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TO PAY OR AUTOPAY? FINTECH INNOVATION AND CREDIT CARD PAYMENTS

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ABSTRACT

Digital technologies and fintech firms have rapidly reshaped the consumer financial landscape in recent years, and have the potential to help consumers make better decisions and improve their financial health. Existing technologies such as autopay are also experiencing increased takeup, a trend that could be accelerated by innovations such as open banking. I examine the extent to which autopay affects payment behavior for customers of a credit card serviced by a fintech company. Using sharp changes in the company's practices in a regression discontinuity design, I find that a small nudge accounts for half of all autopay enrollment during the sample period, and that enrollment at account opening is persistent. Autopay increases the likelihood of making the minimum payment by 20 to 29pp, more than doubling the baseline rate. The results show that seemingly minor technological defaults can have economically large effects on consumer credit outcomes.

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1 Introduction

As shown by Keys and Wang (2019), credit card payments in the United States exhibit a highly bimodal distribution around the minimum and full payment amounts, with 35% of all payments clustered near the minimum and 33% at the full balance. One candidate explanation for this bimodal pattern is autopay, which allows consumers to automatically deduct credit card bill payments from their bank accounts but can generally only be set for the minimum or full balance amounts.[1]

Autopay technology is prevalent not only in the credit card market but for many other forms of consumer credit and non-financial bill payments, yet has received relatively little attention in the literature. With the rise of fintech firms and the increasing use of open banking data in consumer lending, understanding the interactions between credit and deposit accounts is becoming more important. Studying the effects and adoption patterns of existing technologies like autopay also helps us better predict how emerging financial technologies could affect market outcomes.

To my knowledge, this is the first plausibly causal study of the effects of autopay enrollment on credit card payments. I examine the extent to which the autopay features of a credit card issued by a bank and serviced by a fintech credit card company affect consumer payment behavior. Using two sharp changes in the card issuer's underwriting practices, I find that autopay enrollment dramatically increases the probability of making the minimum payment, a result that is graphically visible and robust across a range of specifications. The results suggest that autopay may play an important role in the prevalence of minimum payments in the credit card market, and that seemingly minor technological defaults can have economically meaningful effects on payment behavior.

I use data from about 63,000 credit card accounts between 2018 and 2020 that are underwritten using a combination of cashflow metrics based on transactions from users' bank accounts and traditional credit metrics. Cashflow-based underwriting requires consumers to link their bank accounts to the fintech app, reducing the frictions associated with autopay enrollment. Customers

[1] Autopay is available for all major U.S. credit cards during this study period. According to the Consumer Financial Protection Bureau (2021), the rate of autopay enrollment increased steadily from 16% to 20% between 2018 and 2020.

undergoing cashflow underwriting are asked whether or not they would like to enroll in autopay after linking their bank accounts as part of their initial application, and can opt-in immediately with a few more clicks.

Two major changes were made to the underwriting process during the sample period. The first underwriting change significantly decreased the requirement to link bank accounts starting with accounts originated at a specific calendar date. Consistent with the role of small frictions in generating large effects on autopay enrollment, this change led to a 19pp discontinuous decrease in autopay enrollment. The second underwriting change significantly increased the requirement to link bank accounts, and increased autopay enrollment by 13pp. Although users can unlink their bank accounts after underwriting and can change their autopay settings at any time, I find that initial autopay settings are highly persistent for at least 10 months after account origination. These first stage results are consistent with a large body of work showing that default effects in household savings are often persistent.[2] Nonetheless, the literature on default effects in consumer debt remains relatively slim, and their effects on ultimate consumer outcomes are an open question. This paper helps to bridge that gap.

I examine the causal effects of autopay on a variety of payment outcomes using parametric and non-parametric regression discontinuity designs around these two underwriting changes to instrument for autopay enrollment. The effects on minimum payments are the strongest and most robust result, and are very large in economic magnitude. Moving from 0% to 100% autopay enrollment increases minimum payments by 20 to 29pp across a range of instrumental variables (IV) specifications and across both underwriting changes, more than doubling the baseline rates.

An intriguing finding is that autopay *can* have significant effects on serious consequences such as chargeoffs. Moving from 0% to 100% autopay enrollment reduces chargeoffs by 13 to 19pp based on the first underwriting change, but does not have graphically obvious or robust effects across specifications for the second underwriting change. The mechanism by which autopay could

[2]See, for example and Madrian and Shea (2001), Choi, Laibson, Madrian and Metrick (2004), Beshears, Choi, Laibson and Madrian (2009), Choi, Laibson, Madrian and Metrick (2006), Chetty, Friedman, Leth-Petersen, Nielsen and Olsen (2014) and Choukhmane (2019).

reduce chargeoffs is simple, and is consistent with evidence on short-run delinquencies from the U.K. (Gathergood, Sakaguchi, Stewart and Weber, 2021; Sakaguchi, Stewart, Gathergood, Adams, Guttman-Kenney, Hayes and Hunt, 2022). By auto-deducting payments from a consumer's bank account before the due date, autopay reduces the likelihood of late payments, including ones that may eventually evolve into serious delinquency and chargeoff. Nonetheless, the seriousness of a chargeoff for a consumer's credit record is significant enough that it may be surprising for autopay to have a noticeable effect. This result has not been documented previously, and even if it may not generalize to all settings in and out of sample, it is a proof of concept that small technological defaults *can* have serious impacts on credit risk for both firms and consumers, an important possibility that can be explored further in future work.

Even if autopay increases minimum payments as described above, it may still have negligible effects on long-run outcomes if consumers offset more-frequent minimum payments with occasional larger payments. While the effects of autopay on minimum payments persist steadily for at least 10 months after account opening, the effects on full payments decay over time, suggesting that minimum payments are a stickier default than full payments in the autopay context. Because average payment amounts are largely driven by full payments, which are typically much larger in dollar terms than minimum payments, the results provide suggestive evidence that autopay affects average payment amounts in addition to the distribution of payments. However, the direction of these effects may be context-dependent, and this study lacks the power to estimate them precisely.

This paper contributes to significant literatures on the credit card market, the behavioral economics of household financial decisions, and financial technology. It is most closely related to recent work studying the determinants of consumer credit card payment behavior. This work has shown significant deviations between observed payment patterns and the predictions of rational models that trade off intertemporal consumption smoothing with interest costs. Keys and Wang (2019) show that the credit card payment distribution is highly bimodal around the minimum and full statement balance, and that a significant amount of clustering around the minimum payment can be explained by anchoring bias. Gathergood, Mahoney, Stewart and Weber (2019) find that individuals with multiple credit cards do not allocate payments toward the highest-interest card. Medina (2021) shows that while nudges help reduce late payments on credit cards in Brazil, they have the unintentional consequence of increasing overdraft fees. Kuchler and Pagel (2021) show that many consumers underpay their credit card bills relative to self-reported plans, and that this behavior can be explained by models of naive and sophisticated present bias.

This study is the first to my knowledge to plausibly estimate the causal effects of autopay enrollment. But because my findings are consistent with descriptive and indirect evidence from other treatment settings produced by independent teams of researchers in the U.K., the prior literature lends credence to the external validity of my findings and the intrepretation that they are driven by enduring features of consumer behavior and autopay technology as opposed to idiosyncratic or transitory features of my specific research setting (Gathergood, Sakaguchi, Stewart and Weber, 2021; Sakaguchi, Stewart, Gathergood, Adams, Guttman-Kenney, Hayes and Hunt, 2022; Guttman-Kenney, Adams, Hunt, Laibson, Stewart and Leary, 2023).

This paper also contributes to the growing literature on regulation and competition in the credit card market, showing that while the market remains one of the most profitable sectors of the banking industry, recent regulation has reduced revenues from back-end fees and interest rate changes but had limited effects on consumer payment behavior (see, e.g. Agarwal, Chomsisengphet, Mahoney and Stroebel 2014; Stango and Zinman 2015; Ru and Schoar 2023; Agarwal, Chomsisengphet, Mahoney and Stroebel 2017; Nelson 2018; and Gross, Kluender, Liu, Notowidigdo and Wang 2021). Moreover, these studies show that the most vulnerable consumers in the market such as those with lower credit scores and lower education levels face the combined pressures of lower credit supply, higher fees, and more back-loaded fees. This paper contributes important evidence on how autopay and open banking may shape profitability, consumer outcomes, and the redistributive properties of the credit card market.

Finally, this paper relates to the literature on the role of technology in financial markets. Philippon (2016), Goldstein, Jiang and Karolyi (2019), Thakor (2020), and Berg, Fuster and Puri (2022) provide overviews of this literature. Prior studies examining the potential for technology to help consumers improve their decisions include D'Acunto, Prabhala and Rossi (2019) and Carlin, Olafsson and Pagel (2019). Related to this paper, recent work by Nam (2023) and Babina, Buchak and Gornall (2022) examine the implications of open banking data and regulations on consumer and market outcomes.

2 Data and Methods

2.1 Data

The data used for this analysis comes from an anonymous fintech credit card company, and includes account-level data for about 63,000 customers between 2018 and 2020. The credit card products offered by the fintech company range from $500 to $10,000 in credit limit and 10% to 30% APR, and are generally targeted toward consumers with lower credit scores and/or shorter credit records compared with the general population of cardholders. The cards include fewer fees and features compared with traditional credit cards issued by large banks, but otherwise function similarly to traditional cards and include a grace period over which interest charges can be avoided if the balance is paid in full each month. The card is widely accepted at online and physical retailers similar to traditional credit cards.

The cards also feature an online and mobile app that helps users track their budgets, transactions, and credit score. The app includes several tools to help consumers pay off their balance. Like all major credit cards, the company offers an autopay function. A key innovation that distinguishes this company from traditional credit card companies is its use of cashflow-based underwriting, which is supplemented by metrics such as traditional credit scores to help it predict the riskiness of new applicants. Users without sufficient traditional credit histories are required to undertake cashflow-based underwriting by linking their bank accounts to the app before being approved. After linking a bank account, approved users go through a few screens that ask whether they would like to opt in to autopay their credit card bill directly from a linked bank account each month.

Panel A of Figure 1 shows a stylized screenshot of the app interface for the autopay feature including three options: statement balance, minimum payment, and off. If autopay is enabled, the selected amount is automatically withdrawn from the user's linked bank account prior to the statement due date each month. Automatic payments can be cancelled up to three days prior to the due date without turning the feature off for future months, and the user receives an alert before the payment is withdrawn. The autopay setting can be changed at any time for all consumers in the sample, regardless of their treatment status based on the research design described below.

Whether or not autopay is enabled, users can make manual payments at any time using the stylized interface shown in Panel B. The standard home screen on the left shows the number of days until the next due date, and also prominently displays the current balance and statement balance due. After clicking "pay now," the user is shown the middle screen allowing them to choose a payment amount. The payment screen allows them to choose a payment amount, and displays the difference between the chosen amount and the full statement balance and the estimated interest charge for the next month given the chosen payment amount. At the bottom of the screen, the interface features a slider tool that allows users to see how the estimated interest charge would change with different payment amounts. Once they decide on a payment amount, users can schedule a payment for the current day or a date in the future.

For each anonymized user in the data, I have information on their basic demographics and credit score; monthly purchases, balances, and payments; and contract terms such as APR and credit limit. Table 1 presents summary statistics on the analysis sample. Panel A reports basic statistics at the borrower level. Account-holders have an average income of $44,363 as reported at the time they apply for a credit card. The average credit score at application is 664. On average, 26% of customers are enrolled in autopay.

Panel B presents statistics for monthly account-level panel. On a panel basis, 27% of customers are enrolled in autopay, which is very similar to the number when averaged by account in Panel A. The average credit limit is $1,839, and the average APR is 21%. Average utilization is 60%, and purchase volume is $384. The last section of Panel B presents some of the key outcome variables on customer payment behavior. The average payment is 39% of the statement balance, and the average minimum payment is $169. The average actual monthly payment is $259, combining all payments made in a given statement month.

Months where the actual payment is less than the minimum payment are considered delinquent, and this occurs in 14% of months. Twenty-two percent of payments are exactly equal to the minimum payment, and 27% are greater than or equal to the statement balance.[3] "Intermediate" payments are defined as those between the minimum and the full statement balance, and represent 36% of all months.

