Default Effects of Credit Card Minimum Payments

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September 2, 2021

Abstract

Credit card minimum payments are designed to ensure that individuals pay down their debt over time, and scheduling minimum automatic repayments helps to avoid forgetting to repay. Yet minimum payments have additional, unintended psychological default effects by drawing attention away from the card balance due. First, once individuals set the minimum automatic repayment as the default, they then neglect to make the occasional larger repayments they made previously. As a result, individuals incur considerably more credit card interest than late payment fees avoided. Using detailed transaction data, we show that approximately 8% of all of the interest ever paid is due to this effect. Second, manual credit card payments are lower when individuals are prompted with minimum payment information. Two new interventions to mitigate this effect are tested in an experiment, prompting full repayment and prompting those repaying little to pay more, with large counter effects. Hence, shrouding the minimum payment option for automatic and manual payments and directing attention to the full balance may remedy these unintended effects.

keywords: default; inattention; automatic repayment; credit card; consumer debt

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We thank David Laibson and Nick Lee for comments and discussion.

This research was funded by Economic and Social Research Council grants ES/N018192/1, ES/K002201/1, ES/P008976/1, and ES/V004867/1, and Leverhulme Trust grant RP2012-V-022. Sakaguchi acknowledges PhD studentship 1499648 from the Economic and Social Research Council.

This research was conducted while Adams, Guttman-Kenney, Hayes and Hunt were employees of the Financial Conduct Authority. The views and opinions expressed here are those of the authors and do not necessarily reflect the views of the Financial Conduct Authority, Competition & Markets Authority, their affiliates, or their employees.

Default options are frequently presented to individuals across a broad set of contexts and varying financial stakes, including in the domains of organ donation, retirement saving, charitable donations and energy efficiency. Typically, defaults are applied either automatically or as a prompt to action. How do defaults affect individual actions in practice, and do they work as intended? We examine this question using a ubiquitous example of a default payment option — the credit card minimum payment. Designed to protect consumers from spiralling debts and ensure that credit card companies receive a flow of payments, credit card minimum payments are designed to suppress levels of debt and, in the case of automatic minimum payments, provide a form of insurance against forgetting to pay.[1]

Analyzing the default effects of minimum payments is important both for understanding the effects of defaults and for consumer protection in the credit card market. Credit card holders can choose from a range of levels of repayment, such as the full balance on their credit card, a fixed sum of money, or the minimum payment due. They can choose to make payments manually each month, or set up an automatic payment (with the option to manually pay more on top each month). The minimum payment is typically a small fraction of the balance, plus fees and interest. If choosing automatic minimum payments, the card provider automatically debits the card holder's checking account with the minimum due each month.

Understanding the consequences of minimum payment choices for repayment behavior is an important policy issue. Financial media outlets regularly warn their readerships against persistently paying only the minimum payment due (e.g., Nerdwallet's online calculator of the excess interest arising due to making only the minimum payment – CNBC 2018). Moreover, credit card borrowing is the main form of unsecured borrowing by consumers in the United States. Credit card debt in the United States exceeds $900bn (Federal Reserve Bank of New York Consumer Credit Panel / Equifax, 2019), having grown steadily over the past decade following a decline during the 2007-2009 Great Recession. Most households are affected, with average household credit card debt of approximately $6,300 (Federal Reserve Survey of Consumer Finances, 2019).

[1] Other examples of default automatic payments include standard repayments for mortgages and cell phone bills. Other examples of default prompted payments include suggested tip amounts for cabs and restaurants (Haggag and Paci, 2014).

In this paper we use credit card-level transaction data and an online experiment to study the default effects of minimum payments. In particular, we investigate the unintended consequences of minimum payments. While in theory minimum payments reduce the level of debt, and automatic minimum payments minimise forgetting, in practice there may be unintended outcomes. For example, while minimum automatic repayments reduce missed payments as consumers do not need to remember to repay, an unintended affect may be lower average payments due to consumers not paying attention to the balance (which might prompt them to make larger payments). In this way, understanding effects across multiple outcomes (in this case missed and average payments) is critical to both for evaluate how and whether defaults work, and for policy recommendations. In the presence of unintended consequences, the positive effects of this default in theory may not be seen in practice.

Our first main contribution is to show that the default effect of credit card minimum automatic payments has an unintended consequence of higher interest payments. We use a field study of credit card transaction data to study what happens when consumers switch to automatic repayments. We estimate causal effects using two identification methods: (i) a difference-in-differences design utilizing a matched control group and (ii) an instrumental variables analysis exploiting peer effects in the adoption of minimum automatic repayment. Our results show that, while reducing the probability of missed payments to near-zero, default minimum automatic payments also reduce overall payments (by approximately 40%). This thereby causes higher revolving balances, and higher interest costs. We simulate the magnitude of this effect and show that it is large. The simulations show that cards using minimum automatic repayments at least once in the data period could save about 19.8% of interest and fees if they did not switch to minimum automatic repayment. This is about 8.4% of the all interest and fees paid in the credit card market.

Our second main contribution is to test new remedies to overcome the default effect of presenting the credit card minimum payment. Using an experimental study, we replicate the existing finding that merely presenting minimum payment information to card holders whilst they choose a level of manual repayment is also detrimental, reducing average repayments by 16% and causing the distribution of payments to be bunched at or just above the minimum.

Presenting the minimum payment as a default, implicit repayment amount appears to anchor the manual repayment decision upon the minimum payment. We test two new remedies, both of which have large effects. First, we find that prompting people to pay in full increases average repayments by 24% - without crowding out the effect of removing the minimum payment anchor (a result we replicated from prior studies). Second, we find that, when low payment prompts are provided only to people who would had initially chosen to pay the minimum or an amount close to it, these people are then more than twice as likely than not to revise their repayments choice to a higher amount. Hence, our results show that interventions can work to override the default effect. This finding may be applicable in other contexts in which a default option delivers benefits, but unintended consequences require mitigations to ensure that adverse outcomes are avoided.

Previous Literature

Effects of defaults Psychological theories suggest that a default option has a large probability of being chosen because of people's cognitive laziness or status quo bias (Johnson and Goldstein, 2003). A large literature assumes that default options can be used to the benefit of consumers. This literature has achieved significant policy impact, in a wide variety of domains including organ donation, retirement saving, energy efficiency and web marketing. Jachimowicz et al. (2019) provide a meta analysis of 58 published studies. However, while more than 45 of these studies identify positive effects from defaults in the outcomes observed (with only 4 estimating negative effects), the authors find a wide range of effect sizes.

In addition, studies which focus on target outcomes (in our example, whether an individual missed a credit card repayment) might omit important other outcomes, especially those which exhibit unintended consequences (in our example, reduced average repayment). Hence, while a particular default might in theory achieve a *proximate* outcome indicating success, the *distal* effects of the default include unintended consequences which could potentially render the net effect of the default harmful (Guttman-Kenney et al., 2021).

We summarize the findings from previous studies, including evidence of potential downsides of default options, in Table 1 (see Jachimowicz et al. (2019) for more details). For example, studies of automatic enrolment into retirement saving using pension contribution data from

US employers show that the introduction of automatic enrolment increases pension coverage among employees. However, automatic enrolment to a default level of saving can reduce savings rates for employees who would otherwise have saved at a higher rate when making an active choice (Choi et al., 2001), reduce saving by other means, and increase the level of indebtedness held by consumers (Beshears et al., 2019). Hence it is not clear whether auto-enrolment increases the overall level of saving (which is the key objective of the policy). Hence, in a variety of settings, there is a need to understand the full effects of defaults and how to mitigate against potential downsides of default options.

Table 1: Summary of Studies on Effects of Defaults

Topic	Studies	Key Findings
Organ Donation	Johnson and Goldstein (2003); Willis and Quigley (2014); Shepherd et al. (2014); Arshad et al. (2019); Li et al. (2013); Van Dalen and Henkens (2014)	Opt-out policies for organ donation (in contrast to opt-in policies) lead to large increases in donation rates, though raise ethical concerns and can lead to post-death dispute with next of kin.
Retirement Saving	Cronqvist and Thaler (2004); Choi et al. (2001); Choi et al. (2003); Beshears et al. (2006); Madrian and Shea (2001); Beshears et al. (2019)	Auto-enrolment into pensions increases coverage rates and level of pension saving for most participants. However, reduced other saving (including non-pension saving) and increased levels of debt might offset the increase in pension saving.
Energy Efficiency	Momsen and Stoerk (2014); Anda and Temmen (2014); Fowlie et al. (2017); Bastida et al. (2019); Hedlin and Sunstein (2016)	Defaults can be effective at increasing uptake of renewable energy usage, but may have only short-lived effects as consumers switch to cheaper energy deals at renewal points.
Charitable Giving	Zarghamee et al. (2017); Goswami and Urminsky (2016); Fiala and Noussair (2017); Schulz et al. (2018)	Default options increase occurrences of charitable giving, but may reduce overall value of amount given by reducing giving over-and-above the default level.

Defaults in the credit card market A large number of studies have focused on default effects in the credit card market. Attention to this issue has been drawn both by the magnitude of credit card debt, and therefore importance of regulatory policy regarding minimum payments, but also due to the availability of objective, high frequency credit card records providing data on a variety of outcomes. This makes the credit card market a promising test-bed for evaluating defaults and informing policy design, especially as policy changes result in fast feedback compared with other settings (such as organ donation and retirement saving).

Findings from these studies are summarised in Table 2. A first set of studies has focused on whether default information disclosures help to stimulate higher payments. For example, Agarwal et al. (2015) find, in mass transaction data, that including the minimum level of repayment required to clear debt in 3 years by default, as mandated by the US CARD Act, had no overall effect on repayments. Studies from Mexico (Seira et al., 2017) and the UK (Adams et al., 2021) show similar ineffectiveness of informational nudges. A second set of studies has focused on whether shrouding the minimum payment on credit card statements and/or online payment journeys (for example, by either removing the minimum payment completing, or only showing the minimum payment when the card holder attempts to pay below the minimum payment due) might help to increase payments.[2]

Our study complements the existing literature by studying the unintended consequences of minimum payments, exploring whether the forms of unintended consequence seen in the domains described in Table 1 might also exist in the credit card market. If such unintended consequences exist, there is a rationale for mitigating actions to preserve the positive impact of the default while mitigating the unintended negative consequences. We explore examples of this in the experimental study which follows.

[2]For example, showing the level of minimum payments may reduce attention to balances, which might also interact with present-biased preferences (O'Donoghue and Rabin, 1999). It is well known that an anchor value selected by an experimenter causes the participant's subsequent estimation to biased towards the anchor (Tversky and Kahneman, 1974). Theoretical explanations vary from insufficient adjustment from the anchor (Tversky and Kahneman, 1974; Epley and Gilovich, 2001, 2006), numerical priming (Jacowitz and Kahneman, 1995; Wong and Kwong, 2000), selective availability and semantic priming of information during hypothesis testing (Chapman and Johnson, 1994; Mussweiler and Strack, 1999, 2001), elaboration (Wegener et al., 2010), and scale distortion (Frederick and Mochon, 2012) — an overlapping and non-exhaustive list. Even arbitrary numbers affect choices, such as using the last few digits of a phone number (Russo and Shoemaker, 1989) or social security number (Ariely et al., 2003).

Table 2: Summary of Studies on Effects of Defaults in the Credit Card Market

Topic	Studies	Key Findings
Repayment horizon information disclosures	Agarwal et al. (2015); Adams et al. (2021); Seira et al. (2017); Hershfield and Roese (2015); Keys and Wang (2019); Navarro-Martinez et al. (2011)	Disclosing the level of repayments necessary to clear debt within shorter time periods and/or illustrating savings from faster repayments has no effect on observed payments even if information is non-neutral nudging encouraging debt reduction.
Shrouding the minimum payment	Bartels and Sussman (2018); McHugh and Ranyard (2016); Navarro-Martinez et al. (2011); Salisbury (2014); Salisbury and Zhao (2019); Stewart (2009); (Guttman-Kenney et al., 2018); Guttman-Kenney et al. (2021)	The minimum payment may act as an anchor, reducing payments below the level which would otherwise be paid.

FIELD DATA STUDY: AUTOMATIC MINIMUM PAYMENTS

In this first study, we explore the effects on repayments of switching to a default minimum automatic repayment.

Empirical Approach

Our empirical approach uses card-level data for a large sample of UK card holders. Assignment to a default minimum automatic repayment is non-random, so we therefore use quasi-experimental methods. We first use a difference-in-differences approach based upon a matched sample of non-treated cards. We also use an instrumental variable approach, exploiting peer effects in the adoption of default minimum automatic repayment. These methods yield consistent results.

Data and Sampling

Data source. The data were provided by UK credit card issuers, who together account for 40% of the UK credit card market by number of cards. Credit card products in the UK resemble those in the US, with many US credit card issuers active in the UK market. The distributions of credit card spending, balances and payments in UK (Financial Conduct Authority, 2016) and US data (Keys and Wang, 2019) are similar to one another. In addition, repayment patterns found in UK data (Gathergood et al., 2019b) have replicated in US data (Gathergood et al., 2019a). The data were extracted and provided by Argus Information & Advisory Services in collaboration with the UK Cards Association, without constraint on the research agenda. Card holders and issuers are not identified in the data we received. The data are a 10% random sample of all UK consumers who held a credit card during January 2013 to December 2014 within Argus's database, which covers nearly 100% of UK card holders.[3]

Data cleaning. The cleaned data include card identifiers, balances, required minimum amounts, purchase amounts, purchase types, repayment amounts, and various types of fees and finance charges. The unit of observation in the sample is a card-month. Repayments appear on the statement date for the month after the statement containing the balance. For example, repayments reported in December 2014 statements were made against the bill showing the balance and the required minimum in November 2014. Because no repayment data are available for January 2015, repayments for balances in December 2014 are unknown. Thus, the data provide at maximum 23 balance-repayment observations per card from January 2013 to November 2014. Automatic payment was available as a repayment option for all cards throughout the data period.

The data include records of minimum payment amounts due, together with a record of whether the payment was made manually or automatically. The minimum amount card holders must pay each month is, in the UK, normally interest and fees accrued within the month plus 1% of the card balance, or a fixed sum (typically £5 or £25) whichever is the greater. As long as the card-holder pays at least the minimum payment they will be in good standing with the card issuer and avoid a late payment fee or other costs such as additional interest costs on balances in arrears and missed payments being recorded on their credit file. Making a repayment of at least the monthly accrued interest ensures that the value of the debt does not grow. Additionally, repaying 1% of the balance implies that over time the debt will be repaid, though the pay-down horizon is typically many years. Automatic repayments are made by a mechanism known as "Direct Debit". Direct Debit is an extremely common method for paying bills in the UK, and

[3]Complete R source code is available for all steps from importing the data exported from Argus to the statistics, tables, and figures in this paper. We are retaining the data for 10 years. Data are proprietary but are available for replication on a local computer.

has growing coverage in the US, where it has been introduced more recently and is variously known as “AutoPay” or “automatic repayment”.

Sample restrictions. We applied a number of sample restrictions to create a baseline sample for analysis. First, we excluded cards which were closed or charged-off during the data period. Second, we excluded cards having a balance transfer and those having a zero merchant APR for part of the data period, as balances on these cards do not accrue interest.[4] Fourth, we excluded a small number of cards with unidentified transactions. Fifth, we restricted the sample to extracted card-months with a positive balance due.

Illustrative Results

Finding 1: Switching to automatic payment is common, especially automatic minimum payment. Over one-third of cards in the sample pay by automatic payment, with this share increasing over time. In the sample, 5% of cards switched to automatic repayment during the sample period, with 29% already paying by automatic payment from the start of the sample period (the remaining 66% using manual payment throughout the period).[5]

We classify cards switching to automatic payment into three types by the level of payment: the minimum payment, a fixed monetary value between the minimum and full payment, and the full payment. Table 3 presents a breakdown of switches to different automatic repayment types. Among all cards switching during the data period, we observe 46.7% switching to a minimum automatic repayment.[6]

[4]Cards were treated as having a balance transfer when an aggregation of the beginning balance and all transaction amounts within a month including purchases, cash advances, fees, finance charges, and repayments differ from the end of the month balance by £10 or more.

[5]Among the 5% who switch to automatic payment, switching back to manual payment is a rare event.In some cases, card holders do not cancel their autopay instruction but instead make payments prior to the autopay debit date (in which cases the autopay instruction is paused). Given that card holders can pre-pay in this way, the benefits to cancelling an autopay instruction are small and this may explain the rarity in the data.

[6]For approximately 15% of card we cannot identify the repayment type. For example, we cannot identify the automatic repayment policy for cards whose balance is sufficiently small in all months that the minimum payment required is sufficient to clear the balance. Such cards could be set to repay the full balance, for example, but with the full balance being equal to the minimum payment we cannot determine the repayment policy from observed payment behavior.

Table 3: Counts of Switching from No Automatic Repayment to Different Automatic Repayment Types.

Statistics	Min	Fixed	Full	Mixed/Unknown	Total
Num. of Repayments	188,288	53,363	91,203	63,542	396,396
Proportion (%)	47.5	13.5	23.0	16.0	100.0
Num. of Cards	9,803	2,698	5,162	3,315	20,978
Proportion (%)	46.7	12.9	24.6	15.8	100.0

Note. "Min" column represents minimum automatic repayment. "Fixed" column represents fixed automatic repayment covering more than the minimum but less than the full balance. "Full" column represents full automatic repayment of the balance. "Mixed/Unknown" column represents automatic repayments which changed between types across months or automatic repayments which we cannot identify the type mostly due to sufficiently small balances and payments throughout the data period.

Table 4 shows card-month level descriptive statistics for account terms and card usage before and after switching for the sample of switchers. These indicate that switching does not appear on average to be associated with large changes in account terms (such as APR or credit limit).[7]

Table 4: Summary Statistics.

Statistics	Switch to Min-Auto		Switch to Fixed-Auto		Switch to Full-Auto	
	Before	After	Before	After	Before	After
Number of observations	62,708	125,580	21,728	31,635	44,435	46,768
Number of cards	9,803	9,803	2,698	2,698	5,162	5,162
Median balance	1,089	1,081	1,122	1,140	387	317
Median credit limit	4,500	4,850	4,000	4,000	5,000	4,800
Median utilization	0.343	0.319	0.419	0.420	0.084	0.074
Median spending amount	124	77	72	24	383	333
Median merchant APR	0.180	0.189	0.189	0.199	0.169	0.170
Median cash APR	0.249	0.249	0.249	0.249	0.249	0.249
Median charged-off rate	0.005	0.006	0.005	0.005	0.002	0.002

Note. "Min-Auto" represents minimum automatic repayment. "Fixed-Auto" represents fixed automatic repayments covering more than the minimum and less than the full balance. "Full-Auto" represents full automatic repayment. Median values are calculated using all card-month observations with a positive balance.

