To Pay or Autopay? Fintech Innovation and Credit Card Payments*

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January 2022

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Abstract

Digital technologies 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. At the same time, existing technologies such as autopay have received little attention in the literature and face limited takeup despite being widely available in the market. 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 enrollment increases the likelihood of making the minimum payment by 25 to 43pp, more than doubling the baseline rate. Conditional on not charging off, autopay leads to lower average payment amounts. The results show that seemingly minor technological defaults can have economically large effects on consumer outcomes.

JEL codes: D12, D14, G21, G51
Keywords: fintech, consumer finance, credit cards, behavioral finance

*I am thankful to several employees at the anonymous data provider for sharing their time and expertise in making this project possible. Kelly Hyde, Daniel Grodzicki, and seminar and conference participants at the University of Illinois, the Data, Information and Welfare in Household Finance at Chicago Booth, RAND BeFi, and the OCC Symposium on the Implications of Financial Technology for Banking provided useful feedback. Peichen Li, Yucheng Zhou, Chenyue Yin, Senada Mankolli, and Kevin Tzeng provided outstanding research assistance. Contact: jialanw@illinois.edu

1 Introduction

Digital technologies 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. Moreover, some existing technologies such as autopay have also received little attention in the literature and face limited takeup in the United States.[1] As shown by Keys and Wang (2019), credit card payments exhibit a highly bimodal distribution around the minimum and full payments, with 35% of all payments clustered near the minimum and 33% at the full balance. The role of technology in contributing to consumer payment patterns, credit risk, and overall indebtedness remains an open question.

I examine the extent to which autopay and the other digital payment features of a credit card issued by a bank but serviced by a fintech credit card company affect consumer payment behavior. Using two sharp changes in the card issuer's underwriting practices, I estimate that the use of autopay dramatically increases the probability of making the minimum payment and decreases chargeoffs for the first underwriting change. Conditional on not charging off, autopay decreases full payments and average payment amounts, purchases, and balances. These estimates show that seemingly minor technological defaults can have economically large effects on credit risk and outcomes for both consumers and firms.

This paper is one of the first studies to my knowledge of the causal effects of autopay enrollment and technological payment features on credit card payments in the United States, and uses data from about 63,000 credit card accounts between 2018 and 2020. The cards are underwritten using a combination of cashflow metrics based on transactions from users' bank accounts and traditional credit metrics. Two major changes were made to the underwriting process during the sample period. Cashflow analysis requires consumers to link their bank accounts to the fintech app, reducing the frictions associated with autopay enrollment. Customers 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. The fintech company's payment interface also includes a payment slider tool, which is designed to de-anchor payment choices from the minimum payment and to calculate and display the interest costs associated with the continuum of payment choices between the minimum and full balance.

[1] Autopay is a feature that consumers can enable to allow credit card bill payments to be automatically deducted from their bank accounts. While this feature is available for all major U.S. credit cards during this study period, according to the Consumer Financial Protection Bureau (2019) , only 16% of active accounts for general purpose cards were enrolled in autopay in 2018.

Consistent with the role of small frictions in generating large effects on autopay enrollment, the first underwriting change significantly decreased the requirement to link bank accounts starting with accounts originated at a specific date, and generated a 19pp discontinuous decrease in autopay enrollment. The second underwriting change significantly increased the requirement to link bank accounts, and increased autopay enrollment by 12pp. 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.

Using parametric and non-parametric regression discontinuity designs around these two underwriting changes to instrument for autopay enrollment, I find that autopay has very large effects on credit risk for the first change. Based on instrumental variables (IV) estimates, going from 100 to 0 percent autopay enrollment would more than double the baseline chargeoff rate based on the complier population for the first underwriting change. In contrast, the effects on chargeoffs is not robust across specifications for the second underwriting change. Thus, in at least in one instance, the details of bank account linkage and payment settings have first-order effects on credit risk, which result in economically important impacts on credit losses and consumer credit scores and access. Moreover, the direction of the these effects depend on the details of implementation and characteristics of the complier population.

Autopay also significantly affects payment outcomes among accounts that do not charge off. Autopay typically has two settings - the minimum payment and the full statement balance. While enrollment in autopay significantly increases minimum payments by between 23 and 35pp, its effects on full payments are smaller and less consistent, with preferred estimates between negative 6pp and negative 18pp for non-chargeoff accounts. Overall, autopay decreases the fraction of balance paid by between 8 and 17pp, the average payment amount by $25 to $48, average monthly purchases by $73 to $179, and average balances by $66 to $334 for non-chargeoff accounts. Although rational models in the presence of attention costs could help explain the role of autopay in generating a bimodal payment distribution, the effect on average payment amounts suggests that technological defaults may affect long-run consumption and indebtedness.

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 consumer payment patterns and the predictions of rational models that trade off intertemporal consumption smoothing with interest costs. 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. Keys and Wang (2019) show that the 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 (2020) finds that while nudges help reduce late payments on credit cards in Brazil, they have the unintentional consequence of increasing overdraft fees. Closely related to this paper, Adams, Guttman-Kenney, Hayes, Hunt, Laibson and Stewart (2018) use a randomized field experiment to test for the effect of a choice architecture intervention that nudges consumers away from choosing autopay on the minimum payment. Consistent with this study, they find that autopay enrollment is associated with changes in delinquency, but unlike this study they find no effect on overall spending or balances.

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 2016; 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.

Finally, this paper relates to the literature on the role of technology in financial markets.[2] While some prior studies examine the potential for technology to help consumers improve their decisions (e.g. D'Acunto, Prabhala and Rossi 2019 and Carlin, Olafsson and Pagel 2019), I know of few other papers that examine the role of specific technological interfaces on consumer debt payments using data on transactions and app engagement. The rest of this paper is organized as follows. Section 2 describes the data and regression discontinuity design. Section 3 describes the first stage, reduced form, and instrumental variables results, and Section 4 concludes.

[2] Philippon (2016), Goldstein, Jiang and Karolyi (2019), and Thakor (2020) provide overviews of this literature.

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 several tens of thousands of 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 accepted widely 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 most traditional 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 directly from their linked bank account each month.

Panel A of Figure 1 shows the app interface for the autopay feature, which includes 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. The 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 a payment is withdrawn. The autopay setting can be changed at any time.