2.2 Descriptive Statistics

Next, I present descriptive evidence of monthly payment behavior based on autopay enrollment status. An account-month is classified as autopay if that feature is enabled, even if the consumer cancels the scheduled autopayment, over-rides the autopay setting and makes a manual payment instead, or makes both an autopayment and one or more manual payments. Account-months are classified as manual if autopay is disabled. Panel A of Figure 2 shows the distribution of monthly payments as a fraction of balances for each payment type. Consistent with evidence from Keys and Wang (2019) (henceforth KW) on traditional credit cards, both the manual and autopay distributions are highly bimodal. About 40–50% of payment months for both payment methods amount to less than 10% of the statement balance, while 25–40% are greater than or equal to the statement balance. Autopayments are much more likely to be for the full balance than manual payments.

Since the minimum payment is not a constant fraction of the statement balance, Panel B shows an alternative distribution of payment amounts in categories defined relative to the minimum and full statement balance. Following KW, payments are defined as near the minimum if they are strictly greater than but within $50 of the minimum, and those between the minimum plus $50 and the full payment are defined as "intermediate." This view shows significant differences based on autopay enrollment status. Delinquency rates are 17% for manual payments and 6% for autopayments, consistent with the idea that autopay could reduce delinquencies by, for example,

[3] Payments can be greater than the statement balance if, for example, the customer made additional purchases after the statement date and chooses to pay off the entire outstanding balance instead of just the statement balance.

reducing the likelihood that consumers forget to pay due to limited attention. Non-delinquent manual payments are fairly evenly split between exact minimum, near minimum, intermediate, and full payments. In contrast, autopayments are highly bimodal, with 71% of payments equal to the minimum or statement balance, about evenly split between the two extremes. Nonetheless, there appears to be a significant incidence of manual over-rides of the autopay setting, since 25% of autopayments are between the minimum and full balance.

While the payment behavior of customers of this fintech company is not directly comparable to the statistics in KW due to differences in time period, contract and customer characteristics, and the much wider range of balances on traditional credit cards, these descriptive results nonetheless provide a benchmark for comparison. The payment distribution by fraction of balance is significantly less bimodal for the fintech company than the general card population, comparing Panel A of Figure 2 to Figure 3 of KW, which is reproduced as Appendix Figure A1 of this paper. However, this could be due to the larger dispersion of balances in traditional, more mature card accounts that had more time to accumulate large balances.

The significant differences in delinquency rates across payment methods in the fintech data also make comparison a bit more challenging. After rescaling the distributions in Panel B of Figure 2 to consider only non-delinquent payments, a few patterns emerge. First, autopayments are more bimodal than manual payments and more than the general population in KW. While autopay is widely available across all major credit cards, the KW dataset did not include an indicator autopay enrollment, and I do not know of other studies on the use and effects of autopay for U.S. credit cards. The summary statistics in KW include both autopay and manual payments, and are pooled across different methods such as online, app-based, and mail (app-based payments were likely to be rare during the KW sample period). I reproduce the summary statistics from KW in the last column of Table 1. Among non-delinquent payments, 74% of autopayments in the fintech data are either the minimum or full, compared with 52% in KW. This comparison provides an initial indication that the use of autopay may be one reason why credit card payment distributions are so highly bimodal in the U.S. Non-delinquent manual payments in the fintech sample are remarkably similar to the general statistics in KW, with 51% equal to either the minimum or full payment, 22% near the minimum, and 28% of intermediate amounts compared with 52%, 22%, and 26% in KW.

2.3 Regression Discontinuity Design

As described above, the fintech company made two significant changes to its underwriting strategy during the sample period, which I use to provide causal evidence of the effects of autopay on account outcomes. The changes in underwriting flow were implemented at specific dates and apply to accounts opened after those dates, unknown to the customers. Thus, they generate sharp cutoffs around which the fraction of customers linking their bank accounts and enrolling in autopay changed discontinuously, but most other account characteristics remained constant or evolved smoothly.[4]

Under the standard assumptions of the regression discontinuity (RD) design described in more detail below, the results can be interpreted as the causal effect of autopay enrollment versus having to use the manual payment interface each month. Autopay settings can be changed or overridden at any time, so all users still have the choice to pay any amount each month. However, autopay enrollees would have either the minimum or full payment deducted automatically from their bank accounts without any action on their part, while non-enrollees must make a manual payment greater than or equal to the minimum in order to remain in good standing. Autopay enrollees may still fall delinquent or incur bank fees if they cancel their scheduled payment or do not have enough funds in their bank accounts to cover the scheduled payment.

The first underwriting change decreased the fraction of applicants required to undergo cashflow underwriting, in which users have to link their bank accounts to the app and a predictive algorithm is applied to their transactions in order to assess credit risk. Prior to the first change, most applicants underwent cashflow underwriting. After the change, borrowers are first screened based on their traditional credit history, and only those who cannot be underwritten successfully based on traditional credit information are required to undergo cashflow underwriting. This change was

[4]Initial versions of some of the analysis below were adapted from replication code from Deshpande (2016).

implemented for all accounts starting at a specific calendar date during the sample period that was not announced to applicants, and generates a discontinuity in the fraction of customers who linked their bank accounts based on the date of application. A significant fraction of applicants were still required to undergo cashflow underwriting after the change.

Figure 3 summarizes the empirical strategy for the RD design based on account origination date. Panel A shows the first stage outcomes for the first underwriting change. The x-axis shows the credit card origination week relative to the cutoff date, with a vertical line at the cutoff date. The graph plots the proportion of accounts originated in each week that are required to undergo cashflow underwriting and that are enrolled in autopay. While cashflow underwriting is a one-time process for each account at the time of origination, autopay enrollment is averaged across all active months for each account.

In orange triangles, the figure shows a sharp and discontinuous change in the cashflow underwriting requirement at the cutoff date, although exact levels are suppressed to preserve the anonymity of the data provider. Because a significant share of consumers no longer had to link their bank accounts when opening an account, there was also a sharp negative effect on autopay enrollment. Even though users could change their autopay settings at any time, the green circles show that autopay enrollment is about 20% lower for accounts originated after the cutoff date.

The company also implemented a second underwriting change several months after the first one, shown in Panel B. In this case, the underwriting change increased the fraction of accounts required to undertake cashflow underwriting. Instead of being implemented at a sharp calendar date, this change was rolled out across a three-week period, and resulted in about a 15% increase in autopay enrollment. I use both of these changes in underwriting flow, resulting in both increases and decreases in autopay enrollment, throughout the analysis below.

2.4 Covariate Balance

I use the discontinuities based on account origination date to identify the effect of autopay enrollment on payment outcomes. The key identifying assumptions of my RD design are that assignment to the underwriting treatments are as good as random around the origination date cutoffs, and that the outcome variables of interest would be smooth across the cutoffs if not for the changes in autopay enrollment conditional on observables. The cutoff dates were not publicly announced, and were a function of internal firm operations as they increased the sophistication of their underwriting system. Users were not aware of the cutoff dates or the different processes before and after the cutoffs, so were unlikely to game the timing of their applications to take advantage of different underwriting algorithms. I am also unaware of other specific changes to credit supply or other firm practices around the exact cutoff dates used to instrument for autopay enrollment.

I use a parametric RD specification to test whether the origination date instrument predicts observable customer and account characteristics around the cutoffs:

$$Y_i = \alpha + \beta Post_i + \gamma OrigDate_i^n + \kappa(Post_i \times OrigDate_i^n) + \varepsilon_i \tag{1}$$

where Y_i is a characteristic of account i, $Post_i$ is a dummy for accounts originated after the cutoffs, and $OrigDate_i^n$ is the origination date running variable of polynomial order n. Due to the gradual rollout of the second underwriting change between weeks 0 and 4, accounts originated between these dates are removed from the main analysis to improve the precision of the results.

Appendix Figures A2 and A3 show the average characteristics for accounts originated in each week within 10-week bandwidths around the cutoffs. The graphs in Figure A2 show that while account characteristics are evolving around the cutoff, only income shows a noticeable discontinuity. Income increases visually by about $2,000 relative to an average income in the mid-40,000s. Panels A and B of Table 2 show the results from equation (1) for the first underwriting change. Consistent with the graphs, the covariate balance tests show that while the Chi-square rejects covariate balance under both the linear and quadratic specifications, the estimated discontinuities are less than or equal to 10% of the mean in all specifications. The IV results on payment outcomes are robust across linear and quadratic specifications and the inclusion of controls for these characteristics.

Figure A3 and Panels C and D of Table 2 show tests for covariate balance for the second underwriting change. Estimation and inference are more challenging for the second change, because instead of a discontinuous change at an exact date, the change in underwriting was implemented across a three-week period from weeks 0 to 3 as shown in the figures. Furthermore, the graphs show that while vantage score and interest rate evolved smoothly over the period, income and credit limit at origination changed significantly around the cutoff dates coincident with the underwriting change. Thus, the underwriting change seemed to result not only in changes in bank account linkage and autopay, but also in at least two underlying characteristics of originated customers and accounts. I show below that all of my key results are robust to and quantitatively unchanged by the inclusion of detailed observables. I also address the potential for selection on unobservables in Section 3.4 below. Despite the significant changes in income and credit limit, the estimated discontinuities across other characteristics are extremely small and largely insignificant – one percent or less of the mean for vantage score and APR.

Appendix Table A1 shows the results of balance tests based on nonparametric local linear regression for different bandwidths for both underwriting changes. Due to the nonlinear evolution of underlying characteristics around the cutoffs, Chi-square statistics and estimated discontinuities are larger for larger bandwidths. This is especially true for income and credit limit. Based on visual inspection, I use a 10-week bandwidth for the main specifications, and show robustness to different polynomial orders, nonparametric local linear regression, and the inclusion of controls for the main results described below.

3 Results

3.1 First Stage Estimates

The goal of the analysis is to estimate the causal effect of autopay enrollment on account outcomes relative to the baseline manual payment interface. The following equation describes this causal relationship:

$$Y_{it} = \alpha + \beta Autopay_{it} + X_{it} + \varepsilon_{it} \tag{2}$$

where Y_{it} is an outcome of interest for consumer i in month t and $Autopay_{it}$ is an indicator for whether the consumer is enrolled in autopay in month t. Based on the empirical strategy described above, I use the changes in underwriting flow around two different cutoff dates as instruments for autopay enrollment. The first-stage equation is the following:

$$Autopay_{it} = \alpha + \beta Post_i + \gamma OrigDate_i^n + \kappa(Post_i \times OrigDate_i^n) + X_{it} + \varepsilon_{it} \tag{3}$$

I run these specifications both with and without controls. When included, the covariates in X_{it} are calendar month, state, and origination channel fixed effects; account age and account age squared; and non-parametric indicators for quintiles of vantage, income, and age at application, and current APR.

The $Post_i$ indicator is equal to one for accounts originated after a given cutoff date. As with the covariate balance tests described above, due to the gradual rollout of the second underwriting change between weeks 0 and 4, accounts originated between these dates are removed from the analysis of the second underwriting change.[5] As shown in Figure 3, the first cutoff date is associated with a significant decline in the fraction of accounts required to undergo cashflow underwriting, and the second cutoff date is associated with a significant increase.[6] The first column of Tables 3 and 4 show the regression results from equation (3) for the first stage outcome of autopay enrollment without the inclusion of control variables. Autopay enrollment is measured in a monthly panel, so the results represent average treatment effects over the entire sample period. Appendix Tables A2 and A3 present the results when controls are included.

[5] Results remain economically large and qualitatively unchanged, but slightly attenuated and noisier when all accounts are included, and are available upon request.

[6] Exact levels of the changes in cashflow underwriting requirements are shrouded to protect the proprietary information of the data provider. The discontinuous changes in the fraction of accounts required to undergo cashflow underwriting are larger than the changes in autopay enrollment, consistent with the interpretation that cashflow underwriting acts as a nudge to enroll in autopay that is taken up by a significant fraction of those treated.

The results show that the first underwriting change is associated with a 19 to 23 percentage point decline in autopay enrollment, with the results highly robust across specifications and to the inclusion of controls. The second underwriting change is associated with a 12 to 18 percentage point increase in autopay enrollment across all specifications. Figure 4 shows the first stage effects on autopay enrollment when equation (3) is run separately for each month relative to origination, with each point representing a separate cross-sectional regression. The results show that the effect of cashflow underwriting on autopay enrollment is highly persistent for both underwriting changes. Although users can unlink their bank accounts after origination and can change their autopay settings at any time, the decision to enroll in autopay at origination is sticky and stable for at least 6-10 months.