Finding 2: Missed payments drop after switching to automatic minimum payment. Figure 1 illustrates the proportion of payments before and after switching to automatic minimum payment. The categories shown are missed (i.e., payment below the minimum payment due), minimum, large (a payment between minimum and full) and full. Note that the figure shows their total actual payments made rather than their automatic payment amount. For example, an

[7] In additional analysis using linked geodata we find there are socio-economic differences between those switching to full automatic repayments and those switching to minimum automatic repayments (full results in Web Appendix table A1). The latter group are disproportionately drawn from localities with on average lower incomes, education levels and house prices, and higher rates of jobless claimants and claimants of free school meals for children, which is a proxy measure of UK social security benefit receipt.

individual might have an automatic minimum payment but choose to pay a larger amount, or full amount. The leftmost bars show that after switching the proportion of missed payments falls from approximately 12% per month to 1% per month. Missed payments are not completely eliminated because the account holder may have insufficient funds in the checking account from which the automatic payment is being drawn. This large reduction in missed payments shows the benefit of automatic payment.

Finding 3: Switching to automatic minimum payment reduces large and full payments. The bars on the right-hand side of Figure 1 illustrate that after switching the proportion of large and full payments drops (from approximately 40% to 10%, and 29% to 25% respectively). This is due to a large increase in the proportion of minimum payments, which increases from approximately 19% to 52%. This reduction in large and full payments shows the effect of individuals choosing automatic minimum payments as the default no-action outcome, and not making manual payments to the same value as they made before switching. This is the potential downside of automatic minimum payments, as it implies higher revolving balances and therefore excess interest charges.

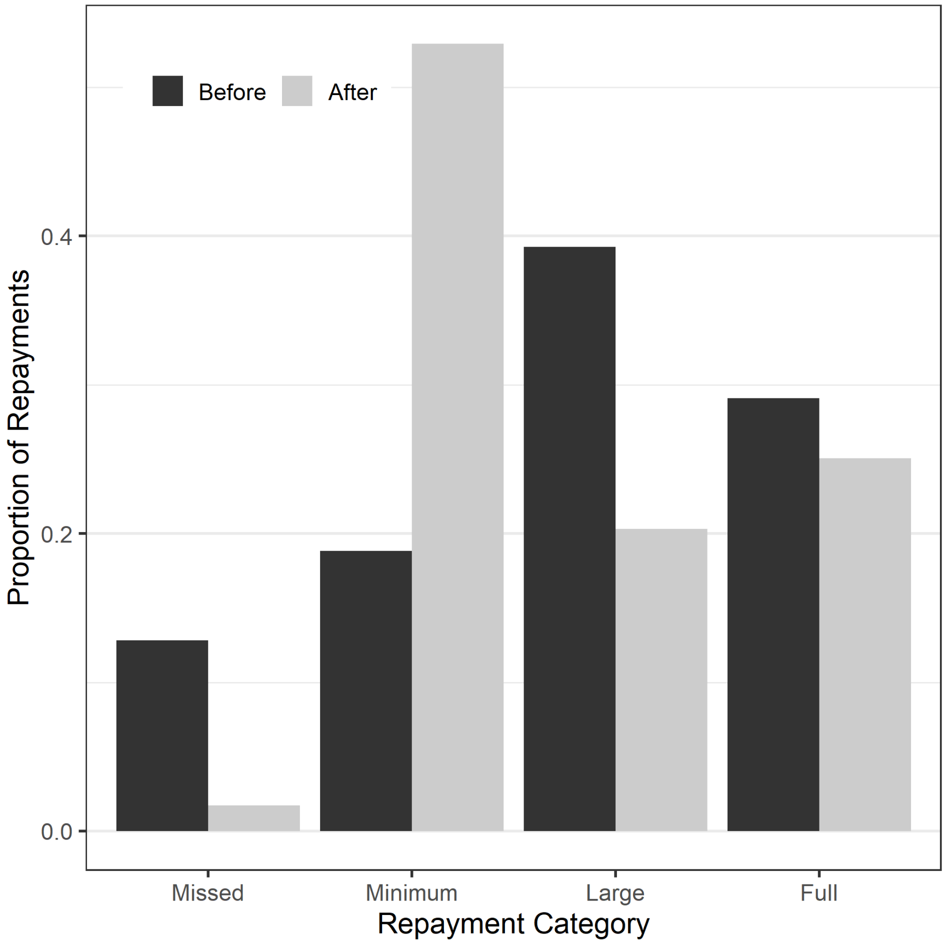


Figure 1: Bar chart of proportion of repayments in months before and after switching in four mutually exclusive and exhaustive categories: missed (below minimum), minimum, large and full. The unit of observation is a card-month (total repayments to a particular card in a particular month). The sample consists of monthly repayments made between an account opening date or the beginning of the data period (Jan. 2013) whichever is latter and the end of the data period (Nov. 2014). The sample was restricted to cards switching to automatic repayments during the data period. The number of months contributing to the figure differs among cards while the number of months included in before and after periods differs within a card. Card-months with no balance were excluded.

Difference-in-Differences Estimates

Identification Strategy The illustrative results presented suggest that switching to minimum automatic repayments leads to reduced payments due to card holders being much less likely to make additional payments over the minimum. This result is descriptive. Ideally to test the causal effect of the treatment (switching to minimum automatic repayment) on the outcome variable (level of repayment) we might like to exploit a randomised-control trial or naturally occurring experiment. However, in reality switching is likely to be non-random, and might be correlated with an intention to pay less in future, an omitted variable which could generate endogeneity bias. In such a scenario, payments would have decreased even absent the switch to the automatic minimum payment as default. We therefore need an empirical approach to determine the casual effect of automatic minimum repayment on repayment behavior.

To address this, we utilise a difference-in-differences model and, separately, an instrumental variable model. The difference-in-differences approach is to estimate the effect of minimum automatic repayments on payment behavior by introducing a control group whose payment behavior is used as a counterfactual for the switching group (i.e., what would have happened in the absence of the switch). This approach compares the change in repayments of the treatment group (those switching) with the change in repayment of a control group of non-switchers. The general form of the econometric equation to be estimated is given by:

$$Repayment_{i,t} = \beta_1 + \beta_2 Switcher_i + \beta_3^T D_{i,t} + \beta_4^T Switcher_i \times D_{i,t} + \epsilon_{1,i,t}$$

in which the outcome variable is the level of repayment by individual i in month t, $Switcher_i$ is a dummy variable whose coefficient captures level differences in repayment between switchers and non-switchers, $D_{i,t}$ is a dummy variable whose coefficient captures level differences in repayment before and after the month individual i switches, and the coefficient for the interaction between the two captures the difference in the change in repayments across the switching and non-switching groups given by β_4^T. This general model relies upon the assumption that the treatment and control groups have common trends in repayment, and that no other confounding change which might differentially affect repayment by the treatment and control group occurs at the same time as the treatment.

In our setting, estimation of the difference-in-difference model is complicated by two factors. First, we observe repayments only for individuals in months when card holders make a repayment (i.e. non-missed payments). As seen in Figure 1, some card holders miss payments in months both before and after adoption of automatic minimum payment. Second, we do not have a naturally occurring control group. We address the first issue using a first-stage equation to model repayment (using a single-hurdle model exploiting card tenure as an exclusion restriction) and address the second issue through construction of a control group using matching methods.

Single hurdle difference-in-differences model specification. Given that switching to minimum automatic repayments greatly affects the likelihood of a missed payment, our econometric model needs to account for whether a payment is made. One option could be to omit observations of missed payments from the analysis altogether (see later robustness tests), however this approach reduces the sample size and fails to account for the existence of missed payments.

We therefore use a single-hurdle selection model (Cragg, 1971) (implemented in R using the code provided by Carlevaro et al. (2009)) to model missed payments. The model includes a single hurdle of a repayment being made (i.e., not-missed) before card holders making a "choice" of a positive repayment value. As an exclusion restriction we draw upon card tenure, exploiting the tendency of missed payments to reduce with card tenure due to a tendency of cardholders to forget to repay their cards when first issued, a feature of credit card repayments we document and explore in a recent paper (Gathergood et al., 2021). This provides an arguably exogenous source of variation in missed payments.

In the model, $I_{i,t}$ is an indicator having a value of 0 if the card holder i misses the repayment in month t, otherwise 1. Using the probit model, the first component of our model estimates the probability of not-missing a repayment through a latent dependent variable, $P(I_{i,t} = 1) = P(Repayment^*_{1,i,t} > 0)$. Note that the superscript $*$ for $Repayment^*_{1,i,t}$ indicates latent repayment. The subscript of 1 for $Repayment^*_{1,i,t}$ indicates the first component of the model, the choice over missed or not-missed. Similarly, the subscript 2 for $Repayment^*_{2,i,t}$ used below indicates the second component of the model, the level of payment conditional on not missing a payment.

The first equation, for missed or not-missed payment, is given by:

$$Repayment^*_{1,i,t} =$$
$$\beta_0 + \beta_1 Switcher_i + \beta_2^T D_{i,t} + \beta_3^T Switcher_i \times D_{i,t} + \beta_4 Tenure_{i,t} + \beta_5^T x_{i,t} + \epsilon_{1,i,t},$$
$$P(I_{i,t} = 1) = P(Repayment^*_{1,i,t} > 0),$$

where $Switcher_i$ is a dichotomous variable having a value of 1 if the card i switches to a minimum automatic repayment, otherwise 0. $D_{i,t}$ is a vector of dummies specifying months from switch (11 levels from -5 to 5). $Tenure_{i,t}$ is card tenure in years. $x_{i,t}$ is a vector of covariates including balance, utilization, spending amount, merchant APR, cash APR, and charge-off rate (all are month-card level variables). The error term, $\epsilon_{1,i,t}$, is assumed to be normally distributed.

The second stage of the model estimates the differences in differences model (the general form of which is shown above) where the outcome is the repayment amount $Repayment^*_{2,i,t}$, and given by:

$$Repayment^*_{2,i,t} = exp(\beta_6 + \beta_7 Switcher_i + \beta_8^T D_{i,t} + \beta_9^T Switcher_i \times D_{i,t} + \beta_{10}^T x_{i,t} + \epsilon_{2,i,t}),$$

where $Switcher_i$, $D_{i,t}$ and $x_{i,t}$ are defined as in the first regression. The error term, $\epsilon_{2,i,t}$, is assumed to be normally distributed. In addition, $\epsilon_{1,i,t}$ and $\epsilon_{2,i,t}$ are assumed to be uncorrelated.

The above first and second components of the model are jointly estimated on observed monthly repayment amounts, $Repayment_{i,t}$.

$$Repayment_{i,t} = I_{i,t} \times Repayment^*_{2,i,t}$$

Control group design. We construct the control group by using matching methods to select a set of non-switching cards that have very similar characteristics to the switching cards in the pre-switch time period. The implicit assumption in this design is that the constructed control group represents the counterfactual repayment behavior of switchers were they not to have switched to automatic repayments. Hence, any observed difference in post-switch repayments between the groups is attributable to the effects of switching to automatic repayment. This approach relies on the availability of a good match between treatment and control observations such that the two groups show common trends and characteristics in the outcome of interest. By constructing a control group to resemble the treated group, this matched differences in differences approach estimates the average treatment effect on the treated (ATT) group of switchers.

Control group designs based upon matching methods have become widely applied in empirical research in economics and other disciplines (on which see (Abadie, 2021)), with Athey and Imbens (2017) arguing that they are "arguably the most important innovation in the policy evaluation literature in the last 15 years". Such control group designs offer "systematically more attractive comparisons" by creating improved control groups compared with standard difference-in-differences designs.

The data we use provide repeated observations of the same individuals (i.e., time-series cross-sectional sample), and thus, the standard matching methods, which have been developed mostly for cross-sectional sample, may not be appropriate for our case. To overcome this issue, we follow a control group designed by Simmons and Hopkins (2005), together with the matching method formalized by Ho et al. (2011).

We first extracted card-months of switchers for the six month periods pre- and post- switch (total 12 months; we label theses as the treatment observations). For each card, we then take the average of the covariate values over card-months for the six months before the switch. The covariates used are card balance, utilization rate, spending amount, merchant APR, cash APR, charge-off rate, and card tenure. Second, to create a candidate sample for matches, we extract all consecutive six card-months of non-switchers and all consecutive six pre-switch card-months of switchers which do not overlap to the treatment observations.[8] Then, the covariate values within those six card-months are averaged. This procedure reduces the treatment and control observations to a single data point per covariate. We then apply matching to these observations.

After the above transformation of the data, we conducted one-to-one nearest neighbor matching between switchers and candidate cards based on the Mahalanobis distance in the averaged covariates. The result is that each switching card has one matched control card which is very similar in its six month average of covariate values. Finally, post-switch card-months of switchers and subsequent six months following the candidate observations of the matched control group were combined to the matched pre-switch observations. As a consequence, the matched data for analysis include 12 consecutive card-months of each card which consist of six months pre-switch and six months post-switch observations. As a result, treatment group includes 3,311 cards and control group includes 3,239 cards.[9] Both groups have 39,732 card-month observations, equally split between pre- and post-switch periods.

Results. Table 5 compares pre-switch average card profiles between two groups, indicating that the matching created a well-matched control group with similar pre-switch card profiles to those of switchers. (p-values were taken from the bootstrap Kolmogorov-Smirnov test.)

[8]We follow Simmons and Hopkins (2005) where the effect of countries signing to an international agreement (treatment effect) is studied. They define treatment observations as country-years taken from signer countries between 4 year before the signatory and 1 year after that (i.e., 6 country-years for each signer country), and then, define the universe of possible control cases as all continuous 6-year country-periods of both signer and non-signer countries, except signers' country-years which overlap with the treatment observations.

[9]Note that some cards in control group match to multiple cards in treatment group.

Table 5: Comparison of Pre-Switch Average Card Profile

	Treatment	Control	p-value
Ave. Balance	1,843.25	1,827.69	1.000
Ave. Credit Limit	5,462.62	5,425.64	0.951
Ave. Utilization	0.41	0.41	1.000
Ave. Spending	406.43	397.30	0.537
Ave. Merchant APR	0.20	0.20	1.000
Ave. Cash APR	0.25	0.25	1.000
Ave. Charge-off Rate	0.03	0.03	0.118
Card Tenure	7.54	7.54	0.450

Note. p-values are taken from bootstrap Kolmogorov-Smirnov test (1,000 resamples).

Web Appendix D also shows the matching exercise produces switching and control groups which are closely matched by socio-economic characteristics (Table D1) and by pre-switch monthly balances (Figure D1).

The main estimates from the single hurdle differences-in-differences are illustrated in Figure 2 which plots the coefficient estimates and 95% confidence intervals for $Switcher \times Months\ from\ Switch$ interactions for the dummies from five months before switching to minimum automatic repayment through to five months after the switch, shown on the x-axis.

Results indicate that the coefficient on the interaction term is at or close to zero for the months preceding the month of switch, indicating no difference in repayment levels prior to the switch. At the switch month (month zero) the coefficient becomes negative and statistically significantly different from zero. The coefficients also show no trend upwards or downwards in the pre-switch period. This indicates a reduction in payments due to switching. The coefficient on the interaction terms for the subsequent months remains negative and statistically significantly different from zero in each month. Hence there is a precisely defined downwards effects of switching to minimum automatic repayments on the level of payment.

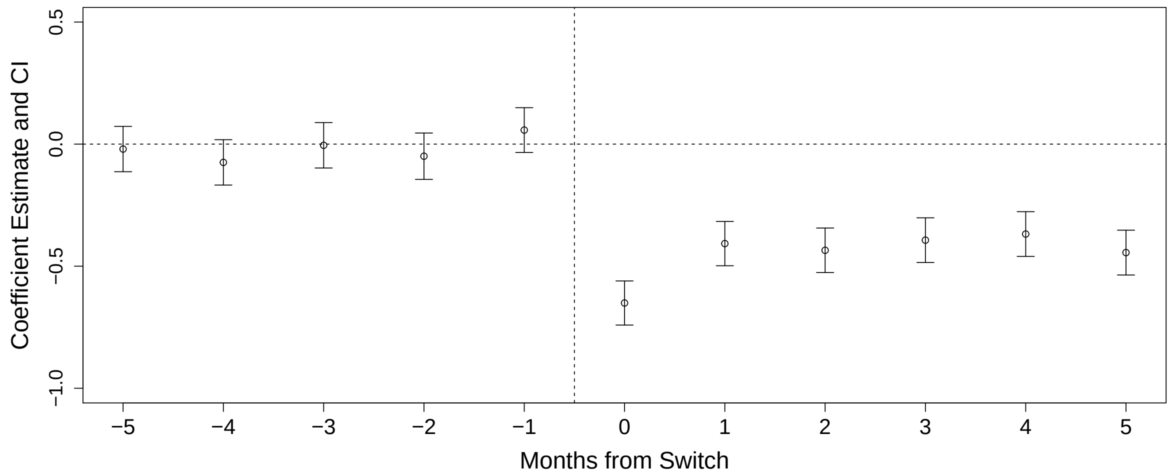


Figure 2: Coefficient estimates and confidence intervals for the effect on payments of switchers switching to a minimum automatic repayment. Error bars are 95% confidence intervals.

The coefficient estimates are shown in Table 6. Results from the first-stage regression are shown in the top panel. The coefficient on the tenure variable is positive, indicating that the probability of not missing a payment rises with tenure, as suggested found in previous studies. The coefficients for the interaction terms of switcher dummy and months from switch dummies are positive and precisely estimated for post-switch months, confirming the beneficial effect of automatic repayments eliminating a chance of forgetting a repayment.

Results from the second-stage regression are shown in the bottom on panel. The coefficients on the covariates indicate that the level of repayment increases with card balance and card spend in the preceding month, and decreases with utilisation, merchant and cash APRs, and the charge-off rate.