Whether or not autopay is enabled, users can make manual payments at any time using the 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 dollar amount, and calculates the difference between the chosen amount and the full statement balance that would allow them to avoid interest charges. It also calculates 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 payment changes 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. In addition, I have transaction-level information about each payment made toward their balance, including the payment method and exact time and date the payment is made, and information about each time they use the payment slider tool. 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 Vantage score at origination is 664. On average, 26% of customers are enrolled in autopay, and 21% of customers have used the payment slider at least once and 7% of them use it in a given month.

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.

[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.

2.2 Descriptive Statistics

Next, I present descriptive evidence of monthly payment behavior. First I classify each account-month into three mutually exclusive payment categories: manual, autopay, and slider. Slider payments are those made when slider tool is used on the same day as a payment is made in a given month. Autopay payments are those made when a customer is enrolled in autopay in that month, but did not use the slider on the same day a payment was made. The remaining account-months that are neither slider nor autopay payments are classified as manual. 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, all three distributions shows a highly bimodal distribution. About 40–50% of all payment months for all three payment methods represent less than 10% of the statement balance, while 25–40% are greater than or equal to the statement balance. However, the distribution for slider payments are significantly shifted toward the center compared to the other two payment methods, with more than 30% of all payments ranging between 10–99% of the balance. This provides suggestive evidence that the payment slider tool and interface that de-emphasizes the minimum payment relative to traditional credit card payment interfaces may help "de-anchor" payments from the minimum.

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 in between min plus \$50 and the full payment are defined as "intermediate." This view shows significant differences between the three payment methods. Delinquency rates vary widely, from 18% for manual payments to 2% for slider payments. Otherwise, manual payments are fairly evenly split between exact minimum, near minimum, intermediate, and full payments.

Consistent with only having two options available for the autopay setting, autopay payments are highly bimodal, with 69% 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 autopay payments are between the minimum and full balance. Slider payments are the most evenly split across non-delinquent payment amounts, with 20–27% of payments in each category, consistent with the intuition that the slider tool helps to de-emphasize the extremes.

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, it nonetheless provides 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. However, this could be due to the significantly 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, autopay payments are significantly more bimodal than other payment methods 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 for which accounts are signed up for autopay, and I do not know of any other studies on the use and effects of autopay for credit cards in the U.S. The summary statistics in KW include both autopay and non-autopay payments, and are pooled payments across different methods such as online, app-based, and mail (app-based payments were likely to be rare during the KW sample period). Among non-delinquent payments, 73% of autopay payments in the fintech data are either the minimum or full, compared with 52% in KW. This comparison provides some of the first evidence 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 similar to the general statistics in KW, with 52% 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). Among non-delinquent slider payments, 25% are equal to the minimum, 21% are near-minimum, 28% are intermediate, and 27% are full payments.

2.3 Regression Discontinuity Design and Conceptual Framework

As described above, the fintech company made two significant changes to its underwriting strategy during the sample period, which I will use to provide causal evidence of the effects of autopay and the digital payment app 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 this regression discontinuity design (RDD), the results can be interpreted as the causal effect of enrolling in autopay versus having to use the manual payment interface each month. Autopay settings can be changed or overridden, so all users still have the choice to pay any amount and any number of times each month. However, autopay enrollees will 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 the minimum in order to remain in good standing. Nonetheless, autopay enrollees may still fall delinquent or incur bank fees if they do not have enough funds in their bank accounts to cover the scheduled payment. For users not enrolled in autopay, payment decisions may be affected by various features of the digital payment interface shown in Figure 1.

The first underwriting change significantly 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 its bank transactions in order to assess borrower riskiness. 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 implemented for all accounts starting at a specific 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. It also shows the average fraction of accounts that use the slider tool. While cashflow underwriting is a one-time process for each account at the time of origination, the autopay and slider use measures are pooled across all active months for each account.

[4] Some initial versions of the analysis below are adapted from replication code from Deshpande (2016).

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. In blue + signs, slider use does not change discontinuously around the cutoff date, and remains around 10% of account months regardless of account origination 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. Again, slider use remained relatively consistent around the cutoffs. We will use both of these changes in underwriting flow, that resulted in both increases and decreases in autopay enrollment, throughout the analysis below.

2.4 Covariate Balance

I use the discontinuities in origination date to identify the effect of autopay enrollment on payment outcomes. The key identifying assumptions of the RD design are that assignment to the underwriting treatments are as good as random around the cutoff, and the outcome variables would be smooth across the cutoff if not for the changes in autopay enrollment, conditional on observables. The cutoff dates were not publicly announced, and were based on internal operations of the firm as they increased the sophistication of their underwriting system. Users were not aware of the cutoff date or the different processes before and after the cutoff, 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.

I use a parametric RD specification to test whether the origination date instrument predicts observable characteristics of customers and accounts around the cutoff:

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

where Y_i is a characteristic of account i, $Post_i$ is a dummy for accounts originated after the cutoff, 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, and only the sample before and after the full implementation of the second underwriting change are used.

Appendix Figures A1 and A2 show the average characteristics for accounts originated in each week within 10-week bandwidths around the cutoffs. The graphs in Figure A1 show that while account characteristics are evolving around the cutoff, only income shows a noticeable discontinuity. Income increases 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 F-test rejects covariate balance under both the linear and quadratic specifications, the estimated discontinuities are generally small relative to sample means. Under the quadratic specification, the estimated discontinuities are smaller than 10% of the mean for all variables. While income, credit limit, and APR were evolving significantly over this time period, the discontinuities at the cutoff are small or imperceptible, and the main IV results are robust across linear and quadratic specifications and the inclusion of controls for these characteristics.

Figure A2 and Panels C and D of Table 2 show tests for covariate balance for the second underwriting change. Inference is 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 between weeks 0 and 3 coincident with the underwriting change. Thus, the underwriting change seemed to result not only in changes in bank account linkage and autopay, but also these two underlying characteristics of originated customers and accounts. Nonetheless, RD estimates when controlling for these observable characteristics may still yield informative findings for the effects of autopay on payment behavior. Despite the significant changes in income and credit limit, the estimated discontinuities across other characteristics are extremely small – less than three percent of the mean for all other outcomes.

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 cutoff, F statistics are larger for and estimated discontinuities are 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.

