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For Dad.

You would have loved to read this.

And I wish you could have.

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"Success is the ability to go from one failure to another with no loss of enthusiasm."

- Winston Churchill.

There are many failures in research. I am grateful for the many people who keep me enthusiastic about research.

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"A person who seeks rest finds boredom.

A person who seeks work finds rest."

ABSTRACT

This dissertation has three chapters. Each chapter of my thesis studies different topics within the field of household finance. My research focuses on household finance topics that are both academically interesting and relevant to inform public policymaking on the topic of consumer financial protection regulation. Chapter one and chapter two both study the topic of the economics of credit information. Chapter one is my Job Market Paper "Unraveling Information Sharing In Consumer Credit Market". Chapter two is my Curriculum Paper "Disaster Flags: Credit Reporting Relief from Natural Disasters". Chapters one and two complement my other research research also on the topic of the economics of credit information (Guttman-Kenney et al., 2022; Gibbs et al., 2024; Cookson et al., 2024). Chapter three studies the topic of behavioral household finance. Chapter three is "The Semblance of Success in Nudging Consumers to Pay Down Credit Card Debt". This complements my other research research on the topic of behavioral household finance that is published (Gathergood et al., 2019a; Sakaguchi et al., 2022; Adams et al., 2022a; Guttman-Kenney et al., 2023b) and work-in-progress (Allen et al., 2024; Guttman-Kenney, 2024b; Guttman-Kenney and Shahidinejad, 2024b). The abstracts for each of these three chapters follow on the next three pages. In addition to these three chapters, during the PhD I have also completed a variety of other research (Sakaguchi et al., 2022; Adams et al., 2022a; Guttman-Kenney et al., 2022, 2023b; Gathergood and Guttman-Kenney, 2021; Gathergood et al., 2021a) and have work-in-progress (Allen et al., 2024; Cookson et al., 2024; Gibbs et al., 2024; Guttman-Kenney, 2024b; Guttman-Kenney and Shahidinejad, 2024b).

CHAPTER 1. Unraveling Information Sharing
In Consumer Credit Markets[1]

I study the breakdown of information sharing in US consumer credit markets. There is a 53 percentage point decrease in US credit cards sharing actual payments information with credit bureaus between 2013 and 2022, without any decrease for other credit products. I show the sensitivity of information sharing to innovations enabling targeting of profitable customers. Credit card lenders are responding to credit bureaus' innovation that uses actual payments information to reveal credit card behaviors that predict profitability components: spending drives interchange revenue, and revolving debt drives interest revenue. Spending is a non-default source of uncertainty lenders face and, by not sharing actual payments information, lenders limit their competitors' ability to target high spenders. Mandating information sharing increases competition.

CHAPTER 2. Disaster Flags: Credit Reporting Relief
From Natural Disasters[2]

I study the use of "disaster flags" applied to credit reports to provide relief to consumers affected by natural disasters. Between 2010 and 2020, 59 million consumers have a disaster flag on their US credit report. Flags tag a riskier subset of consumers' tradelines exposed to disasters and temporarily mask defaults on credit reports. Consumers with pre-disaster financial distress experience the largest, but temporary, VantageScore credit score increases from flags. Flags do not increase credit access. Counterfactual policies offering more relief by comprehensively masking all defaults during disasters appear proportionate with limited informational loss to lenders.

CHAPTER 3. The Semblance Of Success In Nudging Consumers To Pay Down Credit Card Debt[3]

I show an active choice nudge changes enrollment choices without changing real outcomes. The nudge is designed to reduce the anchoring of credit card payments to the minimum payment. In a field experiment, the nudge reduces enrollment in Autopaying the minimum from 36.9% to 9.6%. However, the nudge does not reduce credit card debt accumulated after seven payment cycles. Nudged cardholders often choose Autopay amounts that are only slightly higher than the minimum payment. The nudge lowers Autopay enrollment which increases missed payments. The nudge reduces manual payments by Autopay enrollees. Cardholders frequently lacking liquid cash best explains the results.

CHAPTER 1

UNRAVELING INFORMATION SHARING IN CONSUMER CREDIT MARKETS

1.1 Introduction

Information is central to the functioning of financial markets. Historically, voluntary information sharing among firms has developed through the establishment of intermediaries such as trade associations and exchanges. In consumer credit markets, credit reporting agencies ("credit bureaus") act as financial intermediaries to facilitate information sharing between lenders. Despite the central role of these intermediaries in markets with information asymmetry, little is empirically known about the limits of such information sharing arrangements.

This paper documents and explains the reasons for the breakdown of voluntary information sharing in US consumer credit markets. We study the sharing of information about how much credit card account holders actually paid ("actual payments"). Between 2013 and 2022, we find the fraction of credit card accounts that shared actual payments information with credit bureaus *decreased* by 53 percentage points (Figure 1.1). None of the six largest credit card lenders share actual payments information and none plan to voluntarily do so (CFPB, 2023b). Also between 2013 and 2022, sharing actual payments information *increased* for auto loans, mortgages, and unsecured loans. We call this breakdown of co-operative information sharing between credit card lenders an "unraveling" in the spirit of classical information economics (e.g., Akerlof, 1970; Rothschild and Stiglitz, 1976; Roth and Xing, 1994).[1]

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1. We call this paper "unraveling" both due to being inspired by the information economics literature, with a breakdown in the co-operative market for firms voluntarily sharing information, and because we unravel the economic reasons for this breakdown occurring. The literature uses the term "unraveling" to characterize several different economic ideas associated with asymmetric information. Akerlof (1970) shows how private information can mean a market with exogenous contracts unravels such that only the worst quality of good are traded in equilibrium. Rothschild and Stiglitz (1976) show private information can mean companies have incentives to modify their contracts to cream skim lower risk consumers from their competitors and no pure strategy equilibrium exists. Although unraveling is the term used to describe both Akerlof (1970) and Rothschild and Stiglitz (1976), they are mutually exclusive events (Hendren, 2014). Private information can remove all gains from trade under endogenous contracts, and the residual dispersion can explain which

The timing of this information sharing breakdown follows an innovation created by credit bureaus. Before 2013, lenders observing two credit cardholders with the same statement balance could not distinguish one cardholder who pays the minimum due and whose statement balance is mostly "revolving debt" generating interest revenue, from another cardholder who pays their full statement balance and has a high flow of new spending generating interchange revenue. This changed in 2013 when the credit bureaus launched a new product: "Trended Data". A key component of this product uses histories of credit card statement balances and actual payments information to create measures of revolving debt and spending. This product reduces the amount of asymmetric information and enables lenders to distinguish heterogeneous credit card behaviors.

Why would credit card lenders respond to this innovation by stopping sharing actual payments information? In this paper, we provide evidence that this is because the innovation is a competitive threat to incumbent credit card lenders. Credit cards (and other consumer credit markets) are selection markets (e.g., Einav et al., 2021) where profitability is determined by consumers' uncertain behaviors after origination. It is therefore important for lenders to accurately predict consumers' profitability. Lenders need to know a credit cardholder's behavior to decide which marketing offer they are likely to accept, which credit card product (if any) is profit maximizing to offer, and which contract terms to offer (e.g., interest rate, credit limit). Lenders can use the innovation's measures of revolving debt and spending to locate profitable consumers and send targeted marketing of pre-selected credit card offers to attempt to acquire them. However, if lenders do not share the actual payments information that the innovation relies on, it potentially limits the ability of competitors to target and acquire such profitable consumers.

We evaluate the value of observing actual payments information for predicting consumer

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markets unravel (Hendren, 2013). The term "unraveling" is used in matching markets (e.g., Roth and Xing, 1994; Li and Rosen, 1998). In matching markets, there can be large efficiency gains from centralized clearing connecting many buyers and sellers, however, coordination failures can mean market participants move early in an uncoordinated fashion resulting in matches based on incomplete information. Doing so reduces the volume in the centralized process or, in the extreme, means no centralized process occurs.

credit profits and its components. We construct a model of lifetime credit card profits and find that actual payments information increases the ability to predict (measured by R^2), at the account-level over a ten-year horizon, interchange revenue (the transaction fees credit card lenders receive from merchants when a consumer spends on their card) net of rewards by 31%, and financing charges (the sum of interest and fees) net of charge-offs by 4%. This information increases the ability to predict overall profits. Hence, observing actual payments information increases the accurate targeting of profitable cardholders, especially high-spending ones, by credit card lenders. By contrast, in auto loans and unsecured loans, we show that observing actual payments information does little to predict profits, which explains why these lenders are willing to keep sharing such information.

The selection of credit card lenders by their actual payments information sharing decisions is consistent with the innovation being a particular competitive threat to some lenders. Lenders that stop sharing information have higher profitability portfolios with 36% higher financing charges net of charge-offs and higher spending, generating interchange revenue, with 31% higher mean and 41% higher variance compared to lenders who keep sharing information. Lenders that do not share information before the innovation also appear to have higher spending portfolios than the rest of the market. The credit card lenders that stop sharing information have portfolios with lower credit risk and better characteristics on non-credit risk dimensions (e.g., longer tenure, higher balance) after controlling for credit risk. The credit card lenders who keep sharing information have portfolios with the worst types on multiple dimensions (the "lemons" in Akerlof, 1970).

We show that the innovation is a competitive threat as its introduction immediately increases switching. We use a difference-in-differences design with varying treatment intensity where our source of variation is the fraction of a consumer's credit card balances held with lenders that share actual payments information before the innovation. More information would be revealed for consumers with a higher fraction. We find more exposed consumers open relatively more new credit cards after the innovation. We interpret such switching

prompts incumbent lenders to stop voluntarily sharing information.

If lenders do not voluntarily share information, how would mandating information sharing affect markets? Sharing of actual payments information is not mandatory. We instead learn from studying the effects of a prior historical event: the Federal Trade Commission mandating sharing of credit card limit information.[2] We use a difference-in-differences design with varying treatment intensity by how much a cardholder's credit card limit reveals. Cardholders whom this information reveals to be lower risk take out new credit cards from other, outside lenders to which their information is revealed. We interpret these results as showing mandating information sharing can increase switching in line with increasing competition. The US Consumer Financial Protection Bureau is investigating the lack of actual payments information sharing (CFPB, 2023b) and our research findings support a policy to mandate information sharing.

We make two main contributions. Our first contribution is empirically documenting the fragility of information sharing, which is sensitive to innovations enabling targeting of profitable customers. We show how, in a large and highly developed market, an innovation enabling targeting of profitable customers pushes incumbent firms beyond their limit to voluntarily share information. Theoretical literature shows, under information asymmetry, it can be beneficial for firms to voluntarily share information with their competitors through financial intermediaries (e.g., Diamond, 1984; Ramakrishnan and Thakor, 1984) but it is theoretically ambiguous whether they do (e.g., Pagano and Jappelli, 1993; Raith, 1996; Bouckaert and Degryse, 2006). While our paper studies consumer credit markets, it more generally contributes to the literature on information economics and the economics of data (e.g., Bergemann and Bonatti, 2019; Jones and Tonetti, 2020) with the idea that incumbent

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2. Prior research examines the effects of consumer credit reports having additional information added (e.g., Foley et al., 2022a) or information removed (e.g., Musto, 2004; Bos et al., 2018; Liberman et al., 2020; Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023; Fulford and Nagypál, 2023) and how to design credit reporting systems focusing on the length of histories to remember (e.g., Elul and Gottardi, 2015; Bhaskar and Thomas, 2017, 2019; Chatterjee et al., 2020; Blattner et al., 2023; Kovbasyuk and Spagnolo, 2023).

firms can preserve their incumbency position by stopping sharing information as doing so undermines technological innovations that pose a competitive threat.[3]

Our second contribution is revealing two new insights for understanding the credit card market: the importance of spending and card tenure. Default risk is a well-documented source of information asymmetry in lending markets (e.g., Jaffee and Russell, 1976; Adams et al., 2009). We show credit card lenders face a second source of uncertainty: how much a cardholder will spend and so generate in interchange revenue. We document a new fact: credit card tenure varies across and within the credit risk distribution. This fact indicates a need to evaluate credit card profitability over a card's lifetime rather than on a fixed period basis. A card lifetime perspective helps to understand why credit card lenders lend to and heavily concentrate their marketing towards high credit score consumers (e.g., CFPB, 2021) despite these generating little-to-no revenue from financing charges. But it makes sense given acquiring new consumers incurs an up-front fixed cost so consumers with longer tenures can be profitable on interchange alone over their card's lifetime.[4] These insights advance research on the supply of credit cards (e.g., Ausubel, 1991; Agarwal et al., 2018), credit card rewards (e.g., Agarwal et al., 2023b), and payment systems (e.g., Evans and Schmalensee, 2004; Mukharlyamov and Sarin, 2019; Wang, 2023b).[5]

The paper proceeds as follows. Section 1.2 contains background and data. Section 1.3 describes the breakdown of information sharing. We understand this breakdown by studying

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3. For example, Amazon US used to share details of what a consumer purchased in order confirmation emails but stopped doing so following advances in scraping technology that could use this information for targeted marketing. Amazon UK continues to share this information — potentially due to stricter data protection laws. Apple stopped sharing information on device locations following advances in tracker technology that could be used for targeted marketing. Many firms have stopped sharing information following AI developments such as ChatGPT. For examples, Twitter / X used to provide free API access to its data but stopped doing so and Google has restricted the dissemination of its research.

4. This explanation is in line with industry statements. For example, Capital One's US Head of External Affairs states "Even those customers who pay in full every month are profitable and desirable customers for Capital One and other issuers across the industry."

5. Prior literature on the credit card market includes Ausubel (1997, 1999); Agarwal et al. (2010a,b, 2015a,b); Stango and Zinman (2016); Han et al. (2018); Keys and Wang (2019); Ru and Schoar (2020); Galenianos and Gavazza (2022); Grodzicki (2023a,b); Herkenhoff and Raveendranathan (2023); Nelson (2023).

and predicting profitability across consumer credit markets in Section 1.4 and then examining the selection of credit card lenders by their information sharing decisions in Section 1.5. Section 1.6 shows the effects of mandating sharing of credit card limits information. Finally, Section 1.7 concludes.

1.2   Background and Data

1.2.1   Consumer Credit Reporting

Consumer credit reporting agencies ("credit bureaus") — Equifax, Experian, and TransUnion in the US — are financial intermediaries created as a coordination mechanism for lenders to share information about their borrowers with each other. Credit reporting data record information on consumers' borrowing histories. Credit information sharing reduces information asymmetries about credit applicants (e.g., Pagano and Jappelli, 1993; Liberti et al., 2022), helping to limit credit rationing (e.g., Jaffee and Russell, 1976; Stiglitz and Weiss, 1981), and expand credit supply (e.g., Djankov et al., 2007). The consumer credit agencies' technology is to ingest, collate, and store data from a large number of lenders and then produce variables on consumers that are value-added for lenders (and non-lenders). For example, consumer credit reporting data are used to construct credit scores such as FICO and VantageScore.

Lenders demand credit information as it helps to reduce adverse selection (e.g., Bouckaert and Degryse, 2006; Blattner et al., 2023) and moral hazard (e.g., Padilla and Pagano, 1997, 2000; Gehrig and Stenbacka, 2007).[6] US consumer credit reports – and credit scores derived from them – are used for managing credit risk, marketing, and screening.[7] Their

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6. Moral hazard and adverse selection are empirically challenging to distinguish without experimental data such as in Karlan and Zinman (2009). Positive correlation tests between interest rates and default (e.g., Chiappori and Salanie, 2000) may be neither necessary nor sufficient if there are multiple sources of information asymmetry (e.g., Finkelstein and McGarry, 2006; Einav et al., 2021). Information sharing games may have multiple equilibria and, which equilibrium outcome occurs is ambiguous (e.g., Pagano and Jappelli, 1993; Raith, 1996; Bouckaert and Degryse, 2006). Decisions of lenders to share information are a repeated game, and the more information shared, the greater the network effects of credit reporting data (e.g., Hunt, 2002).

7. We study on the US, but the data contents, legal requirements, and industry practices of credit report-

primary purpose is for credit risk assessments: underwriting new credit applications, managing existing portfolios, and pricing credit based on repayment risk. Lenders can attempt to acquire new customers by purchasing consumer lists from credit reporting agencies to use for pre-selected credit card offers. In these offers, lenders will screen consumers by specifying the targeting criteria for credit reporting agencies to use to create these lists and lenders will tailor their product offers to these consumers (studied in Stango and Zinman, 2016; Han et al., 2018; Ru and Schoar, 2020). Furthermore, credit reports are also used to incentivize timely payment of other household bills, such as medical and utility bills, and to help screen applicants in labor, insurance, and housing markets (e.g., Dobbie et al., 2020; Bartik and Nelson, 2023).

Sharing credit information is voluntary and access to information is non-reciprocal. In the US, there is no law requiring lenders to share information with credit reporting agencies.[8] There is also no requirement that sharing be reciprocal: lenders who want to access information shared by other lenders do not need to share their own information. Although sharing is voluntary, the Fair Credit Reporting Act (FCRA) amended with the "Furnisher Rule" of the Fair and Accurate Credit Transactions Act (FACTA) regulates *how* information should be shared (Appendix 1.9.1). This requires that information shared with credit bureaus is done both "accurately" and "with integrity" and provides guidelines for reporting. Information is reported "accurately" if it reflects the terms, liability, and performance of the account. Information is reported "with integrity" if it includes data such that "absence would likely be materially misleading in evaluating a consumer's creditworthiness, credit standing, credit capacity". The specific categories of information that lenders should share with credit reporting agencies, if they decide to share, are not specified – except for a requirement to share credit card limit information. In addition to these laws, the industry body – the "Consumer

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ing vary around the world (e.g., Jappelli and Pagano, 2002). See Barron and Staten (2003); Hunt (2005) for a history of US credit reporting.

8. The data contents, legal requirements, and industry practices of credit reporting vary around the world (e.g., Jappelli and Pagano, 2002). See Barron and Staten (2003); Hunt (2005) for a history of US credit reporting.

Data Industry Association" (CDIA) – governs the terms and format of sharing information. In practice, to satisfy regulation (and the industry body's terms) if lenders share information with credit reporting agencies, they must include information on an account's outstanding balance, delinquency status, closing date, origination terms, scheduled payment amount, and credit limit.

Economic theory can rationalize why lenders are willing to voluntarily share information on a non-reciprocal basis. Lenders have strong incentives to share information to reduce adverse selection (e.g., Pagano and Jappelli, 1993) and moral hazard (e.g., Padilla and Pagano, 2000). Sharing information can increase the likelihood that a consumer repays their debt and avoids the lender incurring costly charge-offs from unpaid debt. In addition, consumers have non-exclusive contracts with different lenders over time (e.g., Bizer and DeMarzo, 1992; De Giorgi et al., 2023). Such "sequential banking" means the lending decision of one lender can affect the repayment of another lender's loan. This interdependence means lenders are privately incentivized to reduce how adversely selected their competitors are by sharing information even if other lenders do not reciprocate. However, lenders will trade off such benefits against the risks of increased competition. More specifically, by sharing their private information with competitors, lenders may be risking giving away a competitive advantage that exists due to the private information they hold and enabling competitors to target their profitable customers. This can explain why some credit market segments do not voluntarily share information at all. For example, most "buy now, pay later" (BNPL) loans, most payday loans, and some subprime auto loans do not share information with credit bureaus.[9]

Another theoretical explanation can be found in Bouckaert and Degryse (2006) which pro-

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9. The main unobserved segment of the auto loan market are high interest rate loans from "buy-here-pay-here" auto dealerships and small finance companies (Low et al., 2021). The main unobserved segment of the unsecured loan market are small-value products such as interest-free buy now pay later (BNPL) loans (e.g., Guttman-Kenney et al., 2023b) and high interest rate payday loans (e.g., Gathergood et al., 2019a). BNPL lenders such as Klarna and payday lenders report in the UK but not in the US. We understand this is due to a combination of greater regulatory pressure to do so in the UK and also lower competitive risk. The UK limits on marketing prevent credit card (or other lenders) using consumer credit reporting information to target marketing to target profitable BNPL or payday lending consumers.

vides a framework for the conditions under which lenders voluntarily and non-reciprocally share all, partial, or no information with their competitors. In Bouckaert and Degryse (2006), an incumbent's decision whether to share information depends upon the extent of adverse selection and market power from consumer switching costs, with lenders sometimes willing to non-reciprocally share information to limit the scope of competition from potential entrants.[10] Another possible explanation is that voluntary information sharing is the strategic response within a repeated game of lenders with regulators. Regulatory guidance "encourages voluntary furnishing of information" by lenders (Appendix 1.9.1). Once a lender grows large enough, regulatory pressure to voluntarily share information can accumulate – as most recently seen with the CFPB pressuring BNPL lenders to do so. A final potential reason is the industry body helps to create a social norm of sharing data and a firm deviating from this harms its reputation in the market. In this paper, we take the initial voluntary information sharing as given and try to understand the breakdown of information sharing.

1.2.2   Data

Consumer Credit Reporting Data

We use the University of Chicago Booth TransUnion Consumer Credit Panel (BTCCP) data (TransUnion, 2023).[11] BTCCP is anonymized consumer credit reporting data from a US consumer credit reporting agency: TransUnion. BTCCP is a 10% random sample of consumers with US consumer credit reports with new entrants added each month to keep the panel representative of the population of credit reports. We use monthly data from 2009 to 2022. Each month of data is a historical archive recreating how a credit report appeared.

BTCCP contains information at the consumer-level (e.g., credit scores) and at the trade-line level i.e., monthly observations for each of the consumer's credit accounts (such as auto

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10. See Padilla and Pagano (1997, 2000); Marquez (2002); Bouckaert and Degryse (2004); Dell'Ariccia and Marquez (2004, 2006); Hauswald and Marquez (2003, 2006); Gehrig and Stenbacka (2007); Schenone (2010).

11. Examples of published research using BTCCP include Kluender et al. (2021); Guttman-Kenney et al. (2022); Keys et al. (2022); Yannelis and Zhang (2023). For a guide and review of consumer credit reporting data see Gibbs et al. (2024).

loans, credit cards, mortgages, unsecured loans). Importantly for our paper, BTCCP trade-line data includes the actual payments variable for each credit account. Each row of trade-line data contains variables for account opening details (e.g., origination date, origination amount, scheduled term) and subsequent performance (e.g., delinquency status, outstanding balance, credit limit, scheduled payment due amount).[12]

BTCCP has anonymized consumer and tradeline identifiers enabling tracking these over time. It also contains anonymized identifiers for the firm reporting tradeline-level information. This enables us to observe what information each firm ("furnisher") shares over time. One lender's data may be reported by multiple furnishers, which may correspond to different regional branches, different portfolios, or be due to internal operational reasons. For credit cards and most credit markets, furnishers are typically the lenders themselves. In the mort-gage market, the furnisher of data may be the firm that services the loan as opposed to the firm that originated the loan. Furnishers enter and exit these data over time. No individual consumers or individual lenders are identified in BTCCP.

We apply standard data restrictions following Gibbs et al. (2024). We drop consumers with missing birth dates and who do not appear in tradeline data. We drop tradeline months not updated in the last twelve months (see Appendix 1.9.2 for a time series). In addition, when we study portfolios as of December 2012, we drop inactive credit cards: we drop cards that are closed, are 180+ days past due, or have no balance on the account in the last twelve months. For account-level analysis, we de-duplicate accounts attached to multiple consumers (i.e., joint accounts, accounts with authorized users) by assigning them to the card's primary cardholder. We deal with outliers by top coding variables at their 99.99 percentiles (and for

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12. BTCCP (and other consumer credit reporting datasets) do not contain variables showing the prices (e.g., interest rates, fee schedules) or revenues credit products generate. Regulatory datasets contain some of this information but have data access restrictions and, even for those with access, have some important limitations. The Federal Reserve's FR Y14-M credit card data, described in Agarwal et al. (2023b), do not have account-level data on interchange revenues, requires estimating rewards, cover a selected subset of the market (19 banks), cannot be linked to credit reports, and cannot link individual accounts across lenders. Similar challenges apply to the OCC's Credit Card Metrics dataset used in Agarwal et al. (2015b, 2018) and the CFPB's Credit Card Database used in Nelson (2023).

those that can have negative values, also at their 0.01 percentiles).

Classifying Credit Card Lenders in BTCCP

BTCCP includes anonymized furnishers of data: this is the relevant unit of analysis as the data furnisher is the firm that makes decisions on what information to share. For predicting credit card profitability and understanding selection, we keep credit card furnishers where we observe at least 10,000 active credit cards (i.e., their portfolio is representative of at least 100,000 cards) in December 2012 and in December 2015. This leaves us with 84 credit card furnishers whose joint market share is 92%. The six largest furnishers jointly account for 66% of the market.[13]

We examine these 84 credit card furnishers' sharing of actual payments information and classify them into four groups: *Always*, *Stoppers*, *Nevers*, and *Others* (Appendix Figure 1.19). *Always* (18% of accounts) are furnishers sharing actual payments information for more than 75% of their credit cards in both December 2012 and December 2015.[14] *Stoppers* (47% of accounts) are furnishers sharing actual payments information for more than 75% of their credit cards in December 2012 and for less than 10% in December 2015. *Nevers* (32% of accounts) are furnishers sharing actual payments information for less than 10% of their credit cards in both December 2012 and December 2015. *Others* contains the remaining furnishers (3% accounts) who we exclude from analysis.

This produces a dataset of 33.3 million open credit card accounts in December 2012, which is representative of 333 million credit card accounts. For these credit card accounts, the mean average credit score is 728, card tenure is 106 months, credit limit is $9,614, statement balance is $2,336, and utilization rate is 35%. For these accounts, we examine

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13. Across our entire dataset there are 7,547 furnishers of credit cards and between 2012 and 2015 there are 5,533. In December 2012 there are 4,912 and in December 2015 there are 4,518. For our 84 furnishers, we follow outcomes on these accounts even if the furnisher changes.

14. A furnisher that shares information for its entire portfolio would not appear as exactly 100% sharing because some months will be accurately reporting a consumer making actual payments information of zero dollars. Most furnishers that do not have trivially small portfolios share actual payments information for either exactly 0% or more than 75% and therefore our threshold choice does not affect results – it merely changes who is classified as *Always* or *Others*. If we added smaller credit card furnishers beyond the 84 in our sample they would generally be categorized in the *Always* or *Other* category.

monthly data from January 2009 to December 2022 (inclusive).

Other Data

We refer to public data released by the US Consumer Financial Protection Bureau (CFPB) summarizing its findings from interviewing credit card lenders about their sharing of actual payments information (CFPB, 2023b).

We also use credit card industry data from R.K.Hammer (a data source used in Jørring, 2023). These aggregated summary data are presented in Appendix 1.9.3 and show the profitability of this market and costs of acquisitions.

1.3   The Breakdown Of Information Sharing

1.3.1   Describing The Breakdown

In consumer credit reports, the "actual payments" variable records information on the total amount of actual payments made on an account in the last month.[15] Actual payments information is *not* required to be shared under FCRA or other laws. If a lender voluntarily shares this information, then other lenders can non-reciprocally access this information and measures derived from it.

For credit cards, but not other consumer credit products, *actual* payments frequently substantially differ from *scheduled* payments. Actual payments on credit card accounts are highly dispersed: a quarter are at or within one percentage point of the scheduled payment amount (the minimum payment due), a third are paying the full statement amount (or more), and the remainder are spread in-between (Appendix Figure 1.11). Whereas for other consumer credit products, the majority of actual payments are at or within one percentage point of the scheduled payment amount: 83% of mortgages, 69% of auto loans, 58% of unsecured loans.

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15. If a consumer makes multiple payments in a month then the actual payments variable is the sum of these.

Figure 1.1 Panel A shows the fraction of accounts in consumer credit reports where actual payments information is observed.[16] This coverage measure is calculated for each consumer credit product: auto loans, credit cards, mortgages, and unsecured loans. The numerator and denominator of this measure are restricted to accounts with positive statement balances and where the date of the last payment is in the last month.

We find a 53.3 percentage point (59.8%) decline in credit card accounts sharing actual payments information from a peak of 89.1% in November 2013 to 35.8% in December 2022. Between 2010 and 2012 the coverage of actual payments information in credit reports is stable with the majority of credit cards, auto loans, mortgages, and unsecured loans accounts sharing this information. There is a short-lived increase in credit cards sharing actual payments information during 2013 due to one furnisher starting sharing this information. This one furnisher later reverses its decision and stops sharing this information. The decline in coverage occurs sharply between 2013 and 2015, resulting in 75 million *fewer* US consumers having such information on their credit reports, and persists after 2015. Credit card lenders are still reporting their credit card accounts to credit bureaus and other information on these (e.g., credit limits, scheduled payment amounts).[17] Our results are robust to not conditioning on the date of the last payment, weighting accounts by balances or credit limits, and including retail or private label credit cards (Appendix 1.9.4).[18] Our results are not specific

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16. We define actual payments information as observed if it is non-zero and non-missing. We classify zeros in this way because some lenders report zeros for all their accounts and therefore are missing. However, there will be some accounts that are zeros and therefore may not be exactly 100% reporting by this measure.

17. See Appendix Figures 1.8, 1.14, and 1.33. Complete coverage does not mean precisely 100% have non-zero amounts as some accounts will accurately have zero credit limits or zero scheduled payment amounts. Complete coverage is conditional on lenders who report information. CFPB (2020) reports "the coverage of other data variables in a consumer's consumer report, such as balance amount and credit limit, are consistently furnished across loan types".

18. Not conditioning on the date of the last payment shows the same pattern but makes the baseline levels of coverage lower as some accounts with positive balances will have zero actual payments made because a zero payment was due or because a consumer missed a payment. Sometimes (general purpose) credit cards and retail credit cards (also known as private label credit cards) are grouped together and we find consistent results with such an approach. Retail credit cards are only able to be used at one merchant or a small group of merchants. This is in contrast to (general purpose) credit cards that are widely accepted by merchants. Retail credit cards are otherwise similar to credit cards and we do not examine them further because it is a much smaller economic market with outstanding balances being approximately a tenth of the size of the

to TransUnion credit reports; CFPB (2020) displays a consistent pattern in Experian credit reports.[19]

165 million credit card borrowers are missing actual payments information on at least one of their open credit cards with a positive balance in December 2022, and only 24% of credit cardholders have actual payments information on all their open credit cards with positive balances. Credit cards are of central importance to consumers' credit reports: 46% of open accounts with positive balances on credit reports are credit cards and 83% of consumers with a positive balance on any credit product in their credit report have at least one active credit card with a positive balance in December 2022.[20]

CFPB (2023b) names the six large credit card lenders who do not share actual payments information as American Express, JP Morgan Chase, Citibank, Bank of America, Capital One, and Discover. Since 2005 these six lenders have had a market share of over two thirds of credit card balances with a market share of 69% in 2021 (Nilson Research). Two of these large credit card lenders have not shared actual payments information since 2012 or earlier. One of these large credit card lenders used to share information but stopped doing so in 2014. Following this, one of these large credit card lenders stopped sharing information in 2014 and the remaining two of these large credit card lenders also stopped in 2015. The remaining set of credit card lenders sharing actual payments information as of 2022 contains none of these six large credit card lenders. None of these lenders intend to voluntarily start sharing information and there are no material barriers preventing them doing so (CFPB, 2023b). Other smaller lenders beyond these six large lenders may also have stopped sharing

credit card market (CFPB, 2021).

19. CFPB (2020) show a decline in actual payments information for credit cards from a peak of 88% in Q3 2013 to 40% from 2015 onwards. Lee and Maxted (2023) report only 30% of their Experian data has credit card actual payments information. We understand our results also hold for Equifax data.

20. The lack of information for a substantial number of consumers and a large fraction of accounts may have general equilibrium effects indirectly affecting the credit risk assessments and marketing decisions of other consumers. For example, some lenders may not purchase information that uses credit card actual payments due to its poor coverage and therefore this may impact decisions for consumers where information is shared by their lenders. See Liberman et al. (2020) and Fulford and Nagypál (2023) for studies of the general equilibrium effects of credit information in other contexts.

information during this time but this was not reported by the CFPB.[21]

There is no decline in sharing actual payments information for installment loans: auto loans, mortgages, and unsecured loans. Coverage trends up over time for all types of installment loans and is effectively 100% by December 2022: actual payments information is shared for 98.4% of auto loans in 2022 (79.4% in 2013), 99.6% of mortgages (84.1% in 2013), and 97.9% of unsecured personal loans (74.4% in 2013).

1.3.2 Innovation

"Trended Data is the most important tool developed by the credit reporting agencies since the advent of the credit score." – Director of Credit Card Risk, 2014

What changed to prompt large credit card lenders to stop sharing actual payments information? We explain that this followed the launch of a technological data innovation. From 2013, credit reporting agencies launched a new product: "Trended Data". This innovative new product created a bundle of variables extracting more insights from information – most notably actual payments – that lenders already shared with credit reporting agencies. Trended Data combines information from the latest available point in time with information in historical archives. Before Trended Data, consumer credit reports created variables using data from the latest available point in time. For example, they may show a consumer's total outstanding credit balances as of last month or whether the consumer had any delinquency in the last seven years. By linking data across multiple archives, Trended Data enables the creation of trended variables such as whether a consumer's total outstanding credit balances have trended up or down in the last year.

In the context of our study, the relevant part of Trended Data is how it uses histories of credit card statement balances and actual payments to reveal heterogeneous credit card

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21. The 30 largest lenders account for 95% of the credit card market. Smaller lenders beyond he top six large lenders account for 19% of the market based on public data from Nilson Research. They are, in decreasing order market share: U.S. Bank, Wells Fargo, Barclays (which only offers co-branded cards), Navy FCU, Synchrony, USAA, Credit One, Goldman Sachs, and PNC. A tail of very small lenders account for the remaining 12% of the market.

behaviors. Trended Data products include measures of credit card spending and credit card revolving debt. Before Trended Data these measures were not observed. Trended Data measures are available to purchase for marketing. Doing so enables highly targeted marketing screening consumers based on their revolving and spending behaviors for consumers of a given credit risk and statement balance. Use of Trended Data for marketing and other purposes (e.g., credit risk) is on a non-reciprocal basis.[22] Lenders can purchase Trended Data without sharing the input data they require – most notably credit card actual payments.

Why are lenders still willing to keep sharing such information on installment loans but not for credit cards after this innovation? Trended Data is a more disruptive innovation for competition for credit cards than for installment loans because it enables targeted marketing based on credit card behaviors. This information increases the ability to target a competitor's profitable credit cardholders. For example, Experian states its spending measure helps clients to *"calculate profit by providing an estimate of consumer spend"* including to *"prioritize marketing investments and target higher spending consumers"* and to *"optimize enhanced value propositions to the right spending segments"*. Similarly Equifax describes how *"a national bank wanted to build more market share and also proactively target consumers who are more likely to be high spenders in the next 12 months. They needed a solution to more accurately predict propensity to spend while creating profitable returns on marketing investments"*.

Whereas for installment loans, Trended Data's value is in improving credit risk assessments. In the mortgage market, Fannie Mae found *"including Trended Data materially improved modeling of loan performance"* and from 2016 requires its use for underwriting. This is consistent with statements by Equifax, Experian, TransUnion, and also with both FICO and VantageScore who incorporate Trended Data into the latest versions of their credit

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22. One lender who previously shared information suggested in its response to the CFPB (2023b) that *if* data access was reciprocal ("give-to-get") it may share actual payments information. However, the credit reporting agencies are unwilling to set these terms as it would set a precedent and also limit their ability to sell this product to a broader market. And, even if the agencies did do so, there's no indication that *all* large lenders would start sharing actual payments information.

scoring models (VantageScore 4.0 available from 2017 and FICO 10T available from 2020) both approved for use by the Federal Housing Finance Agency in 2022. This indicates a lack of sharing of credit card actual payments information may have a negative externality: worsening credit risk evaluations, and therefore misallocating or mispricing capital in auto loan, mortgage, and unsecured loan markets. We find evidence of this: trends in credit card actual payments information improve the performance of consumer credit scores predicting not only credit card default but also installment loan default. The lack of credit card actual payments information for 165 million credit cardholders also means that if these cardholders repay their credit card debt in full this positive behavior is unobserved in their credit report so does not improve their credit score (and may potentially be a disincentive for informed consumers from doing so).

Why was Trended Data launched in 2013? From 2010, the CARD Act limited credit card financing charges: fees (e.g., Agarwal et al., 2015b) and interest (e.g., Nelson, 2023). Pressures on these credit card revenue streams increased the relative importance of interchange revenue (e.g., Experian 2023). Substantial charge-offs incurred due to the 2008 financial crisis meant lenders increasingly shifted their focus away from short-term risky profits (e.g., TowerGroup 2010 Note) and so, as a lower risk source of revenue, interchange revenue becomes increasingly attractive. The 2010 Durbin Amendment also restricted interchange fees on debit cards but not on credit cards (e.g., Mukharlyamov and Sarin, 2019). Banks therefore had increased incentives to attempt to shift their customers' spending to credit cards in order to earn higher interchange fees.

Technically, lenders could construct spending and revolving debt measures before Trended Data by purchasing historical account-level credit reporting data containing balances and actual payments. However, discussions with industry participants have confirmed in practice they did not. This is for a combination of three reasons. First, in 2012 and earlier there were technological constraints with storing and processing the volume of data. Even Equifax reports on its 2013 earnings call: *"It took us time just to build the infrastructure to house the*

data". Similarly, Barclays Research said *"Intuitively Trended Data sounds like a no-brainer (with value seen across the credit chain of acquisitions, origination and account management) but the limitations of the technology have historically prevented its widespread use".* Second, before Trended Data constructing measures from account-level data would require purchasing at least twelve historical archives. This can be prohibitively costly – especially for marketing purposes of prospective customers – as credit reporting data is charged on a per-archive basis. Third, industry participants told us of concerns that using historical archives could expose them to costly legal FCRA compliance issues. The credit reporting agencies' Trended Data products are FCRA compliant.

Trended Data was also later launched in Canada (e.g., TransUnion in 2015) and the UK (e.g., TransUnion in 2019). In these countries, unlike in the US, it did not prompt a breakdown of sharing actual payments information for credit cards or other loans.[23] This is explained by different institutional arrangements. The UK has reciprocity in sharing information and data cannot be used for marketing but can *"only for the purposes of control of risk, fraud and over-indebtedness"* (terms of sharing are administered by the industry body "SCOR": the Steering Committee on Reciprocity). The UK caps interchange revenue meaning high-spending consumers generate less revenue than they do in the US where there is no cap.[24] This means by UK lenders sharing information on credit card actual payments they were not, unlike the US, at greater risk of their profitable cardholders being targeted by competitors. Canada's credit reporting arrangements do not have reciprocity in data sharing as the US does. However, unlike the US, Canada does not allow individual marketing of credit cards but only allows aggregated data on geographic areas to be used for target-

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23. The same credit reporting agencies operate in these markets – Equifax, Experian, and TransUnion operate in the UK; Equifax and TransUnion in Canada – and the structure of the credit card market is analogous – indeed some lenders, such as Capital One, operate in all three markets. High and stable coverage of actual payments information means researchers using consumer credit reporting data from Canada or the UK can study credit card payment behaviors across a consumer's credit card portfolio (e.g., Adams et al., 2022a; Guttman-Kenney et al., 2023a; Allen et al., 2024).

24. In the EU and UK, credit cards operating via MasterCard or VISA have caps on interchange of 0.3%. As American Express is both a payment merchant and a credit card lender it is not directly capped.

ing.[25] Without the channel of targeted offers, there may be less of the potential longer-term competitive gains from Trended Data in Canada and the UK then there would be in the US.

1.3.3   Effect of Innovation on Information Sharing

Difference-in-Differences Methodology

We now extend our earlier descriptive evidence to apply a difference-in-differences methodology to estimate the causal effect of Trended Data on credit card actual payments information sharing. CFPB (2023b)'s interviews with lenders provide further corroborating evidence for taking such an approach: *"One company mentioned that, as an impetus to start suppressing data in 2013, some nationwide consumer reporting companies were starting to market new data solutions to lenders that leveraged the actual payment variable without requiring data buyers to furnish it"*.

We estimate effects using the OLS regression specified in Equation 1.1 with one observation per furnisher's credit product portfolio (p), per year-month (t), including fixed effects for furnisher's credit portfolio (γ_p) and year-month (γ_t). We weight observations by the number of accounts in each furnisher's credit product portfolio. Our parameters of interest, δ_τ, show the interaction between calendar year-month indicators (D_τ) and an indicator for a furnisher's credit card portfolio ($CRED_p$). The omitted time period is December 2012. We cluster standard errors by furnisher. We restrict the sample to furnishers with credit portfolios in both 2010 and 2022. We conduct regressions changing the sample to include either auto loans and unsecured loans as control groups (where $CRED_p = 0$), restricting to furnishers' portfolios observed throughout this period to produce a balanced panel of monthly data from 2010 to 2022. Auto loans and unsecured loans are used as control groups based on the rationale that these credit markets are less disrupted by Trended Data than

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25. Equifax Canada state "while the information from a single Equifax credit file can't be divulged, the average of credit behavior and scores of a particular neighborhood can."

credit cards.[26]

$$Y_{p,t} = \sum_{\tau \neq \text{Dec } 2012} \delta_\tau \Big(D_\tau \times CRED_p \Big) + \gamma_p + \gamma_t + \varepsilon_{p,t} \qquad (1.1)$$

Empirical Results

Our difference-in-differences results in Figure 1.1 Panel B show a 50.9 (s.e. 15.0) percentage point decline in December 2015, relative to December 2012, in the fraction of accounts sharing actual payments information on credit cards compared to auto loans and a 54.8 (s.e. 15.0) percentage point decline compared to unsecured loans.[27] While sharing of credit card actual payments information changes little between 2015 to 2022, sharing of actual payments information for auto loans and unsecured loans grows over time and therefore, by December 2022, our difference-in-differences estimates show 65.1 (s.e. 16.1) and 68.5 (s.e. 16.0) percentage point declines relative to auto loans and unsecured loans respectively. Our results are statistically significant at the 1 percent level but we note that the standard errors after 2013 are wide (15 to 16 percentage points) as a result of clustering at the furnisher level where a small number of large credit card furnishers drive the overall results. We interpret our estimates as showing the reduction in information sharing is an unintended response of credit card lenders to consumer credit reporting agencies' innovation designed to reduce information asymmetry and increase information sharing.

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26. For installment loans, the actual payments information is in the same credit archive as the corresponding scheduled payment and balance information. Whereas for credit cards, the actual payments observed in this month's credit archive correspond to the previous credit archive's scheduled payment and balance information (explained more in section 1.4.3). This means Trended Data's use of multiple archives reveals more for credit cards than for installment loans.

27. Estimates in Appendix Table 1.5. Appendix Figure 1.17 and Table 1.6 show results are robust to using our broader sample definition and weighting by balances.

1.4   Consumer Credit Profitability

This section understands the breakdown of information sharing by providing a conceptual framework for how profitability differs for credit cards compared to installment loans: auto loans and unsecured loans. These three markets have $3.4 trillion in outstanding balances in December 2022.[28] This framework enables us to then empirically evaluate the marginal value of actual payments information to predicting profitability. We show how actual payments information helps to measure heterogeneous credit card behaviors and helps to predict lifetime profitability.

1.4.1   Credit Card Profitability

Lenders' expectations of profitability determine which new credit card accounts to attempt to acquire. For acquired accounts, after their contract terms (e.g., interest rate, loan duration) are determined the lender remains uncertain about how a consumer will use the account and the profits the account will ultimately generate. Lenders may be better able to predict profitability if profits are less dependent on uncertain consumer behaviors. Lenders will also be better able to predict profitability if consumer behaviors driving profitability are more persistent over time. If so, historical data such as actual payments information can potentially be informative for predicting profits, and its components.

Two credit cards with identical product features can yield substantially different realized profits (Π_{POST}^{CRED}). This is because credit card profits have multiple, uncertain sources of revenues and costs which are all determined by cardholder behaviors after origination.

Given this uncertainty, lenders need to make decisions based on expected profitability: Π_{PRE}^{CRED} as shown in Equation 1.2. Expected profitability at time $t = 0$ depends on the information available at that time (X_0), primarily information in consumer credit reports. Profitability covers the duration of a card's life from opening at $t = 1$ and held until $t = T$. If

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28. Appendix Figure 1.8 summarizes the market sizes over time and Appendix Table 1.4 summarizes the differences in product structures.

a lender can observe how long a consumer currently holds a credit card for, they may be better able to predict a card's lifetime profitability. The term a represents the acquisition costs (including marketing and underwriting costs) incurred at $t = 0$, which are approximately $140 and range from $50 to $390 in 2012 (R.K.Hammer, Appendix 1.9.3). Other components of revenue and costs have an additive structure and are uncertain: i_t is interchange revenue net of rewards expense, r_t is interest revenue, f_t is consumer fee revenue (primarily late fees and annual fees), and c_t are the costs of charge-offs and fraud. This specification allows for profits to be discounted over time ($\delta < 1$), and for risky interest revenue to be discounted ($\alpha < 1$) if lenders are risk-averse (for example, due to regulation). Lenders also have other organizational-level costs such as costs of funds and operations separate from this account-level measure of profitability.[29]

$$\Pi_{PRE}^{CRED} = E_{t=0}[\Pi_{POST}^{CRED}|X_0] = E_{t=0}\Big[\sum_{t=1}^{T}\delta^t\Big(i_t + \alpha\, r_t + f_t - c_t\Big)|X_0\Big] - a \qquad (1.2)$$

Because cardholder behaviors are heterogeneous, the ability to predict cardholder behaviors is crucial to determining whether they are profitable to lend to and, if so, which type of credit card to market to a consumer (e.g., a low interest rate card or a high rewards card). Charge-offs are rare but costly events - the largest costs lenders face and so credit scoring to predict the risk of default is the foundation for lending decisions. Interest revenue is generated proportionally from revolving debt (d_t), $r_t \propto d_t$, where revolving debt is the stock value of the balance remaining after deducting actual payments (i.e., the amount revolved from one statement to the next statement). For a given interest rate, higher interest revenue is generated from higher revolving balances and from revolving balances for longer durations. The amount of interchange revenue net of rewards expense is proportional to the amount of spending, $i_t \propto s_t$, where spending (s_t) is the flow value of new transactions

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29. R.K.Hammer and Agarwal et al. (2018) estimate costs of funds of under 2% and organization costs of 7% to 8% in 2012.

from one statement to the next statement. If a consumer's historical revolving and spending behaviors are observed and are persistent over time, lenders may be better able to predict interest and spending revenues, and ultimately profitability.

Credit card lenders have different business models and risk tolerances such that they do not all want to lend to the same consumers. This means lenders are not only interested in predicting overall profitability but its component parts. Annual reports show the majority of the revenue generated by many large credit card lenders, such as Capital One, comes from financing charges (the sum of interest revenue and consumer fee revenue) as opposed to interchange revenue. At the other extreme, the majority of American Express's revenue comes from interchange revenue. American Express and Discover are both credit card lenders and payment network providers and so retain more interchange revenue than other credit card lenders who use MasterCard or VISA payment networks, which comes at the cost of splitting the interchange revenue. Capital One's proposed merger with Discover is expected to enable them to earn higher interchange revenue.

Increasing the predictability of consumer-level profits enables lenders to reduce their costs by avoiding marketing to unprofitable consumers. Predicting a consumer's profitability can help lenders not only work out which consumers to attempt to acquire but also which of the large array of available credit card products to market to them. Marketing the wrong card to a profitable consumer may yield a low conversion rate or make them less or even unprofitable. Marketing costs are a large expense for credit card lenders irrespective of their business models: in 2021 American Express spent $5.5 billion and Capital One spent $4.0 billion (public annual reports). Marketing pre-selected credit card offers through direct mail is overwhelmingly concentrated towards very low credit risk ("superprime" or "prime plus" credit score) consumers (CFPB, 2021).

Better prediction reduces the degree of adverse selection a lender faces and enables improved screening. Pre-selected credit offers are a form of screening. For example, a lender may send a consumer a high rewards card that also has a high annual fee. Such offers screen

for high-spending consumers and deter applications from high-risk consumers who can not afford the up-front annual payment. Pre-selected credit offers are highly targeted in their marketing design and contractual features to maximize profits across heterogeneous behavioral types of consumers (e.g., Ru and Schoar, 2020) and vary across the business cycle (e.g., Han et al., 2018).

1.4.2 Installment Loan Profitability

How does credit card profitability compare to that of installment loans? In this section, we analyze this question by studying installment loans: auto loans and unsecured loans. Equation 1.3 shows the profit equation for installment loans. Unlike credit cards, installment loans do not generate an interchange revenue stream. Installment loans also have uncertain consumer fee revenue (f_t). Auto loans and unsecured loans are products that have a fixed term, unlike the open-ended structure of credit cards. As with credit cards, there is uncertainty over whether installment loans will be unpaid and become charged-off (c_t). Auto loans are secured against the auto vehicle which means if the consumer stops paying, the lender can seize the asset to limit their losses. Credit cards and unsecured loans are not collateralized against an asset and therefore if a consumer stops paying, it may be more challenging for the lender to limit their losses.

At origination, installment loans typically have a fixed loan amount, duration, and scheduled monthly payment. This means that, in contrast to credit cards, interest revenue (r_t) is known. However, auto loans and unsecured loans also have a second source of uncertainty: prepayment. If a consumer decides to pay down their loan earlier than scheduled ("prepayment"), the lender may receive less interest revenue (q_t) than originally scheduled (r_t) – although it is sometimes able to recoup some of this through charging prepayment fees.[30]

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30. Grunewald et al. (2020) write *"In both the subprime and prime markets, prepayment risk is substantial"* in the auto loan market.

$$\Pi_{PRE}^{INST} = E_{t=0}[\Pi_{POST}^{INST}|X_0] = \sum_{t=1}^{T} \delta^t \left(\alpha \, r_t - E_{t=0}[q_t|X_0] \right) + E_{t=0}\left[\sum_{t=1}^{T} \delta^t \left(f_t - c_t \right)|X_0 \right] - a \tag{1.3}$$

1.4.3   Measuring Credit Card Behaviors

Observing actual payments information $(p_{i,t})$ enables the measurement of two credit card behaviors: "revolving debt" $(d_{i,t})$ and "spending" $(s_{i,t})$. A credit card's statement balance $(b_{i,t})$ is the amount on a credit card at the time the statement is issued. This includes new spending, revolving debt, and financing charges. Credit card revolving debt is a stock measure defined in Equation 1.4 as the credit card statement balance $(b_{i,t-1})$ less actual payments $(p_{i,t})$ made against that statement, and where negative values are coded as zeros. This differentiates accounts into (1) "revolvers" where some debt is revolved from one statement to the next $(d_{i,t} > 0)$ who generate interest revenue, and (2) "transactors" (also known as convenience users) who do not $(d_{i,t} = 0)$. The term $b_{i,t-1}$ rather than $b_{i,t}$ is used in this equation because credit cards have a grace period where payments are due by a specified date at least 21 days after the date a statement is issued and therefore the actual payments observed in this month's credit archive correspond to the statement balance in the previous month's archive. This is why multiple credit archives need to be observed, as enabled by Trended Data, to accurately measure revolving debt.

$$d_{i,t} \equiv \begin{cases} b_{i,t-1} - p_{i,t} & \text{if } b_{i,t} - p_{i,t} \geq 0 \\ 0 & \text{otherwise} \end{cases} \tag{1.4}$$

Credit card spending $(s_{i,t})$ is a flow measure of consumption defined in Equation 1.5 as used in Ganong and Noel (2020). Multiple credit archives need to be observed, as enabled by Trended Data, to accurately measure spending. This measure is inclusive of financing charges and negative values are coded as zeros. Spending behavior is important for lenders

as credit card interchange revenue is a function of spending. Even if we measure revolving debt perfectly, this is insufficient for measuring spending because some consumers can and do pay more than the statement balance (e.g., paying before their statement is issued, or paying their outstanding balance).[31]

$$s_{i,t} \equiv \begin{cases} b_{i,t} - b_{i,t-1} + p_{i,t} & \text{if } b_{i,t} - b_{i,t-1} + p_{i,t} \geq 0 \\ 0 & \text{otherwise} \end{cases} \tag{1.5}$$

We evaluate how much error is added to the measurement of credit card behaviors when actual payments are unobserved. If we observe both statement balances and actual payments, then we can construct these measures and so mechanically there is no unexplained variation (i.e., $R^2 = 1$). We evaluate R^2 relative to this benchmark by estimating OLS regressions shown in Equation 1.6 where outcomes $Y_{i,t}$ are revolving debt and spending and where predictive inputs are the current statement balance $(b_{i,t})$, previous statement balance $(b_{i,t-1})$, the difference between these conditional on being non-negative $(\tilde{\Delta} b_{i,t})$, and indicators for non-zero current and previous statement balances. We run this regression for all credit scores and then separately for each credit score segment: subprime (the lowest credit score group / highest credit risk group), near prime, prime, prime plus, and superprime (the highest credit score group / lowest credit risk group). We use data in December 2013 as the period of highest coverage of actual payments information and drop data from furnishers not sharing payments information. There is one observation per credit card account (i).

$$Y_{i,t} = \alpha + \beta_1 \, b_{i,t} + \beta_2 b_{i,t-1} + \beta_3 \tilde{\Delta} b_{i,t} + \beta_4 \mathbf{1}\{b_{i,t} > 0\} + \beta_5 \mathbf{1}\{b_{i,t-1} > 0\} + \varepsilon_{i,t} \tag{1.6}$$

Figure 1.2 summarizes our results for measuring revolving debt (Panel A) and spending (Panel B) without actual payments information. Across all credit scores, revolving debt

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31. For example, consider a consumer with \$0 statement balance at both time $t = -1$ and time $t = 0$. Therefore their revolving debt is zero $(d_t = 0)$. But their spending could be zero or a positive number.

is measured with an R^2 of 0.94; this shows that not observing actual payments increases measurement error (relative to a benchmark of $R^2 = 1$). The R^2 is decreasing in credit score and is lowest for the superprime group where $R^2 = 0.60$.[32]

Our results show that the actual payments variable is even more important for measuring spending. Across all credit scores, spending is measured with significant noise with an R^2 of 0.51 when actual payments are unobserved (relative to a benchmark of $R^2 = 1$).[33] Adding other variables—credit score, zipcode income, scheduled payment, and trends in statement balances—does not change our findings (Appendix Table 1.7 and Figure 1.18).

This noise in measuring credit card behaviors driving profitability limits the value of Trended Data products to realize their innovative potential for reducing information asymmetry and increasing competition. Such noise is also problematic for academic researchers wanting to use credit reporting data for measuring revolving credit card debt (e.g., Bornstein and Indarte, 2023; Fulford and Schuh, 2023; Lee and Maxted, 2023) and measuring credit card spending as a consumption measure (e.g., Ganong and Noel, 2020; Gross et al., 2020).

1.4.4 Predicting Consumer Credit Profitability

Modeling Profitability

We now take our profitability equations to empirically examine the predictability of profitability and the marginal value of actual payments in such predictions. We begin by constructing measures of realized profits at the account-level for multiple consumer credit products. Having developed empirical measures of profitability, we then perform an exercise in predicting account-level profits. We summarize our methodology in this and the subsequent

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32. R^2 are 0.99 (subprime), 0.98 (near prime), 0.96 (prime), 0.89 (prime plus). R^2 results are similar out-of-sample: 0.94 (all) 0.99 for (subprime), 0.98 (near prime), 0.96 (prime), 0.89 (prime plus), and 0.61 (superprime).

33. R^2 are 0.54 (subprime), 0.58 (near prime), 0.56 (prime), 0.53 (prime plus), 0.50 (superprime). R^2 are similar out-of-sample: 0.50 (all), 0.42 (subprime), 0.50 (near prime), 0.58 (prime), 0.54 (prime plus), and 0.50 (superprime).

subsection with more details provided in Appendix 1.9.7.

Measuring realized profits for installment loans is fairly straightforward as we observe a loan's origination terms and charge-offs. Loan terms (loan origination amount A^{INST}, number of scheduled monthly payments N^{INST}, and the scheduled monthly payment amount M^{INST}) provide the scheduled financing charges ($M^{INST} \times N^{INST} - A^{INST}$).[34] We account for loan prepayment by subtracting a proportion of scheduled financing charges when the loan is repaid before its scheduled end date.

Measuring realized profits for credit cards requires calculating financing charges, interchange net of rewards, and charge-offs. As interchange net of rewards is proportional to spending, we calculate this by measuring spending (exclusive of estimated financing charges) and then applying a 0.5% factor.

We introduce a new methodology to estimate credit card financing charges in credit reporting data despite these data not containing a variable for this or key product terms (e.g., interest rates). We do so using the formula in Equation 1.7 that credit card lenders use to calculate minimum payments. Its first component is a floor dollar amount $\$\mu$.[35] The second component is the sum of (i) a percentage $\theta\%$ of B_t: the statement balance before financing charges ($B_t \equiv b_t - r_t - f_t$) and (ii) financing charges ($r_t + f_t$).

$$M_t^{CRED} = \max\left\{\$\mu,\ \theta\% \, B_t + r_t + f_t\right\} \tag{1.7}$$

Because minimum payments are deterministically calculated with this formula, observing statement balances and scheduled minimum payments (both inclusive of financing charges) suffices to work out the parameters $\$\mu$ and $\theta\%$ for each lender. If a cardholder has zero financing charges, this formula simplifies to $M_t^{CRED} = \max\left\{\$\mu,\ \theta\% \, b_t\right\}$ and as we observe

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34. We note that for some loans, especially high credit score auto loans, this will imply a zero percent interest rate. Interest rates can also be calculated for mortgages (e.g., Shahidinejad, 2023) and installment loan products (e.g., Yannelis and Zhang, 2023).

35. If balances are below this floor amount then balance rather than the floor is owed. We ignore as this is not economically important given how low the floor amounts are.

both M_t^{CRED} and b_t we can find the lowest combination of $\$\mu$ and $\theta\%$ that matches the data. If we find the correct parameters this would not be expected to match all data points as many observations will have financing charges and therefore have higher values of M_t^{CRED} for a given b_t. Having inferred $\$\mu$ and $\theta\%$, we can then estimate the minimum payment *before* financing charges for each month of data. Estimated financing charges are then the difference between the observed minimum payment, which includes financing charges, and our predicted minimum payment before financing charges.

Our methodology appears reasonable in several ways. The most common combination of parameters we find is $\$\mu = \$25$ and $\theta\% = 1\%$ and the most common $\theta\%$ is 1% which is in line with the CFPB's credit card agreement database. Using this methodology, we estimate mean financing charges of \$211 in 2012 which is close to prior research (e.g., Agarwal et al., 2015b, 2023b) using regulatory datasets with different samples and time periods.[36] Figure 1.3 Panel A shows we find a hump shape in financing charges by credit score as found in prior research (e.g., Nelson, 2023) and also find financing charges being higher for accounts revolving debt than those transacting debt: these findings are despite our methodology not using this information. If actual payments are observed, researchers potentially can add additional assumptions to this methodology to separate financing charges into fees and interest, and also estimate effective interest rates.

Methodology for Predicting Profitability

Using data up to December 2012, we predict account-level outcomes ($Y_{i,2012+j}$) for profitability and its component parts over different time horizons (j) up to ten years. This exercise replicates the problem of a lender evaluating which accounts (i) to attempt to acquire and how much profit they can expect to generate from their own accounts. For installment loans,

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36. Our estimates would not be expected to exactly line up given those studies examine different time periods, different samples, and different datasets which may have different variable definitions. Agarwal et al. (2015b) finds mean annualized financing charges of \$223 (April 2008 to December 2011). Agarwal et al. (2023b) finds mean financing charges of \$17.02 in March 2019: which is \$204 annualized.

the ten-year time horizon usually exceeds the loans' scheduled lifetime, which is typically eight years or less. The ten-year horizon covers the lifetime of most credit cards: only 15% of active credit cards in December 2012 remain active (open, not severely delinquent, and without persistent zero balances) by December 2022 (Appendix Figure 1.29).

We show credit card results for lenders who *Always* share actual payments information, as these are the firms we observe outcomes data for their card's lifetime. We show our results are robust to including *Stoppers* (who stop sharing actual payments information) for whom we need to impute spending (classifications described in Section 1.2.2). We cannot evaluate the value of actual payments information for lenders that never share this information (*Nevers*).[37] For *Always* we observe actual payments, so we can estimate spending and interchange net of rewards for all years. For *Stoppers* we observe spending for 2013, but not in subsequent years, and therefore impute spending in years 2014 to 2022 based on the 2013 values, and impute it as zero if the card's statement balances is zero.

Our baseline model in Equation 1.8 uses the vector $(X'_{i,2012})$ of predictors observed in December 2012. These include indicators for 100 credit score quantiles, and credit score interacted with other account-level information: up to three years of balances, delinquency, utilization rates, estimated financing charges, card tenure, and credit limits. For installment loans, we interact credit score with: origination amount, scheduled loan duration, and scheduled payment amount.[38] $X'_{i,2012}$ does not include actual payments information.

$$Y_{i,2012+j} = X'_{i,2012}\beta + \varepsilon_{i,2012+j} \tag{1.8}$$

The comparison model in Equation 1.9 takes the baseline model and adds information on up to three years of actual payments information $(Z'_{i,2012})$ to the set of predictors. These predictors include interactions and combinations with other variables such as credit score

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37. While we cannot evaluate actual payments information for lenders that never share this information, Appendix Figure 1.21 compares the baseline prediction of financing charges net of charge-offs.