The coefficients for the interaction terms are negative and precisely estimated for all post-switch months, confirming the adverse effect of the minimum automatic repayment reducing ongoing repayments as observed in Figure 2. The coefficient values reange from -0.65 in the first month after the switch, rising to -0.36 after four months. A coefficient value of approximately -0.4 for the post-switch interaction in the second element of the model implies that switchers reduce their payments by approximately 40%, which equates to on average £148 (evaluated against the baseline predicted level of repayment from the model).[10]

[10]The predicted effect size of the minimum automatic repayment reducing a repayment given not missing the repayment in months 0 and 1 (the corresponding coefficient estimate=-0.65 and -0.41) are approximately £200 and £150, respectively, assuming that non-switcher's payment level is £450 (the approximate model

Table 6: Coefficient Estimates from Single-Hurdle Selection Model

	Estimate	Std. Error	t-value	p-value	
$h1.(Intercept)$	1.4804	0.0370	39.9968	0.0000	***
$h1.Tenure$	0.0085	0.0013	6.3989	0.0000	***
$h1.Switcher$	-0.1335	0.0493	-2.7059	0.0068	**
$h1.Months\ fr\ Switch : -5$	-0.0393	0.0501	-0.7856	0.4321	
$h1.Months\ fr\ Switch : -4$	-0.0511	0.0499	-1.0247	0.3055	
$h1.Months\ fr\ Switch : -3$	-0.0367	0.0501	-0.7320	0.4642	
$h1.Months\ fr\ Switch : -2$	-0.0063	0.0505	-0.1245	0.9009	
$h1.Months\ fr\ Switch : -1$	0.0250	0.0509	0.4903	0.6239	
$h1.Months\ fr\ Switch : 0$	0.0573	0.0516	1.1103	0.2669	
$h1.Months\ fr\ Switch : 1$	0.1114	0.0526	2.1174	0.0342	*
$h1.Months\ fr\ Switch : 2$	0.0790	0.0522	1.5142	0.1300	
$h1.Months\ fr\ Switch : 3$	0.0687	0.0522	1.3173	0.1877	
$h1.Months\ fr\ Switch : 4$	0.0812	0.0525	1.5456	0.1222	
$h1.Months\ fr\ Switch : 5$	0.0189	0.0515	0.3680	0.7129	
$h1.Switcher \times Months\ fr\ Switch : -5$	-0.0158	0.0687	-0.2301	0.8180	
$h1.Switcher \times Months\ fr\ Switch : -4$	-0.0708	0.0680	-1.0412	0.2978	
$h1.Switcher \times Months\ fr\ Switch : -3$	-0.1479	0.0677	-2.1836	0.0290	*
$h1.Switcher \times Months\ fr\ Switch : -2$	-0.7375	0.0657	-11.2345	0.0000	***
$h1.Switcher \times Months\ fr\ Switch : -1$	0.1643	0.0714	2.3007	0.0214	*
$h1.Switcher \times Months\ fr\ Switch : 0$	1.1440	0.1077	10.6174	0.0000	***
$h1.Switcher \times Months\ fr\ Switch : 1$	1.0999	0.1105	9.9586	0.0000	***
$h1.Switcher \times Months\ fr\ Switch : 2$	1.0992	0.1084	10.1397	0.0000	***
$h1.Switcher \times Months\ fr\ Switch : 3$	0.9672	0.0986	9.8051	0.0000	***
$h1.Switcher \times Months\ fr\ Switch : 4$	0.9391	0.0978	9.5992	0.0000	***
$h1.Switcher \times Months\ fr\ Switch : 5$	0.9511	0.0947	10.0450	0.0000	***
$h2.(Intercept)$	0.3721	0.0381	9.7605	0.0000	***
$h2.Balance$	0.0002	0.0000	75.3268	0.0000	***
$h2.Utilization$	-0.7081	0.0180	-39.4148	0.0000	***
$h2.Spending\ Amount$	0.0008	0.0000	131.1278	0.0000	***
$h2.Merchant\ APR$	-0.8939	0.1321	-6.7660	0.0000	***
$h2.Cash\ APR$	-1.9690	0.1309	-15.0435	0.0000	***
$h2.Charge\text{-}off\ Rate$	-0.4501	0.1042	-4.3213	0.0000	***
$h2.Switcher$	0.0155	0.0335	0.4634	0.6430	
$h2.Months\ fr\ Switch : -5$	-0.0041	0.0330	-0.1244	0.9010	
$h2.Months\ fr\ Switch : -4$	0.0455	0.0330	1.3800	0.1676	
$h2.Months\ fr\ Switch : -3$	0.0098	0.0330	0.2958	0.7674	
$h2.Months\ fr\ Switch : -2$	0.0206	0.0329	0.6260	0.5313	
$h2.Months\ fr\ Switch : -1$	0.0181	0.0328	0.5508	0.5818	
$h2.Months\ fr\ Switch : 0$	-0.0084	0.0328	-0.2562	0.7978	
$h2.Months\ fr\ Switch : 1$	0.0207	0.0328	0.6324	0.5271	
$h2.Months\ fr\ Switch : 2$	-0.0186	0.0329	-0.5660	0.5714	
$h2.Months\ fr\ Switch : 3$	-0.0224	0.0330	-0.6783	0.4976	
$h2.Months\ fr\ Switch : 4$	-0.0130	0.0331	-0.3940	0.6936	
$h2.Months\ fr\ Switch : 5$	0.0237	0.0331	0.7152	0.4745	
$h2.Switcher \times Months\ fr\ Switch : -5$	-0.0201	0.0474	-0.4247	0.6711	
$h2.Switcher \times Months\ fr\ Switch : -4$	-0.0748	0.0474	-1.5770	0.1148	
$h2.Switcher \times Months\ fr\ Switch : -3$	-0.0048	0.0474	-0.1012	0.9194	
$h2.Switcher \times Months\ fr\ Switch : -2$	-0.0495	0.0484	-1.0221	0.3067	
$h2.Switcher \times Months\ fr\ Switch : -1$	0.0576	0.0468	1.2316	0.2181	
$h2.Switcher \times Months\ fr\ Switch : 0$	-0.6508	0.0461	-14.1062	0.0000	***
$h2.Switcher \times Months\ fr\ Switch : 1$	-0.4074	0.0463	-8.7986	0.0000	***
$h2.Switcher \times Months\ fr\ Switch : 2$	-0.4349	0.0465	-9.3490	0.0000	***
$h2.Switcher \times Months\ fr\ Switch : 3$	-0.3933	0.0467	-8.4234	0.0000	***
$h2.Switcher \times Months\ fr\ Switch : 4$	-0.3683	0.0467	-7.8831	0.0000	***
$h2.Switcher \times Months\ fr\ Switch : 5$	-0.4443	0.0468	-9.4871	0.0000	***
$Num.\ Observations$	73,250				
$Log\ Likelihood$	-127838.93				

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. The prefix of $h1$ indicates the estimation of the probability of not-forgetting a repayment (the first component of the model). The prefix of $h2$ indicates the estimation of latent repayment given not forgetting a repayment (the second component of the model).

prediction).

Instrumental Variable Model

The instrumental variable approach uses an exogenous source of variation in the likelihood of treatment, which serves as an instrument to the treatment selection. This approach has the advantage that it introduces as-good-as-random variation in the likelihood of treatment, but has the challenge of needing to find an instrument that predicts treatment (here, switching) but is exogenous to the individual's repayment choice.

Here, we exploit geographic peer group switching rates as the instrument in a two-stage design. The rationale for this instrument is that the switching rate to automatic payments within an individual's peer group is likely to spill over to affect the individual's behavior through shared knowledge and social norms. Peer effects have been found to be important determinants of financial decisions in prior literature (e.g. Bursztyn et al., 2014; Bailey et al., 2018). We estimate the local average treatment effect (LATE) of peer effects induced switching on repayments using a two-stage least squares approach:

$$Post\text{-}Switch_{i,t} = \alpha_0 + \alpha_1^T W_{i,t} + \alpha_1 Switching\text{-}Rate_{t,p} + u_{i,t} \tag{1}$$

$$log(Repayment_{i,t}) = \beta_0 + \beta_1^T W_{i,t} + \beta_2 Post\text{-}\widehat{Switch}_{i,t} + \epsilon_{i,t} \tag{2}$$

where $Repayment$ by individual i in month t is modelled as a function of a set of covariates W which vary by individual and month (as in the earlier analysis, these include card balance, utilization rate, spending amount, merchant and cash APRs, and the charge-off rate) plus a dummy capturing whether the month is pre- or post-switch. This dummy variable is instrumented using a first stage regression in which the instrument is the switching rate in month t among the peer group, p. Here the peer group is defined as the postcode district of residence of the cardholder (UK postcode districts contain 25,000 residents on average). Both regression contain the same set of covariates, W.

Results are reported in Table 7. In the first-stage regression, the coefficients on the covariates indicate that the likelihood of switching increases with the merchant APR and decreases with the cash APR, charge-off rate and level of spending. The coefficient on the instrument is positive (0.950) and precisely defined with a standard error of 0.004, indicating a strong correlation between the switching rate in the locality and the likelihood of the card holder switching.

In the second stage regression the coefficients on the covariates show that repayments increase with the balance and spend, and decrease with the merchant and cash APR, the charge-off rate and account utilization. The coefficient on the instrumented post-switch variable is negative, taking a value of -0.553. This is within the range of the post-switch coefficients from the difference-in-differences model, implying a 55% reduction in repayment arising due to the switch to automatic minimum payment.

Table 7: Coefficient Estimates from Instrumental Variable Estimate

	Dependent variable	
	Post - Switch	*Log(Repayment)*
	OLS	*2SLS*
	(1)	(2)
Balance	-0.00000	0.0002***
	(0.00000)	(0.00000)
Utilization	0.006	-0.443^{***}
	(0.004)	(0.014)
Spending Amount	-0.00001^{***}	0.001***
	(0.00000)	(0.00000)
Merchant APR	0.916***	-0.864^{***}
	(0.027)	(0.101)
Cash APR	-0.930^{***}	-2.316^{***}
	(0.027)	(0.101)
Charge - off Rate	-0.208^{***}	-1.236^{***}
	(0.031)	(0.117)
Switching Rate	0.950***	
	(0.004)	
Post - Switch		-0.553^{***}
		(0.015)
Constant	0.117***	5.376***
	(0.006)	(0.025)
Observations	139,516	139,516
R^2	0.316	0.294
Adjusted R^2	0.315	0.294
Residual Std. Error (df = 139508)	0.378	1.412
F Statistic	9,186.110*** (df = 7; 139508)	

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Robustness Tests

Conditional difference-in-differences model. An alternative approach to modeling repayments in the difference-in-differences framework is to model only non-missed payments. While the single-hurdle selection model including missed payments is the preferred model, for comparison here we also present OLS estimates of the repayment equation. This is estimated on the subset of observations with a non-missed payment (67,475 observations, 92.1% of observations from the sample used in the single-hurdle selection model).

Results, shown in Web Appendix F, reveal the same pattern in the difference-in-differences coefficient estimates on the interaction term (Figure F1), with the estimated coefficient for the pre-switch months indicating no difference in repayment levels prior to the switch. At the switch month (month zero) the coefficient becomes negative and statistically significantly different from zero. This again indicates a reduction in payments due to switching. Coefficient estimates are reported in Table F1. The estimates from the OLS model are similar in magnitude to those from the single-hurdle selection model (the coefficient on the interaction term for month zero is -0.67 in the OLS estimates compared with -0.65 in the single-hurdle selection model estimates).

Type II tobit model. The single-hurdle selection model assumes independence in the error terms across the first-stage and second-stage equations, $\epsilon_{1,i,t}$ and $\epsilon_{2,i,t}$. However, in practice the error terms may be correlated. To test the robustness of our results to this assumption of independence, we also present estimates from an exponential type II Tobit model, which assumes dependence of errors across equations. This model is estimated on the same sample of observations as the single-hurdle selection model.

Results, shown in Web Appendix G, again reveal the same pattern in the difference-in-differences coefficient estimates on the interaction term (Figure G1), with the estimated coefficient for the pre-switch months once again indicating no difference in repayment levels prior to the switch. At the switch month (month zero) the coefficient becomes negative and statistically significantly different from zero. This again indicates a reduction in payments due to switching. Coefficient estimates are reported in Table G1. The coefficient on the interaction term for month zero is again of similar magnitude in the Type II Tobit model estimates (-0.78) to that in the single-hurdle selection model estimates (-0.65).

Lender-induced switching sample. As an additional robustness test, we exploit a feature of lenders' account management practices that arguably introduces a degree of exogeneity in switching for a subset of accounts. Specifically, we focus on a subsample of accounts where lenders are more likely to have played a role in setting-up minimum automatic repayments via offering refunds to customers to induce them to switch to automatic payment. Results from this analysis, which are included in Appendix E, are consistent with our main results for the effects on levels of repayment of switching to automatic minimum payment.

Excess Interest Cost Simulations

We have used Monte Carlo simulation to estimate the financial cost arising from lower repayments among card holders switching to a minimum automatic repayment (see Web Appendix I for details). We also conducted a simulation estimating what proportion of total interest and fees incurred by all cards across the entire credit card market is due to minimum automatic repayments (detailed in Web Appendix J).

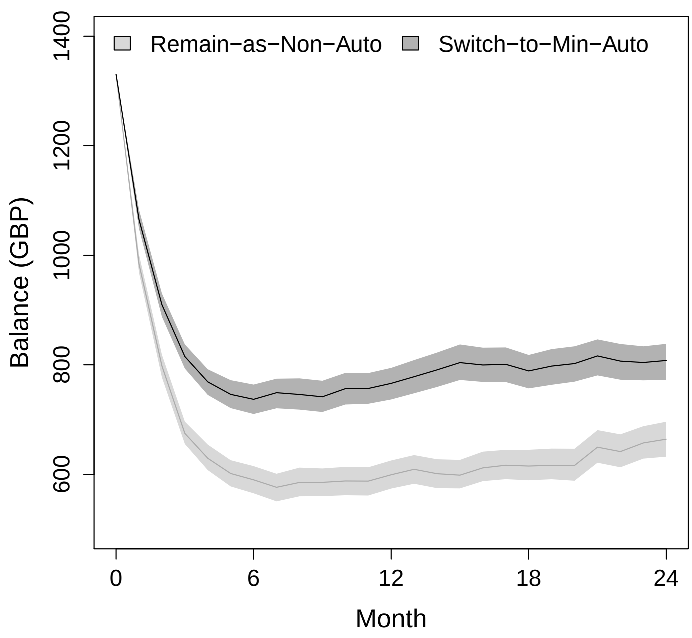
The simulation is implemented as follows. We assume two types of agents. The first type of agents never switch to automatic repayment (Remaining as Non-Auto Cards) while the second type of agents switch to a minimum automatic repayment (Switching to Min-Auto Cards). To resemble real-life use of credit cards, the simulation assumes a steady continuation of purchases and repayments.

For both types of agents, we simulate their monthly card usages and repayments, using card-month observations of cards switching to minimum automatic repayments during the data period. At each time-step (i.e., month) in the simulation, a repayment category is drawn from the actual distributions in card-months with similar card profiles. The categories include ”missed”, ”minimum” (with £10 buffer for rounding-up), and ”full”. Repayments in the actual distribution which are included in neither missed, minimum, nor full category were categorized as their own absolute value (e.g., 50.00, 100.00, 200.00). Repayments are capped at a corresponding full balance at the time-step. For Remaining as Non-Auto Cards, we use the pre-switch distribution while, for Switching to Min-Auto Cards, we use the post-switch distribution. Thus, in the simulations, Remaining as Non-Auto Cards are repaid as if card holders had not switched to a minimum automatic repayment. If an agent missed a repayment, a late payment fee was incurred. In addition, spending and cash advance amounts are also drawn from these distributions. If an agent made a cash advance or the utilization rate exceeded 1, a cash advance fee or an over-limit fee was incurred (regulated fee levels in the UK were used). The simulation continued for 24 months. We ran the simulation with the mean balance in the months where card holders switched to minimum automatic repayments (£1,330.34). Further technical details are presented in Web Appendix I.

Figure 3 shows the results where we see consistently higher balances (Panel (a)) and higher total costs (Panel (b)) in the 24-month period. Therefore, even accounting for the higher prevalence of late payment fees among Remaining as Non-Auto Cards, the simulations show that Switching to Min-Auto Cards creates higher costs of debt for the consumer.

Simulation responses (Web Appendix J) further show that cards using minimum automatic repayments at least once in the data period could save about 19.8% of interest and fees if they did not switch to minimum automatic repayment. This is about 8.4% of the all interest and fees paid in the credit card market. Even an effect ten times smaller would be economically significant.

(a) Balance



(b) Interest and Fee

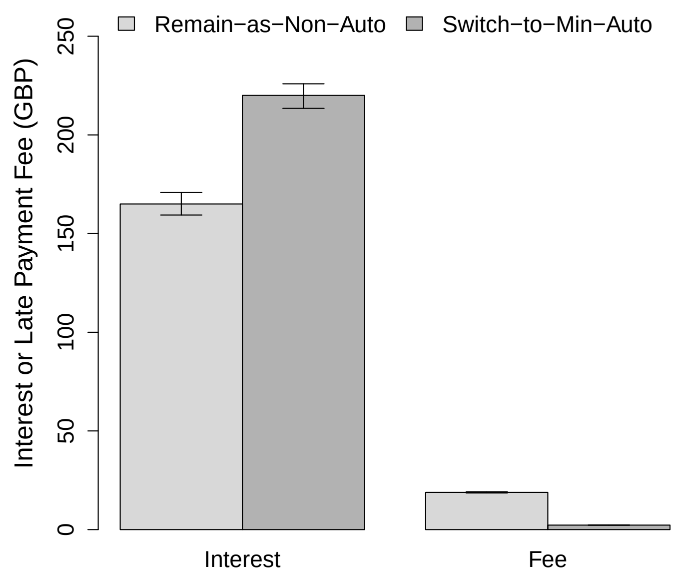


Figure 3: The results from the excess interest cost simulation. The shaded area and the error bars are 95% confidence intervals obtained by the bootstrap method (1,000 resamples) in Panels (a) and (b), respectively.

EXPERIMENTAL STUDY: ANCHORING EFFECTS OF MINIMUM PAYMENTS ON MANUAL PAYMENTS

In our second study, we focus on manual credit card repayments and use an online experiment to explore how minimum payments also affect repayment behavior among manual payers (i.e., not using automatic payments). We first replicate the finding from previous studies that people anchor upon the minimum payment information presented in the credit card payment journey, showing reduced repayments (Jiang and Dunn, 2013; Navarro-Martinez et al., 2011; Salisbury and Zhao, 2019; Stewart, 2009).

We extend the experimental design to test potential remedies to the effect of presenting minimum payments. Specifically, we test for effects of i) prompting people to pay in full before offering partial and minimum payments options and, ii) prompting those who choose to repay below or near the minimum to repay more. These interventions aim to lower the anchoring effect of minimum payments. In the first case, prompting people to pay in full before offering other options was intended to raise this option in people's minds before they chose an amount to repay. In the second case, prompting people who pay too little to pay more provided them with critical information about the time to repay at the moment of choice, without suggesting low repayments to those who spontaneously chose to repay more. We anticipated that these treatments would be effective because they were easy and timely (Service et al., 2014).

The experiment was conducted in collaboration with the UK financial regulator – the Financial Conduct Authority (FCA) – as part of their study investigating the UK credit card market. The experiment was not externally preregistered because of the nature of this. The experiment received ethical approval from the University of Warwick research ethics committee (128/15-16:DR@W). A unique property of this experimental design is that it has increased external validity, as a subsequent study demonstrates that hypothetical repayment responses are correlated with real-world repayments on linked credit card transaction data (Guttman-Kenney et al., 2018).