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 that includes the app and slider tool. 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 the 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, and only the sample before and after the full implementation of the second underwriting change are used.[5] As shown in Figure 3, the first cutoff date is associated with a significant decline in the fraction

[5] Results are attenuated and significantly noisier when all accounts are included, and are available upon request.

of accounts required to undergo cashflow underwriting, and the second cutoff date is associated with a significant increase.[6] The first two columns of Tables 3 and 4 show the regression results from equation (3) for the two first stage outcomes, without the inclusion of control variables: autopay enrollment and slider use. Autopay and slider use are 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.

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 based on the linear and quadratic specifications and local linear regression both with and without controls. Estimates are unstable under the cubic and quartic 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

Figures 5 and 6 show reduced-form graphs for the key account outcomes, and columns (3) through (11) of Tables 3 and 4 present parametric RD estimates for several polynomial orders as well as nonparametric local linear regression estimates, excluding controls. The same columns in Appendix Tables A2 and A3 present the results with controls. The graphs in Figure 5 show clear discontinuities for chargeoff, delinquency, and minimum payments associated with the first underwriting change. Since other account characteristics evolve smoothly or change only slightly around the cutoff as described above, the graphs look similar after residualizing with respect to the control variables, and that version is omitted from the paper and available upon request.

Along with a decline in autopay enrollment, the first underwriting change is associated with discontin-

[6]Exact levels of the changes in cashflow underwriting requirements are shrouded to protect the proprietary information of the company. The discontinuous changes in cashflow underwriting are larger than the changes in autopay enrollment, suggesting that the ease of enrolling in autopay once bank accounts are already linked drives the autopay results.

uous increases in delinquency and chargeoff, and a decrease in minimum payments. While the discontinuities around other payment outcomes are less clear in the figures, the regression results show that the reduced-form declines in minimum payments are accompanied by declines in full payments, fraction of balance paid, and overall payment amount, and an increase in intermediate payment amounts. These results are highly robust across specifications and to the inclusion of controls.

They are also consistent with intuitive effects of autopay. By reducing the attention costs of making payments, autopay reduces delinquency. Interestingly, the 19 percentage point reduction in autopay enrollment leads to a very large 7 percentage point increase in chargeoffs, compared to a pre-period mean of 10 percentage points. This shows that autopay not only reduces short-term delinquencies, but can dramatically affect the rate of permanent chargeoff, generating economically significant consequences for both consumers and firms. Thus, seemingly minor changes in technological settings can have first-order effects in the context of online lending.

Columns (4) and (5) of Table 3 break down the delinquency outcomes into short-run and long-run delinquency. The outcome in column (4) is a cross-sectional indicator for whether an account has ever been between 1 and 30 days past due, excluding any accounts that were ever more than 30 days past due. Column (5) indicates accounts that were ever more than 30 days past due, which often eventually lead to chargeoff. The results show there is no effect on short-run delinquency, and that autopay only effects delinquency through permanent chargeoffs in this setting.

Turning to the panel outcomes in the rest of the table, column (6) shows that on a monthly basis, the increase in delinquency translates into a 3 percentage point increase in delinquency per month, relative to a 9% baseline rate. Based on the panel outcomes, another intuitive result is that the reduction in autopay causes declines in minimum and full payments, the two settings for the autopay tool. Thus, many users of the autopay tool stick to those settings and allow autopay to automatically deduct either the minimum or full payment from their accounts, resulting in the bimodal payment distributions shown above. However, if consumers optimize their payments to balance attention costs with interest costs and consumption smoothing motives, they should still deviate from the autopay settings occasionally, to either pay more or less than the autopay amount depending on their preferences or liquidity shocks. Thus under constrained optimization, autopay should not have a long-run effect on the average amount that users pay.

However, the results in columns (10) and (11) show that the reduction in autopay enrollment causes declines in the average fraction and amount of payments. Appendix Table A4 shows some additional outcomes, indicating that the reduction in autopay leads to an increase in round number payments (with dollar amounts equal to a multiple of $50), a decrease in the number of payments per month, and an increase in balances. Thus, a potential mechanism behind this effect is that autopay is associated with a higher fraction of full payments, which are approximately the maximum amount that can be paid each month. When autopay is set to the minimum, users are also more likely make more than one payment per month. Thus, on average they pay off more of their balance when autopay is enabled compared to when it is not enabled. The effect of autopay on monthly purchases is not robust across specifications, but the negative reduced-form effect on payments seems to lead to an increase in average balances.

A remaining question is the extent to which the first underwriting change decreased overall payments above are driven by charged-off accounts, that experience a period of nonpayment and potential balance accumulation before being closed. Appendix Tables A6 and A7 present the reduced-form results of the first underwriting change conditional on accounts that never charge off. Consistent with the results in Table 3, the first underwriting change has no effect on delinquency conditional on accounts that never charge off.

The rest of the tables show that the reduction in minimum payments, full payments, and overall payment amounts and payment frequency remain significant and of similar magnitude in the sample of non-chargeoff accounts, confirming that changes in autopay have persistent effects on overall payment amounts. However, the results in columns (9) and (10) suggest that purchases also decreased, leading to a small overall decline in balances in this sample.

Next, let's go back to the reduced form results for the second underwriting change, shown in Figure 6 (again, observations for accounts originated in between the two vertical lines, during the period when the underwriting change was being rolled out, are omitted from the regression analysis). Since the first stage on autopay enrollment is significantly smaller for the second underwriting change, the discontinuities in outcomes are less visible. In addition, the underwriting change is also associated with significant changes in underlying account characteristics such as income and credit limit, so for this underwriting change my preferred specification includes controls for consumer and account characteristics. The residualized outcomes controlling for the standard set of covariates are shown in Figure A3.

Consistent with the results from the first underwriting change, the graphs show a clear increase in minimum payments associated with the increase in autopay enrollment due to the second underwriting change.

Based on columns (7) through (11) of Table 4 and Appendix Table A3 show that minimum payments increase by 4 to 5 percentage points after the second change, and this result is very robust across the main specifications and to the inclusion of controls. The effects on total chargeoffs differ somewhat across the linear and quadratic specifications. However, both the linear and quadratic specifications indicate opposite effects on short-term versus long-term delinquency (1-30 versus 30 days past due). These results suggest that increases in autopay decrease short-run delinquency but increase long-run delinquency, leaving the overall delinquency rate somewhat ambiguous. Full payments and the overall fraction paid decrease by about 3 percentage point, and overall payment amount decreases by about \$5 to \$40 relative to a baseline of \$293 per month in the specification with controls (the estimates are between -\$58 and -\$82 without controls). The additional outcomes shown in Table A5 show that round number payments, the number of payments per month, purchases, and balances also declined. These main results are very similar in the subsample conditional on no chargeoffs, showing that the changes in payment behavior are consistent regardless of whether accounts eventually charge off (Tables A8 and A9).