38. We examined different specifications of predictors and use the one that best predicts out-of-sample. As a result the specifications differ for auto loans, credit cards, and unsecured loans.

and balances. In the case of credit cards, these additional predictors include measures of spending and revolving debt, both derived from actual payments information.

$$Y_{i,2012+j} = X'_{i,2012}\beta + Z'_{i,2012}\lambda + \varepsilon_{i,2012+j} \tag{1.9}$$

We predict profitability using OLS regressions trained on half the data and test its performance on the remaining half. We evaluate the value-add of actual payments information for predicting profitability using the out-of-sample R^2 for the baseline and the comparison model.

Results Predicting Profitability

Table 1.1 shows the out-of-sample R^2 from models without and with actual payments information to predict lifetime (ten-year) profits on credit cards, auto loans, and unsecured personal loans. We find actual payments information increases the ability to predict lifetime profits for credit cards R^2 from 0.1919 to 0.2003: a 4.4% increase.[39] In contrast, actual payments information does not substantially improve the ability to predict lifetime profits for either auto loans or unsecured personal loans. Actual payments information may have been expected to increase profits by improving the prediction of prepayment on installment loans; however, we find little evidence of this.[40] This empirical finding helps to explain why installment loans are willing to keep sharing actual payments information after Trended Data is launched: doing so does not pose a competitive threat enabling competitors to target their profitable customers. While credit cards have a revenue stream directly dependent on

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39. Appendix Figure 1.22 shows results hold for predicting profitability over one- to ten-year time horizons. Our calculation of low predictability of credit card profitability complements the wide cross-sectional dispersion in credit card borrowing costs previously found in Stango and Zinman (2016).

40. Actual payments information may have limited ability to predict prepayment in auto loans as these loans may be prepaid when a car is sold or traded in. In such cases, the actual payments information would only not be equal to the scheduled amount at the end of the agreement. Adverse selection due to default risk is present across consumer credit markets (e.g., Ausubel, 1991; Edelberg, 2004; Adams et al., 2009; Crawford et al., 2018) so also appears unlikely to explain differential sharing decisions for credit cards compared to installment loans.

spending – interchange – that actual payments information can be used to target, installment loans do not have an analogous revenue stream and so Trended Data is less of a competitive threat.[41]

How does actual payments information increase the ability to predict the components of credit card profitability? Figure 1.4 shows the out-of-sample R^2 for predicting interchange net of rewards (Panel A) and financing charges net of charge-offs (Panel B), both over one-year to ten-year horizons. Panel C shows the profitability components together for the ten-year lifetime horizon. We also evaluate this in Table 1.2 by comparing the realized portfolio values of the top ranked 100,000 accounts when ranking accounts by the out-of-sample predictions made with and without using actual payments information. This shows actual payments increases the net present value of lifetime profits by 2.7%. Our prediction results may be a lower bound since improved predictability would also be expected to reduce acquisition costs by enabling lenders to send pre-selected credit card offers that more closely align to consumer behaviors and so may yield improved solicitation response rates.[42]

Actual payments information substantially improves the prediction of interchange net of rewards. Actual payments information increases the R^2 for predicting interchange net of rewards over a one-year horizon by 53% from 0.401 to 0.614 and over a ten-year horizon by 31% from 0.129 to 0.169 (Figure 1.4 Panel A). Table 1.2 shows observing actual payments information increases the portfolio value of interchange net of rewards over a one-year horizon by 24% ($42 mean increase) and over a ten-year horizon by 13% ($63). Results are qualitatively similar for *Always* + *Stoppers*.[43] We interpret these results as showing how ob-

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41. In addition pre-selected offers using credit reports are a common acquisition channel for credit cards but less natural for auto loans (as a loan is typically taken out at dealerships) and unsecured personal loans (which are long-term products taken out less frequently).

42. If there is cross-subsidization, then greater prediction may also enable some lenders to acquire low risk but expected to profitable consumers to lower the risk of their overall credit card portfolio enabling them to lend more to higher expected profit but riskier consumers and so generate higher overall profits that are also more stable over the business cycle.

43. Actual payments information increases R^2 by 49% from 0.415 to 0.619 on a one-year horizon, where spending is observed for both *Always* and *Stoppers*, and by 33% from 0.181 to 0.241 on a ten-year horizon, where spending post-2013 is imputed for *Stoppers*. Portfolio values increase by 25% over a one-year horizon

serving actual payments information improves the ability of lenders to target high-spending accounts generating high interchange net of rewards.

Actual payments information also improves the prediction of financing charges net of charge-offs. Actual payments information increases the R^2 for predicting financing charges net of charge-offs over a one-year horizon by 2.1% from 0.217 to 0.222 and over a ten-year horizon by 4.2% from 0.192 to 0.200 (Figure 1.4 Panel A). Table 1.2 shows observing actual payments information increases the portfolio value of financing charges net of charge-offs over a one-year horizon by 3% ($14 mean increase) and over a ten-year horizon by 1% ($140). Results are similar using $Always + Stoppers$.[44] These predictive increases are smaller for financing charges net of charge-offs than for interchange net of rewards; however, as the former is a larger component of profits even small percentage uplifts are quantitatively important in levels.

Our results are likely to underestimate the importance of interchange revenue for three reasons. First, we assume a flat 0.5% margin of interchange net of rewards, however, rewards cards—most commonly at higher credit scores where high spenders are—have higher margins (Agarwal et al., 2023b). Second, interchange net of rewards may increase further if lenders are able to convert an account from a standard card to a rewards card as doing so causes higher spending and so generates more interchange revenue (e.g., Agarwal et al., 2023a,b; Han, 2023) and also more revenue via annual fees.[45] Third, our results do not include lenders that do not share actual payments information. In the next section, we show such lenders appear to have higher spending accounts and so would generate more interchange revenue.

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and 18% over a ten-year horizon.

44. Actual payments information increases R^2 by 1.3% from 0.257 to 0.261 on a one-year horizon, and by 2.2% from 0.204 to 0.209 on a ten-year horizon. Portfolio values increase by $< 1\%$ over a one-year horizon and 1% over a ten-year horizon.

45. Gelman and Roussanov (2023) shows consumers exogenously receiving a new credit card, without any rewards or promotion, causes higher total credit card spending and attribute this to mental accounting.

1.5   Selection in Credit Card Lenders Sharing Information

In this section, we explore the selection of credit card lenders by their sharing decisions to better understand lenders' motivations for no longer sharing information. The decision of credit card lenders to share actual payments information is non-random: *Nevers* (who never share this information), compared to *Always* (who always share this information) or *Stoppers* (who stop sharing this information), have portfolios with higher mean credit scores and credit limits, lower mean utilization rates, higher mean and higher standard deviation card tenure and statement balances (Appendix Table 1.8).

1.5.1   Default Risk

Can default risk explain differential information sharing decisions across credit card lenders? Adverse selection due to default risk is well-documented in prior literature in the credit card market (e.g., Ausubel, 1991; Agarwal et al., 2010b).[46] In our data, lenders that never share information (*Nevers*) have more creditworthy cardholders (mean 744) than *Always* or *Stoppers* (means around 720) (Appendix Table 1.8).

We condition cards on their default risk in December 2012 and examine whether these cards become delinquent (90+ days past due or 180+ days past due) at any point from January 2013 to December 2022. Default rates convexly decline in credit score as one would expect from non-linear models such logistic regressions. Default rates conditional on credit score are generally similar across lenders with different information sharing decisions (Appendix Figure 1.24). Given this result, default risk is *not* the primary reason for differential information sharing decisions across credit card lenders.

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46. Also see Jaffee and Russell (1976); Stiglitz and Weiss (1981); Stavins (1996); Ausubel (1999); Calem and Mester (1995); Calem et al. (2006); Adams et al. (2009); Karlan and Zinman (2009); Einav et al. (2012); Ambrose et al. (2016); Crawford et al. (2018); Blattner et al. (2023); DeFusco et al. (2022); Gupta and Hansman (2022); Matcham (2023); Nelson (2023).

1.5.2 Non-Default Behaviors

We next show how non-default credit card behaviors, after accounting for default risk, explain differential information sharing decisions across credit card lenders.[47] We present results in two ways. First, Table 1.3 shows the residualized means and standard deviations in cardholder behaviors. We residualize using OLS regressions of outcomes on values of credit scores and adding back population means to ease interpretation $(Y_i - \widehat{Y}_i + \bar{Y})$. Second, Figure 1.5 (as well as additional Figures in Appendix 1.9.8) shows the means and standard deviations in non-default behaviors for 50 quantiles of credit score where the quantile thresholds are defined globally and fixed across classifications of lenders (*Always*, *Stoppers*, *Nevers*).[48]

Revolving Behaviors

Table 1.3 shows the portfolios of lenders who stop sharing information (*Stoppers*) have 11% higher mean and 12% higher standard deviation residual revolving debt than those who keep sharing information (*Always*). Figure 1.5 Panel A shows the difference in means is only in the middle of the credit score distribution while Panel B shows this gap in standard deviations is present across the whole distribution.

These differences in revolving behavior translate into *Stoppers* having more profitable portfolios (Figure 1.3 Panel B). Financing charges net of charge-offs $(2012 - 2022)$ for *Stoppers* are 36% ($259) higher mean and 8% ($209) higher standard deviation than *Always* (mean $710, s.d. $2,691).

We do not observe revolving debt for the *Nevers*, and instead use statement balance as an observed but biased proxy for revolving debt. We find monotonicity (*Nevers* > *Stoppers* > *Always*) in means and standard deviations; however, this relationship only holds for below

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47. Examining non-default behaviors that do not go into the construction credit scores is conceptually similar to an unobservables test in Finkelstein and Poterba (2014).

48. We use this approach to present results because the distribution of credit scores is uneven with low density mass for a large number of low credit score values but a high density for particular high credit score values: quantiles display how 60% of cards are prime plus or superprime and a 38% of cards are superprime (CDFs in Appendix Figure 1.23).

median credit scores (Appendix Figure 1.25). The other way we infer *Nevers*'s revolving debt is comparing our *Always+Stoppers* estimates to public revolving debt estimates for from the Federal Reserve Bank of Philadelphia. This implies the *Nevers* revolve a slightly higher share of balances and have more accounts revolving debt than *Stoppers* or *Always*.[49]

Spending Behaviors

Figure 1.5 Panel D shows substantial dispersion in spending conditional on credit score: showing spending is a second source of uncertainty lenders experience beyond default risk. Differences in spending behaviors residual of default risk across lenders appear to most clearly explain differential information sharing decisions: with adverse selection of lenders into sharing information. Higher spending is important to lenders' business models as it generates higher interchange revenue. In addition, high spending cardholders may also be more willing to pay high annual fees associated with rewards cards as for low spending customers the potential rewards benefits of such cards may not outweigh their annual fee cost. Table 1.3 shows *Stoppers*'s spending, residual of default risk, is 31% ($1,643) higher mean and 41% ($4,275) higher standard deviation than *Always* (mean $5,246, s.d. $10,345). Differences in standard deviations of spending between *Stoppers* and *Always* occur across the credit score distribution (Figure 1.5 Panel D) and differences for mean spending (Figure 1.5 Panel C) occur for prime, prime plus, and superprime segments (i.e. those that often contain transactors). Part of the reason for this standard deviation being so high is consumers often hold multiple credit cards and so credit card lenders are competing to be "top of wallet" – the main (or ideally only) card a consumer uses.[50]

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49. FR Y-14K data for Q4 2012 estimate revolving debt is 77% of balances and 71% of accounts revolve debt. Aggregating *Always* and *Stoppers* in our data, we estimate revolving debt is 73% of balances and 63% of accounts revolve debt. We caveat that FR Y-14K data covers lenders with over $100bn in assets with material credit card portfolios covering three quarters of the population of outstanding balances so it is not an exact like-for-like comparison.

50. Discussions with industry participants indicate a cardholder needs to spend at least $10,000 to $20,000 per year for several years to overcome their acquisition and other costs and become profitable on interchange revenue alone. Discussions mentioned how airline credit cards where profits are split between the airline

How does the spending of *Nevers* compare? *Nevers* have more cards held by high credit score consumers which would be, on average, expected to generate higher spending. We investigate this using a proxy for spending – change in statement balances conditional on being positive ($\tilde{\Delta} b_{i,t}$) – that we observe across *Always*, *Stoppers*, and *Nevers*. Equation 1.10 shows how this proxy measure is spending plus a non-random error term $\nu_{i,t}$ which is biased downwards as actual payments increase ($p_{i,t}$ can only be greater than or equal to zero) and is only zero if both payments ($p_{i,t}$) and financing charges ($r_{i,t} + f_{i,t}$) are zero or, by chance, net out at zero. This measure (residual of default risk) shows *Nevers* have a higher mean and higher standard deviation than *Stoppers*, who in turn have a higher mean and a higher standard deviation than *Always* (Table 1.3).[51] We also compare our estimates to public, population estimates of total market credit card spending being \$2.55 trillion in 2012 from the Federal Reserve Payment Study (conducted triennially).[52] If we calculate spending aggregating *Always* and *Stoppers* and multiply by their market share it would imply total spending of \$2.43 trillion and so indicates *Nevers*'s mean spending is higher than the rest of the market average.

$$
\tilde{\Delta} b_{i,t} \equiv
\begin{cases}
b_{i,t} - b_{i,t-1} \equiv s_{i,t} - \underbrace{p_{i,t} + r_{i,t} + f_{i,t}}_{\nu_{i,t}} & \text{if } b_{i,t} - b_{i,t-1} \geq 0 \\
\\
0 & \text{otherwise}
\end{cases}
\tag{1.10}
$$

Card Tenure

The longer a credit card is held for, the more private information a lender may hold, which they could use to extract information rents from the cardholder.[53] Holding a card for longer

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and the credit card provider (whereas with a lender's own-brand products there is no split) need to have long-duration contracts for it to be a worthwhile venture for the lender.

51. See Appendix Figures 1.26 and 1.27 for results by credit score.

52. The Federal Reserve statistics are the sum of (general-purpose) credit cards and retail (private label) credit cards. For this comparison we therefore include data on retail credit cards where actual payments are observed.

53. See Sharpe (1990); Rajan (1992); Petersen and Rajan (1994); Von Thadden (2004); Nelson (2023)

may indicate a consumer's switching costs have increased – potentially due to preferring that card to alternatives.

We document a new fact: card tenure varies across and within the credit score distribution as displayed in Figure 1.5 Panels E (mean card tenure) and F (s.d. card tenure). There's a clear pattern of adverse selection in information sharing decisions by card tenure. Table 1.3 shows *Nevers*'s card tenure, residual of default risk, has the highest means and s.d. (mean 136 months, s.d. 106 months) compared to *Stoppers* (mean 98 months, s.d. 76 months) and *Always* (mean 71 months, s.d. 74 months). Figure 1.5 Panel E shows this pattern exists in means across the distribution of credit scores, and in the differences in s.d. between *Nevers* and *Always+Stoppers*.

Substantial differences in card tenure have important broader implications for how to measure credit card profitability. Traditionally, credit card profits have often been measured in empirical economic research on a per-period basis using data on realized profits covering a few years (e.g., Agarwal et al., 2015b) or a single point-in-time (e.g., Agarwal et al., 2023b). Given that we find different segments of the credit score distribution, cards within these segments, and different credit card lender portfolios have substantial variation in card tenures, the lifetime profitability of credit cards may differ from the profitability over a short, fixed horizon. For example, consider credit card A is held for five years and generates $100 per year in profits whereas credit card B is held for ten years and generates $80 per year in profits. Over a five year (or less) horizon card A appears more profitable: generating $100 more than card B. However, over these cards' lifetimes card B is more profitable: generating $300 more than card A.

This lifetime perspective can also help to explain an otherwise puzzling fact that credit card lenders lend to and heavily concentrate marketing towards high credit score consumers despite those consumers frequently being transactors generating little-to-no revenue from financing charges (Figure 1.3 Panel B).[54] 60% of credit cards accounts are held by high

54. Similar aggressive competition for low-risk consumers is also observed in other markets with adverse

credit score consumers (Appendix Figure 1.23), the overwhelming majority of marketing offers are sent to high credit score consumers (CFPB, 2021), and such offers are primarily for rewards cards (CFPB, 2015). High credit score transactors' longer tenure can be mean their accounts are NPV > 0 on interchange – especially if they can find high-spenders and get these to take out rewards cards with higher profitability margins (e.g., Agarwal et al., 2023a,b; Han, 2023) – and also avoids future acquisition costs.[55]

The portfolios of the credit card lenders remaining in the market for sharing actual payments information are the worst (the "lemons" in Akerlof, 1970) residual types on multiple dimensions: they have lower residual tenure, spending, statement balances, revolving debt, and financing charges net of charge-offs. Thus, the market for sharing information is adversely selected.[56] Our results are consistent with *Nevers* and *Stoppers* holding information rents over other lenders: as incumbent lenders they are especially exposed to actual payments information in Trended Data being used for marketing targeted to their large number of low-risk, long-tenure, high-spending cardholders that generate interchange revenue. By not sharing information, incumbent lenders with market power from informational rents make it more difficult for competitors to successfully target their profitable customers by raising their competitor's costs of acquiring new consumers.[57]

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selection such as healthcare where it leads to higher an "(un)-natural monopoly" where a small number of firms profitably operate with high mark-ups (Kong et al., 2023).

55. This explanation is in line with industry statements. For example, Capital One's US Head of External Affairs states "Even those customers who pay in full every month are profitable and desirable customers for Capital One and other issuers across the industry." It also explains why credit card lenders lobby against legislation such as the Credit Card Competition Act that would be expected to restrict credit card interchange revenue. Appendix Figure 1.31 shows how interchange net of rewards increases with card tenure and is noticeably higher for the high credit score segments. Furthermore, given high credit score transactors are very low risk there is little-or-no risk-adjustment required.

56. The greater dispersion among the part of the market not willing to share information may appear reminiscent of Hendren (2013) who finds greater dispersion explains which consumer segments are served by which insurance markets. However, these are different. In Hendren (2013) consumers are unable to access insurance because the dispersion from private information makes them unprofitable. Whereas in our case the dispersion appears to be for a profitable segment where lenders hide their profitable consumers to prevent targeting by their competitors.

57. Industry data from R.K.Hammer (Appendix 1.10) shows the mean costs of acquisitions increasing over time because more solicitations are required to successfully acquire each new account. A separate example of incumbent lenders with market power not sharing information is UK banks restricting non-banks' access

1.5.3   Effect of Trended Data on New Account Openings

Analyses in the previous section suggest that Trended Data was expected to be a competitive threat to profitable incumbent lenders by enabling their competitors to targeted marketing to acquire profitable consumers. In this section we provide evidence that is consistent with this hypothesis. In subsection 1.5.3 we explain our research design based on heterogeneous consumer exposure to Trended Data and then in subsection 1.5.3 we show our results.

Research Design

We identify the causal effect of Trended Data on new credit card openings by creating a measure of heterogeneous consumer exposure (Equation 1.11) to this innovation. A consumer (i) holds credit cards ($c \in \{1, ..., C\}$) with a furnisher (F_c) and each card has a statement balance ($b_{i,c}$). Our exposure measure ($EXPT_i$) shows the proportion of a consumer's 2012 credit card statement balances held with lenders who share actual payments information. The higher the share of balances held with furnishers where actual payments information is shared in 2012, the more information is revealed to the market on a consumer's behavioral type (e.g., spending and revolving behaviors) by Trended Data's introduction.

$$EXPT_i \equiv \frac{\sum_c 1\{F_c \in \text{Sharers}\} \times b_{i,c}}{\sum_c b_{i,c}} \tag{1.11}$$

We use this exposure measure to estimate the difference-in-differences with varying treatment intensity equation shown in Equation 1.12. We estimate an OLS regression with consumer fixed effects (γ_i) and year-quarter fixed effects (γ_t) and cluster standard errors at the consumer-level. Our parameters of interest are δ_τ which are the coefficients on the inter-

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to current account turnover (CATO) data that is used as a proxy for income. In the US, lenders do not share information on defaults under 30 days past due (Nelson, 2023) which may be partially to prevent competitors learning about consumers who profitably incur late fees without defaulting. In both the UK and US, information on new credit application searches and the longest duration a credit account is held for is shared and is factored into credit scores. Such an information design is beneficial for incumbent lenders as it may deter profitable consumers from searching and switching to lenders' competitors as such actions would be expected to adversely impact their credit score.

action between our exposure measure ($EXPT_i$) and year-quarter indicators (D_τ) after τ quarters where our omitted group ($\tau = -1$) is Q4 2012 before Trended Data's launch. Our outcome of interest ($Y_{i,t}$) is whether the individual has any new credit card openings – an indicator of the competition for consumers whose information was about to be revealed. We use quarterly data from Q1 2011 to Q4 2016 and restrict to a balanced panel of 0.51 million consumers with $0 < EXPT_i < 1$ who hold two cards with positive balances in 2012.[58] Figure 1.6 Panel A shows the CDF of the exposure measure is smooth with mean 49.5% and median 49.2%.

$$Y_{i,t} = \sum_{\tau \neq -1} \delta_\tau \left(D_\tau \times EXPT_i \right) + \gamma_i + \gamma_t + \varepsilon_{i,t} \tag{1.12}$$

Empirical Results

Our results show that consumers who are more exposed to Trended Data are more likely to open new credit card accounts for up to two years after introduction (Figure 1.6 Panel B). In 2013 Q4, we estimate going from 0% to 100% exposure causes a 0.42 percentage point (95% C.I. 0.22 to 0.61) increase in credit card openings. This is a 13% increase relative to the Q4 2012 mean 3.22% rate of opening a new credit card in a quarter. We interpret this average increase as indicating the potential of innovations, such as Trended Data, to reduce adverse selection and to increase credit access. After two years, as the unraveling occurs, the effect dissipates to be insignificant from zero.

1.5.4   Discussion

Given our results we now discuss whether the unraveling of information sharing is best understood as a coordination failure – a natural explanation for the phenomenon we document and one that some lenders themselves suggest explains their own behavior. If this were the result of a prisoner's dilemma, the only Nash equilibrium would be for all lenders not to share

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58. Appendix 1.32 shows robustness to including consumers with three cards.

information, even if all lenders would be better off by coordinating. In games with multiple equilibria, there may also be a coordination failure leading lenders to a pareto-dominated equilibrium, even though they would be better off coordinating to reach an alternative equilibrium.

Unraveling does not appear to simply be a coordination failure. An industry body – the Consumer Data Industry Association (CDIA) – exists to facilitate and coordinate sharing but was unable to prevent the unraveling or undo it in the nine years since, even as it successfully coordinates the sharing of many other types of information. Further evidence comes from the fact that at least two large credit card lenders have never shared this information, even before Trended Data (CFPB, 2023b).[59] These lenders' responses to the CFPB are consistent with them considering that the costs of sharing information outweigh the benefits (CFPB, 2023b).[60]

Our empirical evidence indicates lenders have heterogeneous payoffs from sharing information and Trended Data made not sharing information a dominant strategy for some incumbent lenders. Trended Data changed the payoffs of sharing information: it reduces a lender's private information and increases the risk of its profitable customers being targeted by existing competitors or new entrants.[61] The only lenders willing to share information are those with few high-quality accounts at risk of being targeted (i.e., without market power). They may either be indifferent about sharing or they may share information for other reasons: incentivizing positive consumer behaviors, technological benefits, not-profit motives, or a lack of sophistication.[62] Overall we view the lack of information sharing as a financial

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59. Although we note it remains possible for a coordination failure to only exist between the lenders who stopped sharing information, we view this as an unlikely explanation given the CDIA's existence.

60. When CFPB (2023b) asked lenders' for their rationale for not sharing information, one of these said "Not required to do so. Not consistently furnished nor adequately studied" and another said "Not required, furnishing is voluntary. Doesn't believe cost of furnishing is worth it".

61. A reduction in adverse selection can increase entry (e.g., Dell'Ariccia et al., 1999) and reduce the incumbent's voluntary sharing of information (e.g., Bouckaert and Degryse, 2006).

62. Our empirical findings on the worst residual types being the ones sharing information are consistent with a different domain: investors sharing information. Goldstein et al. (2023)'s theory explains why less informed investors non-reciprocally share information with more informed investors as this reduces the latter's

friction that maintains the status quo levels of both information asymmetry and competition in the market.

1.6 Effects of Mandating Information Sharing: Evidence from Credit Card Limits

Previous sections of this paper document the breakdown of voluntary information sharing and examine the reasons and implications of this event. The natural next question is: what would happen if lenders were mandated to share information? As actual payments information has not been mandated, we instead learn from a prior historical event: the Federal Trade Commission (FTC) mandating lenders to share information on credit card limits. Not sharing credit limit information makes consumers *appear* more utilized and higher risk than they actually are. Such strategic withholding of information benefits the incumbent lender as it makes it harder for consumers to get competitive credit offers from other lenders.[63] In the 1990s, credit limit information was commonly not shared but a combination of regulatory pressure and credit reporting agencies threatening to limit access to any of their data unless lenders shared credit limit information resulted in most, but not all, lenders sharing this information by the early 2000s (Hunt, 2005).[64] The FTC mandate results in the remaining lenders also sharing this information.

1.6.1 Research Design

We produce causal estimates of the effects of mandating sharing of credit card limit information using a difference-in-differences design with varying treatment intensity. In November

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price impact as it can trade less aggressively on its own information. While the more informed investors do not share information as this would reduce their private informational advantage and profits.

63. Giannetti et al. (2017) finds in Argentina incumbent banks strategically downgraded high-quality firms or entrepreneurs in their public credit registry before such information was released to their competitors.

64. From discussions with industry, we understand it would not be credible for credit reporting agencies to threaten to shut off credit card lenders' access to credit bureau data unless they share actual payments information.

2011 ($t = 0$), we observe a small number of lenders start sharing credit card limits information on consumers' credit card accounts (Appendix Figure 1.33).[65] Credit card limits are important information as 20 to 30% of a consumer's credit score is determined by their credit utilization: credit card statement balance divided by credit card limit.[66]

We exploit an institutional detail of how credit card utilization is calculated when credit limits are not shared to produce a consumer-level measure of heterogeneous exposure to lenders' decision to start sharing information.[67] We use variation in how much information is revealed by calculating consumer-level (i) heterogeneous exposure $EXPL_i = \frac{r_i - h_i}{r_i}$ as the difference between the *revealed* credit limits ($r_i \equiv \sum_c r_{i,c}$) and credit limits that could be *inferred* based on information observed prior to the limits being revealed ($h_i \equiv \sum_c h_{i,c}$). For each of a consumer's credit cards (c) we calculate $r_{i,c}$ as the credit limits shared in October 2010 and, for accounts not sharing this information, we use the November 2010 limit. When a credit card account does not share the credit limit information, utilization is calculated using the highest balance historically recorded on the account, which is then used as an input into credit scores (Hunt, 2005). Therefore, for each credit card, we calculate $h_{i,c}$ as the credit limits shared in October 2010 and, for accounts not sharing this information, we use the highest balance historically recorded in October 2010. We then aggregate these card-level calculations to produce a consumer-level exposure measure.

Figure 1.7 Panel A shows the distribution of our exposure measure is smooth with a

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65. We do not observe an increase in credit card limit information sharing on July 2010 when the policy becomes effective but observe an increase in November 2011 and a smaller one in 2013 (Appendix Figure 1.33 Panel A). We therefore expect the CFPB's inception in July 2011 led to these rules being enforced. In another context, Wang and Burke (2022) show payday lending regulations did not have effects when enacted but only had effects when enforced.

66. Approximately 20% for VantageScore and 30% for FICO. Credit utilization may also be measured on revolving credit lines such as retail cards and home equity lines of credit for those with such accounts.

67. Sources of variation in US credit reports are mainly concentrated on riskier subsamples of the population, exploiting the removal of negative information such as on bankruptcy (e.g., Musto, 2004; Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023), medical debts in collections (e.g., Batty et al., 2022), public records (e.g., Fulford and Nagypál, 2023), defaults (e.g., Blattner et al., 2023), or the addition of information about natural disasters (e.g., Guttman-Kenney, 2023). See Gibbs et al. (2024) for a review of credit reporting data.

mean of 17% and median of 14%. A higher exposure value means a consumer's credit limits are higher than historical data shared would indicate. In such cases, revealing a consumer's credit card limit information is expected to lower their utilization, increase their credit scores, and increase their credit access.[68]

We use this exposure measure to estimate the difference-in-differences with varying treatment intensity equation specified in Equation 1.13. We estimate an OLS regression on a balanced panel of 1.09 million consumers with consumer (γ_i) and year-quarter (γ_t) fixed effects and with standard errors clustered at the consumer-level.[69] Our parameters of interest are δ_τ which are the coefficients on the interaction between our exposure measure ($EXPL_i$) and year-quarter indicators (D_τ) after τ quarters where the omitted group is the quarter before information revelation.

$$Y_{i,t} = \sum_{\tau \neq \text{-}1} \delta_\tau \Big(D_\tau \times EXPL_i \Big) + \gamma_i + \gamma_t + \varepsilon_{i,t} \qquad (1.13)$$

1.6.2   Empirical Results

Figure 1.7 Panel B shows moving from 0% to 100% exposure significantly increases credit scores by 22.6 points (95% C.I. 22.4, 22.9) and this effect is persistent but declines in magnitude over time. This effect size can be evaluated relative to the 776 baseline mean.

How does this information revelation affect credit access and competition? We evaluate this by considering the role of inside and outside lenders with different information sets (e.g., Petersen and Rajan, 1994, 1995; Schenone, 2010; Sutherland, 2018). The inside lenders are lenders who start sharing credit limit information but already knew about their own consumers' credit limits and credit risks. The outside lenders already share credit limit information and potentially learn about the consumer from updating their priors with the

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68. This approach is conceptually similar to Liberman et al. (2020) and Foley et al. (2022a), who estimate predicted probabilities of default with and without information in Chilean credit reporting data.

69. Appendix 1.9.9 contains additional details on the sample.

data newly shared by the inside lenders.[70]

This change in credit score increases competition with switching from inside to outside lenders. Figure 1.7 Panel C shows moving from 0% to 100% exposure significantly decreases the rate of opening any new credit card with an inside lender in a quarter by 56% (estimate −1.16 percentage points, 95% C.I. −1.32 to −1.00 percentage points). For outside lenders, at the same time we find a 32% (estimate 2.35 percentage points, 95% C.I. 2.08 to 2.62 percentage points) increase in the rate of opening any new credit card and causes a significant 14% overall increase in the number of new cards opened (estimate 1.24, 95% C.I. 0.93 to 1.54).

Figure 1.7 Panel D shows the change in credit scores also significantly decreases the value of new credit card limits with an inside lender by 90% (estimate −$614, 95% C.I. −$715 to −$512 percentage points) and increases the value of new credit card limits with an outside lender by 48% (estimate $643, 95% C.I. $554 to $732). There is no significant overall increase in total new limits across inside and outside lenders combined (estimate $27, 95% C.I. −$108 to $162 relative to the baseline mean of $2,026) and therefore we expect outside lenders are attracting consumers through improved non-credit limit contract terms (e.g., lower interest rates, higher rewards).

We interpret our results as showing that the potential threat of increased competition explains why some lenders are reluctant to voluntarily share information, and as demonstrating that mandating information sharing can increase competition. This is important since the credit card market has persistently high returns on assets in excess of adjusting for risk (Appendix 1.9.3, and also see Ausubel, 1991; Agarwal et al., 2015b, 2018; Herkenhoff and Raveendranathan, 2023; Nelson, 2023). Therefore increasing competition to reduce mark-ups from informational rents may be a desirable policy.

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70. Outside lenders are measured with error; they may contain some portfolios of the inside lender using a different furnisher who already shared this information.

1.7 Conclusions

We document the fragility of information sharing. We show how, in the economically important and developed US credit card market, an innovation enabling targeting of profitable customers pushes incumbent lenders beyond their limit to voluntarily share information. This results in 165 million US consumers missing information about their credit card actual payments on their consumer credit reports. This missing information leads to mis-measurement of credit card behaviors and limits the ability of lenders to predict profitability and compete for profitable customers. Our results are consistent with the innovation being a particular competitive threat to more profitable incumbent lenders with market power from informational rents.[71] We then show how mandating sharing credit card information can increase competition. This evidence together supports a policy to mandate information sharing, which UK regulators are considering, as we expect it to reduce incumbents' informational rents and improve market efficiency by reducing information asymmetry.

In the process of understanding information sharing we reveal two new insights for understanding the credit card market: the importance of spending and card tenure. We show lenders face a second source of uncertainty separate from default risk: the amount of credit card spending generating interchange revenue. We document a new fact: credit card tenure varies across and within the credit score distribution. This fact indicates a need to evaluate credit card profitability over a card's lifetime and these two insights together help to un-

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71. Understanding the limits of information sharing due to the market power of incumbents – see Philippon (2015, 2019); Traina (2018); Grullon et al. (2019); De Loecker et al. (2020); Eeckhout and Veldkamp (2022) for studies of market power more broadly – may also inform on the limits of open banking *unless* it enables competitors to target incumbents' profitable customers. He et al. (2023) provides theory on the effects of open banking including showing the circumstances when it can leave consumers worse-off. Early empirical research into open banking adoption shows some consumer benefits (e.g., Babina et al., 2022; Nam, 2023). The UK was an early adopter of open banking but the potential competitive gains do not appear to have been realized: six years after its introduction fewer than 10% of UK consumers use it and the positions of incumbent lenders with market power appears little changed: Financial Times, 26 January 2023 and The (unmet) potential of Open Banking" Oxera report 4 July 2023. Open banking's effects may be limited if consumers remain with incumbents even when competitors offer improved terms. This may occur for a variety of reasons including privacy concerns of sharing information, concerns about the stability of FinTech lenders to lend to them, and behavioral frictions such as limited inattention which warrant research.

derstand how high credit score consumers can generate enough interchange net of rewards over their card's lifetime to be profitable to lend to. Lenders therefore want to acquire high-spending, long-tenure credit cardholders.

1.8   Figures and Tables

Figure 1.1: Coverage of Actual Payments Information in Consumer Credit Reports
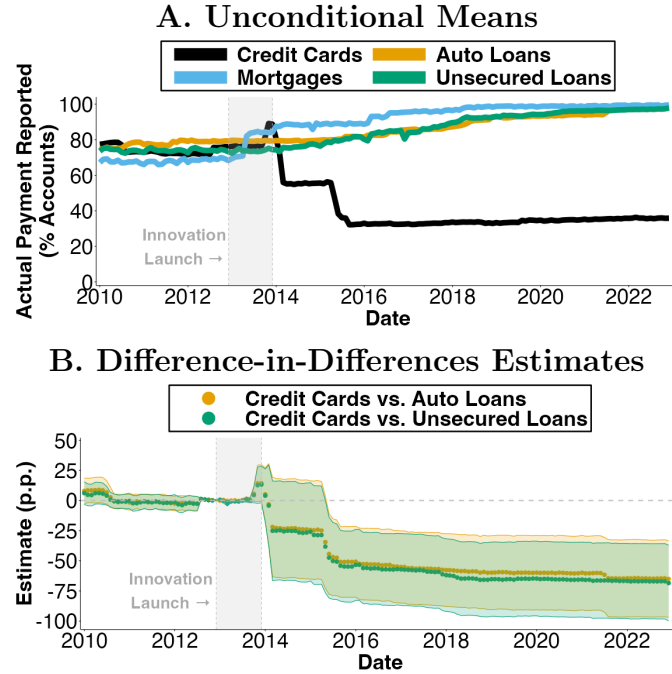
Notes: BTCCP data. 2013 is shaded in gray to denote the period when Trended Data was launched. Panel A shows, for each consumer credit product, the fraction of accounts in consumer credit reports sharing actual payment amounts. In the numerator of this calculation, accounts with actual payment amounts that are non-zero and non-missing are given a value of one, and accounts with zero or missing are given a value of zero. Both the numerator and the denominator of this calculation restricts to open accounts with non-zero balances and which have been updated in the last year. Panel B shows difference-in-differences estimates of sharing actual payment amounts for credit cards relative to auto loans (orange) and unsecured loans (green). Estimates are from OLS regression specified in Equation 1.1 on aggregated data with one observation per furnisher credit product per year-month (with weights applied to the number of accounts) with fixed effects for credit products and year month and December 2012 is the omitted group from the interaction between credit card indicator and year month indicator. Data is a balanced panel 2010 to 2022. 95% confidence intervals from standard errors clustered at the furnisher level. Panel B estimated for 6,068 (Credit Cards vs. Auto Loans) and 6,279 (Credit Cards vs. Unsecured Loans) furnisher portfolios.

Figure 1.2: Measuring Credit Card Behaviors Without Actual Payments (AP) Information

A. Revolving Debt

B. Spending

Notes: BTCCP data. R^2 from Equation 1.6 explaining credit card behaviors (Revolving Debt in Panel A and Spending in Panel B) at the account-level in December 2013. These can be evaluated relative to a benchmark $R^2 = 1$ (horizontal gray dashed line) if actual payments information is observed. Regression includes current statement balance, previous statement balance, the difference between these conditional on being positive, and indicators for non-zero current and previous statement balances. Each bar shows results of a separate regression for all credit scores (N = 4.006 million credit card accounts), and each credit score segment: subprime (the lowest credit score group, N = 0.546 million), near prime (N = 0.561 million), prime (N = 0.697 million), prime plus (N = 0.819 million), and superprime (the highest credit score group, N = 1.384 million).

Figure 1.3: Estimated Credit Card Financing Charges

A. Financing Charges (2012) By Credit Card Behaviors
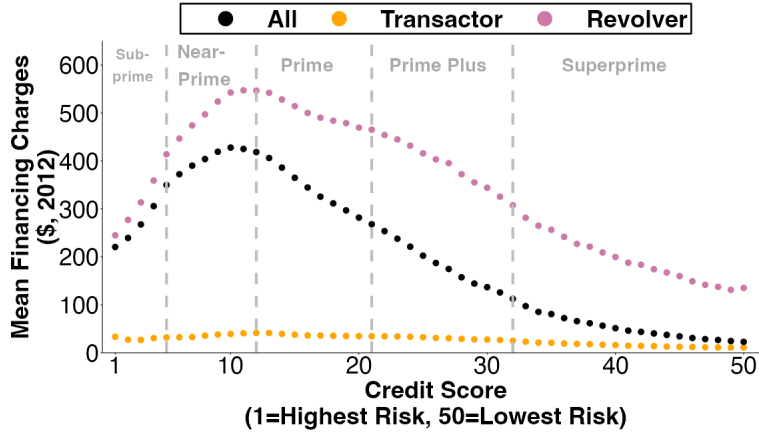
B. Financing Charges Net of Charge-Offs (2013 - 2022)
By Lenders' Actual Payments Information Sharing Decisions
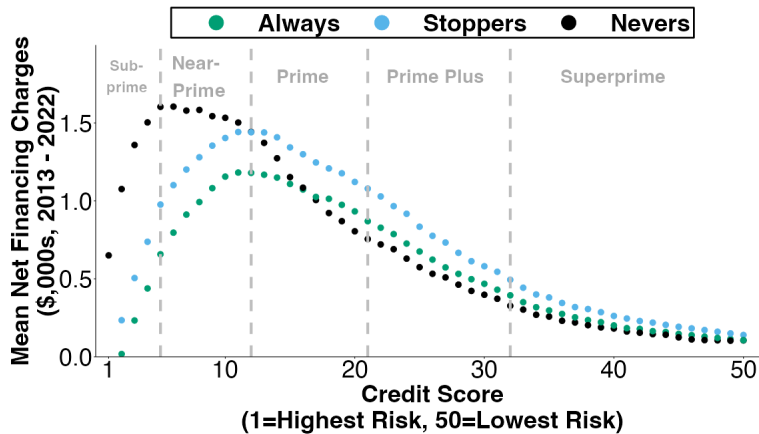
Notes: BTCCP data. Figures shows mean estimates conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Financing charges (the sum of interest and fees) are estimated as described in section 1.9.7. Panel A shows 2012 financing charges splitting by their 2012 card behaviors: transactors pay their statement balance in full, and revolvers pay less than their full statement balance. Panel B shows financing charges accumulated across 2013 to 2022 net of charge-offs over this same time horizon with results split by classifying credit card furnishers by their sharing of information on actual payments information as described in paper section 1.2.2 and Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.4: Marginal Value of Actual Payments (AP) Information for Predicting (A) Interchange Net of Rewards, (B) Financing Charges Net of Charge-Offs, (C) Lifetime Profits

A. Interchange Net of Rewards
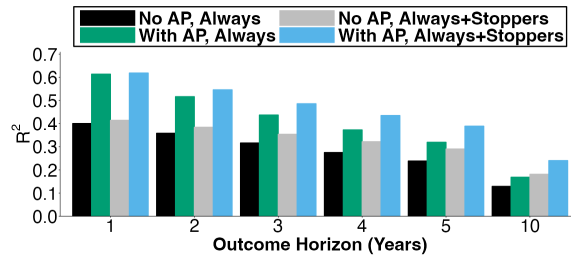
B. Financing Charges Net of Charge-Offs
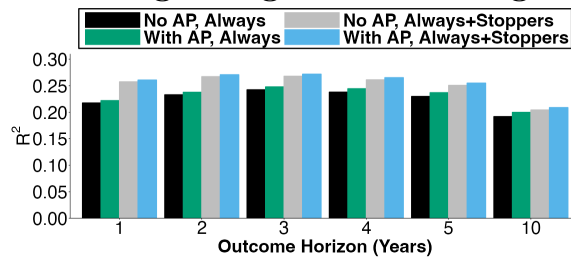
C. Lifetime Profits and its Components

Notes: BTCCP data. Figures use data to December 2012 to predict account-level credit card profitability where predictive performance is measured by out-of-sample R^2. Results are shown without (black, gray) and with (green, blue) actual payments information. Performance is shown for two samples: Always (black, green) and Always + Stoppers (gray, blue) as described in paper section 1.2.2 and Table 1.3 notes. Spending beyond a one year horizon is imputed for Stoppers but observed for Always. Panel A shows predictions of interchange net of rewards over one to ten year horizons. Panel B shows predictions of financing charges net of charge-offs over one to ten year horizons. Panel C shows predictions of lifetime profits and its components over a ten year horizon. Out-of-sample predictions from $N = 3.135$ million Always credit card accounts, and $N = 11.018$ million Always + Stoppers credit card accounts.

Figure 1.5: Credit Card Behaviors Conditional on Credit Score By Lenders' Actual Payments Information Sharing Decisions
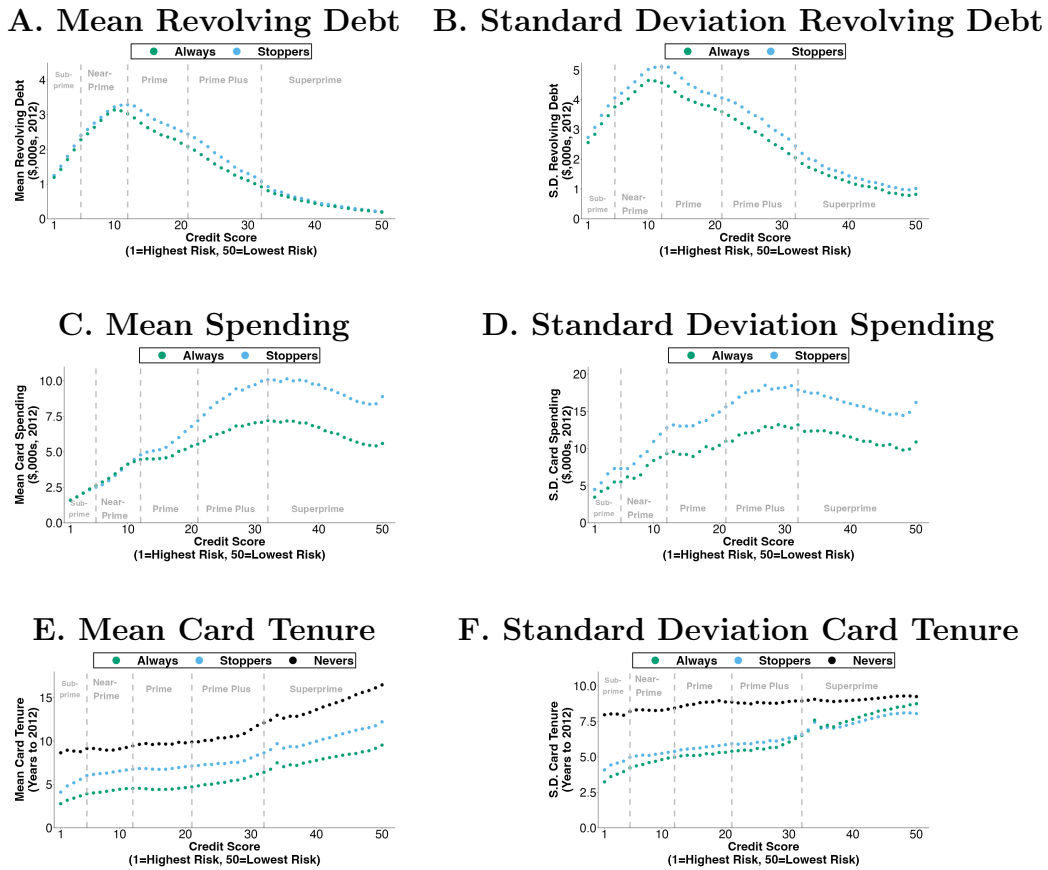
A. Mean Revolving Debt

B. Standard Deviation Revolving Debt

C. Mean Spending

D. Standard Deviation Spending

E. Mean Card Tenure

F. Standard Deviation Card Tenure

Notes: BTCCP data. Figure shows credit card behaviors (y-axis) conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Panels A, C, and E shows means and Panels B, D, and F show standard deviations. Results are split by classifying credit card furnishers by their sharing of information on actual payment amounts as described in paper section 1.2.2 and Table 1.3 notes. Revolving Debt and Spending is unobserved for Nevers as these do not share actual payments information required to calculate such behaviors. Credit card revolving debt is 2012 mean value and credit card spending is total 2012 value and both are shown in thousands of dollars. Card tenure is shown in years to 2012. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.6: Effects of Trended Data on Any New Credit Card Account Opening

A. CDF Exposure to Trended Data
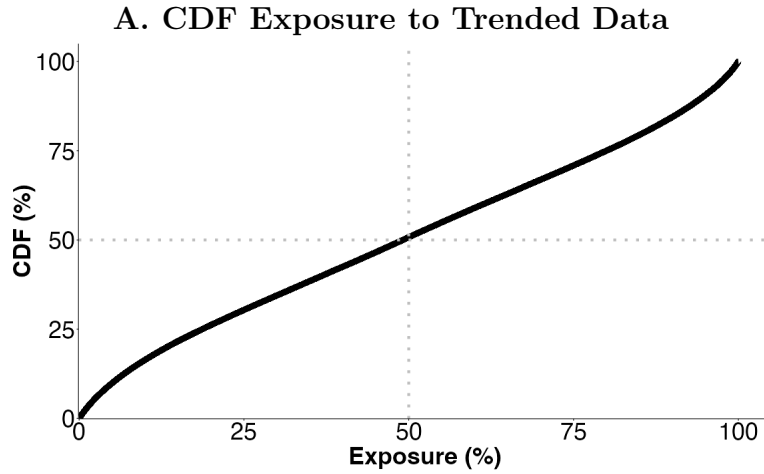
B. Estimates of Effects of Trended Data on Any New Credit Card Opening
(t-1 mean: 3.22%)
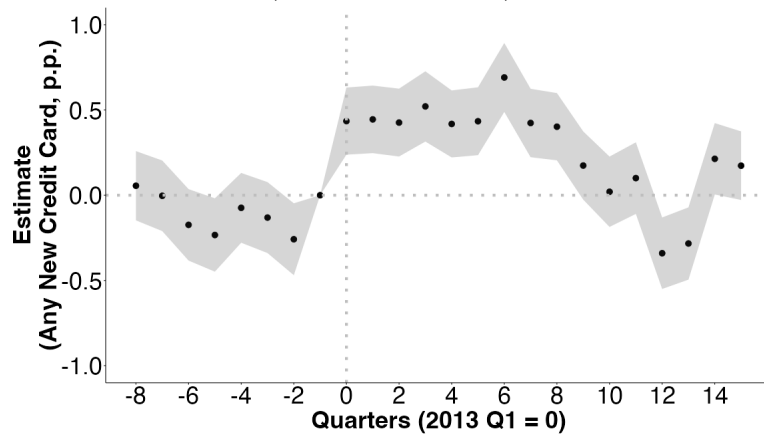
Notes: BTCCP data. Panel A shows CDF of exposure. Exposure is (pre-Trended Data) share of 2012 credit card balances held with furnishers who share actual payments information. Panel B shows difference-in-differences with varying intensity estimates in percentage points (p.p.) where the outcome is any new credit card account openings in a quarter. Difference-in-differences estimates are from balanced panel of 0.51 million consumers, with observations Q1 2011 to Q4 2016, with $0 < EXPT_i < 1$, and holding two cards both of which have positive balances in 2012. Estimates from OLS regression specified in Equation 1.12 with consumer and calendar year-quarter fixed effects and interaction term between exposure and calendar year-quarter where Q4 2012 is omitted category and standard errors are clustered at the consumer level with 95% Confidence intervals displayed.

Figure 1.7: Effects of Mandating Sharing of Credit Card Limit Information

A. CDF Exposure

B. Effects on Credit Score
(t-1 mean: 776)

C. Effects on Any
New Credit Card Opening
(t-1 means: 2.08% Inside, 7.23% Outside)

D. Effects on Value of
New Credit Card Limits ($,000s)
(t-1 means: $0.68 Inside, $1.35 Outside)
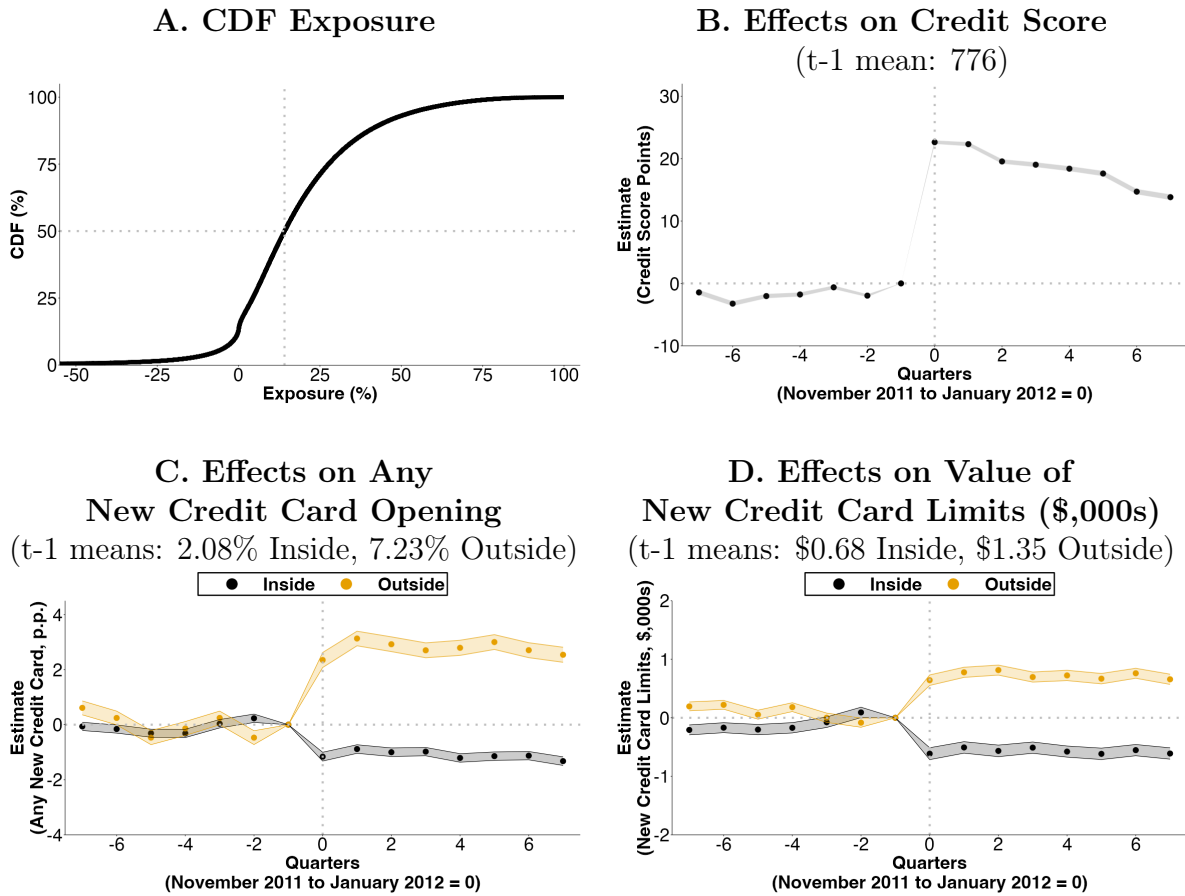
Notes: BTCCP data. Panel A shows CDF of exposure. Exposure is $EXPL_i = \frac{r_i - h_i}{r_i}$ the percentage difference between a consumers' observed credit limit and imputed credit limit. Panel B, C, D show difference-in-differences with varying intensity estimates in percentage points (p.p.) where the outcome is credit score, any new credit card account openings in a quarter, total value of of new credit card limits opened. Data is a balanced panel of 1.09 million consumers. Results are estimating OLS regression specified in Equation 1.13 with consumer and calendar year-quarter fixed effects and interaction term between exposure and calendar year-quarter where the quarter before information revelation is the omitted category. Standard errors are clustered at the consumer level with 95% Confidence intervals displayed.

Table 1.1: Marginal Value of Actual Payments Information for Predicting Lifetime Profitability on Credit Cards and Installment Loans (Auto Loans and Unsecured Loans)

	R^2 **Predicting Lifetime Profitability**		
Model	**Credit Cards**	**Auto Loans**	**Unsecured Loans**
1. Baseline	0.1919	0.1925	0.3508
2. Baseline + Actual Payments	0.2003	0.1928	0.3511

Notes: BTCCP data. Table uses data to December 2012 to predict lifetime profitability (to 2022) on credit cards, auto loans, and unsecured loans where performance is measured by out-of-sample R^2. Predictive performance is shown in a baseline compared to with adding actual payments information as predictors. $N = 3.135$ million credit card accounts, $N = 3.212$ million auto loan accounts, and $N = 0.436$ million unsecured loan accounts for lenders sharing actual payments information.

Table 1.2: Marginal Value of Actual Payments Information for Predicting Credit Card Profitability as Measured by Top-Ranked Predicted Portfolio Values

Sample	A. Interchange Net of Rewards (1 Year)	
	Baseline	Change With Actual Payments (%)
Always	$171	+24%
Always + Stoppers	$319	+25%

Sample	B. Interchange Net of Rewards (10 Years)	
	Baseline	Change With Actual Payments (%)
Always	$473	+13%
Always + Stoppers	$531	+18%

Sample	C. Financing Charges Net of Charge-Offs (1 Year)	
	Baseline	Change With Actual Payments (%)
Always	$1,391	+1%
Always + Stoppers	$2,600	+0%

Sample	D. Financing Charges Net of Charge-Offs (10 Years)	
	Baseline	Change With Actual Payments (%)
Always	$4,959	+3%
Always + Stoppers	$7,954	+1%

Sample	E. NPV (10 Years)	
	Baseline	Change With Actual Payments (%)
Always	$4,772	+2.7%
Always + Stoppers	$7,424	+1.3%

Notes: BTCCP data. Table uses data to December 2012 to predict components of credit card profitability. Table shows out-of-sample portfolio values from sorting predictions of each outcome and choosing top-ranked 100,000 accounts. Baseline shows mean account value ranking accounts by predictions without using actual payments information as predictors. Change with actual payments shows change in portfolio value relative to this baseline when instead ranking by predictions using actual payments information as predictors. Predictions from $N = 3.134$ million Always accounts (tested out-of-sample on $N = 3.135$ million accounts), and $N = 11.014$ million Always + Stoppers accounts (tested out-of-sample on $N = 11.018$ million accounts).

Table 1.3: Summarizing Selection (Residual of Credit Score) in Credit Card Portfolios By Lenders' Actual Payments Information Sharing Decisions

	Always	Stoppers	Nevers
Residual Tenure	71.0	97.6	136.5
(S.D.)	(73.8)	(75.5)	(106.0)
Residual Credit Limit	8,902.2	9,793.4	9,757.4
(S.D.)	(6,687.7)	(8,484.3)	(9,238.6)
Residual Statement Balance	2,004.3	2,294.8	2,576.5
(S.D.)	(3,405.9)	(3,842.4)	(4,130.1)
Residual Proxy Spending	2,486.2	2,800.2	3,286.2
(S.D.)	(4,036.2)	(4,987.6)	(6,998.7)
Residual Financing Charges	130.1	235.0	156.5
(S.D.)	(351.3)	(534.5)	(440.8)
Residual Revolving Debt	1,538.1	1,707.6	N/A
(S.D.)	(3,047.7)	(3,413.6)	
Residual Spending	5,228.3	6,896.5	N/A
(S.D.)	(10,257.8)	(14,345.9)	
Accounts (%)	18.2%	47.2%	31.5%
Statement Balances (%)	16.6%	46.8%	35.3%

Notes: BTCCP data. Table shows means (standard deviations in parenthesis) for residual credit card portfolio characteristics as of December 2012 where data is residual on values of credit score from an OLS regression and then the population means are added back to the means to ease interpretation. Card tenure is measured in months. Proxy spending is measured by change in balances conditional on being non-negative. Financing charges are estimated based on our methodology described in section 1.9.7. Results are split by classifying credit card furnishers by their sharing of actual payments information. The last two rows show the shares of the number of outstanding credit card accounts and the value of outstanding credit card statement balances by each type of furnisher. These data exclude furnishers who do not have at least 10,000 active credit cards (i.e. their portfolio is representative of least 100,000) in both December 2012 and in December 2015. Always are furnishers sharing actual payment amounts information for more than 75% of their active credit cards in both December 2012 and December 2015. Stoppers are furnishers sharing actual payments amounts information for more than 75% of their active credit cards in December 2012 and for less than 10% in December 2015. Nevers are furnishers sharing actual payment amounts information for less than 10% of their active credit cards in both December 2012 and December 2015. The remaining furnishers are Others excluded from the table: these are 3.1% of accounts and 1.3% of statement balances.

1.9 Appendix to *'Unraveling Information Sharing In Consumer Credit Markets"*

1.9.1 Credit Reporting Legal Requirements

This appendix shows credit reporting legal requirements based on relevant extracts (from Title 12 Chapter X CFR §1022.40-43 and Appendix E to Part 1022) of the Fair Credit Reporting Act (FCRA) amended by the Fair and Accurate Credit Transactions (FACT) Act.

PART 660 — DUTIES OF FURNISHERS OF INFORMATION TO CONSUMER REPORTING AGENCIES

§660.2 Definitions.

For purposes of this part and Appendix A of this part, the following definitions apply:

(a) **Accuracy** means that information that a furnisher provides to a consumer reporting agency about an account or other relationship with the consumer correctly:

  (1) Reflects the terms of and liability for the account or other relationship;

  (2) Reflects the consumer's performance and other conduct with respect to the account or other relationship; and

  (3) Identifies the appropriate consumer.

(e) **Integrity** means that information that a furnisher provides to a consumer reporting agency about an account or other relationship with the consumer:

  (1) Is substantiated by the furnisher's records at the time it is furnished;

  (2) Is furnished in a form and manner that is designed to minimize the likelihood that the information may be incorrectly reflected in a consumer report; and

(3) Includes the information in the furnisher's possession about the account or other relationship that the Commission has:

(i) Determined that the absence of which would likely be materially misleading in evaluating a consumer's creditworthiness, credit standing, credit capacity, character, general reputation, personal characteristics, or mode of living; and

(ii) Listed in section I.(b)(2)(iii) of Appendix A of this part.

§660.3 Reasonable policies and procedures concerning the accuracy and integrity of furnished information.

(b) **Guidelines.** Each furnisher must consider the guidelines in Appendix A of this part in developing its policies and procedures required by this section, and incorporate those guidelines that are appropriate.

Appendix A to Part 660—Interagency Guidelines Concerning the Accuracy and Integrity of Information Furnished to Consumer Reporting Agencies

The Commission encourages voluntary furnishing of information to consumer reporting agencies. Section 660.3 of this part requires each furnisher to establish and implement reasonable written policies and procedures concerning the accuracy and integrity of the information it furnishes to consumer reporting agencies. Under § 660.3(b), a furnisher must consider the guidelines set forth below in developing its policies and procedures. In establishing these policies and procedures, a furnisher may include any of its existing policies and procedures that are relevant and appropriate. Section 660.3(c) requires each furnisher to review its policies and procedures periodically and update them as necessary to ensure their continued effectiveness.

I. Nature, Scope, and Objectives of Policies and Procedures

(a) **Nature and Scope.** Section 660.3(a) of this part requires that a furnisher's policies and procedures be appropriate to the nature, size, complexity, and scope of the furnisher's activities. In developing its policies and procedures, a furnisher should

consider, for example:

(1) The types of business activities in which the furnisher engages;

(2) The nature and frequency of the information the furnisher provides to consumer reporting agencies; and

(3) The technology used by the furnisher to furnish information to consumer reporting agencies.