Method

Participants. We recruited 1,000 participants from the crowd-sourcing platform Prolific Academic, who were living and employed in the UK and spoke English as a first language. We

Table 8: Experiment design

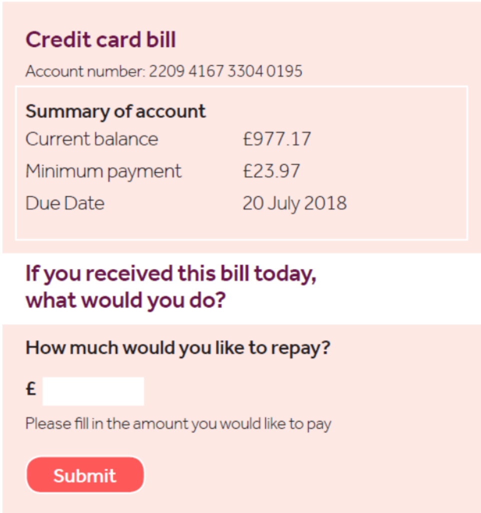
Full repayment prompt	Minimum payment amount	
	Included	Excluded
Excluded	Control: Minimum included, full repayment prompt omitted $N = 181$	Omit Minimum: Minimum excluded, full repayment prompt omitted $N = 176$
Included	Include Prompt: Minimum included, full repayment prompt given $N = 173$	Omit and Prompt: Minimum excluded, full repayment prompt given $N = 169$

decided in advance to eliminate (in a sequential process) those who did not reach the end of the experiment (7), duplicate submissions (112), submissions from the same IP (35), participants who did not answer "yes" to a question asking if they answered carefully (54), the top 5% of fastest responses (37) and a further 56 people for whom the experiment duration was not recorded. This left 699 participants.

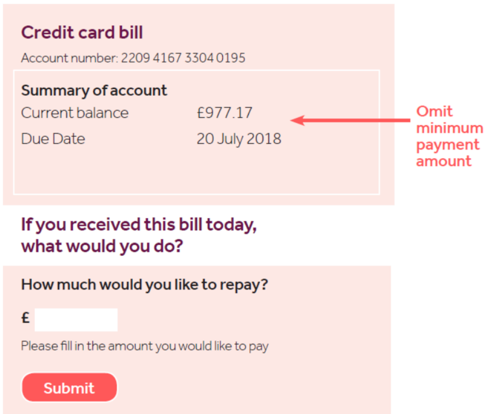
Design. Participants were randomly assigned to one of four cells in a 2×2 design, crossing omission or inclusion of a minimum payment prompt with initially prompting or not for a full repayment. While we did not have a theoretical reason for expecting an interaction, an interaction would have implications for policy design. Table 8 describes the treatments. The Control Condition included the minimum payment prompt but did not prompt for full repayment, just as in real-world credit card statements. The Omit Minimum Condition differed only in omitting minimum payment information from the bill. The Include Prompt Condition retained the minimum payment information and, additionally, initially asked participants whether they would like to pay the bill in full before offering other payment options. The Omit and Prompt Condition combined both of the previous treatments, omitting minimum payment information and initially prompting for full repayment.

Procedure. Participants were asked to imagine that they had received a credit card bill, asked to consider how much money they actually had, and decide how much of the hypothetical bill they would repay. The bill was for the 2011 UK median of £977.17, with the corresponding median minimum payment of £23.97 (Navarro-Martinez et al., 2011).

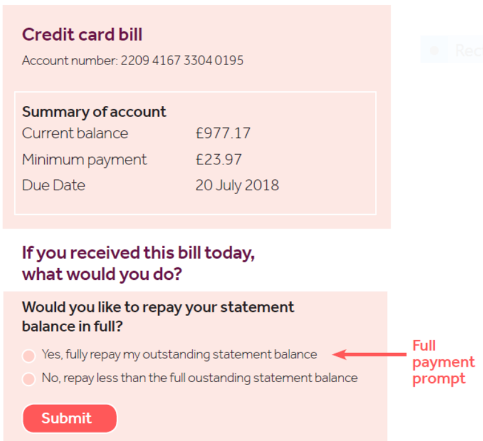
In the Control Condition – with the minimum payment amount and without an initial full repayment prompt – participants were given the bill information and typed their hypothetical repayment (see Figure 4a). In the conditions without the minimum payment amount, the minimum payment information was omitted from the bill (Figure 4b). In the conditions with an initial full repayment prompt participants were first asked whether they would like to pay in full or not (Figure 4c). If they selected "no", they typed the payment amount.



(a) Control condition statement



(b) Omit minimum



(c) Prompt for full repayment

Figure 4: The presentation of hypothetical statements in the experiment.

Afterwards, we asked participants to self-report whether they have a credit card and whether they paid their bill in full in the previous month. Balance tests in Web Appendix K shows that conditions were about equal and did not differ significantly in the fraction of participants using credit cards or paying their bill in full last month.

We also collected a secondary measure for some participants, conditional upon their initial payment choice. If a participant initially chose a repayment amount at or lower than the minimum payment, they were then shown a 'Low payment prompt' offering six options: the payment required for 1, 2, and 3 year paydown, the minimum, the amount they originally chose, and a blank box for any other amount (Figure 5). If participants chose a repayment above the minimum but below 1.5 times the minimum, they were offered five of the six options. For these participants, we omitted the minimum payment option, because they had already spontaneously chosen to pay more than the minimum and did not want to re-anchor them to this amount.

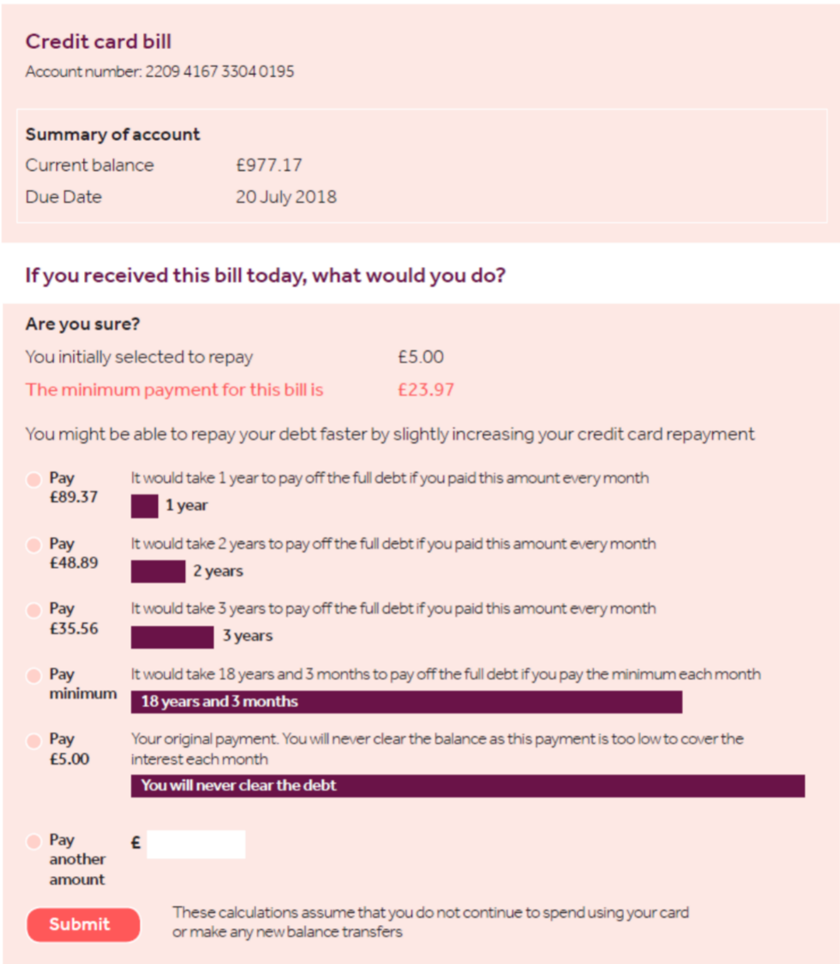


Figure 5: Low payment prompts shown to those selecting to pay below 1.5 times the minimum in the experiment.

Measures. Our primary dependent variable was the amount that participants selected as their repayment for the bill. We also had, as secondary dependent variable, a (potentially) revised repayment amount after participants making low payments were prompted to make higher repayments. We describe these in more detail below, along with the independent variables and covariates.

Repayment Amount. The dependent variable was the amount that participants selected as their repayment for the bill, measured in £. In conditions with a full repayment prompt, participants could select a radio button to make a full repayment, which was recorded as £977.17. In conditions without a full repayment prompt, or after selecting "no, repay less than the full outstanding statement balance" in conditions with a full repayment prompt, participants typed a repayment in £ in a text box.

Revised Repayment Amount. As described above, if participants made a low repayment they were offered a series of prompts to make a higher repayment. The measure was also in £, with radio button responses coded as their £ amount. This secondary dependent variable was only measured for those who chose an initial repayment below 1.5 times the minimum (i.e., below £35.96).

Independent Variables. As described above, we had two independent variables: (a) whether minimum payment information was included or omitted from the credit card statement and (b) whether participants were asked if they would like to pay in full before, if they declined, being able to give an open response of any repayment amount.

Measures for Data Quality. We collected three variables to screen for data quality (e.g., Buchanan and Scofield 2018). We asked participants if they chose a repayment option carefully and thoughtfully, with response options of "Yes, I took care" or "No, I just want the payment". We had decided in advance to exclude anyone answering "No". We also recorded the duration of the experiment, having decided in advance to exlude the 5% of people who were fastest. We recorded the IP address and Prolific Academic ID to delete, as planned, participants making duplicate submissions.

Data and code for the analysis will be posted online on publication.

Results

Table 9: Experiment repayments means [95 % confidence intervals]

	Minimum payment amount	
Full repayment prompt	Included	Excluded
Excluded	£376 [£316–£436]	£500 [£439–£561]
Included	£520 [£458–£581]	£569 [£507–£632]

Note. Mean repayments [95% confidence intervals].

Our primary analysis compares repayments with and without minimum payment information and with and without prompting for full repayment. Table 9 reports the mean repayments. As hypothesised, repayments were higher when minimum payment information was omitted and repayments were higher when participants were first prompted to pay in full.

To quantify the above description, we analyzed repayments with a linear regression of repayment amount on a minimum payment information dummy, a full repayment prompt dummy, and their interaction. Because the interaction was not significant and we had no theoretical reason to expect one, we report here the regression without an interaction. The coefficient for the minimum payment information dummy estimates the increase in repayments when minimum payment information was omitted. The coefficient for the full repayment prompt dummy estimates the increase in repayments when full repayment is prompted.

Model 1 in Table 10 shows the coefficients. Averaging over full repayment prompting, omitting minimum payment information raised the mean repayment from £447, 95% CI [£404, £490] to £535, 95% CI [£491, £579]. This was a significant increase of £88, 95% CI [£26, £149], $t(696) = 2.79, p = .005$. Averging over minimum payment inclusion, repayments were also higher when the full repayment prompt is included. Including a full payment prompt raised the mean repayment from £438, 95% CI [£395, £481] to £545, 95% CI [£501, £589]. This is a significant increase of £107, 95% CI [£45, £169], $t(694) = 3.40, p < .001$.

Table 10: Repayment Regression Estimates Including Controls for Card Usage

	Dependent variable:				
	Repayment in £				
	(1)	(2)	(3)	(4)	(5)
Full Repayment Prompted	106.910***	103.676***	176.690**	77.041**	98.752**
	(45.553 – 168.267)	(42.724 – 164.629)	(60.892 – 292.487)	(26.890 – 127.191)	(31.508 – 165.995)
Minimum Payment Omitted	87.783*	83.343*	173.814**	72.653**	88.837*
	(26.436 – 149.131)	(22.373 – 144.313)	(57.924 – 289.704)	(22.583 – 122.724)	(21.736 – 155.937)
Card User		115.011***	219.800***		
		(46.903 – 183.119)	(105.209 – 334.391)		
Full Repayment Prompted x Card User			−98.691		
			(−234.792 – 37.410)		
Minimum Payment Omitted x Card User			−123.214		
			(−259.398 – 12.969)		
Full Repayer				482.671***	523.955***
				(432.167 – 533.175)	(437.072 – 610.838)
Full Repayment Prompted x Full Repayer					−47.997
					(−149.040 – 53.046)
Minimum Payment Omitted x Full Repayer					−35.198
					(−136.094 – 65.697)
Constant	393.907***	314.590***	239.213***	202.619***	184.839***
	(341.403 – 446.411)	(244.418 – 384.761)	(142.719 – 335.706)	(155.342 – 249.896)	(128.546 – 241.132)
Observations	699	699	699	699	699
R^2	0.027	0.042	0.049	0.354	0.355
Adjusted R^2	0.025	0.038	0.042	0.351	0.350
Residual Std. Error	413.735 (df = 696)	410.808 (df = 695)	409.918 (df = 693)	337.511 (df = 695)	337.671 (df = 693)
F Statistic	9.775*** (df = 2; 696)	10.261*** (df = 3; 695)	7.188*** (df = 5; 693)	126.750*** (df = 3; 695)	76.246*** (df = 5; 693)

Note: Intervals are 95% CIs. $p < 0.05$; * $p < 0.01$; ** $p < 0.005$; *** $p < 0.001$

One possibility is that the behavior of participants in our online experiment in part reflects their own financial situation. For example, some participants might select minimum payments as this is their usual repayment behavior on their own credit card, for example due to facing low income or liquidity constraints in their finances. At the suggestion of a reviewer, we draw upon the self-reported information provided by participants on their real-world financial position to control for this possible confound (note also that, as reported in Appendix K, conditions were balanced on these variables). Models 2 and 3 in Table 10 include a card-user dummy indicating whether the participant has a real credit card. The dummy is included as a covariate in Model 2 and additionally interacted with the experimental manipulations in Model 3. The pattern of results for the experimental manipulations is unchanged. Holding a card is associated with higher repayments. The null interaction show no evidence that the experimental manipulations are affected by card holding. Models 4 and 5 repeat the exercise with a full-repayment dummy indicating whether the participant normally repays their bill in full. Again the pattern of results is unchanged for the experimental manipulations. A history of full repayment is associated with higher repayments but does not interact with the experimental manipulations.

Because the distribution of repayments departs from Gaussian, we have included supplementary analyses. In Appendix K we show, consistent with the analysis of the means above, that the distribtion of repayments when the minimum is omitted first order stochastically dominates the distribution when the minimum is included. Further, the distribution of repayments when full repayment is prompted first order stochastically dominates the distribution when full repayment is not prompted. We also analyse the probability of making minimum repayments, which is reduced when the minimum is omitted and full repayment is prompted, and the probability of making full repayments, which is increased when the minimum is omitted and full repayment is prompted – again consistent with the analysis of the means above.

Low payment prompts. Figure 6 shows how the 106 people paying at or below 1.5 times the minimum (here £35.95) revised their payments after the low payment prompts. 28 kept their original repayment, no one decreased their repayment, and 78 people revised their repayment upwards – typically to be one of the prompted payment amounts. For some people, the increase was substantial - for example, moving from initially selecting to pay only the minimum to then selecting to pay an amount that would amortize debt in a year ('One-Year Paydown') is more than a tripling of payment amounts. Participants were 2.53, 95% CI [1.68, 3.92] times more likely to increase their payment than leave it unchanged.

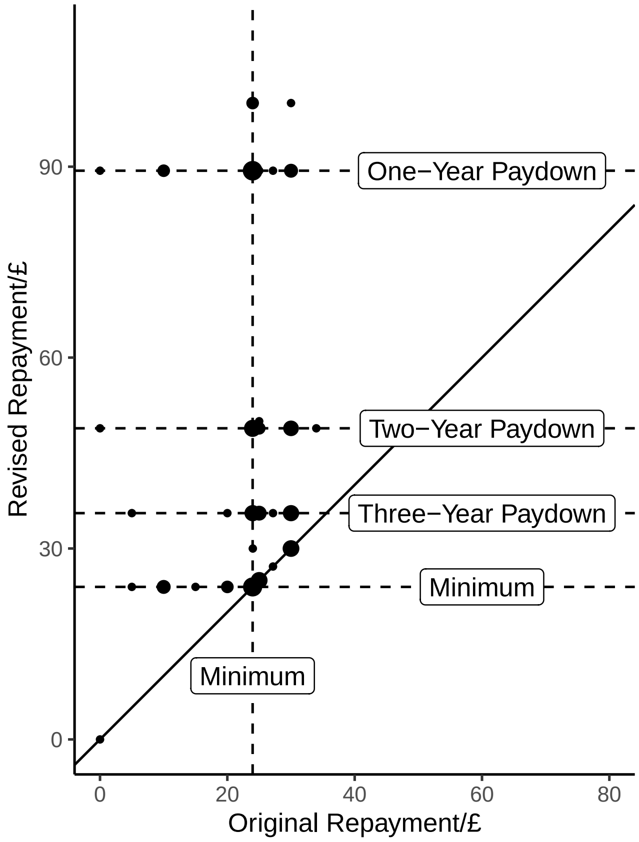


Figure 6: Revised payments for those prompted to pay more in the experiment. Dot area is proportional to the number of participants. Points above the diagonal are participants revising their payment upwards. Points on the diagonal are participants leaving their repayment unchanged.

Experiment Discussion

Omitting minimum payment information increased repayments, replicating findings from Jiang and Dunn (2013), Navarro-Martinez et al. (2011), Salisbury and Zhao (2019), and Stewart (2009). We also tested two new interventions. An initial prompt to pay in full also increased repayments. Prompting people who initially selected to pay at or near the minimum to repay more, by providing scenarios about the payments needed to clear the debt in 1, 2, or 3 years, also increased repayments for the majority prompted. We think this prompt was successful because (a) it provided scenario information at exactly the right time, unlike earlier interventions which found only small effects local to the scenario repayment amount (Agarwal et al., 2015; Hershfield and Roese, 2015; Keys and Wang, 2019; Navarro-Martinez et al., 2011) and (b) prompting people to make a second choice is a Gricean indication that their first choice was poor (Rose and Blank, 1974) - a finding in line with a recent meta-analysis that concluded that defaults are more effective when they are seen to communicate what a choice architect thinks a decision-maker should do (Jachimowicz et al., 2019).

GENERAL DISCUSSION

Defaults exist in many areas of individual choice, and are commonly used by policymakers as a device to achieve behavioral change. However, there is concern that defaults may have unintended consequences and that the positive effect of introducing a default on one outcome may co-occur with negative effects on other outcomes.