3.3 Instrumental Variables Estimates

To better interpret the magnitude of the results, Table 5 presents results of the causal effect of autopay enrollment on payment outcomes, using the two underwriting changes to instrument for autopay. Appendix Table A10 presents these results with the inclusion of controls. The coefficients can be interpreted as the causal estimate of a change from 0% to 100% autopay enrollment.

Columns (1) through (4) show that based on the first underwriting change, a 100% increase in autopay enrollment would reduce chargeoffs by more than double the average rate, showing that digital payment features have an enormous effect on credit risk. These effects are mostly concentrated in long-term delinquency, with accounts that did not link their bank accounts and enroll in autopay at origination becoming delinquent almost immediately, and then eventually charging off. In contrast, the second underwriting change is associated with an ambiguous effect on overall chargeoff rates, with a negative effect on short-term delinquency and positive effect on long-term delinquency.

Figures 7 and 8 show graphs of the IV estimates for key account outcomes conditional on account age, to illustrate how these outcomes evolve over time (Appendix Figure A4 shows the results for the second change with controls). The top left graph of each figure shows the evolution of the effect of autopay on delinquencies. For the first underwriting change, delinquencies are the highest initially, and then decrease over time until they reach 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 delinquencies cured, autopay caused a 13 to 19 percent of reduction in permanent chargeoffs.

The delinquency dynamics differed under the second underwriting change, which increased autopay enrollment. While both changes reduced short-term delinquency, this effect was much larger for the second underwriting change, and the second change also caused an increase in long-term delinquency and chargeoffs, which is the opposite effect of the first change. As shown in the top left graph of Figure 8, autopay increases delinquencies starting at origination, but this spikes up at four months after origation, and returns to baseline by 6-8 months after origination. This suggests that there may be heterogeneous responses based on the average characteristics of accounts originated around these two changes.

For the second treated group, autopay seems to cause an accumulation of delinquency that builds up during the initial months after origination, and becomes unaffordable by month 4. This could occur, for example, if users set their autopay to the full account balance, but do not have the funds to make this payment in their bank accounts, resulting in returned payments and potential negative outcomes. This dynamic also illustrates the ambiguous benefits of autopay, and some of the anecdotal reasons for relatively low takeup in the United States where many consumers hold low levels of liquid assets.

Column (5) shows that autopay leads to a very large 23 to 43 percentage point increase in minimum payments, which is very consistent across both underwriting changes and across specifications. It lends credence to the strong role of default options on payment choices. It is consistent with estimates from Keys and Wang (2019) 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, and shows that autopay settings may be one factor in this anchoring effect and the bimodal distribution of credit card payments in the population.

Interestingly, the results are more varied for the other extreme of full payments, which is the second autopay setting. While the the first underwriting change is associated with a 2 to 13 percentage point effect of autopay on increasing full payments, the second underwriting change is associated with a 0 to 31 percent decrease in full payments. The preferred linear specification with controls yields a comparison of positive 4pp for the first change and negative 20pp for the second change. Again, this could represent a heterogeneous treatment effect based on underlying characteristics of the two treated groups, which I will explore in future drafts. For example, if the second treated sample has higher underlying credit risk and lower ability to pay than the first one, they may be more likely to set their autopay settings to the minimum. Being unable to afford full payments could even lead to delinquency and negative effects of autopay in this case. I will explore whether consumers set autopay to the minimum or full payment and the detailed dynamics of payments in future revisions.

Abstracting from accounts likely to be delinquent or charge off, Appendix Tables A11 and A12 shows the IV results for the subsample of accounts conditional on never charging off. Focusing on the results that include controls in Appendix Table A12, the results are much more consistent across the two underwriting changes when conditioning on non-chargeoff accounts. The effects of autopay on minimum payments are even more consistent in magnitude – between 23 and 35pp across all specifications – and most specifications show a negative but somewhat noisily estimated effect on full payments.

Turning to the effect of autopay on the overall amount of payments, the results again vary depending on whether chargeoffs are taken into account. By reducing chargeoffs, autopay leads to an increase in the average fraction and dollar amount paid based on the first underwriting change, and the opposite for the second underwriting change. In contrast, the effects of autopay on total payment amounts are negative under most specifications for both underwriting changes when conditioning on non-chargedoff accounts. These results suggest that technological settings such as may have contrasting effects on different parts of the credit risk distribution, resulting in tradeoffs for the optimal settings for both firms and consumers.

The dynamics of also vary across different parts of the payment distribution. While the effect of autopay on minimum payments remains stable and persistent over the first 10 months since origination, other payment dynamics change. Full payments converge to the baseline level over time for both underwriting changes, suggesting that when consumers set autopay to the minimum payment, they are likely to leave it there. While when they set autopay to the full payment, which may not be affordable every month, they change their settings over time or over-ride the autopay in months when they cannot pay the full balance.

As more aggregate and long-lasting measures of the effects of autopay on consumer outcomes, average purchases and balances are lower when users enroll in autopay, consistently across both underwriting changes and across specifications, both in the samples including and excluding charged-off accounts. The dynamic graphs show that these effects are usually most pronounced in the first few months after account opening, so consumers enrolled in autopay may reduce their purchases when they know a payment will be deducted from their bank accounts. The persistence, heterogeneity, and implications of these effects will be explored in future drafts.

4 Conclusion

This paper is one of the first studies of the causal effects of autopay enrollment and technological payment features on credit card payments in the United States. Using two changes in the underwriting models of a fintech company that induced significant changes in autopay enrollment rates, I show that autopay has very large effects on both credit risk and payment behavior. Autopay can either significantly increase or decrease chargeoffs, depending on the underlying characteristics of the consumer population.

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. Autopay typically has two settings - the minimum payment and the full statement balance. While enrollment in autopay significantly increases minimum payments by between 23 and 35pp, its effects on full payments are smaller and less consistent, with preferred estimates between negative 6pp and negative 18pp for non-chargeoff accounts. Overall, autopay decreases the fraction of balance paid by between 8 and 17pp, the average payment amount by $25 to $48, average monthly purchases by $73 to $179, and average balances by $66 to $334 for non-chargeoff accounts. These estimates show that seemingly minor technological settings can have economically large effects on credit risk and outcomes for both consumers and firms.