(b) **Objectives.** A furnisher's policies and procedures should be reasonably designed to promote the following objectives:

(1) To furnish information about accounts or other relationships with a consumer that is accurate, such that the furnished information:

    (i) Identifies the appropriate consumer;

    (ii) Reflects the terms of and liability for those accounts or other relationships; and

    (iii) Reflects the consumer's performance and other conduct with respect to the account or other relationship;

(2) To furnish information about accounts or other relationships with a consumer that has integrity, such that the furnished information:

    (i) Is substantiated by the furnisher's records at the time it is furnished;

    (ii) Is furnished in a form and manner that is designed to minimize the likelihood that the information may be incorrectly reflected in a consumer report; thus, the furnished information should:

    (A) Include appropriate identifying information about the consumer to whom it pertains; and

(B) Be furnished in a standardized and clearly understandable form and manner and with a date specifying the time period to which the information pertains; and

(iii) Includes the credit limit, if applicable and in the furnisher's possession;

(3) To conduct reasonable investigations of consumer disputes and take appropriate actions based on the outcome of such investigations; and

(4) To update the information it furnishes as necessary to reflect the current status of the consumer's account or other relationship, including, for example:

(i) Any transfer of an account (e.g., by sale or assignment for collection) to a third party; and

(ii) Any cure of the consumer's failure to abide by the terms of the account or other relationship.

1.9.2 Consumer Credit Markets

Figure 1.8: Consumer Credit Market Sizes

A. Open Accounts (millions)   B. Outstanding Balances ($ trillions)
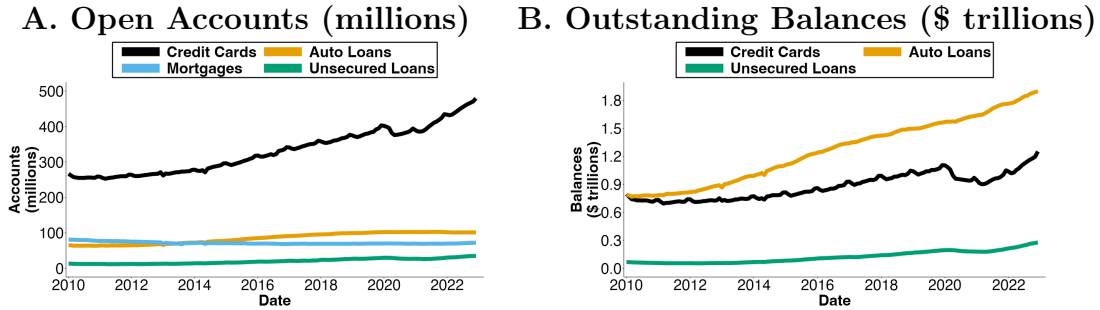
Notes: BTCCP data. Both panels restrict to open accounts with non-zero balances and which have been updated in the last year. Mortgage balances are excluded from Panel B due to their substantially larger balances.

Table 1.4: Consumer Credit Product Comparison

	Auto Loans	Unsecured Loans	Credit Cards
Duration	Fixed-Term		Open-Ended
Revenue Streams	Financing Charges (Interest, Fees)		Financing Charges (Interest, Fees), Interchange
Uncertain Behaviors	Delinquency, Prepayment		Delinquency, Revolving Amount & Duration, Spending
Collateral	Secured		Unsecured

Notes: Financing charges is the sum of interest and consumer fees. The most common consumer fees are late fees. Other consumer fees include annual card fees, over credit limit, and foreign exchange fees. Interchange income is the amount of transaction fees credit card lenders receive from merchants when a consumer spends on their credit card.

1.9.3   Credit Card Industry Statistics

Figure 1.9: Credit Card Profitability

A. Return on Assets (ROA) and its Components (1983 - 2022)
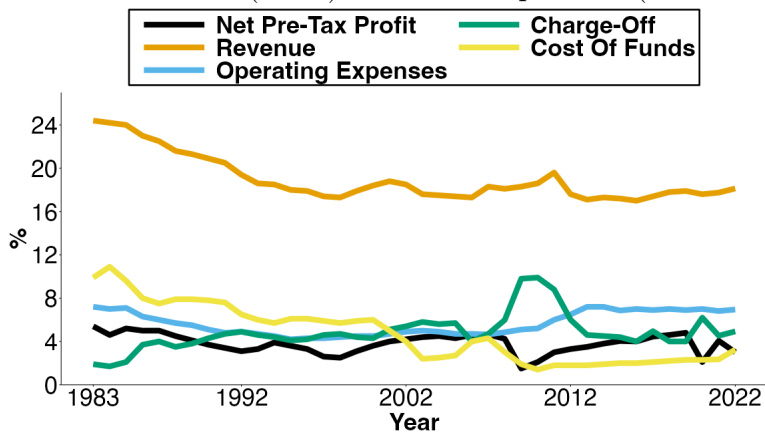
B. Revenue before and after Charge-Offs (2000 - 2022)
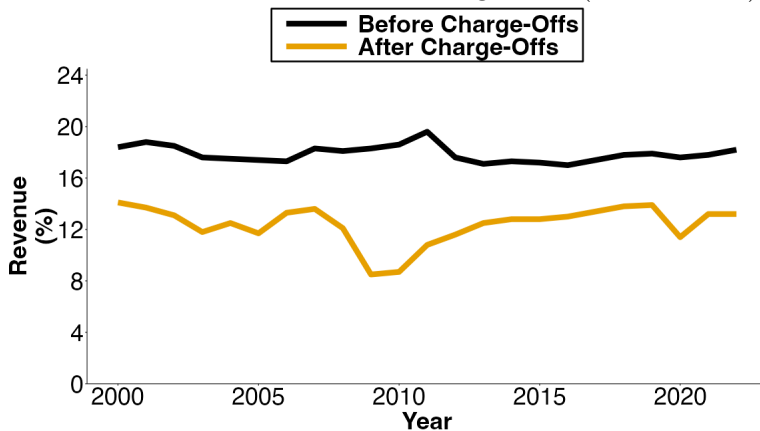
Notes: R.K.Hammer data. Percentages of credit card revolving balances. In Panel B revenues are total revenues (interest, consumer fees, interchange fees) before and after charge-offs as an industry measure of risk adjusting revenue.

Figure 1.10: Costs of Acquiring New Credit Card Account (2000 - 2017)

A. Mean Cost Per Acquisition (CPA)
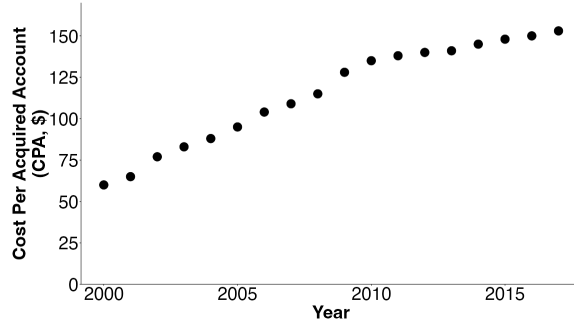
B. Range of Cost Per Acquisition (CPA)
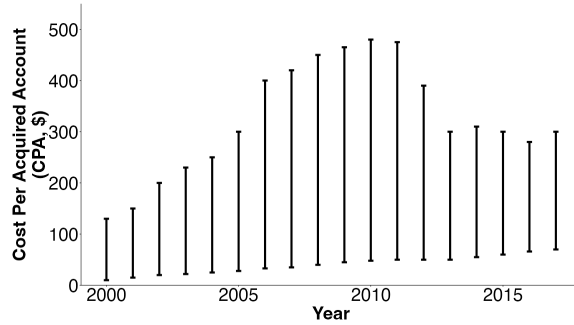
C. Number of Solicitations to Acquire New Account
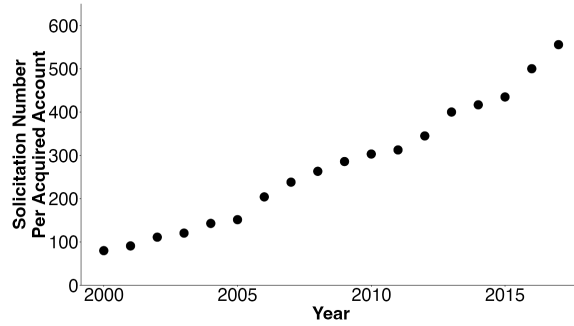
Notes: R.K.Hammer data. These are costs for acquiring new credit card accounts including marketing and underwriting costs.

1.9.4 Actual Payments Information

Figure 1.11: CDF of Actual Payments in Excess of Scheduled Payments

A. CDF

B. CDF where Excess Payment Less Than 10%
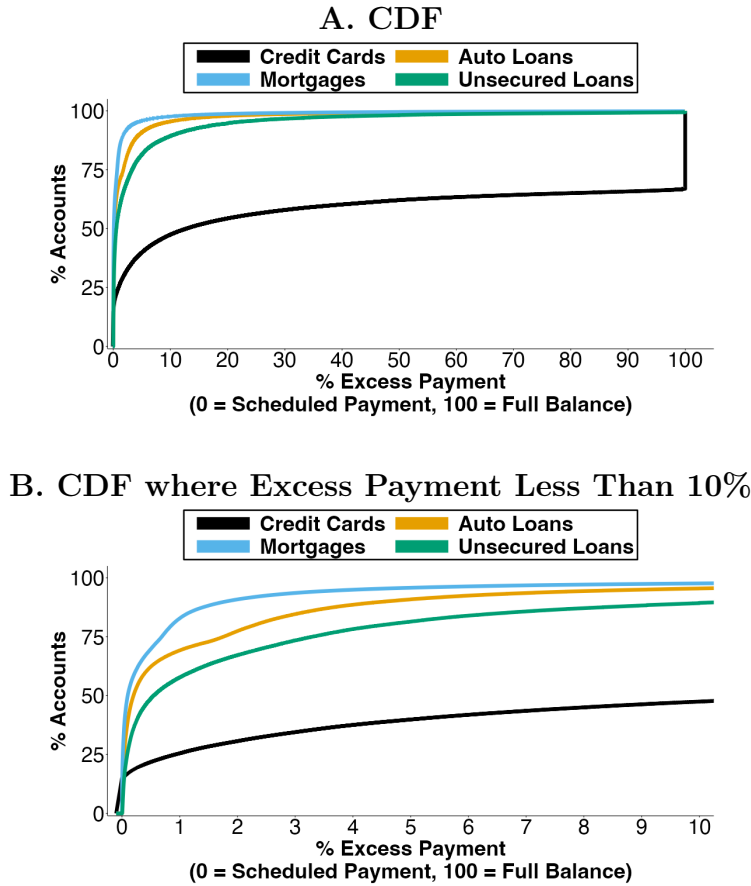
Notes: BTCCP data, December 2012. CDF of non-zero and non-missing actual payments by credit product for accounts with non-zero balances, non-zero scheduled payment amounts, and balances greater than scheduled payment amounts. X-axis shows excess payment calculated as actual payments less scheduled payment amount as a percentage of balance less scheduled payment amount. In this calculation where payments are equal to or in excess of the full balance they are assigned a value of 100%. For credit cards, scheduled payment amount is the minimum amount due and balance is statement balance. For installment loans, scheduled payment amount is the regular payment due (and for mortgages can include taxes and other fees such as to homeowner associations), and the balance is the amount outstanding. Panel A shows the CDF, Panel B focuses on the CDF where excess payment is less than 10%.

Figure 1.12: Robustness of Coverage of Actual Payments Information in Consumer Credit Reports

A. Accounts

B. Balance Weighted

C. Credit Limit Weighted
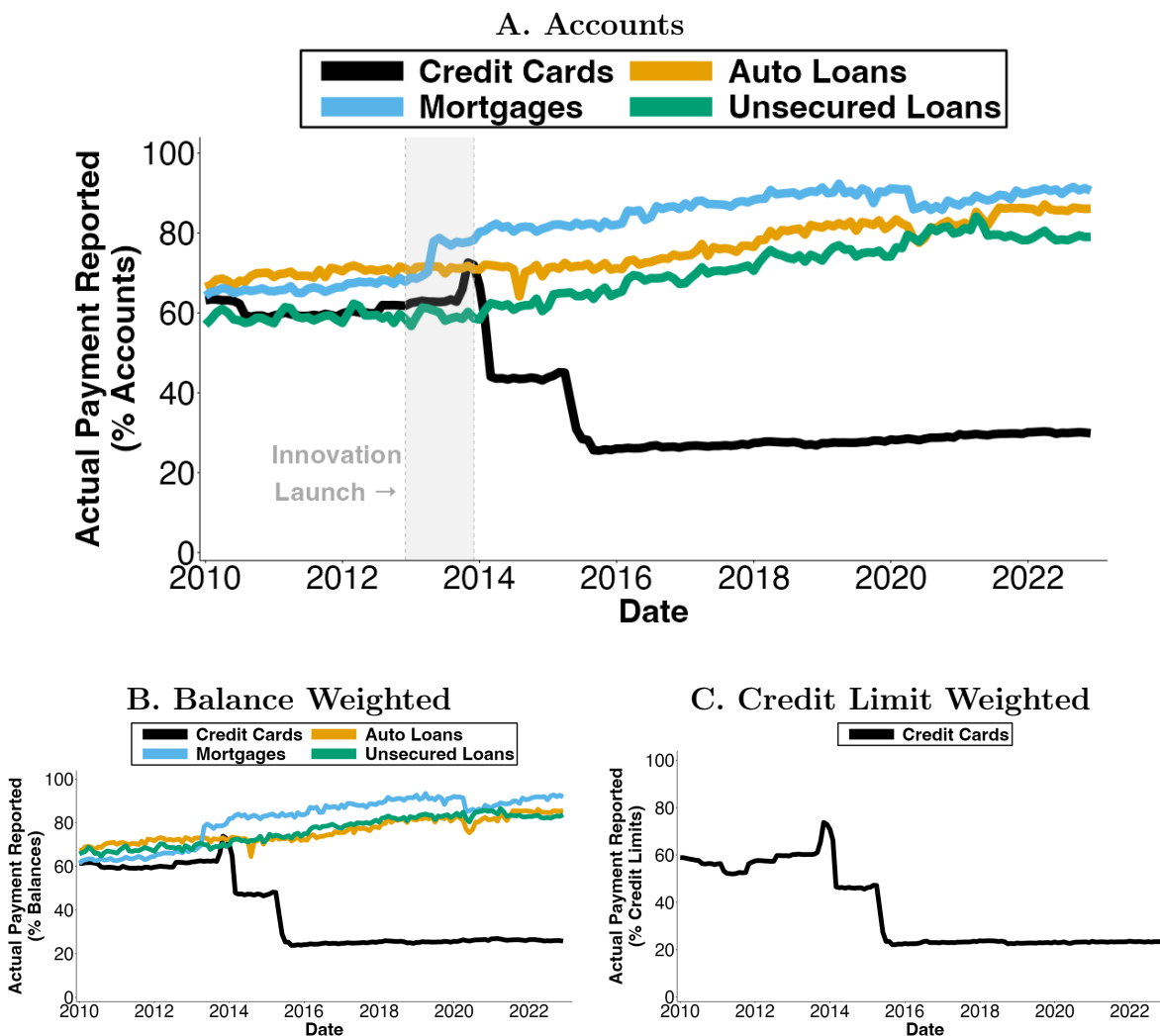
Notes: BTCCP data. In Panel A 2013 is shaded in gray to denote the period when Trended Data was launched. This figure shows the fractions of consumer credit reports with actual payments information. The numerator of these calculations are the number of accounts (Panel A) / value of balances (Panel B) / value of credit limits (Panel C) for accounts with actual payments information that are non-zero and non-missing. The denominator of this calculation is the total number of accounts (Panel A) / value of balances (Panel B) / value of credit limits (Panel C). Both the numerator and the denominator of these calculations restrict to open accounts with non-zero balances and which have been updated in the last year.

Figure 1.13: Robustness of Coverage of Actual Payments Information in Consumer Credit Reports to Inclusion of Retail Cards
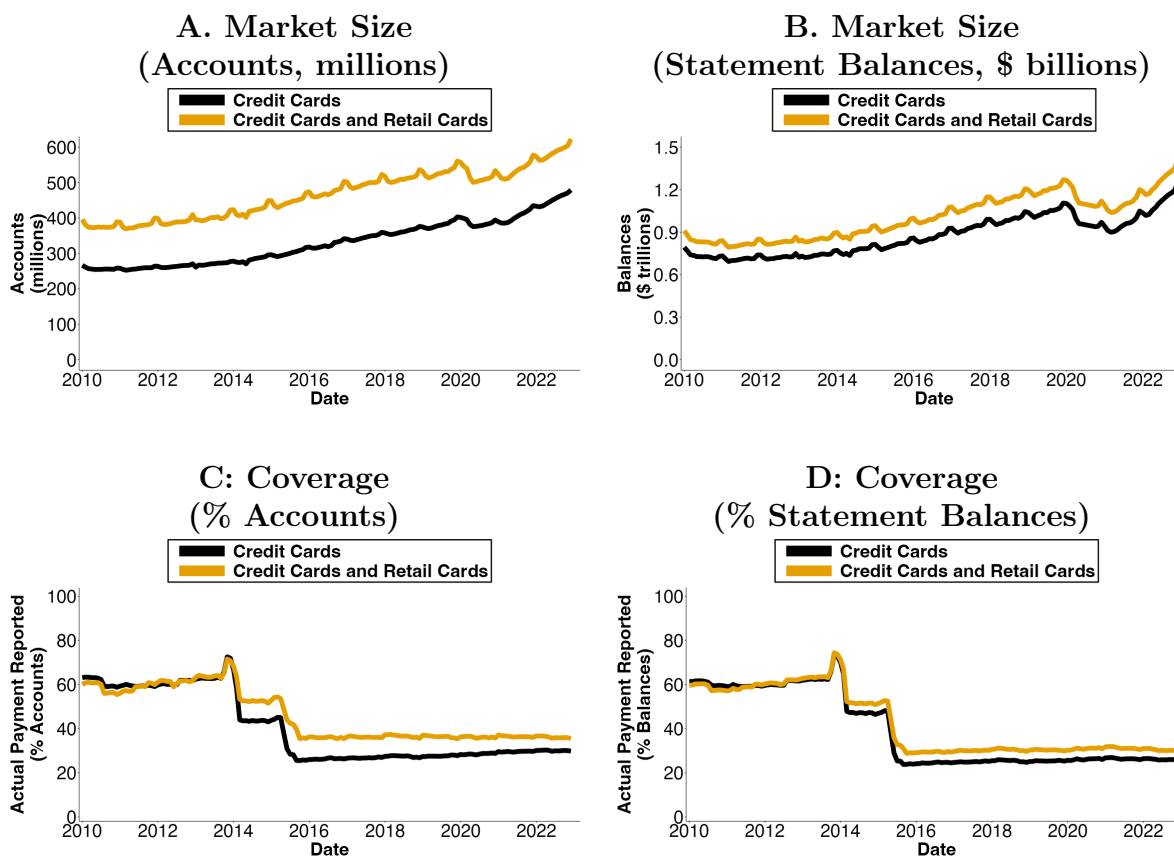
Notes: BTCCP data. These panels compare (general purpose) credit cards to combining these with retail (private label) credit cards. Panels A and B show how market sizes are affected as measured by number of accounts (Panel A) and outstanding statement balances (Panel B). Panels C and D show the fraction of accounts (Panel C) / balances (Panel D) with actual payment amounts in consumer credit reports that are reported as non-zero and non-missing. All panels restrict to open accounts with non-zero balances and which have been updated in the last year.

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Figure 1.14: Coverage of Scheduled Payment Amounts in Consumer Credit Reports
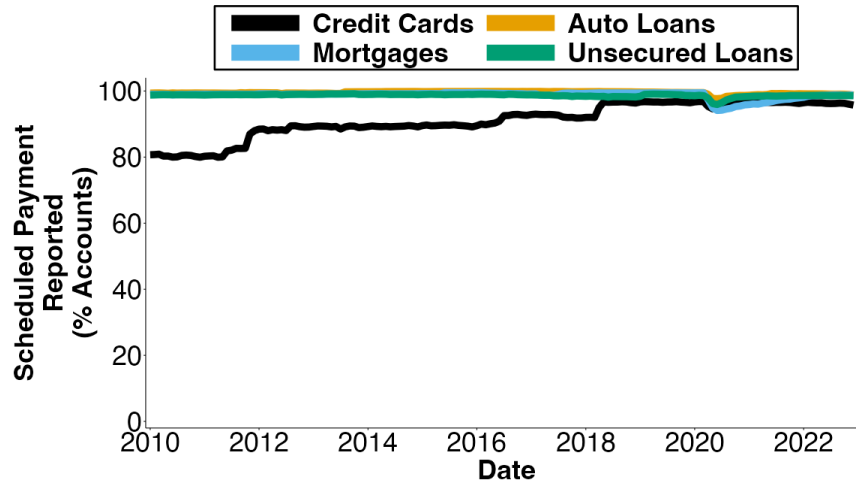
Notes: BTCCP data. Figure shows, for each consumer credit product, the fraction of accounts in consumer credit reports reporting non-zero and non-missing credit card scheduled payment amounts. These calculations restrict to open accounts with non-zero balances and which have been updated in the last year.

Figure 1.15: Credit Cardholders Without Credit Card Actual Payment Information in Consumer Credit Reports on: All Credit Card Accounts (black), Any Credit Card Account (orange), Fraction of Credit Card Accounts (blue)
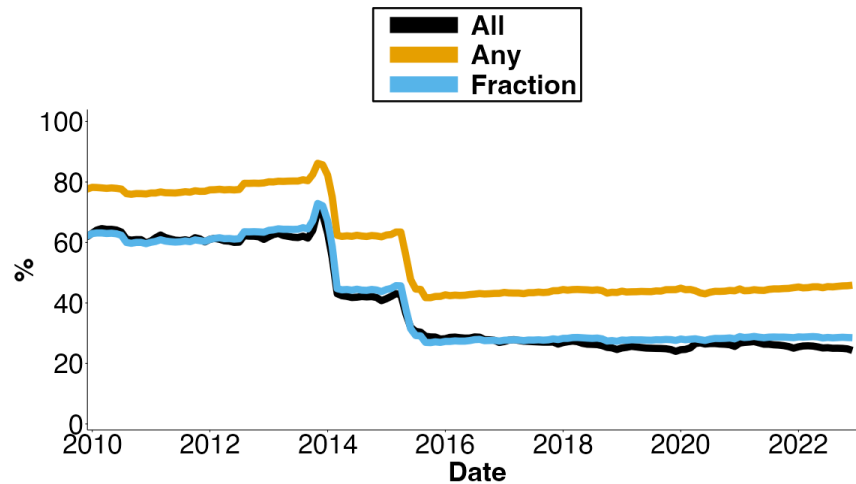
Notes: BTCCP data. The orange line shows the fraction of credit cardholders in consumer credit reports where credit card actual payments are zero or missing on at least one credit card account. The black line shows the fraction of credit cardholders in consumer credit reports where credit card actual payments are zero or missing on all their credit card accounts. The blue line shows, for credit cardholders, the mean proportion of credit cards where credit card actual payments are zero or missing. The denominator for all lines are the number of credit cardholders. The figure restricts to credit cardholders with non-zero credit card balances (which are open and which have been updated in the last year). The figure restricts to accounts which are open with non-zero balances and which have been updated in the last year.

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Figure 1.16: Consumers without Credit Card Actual Payments Information in Consumer Credit Reports

A: Number of Consumers

B: Percentage of Consumers
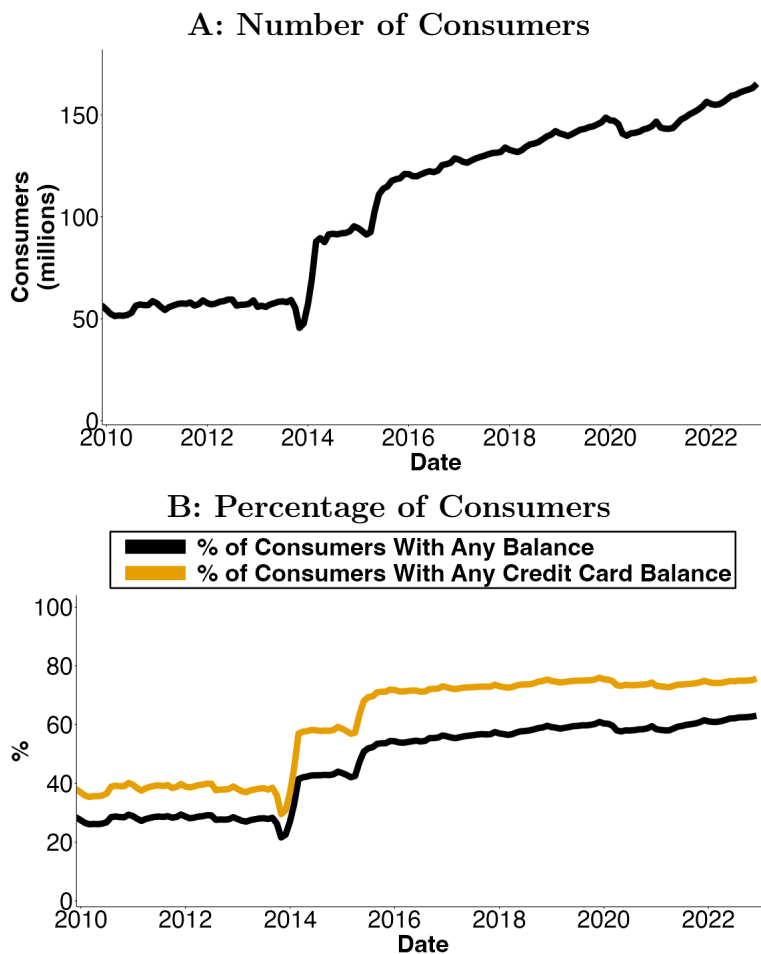
Notes: BTCCP data. Panel A shows the number of consumers in consumer credit reports where credit card actual payments are zero or missing on at least one credit card account. Panel B shows the fraction of consumers in consumer credit reports where credit card actual payments are zero or missing on at least one account (which has a non-zero balance and which has been updated in the last year). The black line uses as a denominator all consumers with non-zero balances on any credit product. The orange line uses as a denominator consumers with non-zero credit card balances. Both panels restrict to open accounts with non-zero balances and which have been updated in the last year)

Figure 1.17: Difference-in-Differences Estimates of Actual Payments Information Sharing in Consumer Credit Reports for Credit Cards Relative to Auto Loans and Unsecured Loans

A. Percent of Accounts
With Payment Reported

B. Percent of All Accounts

C. Percent of Balances
With Payment Reported

D. Percent of All Balances
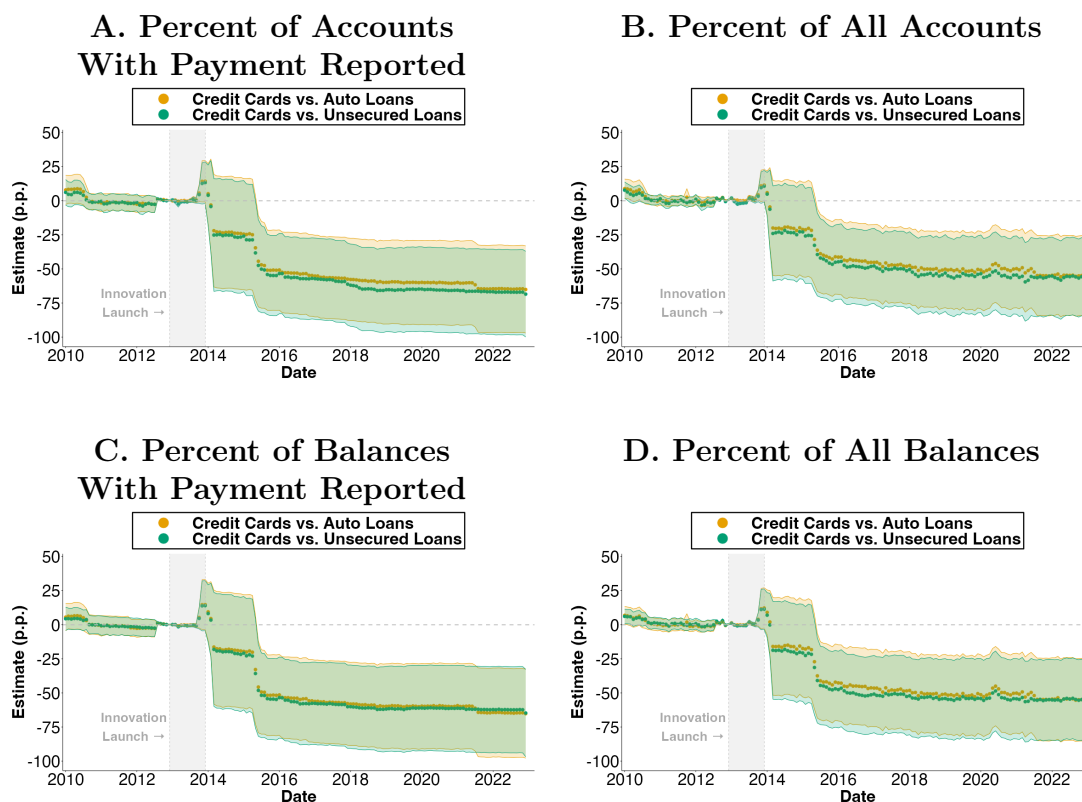
Notes: BTCCP data. 2013 is shaded in gray to denote the period when Trended Data was launched. Figure shows difference-in-differences estimates of sharing actual payment amounts for credit cards relative to auto loans (black, blue) and unsecured loans (orange, green). The outcome for black and orange lines is the fraction of accounts in consumer credit reports sharing actual payment amounts. The outcome for blue and green lines is the fraction of outstanding balances in consumer credit reports sharing actual payments information. Panels A and C condition on accounts where a payment date is recorded in the last month, Panels B and D show all accounts. Estimates are from OLS regression specified in Equation 1.1 on aggregated data with one observation per lender credit product per year-month (with weights applied to the number of accounts) with fixed effects for credit products and year month and December 2012 is the omitted group from the interaction between credit card indicator and year month indicator. Data is a balanced panel 2010 to 2022. 95% confidence intervals from standard errors clustered at the furnisher level. Panels A and C estimated for 6,068 (Credit Cards vs. Auto Loans) and 6,279 (Credit Cards vs. Unsecured Loans) furnisher portfolios. Panels B and D estimated for 6,968 (Credit Cards vs. Auto Loans) and 7,582 (Credit Cards vs. Unsecured Loans) furnisher portfolios.

Table 1.5: Difference-in-Differences Estimates of Actual Payments Information Sharing for Credit Cards Relative to Auto Loans (Column 1) and Unsecured Loans (Column 2)

	(1)	(2)
$D_{Dec\ 2015} \times CRED$	−0.5093***	−0.5483***
	(0.1501)	(0.1504)
$D_{Dec\ 2022} \times CRED$	−0.6507***	−0.6847***
	(0.1629)	(0.1602)

Notes: BTCCP data. Table shows difference-in-differences estimates of sharing actual payments information for credit cards relative to auto loans (column 1) and unsecured loans (column 2). The outcome is the fraction of accounts in consumer credit reports with a payment reported in the last month where there are are non-zero and non-missing actual payments. Estimates are from OLS regression specified in Equation 1.1 on aggregated data with one observation per lender credit product per year-month (with weights applied to the number of accounts) with fixed effects for credit products and year month and December 2012 is the omitted group from the interaction between credit card indicator and year month indicator. Data is a balanced panel 2010 to 2022. Standard errors show in parenthesis are clustered at the furnisher level. Table shows two estimates – the interaction between credit card indicator and (a) the December 2015 indicator; (b) the December 2022 indicator. *** denotes statistical significance at the 1% level. Columns (1) and (2) estimated for 6,068 (Credit Cards vs. Auto Loans) and 6,279 (Credit Cards vs. Unsecured Loans) furnisher portfolios respectively.

Table 1.6: Robustness of Difference-in-Differences Estimates of Actual Payments Information Sharing for Credit Cards Relative to Auto Loans (Column 1) and Unsecured Loans (Column 2)

	(1)	(2)
$D_{Dec\ 2015} \times CRED$	−0.4233***	−0.4687***
	(0.1436)	(0.1438)
$D_{Dec\ 2022} \times CRED$	−0.5624***	−0.5830***
	(0.1529)	(0.1501)

Notes: BTCCP data. Table shows difference-in-differences estimates of sharing actual payment amounts for credit cards relative to auto loans (column 1) and unsecured loans (column 2). The outcome is the fraction of accounts in consumer credit reports with a payment reported in the last month where there are are non-zero and non-missing actual payment amounts. Estimates are from OLS regression specified in Equation 1.1 on aggregated data with one observation per lender credit product per year-month (with weights applied to the number of accounts) with fixed effects for credit products and year month and December 2012 is the omitted group from the interaction between credit card indicator and year month indicator. Data is a balanced panel 2010 to 2022. Standard errors show in parenthesis are clustered at the furnisher level. Table shows two estimates – the interaction between credit card indicator and (a) the December 2015 indicator; (b) the December 2022 indicator. *** denotes statistical significance at the 1% level. Columns (1) and (2) estimated for 6,968 (Credit Cards vs. Auto Loans) and 7,582 (Credit Cards vs. Unsecured Loans) furnisher portfolios respectively.

1.9.5   Consumer Credit Scores

We evaluate how incorporating credit card actual payments information affects the performance of consumer credit scores. We do so using data to December 2012 and estimate logistic regressions predicting outcomes over the next 24 months. We are only able to do this counterfactual exercise for consumers who only hold credit cards where actual payments information was shared in 2012. Broadly we would expect this sample restriction to make our results a lower bound on the uplift in predictive performance one might expect if the portfolio of credit card actual payments were observed. We evaluate predictive performance using out-of-sample AUROC and out-of-sample accuracy. As a baseline we use the performance of a credit score that is constructed without using actual payments information and without Trended Data.

1.9.6   Measurement Error in Credit Card Behaviors

Figure 1.18: R^2 and Root Mean Squared Error (RMSE) Measurement Error in Estimating Contemporaneous Account-Level Credit Card Behaviors in December 2013
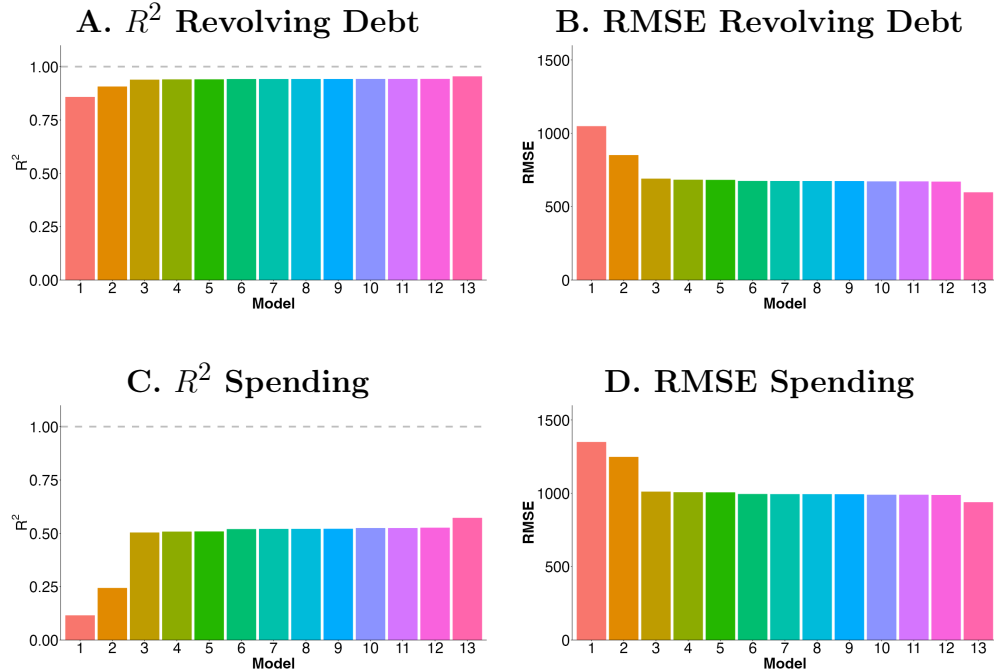
Notes: BTCCP data. Uses December 2013 data for furnishers sharing actual payments to explain contemporaneous account-level credit card behaviors. Figure shows results of OLS regressions where performance is evaluated by R^2 in Panels A and C and by root mean squared error (RMSE) in Panels B and D . Outcomes in Panels A and B are credit card revolving debt and outcomes in Panels C and D are credit card spending. Models 1 to 14 increase in complexity. Model 1 includes current balance, model 2 adds lag balance, model 3 adds change in balance conditional on greater than zero. Models 4 to 11 add in additional account-level variables. Model 12 adds in balance variables from other credit cards held by the consumer. Model 13 includes lags for months 1 to 12, 18 and 24 for the trends of balances and changes in balances conditional on being greater than zero. See Table 1.7 for more details of predictors in models. $N = 4.006$ million credit card accounts in each regression.

Table 1.7: Predictors in Models 1 to 13 for Estimating Contemporaneous Account-Level Credit Card Behaviors

Model	Predictors
1	Statement Balance
2	1 + Lag Statement Balance
3	2 + Change in Statement Balance (zero if negative) + Non-Zero Dummies
4	3 + Credit Score
5	4 + Payment Due
6	5 + Utilization + Credit Limit
7	6 + Card Tenure
8	7 + IRS Zipcode Income
9	8 + Birth Year
10	9 + State
11	10 + Furnisher ID
12	11 + Rest of Credit Card Wallet Behaviors
	(Statement Balances, Changes in Statement Balances)
	Number, Limits, Utilizations)
13	12 + Three Years of Trends in Statement Balances

1.9.7 Estimating Profitability

Financing Charges

Financing Charges are defined as the sum of interest (r_t) and consumer fees (f_t). The most common consumer fees are late fees. Other consumer fees include annual card fees, over credit limit, and foreign exchange fees. Late and annual fees are typically fixed amounts that do not vary with balances.

Credit card financing charges are $117 bn in 2019: 80% is interest revenue ($94.4 bn), and 20% ($23.6 bn) is consumer fees – primarily late fees ($14 billion, 11% of financing charges), annual fees (approximately $5bn) with the remainder being mainly balance transfer fees and cash advance fees (CFPB, 2021, 2022b). Agarwal et al. (2023b) estimates financing charges as $99.6 bn in 2019.

We estimate financing charges using an insight that credit card minimum payments are deterministically calculated. Each month the minimum payment amount due (m_t) on a credit card is typically determined by the formula shown in Equation 1.14. This is the maximum of two components. The first component is a floor dollar amount $\$\mu$.[72] The second component is the sum of (i) a percentage $\theta\%$ of B_t: the statement balance before financing charges: $B_t \equiv b_t - r_t - f_t$, and (ii) financing charges $(r_t + f_t)$. This formula does not vary with cardholder behavior and it is rare for firms to change their minimum payment formula on existing cards.

$$m_t = \max\left\{\$\mu,\ \theta\%\,B_t + r_t + f_t\right\} \tag{1.14}$$

Lenders use this minimum payment formula as it is the easiest way to comply with the Office of the Comptroller of the Currency's (OCC) safety and soundness regulations requiring non-negative amortization. Discussions with industry participants have told us

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72. If balances are below this floor amount then balance rather than the floor is owed. This is not an economically important case given how low the floor amounts are.

other regulators and lenders often apply such regulations even if lenders are not supervised by the OCC. Nelson (2023) reports approximately 90 percent of outstanding credit card balances are held by 17 to 19 large and mid sized lenders who are supervised by the OCC or the CFPB. Some credit unions (credit unions in total are only approximately five percent of the market) and small, subprime lenders capitalize interest and fees and therefore our methodology may produce biased estimates for this small segment.

We find $\$\mu$ and $\theta\%$ in data through a process of manual review of the 84 furnishers we study. For each credit card furnisher, we find the values of μ and θ that matches the lower set of the observed combinations of m_t and b_t. If we find the correct solution, transacting months should be upside errors (observing a minimum payment amount greater than our formula would predict) from fees (or trailing interest) – which are flat amounts not varying with balances – but extremely rarely downside errors (observing a minimum payment amount less than our formula would predict). These parameters can also be found algorithmically for each furnisher with similar results. In an algorithmic approach, focusing on transacting months (which requires observing actual payments information) helps to find these parameters because doing so removes observations which may contain interest in the observed minimum payment.

The values of $\$\mu$ and $\theta\%$ vary across lenders although when we examine a sample of credit card agreements in the CFPB's credit card agreement database they commonly take a small number of values. The most common combination of parameters we find is $\mu^* = \$25$ and $\theta^* = 1\%$ and the most common θ^* is 1%. These are in line with the CFPB's credit card agreement database which contains details of new agreements from Q3 2011 and the CFPB's consumer credit market report which discussed minimum payment rules in 2015.

Given μ^* and θ^*, this produces a predicted minimum payment amount $\hat{m}_t^{interim}$ that would be due before financing charges.

$$\hat{m}_t^{interim} \equiv \max \left\{ \$\mu^*, \ \theta^*\% \, b_t \right\} \tag{1.15}$$

Once we have worked out the minimum payment rules we can apply these across all revolving and transacting months and estimate financing charges. We make an interim estimate of financing charges $(\widehat{r_t + f_t})^{interim}$ in Equation 1.16 as the difference between the minimum payment amount we observe (inclusive of financing charges) and the predicted amount. Since our earlier step applied θ to statement balances *after* including financing charges (i.e., b_t) whereas the correct formula applies it *before* financing charges (i.e., B_t), this interim financing charges estimate is slightly off when financing charges are non-zero (but will be correct when these are zero). We correct for this by subtracting our interim estimate from statement balances. Equation 1.17 then gives us our estimate of financing charges $(\widehat{r_t + f_t})$ as the difference between the observed minimum payment (including financing charges) to the deterministic predicted amount we would expect without financing charges.[73]

$$(\widehat{r_t + f_t})^{interim} \equiv m_t - \hat{m}_t^{interim} \tag{1.16}$$

$$(\widehat{r_t + f_t}) \equiv m_t - \hat{m}_t^* \quad , \text{where } \hat{m}_t^* \equiv \max\left\{\$\mu^*, \ \theta^*\% \left(b_t - (\widehat{r_t + f_t})^{interim}\right)\right\} \tag{1.17}$$

As these are estimated measures they are subject to measurement error. Our data only contains non-negative integers and therefore some error comes from rounding. How may this impact our results? If we choose the incorrect μ this only matters for very low balance account months. If we choose the incorrect θ, then this matters for high balance account months.

If one is willing to impose additional structure on the duration of borrowing, one could estimate effective interest rates, work out a card's interest rate, and decompose interest from the fee component (given the common fees such as late fees are not proportional to balances

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73. This step could be iterated further but doing so makes no substantive difference because credit reporting data is reported as integers and further and so we stop the iteration at this stage.

and do not occur in most months). Furthermore currently we estimate financing charges at the furnisher-level but with sufficient data an analogous method can be applied at the individual card-level to capture intra-furnisher heterogeneity in minimum payment formulae. We may evaluate these in the future.

Charge-Offs

Charge Offs (c_t) are defined as the amount of credit card debt written-off. For profitability we need to calculate financing charges net of charge-offs. We measure charge-off using the manner of payment status: a variable consistently reported as a key input into the standardized credit scoring models firms rely on (FICO and VantageScore). We calculate the amount charged-off based on the outstanding balance in the month preceding an account reaching 120+ days past due. The month preceding is used as some furnishers report the outstanding balance as zero once they update the status of an account as being severely delinquent.[74] We discount this balance to allow for some delinquent debt being cured or later recovered in the collections process. An alternative approach we investigated was to use a variable that records the amount charged-off. However, this variable appears inconsistently reported across furnishers (e.g., some large portfolios have zero charge-offs which appears implausible) possibly due to different debt collections practices.

The humped-shaped pattern in net financing charges is consistent with Nelson (2023) and our discussions with industry participants. Agarwal et al. (2015b) shows a dip in the middle of the distribution which we attribute to the particularly unusual time period their sample covering the Great Recession and their measures are point-in-time whereas ours cover most of a card's lifetime.

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74. Many severely delinquent accounts become impossible to follow as the debt may be consolidated, transferred to a different furnisher, or moved into collections. Such cases can mean the anonymized trade identifier no longer applies to the account.

Interchange Net of Rewards

Interchange Net of Rewards (i_t) is interchange revenue (the amount of merchant fees generated by credit card spending transactions) less rewards expense (the amount credit card lenders pay in rewards to cardholders for spending). Unlike other sources of income that credit card lenders receive from the cardholder, interchange is received from merchants. Credit cards offer rewards to cardholders to incentivize them for spending on the card. These rewards can take a variety of forms including cashback, air miles, and points. Both interchange revenue and rewards expenses are proportional to the amount of spending on a credit card. Interchange revenue and rewards expenses are both higher for "reward" credit cards.

We assume 0.5% spending is interchange net of rewards. Broadly we expect our approach is conservative for evaluating the importance of interchange net of rewards to profitability. Our approach captures the heterogeneity in the amount of spending but will underestimate the variance in net interchange that arises due to consumers holding different types of cards with different mark-ups. This assumption follows closely to Agarwal et al. (2015b, 2018) who use a 2 percent interchange revenue and 1.4 percent rewards and fraud expense and Wang (2023b) who assess merchant fees at 2.25 (MasterCard and VISA interchange revenue of 1.75) and rewards expense of 1.30. Mark-ups are higher on reward cards and such cards are more used by high credit score consumers (Agarwal et al., 2023b). Interchange revenue in 2009 ranges from 1 to 3 percent (GAO, 2009). Agarwal et al. (2023b) assume 1.5 and 2.5 percent for standard and reward cards respectively.

Agarwal et al. (2023a) estimates mean rewards of $4.69 in their main sample and $13.34 per reward card ($160.08 annualized) which is close to the CFPB (2019)'s estimates of $167 in annual rewards per rewards account in 2019 up from $139 in 2015. Rewards expenses have increased 84% from 2015 to 2019 as more consumers hold reward cards and also their rewards have become more generous although more also have annual fees (CFPB, 2019). Interchange fees in 2019 are approximately $50 bn – doubling since 2012 (WSJ 2020; The Ascent / Motley

Fool 2021). Agarwal et al. (2023b) reports the largest banks earnt $41.3 bn in interchange revenue and $34.8 bn in rewards expenses. Interchange revenue varies across issuers. One estimate uses 10-K reports for four (JP Morgan Chase, American Express, Capital One, Discover) of the six largest lenders and find rewards expenses (including partner payments) increased from $21.7 bn in 2019 to $33.1 bn in 2022 and across all six the interchange fees net of these increasing from $28.7 bn in 2018 to $31.9 bn in 2022. By one industry from 2017, American Express earnt $60.43 interchange revenue per active account compared to $34.09 for Capital One, $21.13 for JP Morgan Chase, and $17.40 for Discover.

Figure 1.19: Actual Payments Information Sharing in Consumer Credit Reports by Furnishers from 2012 to 2015
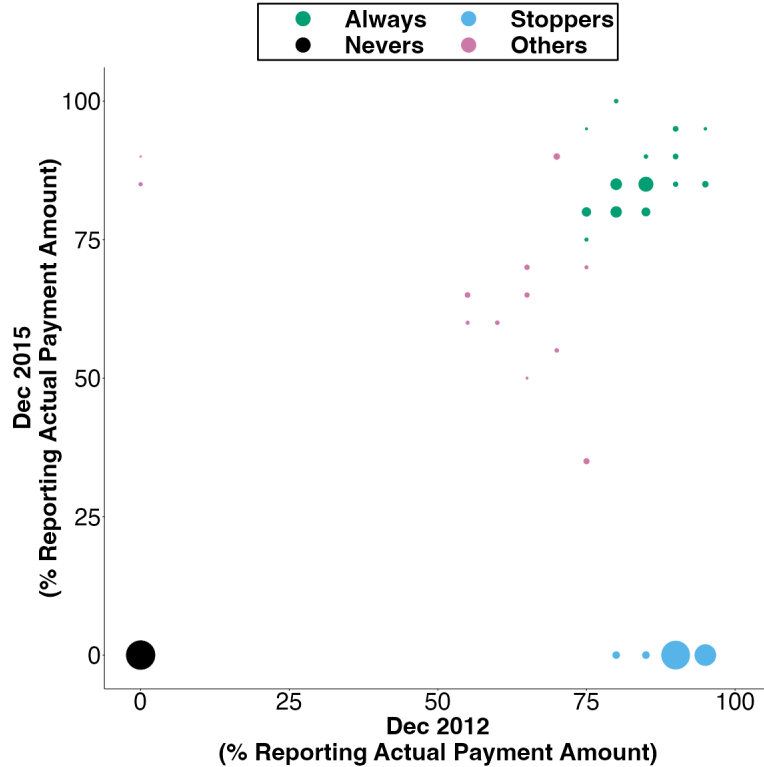
Notes: BTCCP data. This excludes furnishers who have fewer than 10,000 active credit cards (i.e., their portfolio is representative of fewer than 100,000 cards) in both December 2012 and in December 2015. This figure shows, for each consumer credit product, the fraction of accounts in consumer credit reporting data reporting actual payments in December 2012 (x-axis) and December 2015 (y-axis). In the numerator of this calculation, accounts with actual payments that are non-zero and non-missing are given a value of one, and accounts with zero or missing are given a value of zero. Both the numerator and the denominator of this calculation restricts to accounts with positive balances and that are not delinquent. Results are split by classifying credit card furnisher by their sharing of actual payments as described in paper section 1.2.2 or Table 1.3 notes. Dots are shown in five percentage point intervals aggregating furnishers in these intervals. Sizes of dots correspond to the total number of credit card accounts 2012 to 2015.

Figure 1.20: 2012 to 2022 Financing Charges Net of Charge-Offs
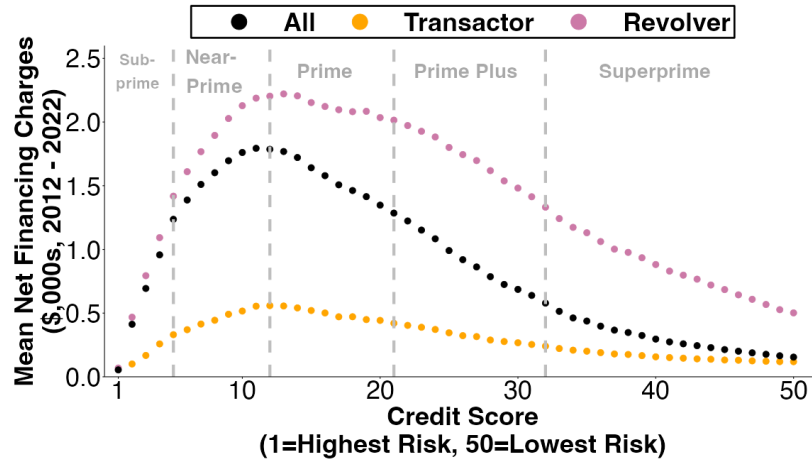
Notes: BTCCP data. Figure shows mean estimates conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Financing charges are estimated as described in section 1.9.7. Figure shows financing charges accumulated across 2012 to 2022 net of charge-offs with results split by classifying accounts by whether the revolved or transacted the majority of months in 2012. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.21: Predicting Financing Charges Net of Charge-Offs Without Actual Payments Information
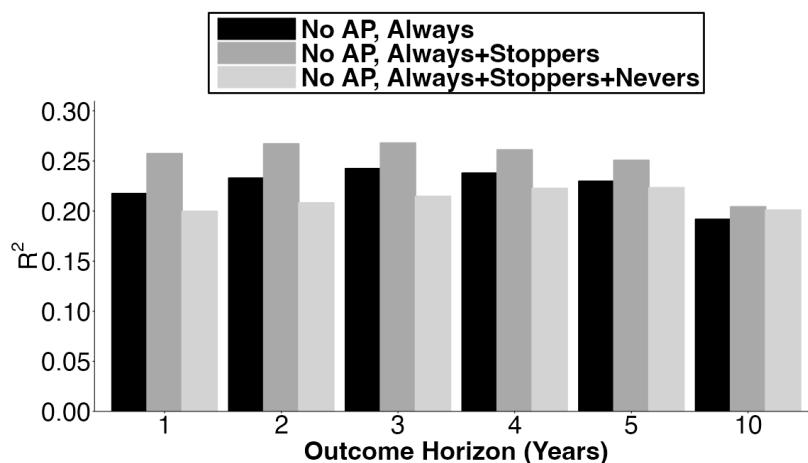
Notes: BTCCP data. Figures use data to December 2012 to predict credit card financing charges net of charge-offs at the account-level over one to ten year horizons. Predictive performance is measured by out-of-sample R^2. Predictive performance is shown without actual payments information. Performance is shown for three samples: Always, Always + Stoppers, Always + Stoppers + Nevers as described in paper section 1.2.2 and Table 1.3 notes. Out-of-sample predictions from $N = 3.135$ million Always credit card accounts, $N = 11.018$ million Always + Stoppers credit card accounts, and $N = 14.927$ million Always + Stoppers + Nevers credit card accounts.

Figure 1.22: Marginal Value of Actual Payments (AP) Information for Predicting Credit Card Profits over 1 to 10 Year Time Horizons
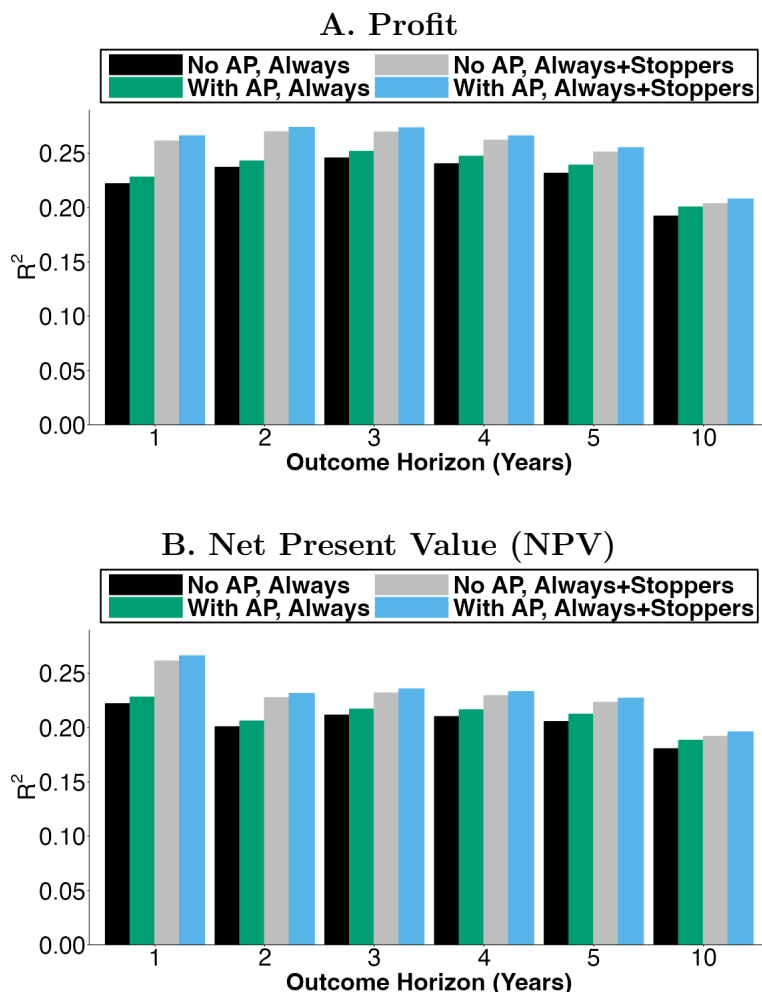
A. Profit

B. Net Present Value (NPV)

Notes: BTCCP data. Figures use data to December 2012 to predict account-level credit card profitability where predictive performance is measured by out-of-sample R^2. Results are shown without (black, gray) and with (green, blue) actual payments information. Performance is shown for two samples: Always (black, green) and Always + Stoppers (gray, blue) as described in paper section 1.2.2 and Table 1.3 notes. Spending beyond a one year horizon is imputed for Stoppers but observed for Always. Panel A shows predictions of profit over one to ten year horizons. Panel B shows predictions of net present value (NPV) over one to ten year horizons. Out-of-sample predictions from $N = 3.135$ million Always credit card accounts, and $N = 11.018$ million Always + Stoppers credit card accounts.

1.9.8   Credit Card Selection

Table 1.8: Summarizing Selection in Credit Card Portfolios

	Always	**Stoppers**	**Nevers**
Credit Score	720.73	719.70	744.23
(S.D.)	(87.10)	(89.61)	(76.16)
Tenure	68.52	95.18	141.21
(S.D.)	(76.65)	(79.13)	(109.75)
Credit Limit	8,574.75	9,460.33	10,403.06
(S.D.)	(7,626.41)	(9,487.96)	(9,446.22)
Statement Balance	2,077.10	2,351.69	2,456.91
(S.D.)	(3,535.00)	(3,954.01)	(4,323.95)
Utilization	36.26	39.08	29.49
(S.D.)	(38.75)	(39.97)	(35.24)
Proxy Spending	2,454.67	2,752.78	3,369.77
(S.D.)	(4,059.19)	(5,044.94)	(7,917.64)

Notes: BTCCP data. Table shows means (standard deviations in parenthesis) for credit card portfolio characteristics as of December 2012. Card tenure is measured in months. Proxy spending is measured by change in balances conditional on being non-negative. Financing charges are estimated based on our methodology described in section 1.9.7. Results are split by classifying credit card furnishers by their sharing of actual payments information on . The last two rows show the shares of the number of outstanding credit card accounts and the value of outstanding credit card statement balances by each type of furnisher. These data exclude furnishers who do not have at least 10,000 active credit cards (i.e., their portfolio is representative of least 100,000) in both December 2012 and in December 2015. Always are furnishers sharing actual payment amounts information for more than 75% of their active credit cards in both December 2012 and December 2015. Stoppers are furnishers sharing actual payments amounts information for more than 75% of their active credit cards in December 2012 and for less than 10% in December 2015. Nevers are furnishers sharing actual payment amounts information for less than 10% of their active credit cards in both December 2012 and December 2015. The remaining furnishers are Others excluded from the table: these are 3.1% of accounts and 1.3% of statement balances.

Figure 1.23: CDF of Credit Score

A. CDF
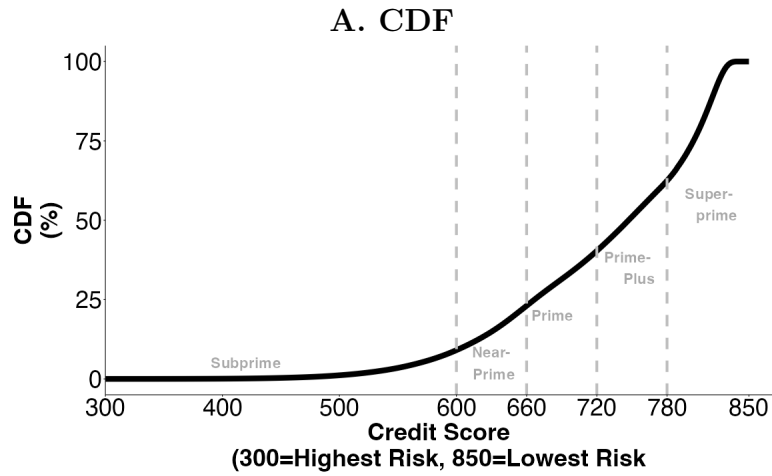
B. CDF by Lender' Actual Payments Information Sharing Decision
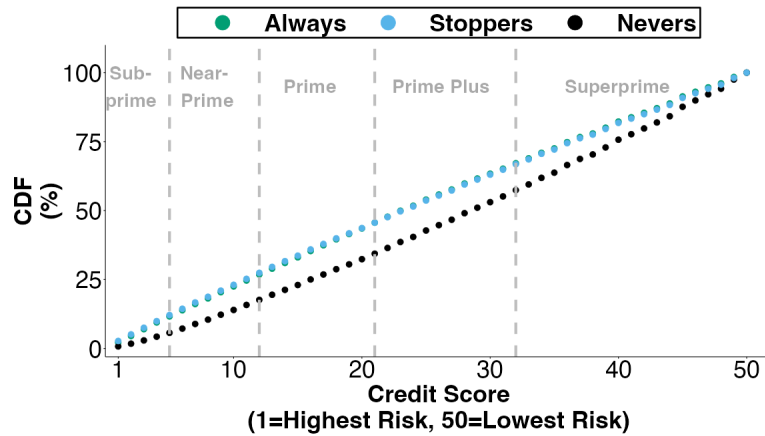
Notes: BTCCP data. Panel A shows CDF and Panel B shows CDF by 50 quantiles where thresholds are defined globally and fixed across classifications. Results in Panel B are split by classifying credit card furnishers by their sharing of actual payments information as described in paper section 1.2.2 or Table 1.3 notes. Gray dotted lines divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores).