3.2 Reduced Form Results

Before turning to the IV analyses that form my headline results, I next present reduced-form evidence to support the assumptions and test the robustness of my regression discontinuity design. Figure 5 shows unconditional binscatter graphs for the key account outcomes for the first underwriting change, and columns (2) through (8) of Table 3 present parametric RD estimates for several polynomial orders as well as nonparametric local linear regression estimates, excluding controls. The same columns in Appendix Table A2 present the results with controls. The graphs in Figure 5 show clear discontinuities for chargeoff, minimum payments, and intermediate payments associated with the first underwriting change. The graphs look similar after residualizing with respect to the control variables, and are omitted from the paper for brevity and available upon request.

The baseline reduced form results for the second underwriting change are shown in Figure 6 and Table 4, with Appendix Table A3 showing the results with controls. Since the first stage on autopay enrollment is significantly smaller for the second underwriting change, the discontinuities in outcomes are not as visibly sharp. However, consistent with the results from the first underwriting change, the top right graph in Figure 6 shows a clear discontinuity in minimum payments between weeks 0 and 4. Thus, the reduced form results for both underwriting changes are consistent with a positive effect of autopay enrollment on minimum payments.

A major difference between the results across the two underwriting changes is that the second underwriting change shows no clear effect on chargeoffs based on the top left subfigure in Figure 6. While the discontinuity in chargeoffs is clearly visible and robustly estimated for the first underwriting change, there is no clear graphical discontinuity and small and largely insignificant regression results for the second underwriting change. Thus, the results across both underwriting changes show that autopay *can* cause significant changes in both short-run delinquency and permanent chargeoff, but that this effect may not manifest in every instance depending on the setting, even within my sample.

3.3 Instrumental Variables Estimates

To interpret my results in terms of the causal effects of a change from 0% to 100% autopay enrollment, Table 5 presents IV estimates using the two underwriting changes as instruments for autopay. My baseline specification omits account-level controls, and Appendix Table A4 presents the results with the inclusion of controls.

I begin by describing the effects of autopay on chargeoffs and delinquencies. As described above, the first underwriting change is associated with visible discontinuities in chargeoff and delinquency rates, while the second underwriting change yields no visible discontinuities and insignificant reduced form results. The IV coefficients from columns (1) and (2) of Tables 5 and A4 show that based on the first underwriting change, a 100% increase in autopay enrollment would reduce chargeoffs by 13 to 19pp which would eliminate all chargeoffs based on the pre-period rate of 10%. In contrast, the second underwriting change is not associated with a robust effect on chargeoff rates.

Next, I turn to the effects of autopay on minimum payments, the most robust and clearly visible discontinuity based on the reduced form results. As shown in column (3) of Table 5, autopay leads to a very large 20 to 29 percentage point increase in minimum payments, more than doubling the baseline rate of 19 to 28%. This result is very consistent across both underwriting changes and across all specifications with and without controls, lending credence to the role of default options on payment choices in the credit card market.

The minimum payment results are complementary to estimates from Keys and Wang (2019) showing that at least 9% of all credit card accounts are 'anchored' to the minimum payment instead of making an active choice to pay that amount. While KW focused on near-minimum payments that are close to but not exactly equal to the minimum, this paper shows that exact minimum payments are also subject to default effects. Extrapolating the estimated effect sizes to the general population, the IV estimates imply that autopay would account for 4 to 6pp of minimum payers in the credit card market.[7] Thus, the combined effect of anchoring and autopay would drive 13 to 15pp of all payments, accounting for half of the 29pp of accounts that make minimum or near-minimum payments documented by KW. While these calculations are based on the assumption that cardholders who choose autopay in the general population would react in the same way as those treated by a nudge to enroll in autopay in a fintech setting, they nonetheless illustrate that autopay settings could be an economically important factor in explaining the bimodal distribution of credit card payments observed in the market as a whole.

Consistent with autopay contributing to the bimodal payment distribution, column (4) of Table 5 shows a large negative effect on payments between the minimum and the full balance, i.e. the middle of the payment distribution. Because autopay includes both a minimum and full payment setting, enrollment could plausibly push payments equally toward both extremes. However, the effects are less consistent for full compared with minimum payments. As shown in column (5), autopay has a positive effect on full payments for the first underwriting change and a negative effect for the second underwriting change, with effect sizes consistently smaller than those for minimum payments.

The results from columns (2) through (5) clearly show that autopay affects the distribution of payment amounts. However, the more important economic question is whether it also affects average payment amounts. If consumers optimize their payments to balance the attention costs of

[7] These percentages are calculated by multiplying the 20 to 29pp effect of autopay on minimum payments by the 20% of consumers who enroll in autopay as of 2020 according to the Consumer Financial Protection Bureau (2021).

deviating from autopay with interest costs and consumption smoothing motives, it is still plausible that autopay could have limited effects on overall payments and indebtedness, with only second-order effects on interest costs.

The results along these lines are inconclusive. While columns (6) and (7) of Table 5 suggest that autopay leads to economically and statistically significant changes in overall payment amounts in the specifications without controls, the directions of the effects differ across the two underwriting changes. Furthermore, the same columns in Table A4 show that the results are significantly attenuated and sometimes change sign with the inclusion of controls. Importantly, the standard errors are too large to rule out economically meaningful effects. Thus, this study leaves the open possibility that autopay affects long-run outcomes such as consumer indebtedness, consumption, and credit scores, but lacks the precision to estimate such effects.

To better understand the mechanisms and dynamics behind autopay enrollment, Figures 7 and 8 show graphs of the IV estimates for key account outcomes at each account age to illustrate how these outcomes evolve over time. The top left graph of each figure shows the evolution of the effect of autopay on delinquencies. For the first underwriting change, the delinquency effect is largest initially, and then decrease over time until it reaches the baseline rate by 8 to 10 months after origination. In the first month after origination, autopay is associated with a very large 40 percentage point decrease in the delinquency rate. This shows that in the context of the first underwriting change, many accounts that did not link their bank accounts or enroll in autopay became delinquent immediately. While some of these initial delinquencies cured, autopay caused a 13 to 19pp reduction in permanent chargeoffs. The dynamics of delinquency do not follow a clear pattern for the second underwriting change.

As shown in the top right of Figures 7 and 8, the effects of autopay on minimum payments are stable and persistent over the first 10 months after origination, suggesting that once set, minimum automatic payments are a 'sticky' default. In contrast, in the right graph of the second row of both figures, full payments converge to the baseline level over time for both underwriting changes. The contrast in dynamics between minimum and full payment settings suggest that when consumers set autopay to the minimum payment, they are likely to leave it there and not over-ride it with larger manual payments. When they set autopay to the full payment, they may change their settings over time or over-ride autopay in months when they cannot pay the full balance, gradually undoing the effects of the full payment default.

3.4 Selection on Unobservables

Selection on unobservables is an important threat to identification in my setting. Although key observables evolve smoothly with economically small estimated discontinuities around the date of the first underwriting change, income and credit limit change significantly around the second underwriting change. Thus, it is plausible that unobserved characteristics also change around the cutoff dates and could drive the outcomes of interest. Furthermore, the instruments I use involve changes in underwriting – the process of screening customer applications. To my knowledge, the underwriting changes were implemented by the fintech company in order to test the usage of different combinations of cashflow and traditional credit information without explicitly aiming to change the credit profile, autopay enrollment, or payment behavior of customers. But the underwriting changes I examine could nonetheless have altered the composition of customers inclined toward different types of payment behavior, thus threatening my interpretation of the causal effects of autopay.

In the spirit of formal tests proposed by Oster (2019) and Diegert, Masten and Poirier (2022) robustness to the presence of omitted variables can be assessed by examining sensitivity to observables. Although I cannot fully disprove the possibility of selection on unobservables, I present several arguments for why they are unlikely to drive my results. First, my dataset includes a rich set of observables, including most variables observed by the lender and indicators for both the economic condition and credit risk of consumers. As described above, the first stage, reduced form and key IV results are virtually unchanged statistically and quantitatively with the inclusion of controls for calendar month, state, and origination channel fixed effects; account age and account age squared; and non-parametric indicators for quintiles of vantage, income, and age at application, and current APR. Because these observables span a range of potential consumer characteristics that could endogenously drive payment behavior, it is unlikely that unobservable characteristics completely uncorrelated with them would overturn my findings.

Second, I show that the coefficients of the observed controls themselves are small relative to the effects of the instrument to further support the interpretation that my results are likely to be driven by autopay enrollment rather than endogenous unobservables. Figure A4 graphs the coefficients for income, vantage, and APR quintile in the first stage regression when the full suite of controls is included, corresponding to the results shown in Panel A of Tables A2 and A3. While my dataset does not include the full set of variables observable to the lender, e.g. digital footprints or the proprietary cashflow model used by the lender, it is not the case that the control variables I do have are weak or uninformative. As shown in the figures, income, vantage score, and APR are predictive of both autopay enrollment and minimum payments in the expected ways. Furthermore, APR and credit limit are the ultimate outcomes of the lender's credit supply function based on the full set of variables it observes, so including these variables as controls acts as a sufficient statistic for the lender's information set.

Consistent with the idea that more sophisticated consumers choose to enroll in autopay, enrollment increases with income and Vantage score and decreases with APR. However, these effects are small to modest relative to the effects of the nudge I examine. While the underwriting instrument yields changes in autopay enrollment of 14 to 23pp in these specifications, moving from the first to the fifth quntile of income changes autopay enrollment by less than 10pp, and similar changes across the entire distribution of Vantage score and APR only affect autopay enrollment by single digit percentage points. These results suggest that changes in technological defaults rather than individual or contractual characteristics drive the bulk of autopay enrollment, so unobserved characteristics of consumers are unlikely to explain autopay enrollment.

Figure A5 graphs the coefficients on the same control variables for the key IV outcome: the propensity to make the minimum payment. Again, compared to the 13 to 32pp effect of autopay on minimum payments shown in the associated regression results from Table A4, the control variables have a much smaller and often economically insignificant effect even across the entire distribution of income, Vantage, and APR. Furthermore, the weak relationship between consumer characteristics and the propensity to make minimum payments is supported by descriptive evidence from Keys and Wang (2019). Overall, the lack of sensitivity of the effects to the inclusion of rich controls and the small economic effect of observables provide supportive evidence that the autopay nudge rather than endogenous unobserved consumer characteristics drives my findings.

4 Conclusion

This paper is the first to my knowledge to study the causal effects of autopay enrollment on consumer credit card payments. Using two changes to the underwriting model of a fintech company that induced significant changes in autopay enrollment rates, I show that autopay has very large effects on the payment distribution. In one of the two research settings, autopay also significantly decreases chargeoffs. Although consumers may change their autopay settings at any time in this context and for most credit cards in the market, I find that the autopay enrollment decision at origination is highly persistent. According to my baseline IV results, a change from 0% to 100% autopay enrollment more than doubles the rate of minimum payments, an increase of between 20 and 29pp. In contrast to minimum payments, the effects of autopay on full payments is small, varying in sign, and less robust to the inclusion of controls, suggesting that full payments are a less sticky default that minimum payments. These estimates show that seemingly minor technological settings can have economically large effects on credit risk and payment outcomes for both consumers and firms.