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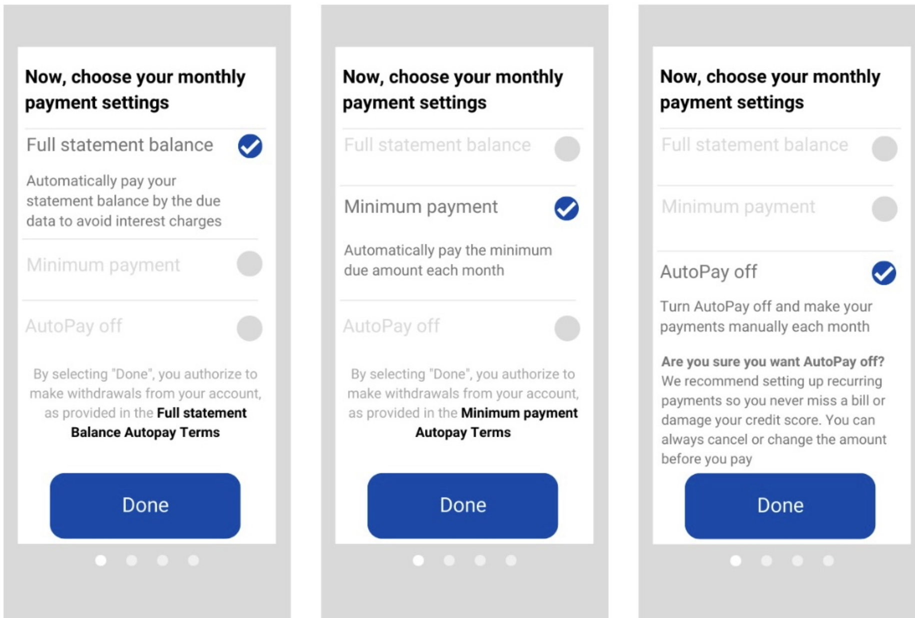
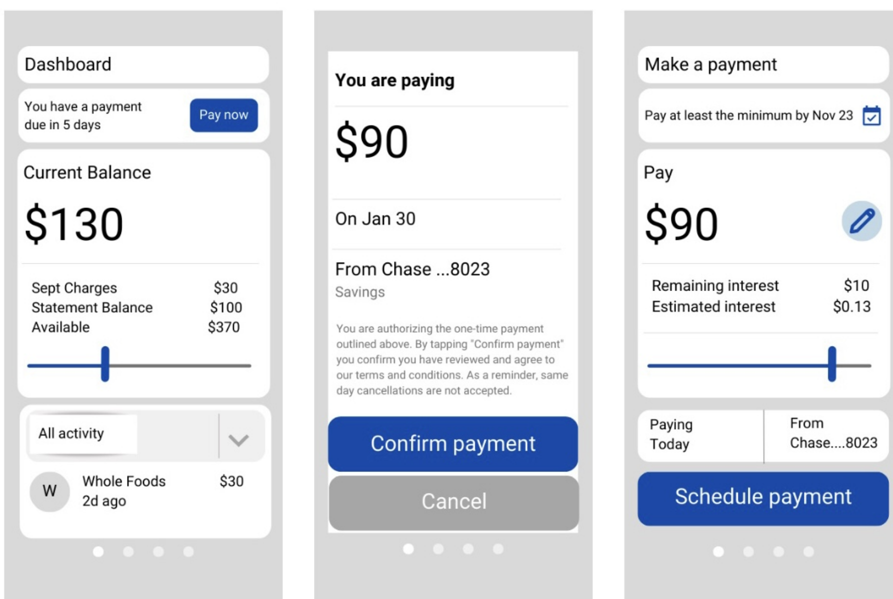
Figure 1: **Screenshots of Autopay and Manual Interface**

