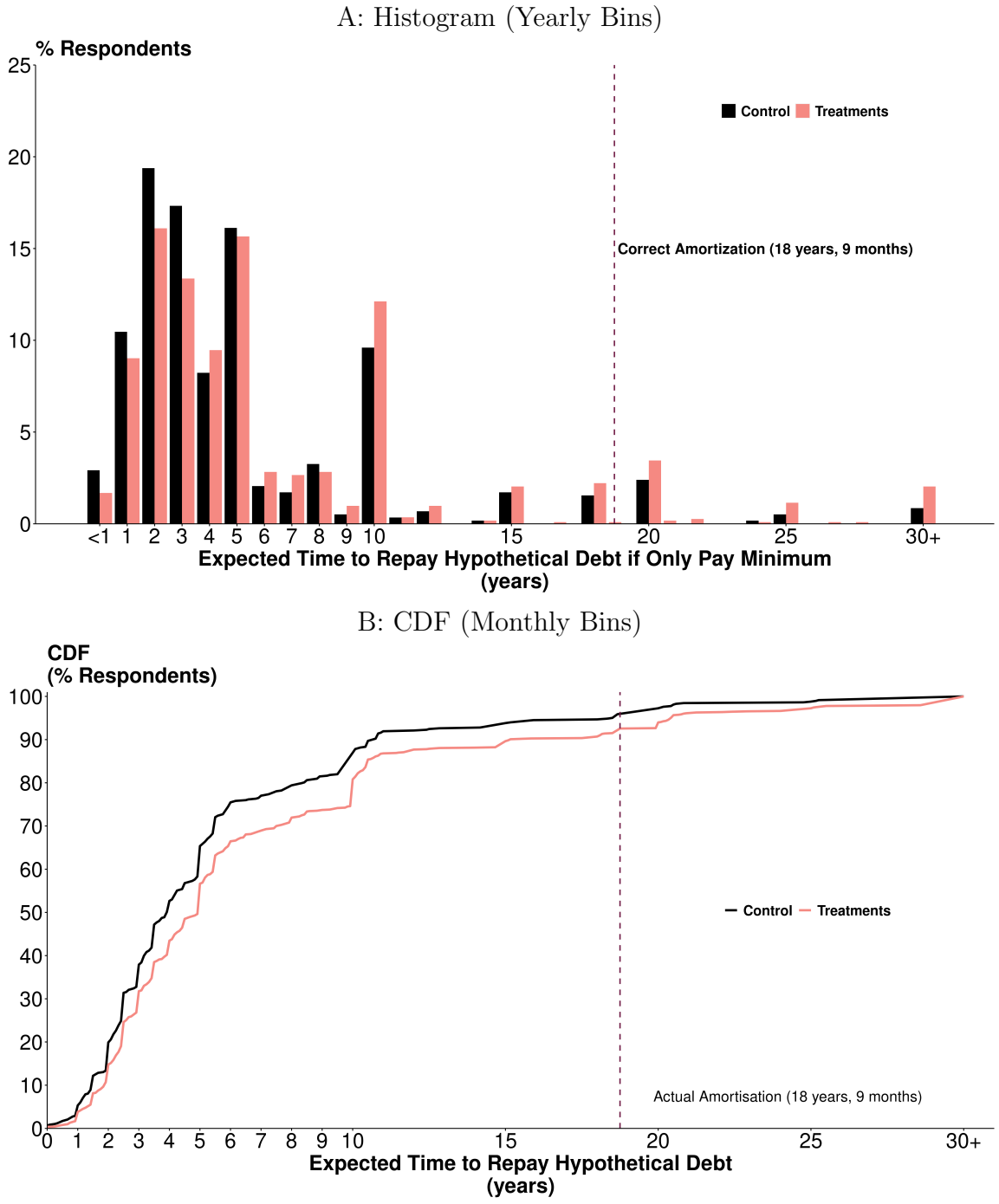


B: Debt Net of Payments



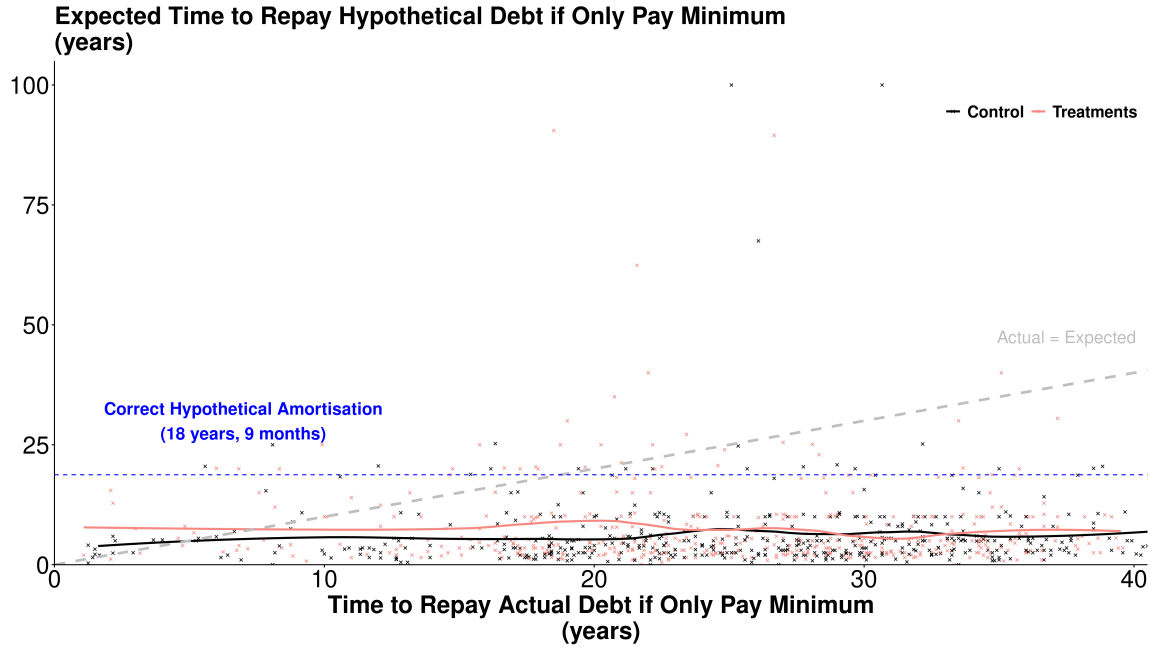
Notes: Estimates are $\delta_{k,t}$ from Equation 1 OLS regression (modified by pooling Lender 1 and Lender 2 and adding lender fixed effects) includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level. Regressions are run separately for samples with and without balance transfer debt.

Figure 11: Automatic minimum payment nudges - effects of treatment on survey respondents' expectations of time to repay hypothetical credit card debt repayment scenario



Notes: "Imagine a credit card statement balance of £1,029.90 with an interest rate of 18.9%. If someone only repays the minimum each month and spends no more on the card approximately how long would you expect it to take them to repay? It doesn't matter whether the answer you give is right or wrong. We just want to find out what people understand and the question after this one lets you indicate how confident you are in your answer." The correct answer is 18 years and 9 months (225 months). Survey responses were open-ended asking them to input years and months but for presentation we group 30+ years responses into a single category. N = 1,713 (Control: 583, Treatments: 1,130). 'Treatments' group aggregates all treatments: 'Time to Repay', 'Time + Cost to Repay', 'Reminder - Time to Repay' and 'Reminder - Time + Cost to Repay' into single category.

Figure 12: Automatic minimum payment nudges - relationship between time to repay actual credit card debt and survey responses on beliefs of time to repay hypothetical credit card debt in repayment scenario, split by control and treatment



Notes: The blue line is a 45 degree line for a one-to-one relationship between these variables. 'Time to Repay Actual Debt if Only Pay Minimum' is calculated using respondent's actual credit card balance and assumes they only pay the minimum each month and spends no more on their card: for the treatment groups this information was provided to them in the nudge. 'Expected Time to Repay Hypothetical Debt' are answers to the survey question: "Imagine a credit card statement balance of £1,029.90 with an interest rate of 18.9%. If someone only repays the minimum each month and spends no more on the card approximately how long would you expect it to take them to repay? It doesn't matter whether the answer you give is right or wrong. We just want to find out what people understand and the question after this one lets you indicate how confident you are in your answer." The correct answer is 18 years and 9 months (225 months) shown by the horizontal dotted line. Survey responses were open-ended asking them to input years and months but for presentation we group 30+ years responses into a single category. N = 906 for survey respondents who provided consent to anonymously merge responses to their administrative data. 'Treatments' group aggregates treatments without reminders: 'Time to Repay', 'Time + Cost to Repay' into single category.