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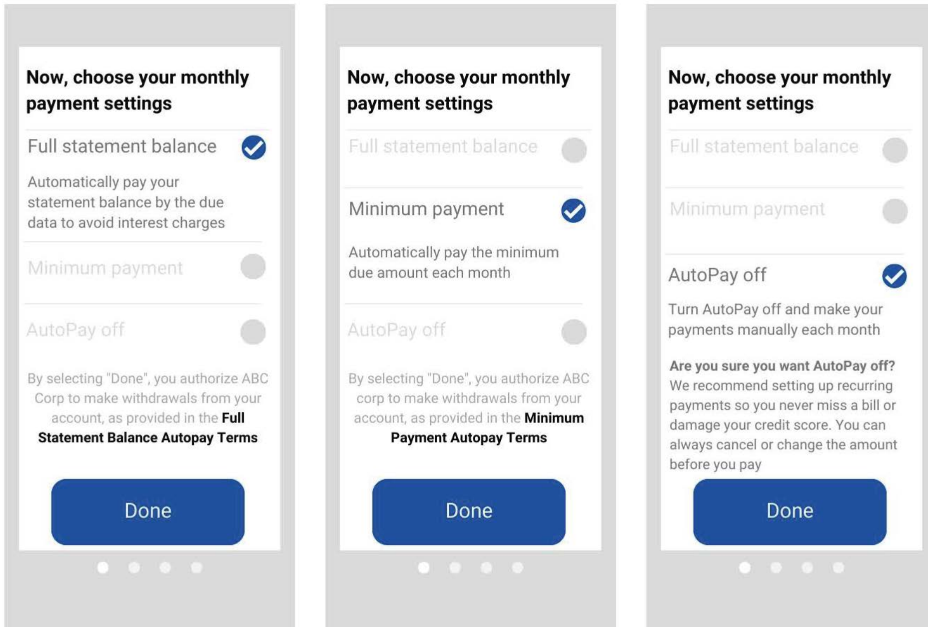
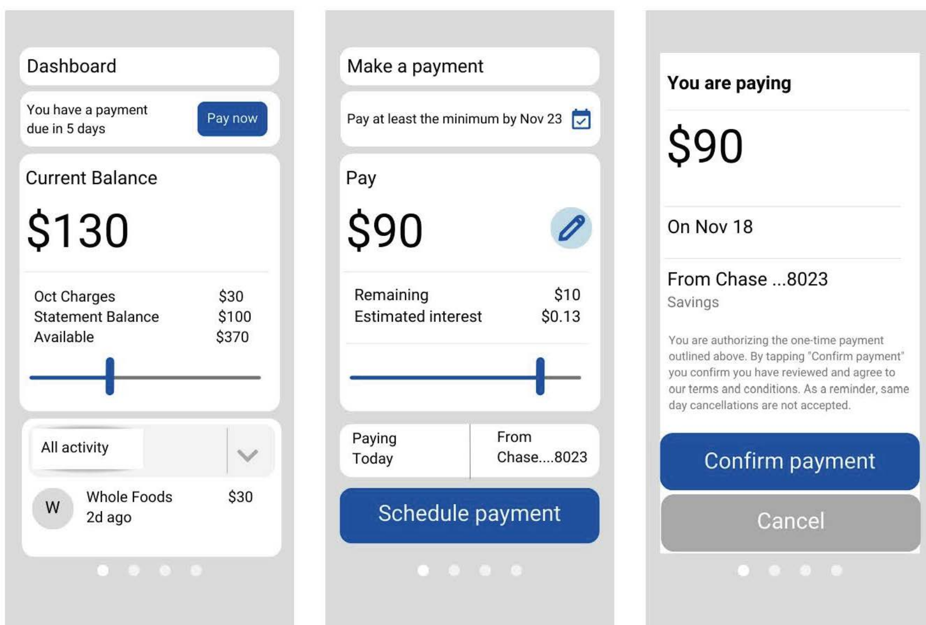
Figure 1: **Stylized Screenshots of Autopay and Manual Interfaces**

Panel A: Autopay



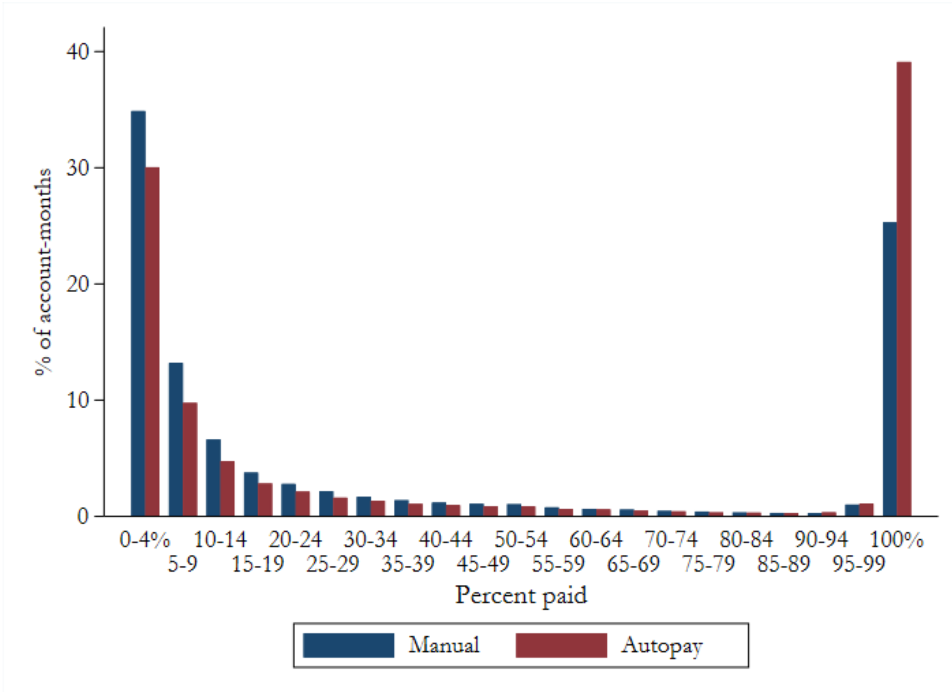
Panel B: Manual



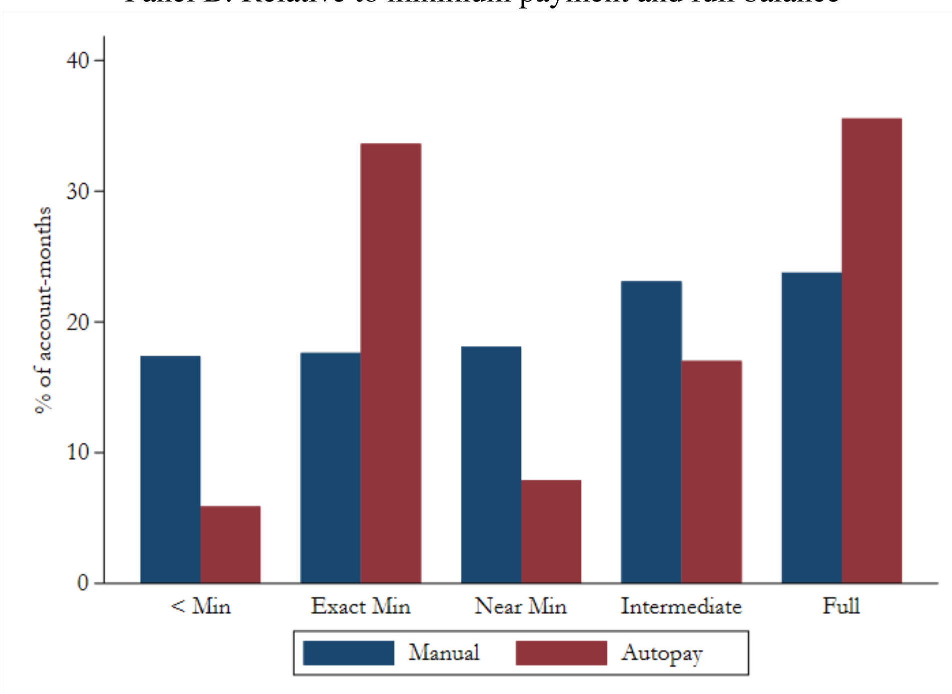
Notes: Stylized screenshots of autopay and manual payment interfaces. While the substantive content and layout are similar to what real customers would have seen during the sample period, some graphical elements have been modified to protect the anonymity of the data provider.

Figure 2: **Payment Distributions by Autopay Enrollment Status**

Panel A: Fraction of balance



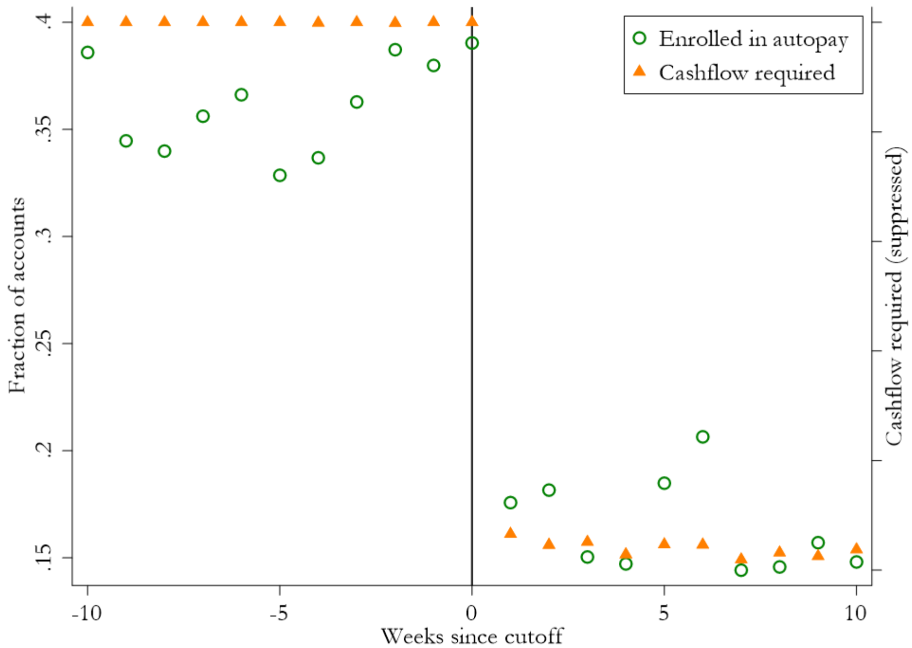
Panel B: Relative to minimum payment and full balance



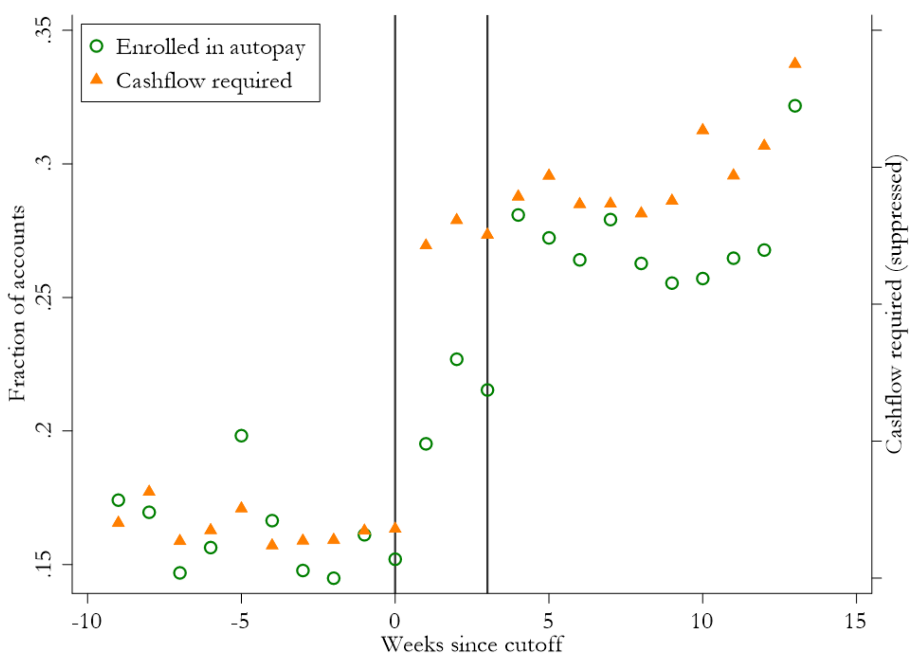
Notes: Figure shows distributions for monthly payment activity based on autopay enrollment status for each account-month. Panel A shows the payment distribution based on fraction of the full statement balance, and Panel B shows categories based on dollar amounts relative to the minimum and full payment. Payments are defined as near the minimum if they are strictly greater than but within $50 of the minimum, and those between the minimum plus $50 and the full payment are defined as “intermediate.”