Panel A: Autopay



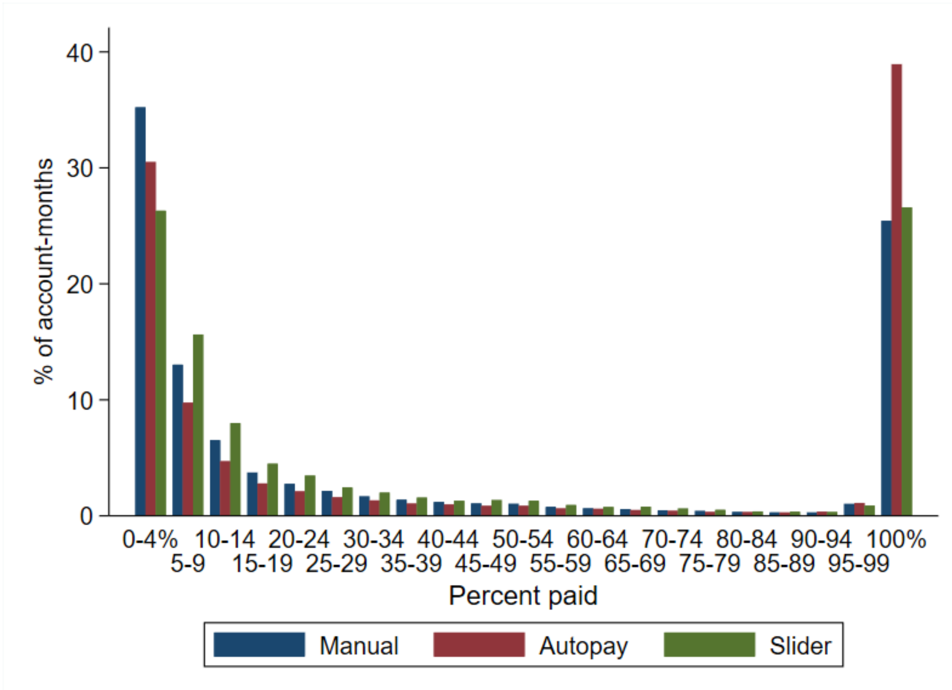
Panel B: Manual



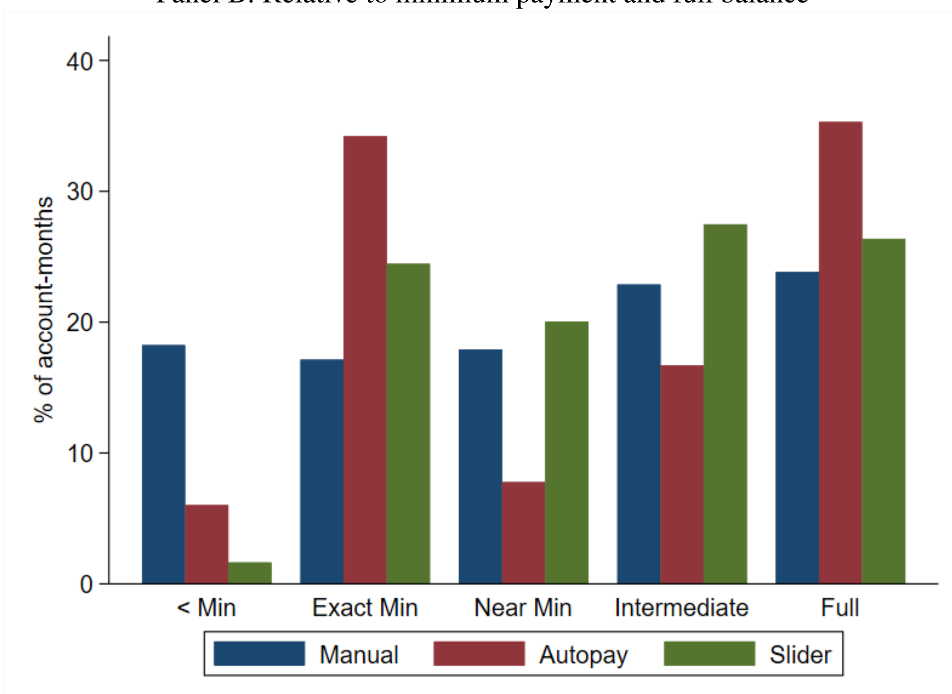
Notes: Example screenshots of autopay and manual payment interfaces. While the substantive content and layout are similar to what real customers would have seen during the sample period, some graphical elements have been modified to protect the anonymity of the data provider.

Figure 2: **Payment Distributions for Manual, Autopay, and Slider Use**

Panel A: Fraction of balance



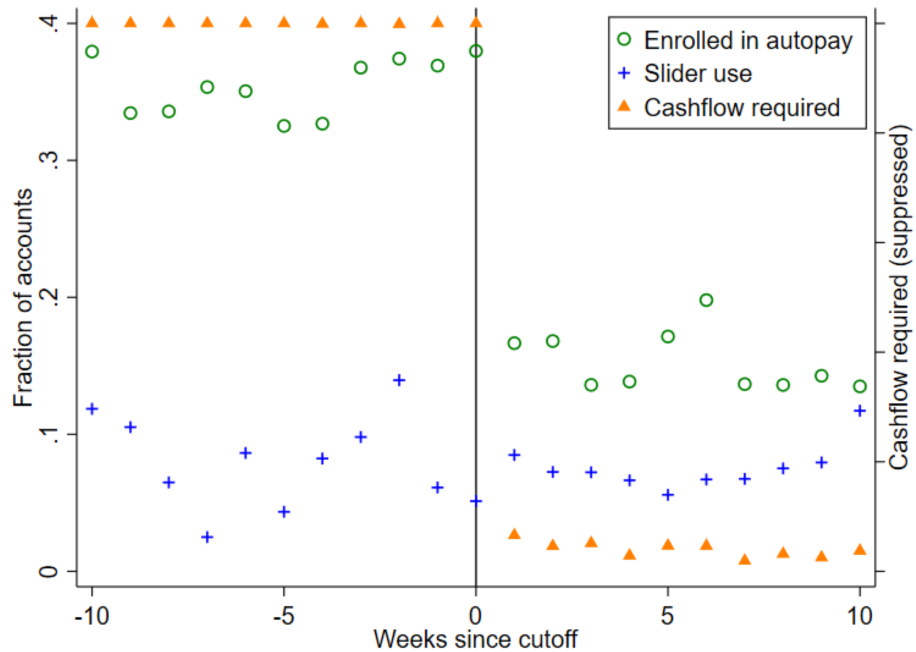
Panel B: Relative to minimum payment and full balance



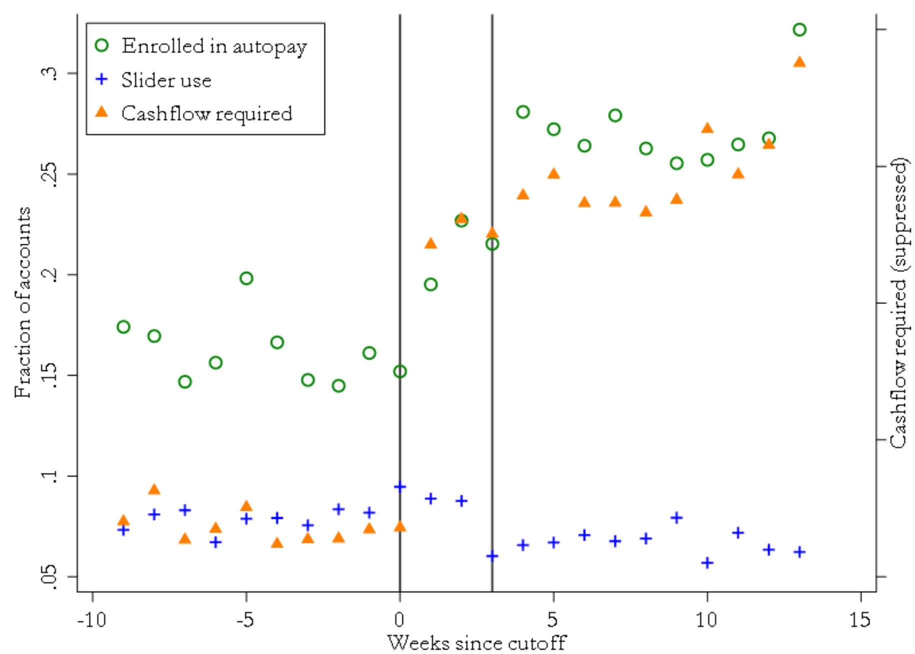
Notes: Figure shows distributions for monthly payment activity for three mutually-exclusive payment methods: autopay, manual, and slider. Slider users are those who used the payment slider tool on the same day as making a payment in that month. Autopay indicates users who were enrolled in autopay in that month, excluding slider users (these users still have the option to manually over-ride the autopay setting without using the slider). Manual users are those who neither used the slider on the payment date nor were enrolled in autopay.