Our analysis of minimum payments indeed shows the unintended effects of defaults once multiple outcomes are examined. Our analysis shows that setting an automatic repayment has the mixed effect of nearly eliminating the likelihood of missed repayments, but it also decreases average payments. Why does this occur? One interpretation is that the minimum payment being paid automatically reduces attention to balances. Consumers choosing minimum automatic repayments are selecting a potentially powerful psychological default, one which facilitates inattention. This results in repeated minimum repayments, which greatly increases the debt revolved from month to month and thus the interest paid.

This unintended effect of the default raises the need for potential mitigations. Our experimental study explored the efficacy of interventions. We first replicate the now robust result that merely providing minimum payment information reduces repayment. We have also shown that providing alternative information—namely a prompt for full repayment or to reconsider low repayments with explicit higher repayment suggested—mitigates the effect of anchoring on the minimum repayment. While other attempts have failed, we believe ours succeeded because the information was presented just in time, exactly when it was required. Whether consumers are opting in to their own minimum payment default by setting an automatic repayment, or are anchoring their free choice manual repayments upon the minimum, minimum payments are having detrimental effects.

We have focused here upon credit card repayments where consumers face an explicit choice about the size of their automatic repayment, and can easily make additional manual repayments. But the power of setting a default repayment is applicable more widely. For example, choosing the term of a mortgage or choosing fixed monthly repayments for a personal loan also sets powerful defaults, and these defaults are administratively harder to change, and changes can even incur additional costs. If people are initially conservative in their choice of level, so they can be sure they can meet their monthly repayments, our findings suggest that they are unlikely to get round to making additional repayments over and above the default even if they can afford to make additional repayments to save interest costs.

A limitation of our study is that we cannot provide direct evidence that attention is the mechanism underlying consumer behavior, and therefore the theoretical contribution of our study is limited. Hence, looking to future research, we suggest approaches which could help understand better the underlying theoretical mechanism, potentially leading to policy solutions. It may be that the unintended effect of minimum automatic repayment could be partially addressed through interventions which bring the repayment decision back to the top of the consumer's mind, drawing attention to the repayment decision and shrouding the minimum payment. More generally, what should policymakers and industry do to avoid introducing defaults with unintended effects? We have two suggestions. The first is to consider the status quo effects resulting from the default itself—is the new status quo unambiguously in the consumer's interest? The second is to assess the effect of the defaults across as broad a range of outcome behaviors as are available, and to follow up on these assessments over time. This approach will help policymakers to fully evaluate the impact of defaults and consider whether mitigations are necessary in the presence of unintended consequences.

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Supplementary Web Appendices

Appendix A Characteristics of Switchers

Table A1 compares socio-economic statistics across card holders switching to different types of automatic repayment. To do so, we retrieved census records from the UK National Census for 2011 which includes detailed household information. (5% sample of raw data are available for researchers via the UK Office for National Statistics.) In our field data, card holders across 2,994 different postcode districts while the census statistical unit is smaller, 8,436 Middle-super output areas (MSOA). We weighted averaged the socio-economic variables across MSOAs within each postcode districts, and matched with postcode districts where card holders locate.

Table A1: Comparison in Socio-Ecnomic Variables Among Cards Switching to Different Autopay Types

	Switch to Min-Auto	Switch to Fixed-Auto	Switch to Full-Auto
Mean house price (GBP)	219,879	201,129	235,457
	(—)	(0.000)	(0.000)
Jobless claimants (%)	2.45	2.64	2.23
	(—)	(0.000)	(0.000)
Mean weekly income (GBP)	763.46	734.18	786.81
	(—)	(0.000)	(0.000)
Education level 4+ (%)	29.50	27.90	30.74
	(—)	(0.000)	(0.000)
Mean Acorn category (1-6)	3.16	3.25	3.05
	(—)	(0.000)	(0.000)
Free-school meal (%)	12.25	13.27	11.27
	(—)	(0.000)	(0.000)

Note. The numbers in the parentheses are p-value taken from the t-test with Switch to Min-Auto cards. Acorn category is a postcode-level affluence score constructed by the UK statistics authority. The lower the Acorn category the higher the affluence.

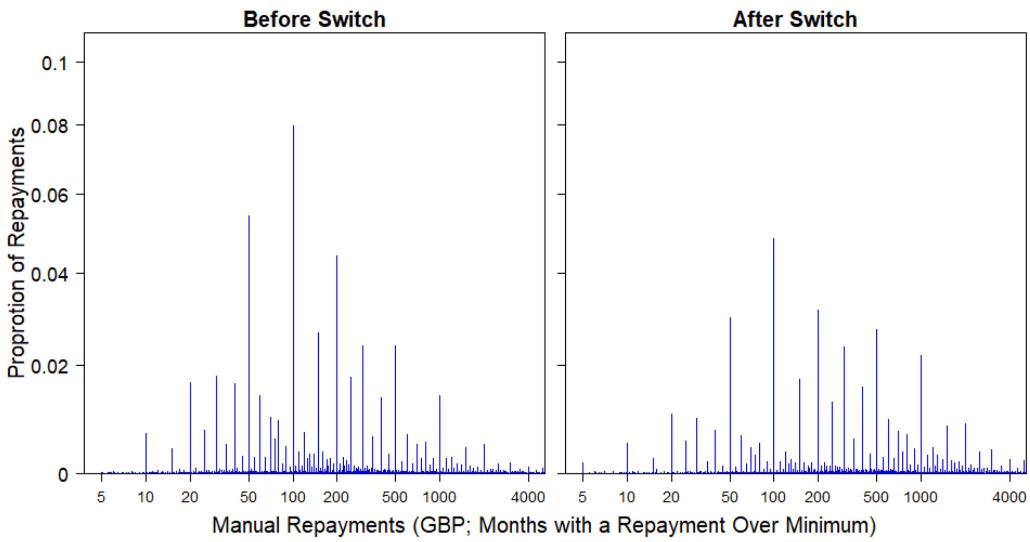


Figure A1: The distribution of repayments in the months where a manual repayment was made. Each bar is a 1-penny-wide bin.

Seeing repayments cluster at prominent numbers is common (Albers, 1997) and, more generally, such a tendency for people to prefer prominent numbers has been evident in both experimental (Whynes et al., 2005) and field data (Ball et al., 1985; Christie and Schultz, 1994; Converse and Dennis, 2018; Harris, 1991; Kandel et al., 2001). However, after switching to automatic payments, the proportion covered by these top four payments is almost eliminated, dropping to only 2.5% (Web Appendix B confirms this pattern in a multinomial logistic regression, which controls for balance, credit limit, how much of the credit limit is utilized, and credit score).

Interestingly, in the rare months in which individuals make additional manual repayments in the range between the minimum and full, the distribution of repayments remains dominated by prominent numbers (see Figure A1 in Web Appendix A). This suggests that, because of inattention, people do not have to take a manual repayment decision, and thus cannot be attracted by psychologically prominent numbers as repayments. Yet when they do pay attention in the post-switch months where there is a manual repayment, people are just as attracted to prominent number repayments as pre-switch. Web Appendix C shows conditional upon a manual repayment being made, the probability of a full repayment is larger after switch than before switch (using a logistic regression with card profile controls), providing further evidence that people's switching to a minimum automatic repayment is unlikely to be due to their financial inability to repay the bill.

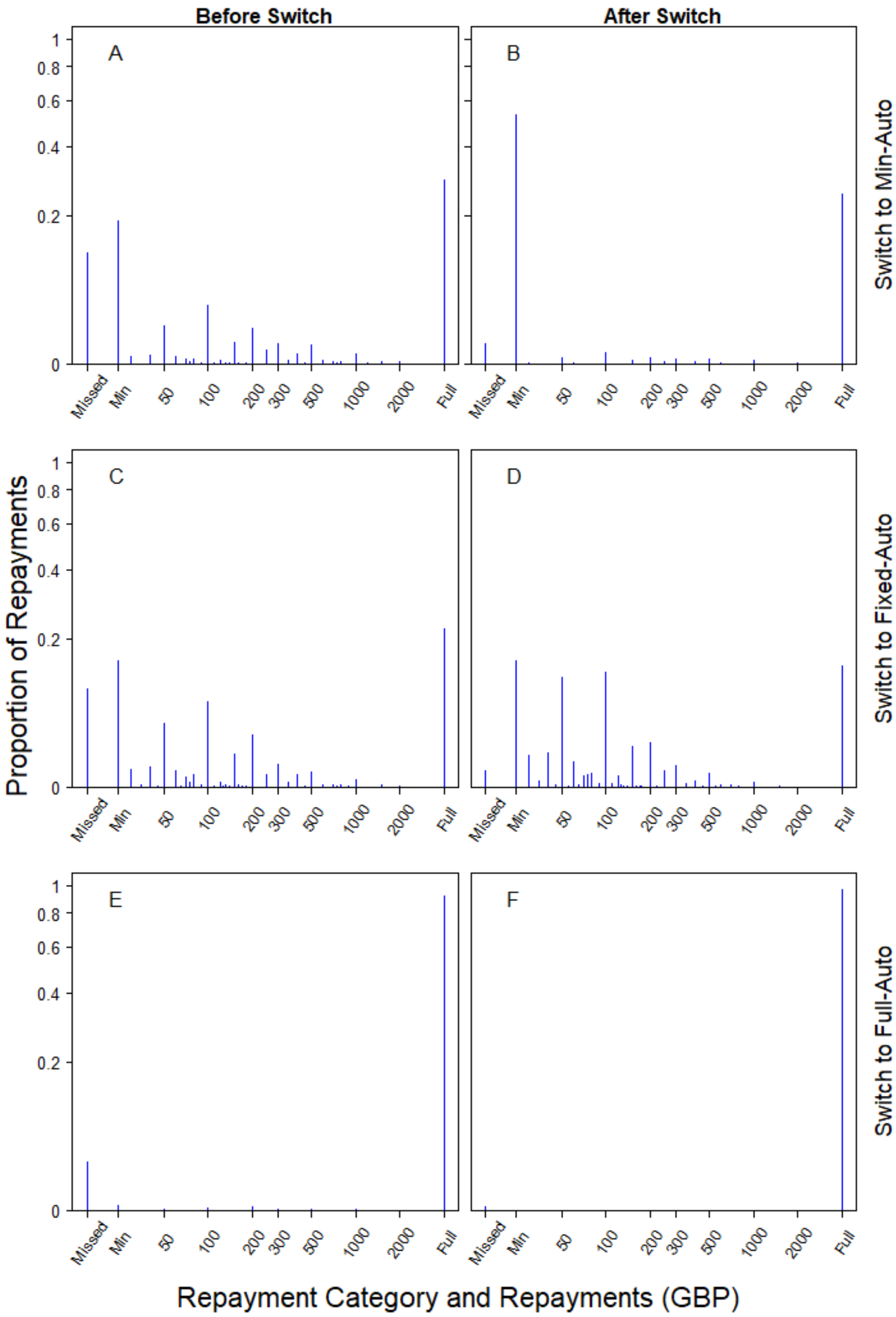


Figure A2: Histograms of the change in monthly repayments before and after cards switch to a minimum automatic repayment ("Min-Auto"), fixed automatic repayment ("Fixed-Auto"), and full automatic repayment ("Full-Auto"). The unit of observation is a card-month (total repayments to a particular card in a particular month). For the absolute amounts, each bar is a 1-penny-wide bin. The sample consists of monthly repayments made between an account opening date or the beginning of the data period (Jan. 2013) whichever is latter and the end of the data period (Nov. 2014). The sample was restricted to cards switching to automatic repayments during the data period. The number of months contributing to the figure differs among cards while the number of months included in before and after periods differs within a card. The card-months with no balance were excluded.

Appendix B Multinomial Logit Model of the Effect of Switching to a Minimum Automatic Repayment

To confirm the findings in the top panels of Figure A2 (i.e., change in repayments before and after switching to a minimum automatic repayment), we fitted a multinomial logit model of repayments to control for card characteristics (Equation B1). The model estimates the probability that card holder i's repayment at time t falls into each of seven categories: $Missed$, $Minimum$, $Larger$ 1, $Larger$ 2, $Larger$ 3, $Larger$ 4, and $Full$. We use this categorical classification because "missed", "minimum", and "full" are discrete repayment types distinguishable from other monetary amounts. $Missed$ includes repayments less than the required minimums. $Minimum$ includes repayments which are equal to or greater than the required minimum and less than the required minimum plus £10. This £10 allowance is for including repayments slightly larger than the minimum, which were possibly caused by rounding up of the required minimum. $Larger$ 1 includes repayments which are not included in $Missed$ and $Minimum$ and are less than 25% of the balance. $Larger$ 2 includes repayments equal to or more than 25% of the balance and less than 50% of the balance. $Larger$ 3 includes repayments equal to or more than 50% of the balance and less than 75% of the balance. $Larger$ 4 includes repayments equal to or more than 75% of the balance and less than the full balance. $Full$ includes repayments equal to or more than the full balance. If a repayment was equal to the required minimum which was also equal to the full balance, the repayment was included in $Full$. We included $Balance$, $Credit\ Limit$, $Utilization$ (how much of the credit limit is utilized), and $Charge\text{-}off\ Rate$ (a monotonic transform of credit score). The independent variable of interest is $Post\text{-}Switch$ which is a dichotomous variable having a value of 1 if a card had started using a minimum automatic repayment, otherwise having a value of 0. We make the standard assumption that the observation of repayment category k follows the Bernoulli distribution with mean $P(Repayment\ Category_{i,t+1} = Category\ k)$.

$$\begin{aligned}\log\left(\frac{P(Repayment\ Category_{i,t+1} = Category\ k)}{P(Repayment\ Category_{i,t+1} = Missed)}\right) =& \beta_0 + \beta_1 Balance_{i,t,k} + \beta_2 Credit\ Limit_{i,t,k} + \beta_3 Utilization_{i,t,k} + \\ & \beta_4 Spending\ Amount_{i,t,k} + \beta_5 Merchant\ APR_{i,t,k} + \beta_6 Cash\ APR_{i,t,k} + \\ & \beta_7 Charge\text{-}off\ Rate_{i,t,k} + \beta_8 Post\text{-}Switch_{i,t,k}\end{aligned} \tag{B1}$$

Figure B1 shows the results. Table B1 reports the coefficients. Consistent with the finding in the top panels of Figure A2, after setting a minimum automatic repayment the likelihood of paying only the minimum within the month increases sharply from .204, 95% CI [.188, .221] to .591, 95% CI [.578, .603]. The likelihood of other levels of repayment decreases: the likelihood of missing the minimum payment decreases sharply from .112, 95% CI [.105, .119] to .016, 95% CI [.014, .018], while the likelihood of repayment in Larger 1 category decreased from .276, 95% CI [.265, .287] to .108, 95% CI [.102, .113] and the likelihood of paying the full balance halves from .243, 95% CI [.228, .258] to .178, 95% CI [.167, .190]. Thus the results of the multinomial logit regression shown in Figure B1 confirms the pattern in the simple histogram in the top panels of Figure A2: after switching to a minimum automatic repayment card holders are more likely to repay the minimum and are less likely to miss repayments, but are also less likely to make larger repayments.

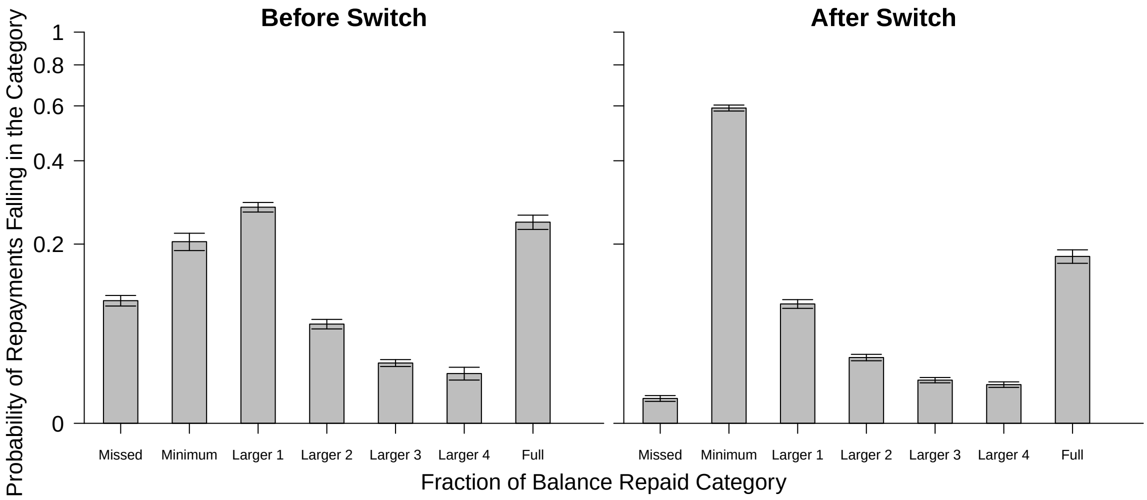


Figure B1: The fraction of balance repaid each month before and after cards switch to a minimum automatic repayment. Repayment category probabilities are from a multinomial logit model. Error bars are 95% confidence intervals. The standard errors were corrected by clustering by cards and months.