Figure 3: **Empirical Strategy Using Changes in Underwriting Flow**

Panel A: First underwriting change



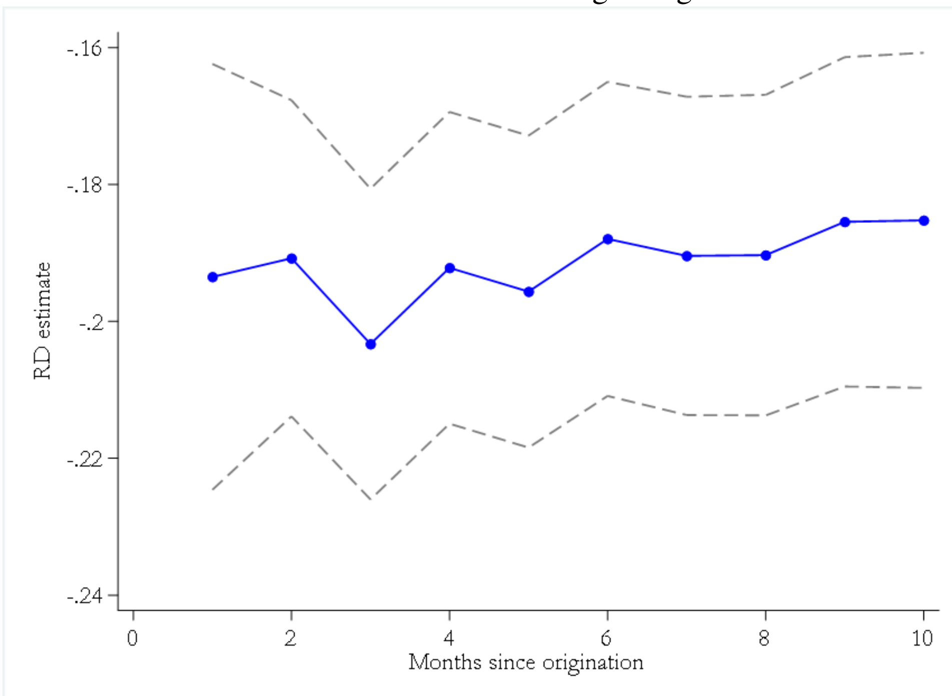
Panel B: Second underwriting change



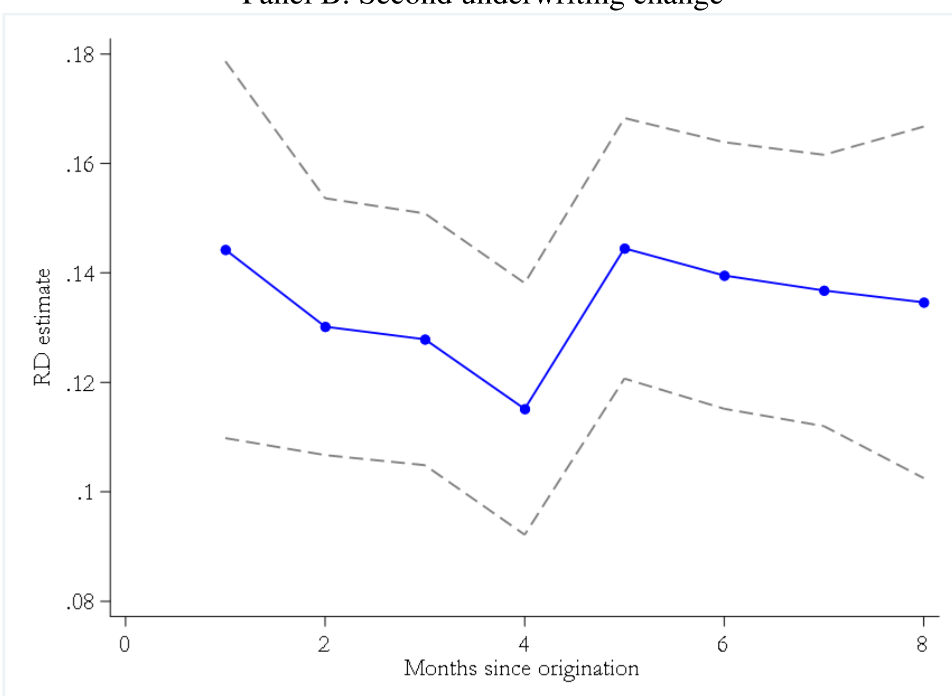
Notes: Figure plots proportion of accounts originated in each calendar week that were required to undergo cashflow underwriting and that are enrolled in autopay. Cashflow underwriting is a one-time measure at the time of origination, while enrollment in autopay is averaged across all observations for each account. The y-axis for cashflow underwriting is suppressed to protect the proprietary information of the data provider. Sample includes accounts originated within 10 weeks of each cutoff date.

Figure 4: **Persistence of First Stage for Autopay Enrollment by Account Age**

Panel A: First underwriting change

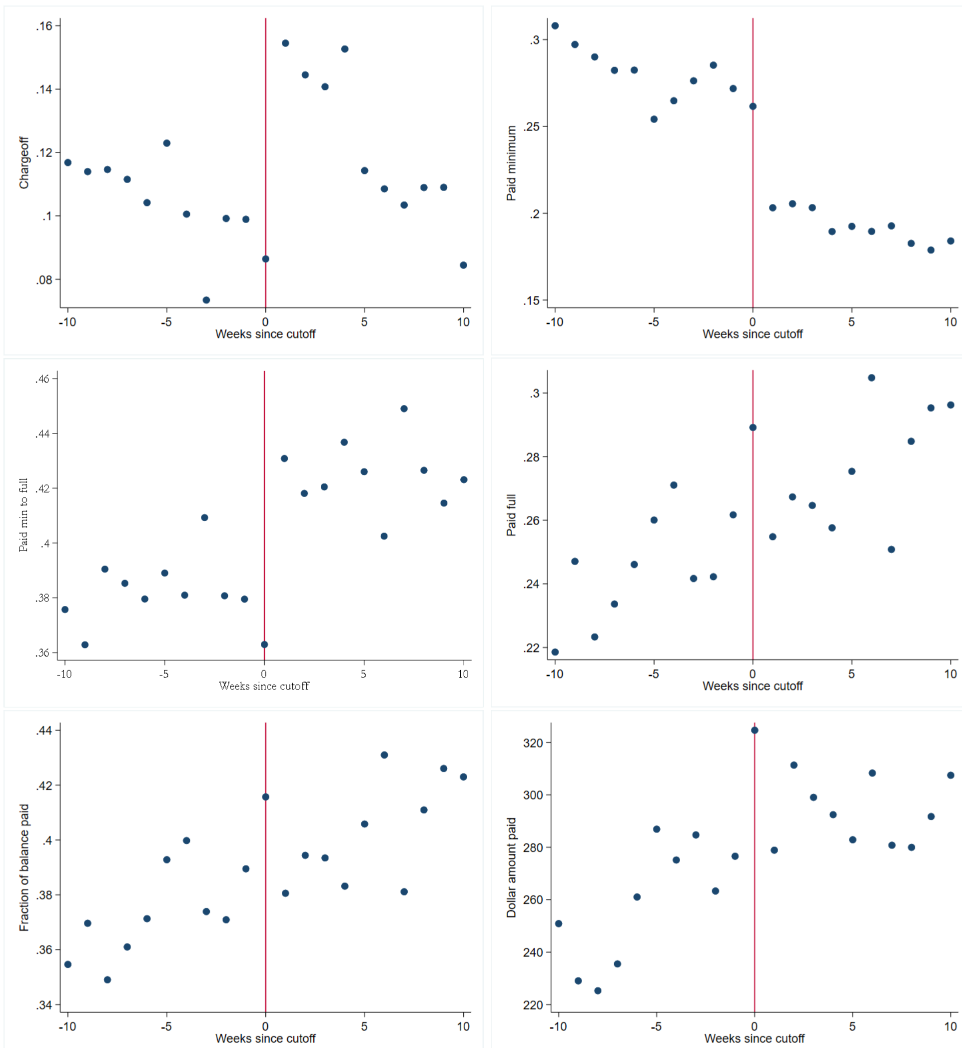


Panel B: Second underwriting change



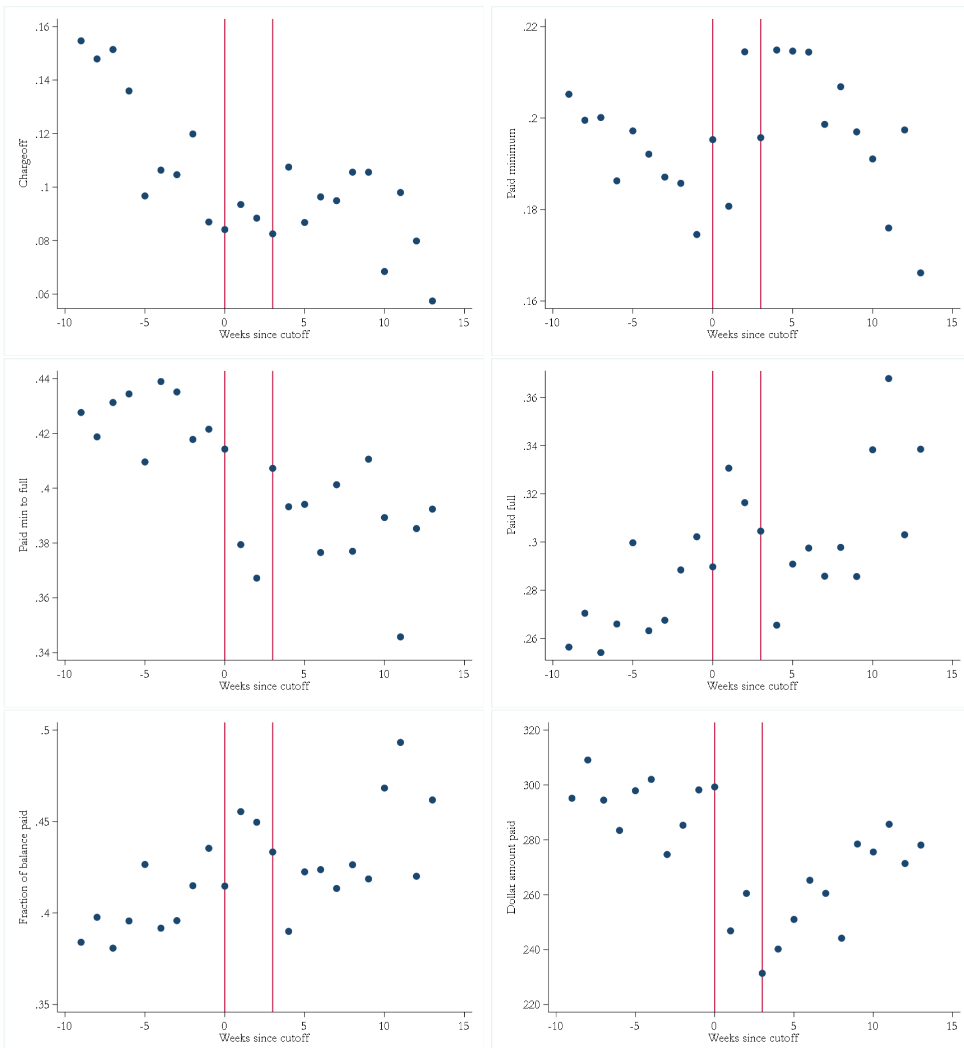
Notes: Figures plot parametric RD estimates of the effects of cashflow underwriting on autopay enrollment around the two changes in underwriting, using a polynomial of order 1 with no covariates. Dashed lines represent 95% confidence intervals. Sample includes accounts originated within 10 weeks of the cutoff dates.

Figure 5: **Reduced Form Outcomes - First Underwriting Change**



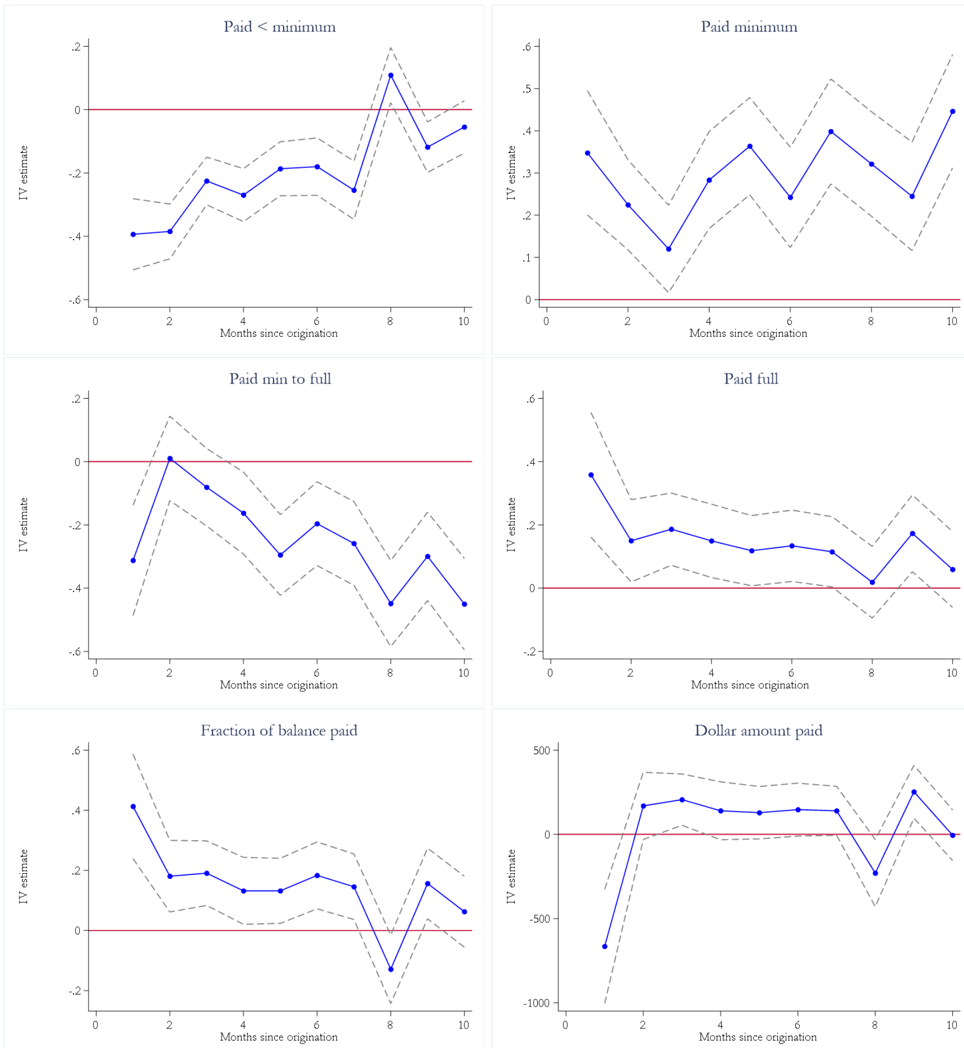
Notes: Figures plot average account outcomes by origination week for accounts originated within 10 weeks of the first change in underwriting flow. Chargeoff is a one-time outcome per account, and the other outcomes are pooled across all observations for each account.