Figure 1.24: Credit Card Default Rates Conditional on Credit Score

A. 90+ Days Past Due

B. Log Odds 90+ Days Past Due

C. 180+ Days Past Due

D. Log Odds 180+ Days Past Due
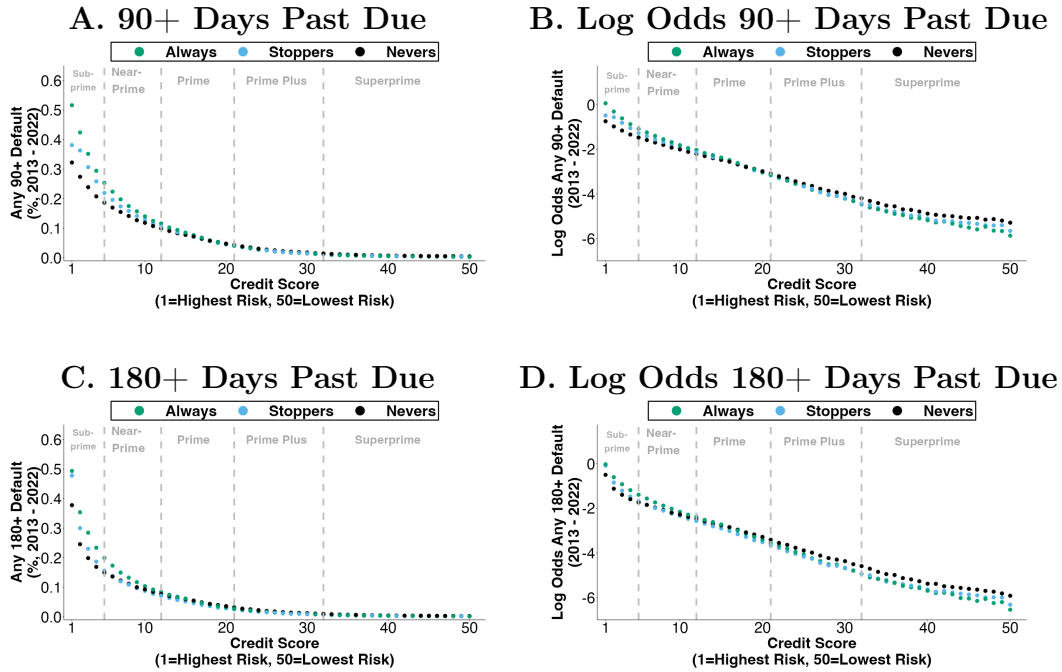
Notes: BTCCP data. Figure shows fraction of credit cards in December 2012 that become delinquent at any point 2013 to 2022 (y-axis) conditional on 50 quantiles of credit score (x-axis). Panel A shows delinquency defined as any 90 or more days past due (DPD) and Panel B shows this in log odds. Panel C shows for 180 or more DPD and Panel D shows this in log odds. Results are split by classifying credit card furnishers by their sharing of information on actual payment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.25: Credit Card Behaviors Conditional on Credit Score

A. Mean Statement Balance
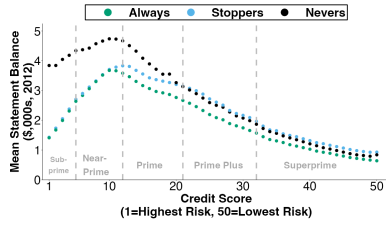
B. Standard Deviation Statement Balance
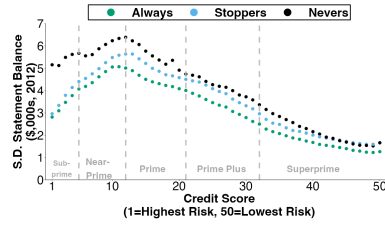
C. Mean Credit Limit
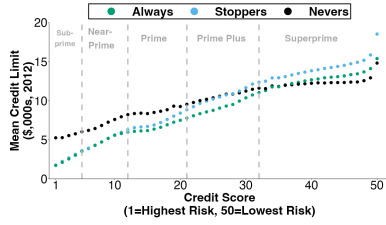
D. Standard Deviation Credit Limit
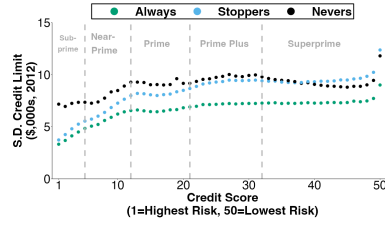
E. Mean Utilization
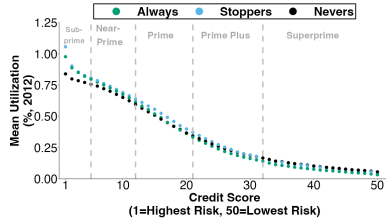
F. Standard Deviation Utilization
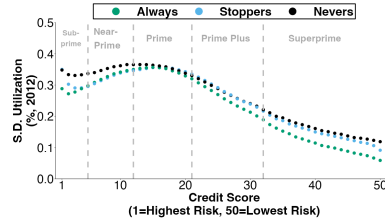
G. Mean Transacting Months
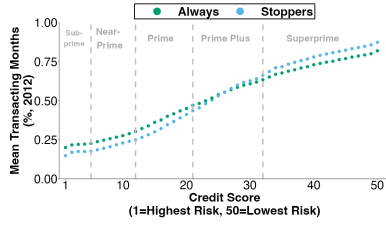
H. Standard Deviation Transacting Months
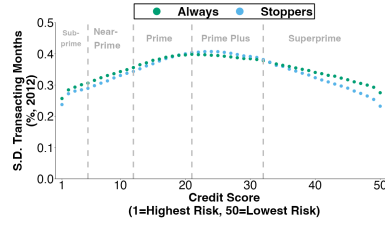
Notes: BTCCP data. Figure shows credit card behaviors conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Panels A, C, E, and G show means. Panels B, D, F, and H show standard deviations. Utilization rate is calculated by statement balance divided by credit limit. Results are split by classifying credit card furnishers by their sharing of information on actual repayment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

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Figure 1.26: Credit Card Spending Behaviors Conditional on Credit Score
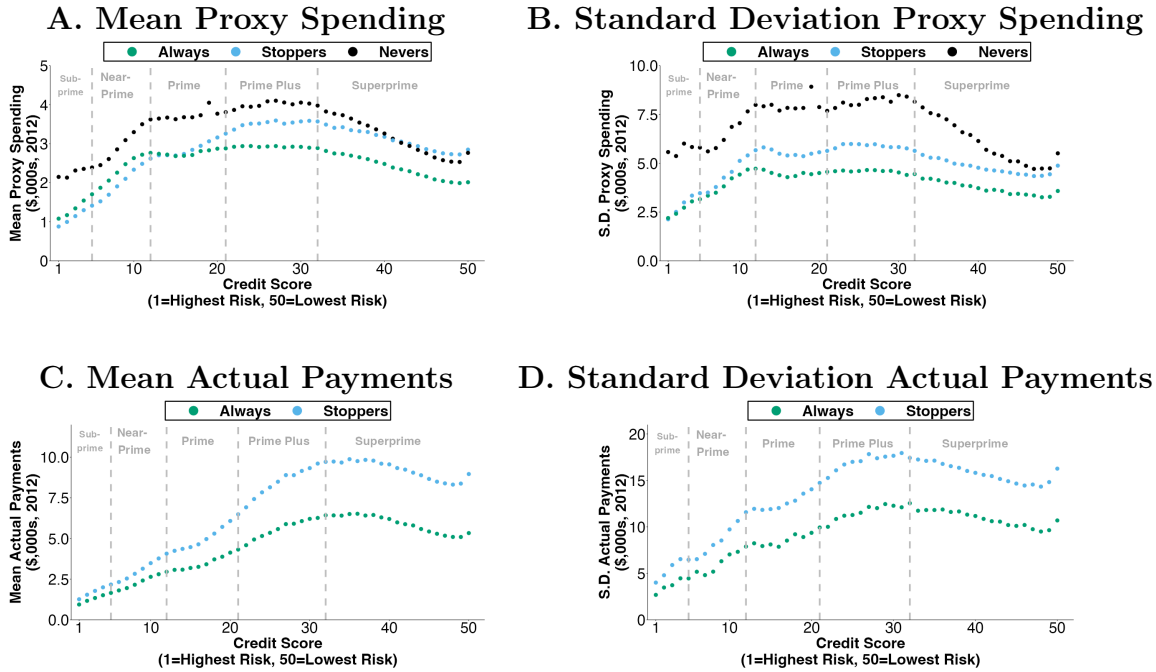
Notes: BTCCP data. Figure shows credit card spending behaviors (y-axis) conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Panels A and C show means. Panels B and D show standard deviations. Proxy spending is calculated by change in statement balance where counted as zero if negative. Results are split by classifying credit card furnishers by their sharing of information on actual repayment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.27: 2013 Credit Card Behaviors Conditional on Credit Score

A. Mean Revolving Debt
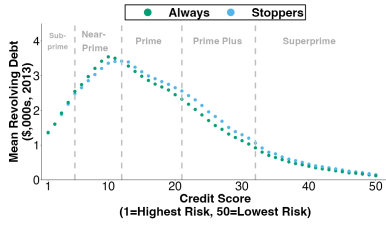
B. Standard Deviation Revolving Debt
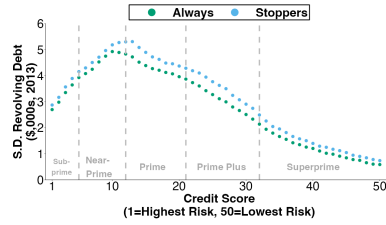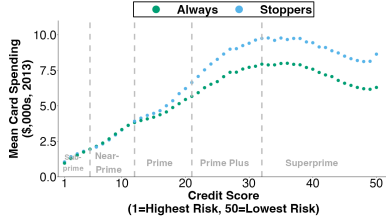
C. Mean Spending

D. Standard Deviation Spending
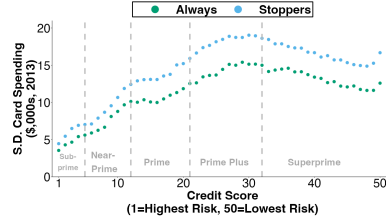
E. Mean Statement Balance
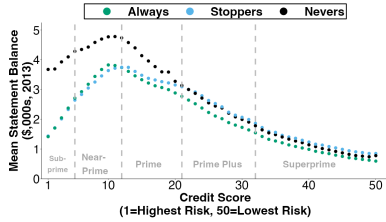
F. Standard Deviation Statement Balance
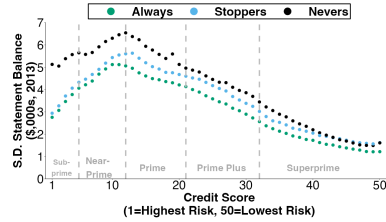
G. Mean Proxy Spending
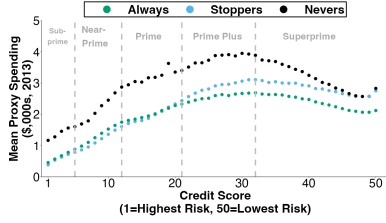
H. Standard Deviation Proxy Spending

Notes: BTCCP data. Figure shows credit card behaviors (y-axis) conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Panels A, C, E, and G show means. Panels B, D, F, and H show standard deviations. Proxy spending is calculated by change in statement balance where counted as zero if negative. Results are split by classifying credit card furnishers by their sharing of information on actual payment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.28: Credit Card Behaviors of Transactors and Revolvers Conditional on Credit Score
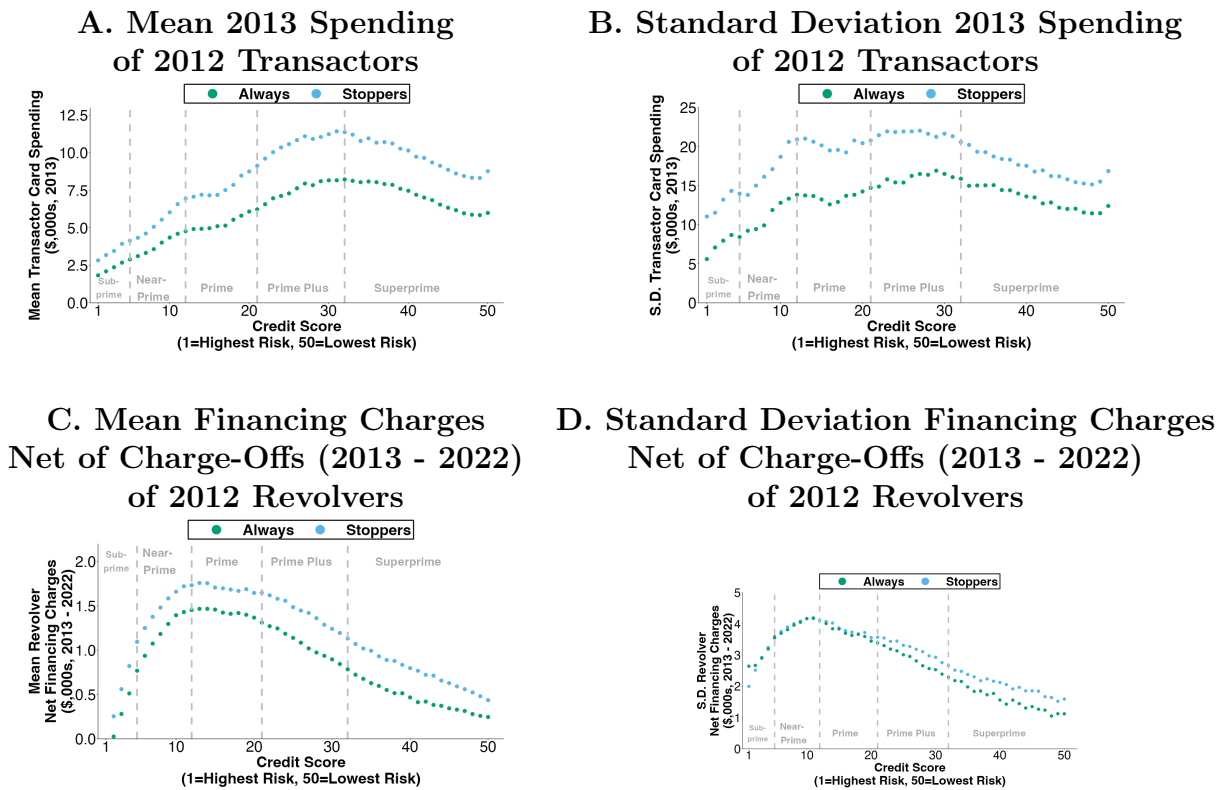
**A. Mean 2013 Spending
of 2012 Transactors**

**B. Standard Deviation 2013 Spending
of 2012 Transactors**

**C. Mean Financing Charges
Net of Charge-Offs (2013 - 2022)
of 2012 Revolvers**

**D. Standard Deviation Financing Charges
Net of Charge-Offs (2013 - 2022)
of 2012 Revolvers**

Notes: BTCCP data. Figure shows credit card behaviors (y-axis) conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Panels A and C show means. Panels B and D show standard deviations. Panels A and C show 2013 spending for accounts transacting the majority of months in 2012. Panels B and D show 2013 to 2022 financing charges net of charge-offs for accounts revolving the majority of months in 2012. Results are split by classifying credit card furnishers by their sharing of information on actual repayment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.29: Credit Card Activity Rates Conditional on Credit Score
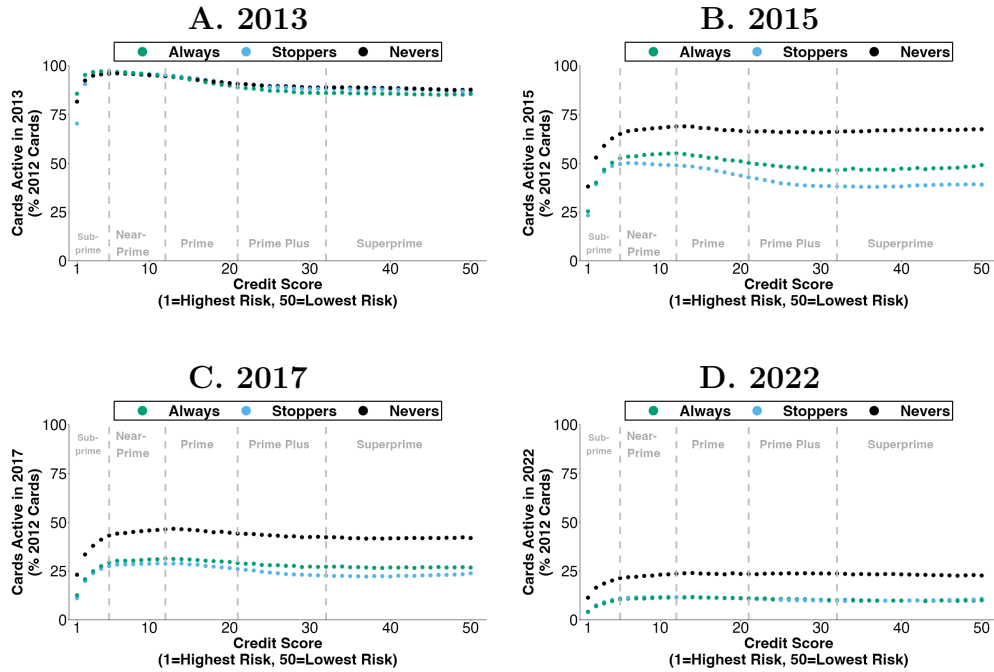
Notes: BTCCP data. Figure panels shows fraction of credit cards in December 2012 that remain active over different horizons. Panel A by 2013, B by 2015, C by 2017, and D by 2022. These are presented conditional on 50 quantiles of credit score (x-axis). A card is active if it remains open with a non-zero statement balance and is not 90+ day past due and has been updated in the last year. Results are split by classifying credit card furnishers by their sharing of information on actual payment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.30: Credit Card Tenure to 2022 and Financing Charges Net of Charge-Offs (2012 to 2022) Conditional on Credit Score
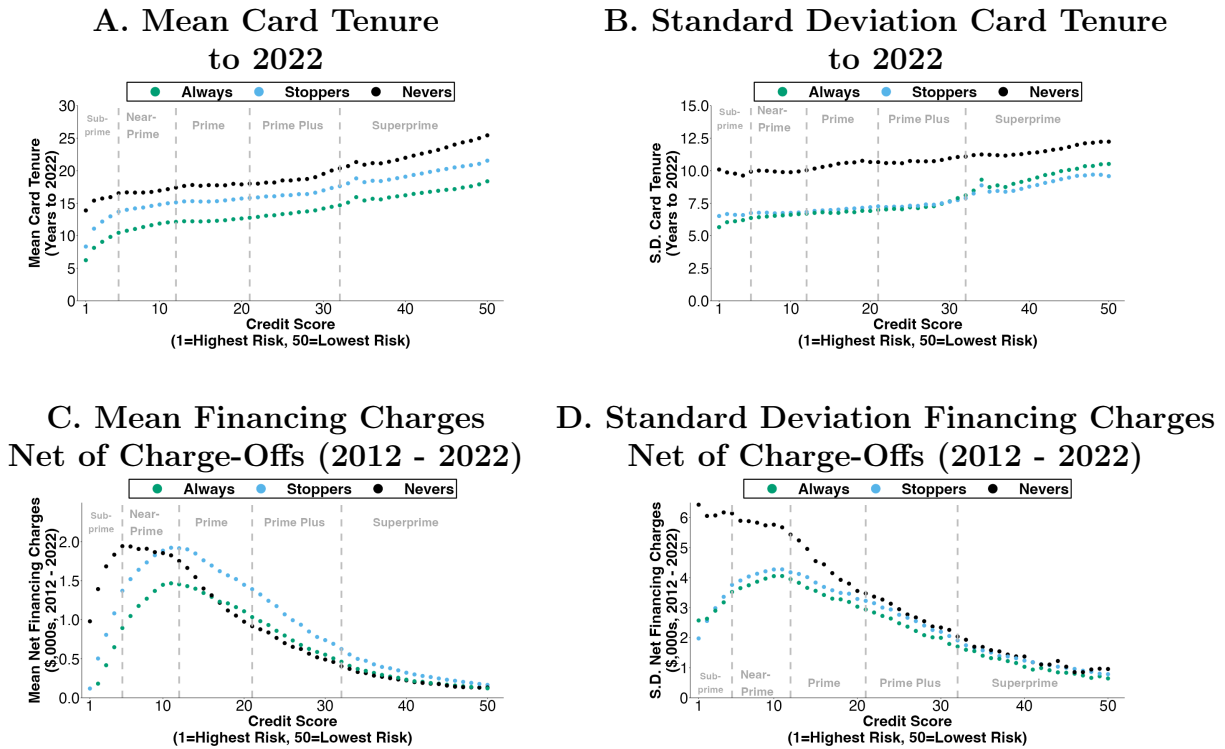
Notes: BTCCP data. Figure shows credit card behaviors (y-axis) conditional on 50 quantiles of credit score (x-axis) for credit cards in December 2012. Panels A and C show means. Panels B and D show standard deviations. Panels A and B show card tenure to 2022. Panels C and D show financing charges net of charge-offs from 2012 to 2022. Results are split by classifying credit card furnishers by their sharing of information on actual payment amounts as described in paper section 1.2.2 or Table 1.3 notes. Credit score quantile thresholds are defined globally and fixed across classifications. Gray dotted lines show quantiles which divide credit score into standard segments for subprime (lowest scores), near-prime, prime, prime-plus, and superprime (highest scores) fall in the distribution.

Figure 1.31: Mean Lifetime Credit Card Interchange Net of Rewards by Card Tenure, Split by Credit Score
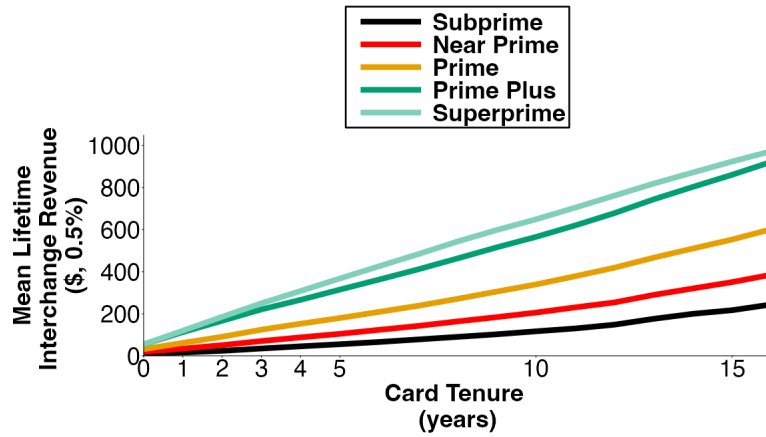
Notes: BTCCP data. Uses cross-sectional data on spending by tenure and credit score for accounts where actual payments information is shared during 2012 to 2013 to estimate lifetime interchange. Assumes constant 0.5% interchange income net of rewards expense. Interchange income is the amount of transaction fees credit card lenders receive from merchants when a consumer spends on their credit card.

Figure 1.32: Effects of Trended Data on Competition for 2 and 3 Card Samples

A. CDF Exposure to Trended Data
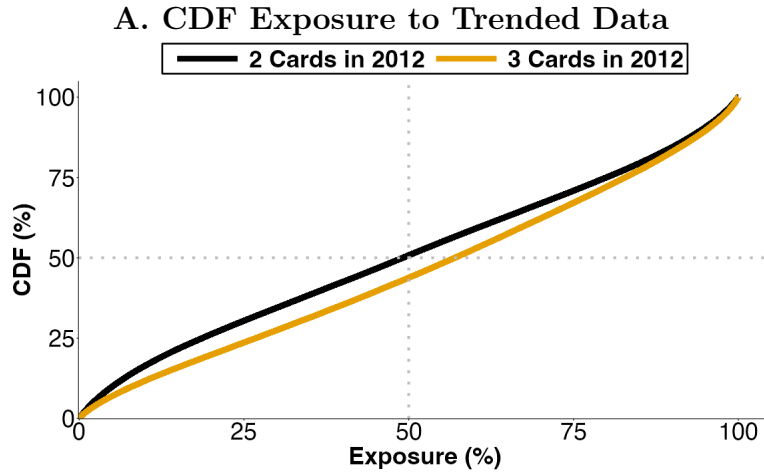
B. Estimates of Effects of Trended Data on Any New Credit Card Opening
(t-1 means: 3.22% for 2 card sample, 4.23% for 3 card sample)
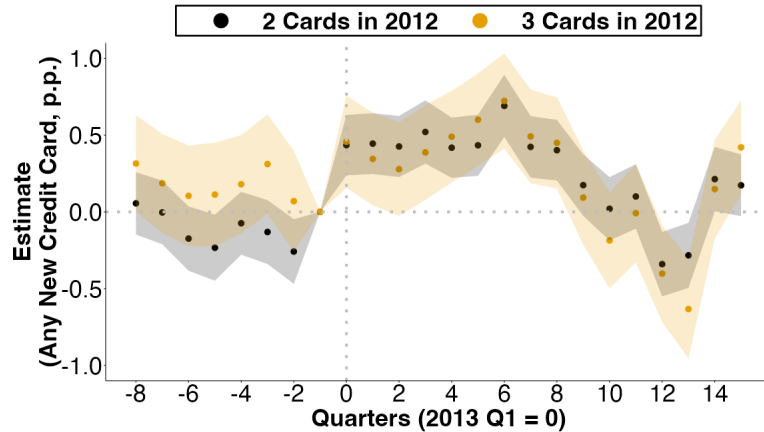
Notes: BTCCP data. Panel A shows CDF of exposure. Exposure is (pre-trended data) share of 2012 credit card balances held with furnishers who share actual payments information. Panel B shows our difference-in-differences with varying intensity estimates in percentage points (p.p.) where the outcome is any new credit card account openings in a quarter. Difference-in- differences estimates from balanced panel of consumers Q1 2011 to Q4 2016, with $0 < EXPT_i < 1$, and holding two (black) / three (orange) cards both of which have positive balances in 2012. The two card sample is 0.51 million consumers and the three card sample is 0.29 million consumers. OLS regression with consumer and calendar year-quarter fixed effects and interaction term between exposure and calendar year-quarter where Q4 2012 is omitted category and standard errors are clustered at the consumer level.

1.9.9   Mandating Sharing Credit Card Limit Information

We isolate which anonymized furnishers revealed information on credit card limits by taking data from October 2011 and November 2011 and compare their credit card tradelines with credit limit information shared in November 2011 that did not share this information in October 2011. We use this to label furnishers as either "insiders" who reveal information in November 2011 and "outsiders" who learn about about the information revealed.

Information is revealed for approximately 30% of these furnishers' open cards, 39% of outstanding balances, and 36% of their consumer base. These revealed accounts are a non-random subset of the furnisher's accounts. Revealed accounts have, on average, higher credit scores (775 vs. 736), higher credit limits ($16,363 vs. $9,461), higher statement balances ($2,622 vs. $1,903), and shorter card tenures (7.4 vs. 8.7 years), compared to accounts with the same furnishers that shared credit card limit information in October 2011.

Figure 1.33: Coverage of Credit Card Limits in Consumer Credit Reports
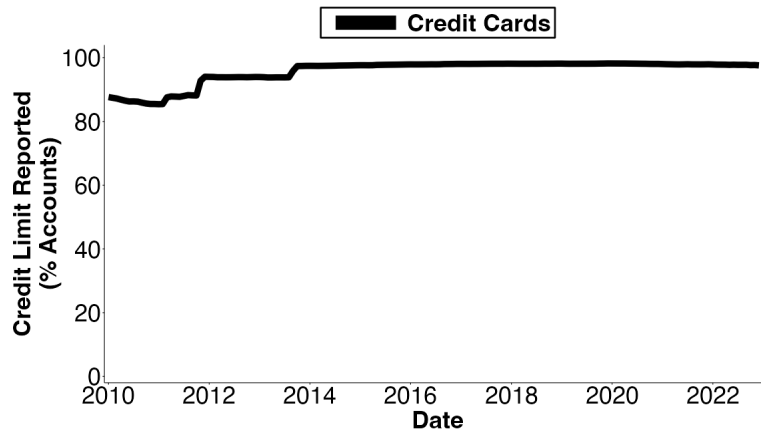
Notes: BTCCP data. Figure shows the fraction of credit card accounts in consumer credit reports with non-zero and non-missing credit card limits.These calculations restrict to open accounts with non-zero balances and which have been updated in the last year.

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CHAPTER 2

DISASTER FLAGS: CREDIT REPORTING RELIEF FROM NATURAL DISASTERS

2.1 Introduction

The United States is increasingly affected by more numerous and more economically damaging natural disasters such as hurricanes, tornadoes, wildfires, and floods.[1] Such climate change poses many challenges for financial markets (e.g., Giglio et al., 2021). Climate change means some consumers are increasingly exposed to events causing them financial distress. Consumer financial distress in the form of temporarily missing one or more payments ("defaults") on their consumer credit report can have longer-term adverse effects including reduced credit access. This is because information on historical defaults from the last seven years remains on credit reports and are a key input to credit scores.

In this paper, I consider the role for masking defaults in credit reports to provide relief to consumers exposed to natural disasters. Masking defaults may mean trading-off heterogeneous impacts: improving credit access for masked defaulters who appear less risky to lenders but reducing credit access for non-defaulters who appear riskier having been pooled with masked defaulters. Crucially whether to mask defaults occurring during natural disasters ("disaster defaults") depends on how informative such data are at predicting future default. If disaster defaults are highly predictive then masking this information would be expected to be costly to lenders and reduce the market efficiency of lending. Whereas if disaster defaults offer limited predictive value then a social planner may want to mask them as it would indicate being adversely affected by natural disasters is more bad luck rather

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1. Between 1980 to 2010, there were only two years (1998 and 2008) with at least ten disasters each resulting in damages of one billion dollars or more. In contrast, every year 2011 to 2022 except one (2014) witnessed at least ten disasters each causing damages of one billion dollars or more. In 2020, there were a record-breaking number of twenty-two billion-dollar disasters. Source: National Oceanic and Atmospheric Administration (NOAA) National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disaster. Billion-dollar disasters are inflation-adjusted.

than informatively revealing a consumer's type and future behavior.

I research this topic by documenting and evaluating the existing, voluntary system of natural disaster credit report relief developed by the market and also considering counter-factual government social insurance policies – masking defaults during natural disasters – that could be implemented. I study how lenders respond to natural disasters by applying "disaster flags" to their customers' credit reports. Disaster flags are designed to provide relief to consumers and protect their credit access following disasters such as hurricanes, forest fires, and COVID-19. Lenders voluntarily apply disaster flags. These flags temporarily mask a consumer's negative information (i.e. defaults) in the calculation of VantageScore credit scores. The only other study on this topic is a short Consumer Financial Protection Bureau report (Banko-Ferran and Ricks, 2018) that concludes "more analysis is needed to better understand whether and how the furnishing of information on natural disasters affects consumer credit". My paper addresses this research gap.

I document five new facts on disaster flag's use from examining 23 years of monthly US consumer credit reporting data. First, disaster flags commonly appear on US credit reports. 59.2 million consumers had a disaster flag on their credit report between 2010 and 2020. This is over 3.5 times the number of consumers bankrupt over the same period. Second, the prevalence of disaster flags greatly increased over time. Disaster flags were introduced following 9/11, were rarely used until Hurricane Katrina in 2005, with use increasing tenfold in 2017 with Hurricanes Harvey and Irma. Third, there is broad geographic coverage of disaster flags. In the 2000s and early 2010s disaster flags were mainly used in the South East coastal areas but in the late 2010s have been used in other areas such as the West of the country that has been affected by wildfires, and were used across the entire US in response to the COVID-19 pandemic. Fourth, disaster flags typically only remain on a credit tradeline for a few months: 90% for less than six months. Fifth, disaster flags are typically only applied to a subset of consumers' credit tradelines.

Why would lenders voluntarily apply disaster flags? I evaluate this by examining the

informational value of flagged defaults to understand how costly it is for lenders to voluntarily remove such information. I find consumers with disaster flags are adversely selected with ex-ante lower credit scores, more credit tradelines, and being more indebted. I construct credit scoring models to quantify how different the predictive value of a flagged default is from a non-flagged default. I find that flagged defaults are riskier than non-flagged defaults: but only when they have occurred in the last six months, otherwise they are similar. While a model predicting future credit default that masks flagged defaults as an input performs worse than a model without such masking, I interpret the difference between the two is economically small. It therefore appears lenders incur a seemingly small cost to voluntarily, temporarily applying such disaster flags to mask defaults.

What are the benefits to consumers of disaster flags? I use event study and difference-in-differences methodologies to study this. Disaster flags mask some defaults leading to an economically small 0.2% increase in average VantageScore credit scores. These average effect sizes are smaller than that found in research on the effects of removing bankruptcy flags from US consumer credit reports (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023). There is heterogeneity in the effects of disaster flags on credit scores. The effect is larger 11 to 18 points (1.9-3.3%), for the subgroup of consumers with low (subprime) credit scores or those with any defaults twelve months before the disaster flag was applied.

Even among consumers who received the largest, boosts to their credit scores, I do not find evidence of flags improving these consumers' real economic outcomes of credit access. Instead credit access significantly declines. Flags not improving credit access is partially because the credit score increases I find are temporary: dissipating within twelve months and turning negative for some consumers. Furthermore, although disaster flags temporarily affect VantageScore credit scores (observed in my data), they do not affect FICO credit scores (unobserved in my data), and therefore credit decisions taken using FICO scores would be unaffected. While a growing literature studies the effects of disasters and government aid on household finances (e.g., Billings et al., 2022; Gallagher and Hartley, 2017; Farrell and

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Greig, 2018; Deryugina et al., 2018; Bleemer and van der Klaauw, 2019; Gallagher et al., 2023), my paper contributes to studying the effects of a little-known but widely-used form of relief.

In the last part of the paper, I consider counterfactual, mandatory social insurance regimes automatically masking all defaults in credit reports for consumers exposed to natural disasters. Such place-based policies may be motivated by redistributive aims (e.g., Gaubert et al., 2021) given the large and persistent geographic inequalities in financial distress across the US (e.g., Keys et al., 2022). I construct counterfactuals by merging in government data on the timing and location of natural disasters. I find these would mask 7 to 18% defaults from credit reports. Yet there appears little trade-off from doing so. Masking disaster defaults would have a small reduction in credit risk predictive performance – especially small relative to the large quantity of information masked. To help interpret the magnitude, I benchmark this against a scenario where all defaults (i.e. irrespective of whether during a disaster or not) are masked: an alternative which more noticeably reduces predictive performance. The limited trade-off of masking disaster defaults could make it proportionate policy – in a similar way to how medical debts are increasingly not included in credit reports due to fairness concerns with limited predictive loss.[2]

These counterfactuals provide evidence to inform public policy discussions pertaining to the potential for providing credit reporting relief from natural disasters (e.g., Banko-Ferran and Ricks, 2018; National Consumer Law Center, 2019; Urban Institute, 2019; FinRegLab, 2020). The topic of understanding the implications of masking information from credit reports is important more broadly. It has received greater public attention in the wake of the COVID-19 pandemic which resulted in laws in US via the Coronavirus Aid, Relief, and Economic Security (CARES) Act preventing lenders from updating adverse information in credit reports. The UK and Canada also introduced regulations in 2020 preventing worsening

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2. `https://newsroom.transunion.com/equifax-experian-and-transunion-support-us-consumers-with-changes-to-medical-collection-debt-reporting/` `https://vantagescore.com/major-credit-score-news-vantagescore-removes-medical-debt-collection-records-from-latest-scoring-models/` `https://www.whitehouse.gov/briefing-room/statements-releases/2023/02/14/fact-sheet-new-data-show-8-2-million-fewer-americans-struggling-with-medical-debt-under-the-biden-harris-administration/`

status of consumers' credit reports during COVID-19. In this context, understanding the role of positive information sharing is increasingly important (e.g., Guttman-Kenney and Shahidinejad, 2024a). Prior literature has studied the effects of changes in credit contract terms such as reductions of principal or monthly payments to alleviate consumer financial distress (e.g., Agarwal et al., 2017; Dobbie and Song, 2020; Ganong and Noel, 2020; Cherry et al., 2021). My study of credit report relief from natural disasters adds consideration of a new policy tool to prior literature. Disaster flags are a form of relief that do not change the contract terms, just how information on a contract is reported.

One can also consider my study of disaster flags in the context of the social insurance literature. One of the main roles of public policymaking is to provide social insurance: Providing insurance against adverse shocks such as becoming unemployed or suffering poor health (e.g., Feldstein, 2005; Chetty and Finkelstein, 2013) or distress from a natural disaster.[3] Deryugina (2017) shows the fiscal costs of social insurance payments (e.g., unemployment insurance, public medical payments) significantly outweigh direct disaster aid. It may be proportionate to use disaster flags to "tag" (e.g., Akerlof, 1978) a group of consumers affected by natural disasters. By empirically studying a voluntary form of social insurance that redistributes credit scores I add to the previous literature on social insurance which finds less use of tags than theory would recommend (e.g., Mankiw et al., 2009; Weinzierl, 2012). The counterfactual policies I consider are feasible place-based policies targeted to regions and time periods suffering financial distress.

The paper proceeds as follows. I explain the data used in my paper in Section 2.2. Section 2.3 provides a motivating framework for studying the credit information of natural disasters. I then explain the institutional details of disaster flags in Section 2.4. Section 2.5 documents new facts of how disaster flags are used. Section 2.6 shows the characteristics of consumers with disaster flags and examines the informational value of masking defaults. Section 2.7

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3. See Hsu et al. (2018); Bornstein and Indarte (2023); Braxton et al. (2023) for studies of the connections between social insurance and household debt.

evaluates the consumer benefits of disaster flags. I conduct counterfactuals in Section 2.8 for how alternative social insurance regimes masking defaults during natural disasters. Finally, Section 2.9 concludes.

2.2 Data

2.2.1 Consumer Credit Reporting Data

This research utilizes a large, anonymized, representative sample of US consumer credit reporting data: the University of Chicago Booth School of Business TransUnion Consumer Credit Panel (BTCCP). The BTCCP is provided by TransUnion to the University of Chicago Booth School of Businesses Kilts Center for Marketing (TransUnion, 2023).[4] These data are a 10% sample of consumers with a TransUnion credit report in July 2000 supplemented with 10% of new entrants added each month to ensure the sample remains representative.

Data are at the individual tradeline account level (e.g., a particular mortgage or credit card account) at the monthly frequency from July 2000 to December 2022. Each month of data is a "retro archive" that recreates the consumer's credit report as it would have appeared at that point-in-time and as lenders would take credit decisions on. Individual tradelines are tracked over time and linked to anonymized furnisher and anonymized consumer identifiers. From January 2009, these data contain more detailed data and so my research focuses on this period.

Each month these data also show a consumer's credit score: VantageScore 3.0. For each consumer, each month I observe their primary address geography with state, zipcode, and the census block group. Census block groups are units of geography typically containing 600 to 3,000 consumers and are more granular than census tracts. I keep observations for consumers with a birth date and restrict to where the birth year is before 1920 or after 2004

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4. TransUnion is one of three US credit bureaus along with Equifax and Experian. The BTCCP sample has been used in prior published research (e.g., Kluender et al., 2021; Guttman-Kenney et al., 2022; Keys et al., 2022).

and when a consumer never has tradeline data in order to remove low-quality, fragmented credit records.

2.2.2   Natural Disasters Data

When a disaster occurs, it is declared as such by the US President under the Stafford Act. I use public government data on these declarations provided by the Federal Emergency Management Agency (FEMA)'s Disaster Declarations Summaries.[5] I restrict analysis to natural disasters (e.g., flooding, hurricanes, wildfires, severe storms, tornados): this excludes chemical, toxic substances, terrorist, or other disasters events. These data report the timing and location of all federally declared disasters. These data are merged to BTCCP by county, state, and date.

2.3   Motivating Framework

I provide a stylized framework of credit scoring that provides the motivation for this paper. The basis of most lending decisions in the US and other countries are credit scores (derived from credit reporting data) predicting the likelihood of future default (missed payment). A credit applicant's credit score determines whether their application is accepted and, if so, what contractual terms are offered (e.g., interest rate and amount of credit). A higher credit score represents a lower probability of default (lower credit risk). Equation 2.1 shows a simple example, where a credit score is predicting at time t, an outcome $(Y_{i,t+j})$, the likelihood of consumer i defaulting on a credit agreement by j periods in the future i.e. $Y_{i,t+j} \equiv \max_{s=1}^{j} D_{i,t+s}$ where $D = 1$ if default, $D = 0$ if otherwise. The credit score has some generic function $f(.)$ – historically this is typically a logistic – where I have partitioned the inputs into a default component $(D_{i,t})$, and a vector of all other non-default inputs $(X'_{i,t})$ such as product holdings, balances, credit card utilization.

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5. https://www.fema.gov/openfema-data-page/disaster-declarations-summaries-v2

$$Pr\left(Y_{i,t+j} = 1\right) = f\left(X'_{i,t}\beta_1 + \theta_1 D_{i,t}\right) \tag{2.1}$$

In such models, past defaults are a strong predictor of future defaults with $\theta_1 > 0$. Consumers with past defaults have lower credit scores resulting in lower access to credit and facing higher interest rates.

As credit scores are predictive models the relationships between input and output variables are not causal. Credit scoring models regard the predictive value of a default as being homogeneous irrespective of the underlying cause or heterogeneity by socio-economic characteristics – even though doing so masks variation that may improve prediction. This is due to a mixture of a lack of data and legal constraints limiting the predictive accuracy of credit scoring models. For examples, lenders have limited visibility of life events such as income shocks and, for equity reasons, cannot discriminate on the basis of protected characteristics such as gender and race.

One source of heterogeneity that is observable in data and is not a protected characteristic is whether defaults differ with natural disasters (e.g., wildfires, floods, hurricanes). Does the ability to predict future defaults vary depending on whether a default occurs during a natural disaster or not? Equation 2.2 allows for this by including an interaction term between the default term (D_t) and a binary variable for the geographical area g and time t experiencing a natural disaster $(N_{g,t})$.

$$Pr\left(Y_{i,t+j} = 1\right) = f\left(X'_{i,t}\beta_2 + \theta_2 D_{i,t} + \pi(D_{i,t} \times N_{g,t})\right) \tag{2.2}$$

Comparing measures of predictive performance (e.g., AUROC) between the models in Equations 2.1 and 2.2 shows whether allowing for a differential impact of natural disaster defaults to other defaults can improve credit risk prediction.

The value of the π parameter in Equation 2.2 is informative of the marginal predictive value of defaults during natural disasters ("disaster defaults") compared to non-disaster de-

faults. It may be that $\pi < 0$ meaning that disaster defaults are lower risk than non-disaster defaults. This could be due to disasters being exogenous shocks to households, disasters disrupting communications making it difficult for households to make payments on-time, and households being better able to recover due to Federal assistance that may only arrive with a lag so be unable to prevent the original default. Conversely, disaster defaults may be higher risk ($\pi > 0$) than non-disaster defaults - possibly due to disasters causing longer-term damage to household resilience. If $\pi = 0$ and predictive performance does not improve then differentiating disaster defaults from non-disaster defaults does is not informative for improving credit risk prediction. If disaster defaults are uninformative noise then predictive performance may even be improved by masking such information.

Given this framework one can quantify how costly it would be for the credit industry to mask disaster defaults in credit reports. This adapts Equation 2.1 to Equation 2.3 where any disaster defaults are recorded as not in default. Comparing the predictive performance of these two models: if the difference is small the credit industry may voluntarily agree to mask disaster defaults, however, if it is large they would be reluctant to and then the government then has to decide the merits based on its social welfare function. While my study is of natural disaster defaults, this framework could be applied to evaluate other characteristics with richer data merged in e.g., defaults linked to life events such as divorce, income shocks, or expenditures shocks.

$$Pr\left(Y_{i,t+j} = 1\right) = f\left(X'_{i,t}\beta_3 + \theta_3 \tilde{D}_{i,t}\right), \text{ where } \tilde{D}_{i,t} \begin{cases} 0 \text{ if } N_{g,t} = 1 \\ \\ D_{i,t} \text{ otherwise} \end{cases} \tag{2.3}$$

Depending on how strict credit reporting practices are, there may be heterogeneity in whether lenders report disaster defaults. Some lenders may choose to mask disaster defaults for a variety of reasons. For example, doing so may provide the lender with private information advantage over its competitors, help to increase customer retention, or it may view it as being their preferred approach for helping distressed consumers for non-profit reasons. It

may also be that when borrowers default during a natural disaster, lenders learn private information on heterogeneous consumer circumstances and select a subset of disaster defaults to mask. With this framework in mind I study how firms currently mask information during natural disasters using "disaster flags".

2.4 What Are Disaster Flags?

Lenders may apply a "disaster flag" to a tradeline on their consumer's credit report to show the consumer has been affected by "natural or declared disasters". These flags were introduced following September 11, 2001 terrorist attacks. Disaster flags aim to provide relief to consumers by protecting credit access following natural disasters such as hurricanes, forest fires, and COVID-19.

There are no governmental or regulatory requirements for lenders to use disaster flags. The industry body's guidance by the Consumer Data Industry Association (CDIA) is not prescriptive on lenders use of disaster flags.[6] Lenders have complete discretion over whether to apply disaster flags and, if so, which consumers and tradelines to apply it to (e.g., all or a subset in an area subject to a natural disaster), and how many months to keep flags on a consumer's credit report for. Disaster flags are a separate field to the reporting of defaults in credit reports. Discussions with industry participants indicate some lenders sometimes do not report new defaults during natural disasters, however, is unclear how common such unreported defaults are. While not the focus of this study, such non-reporting of defaults may explain why average effects of natural disasters on defaults observed in credit reporting data found in prior literature have been described as "modest" (Gallagher and Hartley, 2017). Disaster flags may be applied instead of or in addition to changes in contract terms (e.g., deferring payments or offering forbearance) that may also get recorded in credit reports.[7]

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6. Credit Reporting Resource Guide (CRRG) FAQ 58 explains how these are recorded in credit reports with a comment code "AW" added to the tradeline. In TransUnion data the comment (remark) code is technically named "AND" instead of "AW".

7. CRRG FAQ 44 and 45 explain how these "accommodations" are recorded in credit reports by setting

Disaster flags mask negative information only on the flagged tradeline in the calculation of VantageScore: a widely-used, mainstream credit score.[8] Flags only mask information when they are currently present on an tradeline: once a flag is removed, masked information is revealed. Disaster flags do not factor into the calculation of FICO credit scores i.e. they do not mask negative information.[9] Manual underwriters reviewing a consumers' credit report observe disaster flags and may consider these in their credit decision.

There are potential parallels between the addition of a disaster flag to the removal of a bankruptcy flag 7 to 10 years after bankruptcy studied in many prior papers (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023). Both of these mask information in credit reports resulting in the pooling of consumers with different credit risks. One may even consider "disaster flags" as a type of temporary, low-cost bankruptcy providing a non-governmental form of social insurance for consumers affected by disasters.

2.5   Disaster Flag Facts

I document five new facts describing the use of disaster flags in US consumer credit reports over twenty years. I observe disaster flags in my credit reporting data (BTCCP). Each tradeline, each month these data show whether a disaster flag was applied. This monthly, tradeline level view is crucial. Disaster flags would not be visible in variables aggregated to the consumer-level. Quarterly (or annual) tradeline-level data, would not observe disaster flags applied intra-quarter unless flags were still present on a tradeline at the end of a quarter.

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payments due equal to zero or adding codes to show that the payment is deferred or the agreement is in forbearance. The Consumer Data Industry Association defines a deferred payment as "A loan arrangement in which the borrower is allowed to start making payments at some specified time in the future." and forbearance as: "A period during repayment in which a borrower is permitted to temporarily postpone making regular monthly payments. The debt is not forgiven, but regular payments are suspended until a later time...The consumer may be making reduced payments, interest-only payments or no payments."

8. https://vantagescore.com/newsletter/did-you-know-credit-reporting-and-natural-disasters/
https://vantagescore.com/2022-market-adoption-study/

9. https://www.fico.com/en/covid-19-credit-reporting-impact-US/

2.5.1 FACT 1. 59.2 million consumers had a disaster flag on their credit report between 2010 and 2020.

Across my entire dataset covering July 2000 - December 2022, 67.6 million consumers had a disaster flag on their credit report. This is a disaster flag on at least one open tradeline on their consumer credit report for at least one month. Between 2010 and 2020, 59.2 million consumers had a disaster flag.[10] To help to evaluate how "big" such numbers are: this is over 3.5 times the number of US consumers becoming bankrupt over 2010 to 2020.[11] The large number of consumers with disaster flags on their credit reports makes this an important practice to understand.

2.5.2 FACT 2. A level shift in disaster flag use in 2017 with Hurricanes Harvey and Irma.

Disaster flags were very rarely used until 2005 when Hurricane Katrina hit. Figure 2.1, Panel A displays disaster flag use has greatly increased over time. The growth is so large in 2017 that I separately present the 2000-2017 period in Panel B with a ten times smaller scale to be able to see this earlier period. The growth over time is consistent with Banko-Ferran and Ricks (2018) which examined Hurricane Harvey and found very few tradelines in Texas already had disaster flags in the months just before it hit.

Flags are applied across all mainstream credit types (e.g., auto loans, credit cards, mortgages, and student loans) and across all lender types (banks, non-bank finance companies, and credit unions) – more details are in Appendix Table 2.3 and Figures 2.11 and 2.12. Flags are typically applied to tradelines without deferments: except for student loans and during

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10. If closed tradelines are included this is 70.2 mn (2000 - 2022), 67.0 mn (2012 - 2022), 61.6 mn (2010 - 2020) consumers. This only open tradelines with positive balances are included this is 62.8 mn (2000 - 2022), 59.8 mn (2012 - 2022), 54.6 mn (2010 - 2020) consumers.

11. Credit reports contained 161 million bankruptcies with filing dates 2010 - 2020 based on chapter 7 or chapter 13 filings, dismissals, or discharges observed. For 2012 - 2022, it is over five times the number who became bankrupt: 64.4 million consumers had disaster flags compared to 12.6 becoming bankrupt.

COVID-19 when Federally-mandated payment deferments occurred more broadly. Deferments cover when deferments are listed on the tradeline or when tradelines have positive balances but zero payments due. Between 2009 and 2022, 15% of all tradelines (excluding student loans) that have disaster flags are also deferred at the same time (Appendix Figures 2.13 and 2.14). During the pre-COVID-19 period, 2009 to 2019 then only 6% are also deferred. Between 2009 and 2022, 75% of student loans with disaster flags are also deferred and between 2009 and 2019, this is 93%. While the focus of my study is of relief from natural disasters, there is little research evaluating the effectiveness of relief in credit reports beyond the COVID-19 pandemic (e.g., Cherry et al., 2021; Kim et al., 2022) and further investigation of this topic can help to inform lenders' practices and policymaking.

2.5.3   FACT 3. Broad geographic usage of flags.

In the 2000s and most of the 2010s credit report disaster flags were mainly used in the South East coastal areas which are more prone to hurricanes. Figure 2.2 displays the fraction of consumers in US counties with a credit report who had any disaster flag for each year 2015 to 2022. This pattern is still visible during 2016-2019 in Panels B to E of Figure 2.2. There were also some lower incidence pockets of usage elsewhere in the country in other months such as areas of the Northwest affected by California wildfires.

Panel F displays how disaster flags were used by lenders across the country in response to COVID-19 and other natural disasters in 2020. Coverage is broad based across counties, however, there is noticeable regional variation in the intensity of usage.

2.5.4   FACT 4. The majority of flags only remain on a credit tradeline for a few months.

Figure 2.3, Panel A shows the persistence of disaster flags remaining on a tradeline over time since a flag was first applied. It is at the discretion of lenders how long to keep disaster flags

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on credit report tradelines for. I observe, disaster flags typically only remain on a credit tradeline for up to three months and rarely more than six months. 33% of flagged tradelines are only flagged for one month, 74% for three months or less, 90% for six months or less, and 95% for twelve months or less. These results are broadly similar across lender types (Appendix Figure 2.15), and over time (Appendix Figure 2.16). Figure 2.3, Panel A, shows flags on auto loans are likely to remain on those tradelines slightly longer than occurs credit cards, mortgages, or student loans. This short duration limits the potential relief disaster flags can provide to consumers given consumers may experience disruption from disasters over a much longer time.

2.5.5   FACT 5. Flags are usually only applied to subset of consumers' credit tradelines.

Among consumers with flags, typically only a third of their tradelines on their credit report are flagged: 34% across 2009 to 2022 and 32% as of December 2022. Disaster flags are only attached to the individual tradeline accounts they are listed on. This means a consumer's entire portfolio only has disaster flags on it if all lenders add disaster flags to all their tradelines. I find this is a rare event: only 11% of consumers with at least one disaster flag on one tradeline have disaster flags on *all* of their open tradelines. It is the same both across 2009 to 2022 and as of December 2022. As only a subset of a consumers' tradelines are flagged, this limits the potential relief disaster flags can provide to consumers.

Figure 2.4 shows the intensive margin of flag use by the number of tradelines held: the fraction of a consumers' tradelines flagged (Panel A) and the share of consumers with all tradelines flagged (Panel B). Panel A shows the fraction of tradelines flagged decreases with the number of tradelines held. Panel B shows it is extremely rare (2% or less) for consumers with three or more tradelines to have flags on all of their tradelines. This indicates some frictions exist in the use of disaster flags although there is a slight trend of increasing intensity of use over time (Appendix Figure 2.17).

2.6 Informational Costs of Disaster Flags Masking Defaults

This section describes selection of consumers with disaster flags (2.6.1), then builds predictive models (2.6.2) applying the earlier conceptual framework (from Section 2.3) to quantify the information value contained in flagged defaults (2.6.3).

2.6.1 Describing Selection

What are the characteristics of consumers who have disaster flags? It is ambiguous whether selection into disaster flags will be advantageous or adverse and therefore I study this empirically. I examine this in Table 2.1 by comparing (I) consumers with disaster flags, to (II) consumers without disaster flags in the same geographical region (a combination of census block group and zipcode), and to (III) consumers unflagged across the US.

I find evidence of adverse selection of consumers with disaster flags. Consumers with disaster flags are ex-ante riskier with lower credit scores, with more defaults, and higher indebtedness than "unflagged" consumers without disaster flags. This selection holds both when comparing to unflagged consumers in the same geographical region and to unflagged consumers across the US.

2.6.2 Predictive Methodology

I take my motivational framework (Section 2.3) to my data to evaluate the credit risk costs of disaster flags masking defaults in credit reports. I do so by building a series of predictive models. My outcome (Y_{t+24}) is any *new* default (90+ days past due) in the next 24 months. I predict this outcome using data to October 2017 to ensure there is a large number of disaster defaults in my data. The model is trained on two-thirds of data and tested out-of-sample on the remaining third. $X_{i,t}$ contains non-default variables (e.g., balances, number of products, debts in collections, length of credit report, bankruptcy) that predict default. $D_{i,t}$ are the default variables: I use a variety of these – measuring the number of defaults

in the last 6, 12, 24, 36, and 84 months – to capture how the informativeness of historical defaults data for predicting new defaults may change over time. 84 months is the maximum duration defaults remain on credit reports for.

I construct my own credit scores in order to ensure I can vary the input data these depend on. I use a logistic regression rather than machine learning methods as the former is how most credit scoring models are historically constructed and because I am interested in not only the predictive performance but comparing the coefficients on default parameters.

My baseline predictive model (2.4) is a traditional credit score including default information. My second model (2.5) adds an interaction term for defaults flagged by disaster flags. Comparing the coefficients between models informs of informativeness of this variable. The final model (2.6), adjusts the input data to mask all defaults covered by disaster flags: reclassifying such "flagged defaults" as not in default. Comparing the predictive performance of model 2.4 to model 2.6 shows the information costs of masking defaults.

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_1 + \theta_1 D_{i,t}\right) \tag{2.4}$$

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_2 + \theta_2 D_{i,t} + \pi(D_{i,t} \times FLAG_{i,t})\right) \tag{2.5}$$

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_3 + \theta_3 \tilde{D}_{i,t}\right), \text{ where } \tilde{D}_t \begin{cases} 0 \text{ if } FLAG_{i,t} = 1 \\ D_{i,t} \text{ otherwise} \end{cases} \tag{2.6}$$

2.6.3 Predictive Results

Figure 2.5 shows the average marginal effects from Equation 2.5: θ_2 in black and π in red. I find $\theta_2 > 0$ which means defaults in the past increase the risk of a consumer defaulting in the future. For flagged defaults in the last six months, $\pi > 0$ and much larger than θ_2: showing that recent flagged defaults are economically and statistically significantly higher

risk. For flagged defaults further back in time, π is typically statistically insignificant from zero.

I interpret this as flagged defaults quickly lose their information value at predicting future default. This interpretation is corroborated by comparing the out-of-sample predictive performance measured by AUROC (shown in Table 2.2) from the baseline credit risk model (0.8790) to one masking flagged defaults (0.8786). Masking flagged defaults therefore reduces predictive performance by approximately 0.05%. Such an economically small cost of reduced prediction can explain why lenders voluntarily, temporarily apply disaster flags.

2.7   Consumer Benefits of Disaster Flags

What are the benefits to consumers of having disaster flags on their credit report? I study this using an event study design with the methodology explained in Subsection 2.7.1). I then show descriptive results from this for defaults (2.7.2), credit scores (2.7.3), and credit access (2.7.4). Finally, I use a difference-in-differences methodology (2.7.5) and present results on credit access 2.7.6.

2.7.1   Event Study Methodology

My event study methodology exploits the timing of disaster flags being applied being quasi-random as a function of the timing and geography of natural disasters. This descriptive methodology evaluates how much consumers' finances have changed relative to their pre-disaster levels. I take the first time a consumer has a disaster flagged applied to their credit report. I exclude consumers where the first time they are flagged only occurs for a student loan since these commonly, contemporaneously have payments deferred. I keep cohorts January 2010 to December 2018 to ensure I observe sufficient pre and post periods of each cohort and to exclude cohorts affected by COVID-19 disruptions. I retain consumers with open tradelines with positive balances and credit scores observed twelve months before first

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being flagged as a group of active consumers.

This produces a dataset of 2.8 million consumers representative of 28 million consumers. For all of these consumers I construct a balanced panel of 25 months showing twelve months pre and post flags being first applied. This time window is driven by my descriptive evidence given disaster flags only remain on credit reports for a short period of time, any economic effects are expected to be observed within twelve months.

I show results across flagged consumers and heterogeneous effects based on two measures of pre-disaster financial distress. The first heterogeneous measure is whether a consumer's credit score twelve months before first being flagged was a low score "subprime" (300 - 600) or high score "non-subprime" (601 - 850). 11.4% of consumers in my sample are subprime. The second heterogeneous measure is whether a consumer has any defaults (30+ days past due) on open tradelines with positive outstanding balances on their credit report twelve months before first being flagged. 5.5% of consumers in my sample have any defaults. These two measures are highly correlated: 76.3% of consumers with any defaults have subprime credit scores, 37.1% of consumers with subprime credit scores have any defaults.

This heterogeneity is motivated by prior research showing effects of natural disasters on consumers' finances vary by pre-disaster financial distress (e.g., Billings et al., 2022).[12] The second measure is also motivated by the institutional details of flags where one would expect the potential gains from using disaster flags to be largest for those with defaults that flags mask.

To assist with interpreting these charts, I add linear time trends to credit score event studies which may be a reasonable counterfactual over a short time horizon for how consumers' credit scores would have evolved without a natural disaster or disaster flag occurring (see Dobbie et al., 2020; Gross et al., 2020, for examples using similar approaches). Linear time trends are calculated from OLS regressions on data t-12 to t-1.

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12. Cookson et al. (2023) shows informal crowdfunding via social networks after wildfires are regressive and exacerbates inequality.

2.7.2   Masking Defaults

I examine the mechanism through which disaster flags can affect credit scores and credit access: masking defaults. The first stage of my event study analysis is presented in Figure 2.6 examines the prevalence of defaults on credit reports. The black line on Figure 2.6, Panel A shows the fraction of consumers with any defaults before flag masking in event time. This trends slightly up over time. The orange line masks defaults that occur on tradeline months where flags also appear. Flag masking immediately reduces the fraction of consumers with any defaults appearing on their credit report by 1.5 percentage points. However, it does not go to zero but remains at 5 percent: showing these consumers have defaults on other tradelines without flags and so remain unmasked. The two default series quickly converge within twelve months showing that any potential benefit of flags masking defaults is temporary.

These small, average results are largely driven by the consumers experiencing pre-disaster financial distress. Panels B and C repeat this for heterogeneous cuts of the data by pre-disaster financial distress where t-1 values of defaults are normalized to 0. This shows the temporary effects are concentrated among consumers with pre-disaster financial distress: subprime credit scores or those with any defaults. Flags masks defaults for approximately ten percent of pre-disaster subprime consumers and fifteen percent of consumers with any pre-disaster defaults. But masking of defaults is still only temporary for these groups. There is no discernible difference in defaults before or after masking among consumers without pre-disaster financial distress.

This evidence indicates that any positive effects of flags on credit scores and credit access would be expected to be concentrated on consumers experiencing pre-disaster financial distress and only occur with a few months of the flag being first applied.

2.7.3   Credit Score

How does applying disaster flags affect credit scores? Figure 2.7, Panel A finds an average increases to credit scores of 2 points in the month the flag was applied and by 1.5 points after twelve months. This is an increase of 0.2 percent relative to the t-1 baseline mean (717) and is little different from that predicted by a linear pre-trend. This average change in credit scores is too small to generate economically meaningful differences in credit access. This change is economically small relative to the approximately 15 points average increase from removing bankruptcy flag from credit reports (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023). One might not expect the effects of disaster flags to be as large as the effect of removing bankruptcy flags as (i) bankruptcy is an extreme form of financial distress; and (ii) the average positive effect of disaster flags is expected to be a dilution of a larger positive effect from the subset of consumers with defaults.

Panels B and C show how the most financially distressed consumers – those with subprime credit scores or any defaults – receive the largest increases to their credit scores from disaster flags (results for all credit score segments in Appendix Figure 2.19). These panels normalize credit scores for each subgroup relative to their t-1 baseline mean. Financially distressed consumers experience increases of 11 points for subprime consumers (relative to the t-1 baseline mean of 571) and 16 points for consumers with defaults (relative to the t-1 baseline mean of 584). Such effect sizes are similar in magnitude to the effects of bankruptcy flag removal which, in turn, had real effects. However, such increases in credit scores appear short-lived. Within twelve months credit scores become *lower* than that predicted by a linear pre-trend or relative to a control in a differences-in-difference approach. Credit scores of consumers without pre-disaster financial distress are effectively flat: increasing by 0.8 to 0.9 points on baselines of 737 and 726 respectively for Non-Subprime and No Defaults groups.