B Tables

Table 1: Summary statistics for Experiment 1 'Statement Disclosures' and Experiment 2 'Automatic Minimum Payment Nudges' using mean values from the month preceding start of experiments

	Experiment 1:	**Experiment 2:**		
	Statement Disclosure	**Automatic Minimum Payment Nudge**		
Outcomes	**Lender 3**	**Lender 1**	**Lender 2**	**Lender 3**
Age (years)	46.9	45.79	42.06	44.41
Female (% cards)	45.66	40.54	42.90	40.60
Credit Limit (£)	7,862.74	5,987.56	6,656.97	8,517.67
Credit Score (0-100)	69.91	66.42	63.8	69.24
Purchases Rate (%)	17.97	21.82	22.62	17.75
Balance Transfer (% cards)	48.73	10.51	11.86	52.73
Any Automatic Payment Set-up (% cards)	3.11	99.95	98.73	96.71
Any Automatic Full Payment Set-up (% cards)	0.01	0.00	0.07	0.18
Any Automatic Fixed Payment Set-up (% cards)	1.43	0.02	0.46	5.17
Any Automatic Minimum Payment Set-up (%cards)	1.67	99.93	98.2	91.37
Credit Card Statement Balance (£)	3,532.11	3,423.11	3,630.27	4,430.29
Credit Card Statement Balance Net of Payments (£)	3,236.41	3,191.47	3,352.93	4,186.83
Full Payment in Preceding Cycle (% cards)	13.78	1.80	3.41	9.43
Minimum Payments in Preceding Cycle (% cards)	15.22	76.82	78.46	75.86
Number of Full Payments in Prior 12 Cycles	0.68	1.15	0.77	0.38
Number of Minimum Payments in Prior 12 Cycles	2.03	8.24	8.91	10.33
Any Full Payments In Prior 12 Cycles (% cards)	31.05	34.4	25.57	16.78
All Minimum Payments in Prior 12 Cycles (% cards)	3.01	30.99	34.97	54.23
Credit Card Statement Balance	79.09	91.91	90.55	86.53
Net of Payments (% Statement Balance)				
Total Credit Card Statement Balances (£)	8,916.96	8,834.78	7,979.41	11,664.54
Total Net Credit Card Statement Balances (£)	7,946.98	8,215.13	7,456.34	10,633.98
N	29,683	65,394	60,000	28,364

Table 2: Primary Outcomes for Experiment 1 'Statement Disclosures': Average Treatment Effects from Equation 1 Regressions After Six Statement Cycles

Outcomes	**Control Group Mean**	**Treatment**	**Estimate (s.e.)**
Any minimum payment	0.160	Time	0.0038
			(0.0047)
		Time + Cost	-0.0052
			(0.0046)
Any full payment	0.178	Time	-0.0061
			(0.0056)
		Time + Cost	0.0024
			(0.0056)
Any payment less than minimum payment	0.059	Time	-0.0026
			(0.0033)
		Time + Cost	-0.0001
			(0.0033)
Statement balance net of payments (% statement balance)	0.750	Time	0.0045
			(0.0053)
		Time + Cost	-0.0009
			(0.0054)
Costs (% statement balance)	0.024	Time	-0.0013
			(0.0018)
		Time + Cost	0.0007
			(0.0019)
Transactions (% statement balance)	0.087	Time	0.0070*
			(0.0035)
		Time + Cost	-0.0008
			(0.0034)
CRA share of credit cards only paying minimum	0.130	Time	-0.0026
			(0.0028)
		Time + Cost	-0.0024
			(0.0028)
CRA share of credit cards making full payment	0.390	Time	0.0000
			(0.0034)
		Time + Cost	-0.0025
			(0.0034)
CRA share of credit cards missing payment	0.010	Time	0.0001
			(0.0011)
		Time + Cost	-0.0007
			(0.0010)
CRA total credit card statement balances net of payments (% statement balances)	0.808	Time	0.0020
			(0.0038)
		Time + Cost	0.0019
			(0.0038)

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Estimates are $\delta_{k,6}$ from Equation 1 OLS regression that includes time fixed effects and pre-experiment control variables with standard errors clustered at card-level (29,683 cards).*

Table 3: Automatic minimum payment nudges - T-tests of effects of treatment on survey respondents' expectations of time to repay hypothetical credit card debt repayment scenario

Outcome	Mean (Control)	Mean (Treatments)	Estimate	95 % C.I.	P Value
Expected Months	70.57	84.53	13.96	[4.64, 23.28]	0.00337
Expected < 3 Years	37.91%	31.77%	-6.14	[-10.93, -1.35]	0.01211
Expected < 5 Years	65.35%	56.64%	-8.71	[-13.55, -3.88]	0.00042
Expected < 10 Years	86.96%	80.80%	-6.17	[-9.74, -2.59]	0.00074
Expected < Actual	95.88%	92.57%	-3.32	[-5.54, -1.09]	0.00352

Notes: "Imagine a credit card statement balance of £1,029.90 with an interest rate of 18.9%. If someone only repays the minimum each month and spends no more on the card approximately how long would you expect it to take them to repay? It doesn't matter whether the answer you give is right or wrong. We just want to find out what people understand and the question after this one lets you indicate how confident you are in your answer."
The correct answer is 18 years and 9 months (225 months). Survey responses were open-ended asking them to input years and months but for presentation we group 30+ years responses into a single category. N = 1,713 (Control: 583, Treatments: 1,130). 'Treatments' group aggregates all treatments: 'Time to Repay', 'Time + Cost to Repay', 'Reminder - Time to Repay' and 'Reminder - Time + Cost to Repay' into single category.