Table B1: Coefficients for Equation B1

	Estimate	LL	UL	Clustered SE	z-value	p-value	
Intercept:Minimum	1.428	0.696	2.159	0.373	3.823	0.00013	***
Intercept:Large1	2.336	1.618	3.054	0.366	6.378	0.00000	***
Intercept:Large2	2.210	1.474	2.947	0.376	5.880	0.00000	***
Intercept:Large3	1.859	1.105	2.612	0.385	4.833	0.00000	***
Intercept:Large4	1.797	0.991	2.603	0.411	4.370	0.00001	***
Intercept:Full	5.118	4.397	5.839	0.368	13.919	0.00000	***
Balance:Minimum	0.000	0.000	0.000	0.000	-1.819	0.06889	
Balance:Large1	0.000	0.000	0.000	0.000	-0.694	0.48788	
Balance:Large2	0.000	0.000	0.000	0.000	-5.394	0.00000	***
Balance:Large3	0.000	0.000	0.000	0.000	-5.837	0.00000	***
Balance:Large4	0.000	0.000	0.000	0.000	-6.141	0.00000	***
Balance:Full	-0.001	-0.001	-0.001	0.000	-18.351	0.00000	***
Credit Limit:Minimum	0.000	0.000	0.000	0.000	5.936	0.00000	***
Credit Limit:Large1	0.000	0.000	0.000	0.000	6.595	0.00000	***
Credit Limit:Large2	0.000	0.000	0.000	0.000	3.765	0.00017	***
Credit Limit:Large3	0.000	0.000	0.000	0.000	3.678	0.00023	***
Credit Limit:Large4	0.000	0.000	0.000	0.000	3.753	0.00017	***
Credit Limit:Full	0.000	0.000	0.000	0.000	1.581	0.11396	
Utilisation:Minimum	0.937	0.713	1.161	0.114	8.202	0.00000	***
Utilisation:Large1	1.072	0.848	1.296	0.114	9.374	0.00000	***
Utilisation:Large2	-0.339	-0.627	-0.050	0.147	-2.302	0.02134	*
Utilisation:Large3	-0.562	-0.914	-0.211	0.179	-3.137	0.00171	**
Utilisation:Large4	-0.821	-1.170	-0.473	0.178	-4.621	0.00000	***
Utilisation:Full	-2.133	-2.408	-1.859	0.140	-15.224	0.00000	***
Spending Amount:Minimum	0.000	0.000	0.000	0.000	-5.642	0.00000	***
Spending Amount:Large1	0.000	0.000	0.000	0.000	3.404	0.00067	***
Spending Amount:Large2	0.001	0.001	0.001	0.000	13.994	0.00000	***
Spending Amount:Large3	0.001	0.001	0.001	0.000	15.872	0.00000	***
Spending Amount:Large4	0.001	0.001	0.001	0.000	15.284	0.00000	***
Spending Amount:Full	0.001	0.001	0.002	0.000	24.037	0.00000	***
Merchant APR:Minimum	1.437	0.074	2.801	0.696	2.066	0.03884	*
Merchant APR:Large1	0.818	-0.491	2.127	0.668	1.225	0.22060	
Merchant APR:Large2	1.163	-0.270	2.595	0.731	1.591	0.11159	
Merchant APR:Large3	0.302	-1.297	1.901	0.816	0.370	0.71150	
Merchant APR:Large4	-0.084	-1.894	1.726	0.923	-0.091	0.92757	
Merchant APR:Full	-3.381	-4.748	-2.014	0.698	-4.848	0.00000	***
Cash APR:Minimum	-6.251	-8.935	-3.567	1.370	-4.564	0.00001	***
Cash APR:Large1	-8.744	-11.434	-6.054	1.372	-6.371	0.00000	***
Cash APR:Large2	-10.501	-13.251	-7.751	1.403	-7.484	0.00000	***
Cash APR:Large3	-10.446	-13.325	-7.567	1.469	-7.112	0.00000	***
Cash APR:Large4	-10.741	-13.636	-7.847	1.477	-7.273	0.00000	***
Cash APR:Full	-9.331	-11.957	-6.705	1.340	-6.965	0.00000	***
Charged-off Rate:Minimum	-7.368	-8.863	-5.874	0.763	-9.661	0.00000	***
Charged-off Rate:Large1	-10.884	-12.546	-9.221	0.848	-12.833	0.00000	***
Charged-off Rate:Large2	-19.373	-24.033	-14.714	2.377	-8.149	0.00000	***
Charged-off Rate:Large3	-22.373	-29.545	-15.201	3.659	-6.114	0.00000	***
Charged-off Rate:Large4	-15.499	-20.838	-10.161	2.724	-5.690	0.00000	***
Charged-off Rate:Full	-32.797	-39.086	-26.507	3.209	-10.220	0.00000	***
Post-Switch:Minimum	2.980	2.830	3.130	0.077	38.943	0.00000	***
Post-Switch:Large1	0.979	0.831	1.127	0.076	12.952	0.00000	***
Post-Switch:Large2	1.397	1.213	1.581	0.094	14.875	0.00000	***
Post-Switch:Large3	1.529	1.339	1.719	0.097	15.786	0.00000	***
Post-Switch:Large4	1.631	1.376	1.887	0.130	12.528	0.00000	***
Post-Switch:Full	1.607	1.437	1.778	0.087	18.452	0.00000	***
Log-Likelihood	-223,613.80						
Num. of Observations	186,661						

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. The standard errors were corrected by clustering by card and month.

Appendix C Logit Model of Manual Repayments After Switching to a Minimum Automatic Repayment

We examine how the probability of card holders repaying in full changes in months with a manual repayment (i.e., months in which card holders paid attention to a repayment) before and after switching to a minimum automatic repayment. We conducted a logistic regression (Equation C1). The dependent variable is a dichotomous indicator variable taking the value of 1 if the card i was repaid in full at time t (i.e., fraction equal to or greater than 1) and 0 otherwise. $Balance$, $Credit\ Limit$, $Utilization$, $Spending\ Amount$, $Merchant\ APR$, $Cash\ APR$, and $Charge\text{-}off\ Rate$ were included as continuous control variables. The independent variable of interest is $Post\text{-}Switch$ which is a dichotomous variable having a value of 1 if a card had started using a minimum automatic repayment, otherwise having a value of 0. The data were restricted to repayments above the minimum (i.e., card months with manual repayments above the minimum before switch and card months with an additional manual repayment over and above the automatic repayment of the minimum after switch).

$$\begin{aligned}\log\left(\frac{P(Full\ Repayment_{i,t+1})}{1-P(Full\ Repayment_{i,t+1})}\right) =& \beta_0 + \beta_1 Balance_{i,t} + \beta_2 Credit\ Limit_{i,t} + \beta_3 Utilization_{i,t} + \\ & \beta_4 Spending\ Amount_{i,t} + \beta_5 Merchant\ APR_{i,t} + \beta_6 Cash\ APR_{i,t} + \\ & \beta_7 Charge\text{-}off\ Rate_{i,t} + \beta_8 Post\text{-}Switch_{i,t}\end{aligned} \tag{C1}$$

Figure C1 shows the model prediction for the probability of a full repayment, conditional that a manual repayment is made. The results show that, when card holders make a manual repayment over a minimum automatic repayment, they are more likely to repay in full than before they switch, indicating that the absence of prominent number repayments after switch is unlikely to be due to financial difficulty and is more consistent with the inattention account.

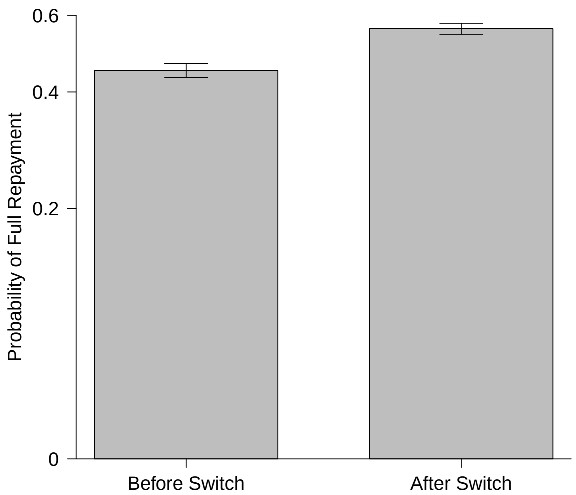


Figure C1: Using only repayments above the minimum, the probability of a full repayment before and after cards switch to a minimum automatic repayment. The probabilities are from a logistic model. Error bars are 95% confidence intervals. The standard errors were corrected by clustering by card and month.

Table C1: Coefficients for Equation C1

	Estimate	LL	UL	Clustered SE	z-value	p-value	
Intercept	1.689	1.443	1.934	0.125	13.495	0.00000	***
Balance	-0.001	-0.001	0.000	0.000	-13.693	0.00000	***
Credit Limit	0.000	0.000	0.000	0.000	-6.006	0.00000	***
Utilization	-2.917	-3.120	-2.714	0.103	-28.189	0.00000	***
Spending Amount	0.001	0.001	0.001	0.000	24.144	0.00000	***
Merchant APR	-4.183	-5.244	-3.123	0.541	-7.731	0.00000	***
Cash APR	-0.422	-1.447	0.602	0.523	-0.808	0.41896	
Charge-off Rate	-6.636	-8.773	-4.499	1.090	-6.087	0.00000	***
Post-Switch	0.444	0.388	0.501	0.029	15.447	0.00000	***
Log-Likelihood	-51,205.73						
Num. of Observations	103,668						

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. The standard errors were corrected by clustering by card and month.

Appendix D   Supplemental Materials for Matching Analysis

Table D1: Matching Validation on Socio-Economic Variables

	Switcher	Synthetic Control	p-value
Mean house price (GBP)	216,829	221,613	0.866
Jobless claimants (%)	2.44	2.39	0.879
Mean weekly income (GBP)	758.89	784.41	0.629
Education level 4+ (%)	29.11	29.77	0.820
Mean Acorn category	3.17	3.02	0.289
Free-school meal (%)	12.25	10.29	0.130

Note. p-values were taken from t-test.

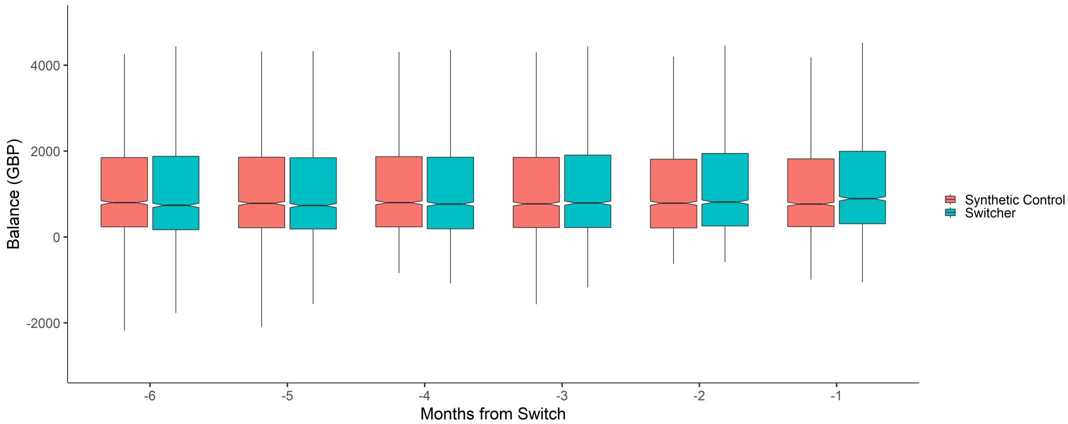


Figure D1: Notched box-plot on median balances before (synthetic) switch. The notches extends to approximately 95% confidence intervals around the median. The boxes show interquartile ranges.

Appendix E Supplemental Materials for Lender-Induced Switching Sample

The difference-in-differences estimates based upon the synthetic control group suggest that the reduction in repayments after switching to minimum automatic repayments is unlikely to be due to an intention of the part of card holders to pay only the minimum (or close to only the minimum) in future months. While we cannot test this assumption further, for example through a randomized-control trial of minimum automatic repayments in which card holders are exogenously assigned to switching, a feature of lenders' account management practices introduces a degree of exogeneity in switching for a subset of accounts.

Specifically, we focus on a subsample of accounts where lenders are more likely to have played a role in setting-up minimum automatic repayments. In the data, many card holders received refunds of missed payment fees in return for switching to minimum automatic repayments. This occurs when card holders contact their credit card company about the fee (often to raise a complaint) and are induced by their card provider to set up the automatic repayment to avoid forgetting a repayment in future, in return for a fee refund. Consequently, some of the refunded cards switched to the automatic repayment while others did not. Those switchers were likely to set up the automatic repayment due to an arguably exogenous inducement by card providers rather than ongoing intention to repay only the minimum.

Adopting this, we extracted cards setting up a minimum automatic repayment after the refund of a late payment fee (within two months after a refund) and matched them to a synthetic control group. In the matching, each switcher is matched to a card which received the refund in the same calendar month but did not switch. In addition, the matching balances covariate values between refund-triggered switchers and the control cards. Using this matched sample, we replicated our main analysis including the single-hurdle difference-in-differences model. As we restrict to cards receiving a refund only, the sample in this analysis is much smaller (6,870 observations, 9.4% of observations from the single-hurdle selection model sample).

Results are shown in Web Appendix H. Table H1 indicates that the pre-switch profile of card holders in the switcher and synthetic control group are closely matched. In addition, the pre-switch repayment distributions are similar between the switchers and the synthetic control group (panels A and C of Figure H1). A plot of the pre- and post-switch distributions of repayments for the switchers (panels A and B of Figure H1) resembles that seen in panels A and B of Figure A2 in the illustrative results (i.e., the sharp increase of the proportion of minimum repayments after switch, eliminating payments at prominent values). On the other hand the pre- and post-switch distributions are similar to the synthetic control group (panels C and B of Figure H1).

A regression with the single-hurdle selection model was conducted on the refund-triggered synthetic matched sample. Figure E1 shows the model predictions for the probability of forgetting a repayment with mean covariate values. The probability of forgetting a repayment jumps at two months before (synthetic) switch for both groups because we matched switchers with non-switchers receiving a refund of the late payment fee in the same calendar month. After (synthetic) switch, the probability is nearly zero for switchers while the synthetic control group keeps 3-4% level even after synthetic switch.

The coefficient estimates from the regression show that, similar to the results of the main regression, the $Switcher \times Months\ from\ Switch$ interaction terms indicate that the switch reduces repayments of refund-triggered switchers but does not impact repayments of the synthetic control group (see Table H2 and Figure H2). This suggests that the effect of a minimum automatic repayment is unlikely to be due to card holders' intentions to make small repayments in future.

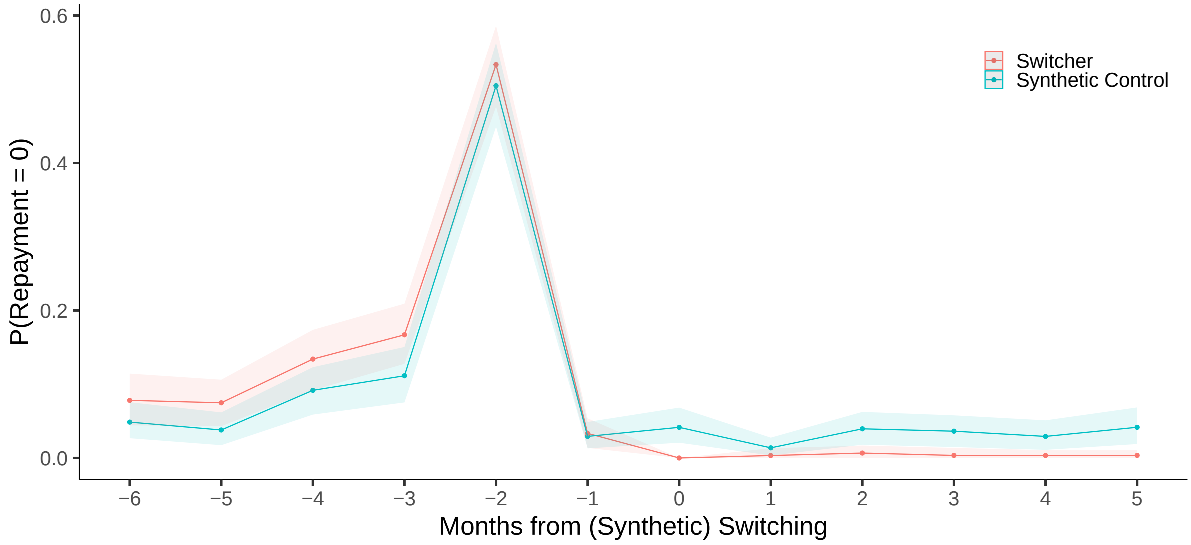


Figure E1: Predictions for the probability of forgetting a repayment from the single-hurdle selection model (refund-triggered cards). The shadowed areas represent 95% confidence intervals obtained by repeating the regression with bootstrapped samples. The iteration is continued until 1,000 regressions successfully converged. Matched pairs were resampled in the bootstrap.

Appendix F Conditional Estimation

We conducted an OLS with Equation F1 on the sample consisting of positive repayments (i.e., conditional on not-missing the repayment).

$$\log(Repayment_{i,t}) = \beta_0 + \beta_1 Switcher_i + \beta_2^T D_{i,t} + \beta_3^T Swither_i \times D_{i,t} + \beta_4^T x_{i,t} + \epsilon_{i,t} \quad \text{(F1)}$$

where $Switcher_i$ is a dichotomous variable having a value of 1 if the card i switches to a minimum automatic repayment, otherwise 0. $D_{i,t}$ is a vector of dummies specifying months-from-switch (11 levels from 5 to 5). $x_{i,t}$ is a vector of covariates including balance, utilization, spending amount, merchant APR, cash APR, and charge-off rate (all are month-card level variables). The error term, $\epsilon_{i,t}$, is assumed to be normally distributed. β_0, β_1, β_2, β_3, and β_4 are a vector of coefficients with the length of 1, 1, 11, 11, and 6, respectively.

Figure F1 shows the coefficient estimates and corresponding 95% confidence intervals for $Switcher \times Months\ from\ Switch$ interactions. The full regression table is shown in Table F1. The results confirm those in the main analysis shown in Figure 2 and Table 6.

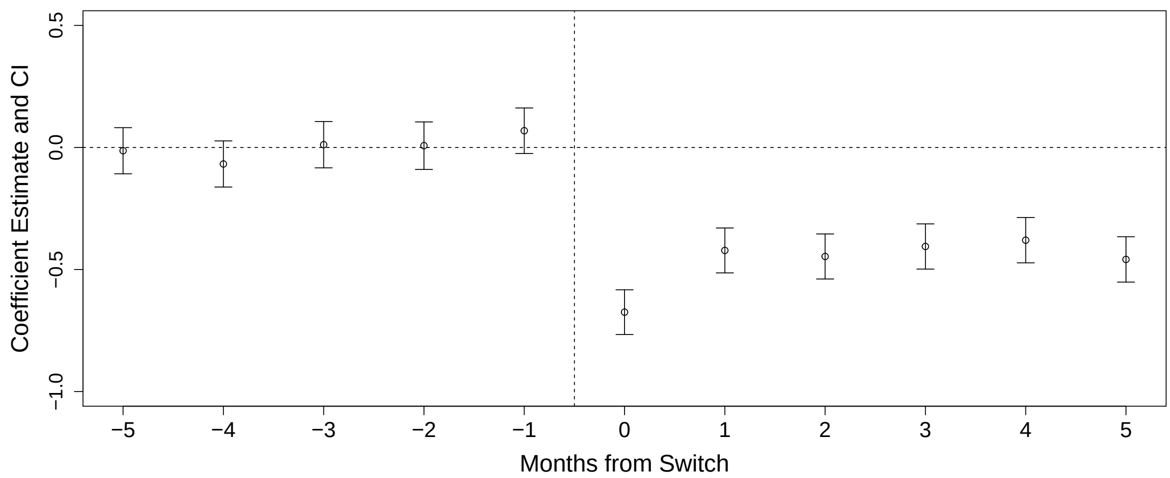


Figure F1: Coefficient estimates and confidence intervals for the effect of switchers switching to a minimum automatic repayment. Error bars are 95% confidence intervals. The coefficients were estimated by OLS with Equation F1 on the sample conditional on positive payments.