It is ambiguous whether such temporary increases to credit scores will translate into improved credit access for financially distressed consumers experiencing non-trivial credit

score increases. In general, an increase in credit scores would be expected to increase credit access. Our results only apply to VantageScore credit scores which consider disaster flags in their calculation. While I do not observe FICO credit scores, it is reasonable to assume the effects on these would be zero since they do not consider disaster flags in their calculation.[13] This means credit decisions by lenders who use FICO credit scores would be unaffected by disaster flags unless lenders also undertook manual underwriting examining a consumer's raw credit report. Effects through VantageScore are only likely to occur if a financially-distressed consumer applies for credit within a few months of the disaster flag being added during the short time they experience a temporary boost to their score.

2.7.4 Credit Access

I do not find disaster flags improve credit access for consumers who experience pre-disaster financial distress who receive the largest boosts to their credit scores.

I examine the extensive margin of credit access using new account openings. New account openings often have lags of several months before they are recorded on a consumer's credit report (Gross et al., 2021; Gibbs et al., 2024). To address this, I use the variable recording an account's opening date to create a new time series of account openings (rather than the credit report archive date) and record zeros for months where no accounts are opened.

Figure 2.8, Panel A shows account openings decline over time and there is no sign of improvement following disaster flags being applied. If anything, there are slight *decreases* in credit access with new account openings falling below their linear pre-trend.

Figure 2.8, Panels B and C show similar conclusions for the consumers experiencing pre-disaster financial distress: there is no sign of improved credit access with estimates remaining around zero relative to the t-1 baseline mean of 0.11. If anything, there is possibly a slight reduction relative to a linear pre-trend.

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13. `https://www.fico.com/en/covid-19-credit-reporting-impact-US/`

2.7.5   Difference-in-Differences Methodology

I estimate the causal effects of adding disaster flags to a credit report on consumers using a stacked difference-in-differences empirical design (Cengiz et al., 2019; Deshpande and Li, 2019). Dube et al. (2023) show this stacked approach is equivalent to using a local projections estimator and it corrects for potential bias of negative weighting arising in designs with staggered, heterogeneous, or dynamic treatments. This stacked difference-in-differences approach, differences out the contemporaneous effects of the natural disaster to leave only the effects of the disaster flag. This methodology exploits the timing of disaster flags being applied as a quasi-random function of natural disasters.

I keep consumers that first received a disaster flag between January 2010 and December 2018. This stacked difference-in-differences empirical design stacks data from each flag event study and, for each event, constructs a clean control group of "unflagged" consumers who are never flagged between July 2000 and January 2020. The control group is constructed using variables calculated twelve months prior to the date the flagged group is first flagged. The clean control of unflagged consumers are in the same combination of geographic area (same census block group × zipcode), pre-disaster credit score group, and any pre-disaster defaults to the flagged consumer. Within that combination I keep unflagged consumers who are the nearest neighbor in Euclidian distance by standardized credit score, credit card limit, number of trades, outstanding balances, and outstanding mortgage balances. I keep cases where flagged consumers have controls that are close matches (Euclidean distance less than or equal to one) which retains 67% of flagged consumers. This leaves me with a dataset of cohorts of consumers where each flagged consumer is matched with one unflagged consumer. This results in a dataset of 3.77 million consumers representative of 37.7 million consumers. For each of these consumers I take twelve months of observations before and twelve months twelve months after the flagged event to create a balanced panel of observations: 25 months per consumer stacked into a single dataset.

I estimate regression shown in Equation 2.7 for individual consumer i in cohort c at time

t. This regression includes fixed effects for each cohort-by-calendar-year-month ($\gamma_{c,t}$) and for each consumer (γ_i). $FLAG_i$ is an indicator taking a value of 1 if a consumer is in the flagged group and a value of 0 if a consumer is in the unflagged control group. Standard errors are clustered at the cohort-level.

$$Y_{i,c,t} = \sum_{\tau \neq -1} \delta_\tau \left(FLAG_i \times D_{c,t}^\tau \right) + \gamma_i + \gamma_{c,t} + \varepsilon_{i,c,t} \tag{2.7}$$

The parameters of interest are δ_τ which is the interaction on event time dummies ($D_{c,t}^\tau$) and the $FLAG_i$ indicator. Under the assumption of common trends δ_τ estimates the effect of disaster flags, among those selected in with suitable controls, on outcomes ($Y_{i,c,t}$) after τ months.

2.7.6   Difference-in-Differences Results

In line with my event study results, I find disaster flags cause a 1.94 point (s.e. 0.20) average increase to Vantagescore credit scores (relative to a baseline of 723) that dissipate to being insignificant from zero within four months (Appendix Figure 2.20, Panel A). After twelve months the effects are insignificantly different from zero (-0.38, s.e. 0.28).

The average, temporary positive effect of credit scores are driven by consumers experiencing pre-disaster financial distress. Figure 2.9, Panel A shows at t = 0, consumers with any pre-disaster defaults experience a 3.3% estimated increase in credit score of 18.29 points (s.e. 1.19, baseline mean 556). As credit scores are non-linear predictors of default risk, an 18 point increase for a consumer with a score of 550 is expected to be more valuable than a similarly sized increase for consumers with higher scores. Consumers without pre-disaster defaults experience an increase of 1.31 points (s.e. 0.16) which is less than 0.2% of the baseline mean (730). Effects for both groups dissipate within twelve months to be insignificant from zero. After twelve months the estimated effects for those with any pre-disaster defaults is -1.64 (s.e. 0.96) and for those without pre-disaster defaults is -0.34 (s.e.

0.27). Appendix Figure 2.21, Panel A shows results by credit score group: positive effects are concentrated among consumers with subprime credit scores peaking at t = 0 at 10.86 points (s.e. 1.20), an increase of 1.9% relative to baseline These increases among subprime consumers are short-lived and turn significantly negative within twelve months.

I find no effects of flags increasing credit access. I find no positive effect of flags increasing credit access even for the consumers who received the largest temporary boosts to their credit scores: those with pre-disaster defaults. There is no significant effect on whether a consumer opened any new credit card account each month. Figure 2.9, Panel B shows credit access declines for both consumers with and without pre-disaster defaults. After twelve months the estimated effects for those with any pre-disaster defaults is significantly negative -0.0042 (s.e. 0.0011): a decline of 23% relative to the baseline mean. The effect for those without pre-disaster defaults is also significantly negative -0.0086 (s.e. 0.0013): a decline of 18% relative to the baseline mean. Results are not specific to new credit cards: examining the number of new accounts opened across all credit types shows the same pattern of results (Appendix Figure 2.22).

There does not appear to be an effect on the intensive margin: Figure 2.9, Panel B shows no positive effect on the value of new credit card limits. On this margin, consumers with pre-disaster defaults experience no significant difference (estimate -$5.26, s.e. $7.53) relative to their t-1 mean of $66.92 ($2,828 conditional on any new credit card being opened). Whereas consumers with pre-disaster defaults experience a significant decrease of 25% (estimate -$107.30, s.e. $18.20) relative to their t-1 mean of $434.70 ($11,749 conditional on any new credit card being opened).

Results on credit access are consistent when segmenting by credit score (Appendix Figure 2.21, Panels B and C) and when averaging across all consumers (Appendix Figure 2.20, Panels B and C).

Flags not improving credit access may be because credit score boosts are not for a long enough period of time for consumers to realize the potential benefits. Furthermore, although

disaster flags temporarily affect VantageScore credit scores (observed in my data), they do not affect FICO credit scores (unobserved in my data), and therefore credit decisions taken using FICO scores would be unaffected. Credit cards are a domain where any positive effects of disaster flags on credit access are most likely to show up as VantageScore is commonly used for credit cards whereas FICO is predominately used for mortgages. This motivates considering a counterfactual system to preserve the credit access of consumers affected by disasters by pooling them with consumers unaffected by disasters.

2.8   Informational Losses From Counterfactuals Masking Disaster Defaults

The existing voluntary regime of disaster flags appears to have limited costs to lenders or benefits to consumers' credit access. What are the feasible alternatives? I quantify the informational losses from counterfactual government policies automatically requiring masking of all defaults during disasters ("disaster defaults"). These counterfactuals are designed to apply equitably to all consumers subject to a disaster: removing selection. Doing so pools consumers affected by disasters with those unaffected by disasters.[14] They are also designed to provide more permanent relief by masking defaults for longer periods of time than disaster flags typically do. Such counterfactual policies have been proposed by consumer organizations (e.g., National Consumer Law Center, 2019; Urban Institute, 2019; FinRegLab, 2020).

If such counterfactual policies were required by law they would affect the underlying credit reporting data which all credit scores (e.g., FICO, VantageScore) and manual underwriters rely on and therefore would be expected to have downstream impacts on consumers' credit access. While I cannot estimate supply responses, the prior research of Cortés and Strahan (2017) studying supply responses to natural disasters more generally may be indicative of

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14. Practically implementing this would mean disaster defaults are masked before they appear on credit reports so that disaster defaults cannot be observed by lenders in their credit decisions. This could either be required of firms furnishing data to credit reporting agencies or required of the credit reporting agencies before they release data to be used by lenders.

lenders being willing to meet increased local credit demand. I consider a larger predictive loss from masking disaster defaults indicates lenders are more likely to restrict credit supply whereas lenders may be expected to more easily absorb a small predictive loss.

To evaluate this I first examine the coefficients (ϕ) on the interaction term between defaults ($D_{i,t}$) and natural disasters ($FEMA_{g,t}$) in the regression specified in Equation 2.8. I define defaults as exposed to a natural disaster if the consumer (i) resides in county g where a disaster was declared in the last six months (i.e. $FEMA_{g,t} = 1$ if exposed to a disaster in the last six months, 0 otherwise). This interaction term informs about whether disaster defaults are different from other defaults in informing of a consumer's future risk.

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_4 + \theta_4 D_{i,t} + \phi(D_{i,t} \times FEMA_{g,t})\right) \tag{2.8}$$

Figure 2.5 shows the coefficients from this regression. All the coefficients on disaster defaults are lower than those on all defaults. The disaster defaults terms are generally insignificant from zero but positive over the longest window: 84 months. I interpret this as recent disaster defaults are not especially informative of a consumer's future risk of default. This leads me to evaluate policies masking disaster defaults.

I consider two feasible government policies automatically masking all disaster defaults. Both of these tag consumers based on whether they reside in an area when it was affected by a natural disaster: as this is based on historical location it limits the potential for consumers moving to an area to gain relief. By tagging all consumers in an area affected by natural disasters this is designed to be a more equitable approach than disaster flags where there is adverse selection and unequal potential access to relief based on idiosyncratic lender policies. I tag defaults that occur in disaster areas and disaster time periods over the last 84 months.

The first policy ("Temporary Masking") automatically masks all defaults that occur for consumers residing in a county FEMA reports to be affected by a natural disaster. It masks defaults for six months from the date of disaster (see Equation 2.9). This is designed to provide temporary relief for consumers – especially those more financially distressed pre-

disaster who are most likely to experience disaster defaults. Such an approach would ensure that any defaults during that six month period only affect subsequent credit access if they are present after six months. This limits the ability of temporary adverse shocks to propagate to have long-term impacts. Six months is chosen as a time window long enough to capture temporary financial distress and for consumers to be able to access relief. It is longer than most disaster flags are currently applied for: which appears too short to deliver real benefits. It provides time for consumers to be able to apply for and receive Federal social insurance and disaster aid as well as contacting their creditors to adjust payments if required. It is also short enough to limit potential moral hazard of consumers strategically defaulting. Although empirically moral hazard appears less of a concern than life events (e.g., Low, 2023; Ganong and Noel, 2023) with consumers also being motivated to repay by non-financial concerns (e.g., Bursztyn et al., 2019). More broadly, applying such an automatic flagging approach could be considered more equitable – removing frictions for lenders and consumers – and easier to understand than the existing, ad hoc approach taken by lenders.

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_5 + \theta_5 \tilde{D}_{i,t}\right), \text{ where } \tilde{D}_{i,t} \begin{cases} 0 \text{ if } \left(D_{i,t} \times FEMA_{g,t}\right) > 0 \\ D_{i,t} \text{ otherwise} \end{cases} \quad (2.9)$$

The second policy ("Permanent Masking") is designed to provide an upper bound on the amount of disaster defaults masked. Some defaults caused by disasters could take longer than six months to be occur. Or some disaster defaults may be persistently reported in my historical data for more than six months but in a counterfactual world may not be. To address these issues, if a tradeline (d) experiences a new default during the six month window my second measure masks defaults not only during that six months but for subsequent tradeline months (see Equation 2.10).

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_6 + \theta_6 \tilde{D}_{i,t}\right), \text{ where} \quad (2.10)$$

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$$\tilde{D}_{i,t} : max \sum_{d=1}^{D} D_{d,i,t} = \begin{cases} 0 \text{ if } \left(\sum_{j=0}^{t} \mathbf{1}\{\Delta D_{d,i,j} = 1\} \times FEMA_{g,j} \right) > 0 \\ D_{d,i,t} \text{ otherwise} \end{cases}$$

These two measures respectively remove 6.7% and 18.4% of all default data present in US credit reports in the seven years of data to October 2017. This is an economically large removal of data from credit reports. Two to four percent of consumers go from some defaults to no defaults in their seven year credit history under these two measures.

I evaluate predictive performance comparing these two models that mask disaster defaults (Equations 2.9 and 2.10) benchmarking relative to two extremes: a baseline credit scoring model that uses all defaults data to represent the status quo (Equation 2.4) and a counterfactual model without any default data (Equation 2.11) which is also a more intrusive policy one could consider. Across these regressions I use the same baseline outcome of new defaults without masking as this is the outcome that would ultimately impact lenders.

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_7 \right) \tag{2.11}$$

Masking disaster defaults reduces the credit risk predictive performance as measured by AUROC from 0.8790 in the baseline to 0.8777 (temporary masking) and 0.8764 (permanent masking) as shown in Table 2.2. Figure 2.10 presents the ROC curves where the difference from masking disaster defaults is barely noticeable. These effects of masking disaster defaults can be benchmarked against a counterfactual policy masking all defaults (i.e. disaster and non-disaster defaults) that significantly reduces predictive performance with AUROC declining to 0.8641 and a clear gap in the ROC curve relative to the baseline. I therefore conclude that counterfactual policies masking disaster defaults offer little trade-offs. Such policies would only slightly reduce lenders' abilities to predict future defaults by 0.15 to 0.30%. This reduction in predictive performance appears especially small relative to the quantity of disaster defaults masked (6.7 to 18.4%) and therefore appears a proportionate

policy to help provide credit reporting relief for consumers from natural disasters.

2.9   Concluding Discussion

This research provides new evidence of the widespread use of "disaster flags" on credit reports that are intended to provide relief to consumers affected by natural disasters. This research advances understanding of the informativeness of past defaults in predicting consumers' future behaviors. My consideration of counterfactual policies masking defaults during disasters can help inform policy discussions on how to design credit information markets and alleviate financial distress from natural disasters.

2.10   Figures and Tables

Figure 2.1: Consumers with any credit report disaster flag

A. July 2000 - December 2022

B. July 2000 - July 2017
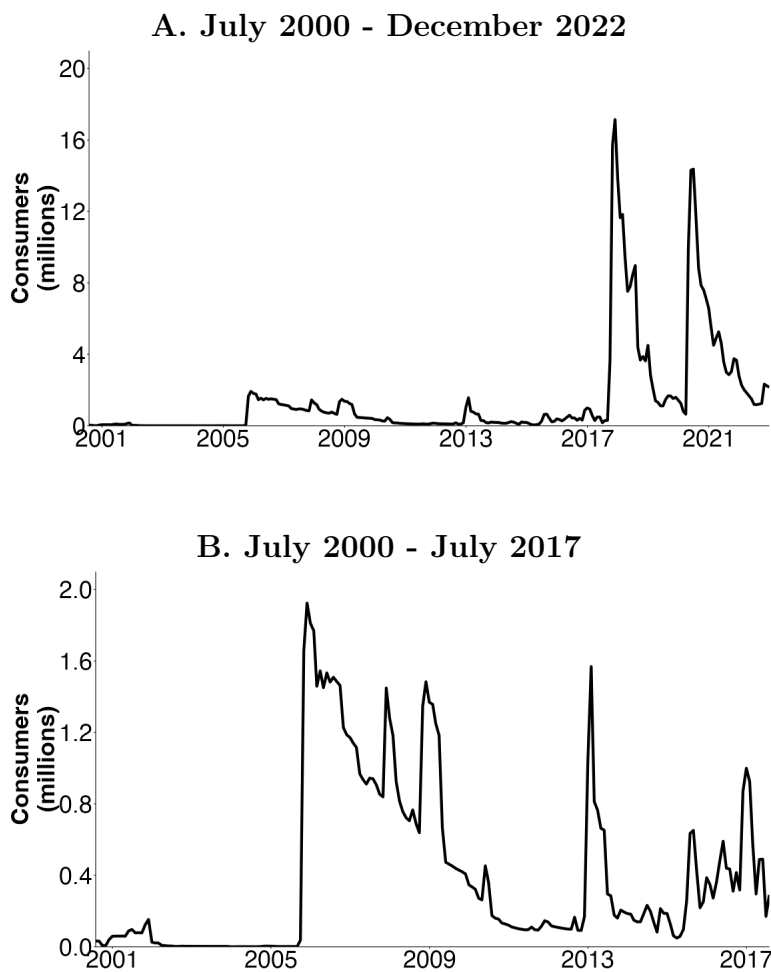
Notes: TransUnion data. Consumers with a credit report disaster flag on at least one open tradeline in their credit report. Numbers extrapolated to population estimates from 10% sample.

Figure 2.2: Fraction of consumers in a county with any credit report disaster flag

A. 2015 (August)

B. 2016 (December)

C. 2017 (November)

D. 2018 (January)

E. 2019 (January)

F. 2020 (June)

G. 2021 (January)

H. 2022 (October)
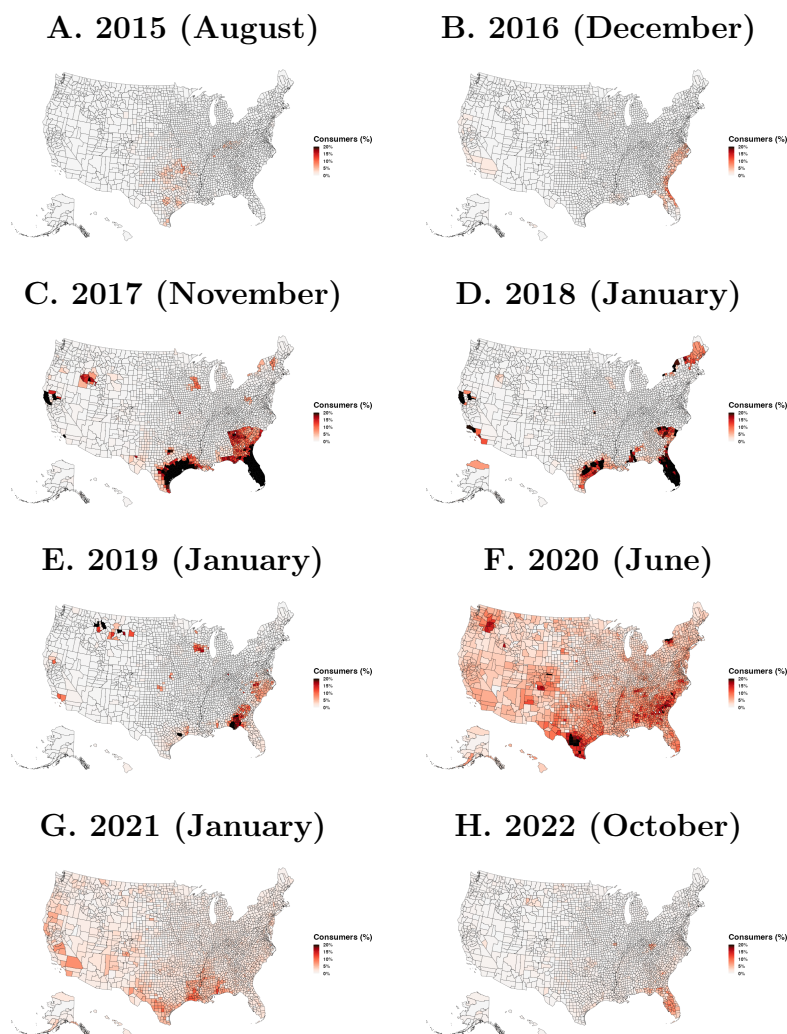
Notes: TransUnion data. Denominator is number of consumers with an open tradeline with a positive balance on their credit report in a county that month. Numerator is the subset of these consumers with a credit report disaster flag on at least one of these tradelines that month. Values in each county are top-coded at 20%. Months shown are those with the highest number of consumers with disaster flags in each year.

Figure 2.3: Persistence of disaster flags on credit report tradelines: aggregated (Panel A) and by credit type (Panel B)
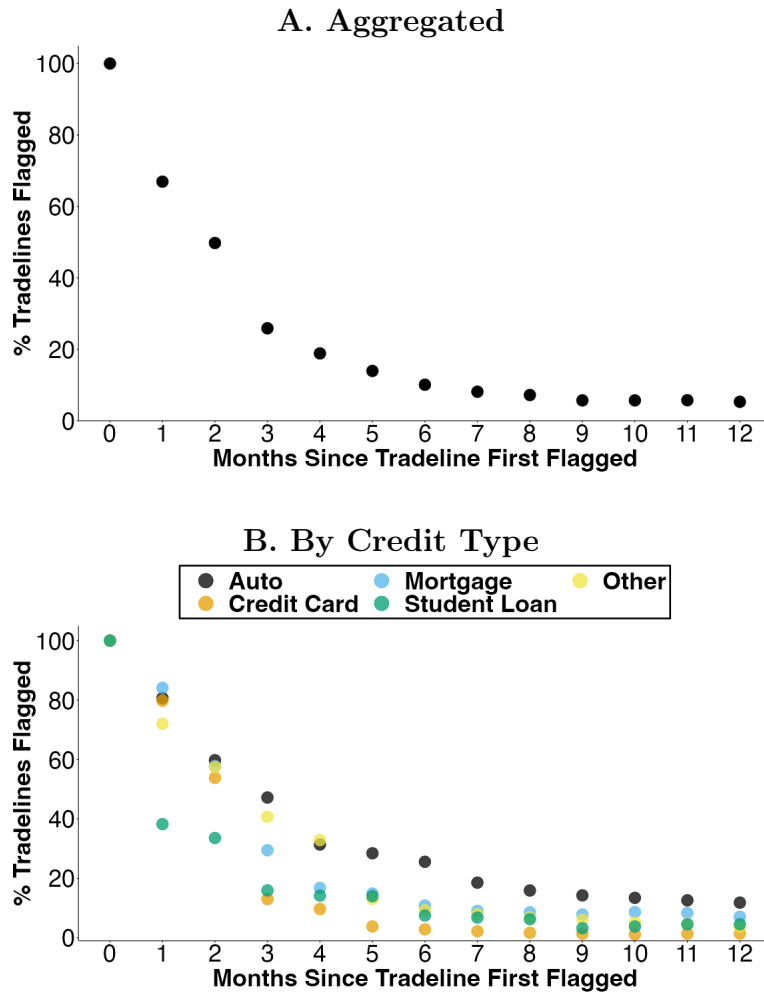
A. Aggregated

B. By Credit Type

Notes: TransUnion data. This takes open credit report tradelines with positive balance that first have a disaster flag added between February 2009 to December 2021. Plots the fraction of these with disaster flags still present 1 to 12 months later. Panel A shows for all disaster flags. Panel B splits by credit type where 'other' contains retail cards and unsecured loans.

Figure 2.4: Intensive Margin: Among consumers with disaster flags, mean fraction of tradelines flagged (Panel A) and fraction with all tradelines flagged (Panel B), split by number of tradelines
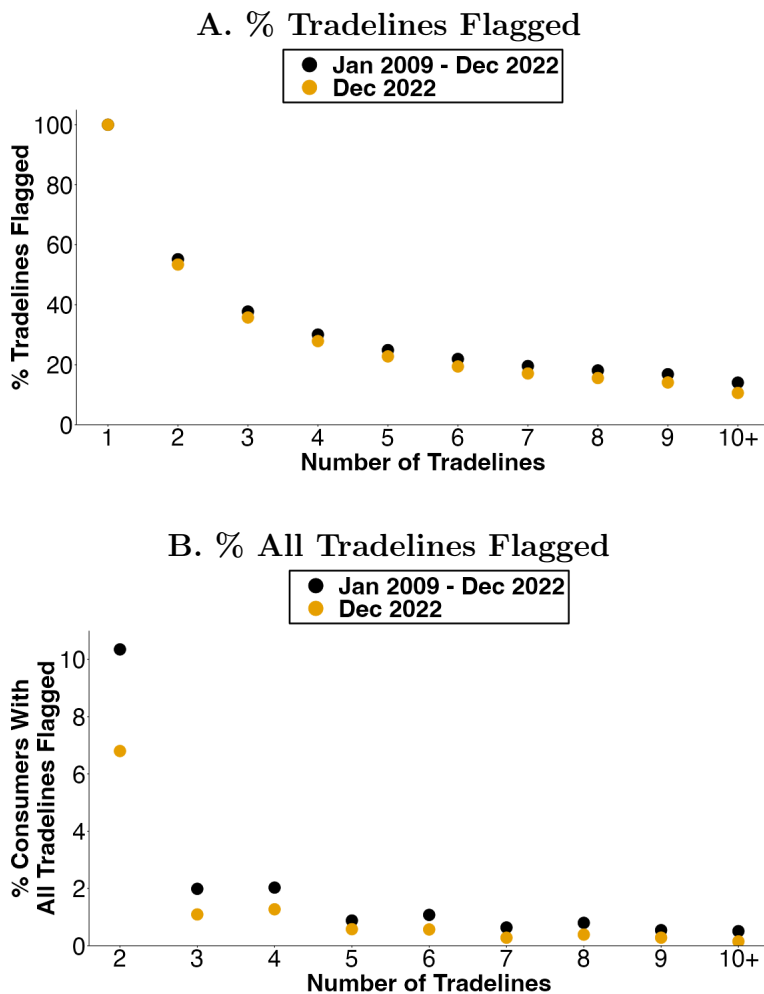
A. % Tradelines Flagged

B. % All Tradelines Flagged

Notes: TransUnion data. Both Panels A and B restrict to consumer months where the consumer has a credit report disaster flag on at least one open tradeline with a positive balance in their credit report. Panel A shows for these consumers, the mean number of tradelines with a credit report disaster flag. Panel B shows the fraction of these consumers where all their tradelines have credit report disaster flags. X axes on both panels plots number of open trades with a positive balance a consumer has on their credit report. Statistics shown combining observations Jan 2009 - Dec 2022 (black) and also for December 2022 only.

Figure 2.5: Average marginal effects of coefficients predicting future default

A. All Defaults (black), Disaster Flag Defaults (red), and FEMA Disaster Defaults (blue)
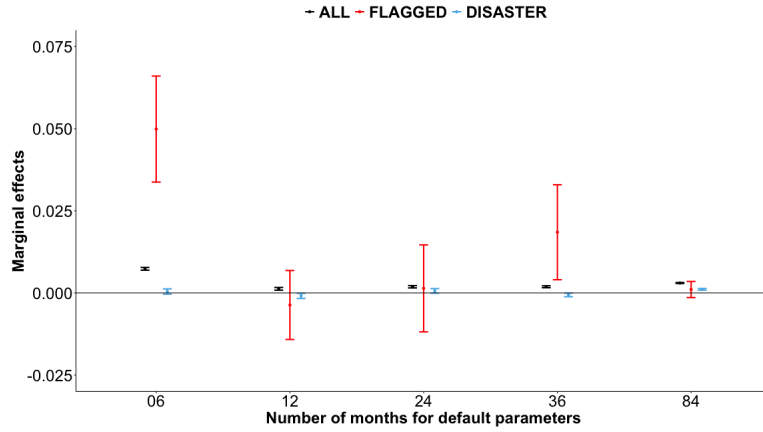
B. All Defaults (black) and FEMA Disaster Defaults (blue)
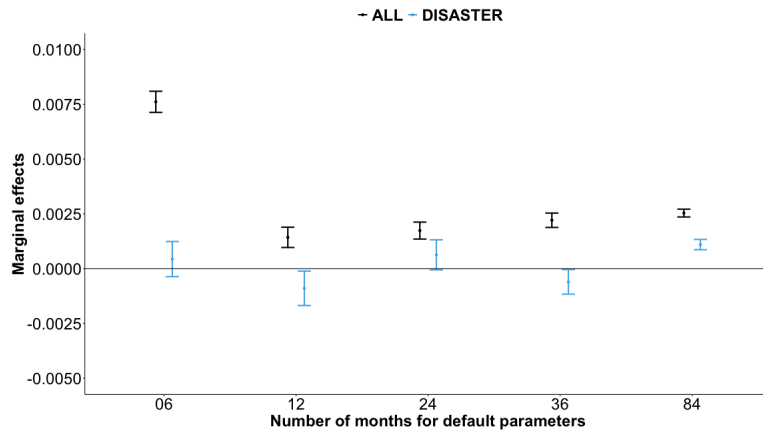
Notes: TransUnion data. X axis show coefficients on parameters with any defaults in last 6, 12, 24, 36, and 84 months respectively. Y axis shows average marginal effects on coefficients from logistic regressions predicting any new defaults in the next 24 months. Black are θ coefficients on default term (D_t) from Equation 2.4, red are π coefficients on default term (D_t) interacted with disaster flag indicator ($FLAG_t$) from Equation 2.5, and blue are ϕ coefficients on default term (D_t) interacted with FEMA indicator ($FEMA_t$) from Equation 2.8.

Figure 2.6: Event study of percent of consumers with defaults on credit reports before (black) and after (orange) flag masking for: (A) all flagged consumers, (B) by pre-disaster credit score, (C) by any pre-disaster defaults
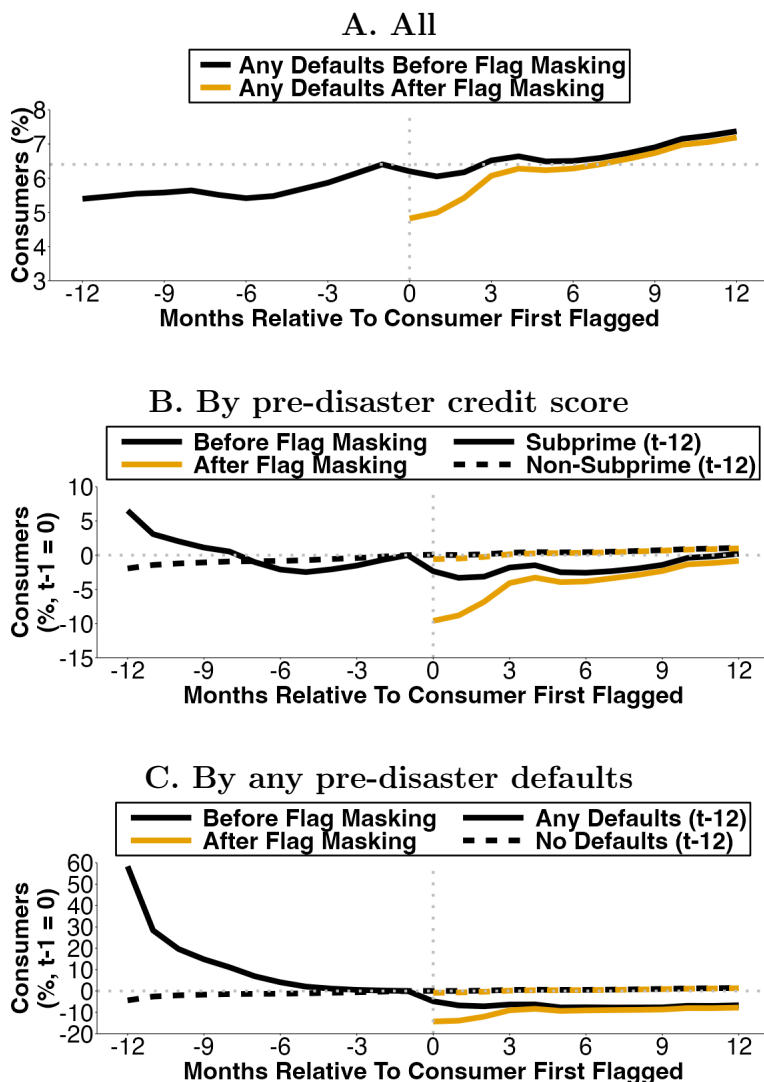
A. All

B. By pre-disaster credit score

C. By any pre-disaster defaults

Notes: TransUnion data. Unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. X axis shows months since consumer first flagged. Y axis shows fraction of consumers with any defaults before (black) and after (orange) tradeline months where defaults masked by flags. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each series to t-1 = 0.

Figure 2.7: Event study of VantageScore credit scores relative to linear pre-flag time-trends for: (A) all flagged consumers, (B) by pre-disaster credit score, and (C) by any pre-disaster defaults

A. All

B. By pre-disaster credit score

C. By any pre-disaster defaults
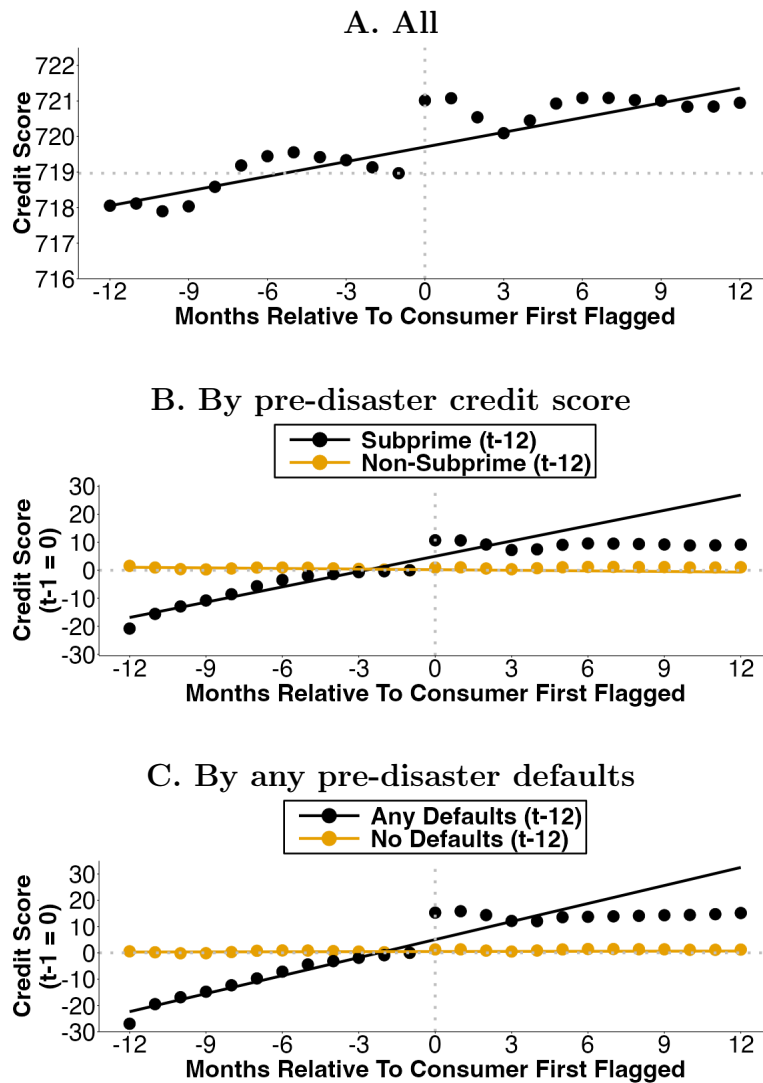
Notes: TransUnion data. Dots are unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. Lines are linear time trend from OLS regressions on data t-12 to t-1. X axis shows months since consumer first flagged. Y axis shows VantageScore credit score. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each credit score to t-1 = 0.

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Figure 2.8: Event study of new account openings relative to linear pre-flag time-trends for: (A) all flagged consumers, (B) by pre-disaster credit score, and (C) by any pre-disaster defaults

A. All

B. By pre-disaster credit score

C. By any pre-disaster defaults
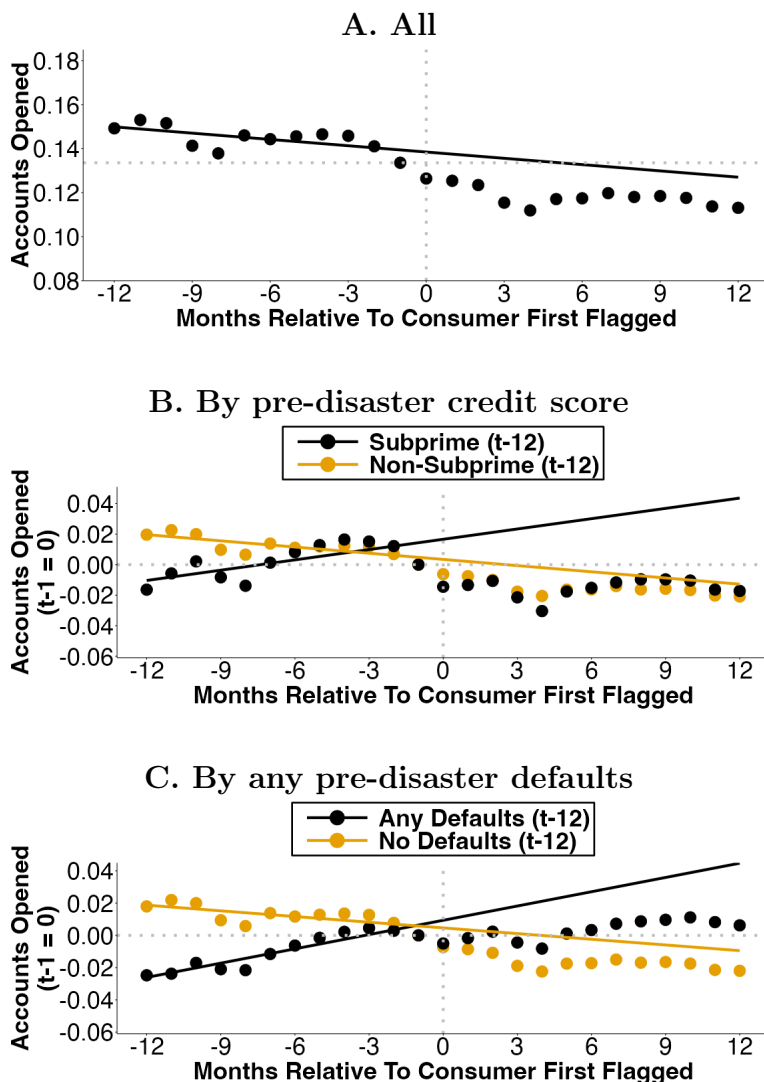
Notes: TransUnion data. Dots are unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. Lines are linear time trend from OLS regressions on data t-12 to t-1. X axis shows months since consumer first flagged. Y axis shows new account openings. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each credit score to t-1 = 0.

Figure 2.9: Difference-in-differences estimates of effects on credit access by any pre-disaster defaults
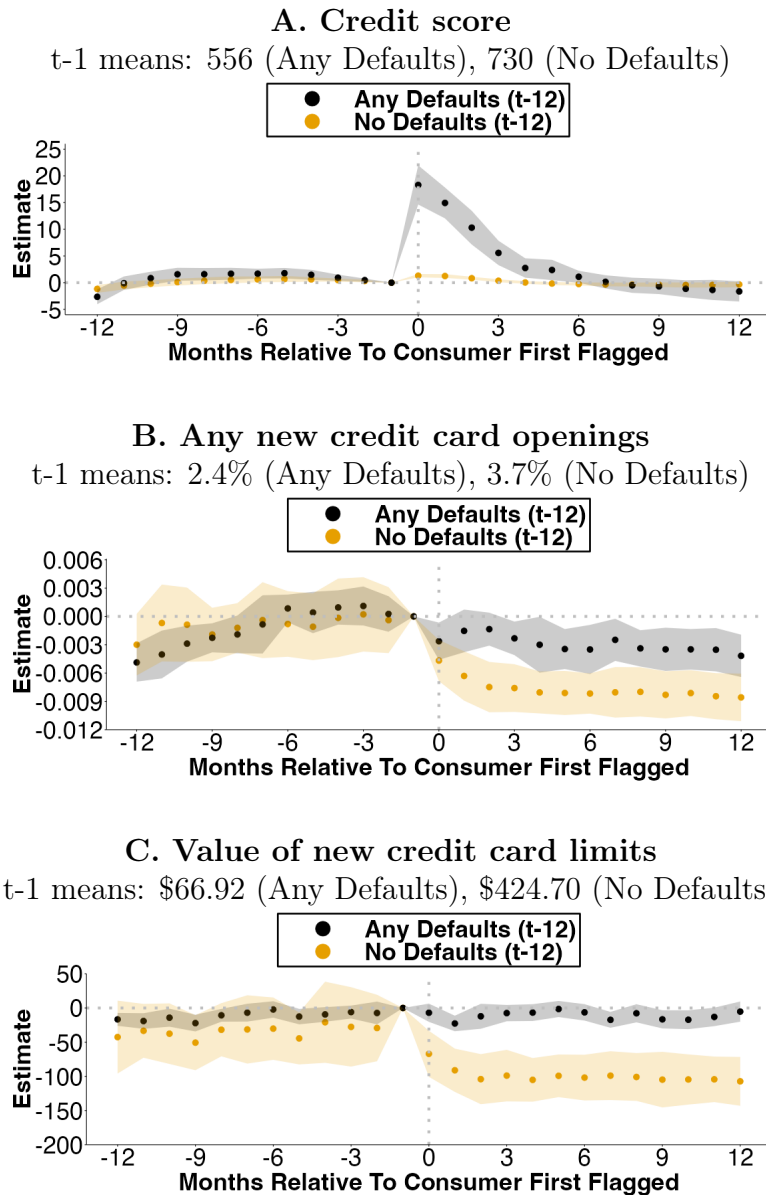
A. Credit score
t-1 means: 556 (Any Defaults), 730 (No Defaults)

B. Any new credit card openings
t-1 means: 2.4% (Any Defaults), 3.7% (No Defaults)

C. Value of new credit card limits
t-1 means: $66.92 (Any Defaults), $424.70 (No Defaults)

Notes: TransUnion data. Plots show estimates of Equation 2.7's δ_τ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.

Figure 2.10: Receiver operating characteristic (ROC) curves showing predictive performance of models comparing baseline model predicting default (black) to temporarily masking FEMA defaults (yellow), permanently (blue) masking FEMA defaults, and masking all defaults (green)
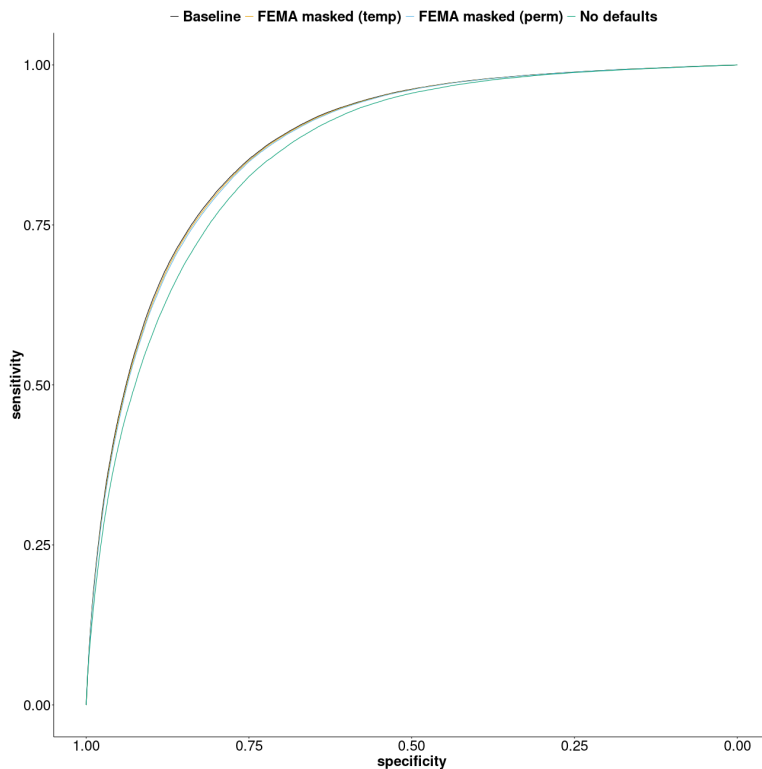
Notes: TransUnion data. ROC curves from out-of-sample prediction from models predicting any new default in next 24 months using data to October 2017. Black is baseline model that includes defaults and non-default information, red is model masking defaults occurring on tradeline months with credit report disaster flags, blue is model masking defaults occurring in six months of FEMA disaster, and green is model masking all defaults.

Table 2.1: Summarizing consumers with (I) disaster flags compared to (II) unflagged in same census block group × zipcode (CBGZIP) and (III) unflagged in US

	(I) Flagged	(II) Unflagged in CBGZIP	(III) Unflagged in US
Credit Score	696	704	704
Age (years)	50.59	49.44	49.45
Accounts (#)	7.84	5.47	5.30
Any 30+ defaults (%)	0.09	0.07	0.08
30+ defaults (#)	0.17	0.12	0.13
Any Balance (%)	0.97	0.89	0.89
Any Auto (%)	0.53	0.38	0.34
Any Credit Card (%)	0.80	0.71	0.68
Any Mortgage (%)	0.45	0.31	0.33
Balances ($)	141,177	88,402	86,143
Mortgage Balances ($)	233,733	205,812	188,589
Non-Mortgage Balances ($)	29,347	19,132	16,273
Auto Balances ($)	21,868	19,202	16,986
Credit Card Balances ($)	7,985	5,866	5,533
Credit Card Limits ($)	32,741	26,788	24,939

Notes: TransUnion data. Table summarizes data for consumers using characteristics twelve months prior. 'Flagged' shows characteristics of consumers with any disaster flags. 'Unflagged CBGZIP' shows consumers with no disaster flags in the same census block group × zipcode where any other consumers had disaster flags. 'Unflagged US' shows consumers with no disaster flags.

Table 2.2: Credit risk prediction performance of models varying masking of defaults

Model	AUROC	Change from Baseline
(1) Baseline	0.8790	-
(2) Disaster Flag Defaults Masked	0.8786	-0.05%
(3) FEMA Defaults Masked (Temporary)	0.8777	-0.15%
(4) FEMA Defaults Masked (Permanent)	0.8764	-0.30%
(5) All Defaults Masked	0.8641	-1.70%

Notes: TransUnion data. Table shows predictive performance as measured by Area under the receiver operating characteristic (AUROC) from logistic regressions predicting any new default 90+ days past due over the next 24 months using data to October 2017. AUROCs are calculated using out-of-sample data to data models were trained on. Models (1) to (5) use non-default data (e.g. number and type of credit accounts, balances, limits, utilization, bankruptcy, duration of credit history) as predictors. Models (1) to (4) also include default data as predictors. Model (1) includes all default data as predictors as in Equation 2.4. Model (2) reclassifies defaults on tradeline months with disaster flags as non-defaults as in Equation 2.6. Defaults of a consumer residing in a FEMA natural disaster that occur within six months of the natural disaster are reclassified as non-defaults for those six months (6.7% of total) in Model (3) as in Equation 2.9. Defaults by a consumer residing in a FEMA natural disaster area that newly occur within six months of the natural disaster are reclassified in Model (4) as non-defaults for all subsequent tradeline months (18.4% of total) as in Equation 2.10. Model (5) has no default data as predictors as in Equation 2.11.

2.11 Appendix to *"Disaster Flags: Credit Reporting Relief From Natural Disasters"*

Table 2.3: Summarizing tradeline months with disaster flags by credit type

Credit Type	2009 - 2019 (%)	2009 - 2022 (%)	Dec 2022 (%)
Auto	12.26	22.09	31.60
Credit Card	31.24	18.03	9.22
Mortgage	20.76	17.35	23.81
Student Loan	15.44	26.51	23.46
Other	20.29	16.02	11.91

Notes: TransUnion data. Displays each credit type's share of all disaster flagged trade months over (i) January 2009 to December 2019 (ii) January 2009 to December 2022 (iii) December 2022. Other contains retail cards and unsecured loans.

Figure 2.11: Trades with credit report disaster flag by credit type, 2000 - 2022
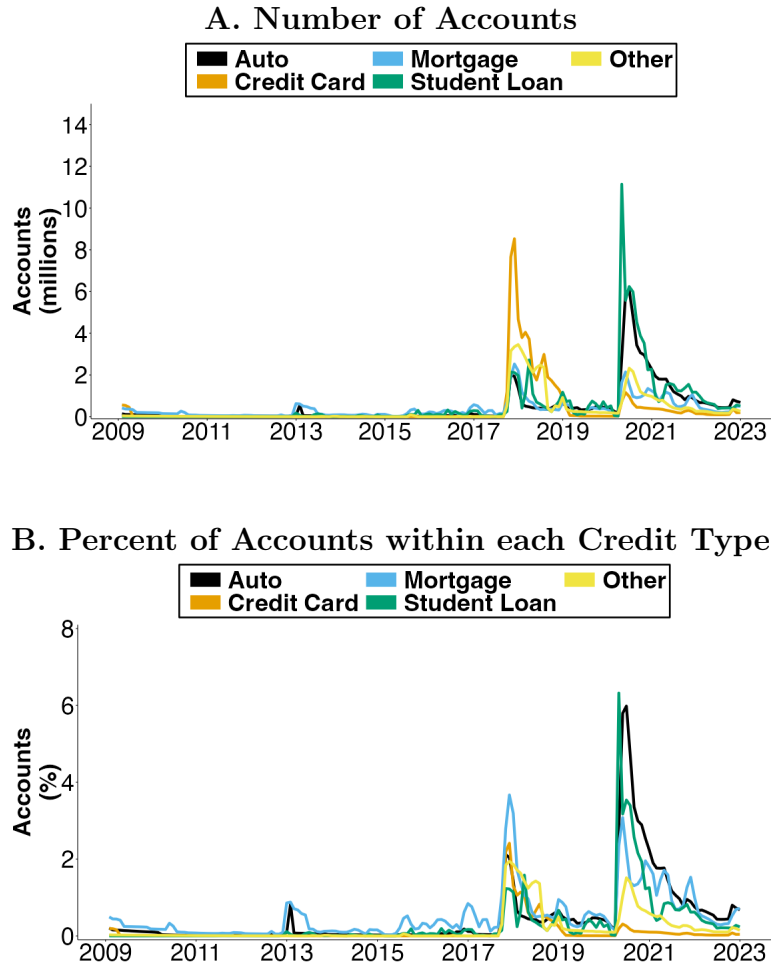
A. Number of Accounts

B. Percent of Accounts within each Credit Type

Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Numbers extrapolated to population estimates from 10% sample. Other contains retail cards and unsecured loans.

Figure 2.12: Trades with credit report disaster flag by lender type, 2000 - 2022
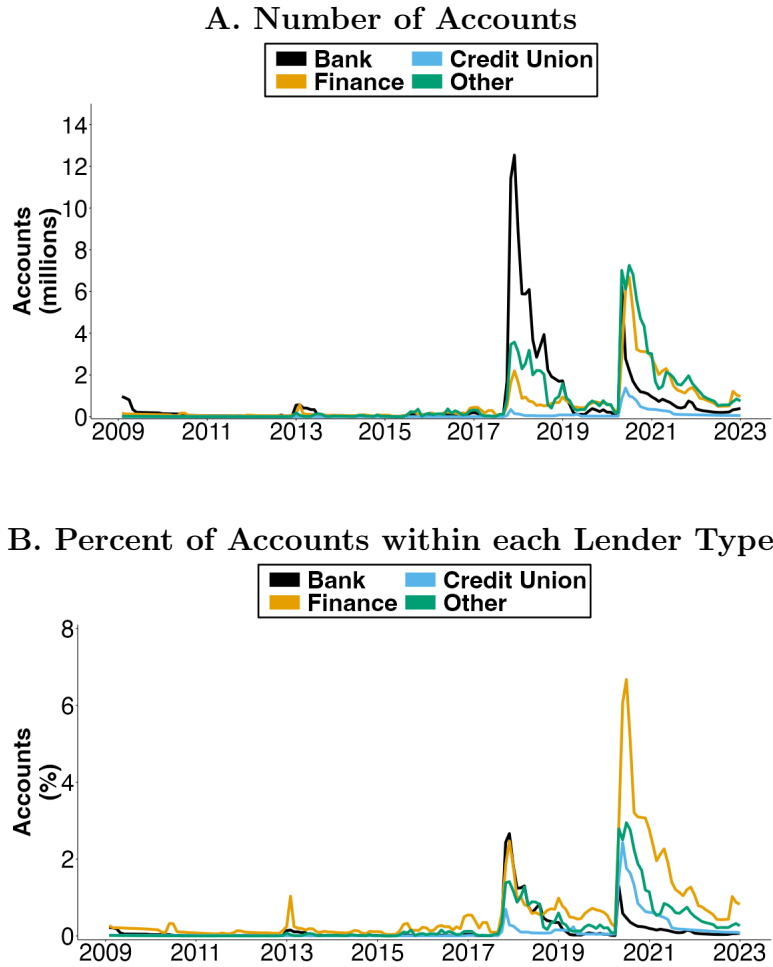
A. Number of Accounts

B. Percent of Accounts within each Lender Type

Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Numbers extrapolated to population estimates from 10% sample.

Figure 2.13: Trades with credit report disaster flag that also had deferments, 2000 - 2022
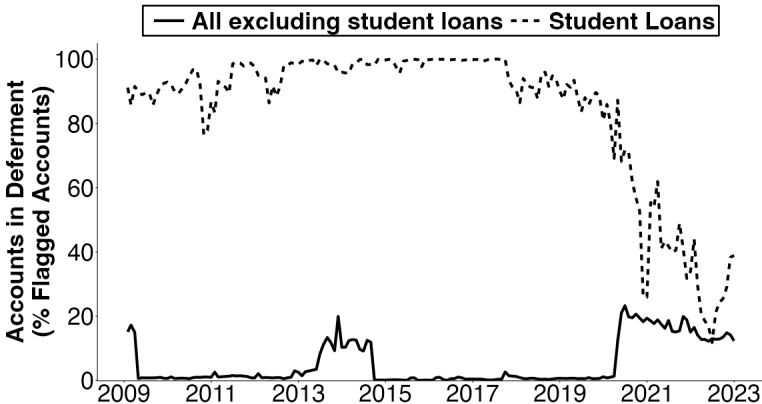
Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Solid line shows fraction of flagged tradelines excluding student loans that also have deferments. Dashed line shows fraction of flagged student loan tradelines that also have deferments: accounts listed with deferments and tradelines with positive balances but zero payments due.

Figure 2.14: Trades with credit report disaster flag that also had deferments, 2000 - 2022, by credit type (Panel A) and lender type (Panel B)
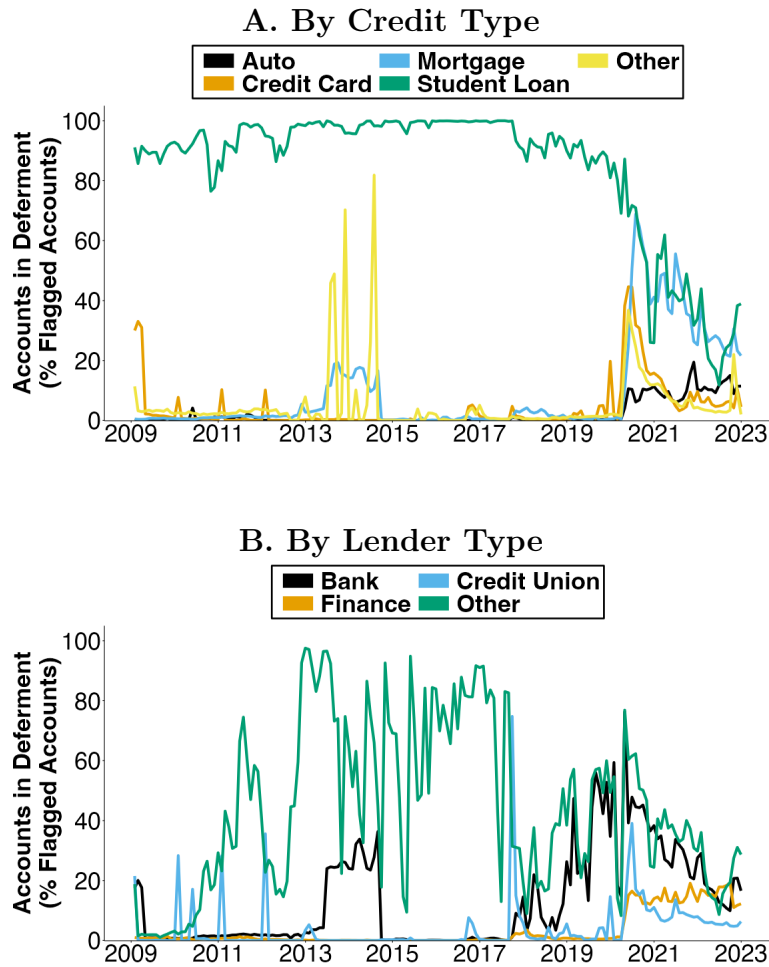
A. By Credit Type

B. By Lender Type

Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Lines show fractions of flagged tradelines that also have deferments: accounts listed with deferments and tradelines with positive balances but zero payments due.

Figure 2.15: Duration of disaster flags remaining on a credit report tradeline, by lender type
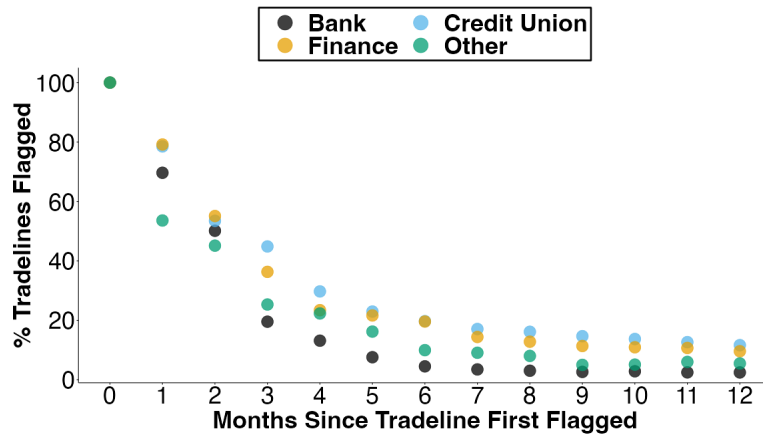
Notes: TransUnion data. This takes open credit report tradelines with positive balance that first have a disaster flag added between February 2009 to December 2021. Plots the fraction of these with disaster flags still present 1 to 12 months later. Colors are lender types.

Figure 2.16: Fraction of disaster flags remaining on a credit report tradeline after 12 months, by cohort
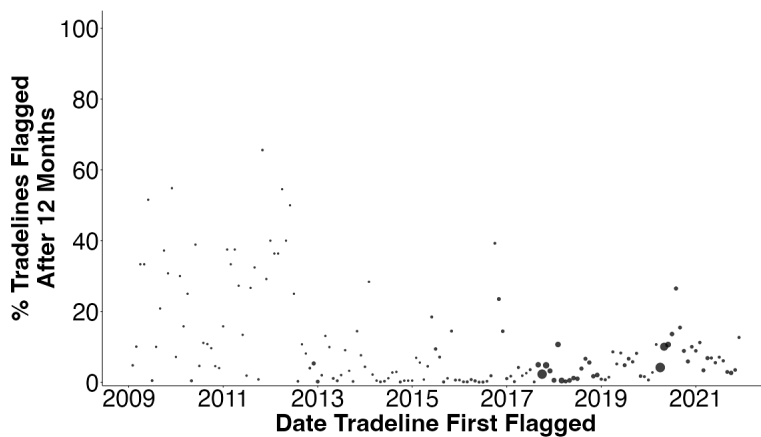
Notes: TransUnion data. This takes open credit report tradelines with positive balance that first have a disaster flag added between February 2009 to December 2021. Plots the fraction of these with disaster flags still present 12 months later for each cohort. X axis is cohort date when disaster flag first added to tradeline. Size of dot is proportional to initial disaster flag cohort size.