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

Panel A: First Underwriting Change



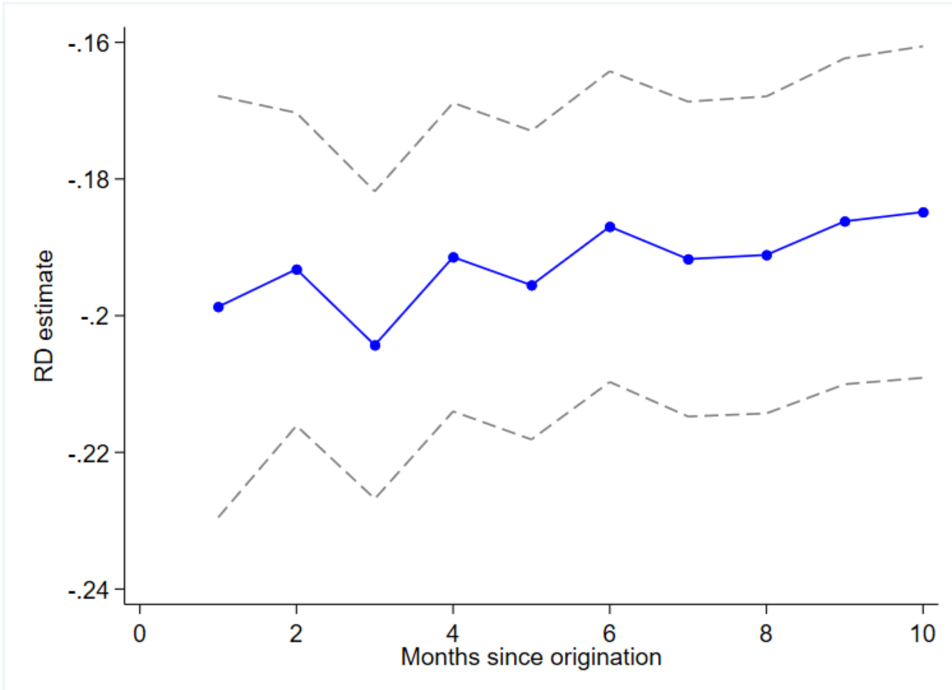
Panel B: Second Underwriting Change



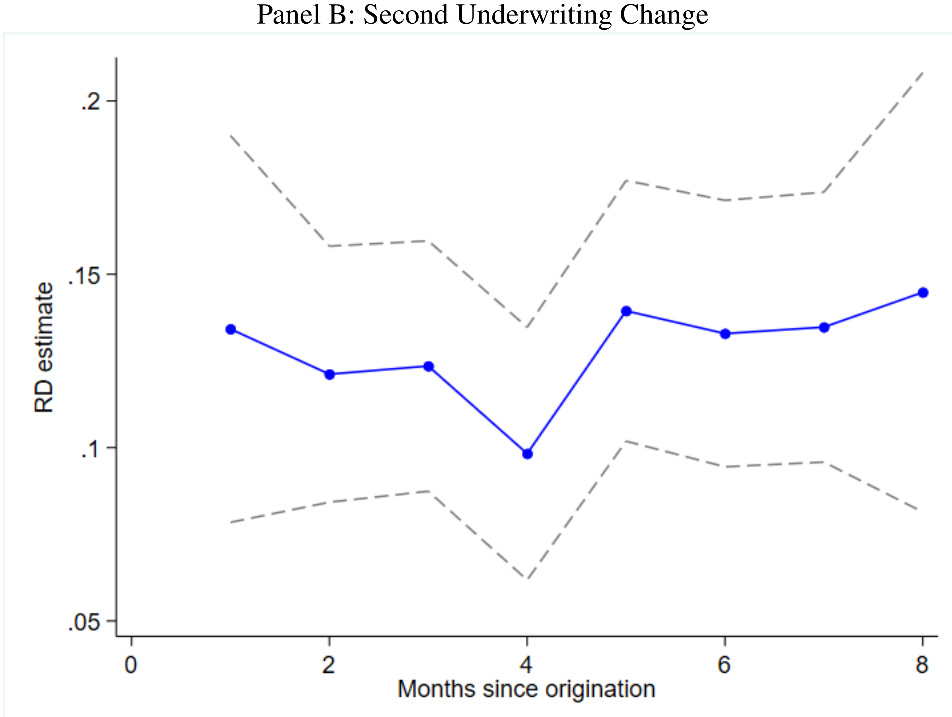
Notes: Figure plots proportion of accounts originated in each calendar week that were required to undergo cashflow underwriting, are enrolled in autopay, and that use the slider tool. Cashflow underwriting is a one-time measure at the time of origination, while enrollment in autopay and slider use are pooled across all observations for each account. The y-axis for cashflow underwriting is suppressed to protect the proprietary information of the company. 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