Table 4: Automatic minimum payment nudges - Treatment effects of relationship between actual time to repay credit card debt (years) and survey responses on beliefs of time to repay hypothetical credit card debt repayment scenario (years)

	Estimate (s.e.)
CONSTANT	5.0738***
	(0.7699)
ACTUAL	0.0423
	(0.0301)
TREAT	3.5774*
	(1.3986)
TREAT * ACTUAL	-0.0906
	(0.0506)

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. Estimates are from Equation 2 OLS regression with robust standard errors. N = 906 survey respondents who provided consent to anonymously merge responses to their administrative data.* $R^2 = 0.0079$.

Table 5: Survey evidence statistics for Experiment 2 'Automatic Minimum Payment Nudges': Summary statistics for respondents with automatic minimum payments

Question	Outcome (%)	Mean	95 % C.I.
Why have automatic payment?	Prevents late fee	0.5815	[0.5530, 0.6101]
	Prevents credit score impact	0.5510	[0.5222, 0.5798]
	Prefer this control	0.2764	[0.2505, 0.3023]
	Easy	0.1822	[0.1599, 0.2046]
	Unstable finances	0.0767	[0.0613, 0.0921]
	Never thought why	0.0235	[0.0148, 0.0323]
	Wanted to cancel, didn't get around to	0.0017	[-0.0007, 0.0042]
	Other	0.0096	[0.0039, 0.0152]
	Number of responses (#)	1.70	[1.64, 1.76]
Why automatic minimum payment not automatic fixed payment?	Prefer this control	0.3976	[0.3692, 0.4259]
	Didn't know could	0.1805	[0.1582, 0.2027]
	Only afford minimum	0.1988	[0.1757, 0.2219]
	Prefer minimum	0.1744	[0.1524, 0.1963]
	Easy	0.1613	[0.1400, 0.1826]
	Unstable finances	0.1046	[0.0869, 0.1223]
	Never thought why	0.0488	[0.0363, 0.0613]
	Didn't understand	0.0340	[0.0235, 0.0445]
	No benefit	0.0323	[0.0220, 0.0425]
	Faster amortisation	0.0148	[0.0078, 0.0218]
	Wanted to cancel, didn't get around to	0.0078	[0.0027, 0.0130]
	Other	0.0174	[0.0099, 0.0250]
	Number of responses (#)	1.37	[1.33, 1.41]
Liquidity Constraints (How well keeping up with bills?)	Keeping up, no problem	0.3810	[0.3529, 0.4091]
	Keeping up, occasional struggle	0.4246	[0.3960, 0.4532]
	Keeping up, constant struggle	0.1404	[0.1203, 0.1605]
	Falling behind with some	0.0366	[0.0257, 0.0475]
	Having real problems & fallen behind with many	0.0113	[0.0052, 0.0175]
	No commitments	0.0061	[0.0016, 0.0106]
Stated Preferences	Reduce debt	0.7071	[0.6807, 0.7334]
	Save more	0.2668	[0.2412, 0.2924]
(In one year prefer to...)	Spend more	0.0087	[0.0033, 0.0141]
	Don't know	0.0174	[0.0099, 0.0250]
Minimum Payment	Correct Answer (months)	225	
Amortization Beliefs	Expected (months)	78.28	[73.64, 82.92]
(If only pay minimum	Expected < 3 years	0.3435	[0.3160, 0.3710]
how long expect to repay?)	Expected < 5 years	0.5920	[0.5635, 0.6204]
	Expected $<$ Correct	0.9372	[0.9232, 0.9513]
	Confidence in Expected (1-10)	3.84	[3.70, 3.98]
Desired Minimum Amortization	Desired (months)	38.36	[35.18, 41.55]
	Unable to Afford	0.0392	[0.0280, 0.0505]

Notes: See Online Appendix section 'B: Survey Questionnaire' for exact question drafting. N = 1,147 respondents who confirmed they still had automatic minimum payment at time of survey.

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