Figure 2.17: Intensive Margin: Among consumers with disaster flags, mean fraction of trade-lines flagged (Panel A) and fraction with all tradelines flagged (Panel B) over time
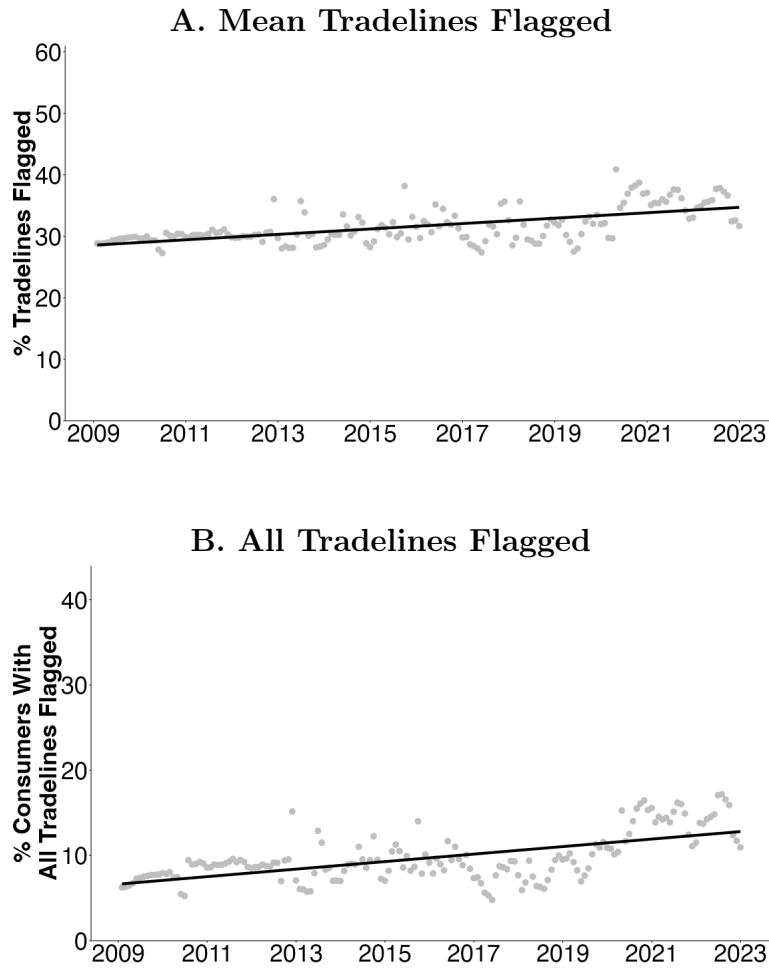
A. Mean Tradelines Flagged

B. All Tradelines Flagged

Notes: TransUnion data. Both Panels A and B restrict to consumer months where the consumer has a credit report disaster flag on at least one open tradeline with a positive balance in their credit report. Panel A shows for these consumers, the mean of tradelines with a credit report disaster flag. Panel B shows the fraction of these consumers where all their tradelines have a credit report disaster flag. Linear time trends added in both Panels.

Figure 2.18: Event study of mean number of defaults on credit reports before (black) and after (orange) flag masking for all flagged consumers (Panel A), by pre-disaster credit score (Panel B), and by any pre-disaster defaults (Panel C)
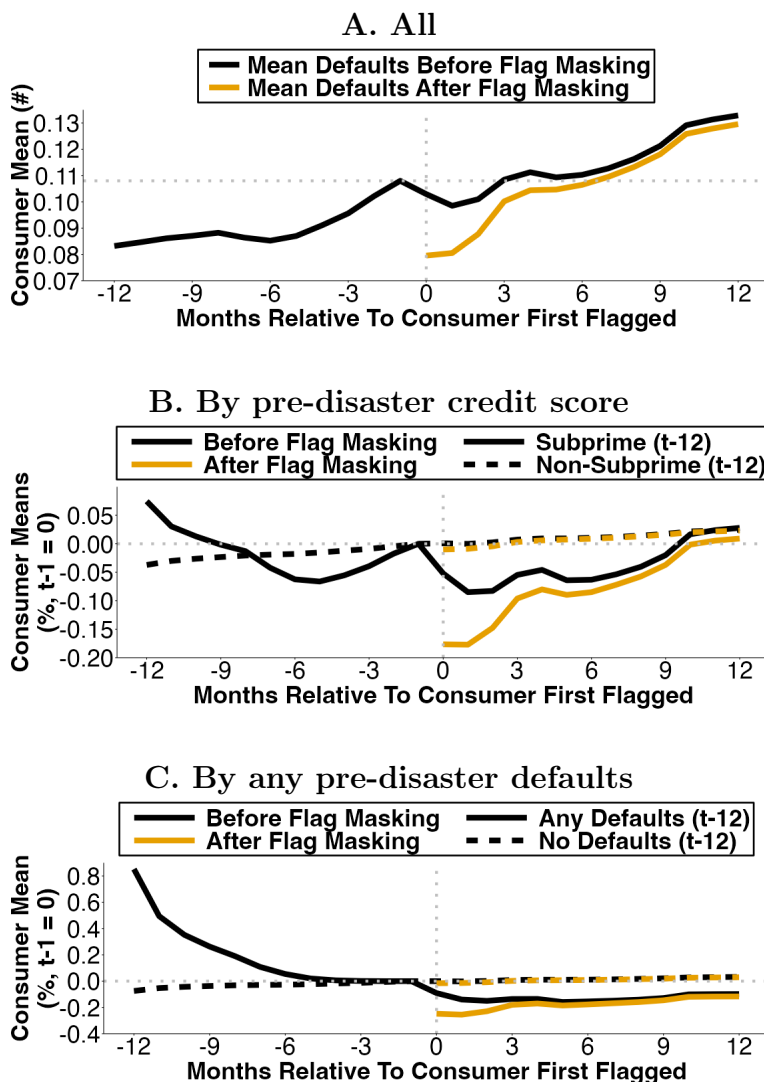
A. All

B. By pre-disaster credit score

C. By any pre-disaster defaults

Notes: TransUnion data. Unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. X axis shows months since consumer first flagged. Y axis is mean number of defaults on consumers' credit reports before (black) and after (orange) tradeline months where defaults are masked by flags. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each series to t-1 = 0.

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Figure 2.19: Event study of VantageScore credit scores by pre-disaster credit score (t-12)
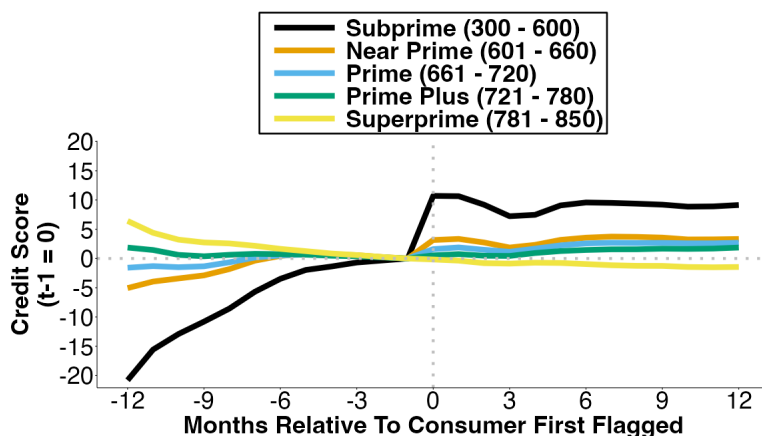
Notes: TransUnion data. Unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. X axis shows months since consumer first flagged. Y axis is fraction of consumers with any defaults before (black) and after (orange) tradeline months where defaults are masked by flags. Splits by consumer's credit score twelve months prior to first being flagged (t-12). Each credit score normalized to t-1 = 0.

Figure 2.20: Difference-in-differences estimates of effects on credit access
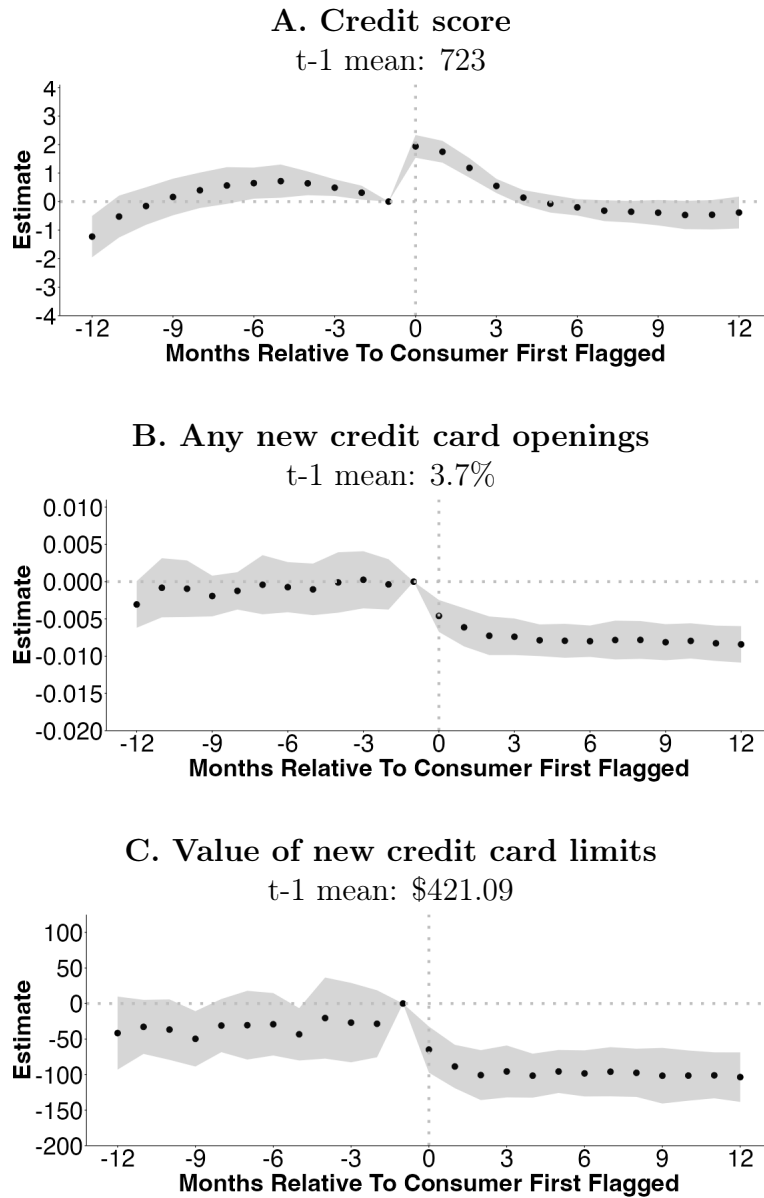
A. Credit score
t-1 mean: 723

B. Any new credit card openings
t-1 mean: 3.7%

C. Value of new credit card limits
t-1 mean: $421.09

Notes: TransUnion data. Plots show estimates of Equation 2.7's δ_τ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.

Figure 2.21: Difference-in-differences estimates of effects on credit access by pre-disaster credit score

A. Credit score

t-1 means: 569 (Subprime), 636 (Near Prime), 690 (Prime)

t-1 means: 747 (Prime Plus), 804 (Superprime)
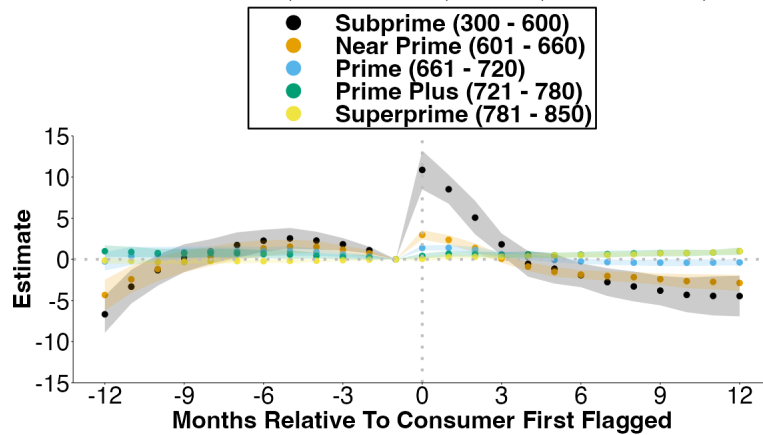
B. Any new credit card openings

t-1 means: 0.0375 (Subprime), 0.0439 (Near Prime), 0.0433 (Prime)

0.0380 (Prime Plus), 0.0293 (Superprime)
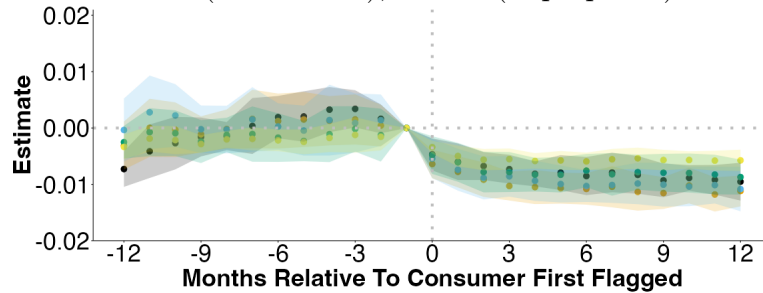
C. Value of new credit card limits

t-1 means: $108.45 (Subprime), $259.77 (Near Prime), $390.73 (Prime)

$534.89 (Prime Plus), $523.67 (Superprime)
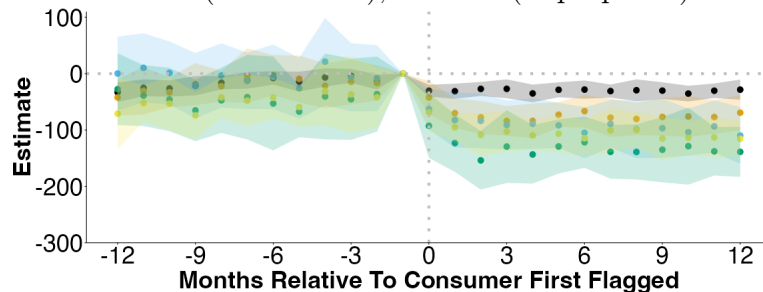
Notes: TransUnion data. Plots show estimates of Equation 2.7's δ_τ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.

Figure 2.22: Difference-in-differences estimates of effects on the number of new account openings across credit types
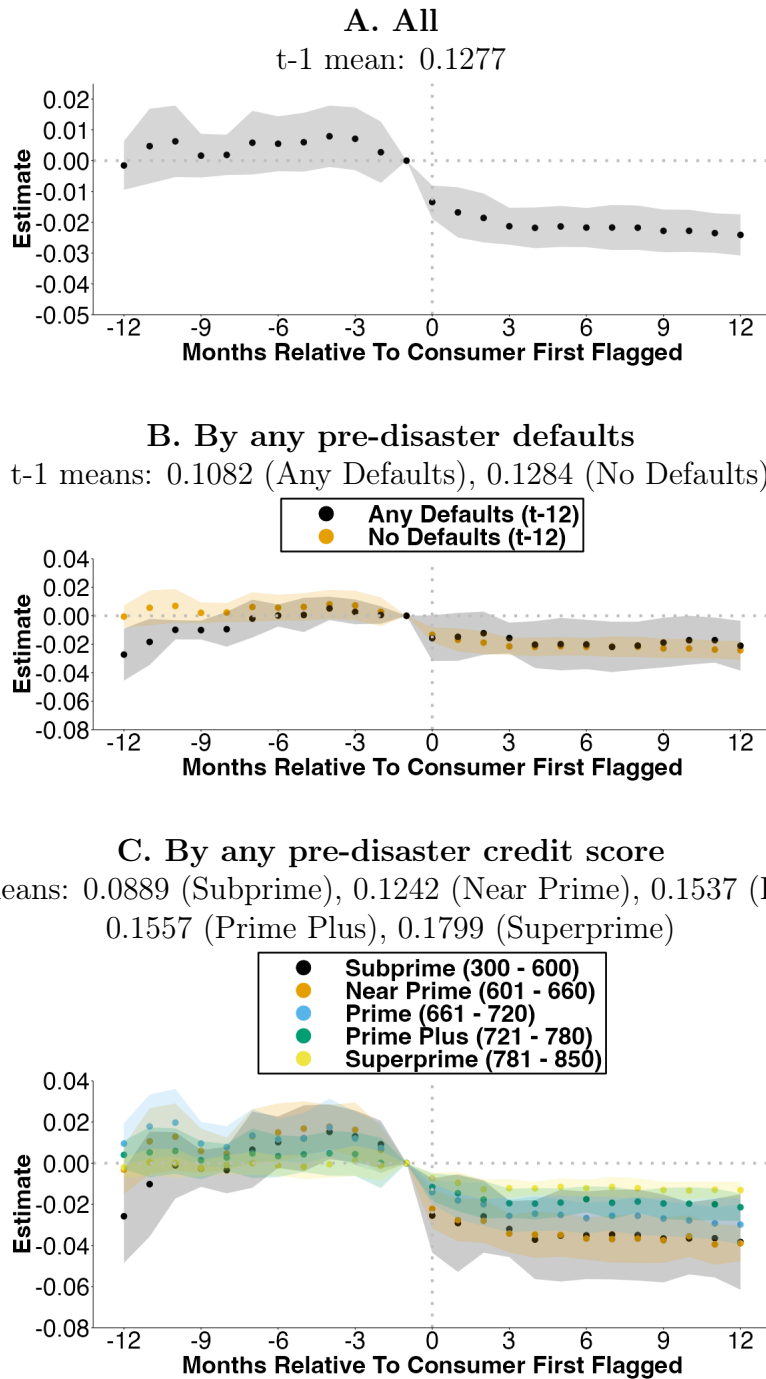
A. All

t-1 mean: 0.1277

B. By any pre-disaster defaults

t-1 means: 0.1082 (Any Defaults), 0.1284 (No Defaults)

C. By any pre-disaster credit score

t-1 means: 0.0889 (Subprime), 0.1242 (Near Prime), 0.1537 (Prime)
0.1557 (Prime Plus), 0.1799 (Superprime)

Notes: TransUnion data. Plots show estimates of Equation 2.7's δ_τ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.

CHAPTER 3

THE SEMBLANCE OF SUCCESS IN NUDGING CONSUMERS TO PAY DOWN CREDIT CARD DEBT

3.1 Introduction

Credit card payments are often at or near the minimum due: 25% of payments in the UK (FCA, 2016a) and 29% in the US (Keys and Wang, 2019). Three mutually compatible reasons may explain the prevalence of low credit card payments at or near the minimum. First, most credit cardholders underestimate how long it will take to pay off credit card debt if they only pay the minimum (e.g., Adams et al., 2022a). Informational disclosures or nudges to address this bias are ineffective at significantly reducing credit card debt (e.g., Agarwal et al., 2015b; Seira et al., 2017; Adams et al., 2022a). Second, a credit card's minimum payment "can serve as an anchor, and as a nudge that this minimum payment is an appropriate amount" (Thaler and Sunstein, 2008). Empirical literature finds evidence supporting this hypothesis of credit cardholders anchoring their payment amount to the minimum (e.g., Stewart, 2009; Guttman-Kenney et al., 2018; Keys and Wang, 2019; Medina and Negrin, 2022; Sakaguchi et al., 2022).[1] Such anchoring suggests a policy removing the anchoring effect ("de-anchoring") could reduce credit card debt. However, a third reason may mean such a policy is ineffective, if credit cardholders frequently only have limited liquid cash available—whether this is the result of rational or behavioral mechanisms—are likely to choose to make low credit card payments.

We conduct a survey experiment and a field experiment on UK credit cardholders. We test whether one active choice nudge that is designed to de-anchor credit card payments from the minimum payment reduces credit card debt. In line with anchoring, our nudge is effective at shifting choices away from the minimum. However, in line with limited liquidity, the nudge

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1. More broadly, following Tversky and Kahneman (1974)'s initial work on anchoring, anchoring effects have been found across domains (e.g., Schley and Weingarten, 2023).

is ineffective in changing the amount of credit card debt. We find that credit cardholders' responses to the nudge make it ineffective. We also show that credit card payment behavior is strongly correlated with a new dynamic liquidity measure: cardholders' minimum liquid cash balances over 90 days.

A mechanism facilitating low credit card payments is the FinTech feature called "Autopay" in the US or "Direct Debit" in the UK. Autopay is a common payment mechanism used across non-financial (e.g., cell phones) and financial (e.g., autos, mortgages) products. Some FinTech credit products, such as "buy now, pay later" in the US, require users to enroll in Autopay (CFPB, 2022a). For credit cards, enrolling in Autopay is an opt-in choice. Cardholders choosing to enroll in Autopay are presented with three options: automatically paying exactly the minimum amount due each month ("Autopay Min"), automatically paying a fixed amount each month ("Autopay Fix") where the automatic payment is the maximum of a fixed amount and the minimum due that month, and automatically paying the full balance due on the statement each month ("Autopay Full"). These three Autopay options are standard in the UK and US. Autopay is used by 42% of UK cards (FCA, 2016a) and 20 to 38% of US cards (CFPB, 2019), with growing use over time.[2] Cardholders enrolled in Autopay can also make supplemental, non-Autopay ("manual") payments (e.g., online, phone).

Persistent minimum payments and high credit card interest costs are concentrated among cardholders enrolled in Autopay Min. 75% of consumers in "persistent credit card debt" (using a regulatory definition of making nine or more minimum payments in a year on interest-bearing cards) are enrolled in Autopay Min (FCA, 2016a). Consumers who switch into Autopay Min pay more in credit card interest than they save in reduced late payment fees (Sakaguchi et al., 2022). The 20% of UK credit cards enrolled in Autopay Min account for 43% of total interest and fees across all UK credit cards (Sakaguchi et al., 2022).

Are credit cardholders enrolled in Autopay Min subject to anchoring? We advance prior

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2. US estimates are more uncertain because they are based on consumer self-reports (CFPB, 2019).

research by studying anchoring effects by Autopay enrollment observed in linked administrative data. We conduct a survey experiment ($N = 7{,}938$) testing a treatment removing the visibility of the minimum payment on a hypothetical credit card online payment screen. De-anchoring minimum payments increases hypothetical credit card payments by 12 percentage points. Credit cardholders enrolled in Autopay Min appear to be subject to anchoring to the minimum payment similarly to cardholders not enrolled in Autopay.

We attempt to exploit anchoring effects with a pre-registered field experiment ($N = 40{,}708$) testing a nudge designed to increase credit card payments on credit card accounts. Our nudge has never been tested before. For consumers in the nudge treated group, we remove the minimum payment as a visible and salient anchor for cardholders enrolling in Autopay at card opening. We do so by removing the explicit appearance of the Autopay Min option for the nudged treated group. Autopay Fix and Autopay Full remain visible options for both control and treatment groups. Autopay Min remains a feasible choice for consumers if they actively chose a low Autopay Fix amount that binds at the minimum. By shrouding the Autopay Min option we increase the salience of the Autopay Fix option which enables an active choice and would, assuming no other behavioral changes, automatically amortize debt faster (see Appendix Section 3.9.1 for more details on our theoretical motivations for this design).

This field experiment is an ex-ante test of a potential nudge that the UK consumer financial protection regulator – the Financial Conduct Authority (FCA) – was considering implementing, in light of regulatory concerns about the substantial amounts of UK credit card debt (FCA, 2014, 2016b). This field experiment is conducted on cardholders who have self-selected to come to the Autopay enrollment web page as these are the policy-relevant population. We measure outcomes in administrative credit card data and consumer credit reporting data.

This de-anchoring nudge reduces Autopay Min enrollment from 36.9 percent of the control group to 9.6 percent in the nudged treatment group: a 74% decline. The nudge increases

Autopay Fix enrollment by 73%. We also conduct a field experiment of the same de-anchoring nudge with a second lender but after observing similarly large treatment effects on Autopay enrollment this second lender withdrew before fieldwork was complete.

We follow cardholders over at least seven months and find that our de-anchoring nudge does *not* change credit card debt. We observe null effects, on average, on credit card debt as well as spending, total payments, and borrowing costs after seven completed credit card cycles on the specific card in the trial and across a consumers' entire portfolio of credit cards. It causes the likelihood of only paying exactly the minimum to fall by seven percentage points (23%) but consumers are no more likely to pay the full balance. These effects are persistent over time. Such null results are critical policy inputs (Abadie, 2020b) especially when the null effects on real outcomes contrast to the large effects on Autopay enrollment outcomes. While our de-anchoring nudge uses psychological insights to change enrollment choices, it does not change economic outcomes of ultimate importance to policymakers.

We contribute to the literature on nudging by demonstrating an example for how policymakers cannot assume changes in enrollment lead to changes in real economic outcomes. Our results demonstrate the importance of considering how the effects of nudges are evaluated. If a policymaker only observes the effects of the nudge on the composition of Autopay enrollments, it may appear effective: we estimate it would be expected to translate into reducing debt by approximately 4.5%. Whereas examining the effects on debt reveals the nudge is ultimately ineffective. Our study contributes to a broader debate on the effects of nudges (e.g., Thaler, 2017; Laibson, 2020; Chater and Loewenstein, 2022). DellaVigna and Linos (2022)'s meta-study documents the heterogeneous effects of nudges and provides evidence for publication bias.[3] Across financial domains, nudges can shift enrollments but consumers may also subtly counteract these effects. For example, Choukmane (2021) finds the long-run

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3. DellaVigna and Linos (2022) show the average effect among academic published studies of nudges is 8.7 pp (33.4% increase in take up) whereas the average effects from the population of studies from Behavioral Insights Teams are smaller: 1.4 pp (8% increase). As this is a meta-study, the average effect is compiled from a mixture of short-run and longer-run outcomes depending upon what outcome is observed in the original studies.

effects of automatic enrollment defaults on savings are smaller than short-run contribution increases found in the earlier, academic literature (e.g., Madrian and Shea, 2001; Thaler and Benartzi, 2004). Some nudges are still highly effective even when potential countervailing effects are measured (e.g., Chetty et al., 2014; Beshears et al., 2022), whereas some nudges may have adverse side effects (e.g., Medina, 2021).

We investigate the mechanisms that cause the enrollment effects of our de-anchoring nudge in our field experiment to be undone so that the effects on economic outcomes are not statistically significant. We find three factors explain why the de-anchoring nudge is ineffective. First, nudged cardholders set up fixed Autopay amounts that are only modestly higher than the minimum payment due, and in the long-run, essentially no higher than the minimum payment because the minimum payment rises mechanically as card balances rise over time. Second, nudged cardholders are less likely to enroll in Autopay, causing more missed payments relative to the cardholders who are not nudged. Third, nudged cardholders enrolled in Autopay make lower manual payments.

Limited liquidity can partially explain why consumers do not reduce their credit card debt. For a selected subsample of our field experiment, we observe daily liquid cash balances from bank account data linked to our credit card data. In the UK, it is common for checking accounts to have an overdraft line of credit facility, so liquid cash balances can be negative. We use these linked data to construct a new dynamic measure of liquid cash balances: the *minimum* liquid cash balances in the last ninety days. This dynamic liquidity measure reveals that approximately 50% of consumers in our linked data have effectively zero liquid cash balances at some point in the last 90 days, compared with just 10% using a traditional static measure of liquid cash balances. Our new measure strongly predicts subsequent credit card payment decisions. Consumers with small positive minimum liquid cash balances (before card opening) repaid approximately 20 percentage points more of their credit card debt seven cycles later than those with zero or small negative minimum liquid cash balances.

3.2 Survey Experiment on Anchoring

Social scientists have documented that consumer choices are influenced by anchoring effects (e.g., Mussweiler et al., 2000). Thaler and Sunstein (2008) write "Credit cards minimum payment...can serve as an anchor, and as a nudge that this minimum payment is an appropriate amount." This conjecture has been supported by a series of empirical studies (e.g., Stewart, 2009; Keys and Wang, 2019; Medina and Negrin, 2022).

3.2.1 Survey Experiment on Anchoring: Design

We conduct a survey experiment testing whether UK credit cardholders enrolled in Autopay anchor to the minimum payment.[4] Prior research does not examine anchoring effects by Autopay enrollment (e.g., Autopay enrollment is unobserved in Keys and Wang, 2019 and Medina and Negrin, 2022). We observe Autopay enrollment in administrative data linked to survey responses.[5] This survey experiment was not pre-registered.

Survey respondents were shown an online credit card payment screen, asked to imagine this was their actual bill and, considering their actual financial situation, report how much they would hypothetically pay. The survey generated 7,938 responses and these are linked to administrative data on credit card behaviors.[6] This is a relatively large and externally valid sample compared to prior studies in this domain that use platforms such as MTurk (e.g., Stewart, 2009; Navarro-Martinez et al., 2011; Salisbury and Zhao, 2020; Sakaguchi et al., 2022). Our survey response rate is 6.7% which is low on an absolute basis, but comparable to other surveys such as the FRBNY's Survey of Consumer Expectations which has 3,853 respondents and a response rate of 6% (Armantier et al., 2017).

Respondents to the survey experiment were randomized across two statement balance

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4. Participation is incentivized through a prize draw with two £500 and fifteen £100 Amazon gift vouchers.

5. Our earlier working paper (Guttman-Kenney et al., 2018) contains more analysis for respondents not enrolled in Autopay. This includes comparing hypothetical responses to actual credit card behavior in these respondents' administrative data.

6. We remove respondents who were inactive on their credit card or in the survey experiment's pilot.

amounts: the 25$^{\text{th}}$ (£532.60) and 75$^{\text{th}}$ percentiles (£3,217.36) of the overall distribution of actual statement balances. We also randomized balance amounts given a wide heterogeneity in credit card balances and it is possible anchoring effects would vary with balances.

Respondents were also randomized across control and treatment groups. The control group's screen design (Appendix Figure 3.12) shows the options cardholders observe in their online manual payment screen: an option to pay in full, an option to pay the minimum amount due, and an option to pay a specific amount they can choose. For the control group, the statement balance amount and minimum payment amount are both presented.

The treatment group's screen (Appendix Figure 3.12) does not show the minimum amount due or have a radio button with which they can pay the minimum. This removes one of the two passive options: the minimum amount due has been removed, leaving full payment as the only passive option. If the respondent does not want to make the full payment, they are forced to make an active choice (Carroll et al., 2009) of how much to pay, which is de-anchored from the minimum.

In both control and treatment groups, if a respondent entered an amount less than the minimum amount due, a prompt appeared that showed the minimum amount due and asked the respondent to re-enter their payment amount. After being prompted once, the respondent was allowed to choose to pay an amount less than the minimum amount due. This sequence of prompts replicates the actual online experience of cardholders.

3.2.2   Survey Experiment on Anchoring: Results

In our survey experiment, we find evidence of anchoring to the minimum payment – conceptually replicating prior lab studies (e.g., Stewart, 2009; Navarro-Martinez et al., 2011; Salisbury and Zhao, 2020; Sakaguchi et al., 2022). Figure 3.1 shows the distribution of hypothetical repayment choices in our experiment as measured by 'payment - minimum (% of statement balance - minimum)' to normalize payment amounts relative to the minimum across balance scenarios. These are grouped by Autopay enrollment status in respondents'

actual credit card administrative data. The dotted lines show the control groups and the solid lines show the de-anchored treatment groups.

The de-anchoring treatment makes respondents significantly less likely to pay exactly the minimum payment, more likely to pay in full, disperses the distribution of payments away from being anchored at or near to the minimum, and makes respondents no more likely to pay less than the minimum (Appendix Figure 3.13 and Table 3.3). These survey experiment results are consistent with anchoring effects found in administrative data by Keys and Wang (2019) and Medina and Negrin (2022) who examine how credit cardholders' repayments change in response to lenders changing their minimum payment formulae.[7]

We find our de-anchoring treatment has no statistically significant effect on respondents enrolled in Autopay Full: with a treatment effect on payments of -1.4 pp of the statement balance (95% C.I. with a 95% C.I. of -7.5 to 4.8 pp). For other respondents, the de-anchored choices in the treatment are significantly different from the anchored choices in the control. The largest de-anchoring treatment effect on payments is for respondents enrolled in Autopay Min: 17.3 pp estimate with a 95% C.I. of 13.8 to 20.9 pp. Effects are similar for Autopay Fix enrollees (14.6 pp treatment effect on payments with a 95% C.I. of 11.5 to 17.6 pp), and those with No Autopay enrollment (11.7 pp treatment effect with a 95% C.I. of 9.5 to 13.8 pp). This indicates credit cardholders enrolled in Autopay Min appear to be subject to anchoring to the minimum payment similarly to cardholders not enrolled in Autopay. Despite regulatory pressure, no UK lender was willing or able to test our treatment de-anchoring manual payments in a field experiment (and no prior literature does so either). From this resistance, we infer that lenders expect the lab results to extrapolate to the field.

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7. In both of these studies the minimum payment is a visible anchor before and after the formulae changes. This means such studies may under-estimate anchoring effects if a consumer remains well-anchored to the minimum. Keys and Wang (2019) write "at least 22% of near-minimum payers (and 9% of all accounts) respond to the formula changes in a manner consistent with anchoring as opposed to liquidity constraints alone". The anchoring effect would ultimately only be revealed if a field experiment shrouded the minimum payment in a way tested in our survey experiment and prior lab studies.

3.3 Field Experiment

3.3.1 Nudge Design

In our survey experiment, we varied how manual payment options are presented. In our field experiment, we vary how Autopay enrollment options are presented to UK consumers who have just opened a new credit card account. Credit cardholders have broad discretion in how much to pay each month (in contrast to fixed term loans); paying any amount between the minimum due and the full balance fulfills their contractual obligations. The minimum payment due is typically calculated by $max\{£5, 1\%$ statement balance $+$ interest $+$ fees$\}$.[8] If a cardholder is only paying the minimum, then (i) their payment is effectively only servicing debt interest payments (with interest rates near 20% typical), and (ii) debt reduction only happens at all if new spending is less than 1% of the statement balance. Even with *no* new spending, debt paydown is only 1% of the statement balance per month if a cardholder only pays the minimum. This credit card amortization structure is somewhat similar to interest-only (or reverse) mortgages although one important difference is that credit cards are open ended agreements.

When a consumer opens a new credit card online they are typically presented with the option to enroll in Autopay. If a consumer decides to opt-in, they are normally presented with three Autopay options: Autopay Full, Autopay Fix, and Autopay Min. These options are shown to our control group (Figure 3.2, Panel A).[9] At this stage consumers can still decide against enrolling in any type of Autopay by not completing the enrollment process. They could also return and complete the Autopay enrollment later.

While Autopay Min is a common payment option, cardholders also have the option to

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

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

enroll in an alternative Autopay option that would pay down debt faster: "Autopay Fix".
Autopay Fix is calculated by: $max\{$Autopay Fix £, Minimum Payment Due$\}$. By contrast,
the minimum payment – and therefore Autopay Min – typically declines with balances. For
example, a typical credit card balance of £1,000 (assuming 18.9% APR and no further card
spending) would take 18 years and 6 months to pay off if no new spending transactions are
made and only the minimum is paid each month (starting around £25 and falling to £5).
However, by fixing payment to £25 each month, the debt pay-off horizon falls to 5 years
and 1 month, saving over £750 in interest costs. Choosing slightly higher fixed payment
amounts sharply decreases amortization times and borrowing costs. For example, with a
fixed payment of £50 each month, the debt pay-off horizon falls to 2 years and interest costs
become only £191 (compared to £509 if paying a fixed amount of £25).

The treatment webpage (Figure 3.2, Panel B) is a nudge that shrouds the option to
automatically make only the minimum payment each month. This is done by removing the
explicit appearance of the Autopay Min option (which is shown to the control group in Panel
A). Removing the Autopay Min option increases the salience of the alternative, Autopay Fix
and the Autopay Full options. This intervention has never been tested before.

Because few consumers can pay their credit card debt in full each month, the treatment
is designed to work by increasing Autopay Fix enrollment which, relative to Autopay Min,
is expected to increase automatic payments and reduce debt and interest costs. It could
possibly also yield an effect of increasing consumer spending via debt paydown increasing
credit limit availability (e.g., Gross and Souleles, 2002; Agarwal et al., 2018).

While there is no longer an explicit Autopay Min option in the treatment arm, card-
holders can choose an operationally equivalent option by setting an Autopay Fix of £5
(or less). These two options are equivalent as the minimum payment is calculated as
$max\{$£5, 1% statement balance + interest + fees$\}$ and so is greater than or equal to
£5 by construction. This means that when the minimum payment due in a particular month
is more than £5, the Autopay attempted to be taken will adjust accordingly, regardless of

whether a consumer has an Autopay Fix amount of £5 or an Autopay Min.[10] This equivalence is not highlighted to consumers and we do not expect them to be aware of this or work this out. We explain this to show that the treatment does not restrict consumer choice of an Autopay option to pay the minimum – and so the treatment is a nudge rather than a restriction (the Autopay Min option is no longer explicitly labelled on the website). If a consumer in either the control or treatment group phones the lender's call center they could still enroll in an explicit Autopay Min if they ask to do so. Thirty days after card opening, cardholders in both the control and treatment groups have identical (control group) screens containing explicit Autopay Min enrollment options. This is relevant if a cardholder comes back to the Autopay launch page to change their Autopay enrollment status.

3.3.2   Experiment Implementation

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

We implement the experiment on new credit cards. When a consumer is applying for a new credit card online and has been accepted by a lender they have the option to set-up Autopay on this new card. If a consumer selects the option confirming that they want to enroll in Autopay, they are included in the experiment. Inclusion in the experiment is irrespective of whether the Autopay enrollment process is completed after reaching the Autopay enrollment screen. At this point consumers are randomly assigned to either control

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10. Example 1: If a consumer had a £5 minimum payment due then £5 would be attempted to be taken if the consumer is enrolled in Autopay Min. If a consumer had an Autopay Fix amount of £5 then £5 would be attempted. Example 2: If a consumer had a £10 minimum payment due then £10 would be attempted to be taken if the consumer is enrolled in Autopay Min. If a consumer is enrolled in Autopay Fix amount of £5 then £10 would be attempted (as the minimum is higher than the fixed amount).

or treatment (the nudge).[11]   Once allocated to control or treatment the consumer would view the same assigned screen if they returned to the Autopay landing page within thirty days.

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

Our field experiment is conducted on two UK lenders. The main lender is a large UK firm and our experiment included 40,708 credit cards newly issued by them between February and May 2017. We wanted at least 20,000 cards in each of control and treatment group. The final achieved number is slightly higher as for logistical reasons new cards were included until the end of May 2017. We also conducted the experiment with a second lender. The second lender stopped the experiment after one week of fieldwork due to the lender's concern over the large treatment effects on Autopay enrollments. The second lender's experiment was not restarted and the pre-agreed target sample size was not reached. The second lender's experiment's achieved sample size of 1,531 cards is insufficiently powered to distinguish between null results and imprecisely estimated non-null effects. Had we known this second lender would pull-out we would not have run the experiment with them. For completeness, results from the second lender are in Appendix 3.9.4. The rest of this paper is based on the field experiment with the main lender unless explicitly stated.

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11. Since we did not know who new applicants were going to be in advance of their application, this randomization had to be done live during the application process instead of in advance. This was carried out through a random number generator JAVA script created by the lender.

3.4 Data and Methodology

3.4.1 Data

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

For the consumers in the experiment, we gather consumer credit reporting data (see Gibbs et al., 2024, for a review of these data) which enables us to observe effects across a consumer's debt portfolio. Consumer credit reporting data provide monthly, account-level data showing credit limits, balances, payments, and arrears from card opening to the end of 2017. For credit cards, we observe statement balances (i.e., before payments), payments, balances after payments (i.e., debt), and indicators for whether a card only paid the minimum. UK credit reporting data contain payments data for all credit cards – this is higher quality than US consumer credit reporting data where only a selected subset of credit cards report payments data (Guttman-Kenney and Shahidinejad, 2024a). We observe credit risk scores and income estimates (where available) at two points-in-time: the month before the card was opened and nine months afterwards. These data mean that if the treatment caused a large increase to payments to credit cards in the experiment that caused financial distress elsewhere in their portfolio we could observe it. The credit card administrative data and consumer credit

reporting data are linked using an anonymous key created for this project. All analysis is conducted on anonymized data.

We also observe bank account data (checking/current accounts and savings accounts) for the subset of cardholders who hold these with the credit card lender in our experiment. The bank account data report end of day balances each day up to a year before (or when the account was opened) the experiment started and up to June 2017 - a month after the last cards are enrolled in our experiment. After restricting these data to cardholders who appear to be actively using this bank as their primary bank account, we observe 3,753 cardholders or 9.2% of our field experiment (Additional details in Appendix Section 3.9.5).

3.4.2   Empirical Methodology

Before analyzing data, we pre-registered our methodology. Our pre-registration designates primary outcomes, regression specifications, and thresholds for statistical significance.[12] We structure our analysis in three parts: primary, secondary, and tertiary analyses. This structure limits the potential for data mining or p-hacking. The primary analysis focuses on ten primary, real economic outcomes upon which the nudge's effectiveness is evaluated.

The first six primary outcomes (1-6) measure the impact on the credit card in the experiment ("target card") - constructed from microdata collected from the lender. All these primary outcomes are bounded between zero and one. Outcomes 1, 2, and 3 are binary: (1) any minimum payment, (2) any full payment, (3) any missed payment. Outcome (4) is a measure of credit card debt: statement balance net of payments (% statement balance) We examine multiple moments because credit card payments have a non-normal, bimodal distribution (e.g., Keys and Wang, 2019) with the tails being economically important. Outcome (5) is a measure of borrowing costs (combining interest and fees): Costs (% statement balance). Outcome (6) is a measure of consumption: Spending (% statement balance). Our

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12. Available at AEARCTR-0009326. The pre-registration jointly covered the field experiments in Adams et al. (2022a) – the only differences being Adams et al. (2022a) had different exclusion criteria given it was conducted on existing rather than new credit cards and also had different treatments.

measures of debt, consumption, and costs are all normalized by statement balances in order to deal with fat tailed credit card balances. Normalizing our measures of debt by credit card statement balance is not ideal as it means our outcome is a ratio of two endogenous components. To address this our secondary analysis also shows the numerator and denominator in levels separately (and having completed the analysis we find the results are consistent).

Primary outcomes 7 to 10 are analogous to primary outcomes 1 to 4 but constructed using consumer credit reporting data to assess the impact across a consumer's portfolio of credit cards. These primary outcomes are: (7) Share of credit card portfolio only paying minimum, (8) Share of credit card portfolio making full payment, (9) Share of credit card portfolio missing payment, (10) Credit card portfolio balances net of payments (% statement balances). See Appendix Section 3.9.3 for more details on primary outcome definitions.

Following Benjamin et al. (2018) we regard a p value of 0.005 as the threshold for statistical significance but also highlight where results are 'suggestively significant' at the 0.01 and 0.05 levels. 0.005 significance aligns with 14+ Bayes factors: often considered substantial evidence for a hypothesis. This approach is analogous to applying Bonferroni or familywise error corrections to ten outcomes evaluated at 0.05 significance levels. Given the precision of our estimates, alternative corrections would not affect our results or conclusions. For our primary outcomes, we have sufficient power to differentiate null effects from economically meaningful ones to inform potential policymaking (the minimum detectable effect sizes are in Appendix Tables 3.6 and 3.7).

The pre-registered secondary analysis considers a broader set of outcomes and empirical approaches to understand our results and their robustness. This secondary analysis measures the effects of the nudge on Autopay enrollment and uses the pounds (£) amounts of credit card debt and payments as robustness checks of our primary outcomes. Conducting secondary analysis depends on the primary analysis's results. We design and implement tertiary analysis after examining the data.

We are able to causally identify the effects of the treatment on consumers in our field

experiment since we are randomizing whether a consumer receives the control or treatment. The average treatment effect is the policy parameter of interest as the treatment was a potential regulatory policy which was being considered to be applied across the UK credit card market. Equation 3.1 shows the OLS regression specification used to derive average treatment effects. To estimate this we construct an unbalanced panel with one observation for each consumer's (i) credit card statement cycle (t) observed. This panel is unbalanced as some cards are opened earlier than others. In this specification, δ_τ are the coefficients on interaction terms between treatment indicator and statement cycle indicators. Therefore δ_τ shows the average treatment effect $\tau \in \{1, 2, ..., T\}$ cycles since the start of the experiment. We hypothesized that treatment effects will vary over time but we did not impose a functional form because it is unclear what the appropriate functional form would be.

$$Y_{i,t} = \alpha + \sum_{\tau=1}^{T} \delta_\tau \left(TREATMENT_i \times CYCLE_\tau \right) + X_i'\beta + \gamma_{m(i,t)} + \gamma_t + \varepsilon_{i,t} \qquad (3.1)$$

Our regression includes a constant (α), a vector of time-invariant control variables (X_i') constructed using information on the new credit card opened and cardholder data from before the start of the experiment (controls listed in footnote).[13] We also include time fixed effects: we control for both the statement cycle (γ_t) and year-month ($\gamma_{m(i,t)}$) because statement cycles do not perfectly align with calendar months and new credit cards have different opening dates - entering the experiment until the pre-registered sample size was achieved. Standard errors are clustered at the consumer-level.

For our primary analysis we focus on the outcomes from the last cycle where the panel

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13. The controls (X_i') are: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. For outcomes constructed from consumer credit reporting data up to eleven dummies for lags of outcomes are included as controls (X_i') for months preceding the start of the experiment.

is balanced: the seventh completed statement cycle (δ_7). The seventh statement cycle is complete when its due date has passed: this is mean 195 and median 196 days from card opening with a range of 175 to 245 days. This seventh statement cycle should be thought of as six genuine statement cycles as a new card's first statement is typically less than a month—in our data the first statement is issued mean 12 and median 11 days from card opening—to on-board the card onto a particular billing cycle and so this first statement has a zero payment due that makes it uninteresting (we show for completeness). A consumer's first full statement is statement two—the second statement is issued mean 43 and median 42 days from card opening—when the cardholder has at least one month to view the control or treatment screens and to use their card (and, if used, has a non-zero payment due).

In tertiary analysis we check the robustness of selected results by pooling across all statement cycles to provide more statistical power. We modify Equation 3.1 replacing the dynamic $\left(TREATMENT_i \times CYCLE_\tau\right)$ with static $TREATMENT_i$ shown in Equation 3.2 where our single static parameter of interest is δ.

$$Y_{i,t} = \alpha + \delta\,TREATMENT_i + X_i'\beta + \gamma_{m(i,t)} + \gamma_t + \varepsilon_{i,t} \tag{3.2}$$

3.4.3   Summary Statistics

As the experiment is conducted on newly-opened cards we describe summary statistics for the control group after seven statement cycles (see Appendix Table 3.5). We observe a diversity of credit cardholders in our data with a wide range of interest rates, credit scores, credit card credit limits, ages, and incomes. The mean credit card statement balance after cycle seven is £2,164 and £1,963 after payments. Cardholders often hold other credit cards in their portfolio: their mean credit card portfolio statement balances (summed across cards held in consumer credit reporting data) is £3,917 and £3,432 after payments. Credit card portfolio balances both before and after payments are higher than consumers' mean income of £2,437.

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

Allocation to the treatment group is balanced, on average, across measures (Appendix Table 3.8). However, we do observe the likelihood of being in the treatment group slightly varies with credit card limit. Investigation revealed that the "live" randomization code used by the lender was not completely random: 526 more consumers (0.65%) are allocated to control than to treatment. As consumers applying for credit cards were unaware of (and unable to manipulate) their likelihood of being allocated treatment, we can recover balance between treatment and control through conditioning on covariates. Conditioning on observables using our pre-registered controls does not change our results.[14]

3.5   Experimental Results

3.5.1   Effects on Autopay Enrollment

The first effect we examine is the mechanism the treatment is designed to work through: changing Autopay enrollment choices by the time of their second credit card statement. Autopay enrollments are secondary outcomes.

Figure 3.3, Panel A shows the treatment causes large, significant initial effects in Autopay enrollment choices. The treatment raises the fraction of cardholders enrolling in Autopay for a fixed amount (Autopay Fix) by 20.9 percentage points: a 72% increase on the control group mean. For comparison, Figure 3.3, Panel B displays these enrollment effects are even larger for the second lender who stopped the field experiment early: increasing Autopay Fix enrollment by 40 pp (216%). Subsequent results are all based on the main lender.

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

The Autopay Fix amounts consumers initially choose are frequently round numbers. 62% of Autopay Fix amounts are for (in descending order of frequency): £100, £50, £200, £150, £20, £30, or £25. Very few consumers select Autopay Fix amounts of £5 or less that are mechanically identical to Autopay Min: 2.4% of the treatment group set an Autopay Fix of £5 or less (4.8% of Autopay Fix enrollees).[15] This is a statistically significant increase relative to 0.5% in the control group but we interpret it as being economically small.

Initial choices of Autopay Fix amounts are persistent over time (Appendix Figure 3.15). 88.3% of those in the treatment group who are enrolled in Autopay Fix at their second credit card statement remain enrolled in Autopay Fix at their seventh statement (7.0% have no Autopay, 4.4% Autopay Min, and 0.3% Autopay Full). Of those, 97% have it set for the same Autopay Fix amount, and, on average, the difference in amount is trivial: £0.78. Among all cardholders in the treatment group enrolled in Autopay Fix at cycle 2, the mean Autopay amount is £96.85 (median £80) compared to £104.60 (median £100) at cycle 7: this indicates that cardholders who enroll in Autopay Fix later on are choosing slightly higher Autopay Fix amounts than the initial group.

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

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

We estimate these treatment effects on Autopay enrollment more precisely using our

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

pre-registered regression specification and find statistically significant changes in enrollment. The regression coefficients after seven statement cycles (δ_7 in Equation 3.1) – presented in Table 3.1 – are in line with initial changes in enrollment: Autopay Min enrollment decreased 21.7 pp, Autopay Fix enrollment increases 16.7 pp, Autopay Full increases 0.6 pp (the latter being only significant at the 5% not the 0.5% level), and any Autopay enrollment declines 4.4 pp (unconditional means in Appendix Table 3.9). Estimates cycle-by-cycle (δ_τ in Equation 3.1) are displayed in purple in Figure 3.4 for Autopay Fix enrollment (Appendix Figure 3.16 for Autopay Full and Autopay Min, and Appendix Figure 3.17 for any Autopay). The small, initial effect on Autopay Full enrollment attenuates over time and becomes statistically insignificant from zero. The Autopay Fix and Autopay Min also attenuate but effects remain large. As initial Autopay choices in the treatment group are highly persistent, this attenuation is primarily driven by some in control group "catching-up" and switching from Autopay Min towards Autopay Fix or Autopay Full. Effects of the treatment on any Autopay enrollment change relatively little between cycles two and eight.

The observed changes in Autopay enrollments – the nudge making consumers more likely to choose full, less likely to choose minimum, and changing the distribution of Autopay amounts – are consistent with the minimum payment amount distorting the control group's choices.

3.5.2 Effects on Long-Term Real Economic Outcomes

We examine the effects on our ten primary outcomes using our pre-registered regression specification. These estimates are seven statement cycles after card-opening (δ_7 in Equation 3.1) and are shown in Table 3.2 (unconditional means in Appendix Table 3.10).

We find a large and persistent effect of the nudge making cardholders less likely to only pay exactly the minimum. The nudge causes a significant 23% reduction in the likelihood of only paying exactly the minimum of 7.1 pp (95% confidence interval of 6.2 to 7.9 pp). Figure 3.5 presents this treatment effect over time showing the effect is -10.9 pp in the second cycle

and stabilizes near -7 pp by the sixth cycle (Appendix Figure 3.18, Tables 3.12 and 3.11 show consistent results examining the cumulative number of minimum payments).

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

How does this translate to the share of a cardholder's credit card portfolio where payments are made only equal to the minimum (constructed from consumer credit reporting data)? There is an average treatment effect a third of the size of that for the card for which the treatment is targeted. This smaller overall effect across the credit card portfolio is due to consumers holding multiple cards – only one of which is directly affected by the nudge.

Table 3.2 shows we observe precisely-estimated null effects on average treatment effects on other primary outcomes for the target card in the experiment: the likelihood of paying debt in full, debt net of payments, borrowing costs, and spending. The exception is an increase in the likelihood of missed payments on the target card of 0.38 percentage points (95% confidence interval 0.02 to 0.75 pp) that is statistically significant at the 5% level but not at our 0.5% threshold.

There are precisely-estimated null effects on average treatment effects across our other consumer credit reporting outcomes: the likelihood of paying in full, the likelihood of missing payments, and outstanding debt when aggregating across the portfolio of credit cards held (cycle-by-cycle results in Appendix Figures 3.20 and 3.21). There is no evidence of the treatment affecting other cards held, although we caveat that in an RCT as an ex-ante test of a potential policy we cannot rule out the possibility of general equilibrium effects if this policy applied to all of a consumers' cards. The lack of negative spillovers on a consumer's portfolio is important as one reason for testing the nudge to inform potential policymaking is to evaluate whether any debt reduction for the card in the experiment is partially or fully

crowded by greater indebtedness or financial distress elsewhere.

Our treatment does not reduce credit card debt at or before the seventh statement cycle (Figure 3.6, Panel A). As a robustness check as part of our secondary analysis we look at debt in pounds and also find no statistically significant effect (Figure 3.6, Panel B) or across the portfolio of credit card debt (Appendix Figure 3.21).

As the cycle-by-cycle estimates on our primary measure of credit card debt are stable over time but persistently, slightly, but statistically insignificantly, below zero, we check the robustness of this result in tertiary analysis by pooling across all statement cycles to provide more statistical power (Equation 3.2). If the treatment has any average effect on debt, the average effect on the target card is at most a 1.1 percentage point reduction based on the maximum of the 95% Confidence Interval (Appendix Table 3.13). Even with this pooling there is no statistically significant effect on credit card debt across the portfolio of cards held: at most a 0.79 pp reduction based on the maximum of the 95% Confidence Interval.

Similarly, even with this pooling exercise, we find no significant effects on the likelihood of payment in full on the target card. At most it increases by 0.1 pp: which we interpret as a trivially small amount. As a robustness check, we examine the cumulative number of full payments and results are consistent with stable, precisely-estimated null effects across cycles (Appendix Figure 3.18, Tables 3.12 and 3.11).

Figure 3.7 shows we find null effects over time for, in Panel A, statement balances (i.e., before payments) and, in Panel B, new spending.[16]

Our null average treatment effects on debt (robust to secondary outcomes in Appendix Tables 3.12 and 3.11) in spite of a seemingly large changes in enrollment and reduction in paying only the minimum payment is surprising. Why does the treatment not, on average, reduce debt if one in five more consumers are enrolled in Autopay Fix (and are not increasing spending)?

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16. Spending results are robust to alternative definitions as shown by the cycle-by-cycle results in Appendix Figure 3.22. Cycle-by-cycle results for the remaining primary outcomes are in Appendix Figure 3.19.

3.6   Mechanisms

3.6.1   Factors Explaining Nudge Ineffectiveness

Having completed the primary and secondary analysis, we now conduct tertiary analysis to understand the mechanisms behind our results. If the only changes are compositional – changing Autopay enrollment but *assuming no other changes* – the effects on Autopay enrollment may have been expected to lead to a effect of reducing debt by approximately 4.5%.[17] Indeed, the fact that the second lender withdrew after only observing effects on enrollment is evidence of our null effects on later real outcomes being unexpected. We find three consumer responses on the target card make the nudge ineffective at reducing debt.

Autopay Fix Amounts "Too Low"

Cardholders often respond to the nudge by setting an Autopay Fix that is "too low": binding at or just above the minimum due. While the treatment causes a 16.7 pp increase in Autopay Fix enrollment by statement seven (the purple coefficients in Figure 3.4), the treatment effect on enrollment with Autopay Fix *exceeding* the minimum amount due is still large but half the size (the pink coefficients in Figure 3.4): 8.6 percentage points which is a 34% increase on the control group mean (Appendix Table 3.9). See Table 3.1 for regression estimates.

As credit card balances accumulate over the first few months of card ownership, the minimum amount due rises, causing the minimum payment amount to exceed many of the fixed payments. After seven statement cycles, the proportion of consumers in the treatment group with an Autopay Fix exceeding the minimum payment amount is 66% - noticeably down from 78% in the second cycle (Appendix Figure 3.23 and Table 3.9).

Examining the distribution of Autopay Fix amounts chosen by the treatment group (Figure 3.8) shows they are often "low", commonly round number pound amounts such as

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17. Calculated using the mean debt net of payments in cycle 7 for cardholders in the control group for each Autopay enrollment type and weighting these by the treatment group's Autopay enrollments shares.

£50 or £100 (Panel A) that are small amounts in excess of the minimum (Panels B and D). We do not show the Autopay Fix for the control group as the treatment causes large changes in Autopay Fix enrollment and so the Autopay Fix groups are not directly comparable. Pooling across all seven cycles, we find that for 48% of Autopay Fix enrollees in the treatment group, the cumulative Autopay Fix amount is £100 or less in excess of the minimum. At the other extreme, it is only over £500, for 13%. We evaluate these relative to the mean cumulative value of payments across these cycles in the control group: £1,277. We interpret that the additional payments from Autopay Fix over the minimum are typically "low" in absolute levels, however, they are large increases relative to the extremely low minimum payment due which averages £46 per month (£320 cumulative across cycles 1-7).

Lower Enrollment In Any Autopay

The second factor is that the nudge causes a 4.3 pp (5.6%) significant decline in enrollment in any type of Autopay (Table 3.1 and Appendix Figures 3.15 , 3.17, and Table 3.9). This lower enrollment explains an unintended slight average increase in the likelihood of missed payments (Table 3.1). If enrolled in Autopay a consumer would only miss a payment if they have insufficient funds in their checking account whereas consumers not enrolled may easily forget to make a payment. While this increase is not statistically significant at our 0.5% significance threshold when examining any particular statement cycle, it is clearly significant when conducting a joint significance test pooling data across all statement cycles (while still clustering at the consumer-level). We find the nudge increases the probability of missed payments by 0.4 pp with a 95% confidence interval of 0.19 to 0.62 pp (Appendix Table 3.15). There are no statistically significant differences in the types of consumers the treatment made more likely to not have any Autopay enrollment.

The effect on missed payments is solely on temporarily being a single payment behind: precise zeros are estimated on being two or three payments behind (Appendix Table 3.15 and Figure 3.18). The treatment does not lead to consumers being in more severe arrears which

the industry defines as being 2+ or 3+ payments behind: these are all null results even when pooling observations across cycles to increase power to account for the low incidence of such severe arrears (Appendix Table 3.15). Only more severe arrears get reported in their credit report (i.e., missing a payment by 1 day would not be reported, but by 31 days would be reported). This explains why we do not observe increased missed payments in our primary outcome measuring this in credit reports (Table 3.2 and Appendix Table 3.13). Given that there is no difference in severe arrears on the card in the experiment and also no difference in severe arrears across the portfolio of cards in credit reports, we infer that severe arrears on others cards is unaffected.

This result indicates that having no Autopay means consumers forget to make a payment which has a temporary impact, most notably incurring a late payment fee (in line with Gathergood et al., 2021c; Sakaguchi et al., 2022) and not reducing debt, rather than causing a debt spiral or severe distress. While lower enrollment in Autopay is not an intended effect of the nudge, it is not increasing consumer indebtedness. This is consistent with consumers being more attentive to their debt if not enrolled in Autopay (Sakaguchi et al., 2022). This is different to other domains where lower enrollment may be a worse economic outcome. For example, if a nudge lowers 401(k) enrollment then consumers can be missing out on "free money" from employer-matched contributions and under-save for retirement.

Manual Payments Substitution

Cardholders can make manual payments instead of or in addition to automatic payments. We find substitution between the two as another potential offsetting effect. Figure 3.9 (and Figure 3.10 Panel A) shows that although there is a positive and significant treatment effect increasing automatic payments, the effect on overall payments is lower due to a negative, but statistically insignificant, negative effect on manual payments. We find the treatment causes consumers to be 1.3 pp more likely to make both an automatic and manual payment in the same cycle in spite of lower Autopay enrollment. More details are in Appendix Table

3.12.

Manual payments are infrequent but large. Just 8.5% of those enrolled in any Autopay option in the control group also made a manual payment in the seventh cycle. The percentages of different subsamples of the control group that made both a manual and automatic payment in the seventh cycle are: 6.7% of all consumers (i.e., with and without Autopay enrollment in the control group); 9.2% of consumers enrolled in Autopay Fix or Min; 12.7% for consumers enrolled in Autopay Fix; 6.3% of consumers enrolled in Autopay Min. Cardholders making both a manual and automatic payment have little differences from other cardholders except being slightly younger and being more likely to not hold mortgage debt (Appendix Table 3.16). However, manual payments account for 45% of the total cumulative value of payments made across cycles 1-7 by those in the control group enrolled in Autopay at cycle seven (54% for those enrolled in Autopay Fix or Min).

Consumers appear to use Autopay as insurance against forgetting to make a payment (in line with Fuentealba et al., 2021; Gathergood et al., 2021c; Sakaguchi et al., 2022) as opposed to paying down debt.[18] In months where manual payments are made by those enrolled in Autopay in the control group, the mean value of the manual payment is £377, with a median value of £105. Automatic payments in such months average £105 with a median of £55 and are similar in months where consumers are not making manual payments. Most manual payments by those enrolled in Autopay do not clear a consumer's debt – just 17.9% do so in the control group. 65% of manual payments are for round number values whose digit to the left of the decimal is a zero or five.[19] These round numbers found to prominently appear in manual payments appear with far less frequency in total payments: 48%.

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18. Survey responses in our earlier working paper (Adams et al., 2018b) are aligned with this explanation. The most common reasons survey respondents enrolled in Autopay provide for using Autopay is to prevent incurring a late fee or to prevent a negative credit score impact, while the most common reason respondents not enrolled in Autopay provide is they prefer the control of manually adjusting payments each month.

19. Such patterns of large, manual payments at round numbers may be consistent with cardholders experiencing adjustment costs (e.g., the psychological cost of logging into online banking to make a manual payment and working out how much to pay) to making a payment above the minimum or having reference-dependent preferences for round numbers (e.g., Sakaguchi et al., 2020).

Comparing automatic and manual payments is conflating two effects: a change in Autopay enrollment composition and a change in Autopay amount. Conditional on being enrolled in Autopay, one would expect automatic payments to be higher in the treatment than the control, since Autopay Fix is greater than or equal to Autopay Min. Yet automatic payments will be lower in the treatment group because fewer consumers enroll in Autopay than in the control group. Similarly we may expect manual payments to be higher in the treatment group, however, this is ambiguous as cardholders may be forgetting to make any payments rather than substituting automatic for manual payments. We disentangle this by decomposing Equation 3.1 by whether the consumer is enrolled in any Autopay (i.e., Autopay Min, Fix, or Full) at cycle seven ($AUTOPAY_{i,7}$) shown in Equation 3.3. This is a decomposition by an endogenous variable and so our estimates are not causal and may be biased.