Figure 6: **Reduced Form Outcomes - Second Underwriting Change**



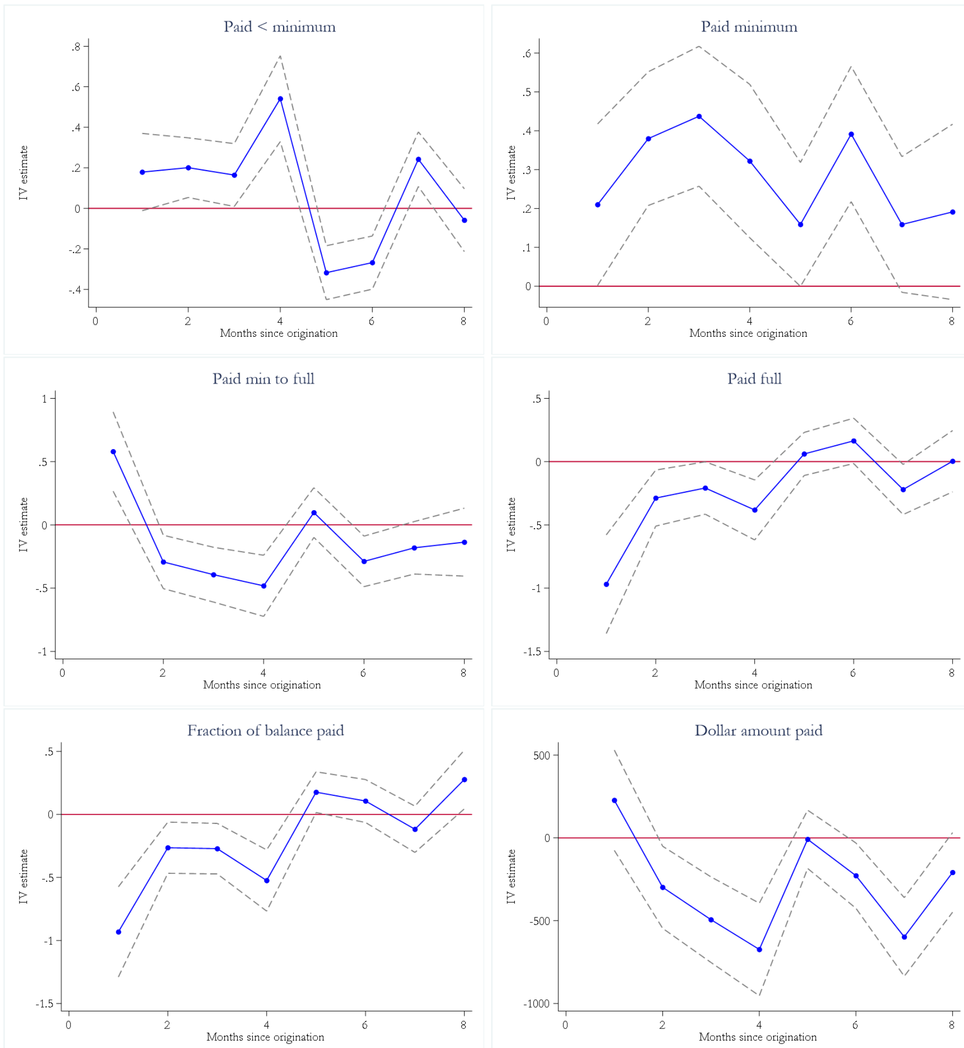
Notes: Figures plot average account outcomes by origination week for accounts originated within 10 weeks of the second change in underwriting flow. Chargeoff is a one-time outcome per account, and the other outcomes are pooled across all observations for each account.

Figure 7: **IV Estimates by Account Age - First Underwriting Change**



Notes: Figures plot parametric IV RD estimates of the effects of autopay enrollment on account outcomes, using a polynomial of order 1 with no covariates. Each point represents a separate cross-sectional IV RD estimate, conditional on the number of months after account origination. Dashed lines represent 95% confidence intervals. Sample includes accounts originated within 10 weeks of the cutoff date.

Figure 8: **IV Estimates by Account Age - Second Underwriting Change**



Notes: Figures plot parametric IV RD estimates of the effects of autopay enrollment on account outcomes, using a polynomial of order 1 with no covariates. Each point represents a separate cross-sectional IV RD estimate, conditional on the number of months after account origination. Dashed lines represent 95% confidence intervals. Sample includes accounts originated within 10 weeks of the cutoff dates.

Table 1: **Summary Statistics**

Panel A: Account-level Characteristics	Mean	Median	Std. Dev.	KW Mean
Income	$44,363	$35,000	$36,321	$65,583
Credit score at application	664	658	39	701
Enrolled in autopay	26%			

Panel B: Monthly data	Mean	Median	Std. Dev.	KW Mean
Card and account				
Enrolled in autopay	27%			
Credit limit	$1,839	$1,500	$1,425	$9,767
Retail APR	21%	22%	5%	16%
Purchases and balances				
Utilization	60%	69%	36%	45%
Balance	$1,075	$737	$1,364	$3,187
Interest charged	$15	$9	$22	
Purchase volume	$384	$126	$1,080	$501
Purchase volume > 0	78%			63%
Payment and delinquency				
Fraction paid	39%	13%	43%	42%
Minimum payment	$169	$27	$943	$82
Actual payment	$259	$94	$542	$570
Payment:				
< Minimum	14%			9%
Minimum	22%			15%
Intermediate	36%			43%
Full	27%			33%

Notes: Panel A shows account-level summary statistics, based on characteristics at origination and average autopay enrollment across all observations for each account. Panel B shows summary statistics based on the monthly panel of account activity. The final column shows analogous mean statistics from Table 1 of Keys and Wang (2019) based on a representative sample of general-purpose credit cards between 2008–2013.

Table 2: **Covariate Balance Tests**

	(1)	(2)	(3)	(4)
	Income	Vantage	Credit limit	APR
	Panel A: First Change, Linear			
Sample Mean:	$45,645	661	$1,926	22%
Post	4648	1.887	122.2	0.013
	(898)	(0.909)	(36.7)	(0.001)
	[0.000]	[0.038]	[0.001]	[0.000]
Percent of mean	0.10	0.00	0.06	0.06
	Observations: 31,160, Chi-square test: 215.39, p-value: 0.000			
	Panel B: First Change, Quadratic			
Post	- 1123	0.204	- 8.882	- 0.003
	(1360)	(1.346)	(55.0)	(0.002)
	[0.409]	[0.880]	[0.872]	[0.080]
Percent of mean	- 0.02	0.00	0.00	- 0.01
	Observations: 31,160, Chi-square test: 3.70, p-value: 0.448			
	Panel C: Second Change, Linear			
Sample Mean:	$46,063	666	$2,115	20%
Post	- 8865	- 2.963	- 530.4	0.000
	(849)	(1.006)	(33.8)	(0.001)
	[0.000]	[0.003]	[0.000]	[0.823]
Percent of mean	- 0.19	0.00	- 0.25	0.00
	Observations: 27,727, Chi-square test: 280.53, p-value: 0.000			
	Panel D: Second Change, Quadratic			
Post	- 7821	- 1.277	- 526.4	0.001
	(1314)	(1.594)	(51.6)	(0.002)
	[0.000]	[0.423]	[0.000]	[0.513]
Percent of mean	- 0.17	0.00	- 0.25	0.01
	Observations: 27,727, Chi-square test: 123.96, p-value: 0.000			

Notes: Table presents balance tests for the linear and quadratic RD specifications from equation (1) for the two underwriting changes. The first row of Panels A and C present sample means in the 10 weeks before the cutoff dates for each variable, and each panel includes a row calculating the discontinuity estimate as a percentage of the pre-period mean. Samples include accounts originated within 10 weeks of the cutoff dates.

Table 3: **First Stage and Reduced Form Estimates - First Underwriting Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Autopay	Chargeoff	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	36%	10%	9%	28%	38%	25%	38%	$266
Panel A: Linear								
Post	- 0.191	0.074	0.033	- 0.056	0.044	- 0.021	- 0.023	- 14.780
	(0.004)	(0.008)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.811)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]
Panel B: Quadratic								
Post	- 0.229	0.074	0.030	- 0.067	0.055	- 0.018	- 0.021	- 24.710
	(0.005)	(0.011)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(7.071)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]
Panel C: Cubic								
Post	- 0.221	0.055	0.024	- 0.056	0.066	- 0.034	- 0.031	- 41.180
	(0.007)	(0.015)	(0.005)	(0.007)	(0.008)	(0.007)	(0.007)	(9.089)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel D: Quartic								
Post	- 0.222	0.031	0.019	- 0.037	0.080	- 0.062	- 0.066	- 88.170
	(0.008)	(0.018)	(0.006)	(0.008)	(0.009)	(0.009)	(0.008)	(11.120)
	[0.000]	[0.085]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel E: Local linear regression								
Post	- 0.193	0.074	0.033	- 0.057	0.045	- 0.021	- 0.023	- 15.440
	(0.004)	(0.008)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.788)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]

Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for first-stage and reduced-from outcomes around the first change in underwriting flow. Sample includes accounts originated within 10 weeks of the cutoff date.

Table 4: **First Stage and Reduced Form Estimates - Second Underwriting Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Autopay	Chargeoff	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	16%	12%	11%	19%	43%	27%	40%	$293
Panel A: Linear								
Post	0.130	0.025	0.012	0.036	- 0.034	- 0.013	- 0.011	- 45.95
	(0.004)	(0.008)	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)	(4.91)
	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]	[0.005]	[0.009]	[0.000]
Panel B: Quadratic								
Post	0.140	0.006	0.018	0.028	- 0.030	- 0.015	- 0.018	- 60.66
	(0.007)	(0.013)	(0.005)	(0.007)	(0.008)	(0.007)	(0.007)	(7.72)
	[0.000]	[0.608]	[0.001]	[0.000]	[0.000]	[0.039]	[0.011]	[0.000]
Panel C: Cubic								
Post	0.122	0.010	0.010	0.020	- 0.032	0.001	0.001	- 67.05
	(0.010)	(0.020)	(0.010)	(0.009)	(0.011)	(0.011)	(0.010)	(10.76)
	[0.000]	[0.410]	[0.120]	[0.038]	[0.005]	[0.953]	[0.898]	[0.000]
Panel D: Quartic								
Post	0.116	0.043	0.045	0.019	- 0.023	- 0.041	- 0.044	- 90.8
	(0.014)	(0.024)	(0.010)	(0.013)	(0.015)	(0.014)	(0.013)	(14.1)
	[0.000]	[0.072]	[0.000]	[0.130]	[0.122]	[0.003]	[0.001]	[0.000]
Panel E: Local linear regression								
Post	0.131	0.024	0.012	0.035	- 0.034	- 0.013	- 0.012	- 46.63
	(0.004)	(0.008)	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)	(4.894)
	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]	[0.004]	[0.008]	[0.000]