Table F1: Coefficient Estimates from OLS model on Conditional Sample

	Estimate	Std. Error	t-value	p-value	
$(Intercept)$	5.4320	0.0388	140.0362	0.0000	***
$Balance$	0.0002	0.0000	73.5639	0.0000	***
$Utilization$	-0.6998	0.0184	-38.1146	0.0000	***
$Spending\ Amount$	0.0008	0.0000	130.2210	0.0000	***
$Merchant\ APR$	-1.0630	0.1349	-7.8784	0.0000	***
$Cash\ APR$	-1.9281	0.1321	-14.5934	0.0000	***
$Charge\text{-}off\ Rate$	-0.2424	0.1241	-1.9531	0.0508	
$Switcher$	0.0183	0.0341	0.5379	0.5907	
$Months\ fr\ Switch : -5$	-0.0056	0.0335	-0.1659	0.8683	
$Months\ fr\ Switch : -4$	0.0443	0.0335	1.3217	0.1863	
$Months\ fr\ Switch : -3$	0.0103	0.0335	0.3080	0.7581	
$Months\ fr\ Switch : -2$	0.0192	0.0334	0.5756	0.5649	
$Months\ fr\ Switch : -1$	0.0141	0.0333	0.4241	0.6715	
$Months\ fr\ Switch : 0$	-0.0073	0.0333	-0.2200	0.8258	
$Months\ fr\ Switch : 1$	0.0200	0.0333	0.6001	0.5484	
$Months\ fr\ Switch : 2$	-0.0219	0.0334	-0.6572	0.5110	
$Months\ fr\ Switch : 3$	-0.0256	0.0335	-0.7649	0.4444	
$Months\ fr\ Switch : 4$	-0.0158	0.0336	-0.4703	0.6382	
$Months\ fr\ Switch : 5$	0.0224	0.0336	0.6655	0.5057	
$Switcher \times Months\ fr\ Switch : -5$	-0.0134	0.0482	-0.2782	0.7808	
$Switcher \times Months\ fr\ Switch : -4$	-0.0677	0.0482	-1.4025	0.1608	
$Switcher \times Months\ fr\ Switch : -3$	0.0113	0.0483	0.2338	0.8151	
$Switcher \times Months\ fr\ Switch : -2$	0.0074	0.0496	0.1481	0.8823	
$Switcher \times Months\ fr\ Switch : -1$	0.0685	0.0476	1.4400	0.1499	
$Switcher \times Months\ fr\ Switch : 0$	-0.6746	0.0468	-14.4213	0.0000	***
$Switcher \times Months\ fr\ Switch : 1$	-0.4218	0.0469	-8.9923	0.0000	***
$Switcher \times Months\ fr\ Switch : 2$	-0.4465	0.0471	-9.4831	0.0000	***
$Switcher \times Months\ fr\ Switch : 3$	-0.4057	0.0473	-8.5802	0.0000	***
$Switcher \times Months\ fr\ Switch : 4$	-0.3798	0.0473	-8.0243	0.0000	***
$Switcher \times Months\ fr\ Switch : 5$	-0.4587	0.0474	-9.6681	0.0000	***
$Num.\ Observations$	67,475				
$Adjusted\ R^2$	0.3296				

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Appendix G Type II Tobit Model

As a robustness check, we conducted an exponential type II Tobit estimation. The sample used is identical to that in the main analysis.

Figure G1 shows the coefficient estimates and corresponding 95% confidence intervals for $Switcher \times Months\ from\ Switch$ interactions taken from the exponential type II Tobit estimation. The full regression table is shown in Table G1. The results confirm those in the main analysis shown in Figure 2 and Table 6.

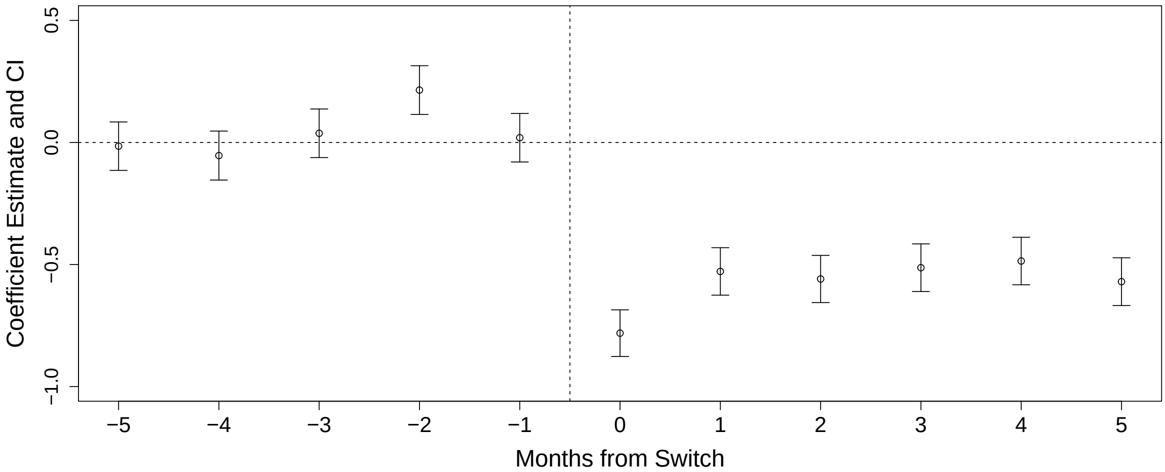


Figure G1: Coefficient estimates and confidence intervals for the effect of switchers switching to a minimum automatic repayment. Error bars are 95% confidence intervals. The coefficients were estimated by the exponential type II Tobit model.

Table G1: Coefficient Estimates from Type II Tobit Model

	Estimate	Std. Error	t-value	p-value	
$h1.(Intercept)$	1.4501	0.0365	39.7755	0.0000	***
$h1.Tenure$	0.0069	0.0014	5.1283	0.0000	***
$h1.Switcher$	-0.0985	0.0482	-2.0445	0.0409	*
$h1.Months\ fr\ Switch: -5$	-0.0322	0.0493	-0.6536	0.5134	
$h1.Months\ fr\ Switch: -4$	-0.0555	0.0492	-1.1278	0.2594	
$h1.Months\ fr\ Switch: -3$	-0.0318	0.0493	-0.6446	0.5192	
$h1.Months\ fr\ Switch: -2$	-0.0039	0.0497	-0.0781	0.9378	
$h1.Months\ fr\ Switch: -1$	0.0310	0.0500	0.6189	0.5360	
$h1.Months\ fr\ Switch: 0$	0.0279	0.0511	0.5466	0.5846	
$h1.Months\ fr\ Switch: 1$	0.1037	0.0518	2.0028	0.0452	*
$h1.Months\ fr\ Switch: 2$	0.0590	0.0516	1.1427	0.2531	
$h1.Months\ fr\ Switch: 3$	0.0388	0.0518	0.7490	0.4538	
$h1.Months\ fr\ Switch: 4$	0.0659	0.0519	1.2696	0.2042	
$h1.Months\ fr\ Switch: 5$	-0.0062	0.0510	-0.1218	0.9031	
$h1.Switcher \times Months\ fr\ Switch: -5$	-0.0274	0.0672	-0.4078	0.6834	
$h1.Switcher \times Months\ fr\ Switch: -4$	-0.0724	0.0666	-1.0873	0.2769	
$h1.Switcher \times Months\ fr\ Switch: -3$	-0.1468	0.0661	-2.2211	0.0263	*
$h1.Switcher \times Months\ fr\ Switch: -2$	-0.7186	0.0641	-11.2066	0.0000	***
$h1.Switcher \times Months\ fr\ Switch: -1$	0.1282	0.0699	1.8324	0.0669	
$h1.Switcher \times Months\ fr\ Switch: 0$	1.2425	0.1041	11.9337	0.0000	***
$h1.Switcher \times Months\ fr\ Switch: 1$	1.1992	0.1061	11.3005	0.0000	***
$h1.Switcher \times Months\ fr\ Switch: 2$	1.2742	0.1026	12.4221	0.0000	***
$h1.Switcher \times Months\ fr\ Switch: 3$	1.1265	0.0938	12.0160	0.0000	***
$h1.Switcher \times Months\ fr\ Switch: 4$	1.0553	0.0936	11.2694	0.0000	***
$h1.Switcher \times Months\ fr\ Switch: 5$	1.0774	0.0907	11.8728	0.0000	***
$h2.(Intercept)$	0.4945	0.0403	12.2588	0.0000	***
$h2.Balance$	0.0002	0.0000	75.6853	0.0000	***
$h2.Utilization$	-0.7125	0.0195	-36.5670	0.0000	***
$h2.Spending\ Amount$	0.0008	0.0000	240.0921	0.0000	***
$h2.Merchant\ APR$	-1.0104	0.1382	-7.3110	0.0000	***
$h2.Cash\ APR$	-1.8762	0.1268	-14.8002	0.0000	***
$h2.Charge\text{-}off\ Rate$	-0.4347	0.1104	-3.9387	0.0001	***
$h2.Switcher$	0.0477	0.0362	1.3173	0.1878	
$h2.Months\ fr\ Switch: -5$	0.0046	0.0360	0.1281	0.8981	
$h2.Months\ fr\ Switch: -4$	0.0561	0.0368	1.5229	0.1278	
$h2.Months\ fr\ Switch: -3$	0.0177	0.0364	0.4859	0.6270	
$h2.Months\ fr\ Switch: -2$	0.0218	0.0364	0.5982	0.5497	
$h2.Months\ fr\ Switch: -1$	0.0125	0.0364	0.3434	0.7313	
$h2.Months\ fr\ Switch: 0$	-0.0220	0.0367	-0.5996	0.5488	
$h2.Months\ fr\ Switch: 1$	-0.0016	0.0365	-0.0428	0.9659	
$h2.Months\ fr\ Switch: 2$	-0.0362	0.0367	-0.9869	0.3237	
$h2.Months\ fr\ Switch: 3$	-0.0385	0.0372	-1.0362	0.3001	
$h2.Months\ fr\ Switch: 4$	-0.0308	0.0368	-0.8368	0.4027	
$h2.Months\ fr\ Switch: 5$	0.0171	0.0372	0.4595	0.6459	
$h2.Switcher \times Months\ fr\ Switch: -5$	-0.0151	0.0506	-0.2989	0.7650	
$h2.Switcher \times Months\ fr\ Switch: -4$	-0.0537	0.0512	-1.0500	0.2937	
$h2.Switcher \times Months\ fr\ Switch: -3$	0.0376	0.0508	0.7402	0.4592	
$h2.Switcher \times Months\ fr\ Switch: -2$	0.2146	0.0509	4.2170	0.0000	***
$h2.Switcher \times Months\ fr\ Switch: -1$	0.0195	0.0508	0.3839	0.7010	
$h2.Switcher \times Months\ fr\ Switch: 0$	-0.7810	0.0488	-16.0212	0.0000	***
$h2.Switcher \times Months\ fr\ Switch: 1$	-0.5283	0.0496	-10.6488	0.0000	***
$h2.Switcher \times Months\ fr\ Switch: 2$	-0.5592	0.0493	-11.3322	0.0000	***
$h2.Switcher \times Months\ fr\ Switch: 3$	-0.5130	0.0498	-10.3022	0.0000	***
$h2.Switcher \times Months\ fr\ Switch: 4$	-0.4856	0.0496	-9.7870	0.0000	***
$h2.Switcher \times Months\ fr\ Switch: 5$	-0.5702	0.0499	-11.4186	0.0000	***
$Num.\ Observations$	73,250				
$Log\ Likelihood$	-127,487.52				

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Appendix H Lender-Induced Switching Sample

In addition to the main analysis with the matched synthetic control group, this section further addresses a particular endogeneity concern that the decision to adopt automatic repayments might coincide with a decision to reduce future payments. If card holders switch with ongoing intention to reduce future repayments, we might falsely attribute the reduction in future payments as arising due to the switch to automatic repayments. In order to account for this concern, we exploit a feature of the data, where there is a (likely exogenous) inducement to set up a minimum automatic repayment. In the data many consumers receive a late payment fee after forgetting to pay their credit card bill. We see that 6.5% of late payment fees are refunded in our sample, and this occurs when card holders contact their credit card company to complain about the fee. Of those card holders who receive a refunded late payment fee, we see that about 10.5% go on to set up a minimum automatic repayment. These consumers are likely to have been prompted by their card provider to set up the automatic repayment to avoid the chance of further late payment fees rather than because of an ongoing intention to repay only the minimum. This is an arguably exogenous inducement to set up a minimum automatic repayment. Adopting this, we further restricted the sample to card holders setting up a minimum automatic repayment after the refund of a late payment fee (within two months) and matched them to a synthetic control group.

The method to create the synthetic control group is as follows. For each switching card, we extracted all non-switching cards which received a refund of the late payment fee in the same calendar month where the switching card received a refund (candidate cards). For the candidate cards, we set a synthetic switch month at one month later from the refund. Covariate values for six months before the (synthetic) switch month were averaged within the switcher and each of candidate cards. Then, one-to-one nearest neighbor matching was done based on the Mahalanobis distance in the averaged covariates. Finally, six card-month observations after the (synthetic) switch month were combined to the matched pre-switch data.

Table H1 shows balanced covariate values. Covariate values are well-balanced between refund-triggered switchers and the synthetic control group, except the charge-off rate. Figure H1 compares the distribution of repayments before and after (synthetic) switch between the refund-triggered switchers and the synthetic control group. The distribution is similar between two groups before (synthetic) switch (Panels A and C). However, the proportion of minimum repayments jumps from 20% to 60%, eliminating repayments at higher prominent values, after refund-triggered cards switch to a minimum automatic repayment, while the distribution is nearly unchanged from before to after synthetic switch for the synthetic control group (except the large proportion of missed payment in pre-switch distribution caused by the way of constructing the sample).

Table H1: Comparison of Pre-Switch Average Card Profile (Refund-Triggered Cards)

	Switcher	Synthetic Control	p-value
Ave. Balance	1,846.13	1,736.60	0.267
Ave. Credit Limit	5,720.44	5,639.04	0.800
Ave. Utilization	0.39	0.37	0.366
Ave. Spending	344.17	350.44	0.979
Ave. Merchant APR	0.19	0.19	0.945
Ave. Cash APR	0.25	0.25	0.616
Ave. Charge-off Rate	0.03	0.02	0.000
Card Tenure	8.33	8.27	0.709

Note. p-values are taken from the bootstrap Kolmogorov-Smirnov test (1,000 resamples).

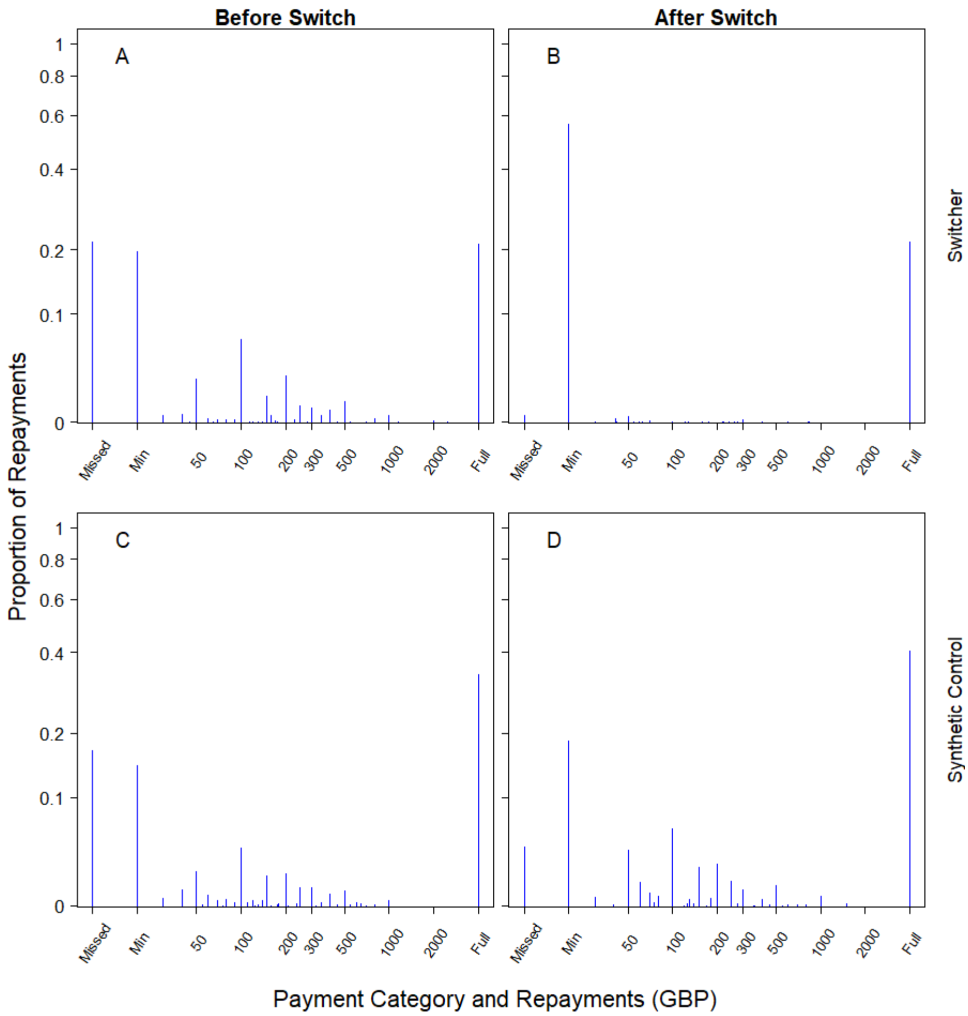


Figure H1: Change in repayments before and after cards switch to a minimum automatic repayment (refund-triggered cards). For the absolute amounts, each bar is a 1-penny-wide bin.

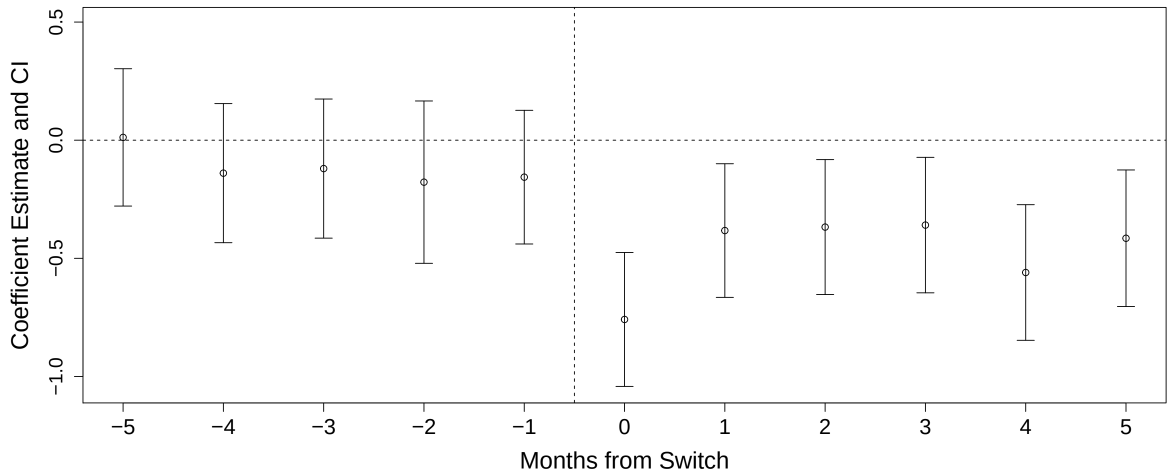


Figure H2: Coefficient estimates and confidence intervals for the effect of refund-triggered switchers switching to a minimum automatic repayment. Error bars are 95% confidence intervals.