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

(3.3)

We examine the cumulative value of payments, in total and split by automatic and manual payments, by the seventh cycle across these subgroups in Figure 3.10. Panel B shows evidence of substitution among consumers enrolled in Autopay: automatic payments increase by £62, manual payments decrease by £57, and so overall payments for this group are unchanged (£2). If all the increased automatic payments had passed through, without offsetting manual payments, average debt would have reduced by approximately 2.9%. Panel C shows zero estimates on automatic, manual, and total payments for those not enrolled in Autopay: this indicates the treatment's main effect on this group is likely shifting this group's size rather than changing its payment amounts differentially to what one would expect from a cardholder in the control group who is not enrolled in Autopay.

We interpret this evidence as suggesting the treatment is changing *how* cardholders make

payments rather than the *amount* of payments they make. This substitution of higher automatic payments for lower manual payments by cardholders enrolled in Autopay shows these consumers are less inert in their payment choices than they initially appeared.

3.6.2 Heterogeneous Effects

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

We examine three groups of consumer vulnerability: credit score, income, unsecured debt-to-income (DTI) ratio. These groups are chosen as variables that are observable to us (and lenders) and relevant to regulators as they are used as inputs for assessing new credit cardholders' ability to pay their debt. These variables are measured from the month preceding card origination. We split these groups into quartiles as it is not clear whether effects would be monotonic. We estimate Equation 3.1 separately for each quartile of each group. To keep the number of results manageable we only examine heterogeneous effects by our primary outcome of debt (statement balance net of payments as a percent of statement balance). In the control group, there is little difference in this outcome across quartiles of income but noticeably more across quartiles of credit score and DTI.

Our heterogeneity analysis does not produce clear effects (See Appendix Figure 3.25 and Table 3.17 for more details). None of the heterogeneous groups show an effect that is statistically significant at our 0.5% threshold. There are no clear effects by income. By credit score we find the second most vulnerable quartile experienced a reduction in debt that is significant at the 5% threshold with a 95% confidence interval of -2.9 to -0.0 pp whereas

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all other quartiles have insignificant effects. The second least vulnerable quartile by DTI also has a reduction in debt that is significant at the 5% threshold with a 95% confidence interval of -3.1 to -0.0 pp with insignificant effects for other quartiles.

3.6.3   Relationship with Liquid Cash Balances

Having documented the effects of the nudge and investigated the factors explaining our null result, we wanted to understand *why* consumers are not paying more on their credit card. The most natural potential explanation is that many consumers have limited liquid cash balances available, which prevents or disincentivizes them from paying down credit card debt. While we may term these liquidity constraints, we caveat that limited liquid cash balances is an observable financial outcome that may arise for many reasons such as financial illiteracy (e.g., Lusardi and Tufano, 2015) and behavioral factors such as naïve present bias leading to impulsive overconsumption (e.g., Heidhues and Kőszegi, 2015).

We explore this in tertiary analysis by constructing three measures of liquidity from our linked bank account data (additional details in Appendix Section 3.9.5). We measure liquid cash balances before card opening. For each measure we show, in Figure 3.11, its CDF (left panels) and its relationship with credit card payments seven cycles later (right panels). Liquid cash balances are the end-of-day balance in bank accounts by aggregating all liquid cash held across checking and non-checking, instantly-accessible cash savings accounts. In the UK, checking accounts often have an overdraft line of credit facility, so liquid cash measures can have negative balances. Based on observed socio-economic characteristics, we expect this selected sample with linked data to be less liquidity constrained than those unobserved (Appendix Table 3.23). We do not have sufficient power to estimate heterogeneous treatment effects by liquid cash balances but describing these data represents an advance on prior credit card research where liquid cash is unobserved (e.g., Keys and Wang, 2019; Medina and Negrin, 2022) or only observed at a monthly frequency (e.g., Vihriälä, 2022).

Our first measure shows approximately 10% of linked cardholders have a zero or negative

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liquid cash balance available at a point-in-time before card opening (Figure 3.11, Panel A). Figure 3.11, Panel D shows consumers who had small, positive liquid cash balances (before card opening) repaid more of their credit card debt, on average, seven cycles later than those with zero or small negative liquid cash balances.

Our other two measures are innovative as they consider the dynamics of liquid cash balances. Our second measure examines a consumer's minimum liquid cash balances over the last 90 days before card opening (along with other time horizons). This accounts for how consumers' finances vary over time; one point-in-time does not reflect how liquidity varies at different points-in-time for different consumers depending on the timing of their incomes and expenditures. Prior literature does not examine heterogeneity by the *minimum* balance but studies different moments: the mean or median balance (see Appendix Section 3.9.5).

This second measure shows a lot of bunching of consumers just managing to keep positive, but small, liquid cash balances (Figure 3.11, Panel B). This second measure reveals effectively zero minimum liquid cash balances for approximately 50% of consumers and near zero for over 75% of consumers: far higher than the 10% a point-in-time liquid cash balance measure (Figure 3.11, Panel A) would indicate. Using a 90 day window the median minimum liquid cash balance is effectively zero (£5) and the 75th percentile only £142. Figure 3.11, Panel E shows consumers with positive minimum liquid cash balances (before card opening) discontinuously repaid approximately 20 pp more, on average, of their credit card debt seven cycles later than those with zero or small negative liquid cash balances.

Our third measure shows the volatility of a consumer's finances. It records the number of days a consumer's liquid cash balance drops below £100 in the thirty days before card opening (along with earlier points-in-time pre-card opening). Approximately 60% of consumers have a low liquid cash balance for one or more days a month. Figure 3.11, Panel F also shows a clear relationship: consumers who have more days with low liquid cash balances (pre-card opening) pay down less credit card debt seven cycles later.

Our results help to understand why these consumers are less "nudge-able" than they first

appeared from their Autopay choices and inert minimum payment behavior. Consumers appear to be making "low" credit card payments and offsetting the nudge to not reduce their debt due to frequently holding limited liquid cash balances. Such limited liquidity may also mean other policies attempting to increase consumers' credit card payments may also ultimately fail to change real outcomes.

3.7 Concluding Discussion

We show how an active choice nudge significantly changes consumer Autopay enrollment choices but has no real economic effects on reducing credit card debt. This is explained by offsetting consumer responses and consumers holding limited liquid cash balances. Our study highlights the need to evaluate nudges on their effects on real economic outcomes and, where possible, do so with ex-ante tests. Otherwise consumer financial protection regulations that sound appealing – and may even change enrollment or other proximate choices – may be introduced that are costly and ineffective at changing distal real economic outcomes e.g., as is only discovered ex-post with the US CARD Act disclosures (Agarwal et al., 2015b; Keys and Wang, 2019).

3.8 Figures & Tables

Figure 3.1: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount, shown by Autopay enrollment
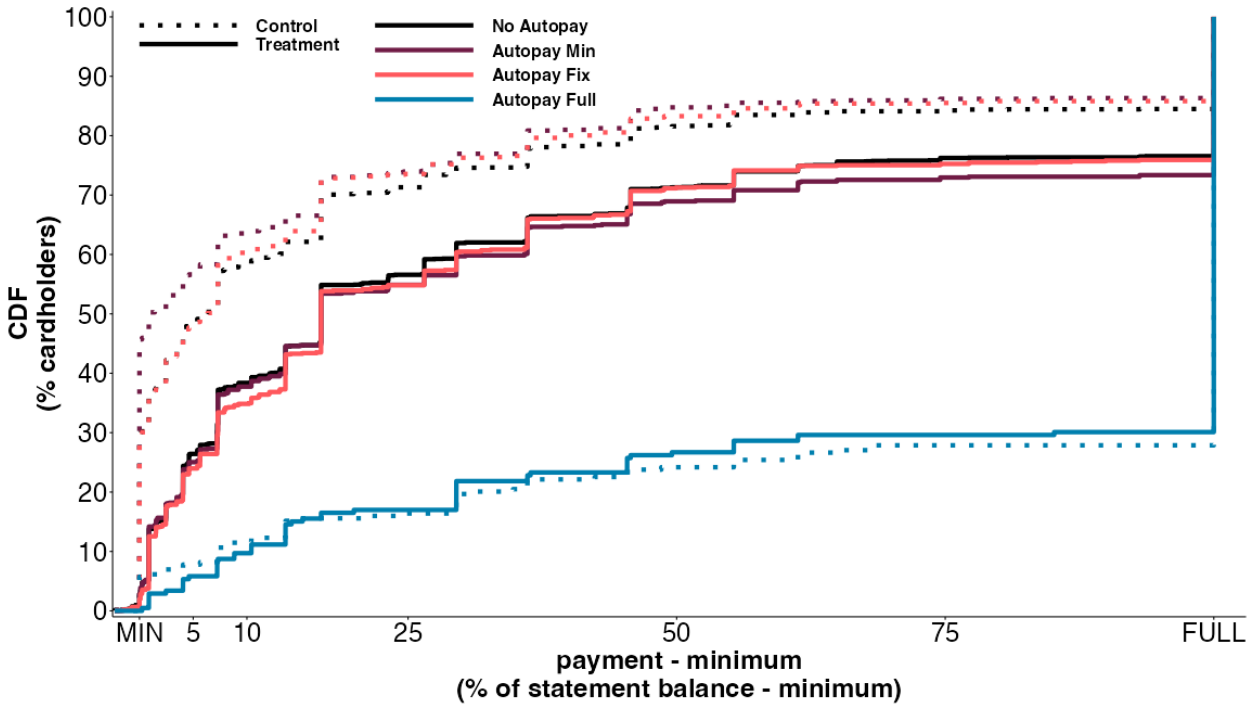
Notes: N = 7,938. Dotted lines are control group where minimum payment amount is displayed. Solid lines are treatment group where minimum payment amount is shrouded. Color of lines show Autopay enrollment observed in administrative data.

Figure 3.2: Autopay enrollment choice architecture presented to control (panel A) and treatment (panel B) groups

A: Control

B: Treatment

Figure 3.3: Autopay enrollment for control and treatment groups after two statements, split by lender

B: MAIN LENDER (N = 40,708)

B: SECOND LENDER (N = 1,531)
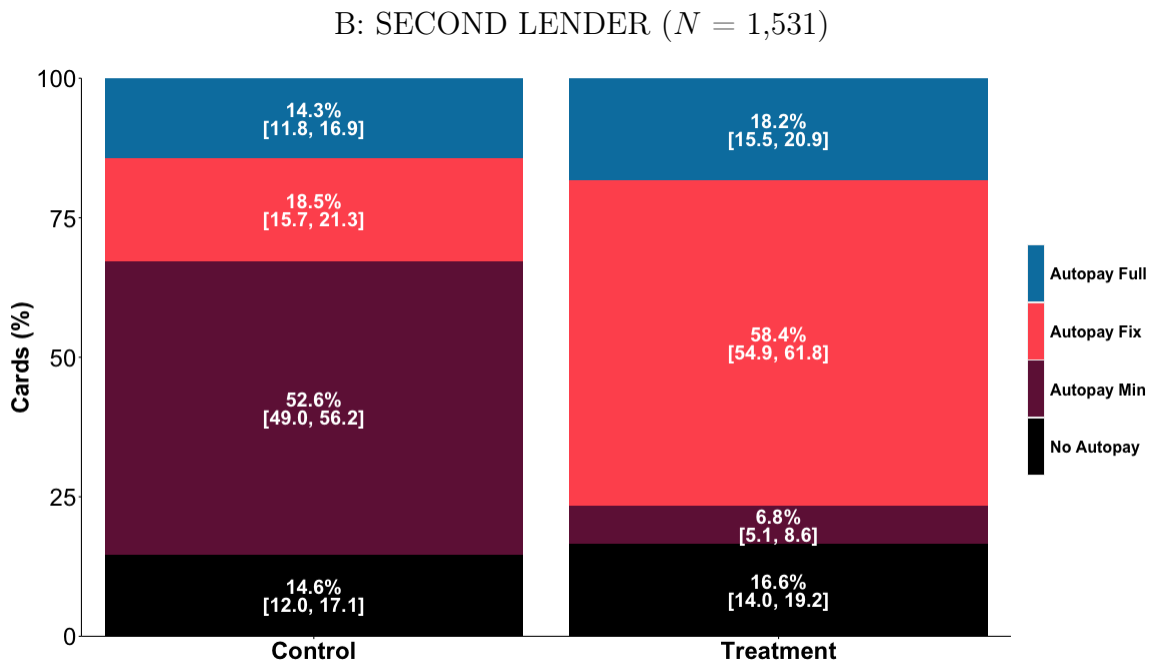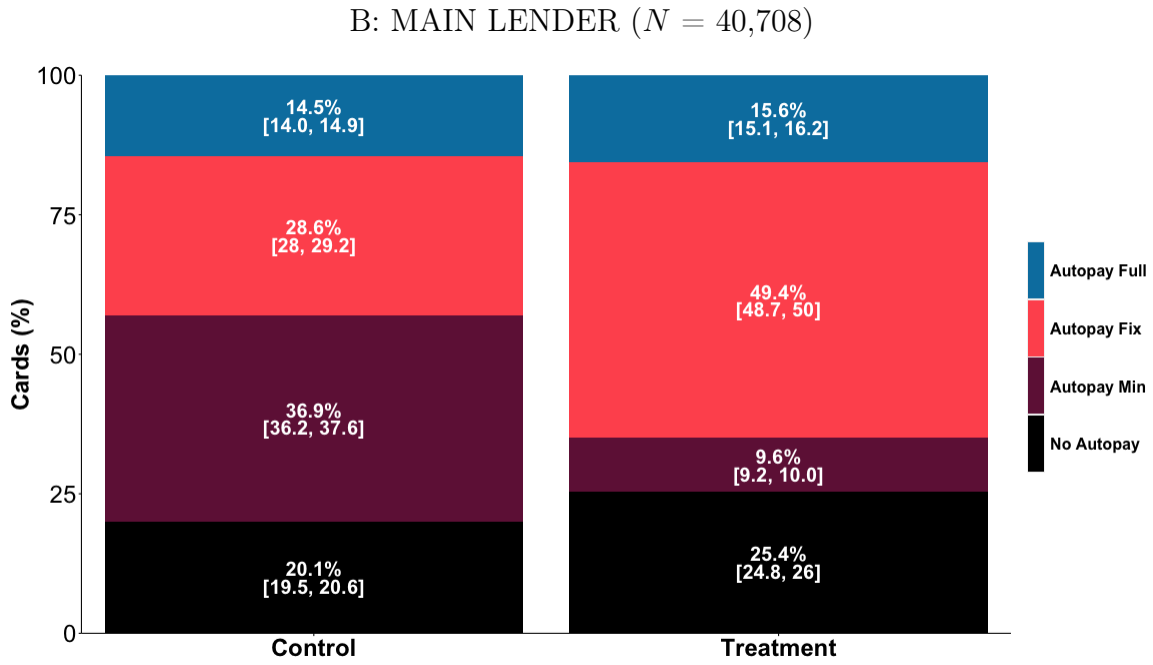
Notes: Numbers display percentage of cards enrolled in each type of Autopay by the second statement cycle. 95% confidence intervals in [ ].

Figure 3.4: Average treatment effects on Autopay Fix enrollment (purple) and Autopay Fix enrollment not binding at minimum payment (pink) across 1-11 statement cycles

Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is any automatic fixed payment enrollment, and outcome in Panel B is any automatic fixed payment enrollment not binding at minimum payment. Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Figure 3.5: Average treatment effects on paying only the minimum payment across 1-11 statement cycles
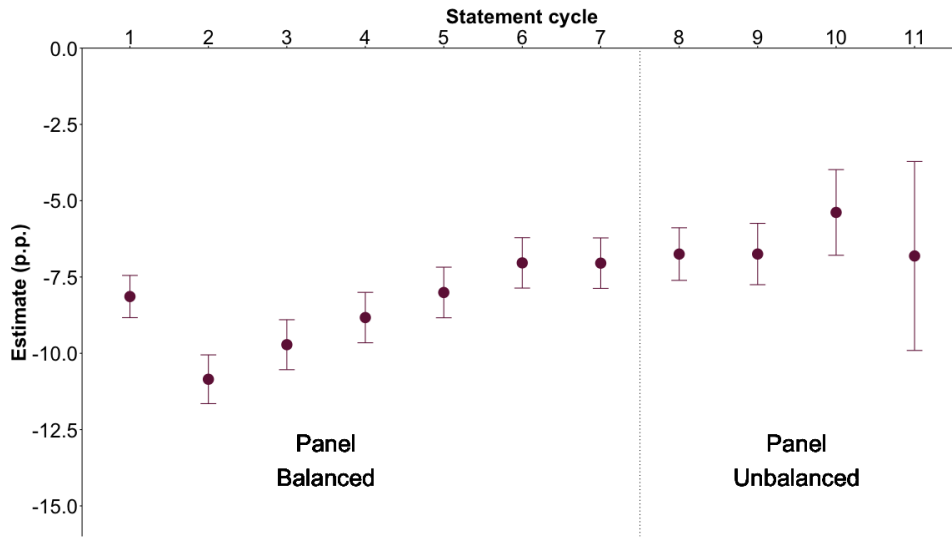
Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome is paying only exactly the minimum payment. Regression also includes statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Figure 3.6: Average treatment effects on credit card debt across 1-11 statement cycles

A: Statement balance net of payments (% statement balance)
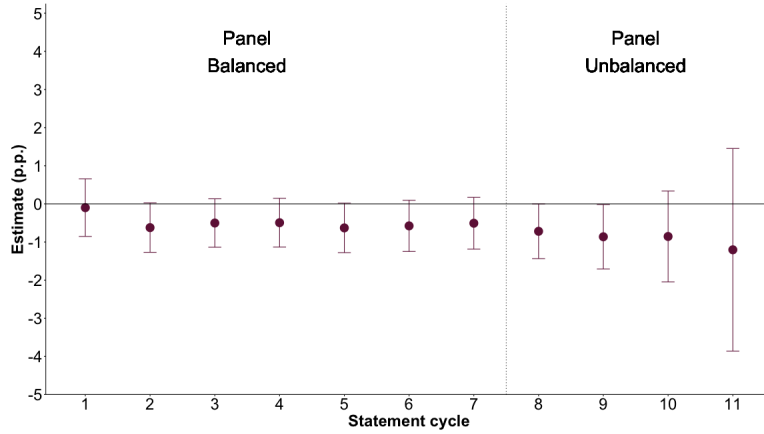
B: Statement balance net of payments (£)
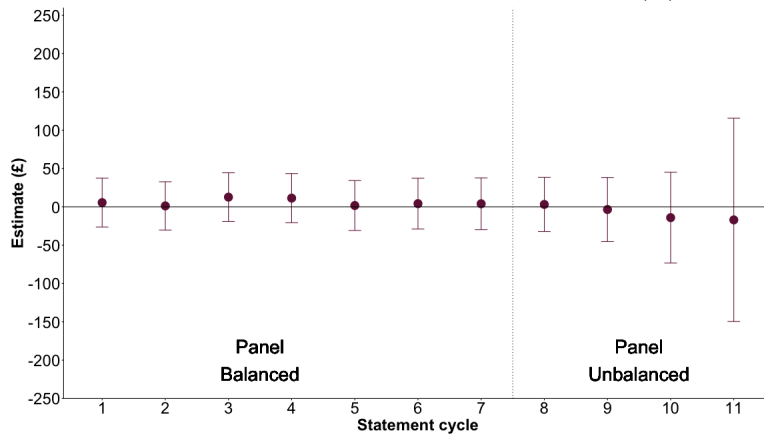
Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is statement balance net of payments (% statement balance), and outcome in Panel B is statement balance net of payments. Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Figure 3.7: Average treatment effects on statement balances and spending across 1-11 statement cycles
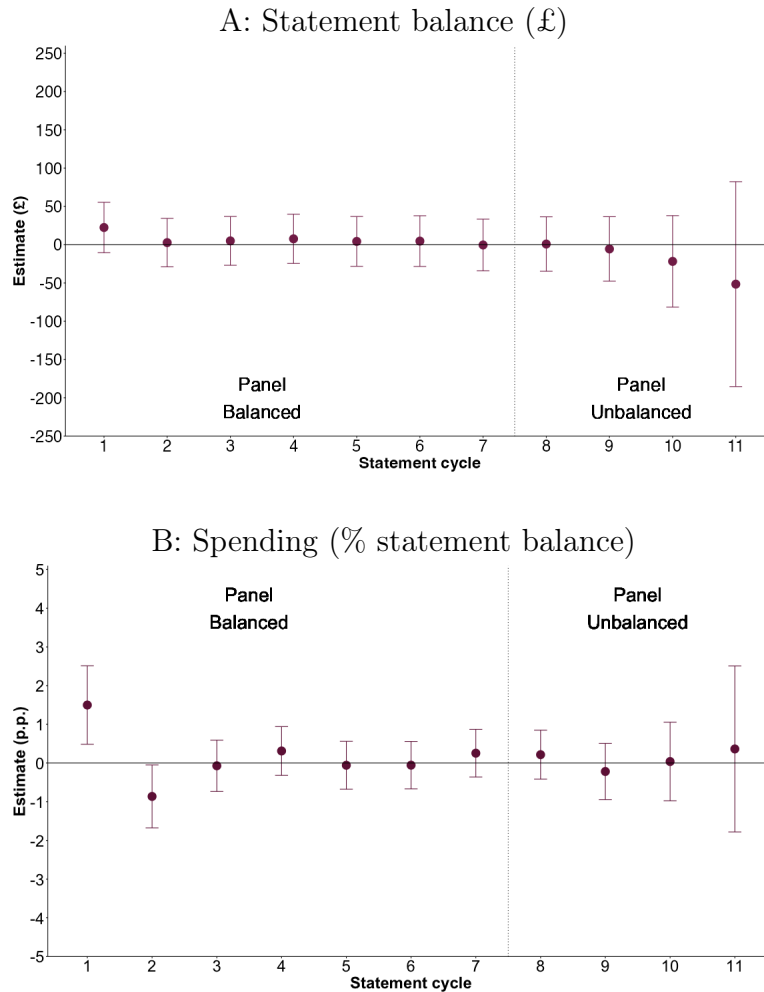
A: Statement balance (£)

B: Spending (% statement balance)

Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is statement balance, and outcome in Panel B is spending (% statement balance). Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Figure 3.8: CDF of Autopay Fix payment amounts for those enrolled in Autopay Fix in the treatment group after seven statements

A: Autopay fix amount (£) at cycle 7
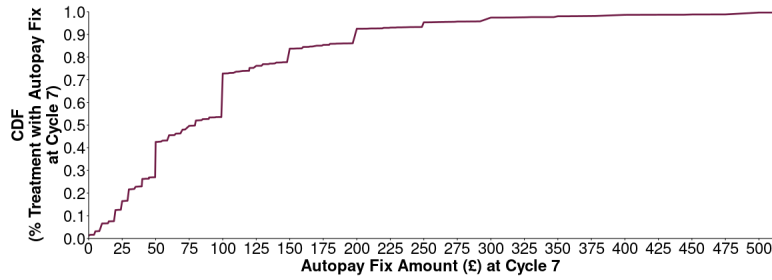
B: Cumulative autopay fix amount in excess of minimum (£) across cycles 1-7
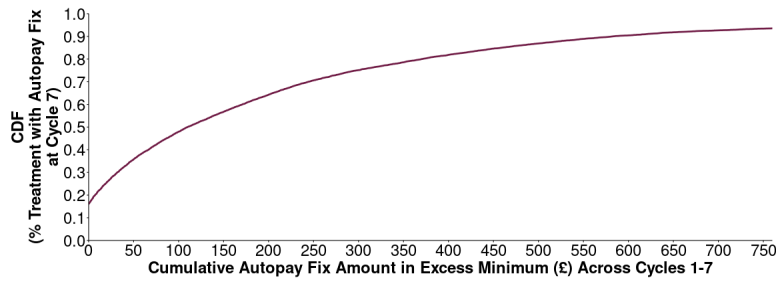
C: Autopay fix amount (% statement balance) at cycle 7
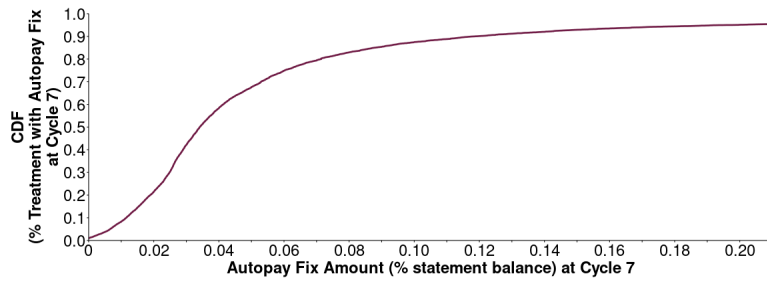
D: Autopay fix amount in excess of minimum (% statement balance) at cycle 7
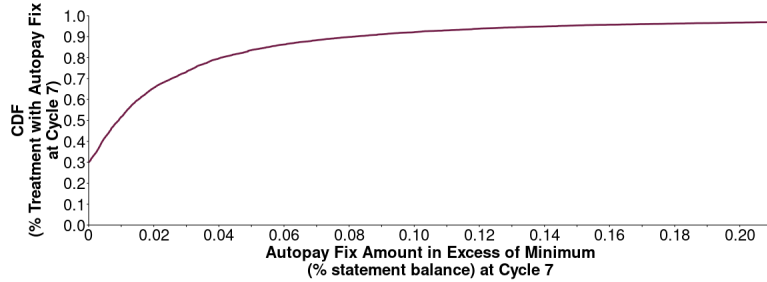
Notes: X-axes of CDFs are right-censored to ease presentation. CDFs are of 9,337 credit cards that are in treatment group and enrolled in Autopay Fix at cycle 7.

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Figure 3.9: Average treatment effects on automatic, manual, and total (automatic + manual) payments across 1-10 statement cycles

A: Payments (% statement balance)

B: Cumulative Payments (£)
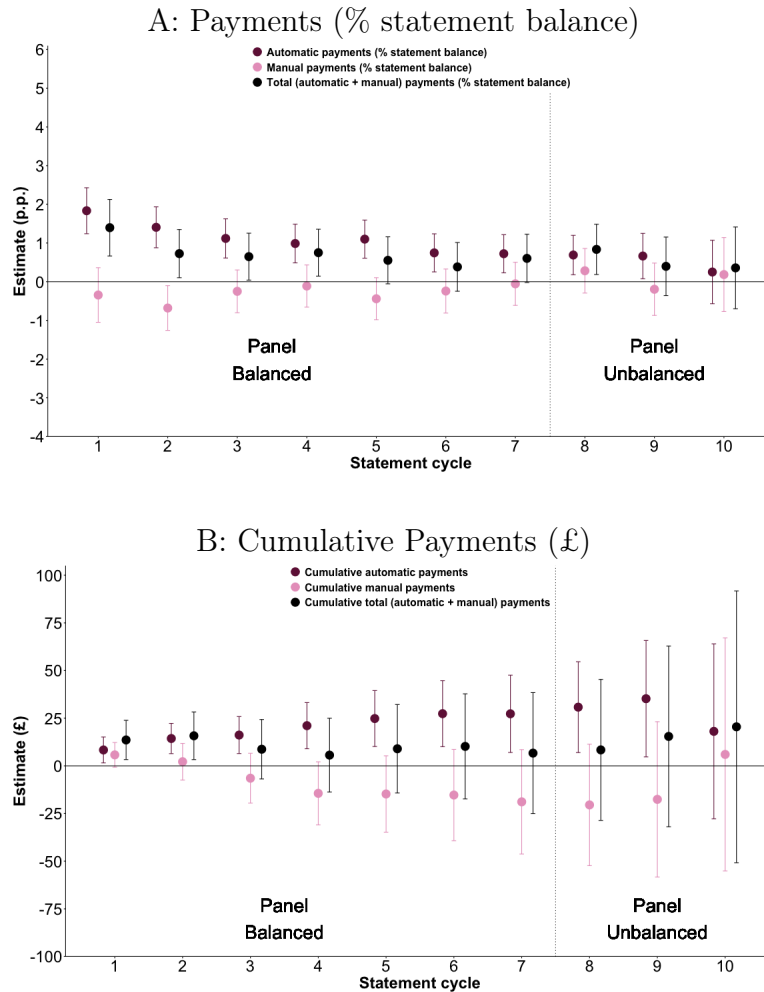
Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is payments (% statement balance), and outcome in Panel B is cumulative payments (£). Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals on Panel B are extremely large such that estimates are uninformative. 40,708 credit cards with 368,162 observations.

Figure 3.10: Estimates on cumulative payments decomposed by any Autopay enrollment after seven statement cycles

A: Causal estimate for all cards ($N = 40,693$)
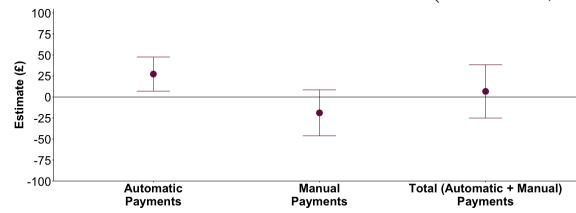
B: Non-causal decomposition for cards enrolled in any Autopay ($N = 31,052$)

C: Non-causal decomposition for cards not enrolled in any Autopay ($N = 9,641$)
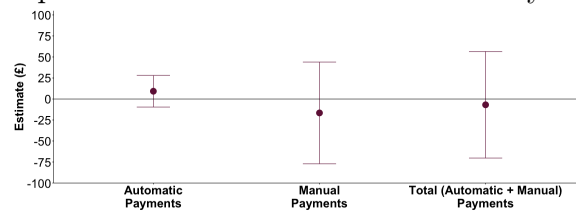
Notes: Panel A is causal estimated treatment effects from coefficient (δ_7) on interaction terms between treatment indicator and the seventh statement cycle indicator in OLS regression specified in Equation 3.1. Each panel shows outcomes from three separate regressions where outcomes are: cumulative automatic payments, cumulative manual payments, and cumulative total (automatic +) manual) payments. Regressions also include: interactions between treatment indicator and other statement cycles, statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. Panel A contains 40,708 credit cards with 368,162 observations. Panels B and C show non-causal estimates (δ_7) from OLS regression specified in Equation 3.3 which has the same specification as explained above except Panel B restricts to the subsample of 31,052 credit cards that are enrolled in any Autopay at statement cycle 7, and Panel C restricts to the subsample of 9,641 credit cards that are not enrolled in any Autopay at statement cycle 7.

Figure 3.11: CDFs of liquid cash balances measured before card opening (left hand side panels) and their non-parametric relationships with credit card debt (statement balance net of payments as a % of statement balance) at statement cycle 7, by treatment group (right hand side panels)
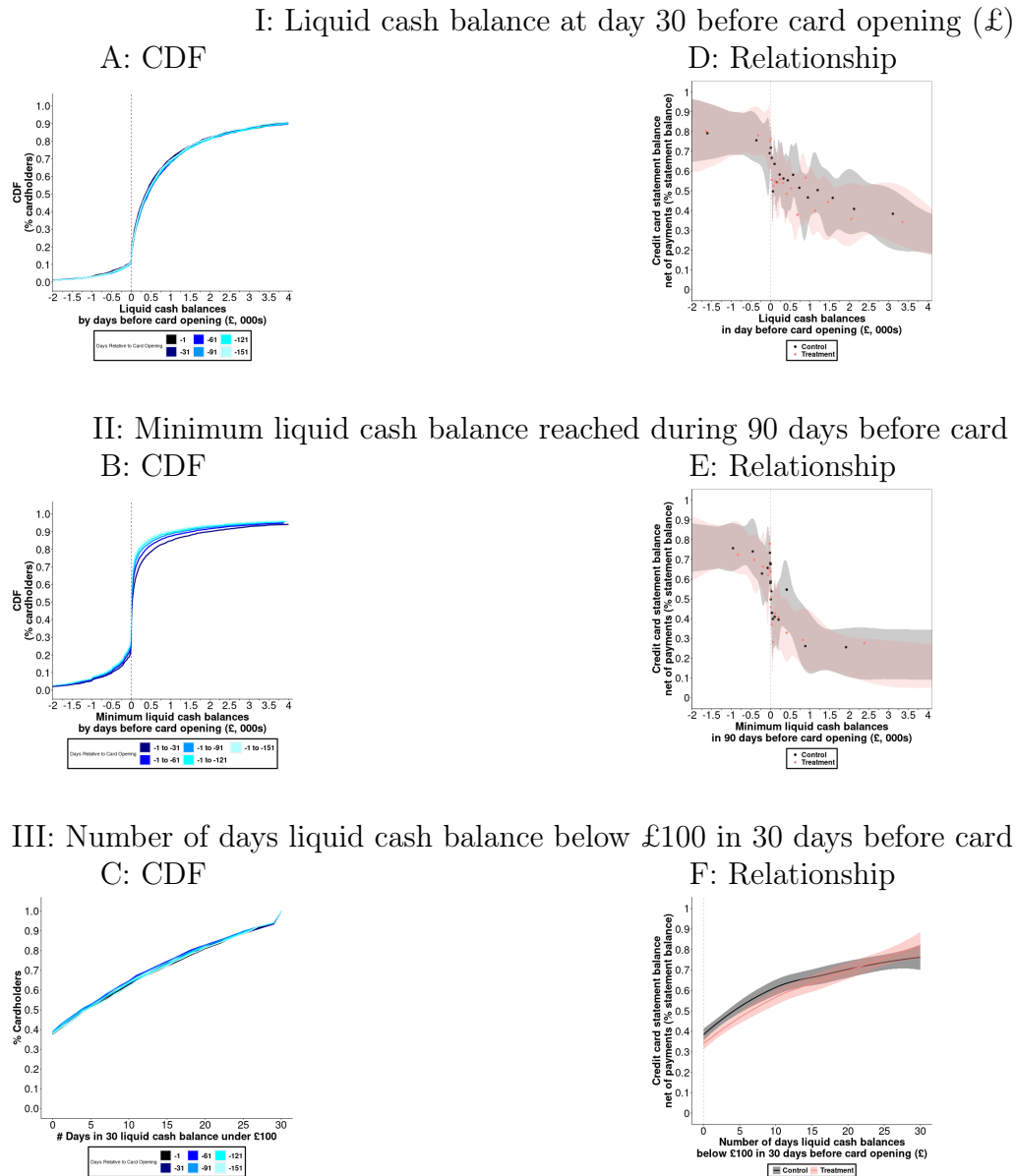
I: Liquid cash balance at day 30 before card opening (£)

A: CDF

D: Relationship

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

B: CDF

E: Relationship

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

C: CDF

F: Relationship

Notes: N = 3,753 consumers. Liquid cash balances are measured before credit card opening. Panels A., B. and C. are CDFs. Panel F. is loess, Panels D. and E. are binscatters by quantiles of the distribution where error bands are 95% confidence intervals. X-axes of A, B, D, and E are censored to ease presentation given a fat tail to the distribution of these variables.

Table 3.1: Average treatment effects on Autopay enrollment after seven statement cycles

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

Notes: Table shows average treatment effects after seven statement cycles. Each row of table shows estimates from separate regressions with different outcomes. Estimated treatment eff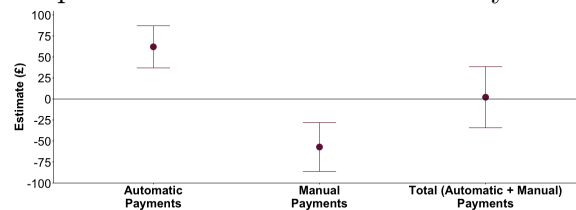ects from coefficient (δ_7) on interaction terms between treatment indicator and the seventh statement cycle indicator in OLS regression specified in Equation 3.1. Regressions also include: interactions between treatment indicator and other statement cycles, statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Table 3.2: Average treatment effects for primary outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any minimum payment	-0.0705 (0.0042)	[-0.0787, -0.0622]	0.0000	0.3012
Any full payment	0.0040 (0.0037)	[-0.0032, 0.0112]	0.2747	0.2397
Any missed payment	0.0038 (0.0019)	[0.0002, 0.0075]	0.0409	0.0369
Statement balance net of payments (% statement balance)	-0.0051 (0.0035)	[-0.0119, 0.0017]	0.1428	0.6936
Costs (% statement balance)	-0.0003 (0.0006)	[-0.0015, 0.0010]	0.6782	0.0111
Spending (% statement balance)	0.0025 (0.0031)	[-0.0036, 0.0087]	0.4199	0.2007
Share of credit card portfolio only paying minimum	-0.0264 (0.0027)	[-0.0317, -0.0210]	0.0000	0.2012
Share of credit card portfolio making full payment	0.0011 (0.0033)	[-0.0054, 0.0076]	0.7340	0.4414
Share of credit card portfolio missing payment	-0.0000 (0.0013)	[-0.0025, 0.0024]	0.9701	0.0236
Credit card portfolio balances net of payments (% statement balances)	-0.0053 (0.0031)	[-0.0115, 0.0008]	0.0896	0.6954

Notes: Table shows average treatment effects after seven statement cycles. Each row shows estimates from separate regressions with different outcomes. Estimated treatment effects from coefficient (δ_7) on interaction terms between treatment indicator and the seventh statement cycle indicator in OLS regression specified in Equation 3.1. Regressions also include: interactions between treatment indicator and other statement cycles, statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are at card origination except for the variables constructed from credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month before card origination. For credit reporting data outcomes, up to eleven dummies for lags of outcomes are included as controls (X_i') for months preceding the experiment's start. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level.40,708 credit cards with 368,162 observations.

3.9 Appendix to ''*The Semblance Of Success In Nudging Consumers To Pay Down Credit Card Debt*''

3.9.1 Theoretical Motivations

Autopay Min may be appealing because of multiple mutually compatible economic and psychological factors. We discuss how these inform our field experiment's design.

Anchoring

The nudge tested in our field experiment removes the minimum payment as an anchor during Autopay enrollment. We purposefully do not include an alternative recommended Autopay Fix amount because we do not want to replace the minimum payment anchor with another anchor (other than the Autopay Full anchor). We want consumers to make active choices Carroll et al. (2009) or be anchored to the Autopay Full option. This design choice is motivated by US studies (Agarwal et al., 2015b; Hershfield and Roese, 2015; Keys and Wang, 2019) which find that providing consumers with credit card repayment scenarios can unintentionally reduce payments for some consumers. Bartels et al. (2024) and Schwartz (2024) show how the minimum payment can act as a target rather than an anchor, and therefore by removing it the full balance can become a new target.

Financial Literacy

We hypothesized that some consumers' decision to enroll in Autopay Min reflects an imperfect understanding of the costs associated with this option.

Cardholders often make non-optimal repayment choices (e.g., Ponce et al., 2017; Gathergood et al., 2019c,b; Hirshman and Sussman, 2022) with prior literature showing that credit card lenders structure products and marketing to exploit a lack of sophistication (e.g., Gabaix and Laibson, 2006; Ru and Schoar, 2020). Approximately half of credit cardholders

in one UK survey incorrectly thought the minimum payment is the amount most people repaid, when in fact only a quarter do (FCA, 2016b). Studies across countries show cardholders significantly overestimate the speed at which debt is cleared (and by implication underestimating the interest costs) if only the minimum payment is made (e.g., Lusardi and Tufano, 2015; Seira et al., 2017; Adams et al., 2022a). Such responses are consistent with exponential growth bias Stango and Zinman (2009b).

Among a survey of UK Autopay Min enrollees 96% of respondents underestimate the time it would take to fully repay a debt if the cardholder made only the minimum required payment (Adams et al., 2022a). Informational disclosures to credit cardholders to address financial illiteracy are ineffective at changing consumer behavior across the US (Agarwal et al., 2015b), Mexico (Seira et al., 2017), and the UK (Adams et al., 2022a).

In Adams et al. (2022a) we conduct field trials across three lenders testing whether personalized, informational nudges explicitly encouraging debt repayment via standalone emails or letters to credit cardholders already enrolled in Autopay Min could change behavior. These interventions had zero or small effects on Autopay enrollment and are ineffective at reducing debt. Given the ineffectiveness of disclosures and informational nudges, our nudge in this paper tests a more intrusive intervention. Our nudge is designed as a policy that can be applied at low-cost to apply at scale (primarily involving a one-time IT compliance cost), in contrast to more costly policies attempting to increase financial literacy.

Inertia and Limited Attention

Consumers may enroll in Autopay for convenience: providing insurance against forgetting to pay a bill. Yet Autopay means credit cardholders no longer need to actively decide each month how much to pay and may become inattentive to their debt and procrastinate on paying it down (e.g., Sakaguchi et al., 2022).

Our nudge is targeted at new card originations to be a preventative measure against inert consumers persistently carrying high credit card debt. We nudge Autopay enrollment

at card origination because these initial Autopay decisions are sticky (e.g., Sakaguchi et al., 2022; Adams et al., 2022a; Wang, 2023a). Sticky Autopay enrollments may arise from limited attention (Sakaguchi et al., 2022). Indeed this is consistent with another domain; Sexton (2015) argues that enrollment into Autopay (Full) for utility bills, reduces price salience and results in 'overconsumption' of electricity.

Targeting behavior at the time of card origination is expected to be more likely to succeed than trying to change habitual cardholder behavior. Consumer inertia is common across household financial domains, including simple decisions such as cash savings (e.g., Adams et al., 2021) and high stakes decisions such as mortgage origination and refinancing (e.g., Andersen et al., 2020). Our nudge attempts to harness inertia by getting consumers to initially enroll in an Autopay Fix (or Autopay Full). Psychological frictions push against consumers exerting effort to frequently change their Autopay choice.

Without an explicit Autopay Min option consumers with limited attention may be forced to make an active choice (e.g., Carroll et al., 2009; Keller et al., 2011) – calculating how much they can afford to regularly pay each month. The nudge makes it difficult for inattentive consumers to default into automatically paying only the minimum. We purposefully design our nudge to not specify a default Autopay choice (other than Autopay Full). A lack of a low-payment default may be socially optimal if there is a high degree of heterogeneity in consumers' socio-economic circumstances and preferences (e.g., Carroll et al., 2009). This is especially likely if there is information asymmetry – making it impractical to implement an optimized individual policy default for heterogeneous consumers. In the domain of retirement savings, Carroll et al. (2009) discuss how a default asset allocation may be optimal but it may be preferable to set contribution rates by active choice given heterogeneity in optimal savings rates. Keller et al. (2011) and Cronqvist and Thaler (2004) present more discussion of comparisons of defaults and active choices in retirement savings.

Present Bias

Present bias (Laibson, 1997; O'Donoghue and Rabin, 1999) may also contribute to low credit card repayments. Theoretical models without present bias struggle to simultaneously explain observed levels of credit card debt and wealth formation (Laibson et al., 2024). If naïve, present biased consumers are over-consuming, this generates welfare losses and therefore provides a rationale for nudging consumers to repay more (e.g., Heidhues and Kőszegi, 2010, 2015; Allcott et al., 2022). The empirical literature finds present biased consumers hold more credit card debt (Meier and Sprenger, 2010) and generally fail to stick to their plans to pay it down (Kuchler and Pagel, 2021).

A present biased consumer may enroll in Autopay Min with the intention of making additional manual payments to reduce debt, however, they may not follow through (O'Donoghue and Rabin, 1999). There is evidence (e.g., Kuchler and Pagel, 2021) that consumers want to repay their debt more quickly than they do. For example, the average respondent enrolled in Autopay Min self-reports wanting to repay their credit card debt in three years, which is substantially faster than the six years they expect it to take, and the eighteen years it would actually take at Autopay Min (Adams et al., 2018a).

Limited Liquidity

Limited liquidity would be a standard economic explanation for consumers enrolling in Autopay Min. Limited liquidity may arise for either classical reasons (e.g., a relatively high exponential discount rate or an adverse income/spending shock) or behavioral reasons (e.g., present bias). Limited liquidity may weaken the effectiveness of our intervention. Consumers who anticipate that they are likely to have low levels of future liquidity may want the flexibility that arises from a low automatic payment. Such consumers may replace Autopay Min with a low fixed automatic payment.

3.9.2 Survey Experiment

Figure 3.12: Choice architecture in survey experiment presented to control (panel A) and treatment (panel B) groups

A: Control

B: Treatment

Notes: Survey experiment where treatment shrouds minimum payment amount. Consumers have to decide how much to pay on a hypothetical credit card balance. Consumers were randomized into (i) control or treatment and (ii) a low or high statement balance scenarios. Example is shown for low statement balance scenario. The high balance scenario was identical except with a statement balance amount of £3,217.36 and a minimum payment amount due of £72.38.

Figure 3.13: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount
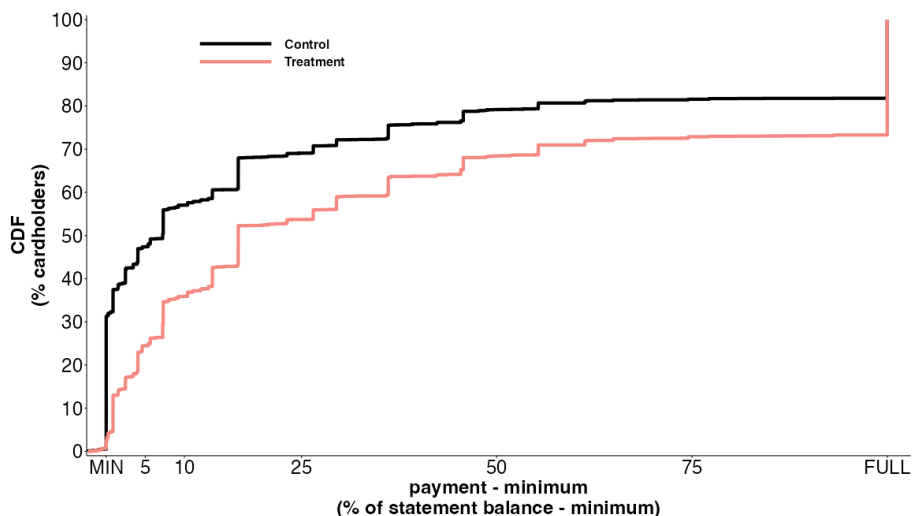
Notes: N = 7,938. Black line is control group where minimum payment amount is displayed. Orange line is treatment group where minimum payment amount is shrouded.

Figure 3.14: Distribution of hypothetical credit card payment choices from survey experiment where treatment shrouds minimum payment amount, shown by self-reported financial distress
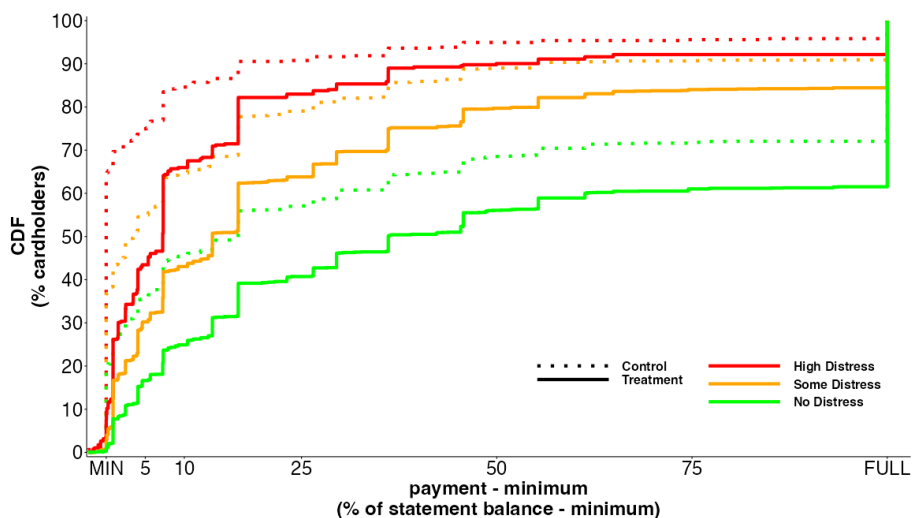
Notes: N = 7,938. Dotted lines are control group where minimum payment amount is displayed. Solid lines are treatment group where minimum payment amount is shrouded. Color of lines show self-reported financial distress.

Table 3.3: Average treatment effects on hypothetical credit card payments from survey experiment where treatment shrouds minimum payment amount

	(1) Payment (% statement balance)	(2) Any full payment	(3) Any minimum payment	(4) Any missed payment
Intercept	0.3783***	0.2408***	0.2491***	0.0000
	(0.0072)	(0.0081)	(0.0073)	(0.0010)
High Balance	-0.1951***	-0.1223***	0.1144***	0.0127***
	(0.0082)	(0.0088)	(0.0075)	(0.0019)
Treatment	0.1240***	0.0840***	-0.2947***	0.0020
	(0.0082)	(0.0092)	(0.0073)	(0.0019)
Control Mean	0.2813	0.1800	0.3060	0.0063

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N = 7,938. Table shows coefficients on high balance scenario indicator (baseline low balance), treatment effect indicator (baseline control) from OLS regressions predicting hypothetical credit card payment decision from survey experiment. Robust standard errors in parenthesis.*

Estimating heterogeneous treatment effects by self-reported financial distress

We estimate an OLS regression (with robust standard errors) shown in Equation 3.4. We include dummies for if the respondent (i) is randomly assigned to the high balance amount presented ($HighBalance_i$) and is randomly assigned to the de-anchoring treatment ($Treatment_i$).

We use an official UK self-reported measure of financial distress used by the Office for National Statistics. Gathergood and Guttman-Kenney (2016) shows this measure is correlated with other measures of financial distress as well as also with measures of subjective well-being. Respondents are asked how well they are keeping up with bills and commitments and we split responses into three groups indicating the severity of financial distress: no distress (the omitted category), some distress, and high distress. The survey question is: "Which of the following statements best describes how well you are keeping up with your bills and credit commitments at the moment?" Respondents can choose from the following options: "1. Keeping up with all of them without any difficulties; 2. Keeping up with all of them, but it is a struggle from time to time; 3. Keeping up with all of them, but it is a constant struggle; 4. Falling behind with some of them; 5. Having real financial problems and have fallen behind with many of them; 6. Don't have any commitments". For analysis we classify responses 1 and 6 as 'no distress'; 2 as 'some distress'; and 3, 4, and 5 as 'high distress'. 52% of respondents report no distress, 38% some distress, and 11% high distress.

$$Y_i = \alpha + \beta\ HighBalance_i + \gamma_1\ SomeDistress_i + \gamma_2\ HighDistress_i + \delta\ Treatment_i +$$
$$\theta_1 \big(\ Treatment_i \times SomeDistress_i \big) + \theta_2 \big(Treatment_i \times HighDistress_i \big) + \varepsilon_i$$

$$(3.4)$$

The results of this estimation are in Table 3.4. Financially distressed respondents are more likely to pay less than the minimum, more likely to only pay exactly the minimum, and less likely to pay the full balance. The effect of the treatment de-anchoring manual payments

is significantly lower for the most distressed respondents.

Table 3.4: Heterogeneous treatment effects by self-reported financial distress on hypothetical credit card payments from survey experiment where treatment shrouds minimum payment amount

	(1) Payment (% statement balance)	(2) Any full payment	(3) Any minimum payment	(4) Any missed payment
Intercept	0.4829***	0.3373***	0.1419***	-0.0044
	(0.0095)	(0.0110)	(0.0085)	(0.0013)
High Balance	-0.1948***	-0.1220***	0.1142***	0.0126***
	(0.0077)	(0.0088)	(0.0072)	(0.0019)
Some Distress	-0.1949***	-0.1860***	0.1640***	0.0027
	(0.0110)	(0.0120)	(0.0147)	(0.0020)
High Distress	-0.2836***	-0.2410***	0.4119***	0.0310***
	(0.0132)	(0.0131)	(0.0241)	(0.0084)
Treatment	0.1344***	0.1057***	-0.1947***	0.0016
	(0.0123)	(0.0144)	(0.0087)	(0.0016)
Treatment × Some Distress	-0.0197	-0.0444*	-0.1540***	0.0020
	(0.0164)	(0.0186)	(0.0152)	(0.0033)
Treatment × High Distress	-0.0534**	-0.0680***	-0.3766***	-0.0005
	(0.0207)	(0.0216)	(0.0262)	(0.0126)

*Notes: Statistical significance denoted at *** 0.5%, ** 1.0%, * 5.0%. N = 7,938 of which 4,100 self-report no financial distress, 3,001 some financial distress, and 837 high financial distress. Table shows coefficients on high balance scenario indicator (baseline low balance), treatment effect indicator (baseline control), self-reported financial distress (baseline is no distress), and interaction treatment and financial distress from OLS regressions predicting hypothetical credit card payment decision from survey experiment. Robust standard errors in parenthesis.*

3.9.3   Additional Results for Main Lender

Definitions of Primary Outcomes

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

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

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

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

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

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

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

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

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

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

Figure 3.15: Autopay enrollment for control and treatment groups, by statement cycles one to eight
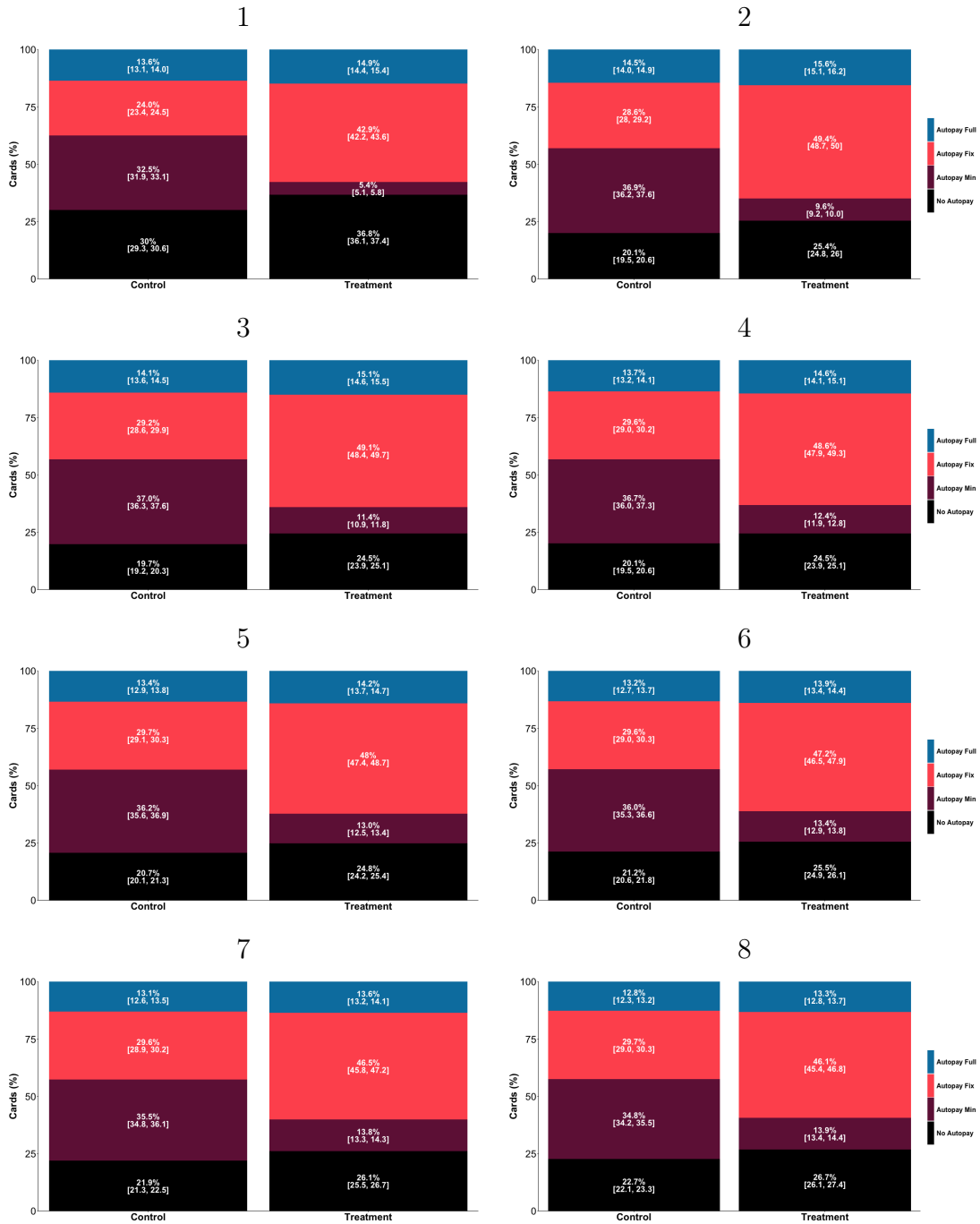
Notes: Numbers display percentage of cards enrolled in each type of Autopay. 95% confidence intervals in [ ]. Cycle 1 is before all treated cards have had 30 days to experience the treatment. Not all cards are observed in cycle 8.

Figure 3.16: Average treatment effects on automatic full (panel A) and minimum (panel B) payment enrollment across 1-11 statement cycles
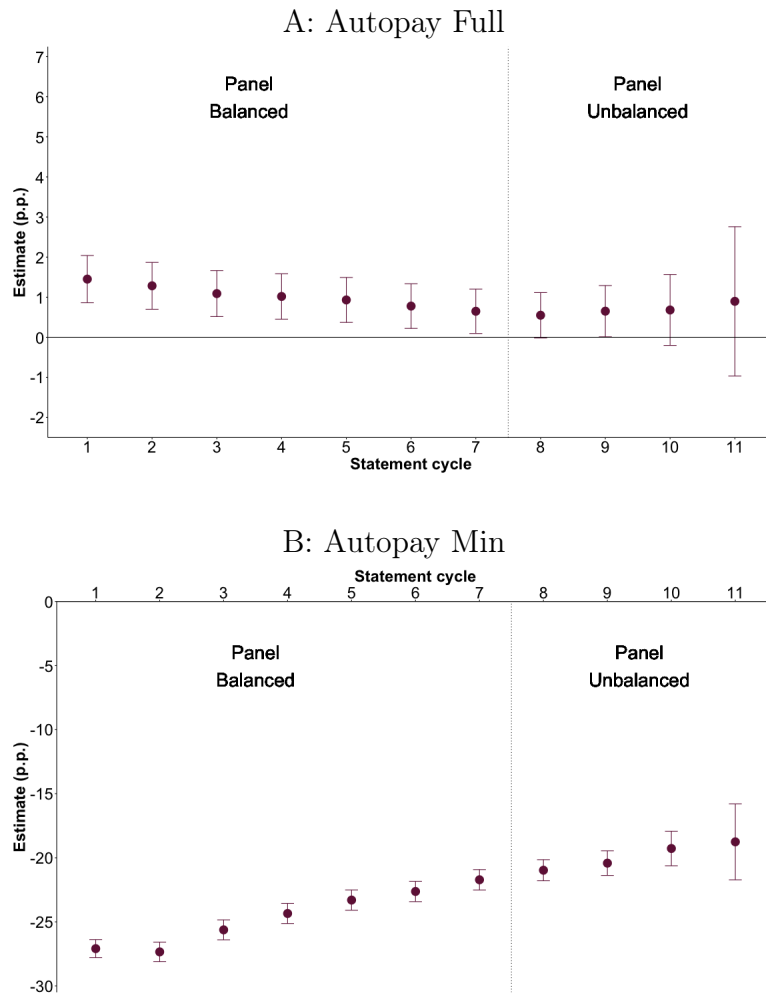
A: Autopay Full

B: Autopay Min

Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is any automatic full payment enrollment, and outcome in Panel B is any automatic minimum payment enrollment. Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Figure 3.17: Average treatment effects on any Autopay enrollment across 1-11 statement cycles
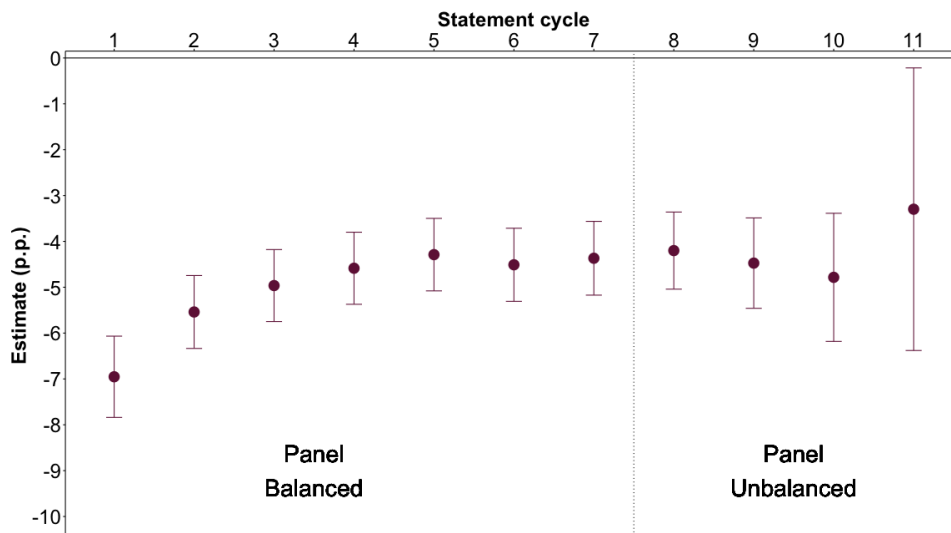
Notes: *Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome is any automatic payment enrollment. Regression also includes statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.*

Figure 3.18: Treatment effects on cumulative number of full, minimum and missed payments across 1-10 statement cycles

A: Cumulative Full Payments
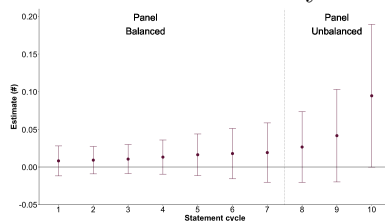
B: Cumulative Minimum Payments
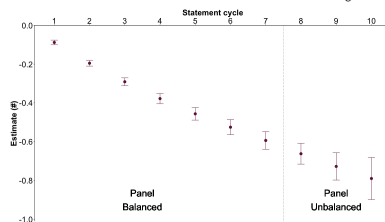
C: Cumulative Missed Payments
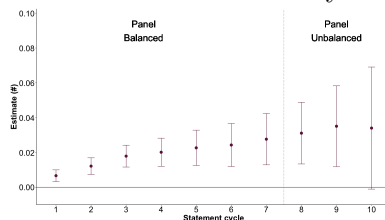
Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is cumulative number of times pay full balance, outcome in Panel B is cumulative number of times pay exactly minimum payment, and outcome in Panel C is cumulative number of times miss a payment. Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that estimates are uninformative. 40,708 credit cards with 368,162 observations.