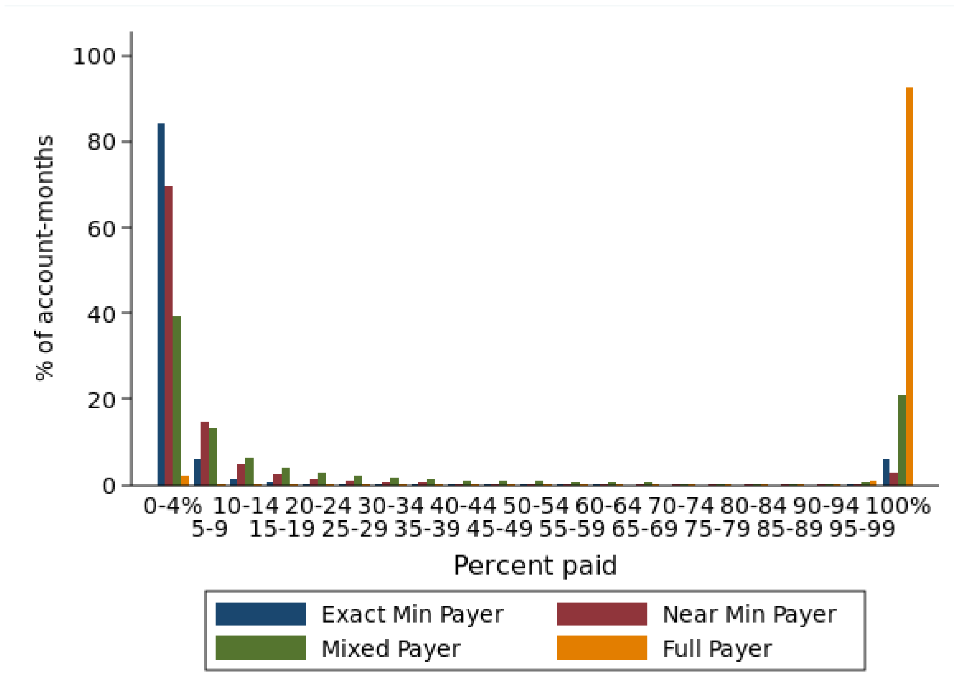
Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for first-stage and reduced-from outcomes around the second change in underwriting flow. Sample includes accounts originated within 10 weeks of the cutoff dates.

Table 5: **Instrumental Variables Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Chargeoff	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
	Panel A: First Change, Linear						
Pre-period mean:	10%	9%	28%	38%	25%	38%	$266
Autopay	- 0.191	- 0.175	0.294	- 0.229	0.110	0.119	77.45
	(0.011)	(0.013)	(0.018)	(0.020)	(0.018)	(0.017)	(25.19)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]
	Panel B: First Change, Quadratic						
Autopay	- 0.133	- 0.128	0.292	- 0.241	0.077	0.092	107.70
	(0.014)	(0.015)	(0.022)	(0.025)	(0.023)	(0.022)	(30.79)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]
	Panel C: Second Change, Linear						
Pre-period mean:	12%	11%	19%	43%	27%	40%	$293
Autopay	0.064	0.091	0.273	- 0.263	- 0.101	- 0.087	- 352.4
	(0.019)	(0.025)	(0.032)	(0.038)	(0.036)	(0.034)	(40.33)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.005]	[0.011]	[0.000]
	Panel D: Second Change, Quadratic						
Autopay	0.034	0.126	0.200	- 0.216	- 0.110	- 0.128	- 433.4
	(0.029)	(0.039)	(0.047)	(0.057)	(0.054)	(0.052)	(60.9)
	[0.242]	[0.001]	[0.000]	[0.000]	[0.042]	[0.013]	[0.000]

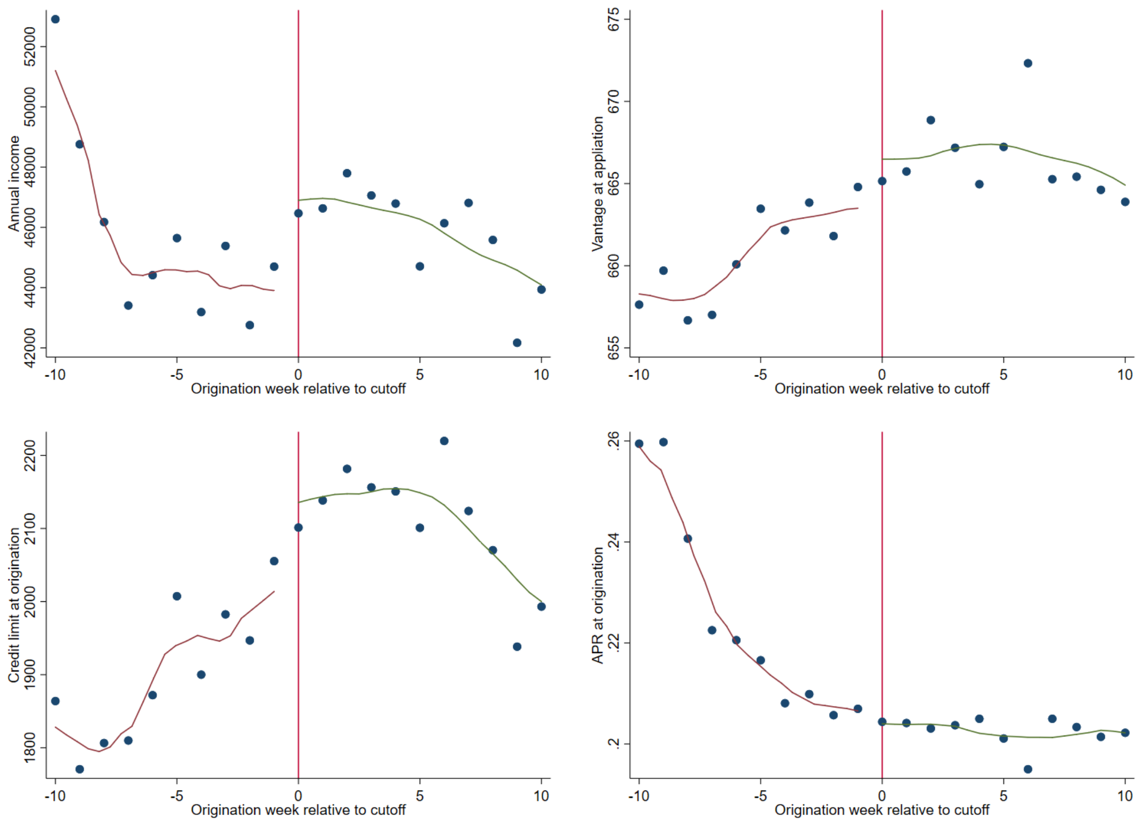
Notes: Table shows parametric RD estimates for the effect of autopay enrollment on payment outcomes, instrumenting autopay enrollment with dummy variables for account origination dates following the two changes in underwriting. Sample includes accounts originated within 10 weeks of the cutoff dates.

Figure A1: **Payment Distribution from Keys and Wang (2019)**



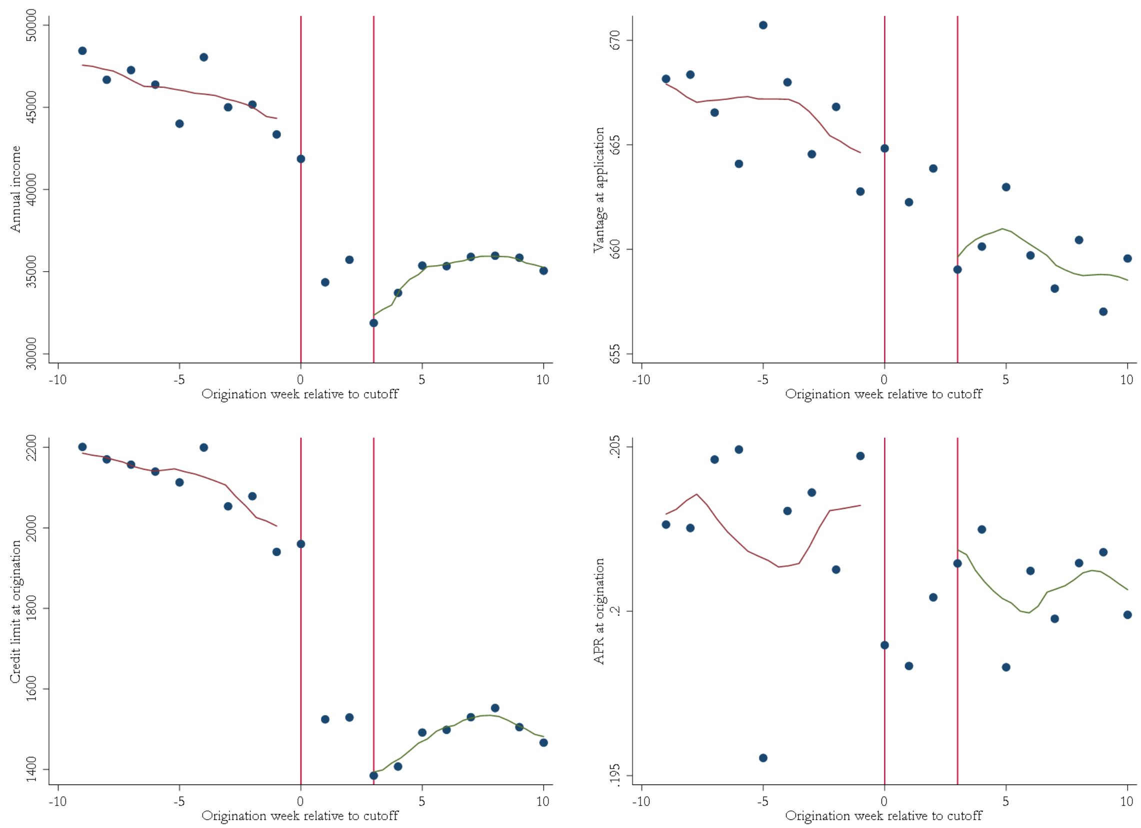
Notes: Figure shows the distribution of payments as a fraction of balance from Figure 3 of Keys and Wang (2019). Each account is classified into a payer type based on whether the account was paid in full or paid at or near the minimum amount in at least 50% of months. Accounts that did not pay any of these three amounts in 50% of months are classified as mixed payers. Payments are defined as "near" the minimum if they are strictly greater than but within $50 of the minimum.

Figure A2: **Covariate Balance - First Underwriting Change**



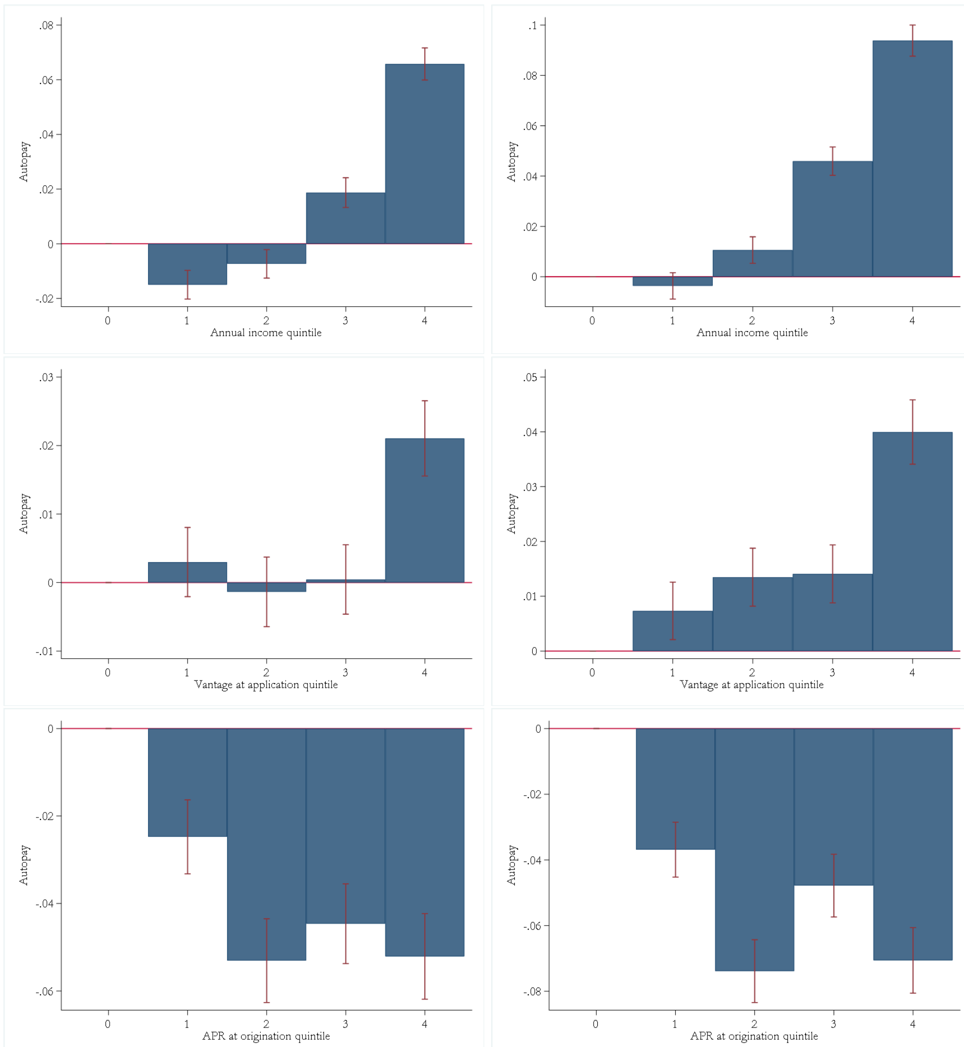
Notes: Graphs plot consumer and account characteristics by account origination date relative to the cutoff for the first change in underwriting flow. Each graph plots the average of the covariate across accounts opened in that calendar week, within a 10-week window of the cutoff.