Table H2: Coefficient Estimates from Single-Hurdle Selection Model (Refund-Triggered Cards)

	Estimate	Std. Error	t-value	p-value	
$h1.(Intercept)$	1.6182	0.1298	12.4673	0.0000	***
$h1.Tenure$	0.0047	0.0039	1.1976	0.2311	
$h1.Switcher$	-0.2404	0.1706	-1.4089	0.1589	
$h1.Months\ fr\ Switch:-5$	0.1167	0.1856	0.6289	0.5294	
$h1.Months\ fr\ Switch:-4$	-0.3280	0.1624	-2.0199	0.0434	*
$h1.Months\ fr\ Switch:-3$	-0.4393	0.1587	-2.7685	0.0056	**
$h1.Months\ fr\ Switch:-2$	-1.6700	0.1451	-11.5076	0.0000	***
$h1.Months\ fr\ Switch:-1$	0.2333	0.1917	1.2167	0.2237	
$h1.Months\ fr\ Switch:0$	0.0748	0.1829	0.4090	0.6825	
$h1.Months\ fr\ Switch:1$	0.5463	0.2319	2.3557	0.0185	*
$h1.Months\ fr\ Switch:2$	0.0978	0.1861	0.5257	0.5991	
$h1.Months\ fr\ Switch:3$	0.1369	0.1894	0.7227	0.4698	
$h1.Months\ fr\ Switch:4$	0.2336	0.1983	1.1779	0.2388	
$h1.Months\ fr\ Switch:5$	0.0740	0.1869	0.3959	0.6922	
$h1.Switcher \times Months\ fr\ Switch:-5$	-0.0937	0.2463	-0.3804	0.7036	
$h1.Switcher \times Months\ fr\ Switch:-4$	0.0177	0.2210	0.0800	0.9362	
$h1.Switcher \times Months\ fr\ Switch:-3$	-0.0122	0.2152	-0.0568	0.9547	
$h1.Switcher \times Months\ fr\ Switch:-2$	0.1684	0.1990	0.8464	0.3973	
$h1.Switcher \times Months\ fr\ Switch:-1$	0.1825	0.2637	0.6920	0.4890	
$h1.Switcher \times Months\ fr\ Switch:0$	4.0858	55.5345	0.0736	0.9414	
$h1.Switcher \times Months\ fr\ Switch:1$	0.7497	0.4195	1.7871	0.0739	
$h1.Switcher \times Months\ fr\ Switch:2$	0.9581	0.3343	2.8662	0.0042	**
$h1.Switcher \times Months\ fr\ Switch:3$	1.1433	0.3988	2.8670	0.0041	**
$h1.Switcher \times Months\ fr\ Switch:4$	1.0463	0.4049	2.5841	0.0098	**
$h1.Switcher \times Months\ fr\ Switch:5$	1.2024	0.3997	3.0082	0.0026	**
$h2.(Intercept)$	0.1823	0.1313	1.3889	0.1649	
$h2.Balance$	0.0002	0.0000	20.6059	0.0000	***
$h2.Utilization$	-0.5986	0.0621	-9.6473	0.0000	***
$h2.Spending\ Amount$	0.0010	0.0000	43.7406	0.0000	***
$h2.Merchant\ APR$	-2.2604	0.4858	-4.6526	0.0000	***
$h2.Cash\ APR$	-0.1602	0.4430	-0.3617	0.7176	
$h2.Charge\text{-}off\ Rate$	-0.8459	0.3827	-2.2103	0.0271	*
$h2.Switcher$	-0.1136	0.1058	-1.0738	0.2829	
$h2.Months\ fr\ Switch:-5$	-0.0331	0.1014	-0.3262	0.7443	
$h2.Months\ fr\ Switch:-4$	0.0180	0.1024	0.1759	0.8604	
$h2.Months\ fr\ Switch:-3$	0.0721	0.1027	0.7020	0.4827	
$h2.Months\ fr\ Switch:-2$	0.0727	0.1206	0.6033	0.5463	
$h2.Months\ fr\ Switch:-1$	0.3329	0.0999	3.3326	0.0009	***
$h2.Months\ fr\ Switch:0$	0.0699	0.1016	0.6880	0.4915	
$h2.Months\ fr\ Switch:1$	-0.0702	0.1006	-0.6976	0.4854	
$h2.Months\ fr\ Switch:2$	-0.1162	0.1022	-1.1370	0.2555	
$h2.Months\ fr\ Switch:3$	-0.0642	0.1024	-0.6275	0.5304	
$h2.Months\ fr\ Switch:4$	0.0675	0.1024	0.6590	0.5099	
$h2.Months\ fr\ Switch:5$	-0.0746	0.1035	-0.7202	0.4714	
$h2.Switcher \times Months\ fr\ Switch:-5$	0.0118	0.1483	0.0798	0.9364	
$h2.Switcher \times Months\ fr\ Switch:-4$	-0.1395	0.1502	-0.9288	0.3530	
$h2.Switcher \times Months\ fr\ Switch:-3$	-0.1202	0.1502	-0.7999	0.4237	
$h2.Switcher \times Months\ fr\ Switch:-2$	-0.1776	0.1753	-1.0130	0.3111	
$h2.Switcher \times Months\ fr\ Switch:-1$	-0.1564	0.1443	-1.0837	0.2785	
$h2.Switcher \times Months\ fr\ Switch:0$	-0.7588	0.1446	-5.2483	0.0000	***
$h2.Switcher \times Months\ fr\ Switch:1$	-0.3825	0.1443	-2.6508	0.0080	**
$h2.Switcher \times Months\ fr\ Switch:2$	-0.3677	0.1457	-2.5240	0.0116	*
$h2.Switcher \times Months\ fr\ Switch:3$	-0.3593	0.1463	-2.4560	0.0141	*
$h2.Switcher \times Months\ fr\ Switch:4$	-0.5600	0.1465	-3.8237	0.0001	***
$h2.Switcher \times Months\ fr\ Switch:5$	-0.4150	0.1475	-2.8145	0.0049	**
$Num.\ Observations$	6,870				
$Log\ Likelihood$	-11418.08				

Note. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. The prefix of $h1$ indicates the estimation of the probability of not-forgetting a repayment (the first component of the model). The prefix of $h2$ indicates the estimation of latent repayment given not forgetting a repayment (the second component of the model).

Appendix I Details of Excess Interest Cost Simulation

We conducted Monte Carlo simulations to estimate the financial cost from lower repayments among card holders switching to a minimum automatic repayment. We simulate two types of agents. The first type of agents never switch to automatic repayment (Remaining as Non-Auto Cards) while the second type of agents switch to a minimum automatic repayment (Switching to Min-Auto Cards). To resemble real-life use of credit cards, the simulation assumes a steady continuation of purchases and repayments. We use a sample of cards switching to a minimum automatic repayment.

For both types of agents, we simulate their monthly card usages and repayments, using card-month observations of cards switching to minimum automatic repayments during the data period. At each time-step in the simulations, a repayment category is drawn from their actual distributions in card-months with similar card profiles. (Card-months with zero balance was excluded.) The categories include ”missed”, ”minimum” (with £10 buffer for rounding-up), and ”full”. Repayments in the actual distribution which are neither missed, minimum, nor full category were categorized as their own absolute value (e.g., 50.00, 100.00, 200.00). In the simulation, repayments are not allowed to be greater than a corresponding full balance at the time-step. In drawing the repayment category, we use card-months before switch for Remaining as Non-Auto Cards while we use card-months after switch for Switching to Min-Auto Cards. Thus, in the simulations, Remaining as Non-Auto Cards are repaid as if card holders had not switched to a minimum automatic repayment. In addition, the total purchase amount and the total cash advance amount in card-months with similar card profiles were also drawn from the actual distribution. (Card-months with zero balance was included.). For the purchase and the cash advance amounts, post-switch distribution was used for the both types of agents. A card profile for sampling consists of balance, utilization, and purchase amount in the month. The credit limit, the merchant APR, and the cash APR for initializing the agents were the mean values in the month where card holders in our sample set up minimum automatic repayments (£5,700, 19.5%, and 24.6%, respectively) and were assumed to be constant throughout the simulation. If an agent missed a repayment, a £12 late payment fee was incurred in the next month. If an agent made a cash advance or the utilization rate exceeded 1, a cash advance fee, which is £3 or 3% of the cash advance amount (whichever is greater) and a £12 over-limit fee were also incurred. (The regulated fee levels in the UK were used.)

Each time step, the balance was updated reflecting a repayment, interest based on the merchant APR and the cash APR, new purchases, new cash advance amount, late payment fees, cash advance fee, and over-limit fee. A repayment made in a given month was first allocated to the balance for the cash advance, and then any remaining part was used to repay the balance on purchases. Interest on purchase and cash advances were separately calculated in each month with the merchant APR and the cash APR, respectively. The simulation continued for 24 months. We ran simulations with the mean balance in the month where card holders switched to minimum automatic repayments (£1,330.34). We assumed that the whole initial balance was on purchases. The simulated results were averaged and the corresponding confidence intervals were calculated with the bootstrap method (1,000 resamples).

Simulation results are in the main paper section ‘FIELD DATA STUDY: AUTOMATIC MINIMUM PAYMENTS’, subsection ‘Excess Interest Cost Simulations’.

Appendix J Details of Simulation for Total Cost Estimate

We also conducted a final simulation estimating what proportion of total interest and fees incurred by all cards is due to minimum automatic repayment. We randomly sampled 10,000 cards from the whole data (excluding cards with a balance transfer but including cards with a zero merchant APR) and extracted 1,943 cards which were repaid by minimum automatic repayment at least once (Some-Min-Auto Cards). In the simulation the Some-Min-Auto Cards were counterfactually repaid over time as if the cards were not switched to minimum automatic repayment: At each time-step in the simulation, the spending amount was drawn from the whole data period of Some-Min-Auto Cards but repayment categories and amounts were drawn from card-months before Some-Min-Auto Cards had a first minimum automatic repayment. The sampling methods are identical to those used in the excess interest cost simulation (see Appendix I). The balance, interest, and fees were then calculated for the month. The simulation continued up to the number of observations of the card in the data.

The simulation results showed that Some-Min-Auto cards could save about 19.8%, 95% CI [10.6%, 27.8%] of total interest and fees if they were repaid as if they did not switch to a minimum automatic repayment. Considering that the proportion of interest and fees for Some-Min-Auto Cards among total interest and fees for all 10,000 cards is about 43%, we estimate that 8.4%, 95% CI [4.5%, 11.8%] of the total interest and fees for all cards is due to minimum automatic repayment.

Appendix K Supplementary Analysis for the Experiment

Balance Tests. Table K1 gives the proportion of participants who reported they were card users and the proportion who reported paying last month's credit card bill in full. Proportions were similar across conditions, showing that these measures were balanced over experimental conditions. Logistic regressions show no significant effect of omitting the minimum payment, including the full repayment prompt, or an interaction between (all ps $> .20$).

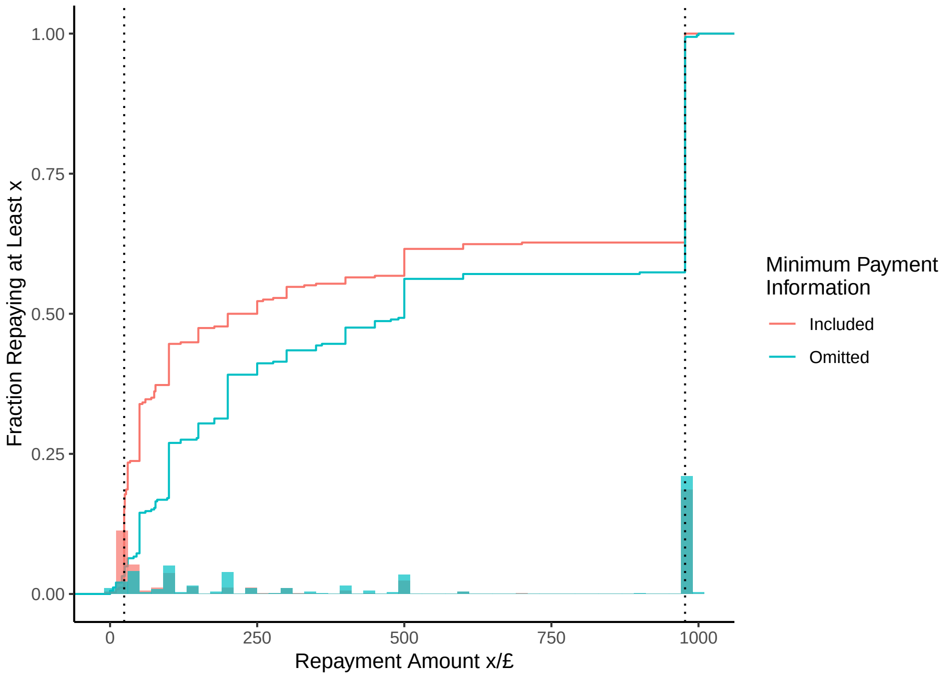
Table K1: Experiment Balance Tests

Condition	Proportion Card Users	Proportion Full Repayers
Control	0.71 [0.64–0.78]	0.52 [0.44–0.61]
Omit Minimum	0.72 [0.65–0.79]	0.51 [0.43–0.60]
Include Prompt	0.71 [0.64–0.77]	0.60 [0.51–0.67]
Omit and Prompt	0.77 [0.70–0.82]	0.62 [0.53–0.69]

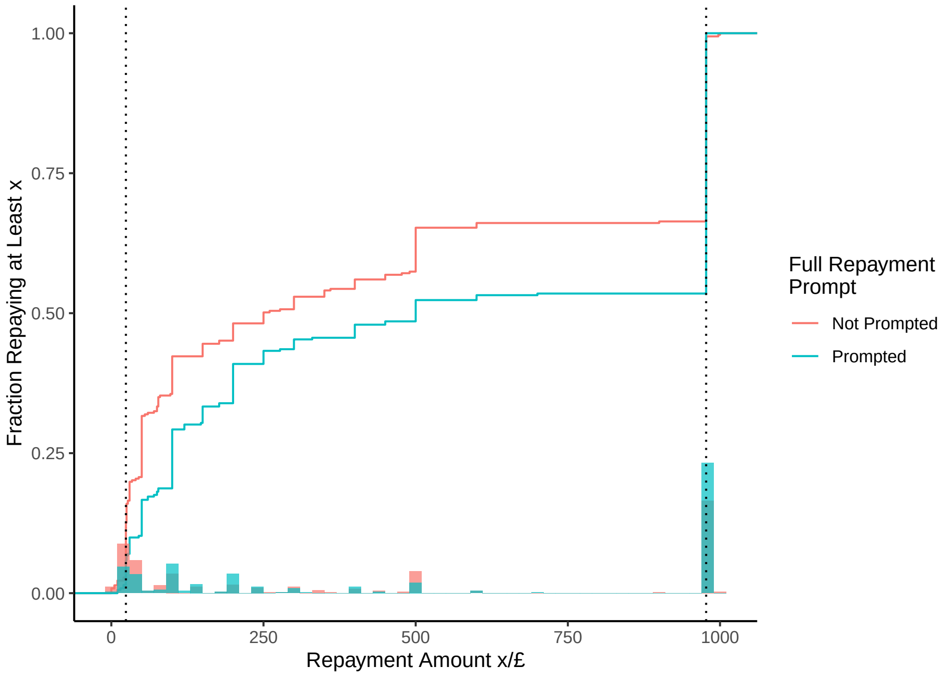
Numbers are proportions of participants with 95% confidence intervals.

The proportion of card users and the proportion of full repayers was also balanced over those included and excluded from the experiment. The fraction of card users was 0.73 for both included and excluded participants. The fraction of full repayers was 0.59 for those excluded and 0.56 for those included in the experiments. (ps $> .50$.)

Repayment distributions. Figure K1 shows the raw distribution of repayments, contrasting the minimum payment information and full repayment prompt conditions. For the minimum payment information inclusion contrast, the cumulative density function of repayments for exclusion is below that for inclusion, indicating that repayments are higher when the minimum payment information is omitted (Figure K1a). A similar pattern is seen when contrasting full repayment prompt conditions. The cumulative density function of repayments for prompting with a full repayment is below that for not prompting, indicating that repayments are higher when people are prompted to pay in full (Figure K1b).



(a) Contrasting the minimum payment inclusion conditions



(b) Contrasting the full repayment prompt conditions

Figure K1: Cumulative density functions showing the distribution of repayments. The y-axis is the probability of making *at least* the repayment on the x axis. Ghosted histograms are the corresponding distributions. Dotted lines are the minimum payment amount and the full repayment amount.

Probability of full and minimum repayments. Because the distribution of payments had mass close to the minimum and at the full amount, we also ran analyses of the probability of making a full payment and the probability of making a payment at or below the minimum. We present the results from linear probability models for ease of interpretation, again without the non-significant interaction between minimum payment information and full repayment prompt.

Logistic regressions lead to the same conclusion.

The effect of omitting minimum payment information is to reduce the probability of paying at or below the minimum from .143, 95% CI [.114, .172] to .031, 95% CI [.002, .060]. This can be seen in Figure K1a as the difference in the step in the cumulative density function at the minimum payment amount. This is a significant absolute decrease of .112, 95% CI [.071, .153], $t(695) = 5.34$, $p < .001$. The effect of omitting minimum payment information increased the probability of paying in full from .374, 95% CI [.324, .425] to .427, 95% CI [.376, .479]. This is a non-significant absolute increase of .053, 95% CI [-.019, .125], $t(696) = 1.444$, $p = .149$. This can be seen in Figure K1a as the difference in the step in the cumulative density function at the full repayment amount.

The effect of including the full payment prompt is to decrease the probability of a minimum payment from .117, 95% CI [.087, .146] to .057, 95% CI [.028, .087]. This is a significant decrease of an absolute .059, 95% CI [.018, .010], $t(695) = 2.81$, $p < .005$. The effect of including the full payment prompt is to increase the probability of a full payment from .337, 95% CI [.286, .387] to .465, 95% CI [.414, .517]. This is a significant increase of an absolute .129, 95% CI [.057, .201], $t(696) = 3.502$, $p < .001$. These differences can be seen in Figure K1b. The effects of including this full payment prompt is additive to that of omitting minimum payment information: one treatment did not crowd out the other.