Figure 3.19: Average treatment effects on primary outcomes on target card across 1-11 statement cycles
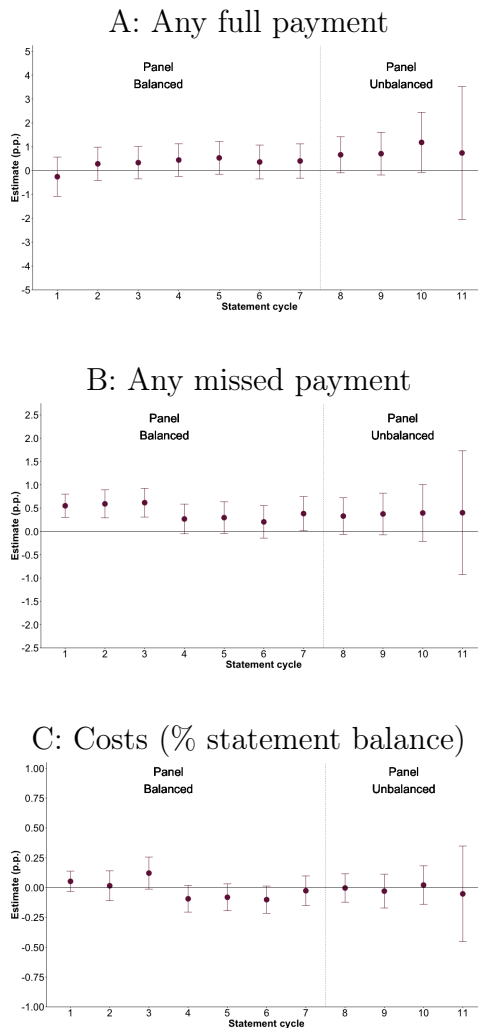
A: Any full payment

B: Any missed payment

C: Costs (% statement balance)

Notes: Treatment effects from coefficients (δ_τ) on interaction terms between treatment indicator and statement cycle indicators in OLS regression specified in Equation 3.1. Regression outcome in Panel A is pay full balance, and outcome in Panel B is any automatic minimum payment enrollment. Regressions also include statement cycle fixed effects, year-month fixed effects, and the following controls: Gender, Age, Age squared, Log Estimated Income, Credit Score, Unsecured Debt-to-Income (DTI) Ratio, Any Mortgage Debt, Log Credit Card Credit Limit, Credit Card Purchases Rate, Subprime Credit Card, Any Credit Card Promotional Rate, Any Credit Card Balance Transfer, Credit Card Open Date, Credit Card Statement Day, Any Credit Card Secondary Cardholder. These are all from the time of card origination except for the variables constructed from consumer credit reporting data (Credit Score, DTI Ratio and Any Mortgage Debt), which are from the month preceding card origination. Error bars are 95% confidence intervals. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Figure 3.20: Average treatment effects on credit card portfolio primary outcomes across 1-11 statement cycles

A: Share of credit card portfolio only paying minimum
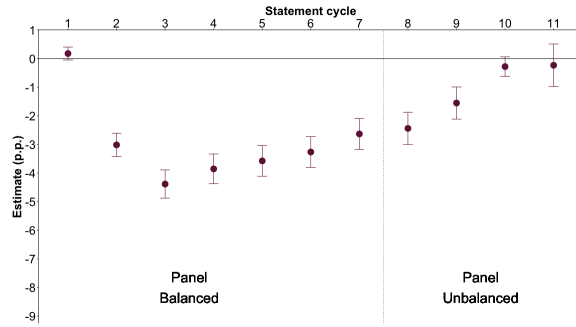
B: Share of credit card portfolio making full payment
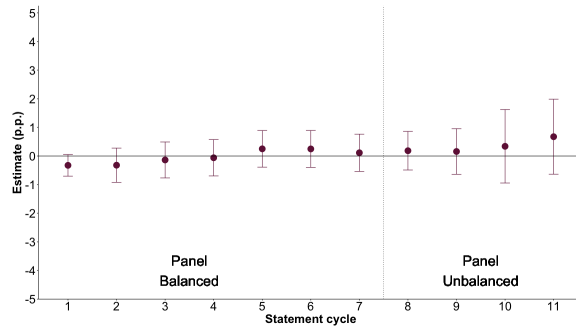
C: Share of credit card portfolio missing payment
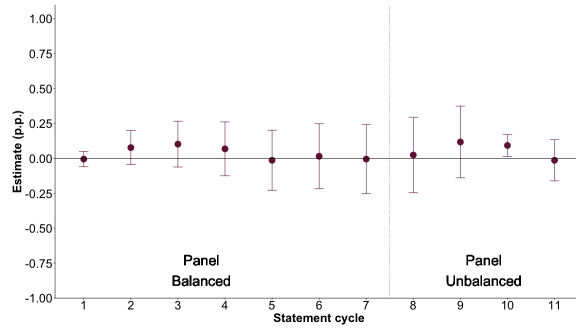
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 3.1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. 40,708 credit cards with 368,162 observations.

Figure 3.21: Average treatment effects on credit card portfolio debt across 1-11 statement cycles

A: Credit card portfolio balances net of payments (% statement balances)
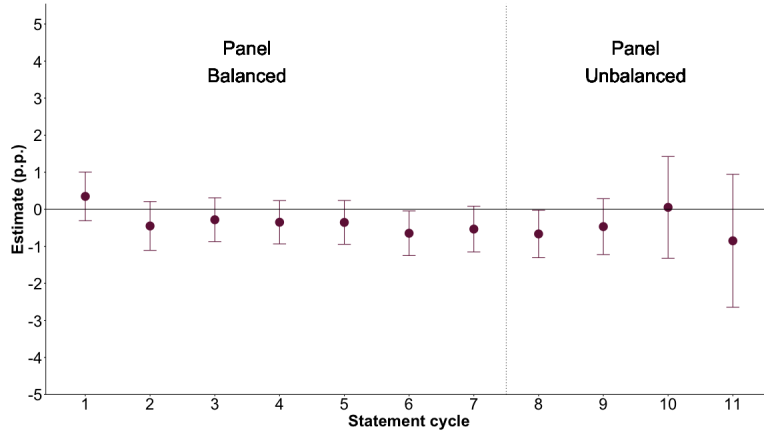
B: Credit card portfolio balances net of payments (£)
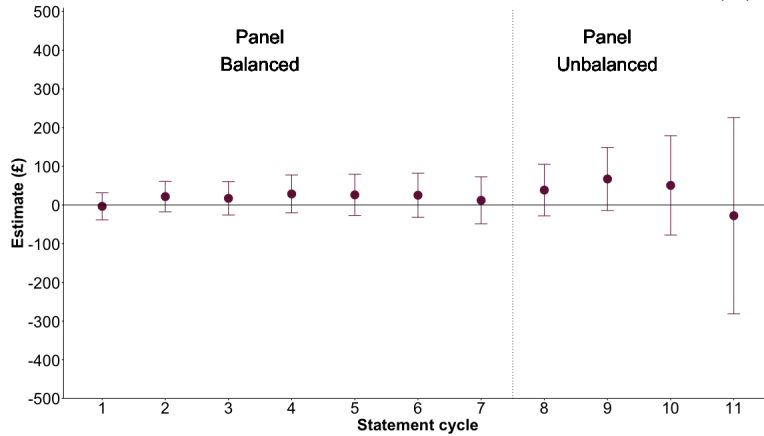
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 3.1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. 40,708 credit cards with 368,162 observations.

Figure 3.22: Average treatment effects on credit card spending across 1-11 statement cycles
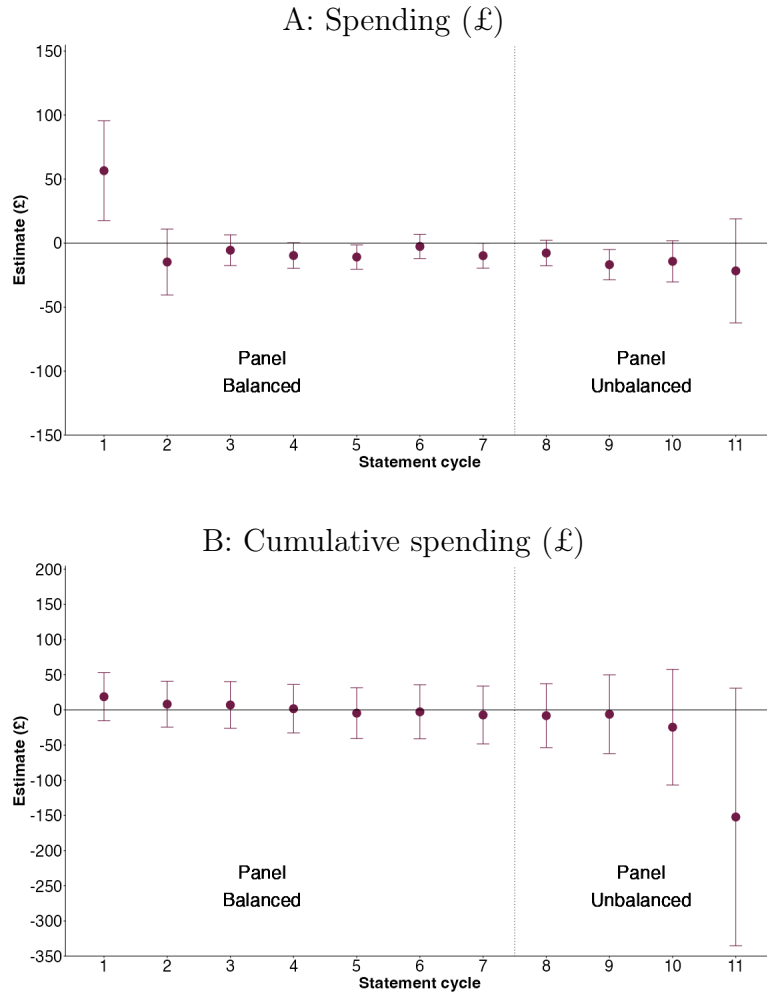
A: Spending (£)

B: Cumulative spending (£)

Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 3.1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. 40,708 credit cards with 368,162 observations.

Figure 3.23: Autopay enrollment - splitting out automatic fixed payments into those that do and do not bind at the minimum payment amount - for control and treatment groups split by statement cycles one to eight
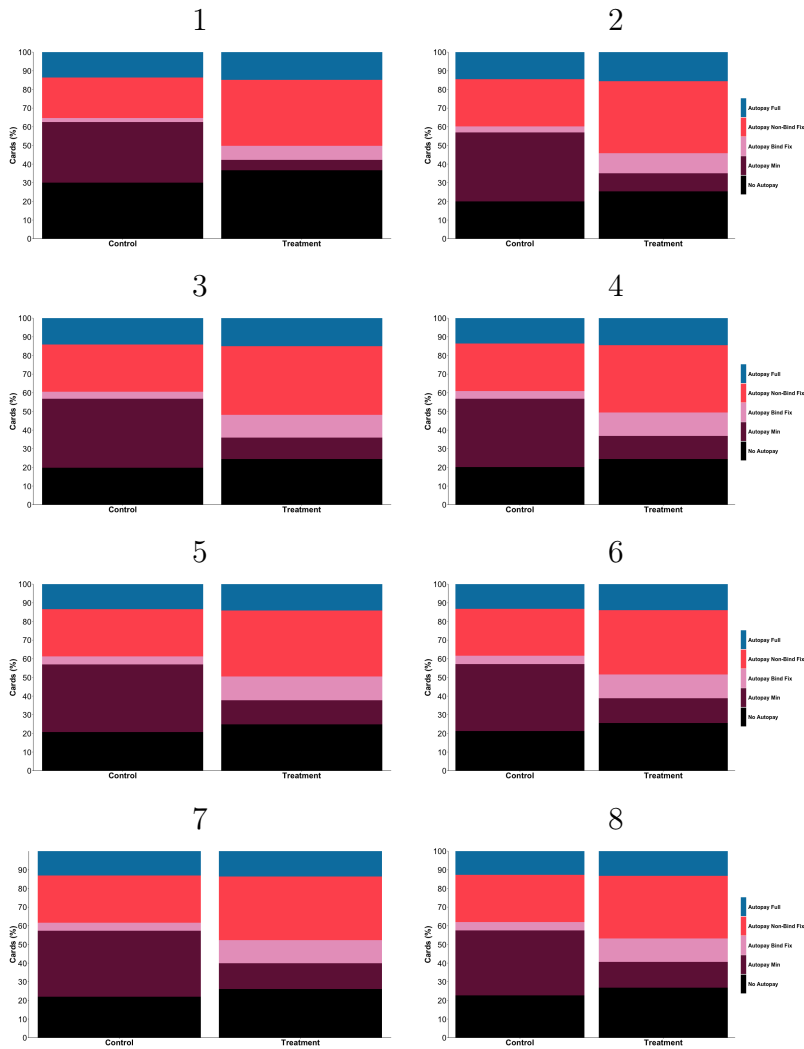
Figure 3.24: Average treatment effects on automatic, manual and total (automatic + manual) payments across 1-10 statement cycles
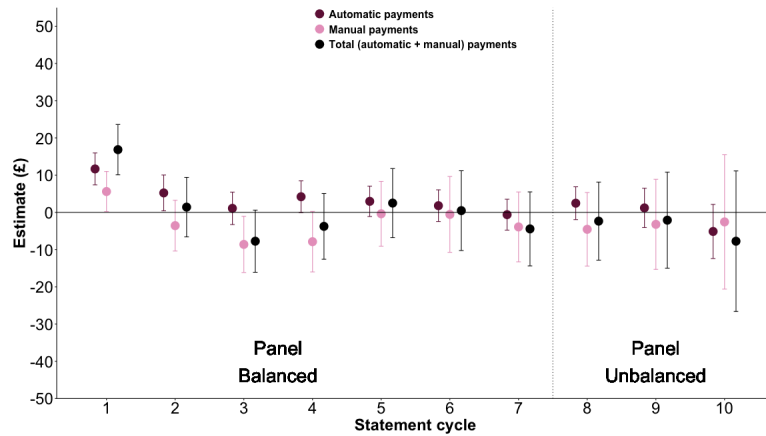
Notes: *Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 3.1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that estimates are uninformative. 40,708 credit cards.*

Figure 3.25: Heterogeneous treatment effects by quartiles of (A) credit score, (B) income and (C) unsecured debt-to-income (DTI) ratio, on credit card debt (statement balance net of payments, % statement balance) across 1-10 statement cycles
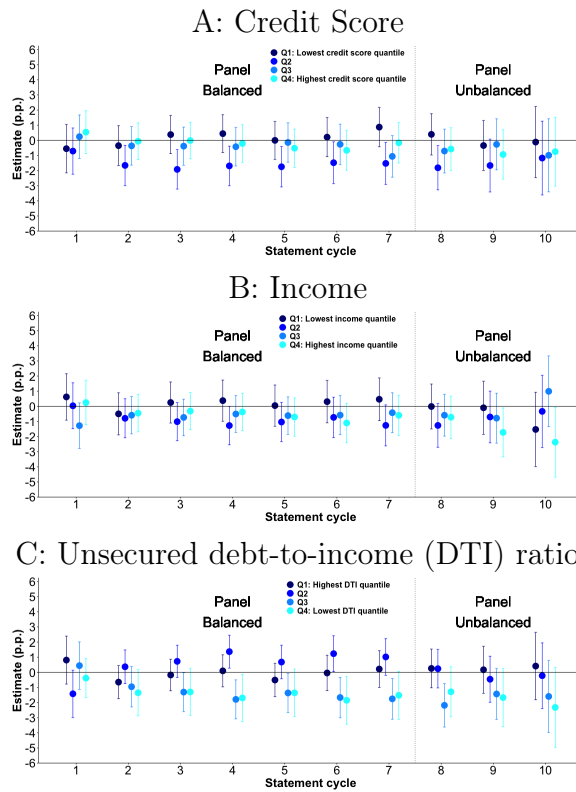
A: Credit Score

B: Income

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

Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 3.1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. Heterogeneous variables calculated from consumer credit reporting data in month preceding credit card opening (& trial start). Cycle 11 excluded from figure as, due to few cards being observed in this cycle, confidence intervals are extremely large such that estimates are uninformative. 40,708 credit cards with 368,162 observations.

Table 3.5: Summary statistics

Outcome	Mean	S.D.	P10	P25	P50	P75	P90
Age (years)	36.46	12.44	23	27	34	45	54
Female (% cards)	0.46	0.50	0	0	0	1	1
Credit limit (£)	4356.81	3366.08	660	1,400	3,800	6,300	9,000
Any credit score	0.99	0.12	1	1	1	1	1
Credit score (0-100)	0.65	0.07	0.560	0.610	0.660	0.700	0.740
Purchases rate (%)	22.85	6.11	18.900	18.900	18.900	29.900	34.900
Any balance transfer debt	0.43	0.50	0	0	0	1	1
Any estimated income	0.97	0.18	1	1	1	1	1
Estimated income (£)	2437.38	2155.22	899	1,321	1,880	2,816	4,336
Any autopay	0.78	0.41	0	1	1	1	1
Autopay full	0.13	0.34	0	0	0	0	1
Autopay fix	0.30	0.46	0	0	0	1	1
Autopay min	0.35	0.48	0	0	0	1	1
Statement balance (£)	2164.49	2416.30	0	373	1,290	3,274	5,437
Statement balance net of payments (£)	1962.52	2369.65	0	41	1,086	3,070	5,162
Statement balance net of payments (% statement balance)	0.69	0.41	0	0.180	0.950	0.980	0.980
Utilization	0.52	0.37	0	0.200	0.530	0.840	0.980
Any minimum payment	0.30	0.46	0	0	0	1	1
Any full payment	0.24	0.43	0	0	0	0	1
Any missed payment	0.04	0.19	0	0	0	0	0
Cumulative number times paid minimum	2.04	2.63	0	0	0	4	7
Cumulative number times paid in full	1.90	2.56	0	0	1	3	7
Cumulative number times paid less than minimum	0.19	0.76	0	0	0	0	0
6+ times paid minimum	0.19	0.39	0	0	0	0	1
6+ times paid in full	0.18	0.38	0	0	0	0	1
6+ times paid less than minimum	0.01	0.07	0	0	0	0	0
Number of credit cards	2.80	1.90	1	1	2	4	5
Number of credit cards with debt	1.52	1.36	0	1	1	2	3
Credit card portfolio statement balances (£)	3916.96	5142.72	90	626	2,284	5,143	9,734
Credit card portfolio balances net of payments (£)	3431.69	4849.58	0	255	1,851	4,597	8,830

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

Table 3.6: Minimum Detectable Effect (MDE) sizes for primary outcomes at cycle 7 across significance levels 0.005, 0.01, & 0.05 (all assuming 80% power)

	Significance Thresholds		
Outcome	0.005	0.01	0.05
Any minimum payment	0.0160	0.0150	0.0123
Any full payment	0.0155	0.0145	0.0119
Any missed payment	0.0070	0.0065	0.0053
Statement balance net of payments (% statement balance)	0.0149	0.0140	0.0114
Costs (% statement balance)	0.0023	0.0022	0.0018
Spending (% statement balance)	0.0127	0.0119	0.0098
Share of credit card portfolio only paying minimum	0.0108	0.0101	0.0083
Share of credit card portfolio making full payment	0.0136	0.0127	0.0104
Share of credit card portfolio missing payment	0.0048	0.0045	0.0037
Credit card portfolio balances net of payments (% statement balances)	0.0141	0.0132	0.0108

Table 3.7: Minimum Detectable Effect (MDE) sizes for secondary outcomes at cycle 7 across significance levels 0.005, 0.01, & 0.05 (all assuming 80% power)

	Significance Thresholds		
Outcome	0.005	0.01	0.05
Any autopay	0.0154	0.0145	0.0119
Autopay full	0.0123	0.0115	0.0095
Autopay fix	0.0176	0.0164	0.0135
Autopay min	0.0156	0.0146	0.0120
Statement balance net of payments (£)	86.2633	80.7966	66.2351
Credit card portfolio balances net of payments (£)	176.3149	165.1413	135.3790
Cumulative total payments (£)	63.2412	59.2334	48.5582
Cumulative automatic payments (£)	40.6805	38.1025	31.2355
Cumulative manual payments (£)	52.0277	48.7305	39.9481

Table 3.8: Balance comparison

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Age (years)	36.4641	36.6078	0.1437	[-0.0985, 0.3860]
Female (% cards)	0.4606	0.4612	0.0006	[-0.0091, 0.0103]
Any estimated income	0.9660	0.9630	-0.0030	[-0.0066, 0.0006]
Estimated income (£)	2437.3804	2457.5071	20.1267	[-21.9344, 62.1877]
Credit limit (£)	4356.8067	4429.0296	72.2228	[6.3640, 138.0817]
Any credit score	0.9856	0.9834	-0.0023	[-0.0047, 0.0001]
Credit score (0-100)	0.6526	0.6538	0.0012	[-0.0003, 0.0026]
Purchases rate (%)	22.8479	22.8168	-0.0311	[-0.1496, 0.0874]
Any balance transfer offered	0.2900	0.2976	0.0076	[-0.0013, 0.0164]
Number of credit cards	2.1757	2.1917	0.0160	[-0.0204, 0.0524]
Number of credit cards with debt	0.8998	0.9135	0.0136	[-0.0080, 0.0352]
Credit card portfolio statement balances (£)	2364.9238	2439.0881	74.1643	[-0.7909, 149.1194]
Credit card portfolio balances net of payments (£)	2001.3480	2072.5311	71.1832	[2.5927, 139.7736]

Notes: N (control) = 20,617 and N (treatment) = 20,091 cards.

Table 3.9: Unconditional mean comparison of treatment effects for Autopay enrollment after seven statement cycles

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

Notes: N (control) = 20,617 and N (treatment) = 20,091 cards.

Table 3.10: Unconditional mean comparison of treatment effects for primary outcomes after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Any minimum payment	0.3012	0.2323	-0.0689	[-0.0775, -0.0603]
Any full payment	0.2397	0.2417	0.0019	[-0.0064, 0.0102]
Any missed payment	0.0369	0.0403	0.0034	[-0.0003, 0.0071]
Statement balance net of payments (% statement balance)	0.6936	0.6910	-0.0026	[-0.0106, 0.0054]
Costs (% statement balance)	0.0111	0.0107	-0.0004	[-0.0016, 0.0009]
Spending (% statement balance)	0.2007	0.2013	0.0006	[-0.0062, 0.0075]
Share of credit card portfolio only paying minimum	0.2012	0.1775	-0.0237	[-0.0295, -0.0179]
Share of credit card portfolio making full payment	0.4414	0.4424	0.0011	[-0.0062, 0.0084]
Share of credit card portfolio missing payment	0.0236	0.0231	-0.0004	[-0.0030, 0.0021]
Credit card portfolio balances net of payments (% statement balances)	0.6954	0.6912	-0.0042	[-0.0118, 0.0034]

Notes: N (control) = 20,617 and N (treatment) = 20,091 cards.

Table 3.11: Unconditional mean comparison of treatment effects for secondary outcomes after seven statement cycles

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Cumulative number times paid in full	1.9020	1.9081	0.0061	[-0.0439, 0.0560]
Cumulative number times paid minimum	2.0444	1.4594	-0.5850	[-0.6329, -0.5372]
Cumulative number times paid less than minimum	0.1892	0.2153	0.0261	[0.0110, 0.0412]
Cumulative total payments (£)	1277.2667	1288.3119	11.0453	[-22.8990, 44.9895]
Cumulative automatic payments (£)	573.7899	605.2636	31.4737	[9.6362, 53.3112]
Cumulative manual payments (£)	711.9684	693.1835	-18.7850	[-46.7112, 9.1412]
Total payments (% statement balance)	0.2271	0.2305	0.0034	[-0.0040, 0.0107]
Automatic payments (% statement balance)	0.1101	0.1164	0.0062	[0.0007, 0.0118]
Manual payments (% statement balance)	0.1212	0.1189	-0.0023	[-0.0081, 0.0035]
Made both automatic and manual payment	0.0672	0.0797	0.0125	[0.0074, 0.0176]
Statement balance (£)	2164.4948	2203.7629	39.2681	[-7.9750, 86.5112]
Statement balance net of payments (£)	1962.5190	2005.4041	42.8851	[-3.4588, 89.2290]
Utilization	0.5223	0.5217	-0.0006	[-0.0076, 0.0065]
Cumulative spending (£)	3186.1868	3221.3178	35.1310	[-21.9622, 92.2242]
Cumulative costs (£)	76.0218	78.2825	2.2607	[0.3669, 4.1544]
Spending (£)	193.2399	187.9163	-5.3236	[-14.5432, 3.8961]
Total payments (£)	201.9758	198.3588	-3.6170	[-13.7509, 6.5168]
Automatic payments (£)	86.9490	87.0953	0.1462	[-4.1894, 4.4819]
Manual payments (£)	116.3833	112.5194	-3.8639	[-13.3054, 5.5776]
Credit card portfolio repayments (£)	485.7041	508.1641	22.4600	[0.8591, 44.0608]
Credit card portfolio repayments (% statement balances)	0.2564	0.2559	-0.0005	[-0.0076, 0.0066]
Credit card portfolio statement balances (£)	3916.9554	4018.9441	101.9887	[1.1026, 202.8748]
Credit card portfolio balances net of payments (£)	3431.6852	3510.7800	79.0948	[-15.6258, 173.8153]

Notes: N (control) = 20,617 and N (treatment) = 20,091 cards.

Table 3.12: Average treatment effects for secondary outcomes after seven statement cycles

Outcome	Estimate (s.e.)	95% C.I.	P value	Control Mean
Cumulative number times paid in full	0.0192 (0.0201)	[-0.0203, 0.0586]	0.3405	1.9020
Cumulative number times paid minimum	-0.5939 (0.0232)	[-0.6393, -0.5485]	0.0000	2.0444
Cumulative number times paid less than minimum	0.0276 (0.0075)	[0.0129, 0.0424]	0.0002	0.1892
Cumulative total payments (£)	6.6774 (16.1915)	[-25.0579, 38.4127]	0.6800	1277.27
Cumulative automatic payments (£)	27.3038 (10.3519)	[7.0141, 47.5935]	0.0084	573.79
Cumulative manual payments (£)	-18.8732 (13.9679)	[-46.2503, 8.5039]	0.1766	711.97
Total payments (% statement balance)	0.0060 (0.0032)	[-0.0002, 0.0123]	0.0579	0.2271
Automatic payments (% statement balance)	0.0072 (0.0025)	[0.0023, 0.0122]	0.0040	0.1101
Manual payments (% statement balance)	-0.0005 (0.0028)	[-0.0061, 0.0050]	0.8477	0.1212
Made both automatic and manual payment	0.0131 (0.0026)	[0.0080, 0.0182]	0.0000	0.0672
Statement balance (£)	-0.3284 (17.2370)	[-34.1128, 33.4561]	0.9848	2164.49
Statement balance net of payments (£)	4.1070 (17.2164)	[-29.6371, 37.8510]	0.8115	1962.52
Utilization	0.0002 (0.0032)	[-0.0061, 0.0064]	0.9604	0.5223
Cumulative spending (£)	-7.2306 (20.9479)	[-48.2885, 33.8273]	0.7300	3186.19
Cumulative costs (£)	1.3903 (0.8260)	[-0.2288, 3.0093]	0.0924	76.02
Spending (£)	-9.8371 (4.9850)	[-19.6078, -0.0664]	0.0485	193.24
Total payments (£)	-4.4353 (5.0735)	[-14.3795, 5.5088]	0.3820	201.98
Automatic payments (£)	-0.6123 (2.1195)	[-4.7665, 3.5419]	0.7727	86.95
Manual payments (£)	-3.9023 (4.7893)	[-13.2893, 5.4847]	0.4152	116.38
Credit card portfolio repayments (£)	9.1092 (9.3858)	[-9.2870, 27.5053]	0.3318	485.70
Credit card portfolio repayments (% statement balances)	0.0017 (0.0030)	[-0.0042, 0.0076]	0.5730	0.26
Credit card portfolio statement balances (£)	23.6451 (31.1548)	[-37.4183, 84.7085]	0.4479	3916.96
Credit card portfolio balances net of payments (£)	12.0581 (30.9206)	[-48.5463, 72.6626]	0.6966	3431.69

Table 3.13: Average treatment effects for primary outcomes pooled across all statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any minimum payment	-0.0807 (0.0033)	[-0.0871, -0.0742]	0.0000	0.2943
Any full payment	0.0041 (0.0028)	[-0.0015, 0.0096]	0.1489	0.2658
Any missed payment	0.0040 (0.0011)	[0.0019, 0.0062]	0.0002	0.0297
Statement balance net of payments (% statement balance)	-0.0056 (0.0027)	[-0.0109, -0.0003]	0.0380	0.6692
Costs (% statement balance)	-0.0001 (0.0002)	[-0.0006, 0.0003]	0.5166	0.0109
Spending (% statement balance)	0.0012 (0.0020)	[-0.0027, 0.0052]	0.5430	0.2918
Share of credit card portfolio only paying minimum	-0.0266 (0.0017)	[-0.0298, -0.0233]	0.0000	0.1631
Share of credit card portfolio making full payment	0.0002 (0.0023)	[-0.0043, 0.0048]	0.9190	0.5150
Share of credit card portfolio missing payment	0.0004 (0.0007)	[-0.0009, 0.0017]	0.5400	0.0144
Credit card portfolio balances net of payments (% statement balances)	-0.0036 (0.0022)	[-0.0079, 0.0006]	0.0967	0.6245

Notes: Table shows average treatment effects pooled across statement cycles. Estimates are δ coefficients from OLS regressions as specified by Equation 3.2 includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Table 3.14: Average treatment effects for secondary outcomes of balances and repayments amounts pooled across all statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Statement balance	3.5857	[-25.6954,	0.8103	2049.8420
(£)	(14.9393)	32.8667]		
Statement balance net of payments	3.9778	[-25.2594,	0.7897	1862.3909
(£)	(14.9169)	33.2150]		
Spending	-2.4569	[-7.6570,	0.3544	395.5314
(£)	(2.6531)	2.7432]		
Total payments	-0.3921	[-4.7841,	0.8611	187.4512
(£)	(2.2408)	3.9999]		
Automatic payments	3.0544	[0.1874,	0.0368	82.6856
(£)	(1.4627)	5.9214]		
Manual payments	-3.2090	[-6.9899,	0.0962	105.9518
(£)	(1.9290)	0.5718]		
Total payments	0.0067	[0.0019,	0.0061	0.2346
(% statement balance)	(0.0025)	0.0115]		
Automatic payments	0.0099	[0.0058,	0.0000	0.1121
(% statement balance)	(0.0021)	0.0140]		
Manual payments	-0.0021	[-0.0060,	0.2922	0.1268
(% statement balance)	(0.0020)	0.0018]		
Credit card portfolio	30.5985	[-13.0648,	0.1696	3506.8973
statement balances (£)	(22.2772)	74.2618]		
Credit card portfolio balances	24.9894	[-18.1908,	0.2567	2961.2714
net of payments (£)	(22.0307)	68.1696]		
Credit card portfolio repayments	4.0665	[-4.4159,	0.3474	545.7112
(£)	(4.3278)	12.5489]		

Notes: Table shows average treatment effects pooled across statement cycles. Estimates are δ coefficients from OLS regressions as specified by Equation 3.2 includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations.

Table 3.15: Average treatment effects for tertiary arrears outcomes pooled across all statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any missed payment	0.0040 (0.0011)	[0.0019, 0.0062]	0.0002	0.0297
Arrears 1+ payments behind	0.0031 (0.0010)	[0.0011, 0.0051]	0.0024	0.0267
Arrears 2+ payments behind	0.0004 (0.0007)	[-0.0009, 0.0018]	0.5476	0.0110
Arrears 3+ payments behind	0.0002 (0.0005)	[-0.0009, 0.0012]	0.7677	0.0071
Share of credit card portfolio missing payment	0.0004 (0.0007)	[-0.0009, 0.0017]	0.5400	0.0144

Notes: Table shows average treatment effects pooled across statement cycles. Estimates are δ coefficients from OLS regressions as specified by Equation 3.2 includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 40,708 credit cards with 368,162 observations. The first row is our 3rd primary outcome: defined as paying zero or less than the minimum due (on the "target" card in the experiment). The last row is our 9th primary outcome: defined as the proportion of credit cards paying zero or less than the minimum due (constructed from consumer credit reporting data containing the portfolio of credit card held). All other rows show effects for non-primary outcomes for the card in the experiment: standard industry point-in-time measures for the number of payments in arrears was when payments became due.

Table 3.16: Coefficients from OLS regressions predicting correlates of making both an automatic and manual payment in cycle 7 (columns 1-2) or across cycles 1-7 (columns 3-4) among subsample of cardholders enrolled in autopay min or fix at cycle 7, split by control (columns 1 and 3) and treatment (columns 2 and 4)

	(1)	(2)	(3)	(4)
Intercept	0.1984	0.3664	0.6083	0.8283
	(0.0552)	(0.0669)	(0.0898)	(0.0982)
Female	0.0074	0.0116	0.0043	0.0194
	(0.0051)	(0.0059)	(0.0081)	(0.0087)
Age	−0.0009	−0.0019	−0.0035	−0.0043
	(0.0002)	(0.0003)	(0.0004)	(0.0004)
Any Income Estimate	−0.0127	0.0030	0.0498	0.0403
	(0.0190)	(0.0220)	(0.0282)	(0.0298)
Income Estimate	0.0018	0.0015	−0.0001	0.0033
(000s)	(0.0012)	(0.0014)	(0.0021)	(0.0022)
Log (Credit Limit)	−0.0081	−0.0171	−0.0117	−0.0300
	(0.0054)	(0.0064)	(0.0089)	(0.0096)
Subprime	−0.0207	0.0056	0.0080	−0.0238
	(0.0131)	(0.0157)	(0.0200)	(0.0220)
Purchases Rate	0.0018	−0.0002	0.0013	0.0036
	(0.0008)	(0.0010)	(0.0012)	(0.0014)
Any Balance Transfer	−0.0063	−0.0257	0.0058	−0.0280
	(0.0056)	(0.0062)	(0.0092)	(0.0098)
Credit Score	−0.0156	0.0063	−0.1174	−0.1255
	(0.0314)	(0.0348)	(0.0489)	(0.0518)
Any Mortgage Debt	−0.0132	−0.0217	−0.0254	−0.0346
	(0.0056)	(0.0064)	(0.0093)	(0.0099)
Credit Card Portfolio Statement	−0.0014	0.0016	0.0039	−0.0074
Balances (000s)	(0.0018)	(0.0023)	(0.0039)	(0.0038)
Credit Card Portfolio Statement	0.0000	−0.0045	−0.0119	−0.0019
Balances Net of Payments (000s)	(0.0020)	(0.0026)	(0.0042)	(0.0042)
Number Credit Cards Portfolio	−0.0020	−0.0040	−0.0050	−0.0053
	(0.0020)	(0.0021)	(0.0033)	(0.0035)
Number Credit Cards Portfolio	−0.0057	−0.0054	−0.0069	−0.0091
With Debt	(0.0034)	(0.0041)	(0.0057)	(0.0063)
Non-Mortgage Debt Value (000s)	−0.0004	−0.0005	−0.0008	−0.0004
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
R^2	0.0119	0.0299	0.0329	0.0593

Notes: Table shows coefficients from OLS regression on binary outcomes. Outcome for columns 1-2 is making both a manual and automatic payment in cycle 7. Outcome for columns 3-4 is making both a manual and automatic payment in any cycle 1-7. Predictors are calculated at card opening or from credit file data in the month preceding card opening. One observation per card using data only for cards enrolled in autopay fix or min at cycle 7. Columns (1) and (3) for control group, columns (2) and (4) for treatment group subsamples. These are run separately for control and treatment groups given different autopay enrollment.

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Table 3.17: Heterogeneous treatment effects on credit card debt (statement balance net of payments % statement balance) by quartiles of pre-trial (A) credit score, (B) income and (C) unsecured debt-to-income (DTI) ratio after seven statement cycles

	Q1: Most Vulnerable	Q2	Q3	Q4: Least Vulnerable
A. Credit Score				
Estimate, p.p.	0.0087	-0.0153	-0.0107	-0.0016
(s.e.)	(0.0066)	(0.0071)	(0.0070)	(0.0068)
95% C.I.	[-0.0043, 0.0217]	[-0.0291, -0.0014]	[-0.0244, 0.0031]	[-0.0150, 0.0117]
P value	0.1900	0.0306	0.1278	0.8097
Control mean	0.7592	0.7226	0.6686	0.6220
B. Income				
Estimate, p.p.	0.0046	-0.0126	-0.0042	-0.0060
(s.e.)	(0.0072)	(0.0069)	(0.0067)	(0.0067)
95% C.I.	[-0.0095, 0.0188]	[-0.0262, 0.0009]	[-0.0174, 0.0089]	[-0.0192, 0.0073]
P value	0.5202	0.0681	0.5286	0.3778
Control mean	0.6793	0.7144	0.7107	0.6694
C. Unsecured Debt-to-Income (DTI)				
Estimate, p.p.	0.0022	0.0102	-0.0176	-0.0152
(s.e.)	(0.0062)	(0.0062)	(0.0069)	(0.0081)
95% C.I.	[-0.0100, 0.0143]	[-0.0019, 0.0222]	[-0.0310, -0.0041]	[-0.0311, 0.0006]
P value	0.7275	0.0993	0.0106	0.0598
Control mean	0.8142	0.8044	0.7514	0.4027

Notes: Estimates are δ_7 coefficients from OLS regressions as specified by Equation 3.2 that includes month and statement cycle fixed effects along with pre-trial controls. Each estimate is from a separate regression for subsamples by quartiles of each heterogeneous variable: credit score, estimated monthly income and unsecured debt-to-income (DTI) ratio. Heterogeneous variables are calculated from consumer credit reporting data in month preceding credit card opening. Q1 (Q4) denotes the most (least) vulnerable quartiles with the lowest (highest) credit score, lowest (highest) income or highest (lowest) unsecured DTI ratio. Standard errors are clustered at consumer-level with N = 40,708 credit cards in total.

3.9.4 Additional Results for Second Lender

Figure 3.26: Second Lender - Average treatment effects on making only a minimum payment across 1-12 statement cycles
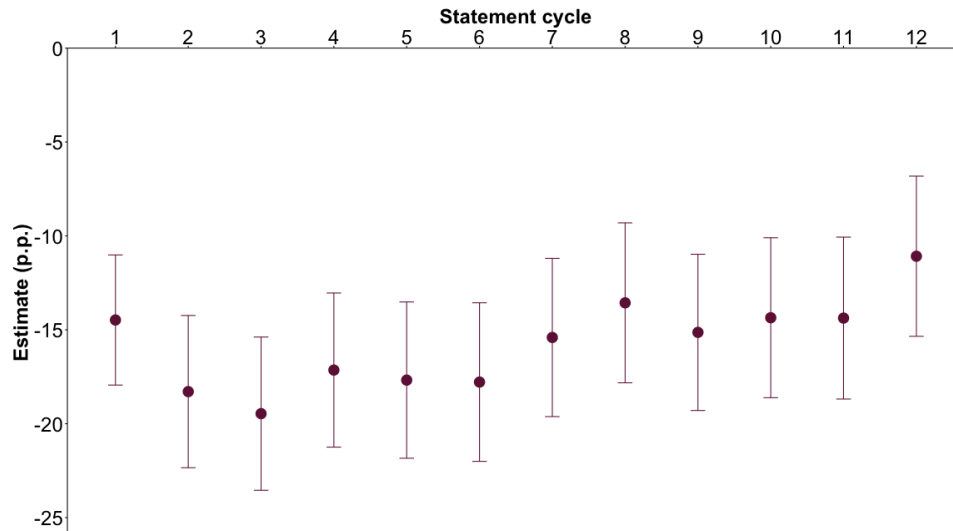
Notes: Treatment effects from coefficients (δ_τ) in OLS regression specified in Equation 3.1 (standard errors clustered at consumer-level). Error bars are 95% confidence intervals. 1,531 credit cards with 19,578 observations.

Figure 3.27: Second Lender - Average treatment effects on credit card debt across 1-12 statement cycles
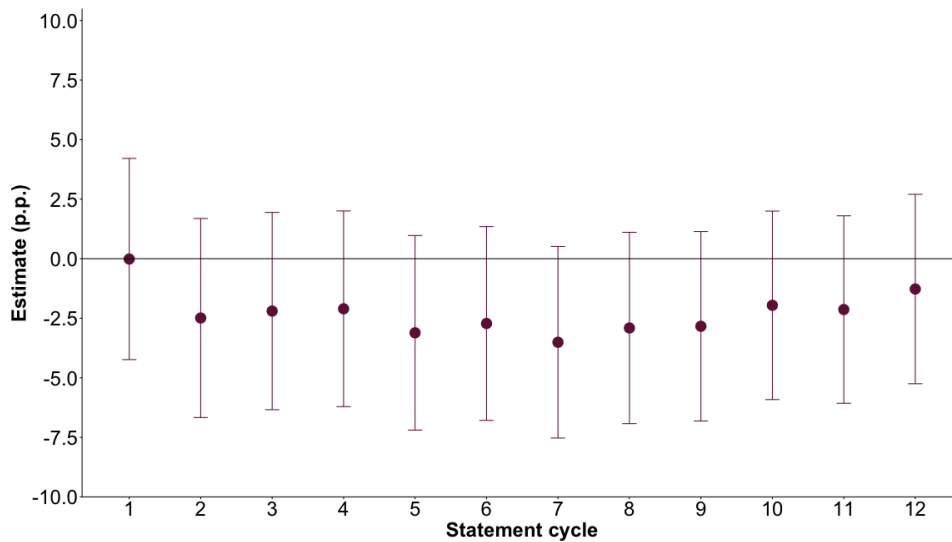
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Table 3.18: Second Lender: Balance comparison

Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Age (years)	37.0547	36.4839	-0.5708	[-1.7761, 0.6345]
Female (% cards)	0.4774	0.5264	0.0490	[-0.0016, 0.0995]
Any estimated income	0.9248	0.9395	0.0148	[-0.0107, 0.0402]
Estimated income (£)	2073.0199	1890.8578	-182.1621	[-349.5416, -14.7825]
Credit limit (£)	608.9603	587.3874	-21.5729	[-82.0721, 38.9263]
Any credit score	0.9863	0.9897	0.0034	[-0.0076, 0.0144]
Credit score (0-100)	0.5369	0.5406	0.0036	[-0.0057, 0.0129]
Purchases rate (%)	22.9667	23.4588	0.4920	[-0.6872, 1.6713]
Any balance transfer offered	0.1724	0.1699	-0.0025	[-0.0406, 0.0356]
Number of credit cards	2.0356	1.9974	-0.0381	[-0.1850, 0.1087]
Number of credit cards with debt	0.6389	0.6319	-0.0069	[-0.1036, 0.0897]
Credit card portfolio statement balances (£)	934.2079	872.6435	-61.5644	[-269.9267, 146.7978]
Credit card portfolio balances net of payments (£)	855.7415	803.0631	-52.6784	[-249.6079, 144.2511]

Notes: N (control) = 740 and N (treatment) = 791 cards.

Table 3.19: Second Lender: Unconditional mean comparison of treatment effects for Autopay enrollment after seven statement cycles

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

Notes: N (control) = 740 and N (treatment) = 791 cards.

Table 3.20: Second Lender: Unconditional mean comparison of treatment effects for primary outcomes after seven statement cycles

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Outcome	Control	Treatment	Difference (p.p.)	95% C.I.
Any minimum payment	0.3160	0.1622	-0.1538	[-0.1964, -0.1113]
Any full payment	0.2503	0.2690	0.0186	[-0.0257, 0.0630]
Any missed payment	0.1176	0.1287	0.0111	[-0.0222, 0.0443]
Statement balance net of payments (% statement balance)	0.6753	0.6440	-0.0313	[-0.0732, 0.0105]
Costs (% statement balance)	0.0391	0.0294	-0.0096	[-0.0180, -0.0013]
Spending (% statement balance)	0.2245	0.2330	0.0084	[-0.0287, 0.0456]
Share of credit card portfolio only paying minimum	0.2016	0.1245	-0.0771	[-0.1051, -0.0492]
Share of credit card portfolio making full payment	0.3455	0.3556	0.0101	[-0.0287, 0.0489]
Share of credit card portfolio missing payment	0.0904	0.1021	0.0117	[-0.0132, 0.0366]
Credit card portfolio balances net of payments (% statement balances)	0.7281	0.6997	-0.0284	[-0.0667, 0.0099]

Notes: N (control) = 740 and N (treatment) = 791 cards.

Table 3.21: Second Lender: Average treatment effects for Autopay enrollment outcomes after seven statement cycles

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

Notes: Table shows average treatment effects from after seven statement cycles. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 3.1 that includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 1,531 credit cards with 19,578 observations.

Table 3.22: Second Lender: Average treatment effects for primary outcomes after seven statement cycles

Outcome	Estimate, p.p. (s.e.)	95% C.I.	P value	Control mean
Any minimum payment	-0.1541 (0.0215)	[-0.1962, -0.1119]	0.0000	0.3160
Any full payment	0.0223 (0.0219)	[-0.0207, 0.0653]	0.3092	0.2503
Any missed payment	0.0089 (0.0170)	[-0.0244, 0.0421]	0.6011	0.1176
Statement balance net of payments (% statement balance)	-0.0351 (0.0205)	[-0.0753, 0.0051]	0.0874	0.6753
Costs (% statement balance)	-0.0089 (0.0040)	[-0.0168, -0.0010]	0.0276	0.0391
Spending (% statement balance)	0.0122 (0.0185)	[-0.0241, 0.0485]	0.5113	0.2245
Share of credit card portfolio only paying minimum	-0.0814 (0.0136)	[-0.1080, -0.0549]	0.0000	0.2016
Share of credit card portfolio making full payment	0.0089 (0.0187)	[-0.0278, 0.0456]	0.6342	0.3455
Share of credit card portfolio missing payment	0.0120 (0.0124)	[-0.0123, 0.0363]	0.3315	0.0904
Credit card portfolio balances net of payments (% statement balances)	-0.0274 (0.0180)	[-0.0627, 0.0078]	0.1276	0.7281

Notes: Table shows average treatment effects from after seven statement cycles. Estimates are δ_7 coefficients from OLS regressions as specified by Equation 3.1 that includes month and statement cycle fixed effects along with pre-trial controls. Standard errors are clustered at consumer-level. 1,531 credit cards with 19,578 observations.

3.9.5   Liquid Cash Balances

Bank Account Data Sample Restrictions

We keep bank account data on cardholders who appear to be actively using this bank as their primary bank account for a sustained period of time meeting the following criteria: where we observe a solely-held checking account for six months to June 2017, first observed the account at least 180 days before card opening, and where the 3 month moving average of account credits average at least £250 and account debits at least £100 per month during this time. This approach is similar to that used in other research such as that using the JP Morgan Chase Institute data. For these cardholders we include their liquid cash savings from any other checking accounts held as well as non-checking cash savings accounts with instant access.

The choice of threshold used produces similar sample sizes: requiring average account credits and debits are both £500 results in 3,552 cardholders compared to a threshold of £100 that results in 3,831 cardholders. These cardholders are more likely to be younger, with higher incomes and credit scores, fewer credit cards and lower credit card debts as shown in Appendix Table 3.23.

Measuring Liquid Cash Balances

Having documented the proximate and distal effects of the policy (along with the lack of clear heterogeneous effects) and investigated the mechanisms explaining our null result, we wanted to understand *why* consumers were not paying more on their credit card. The most natural potential explanation is that many households have limited liquid cash balances available, which prevents or disincentivizes them from paying down credit card debt.

We explore this by constructing new measures of liquidity from our linked bank account data. Unfortunately, we only observe these linked data for a selected subset of cardholders who also bank with their credit card provider. Based on observed socio-economic characteristics (e.g., income, credit score), we would expect this sample to be less liquidity constrained than those for whom we do not observe linked data (Appendix Table 3.23).

In addition to being a selected subsample, we do not have sufficient power to estimate treatment effects for this group. If we had sufficient power we would evaluate the nudge's heterogeneous effects by liquidity. We present descriptive analysis that we consider informative for updating a Bayesian reader's priors. Despite such limitations, these data represent an advance on research on credit card payments decisions where liquid savings data is unobserved (e.g., Keys and Wang, 2019; Medina and Negrin, 2022).

We construct three measures of liquid cash balances. Our first measure is a static one. It measures "liquid cash" as the end-of-day balance in bank accounts by aggregating all liquid cash held across checking and non-checking, instantly-accessible cash savings accounts. In the UK, it is common for checking accounts to have an overdraft line of credit facility, so liquid cash measures can have negative balances. Our first measure simply takes liquid cash balances at the day before card opening (-1) but we also show it at earlier points-in-time before card opening (-31, -61, -91, -121, -151).

Our other two measures are innovative as they consider the dynamics of liquidity. These measures go beyond measures used in prior literature using transaction data. Prior literature does not examine heterogeneity by the *minimum* balance reached but instead focus on different moments: the mean or median balance:

- Agarwal and Qian (2014) segments by the mean value of checking account balance.

- Gelman et al. (2014) segments by the mean value of checking and savings accounts balances (normalized by the daily average spending of each consumer).

- Olafsson and Pagel (2018) segments by the mean and median values of cash and available liquidity (normalized by the daily average spending of each consumer to provide measures of "consumption days").

- Baker (2018) segments by the mean of liquid assets / income, illiquid assets / income, total assets / income, debt / (debt + assets), and debt / income.

Our second measure examines a consumer's minimum liquid balances over the last 90 days before card opening (along with other time horizons). This accounts for how con-

sumers' finances vary over time; one point-in-time does not reflect how liquidity varies at different points-in-time for different consumers depending on the timing of their incomes and expenditures.

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

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

Summarizing Liquid Cash Balances

We show the distribution of these three measures of liquidity in the left hand side panels of Figure 3.11 (Summarized in Appendix Table 3.24). The blue lines show the robustness of these measures across alternative time horizons. Our first static measure (Panel A) shows a clear kink with liquid cash balance above zero being much more likely than those below. This kink may reflect there being a discontinuous increase in costs from becoming overdrawn on checking accounts and precautionary rationale to keep a small amount of buffer stock savings. By this measure approximately 10% experience have limited liquidity of having a zero or negative liquid cash balances available. We also observe this distribution has very fat tails (and so the mean is not well-estimated) but is stable over time with a median balance near £400.

Our second dynamic measure (Panel B) reveals clear sorting of consumers into two types (Distribution summarized in Appendix Table 3.24). One group of consumers has a zero or negative minimum liquid cash balance. There is a lot of bunching with another group of consumers just managing to keep positive, but small, liquid cash balances. A longer time

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window for calculating minimum liquid balances results in a slight steepening of the CDF around zero. Using a 90 day window the median minimum balance is effectively zero (£4.76) and the 75$^{\text{th}}$ percentile £142.39. This second measure reveals effectively zero cash balances for approximately 50% of consumers: far higher than the 10% a point-in-time liquid balance measure (Panel A) would indicate.

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

Relationship Between Cash Balances and Credit Card Payments

We show in the right hand side panels of Figure 3.11, the relationship between these variables and credit card payment decisions using our primary measure of credit card debt (statement balance net of payments as a fraction of statement balance). Panels D and E use binscatters by quantiles of the distribution, whereas Panel F uses loess (non-parametric smoothing) given the integer scale and high mass at both tails.

Panel D shows consumers who had small, positive liquid balances (before card opening) repaid more of their credit card debt, on average, seven cycles later than those with zero or small negative liquid balances. However, this relationship is quite noisy given how fat the distribution of liquid balances are.

Panel E shows a clearer relationship when we use our measure of minimum liquid cash balances over 90 days. Consumers with positive minimum liquid balances (before card opening) discontinuously repaid approximately 20 pp more, on average, of their credit card debt seven cycles later than those with zero or small negative liquid balances. Given the bimodal distribution to payments we also examine the other moments: payments at the minimum, full, and less than minimum. The discontinuity in average payments is driven by discontinuous increases in the likelihood of paying in full and decreases in the likelihood of missing a

payment (Appendix Figure 3.29). The relationship with Autopay choices is less clear except for a discontinuous increase in Autopay Full enrollment (Appendix Figure 3.28). Paying only the minimum becomes less likely among less liquidity constrained consumers, however, there is a less clear discontinuity around zero. Panel F also shows a clear relationship: consumers who have more days with low liquid cash balances (pre-card opening) pay less credit card debt seven cycles later.

Table 3.23: Coefficients from OLS regression predicting correlates of observing linked liquid savings data

	(1)
(Intercept)	0.0685
	(0.0237)
Female	0.0035
	(0.0028)
Age	−0.0007
	(0.0001)
Any Income Estimate	−0.0155
	(0.0088)
Income Estimate (000s)	0.0034
	(0.0007)
Log (Credit Limit)	0.0025
	(0.0026)
Subprime	−0.0470
	(0.0070)
Purchases Rate	0.0031
	(0.0003)
Any Balance Transfer	−0.0598
	(0.0026)
Credit Score	0.0705
	(0.0152)
Any Mortgage Debt	−0.0265
	(0.0029)
Credit Card Portfolio Statement Balances (000s)	−0.0025
	(0.0011)
Credit Card Portfolio Statement Balances Net of Payments (000s)	0.0044
	(0.0012)
Number Credit Cards Portfolio	−0.0152
	(0.0010)
Number Credit Cards Portfolio With Debt	−0.0112
	(0.0017)
Non-Mortgage Debt Value (000s)	−0.0011
	(0.0002)
R^2	0.0453

Notes: Table shows coefficients from OLS regression where binary outcome is whether observe linked liquid savings data. Predictors are calculated at card opening or from credit file data in the month preceding card opening. One observation per card.

Table 3.24: Summary statistics on liquid cash balances by date preceeding credit card opening

Date	Mean	S.D.	P10	P25	P50	P75	P90
-1	2109.85	12324.35	-84.58	48.07	368.65	1,310.91	4,054.58
-31	2142.00	14616.85	-95.17	56.37	364.06	1,297.43	3,757.13
-61	2048.65	9222.26	-61.84	66.93	432.80	1,394.05	4,094.95
-91	2342.60	22005.76	-38.10	66.26	433.57	1,397.41	3,986.56
-121	2164.82	14861.37	-59.16	55.72	396.25	1,401.18	3,949.21
-151	1800.46	7761.59	-75.71	57.62	386.68	1,342.17	3,508.93

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

Table 3.25: Summary statistics on minimum liquid cash balances over windows preceeding credit card opening

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

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

Figure 3.28: Non-parametric relationship between minimum liquid cash balance during 90 days before card opening with credit card Autopay enrollment at statement cycle 7, by treatment group
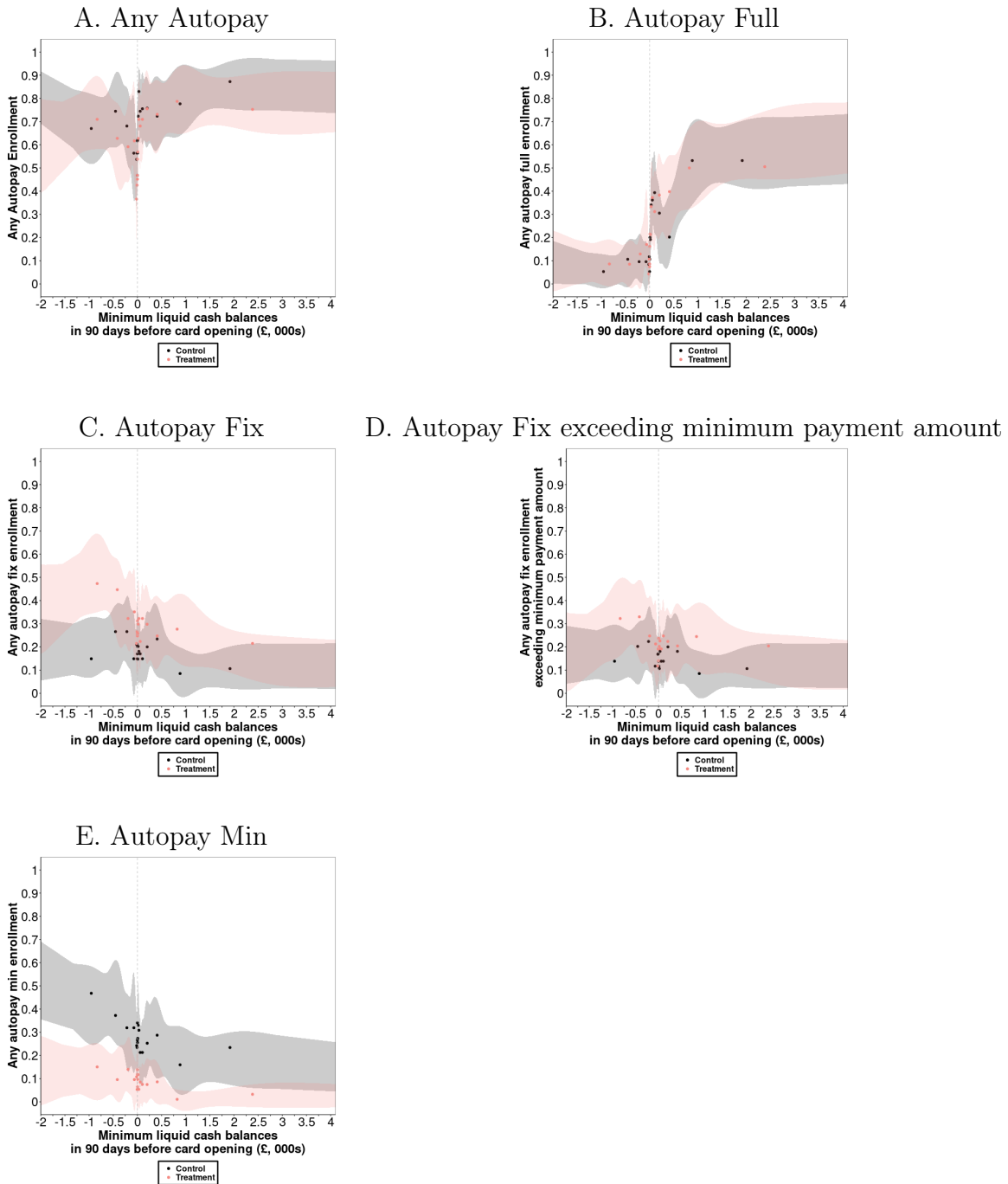
A. Any Autopay

B. Autopay Full

C. Autopay Fix

D. Autopay Fix exceeding minimum payment amount

E. Autopay Min

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

Figure 3.29: Non-parametric relationship between minimum liquid cash balance during 90 days before card opening with credit card payments at statement cycle 7, by treatment group
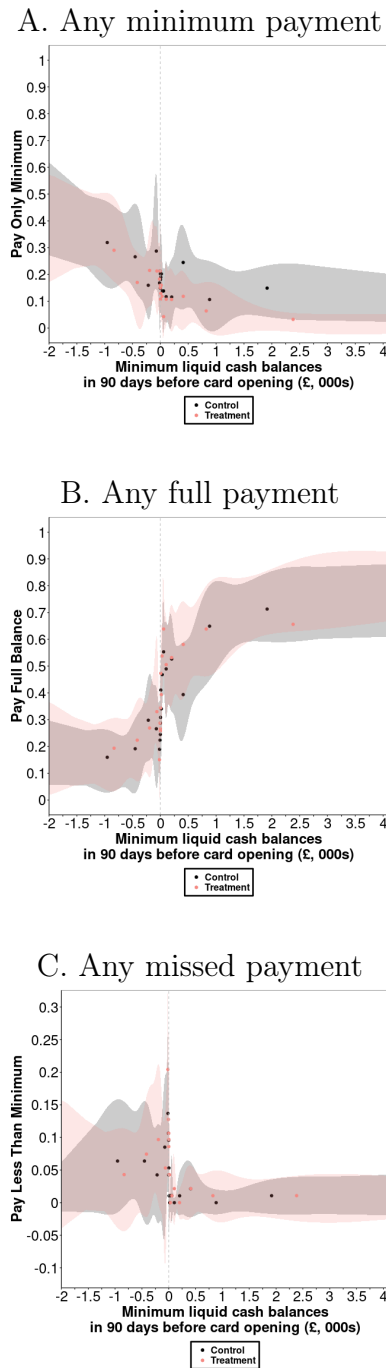
A. Any minimum payment

B. Any full payment

C. Any missed payment

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

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