Figure A3: **Covariate Balance - Second Underwriting Change**



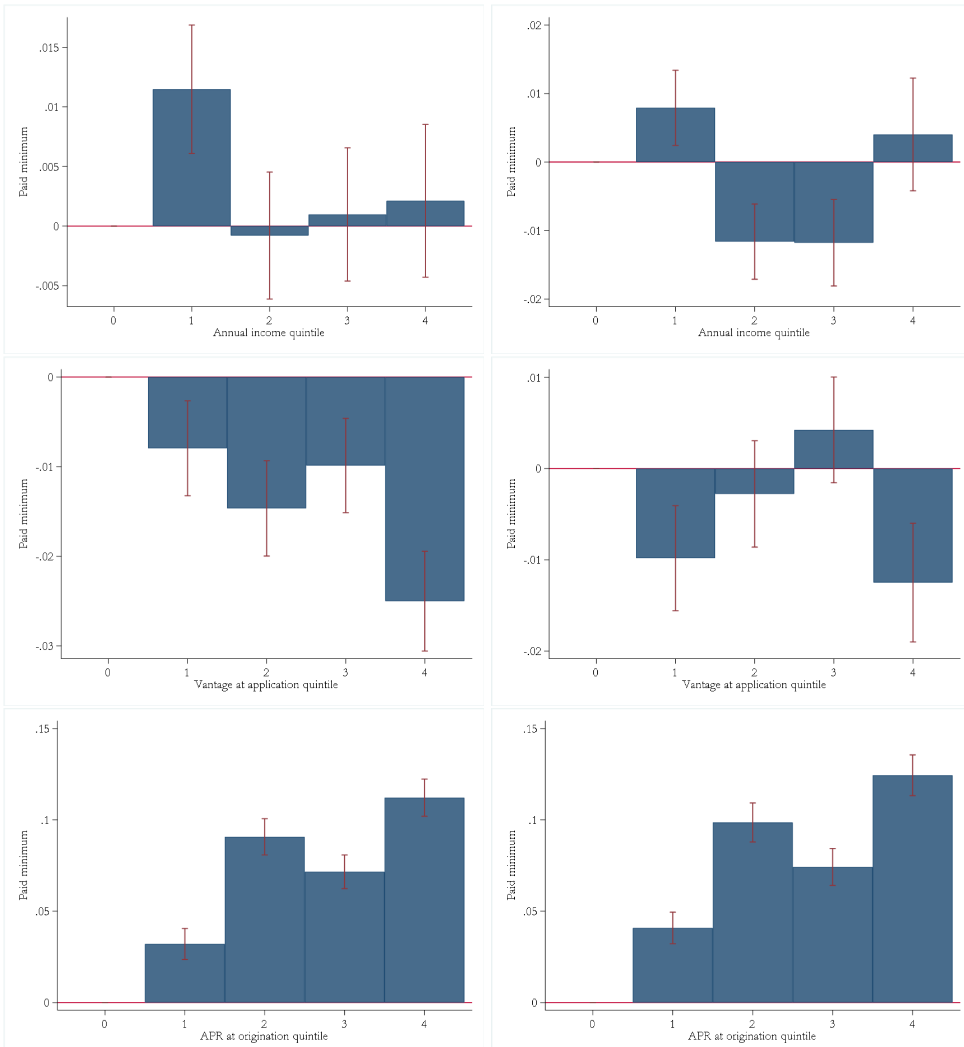
Notes: Graphs plot consumer and account characteristics by account origination date relative to the cutoffs for the second change in underwriting flow. Each graph plots the average of the covariate across accounts opened in that calendar week, within 10-week windows of the cutoff dates.

Figure A4: **Control Coefficients for First Stage**



Notes: Figures plot control coefficients for first stage regressions with linear specification. The left column plots these coefficients for the first underwriting change, and the right column plots them for the second underwriting change.

Figure A5: **Control Coefficients for IV - Minimum Payments**



Notes: Figures plot control coefficients for IV regressions with linear specification. The left column plots these coefficients for the first underwriting change, and the right column plots them for the second underwriting change.

Table A1: **Covariate Balance Tests Using Local Linear Regression With Different Bandwidths**

	(1)	(2)	(3)	(4)
	Income	Vantage	Credit limit	APR
	First Change: (Bandwidth 5 weeks)			
Sample Mean:	$45,645	661	$1,926	22%
Post	2413.1	1.889	95.1	0.001
	(1229)	(1.252)	(50.2)	(0.002)
	[0.050]	[0.131]	[0.058]	[0.363]
	Observations: 16,791, Chi-square test: 55.28, p-value: 0.000			
	First Change: (Bandwidth 10 weeks)			
Post	4515.8	1.768	128.7	0.011
	(886)	(0.910)	(36.1)	(0.001)
	[0.000]	[0.052]	[0.000]	[0.000]
	Observations: 31,160, Chi-square test: 902.07, p-value: 0.000			
	First Change: (Bandwidth 15 weeks)			
Post	7639	5.164	302.0	0.009
	(774)	(0.805)	(31.6)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]
	Observations: 38,193, Chi-square test: 2232.37, p-value: 0.000			
	Second Change: (Bandwidth 5 weeks)			
Sample Mean:	$46,063	666	$2,115	20%
Post	- 9401.2	- 5.702	- 562.1	- 0.001
	(551)	(0.644)	(22.1)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.409]
	Observations: 16,443, Chi-square test: 772.50, p-value: 0.000			
	Second Change: (Bandwidth 10 weeks)			
Post	- 10110.0	- 6.464	- 596.0	- 0.002
	(434)	(0.508)	(17.5)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.002]
	Observations: 27,727, Chi-square test: 1446.96, p-value: 0.000			

Notes: Table presents covariate balance tests for local linear regression with the indicated bandwidth. Sample is accounts originated within the given bandwidth of the cutoff dates. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A2: **First Stage and Reduced Form Estimates with Controls - First Underwriting Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Autopay	Chargeoff	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	36%	10%	9%	28%	38%	25%	38%	$266
				Panel A: Linear				
Post	- 0.182	0.065	0.029	- 0.058	0.037	- 0.008	- 0.009	- 2.601
	(0.004)	(0.008)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.724)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.024]	[0.005]	[0.582]
				Panel B: Quadratic				
Post	- 0.226	0.069	0.024	- 0.066	0.047	- 0.005	- 0.008	- 16.500
	(0.005)	(0.011)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(6.866)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.305]	[0.090]	[0.016]
				Panel C: Cubic				
Post	- 0.218	0.050	0.021	- 0.054	0.059	- 0.025	- 0.022	- 37.360
	(0.007)	(0.015)	(0.005)	(0.007)	(0.007)	(0.007)	(0.006)	(8.768)
	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
				Panel D: Quartic				
Post	- 0.214	0.032	0.017	- 0.043	0.064	- 0.038	- 0.042	- 58.680
	(0.008)	(0.018)	(0.006)	(0.008)	(0.009)	(0.008)	(0.008)	(10.750)
	[0.000]	[0.077]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
				Panel E: Local linear regression				
Post	- 0.185	0.065	0.029	- 0.059	0.038	- 0.008	- 0.009	- 3.641
	(0.004)	(0.008)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.681)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.024]	[0.004]	[0.437]

Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for first-stage and reduced-from outcomes around the first change in underwriting flow with the inclusion of controls for calendar month, state, and origination channel fixed effects; account age and account age squared; and non-parametric indicators for quintiles of vantage, income, and age at application, and current APR. Sample includes accounts originated within 10 weeks of the cutoff date.

Table A3: **First Stage and Reduced Form Estimates with Controls - Second Underwriting Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Autopay	Chargeoff	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	16%	12%	11%	19%	43%	27%	40%	$293
				Panel A: Linear				
Post	0.153	0.019	0.009	0.031	- 0.046	0.005	0.004	3.046
	(0.005)	(0.008)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(5.995)
	[0.000]	[0.015]	[0.013]	[0.000]	[0.000]	[0.328]	[0.396]	[0.611]
				Panel B: Quadratic				
Post	0.179	0.005	0.022	0.023	- 0.065	0.019	0.012	- 12.99
	(0.008)	(0.013)	(0.006)	(0.007)	(0.009)	(0.008)	(0.008)	(8.66)
	[0.000]	[0.695]	[0.000]	[0.002]	[0.000]	[0.018]	[0.112]	[0.134]
				Panel C: Cubic				
Post	0.160	0.019	0.019	0.019	- 0.072	0.034	0.031	- 13.50
	(0.010)	(0.018)	(0.008)	(0.010)	(0.012)	(0.011)	(0.010)	(10.96)
	[0.000]	[0.289]	[0.014]	[0.057]	[0.000]	[0.001]	[0.002]	[0.218]
				Panel D: Quartic				
Post	0.142	0.038	0.036	0.009	- 0.061	0.016	0.013	- 28.9
	(0.013)	(0.024)	(0.010)	(0.013)	(0.015)	(0.014)	(0.013)	(13.5)
	[0.000]	[0.108]	[0.000]	[0.489]	[0.000]	[0.234]	[0.317]	[0.032]
				Panel E: Local linear regression				
Post	0.153	0.018	0.010	0.031	- 0.048	0.007	0.005	2.700
	(0.005)	(0.008)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(5.904)
	[0.000]	[0.019]	[0.006]	[0.000]	[0.000]	[0.212]	[0.308]	[0.647]

Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for first-stage and reduced-from outcomes around the second change in underwriting flow with the inclusion of controls for calendar month, state, and origination channel fixed effects; account age and account age squared; and non-parametric indicators for quintiles of vantage, income, and age at application, and current APR. Sample includes accounts originated within 10 weeks of the cutoff dates.

Table A4: **IV Estimates with Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Chargeoff	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
				Panel A: First Change, Linear			
Pre-period mean:	10%	9%	28%	38%	25%	38%	$266
Autopay	- 0.164	- 0.159	0.319	- 0.203	0.043	0.051	14.300
	(0.012)	(0.013)	(0.019)	(0.021)	(0.019)	(0.018)	(25.95)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.023]	[0.005]	[0.582]
				Panel B: First Change, Quadratic			
Autopay	- 0.128	- 0.108	0.292	- 0.208	0.023	0.036	72.910
	(0.014)	(0.016)	(0.022)	(0.025)	(0.022)	(0.021)	(30.310)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.304]	[0.089]	[0.016]
				Panel C: Second Change, Linear			
Pre-period mean:	12%	11%	19%	43%	27%	40%	$293
Autopay	0.022	0.062	0.203	- 0.298	0.034	0.027	20.0
	(0.018)	(0.025)	(0.031)	(0.038)	(0.034)	(0.032)	(39.240)
	[0.208]	[0.014]	[0.000]	[0.000]	[0.326]	[0.394]	[0.611]
				Panel D: Second Change, Quadratic			
Autopay	0.022	0.125	0.129	- 0.361	0.107	0.068	- 72.8
	(0.026)	(0.034)	(0.041)	(0.050)	(0.045)	(0.042)	(48.7)
	[0.390]	[0.000]	[0.002]	[0.000]	[0.017]	[0.110]	[0.135]

Notes: Table shows parametric RD estimates for the effect of autopay enrollment on payment outcomes, instrumenting autopay enrollment with dummy variables for account origination dates following the two changes in underwriting. The regressions include controls for calendar month, state, and origination channel fixed effects; account age and account age squared; and non-parametric indicators for quintiles of vantage, income, and age at application, and current APR. Sample includes accounts originated within 10 weeks of the cutoff dates.