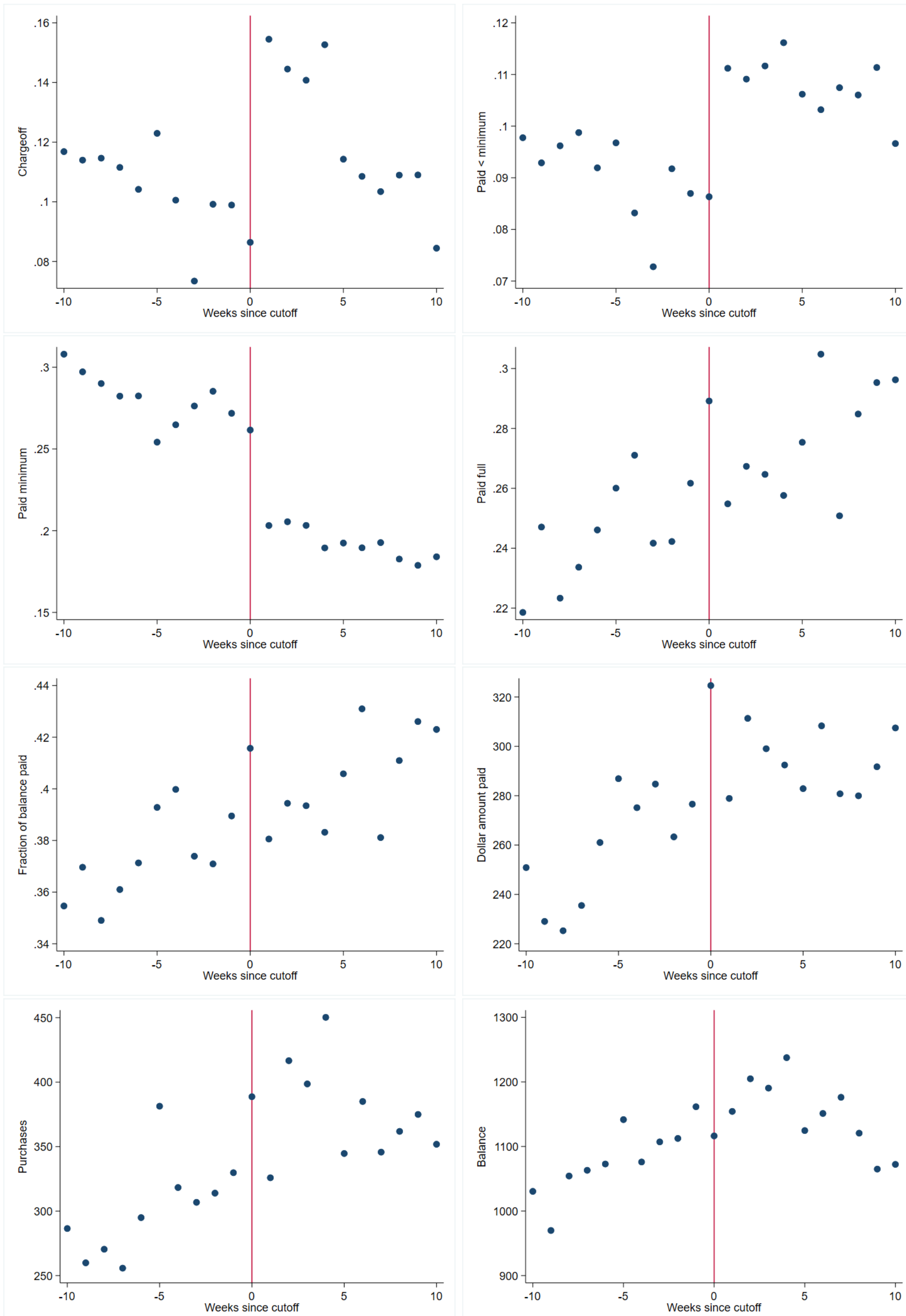


Panel B: Second Underwriting Change



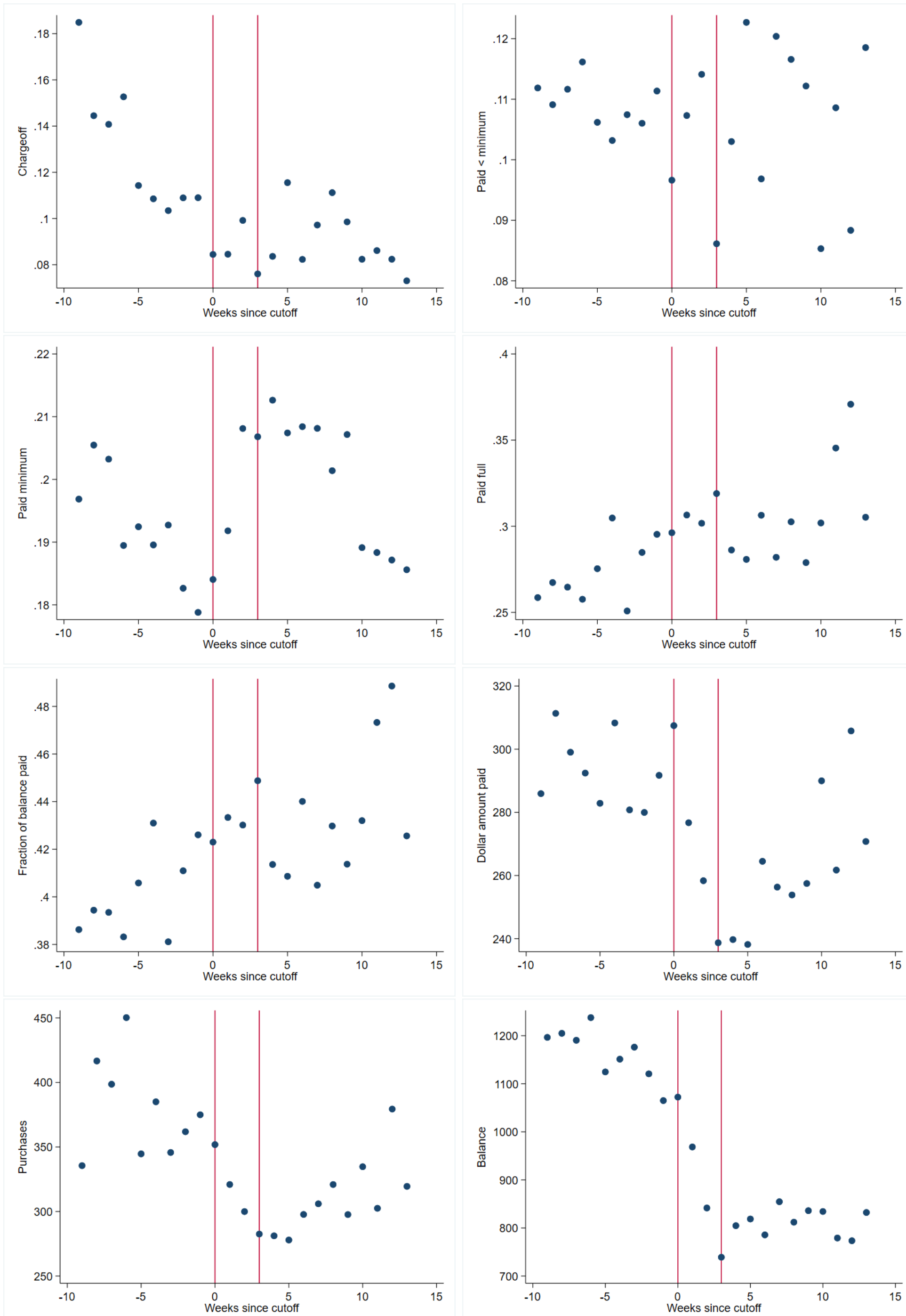
Notes: Figures plot parametric RD estimates of the effects of cashflow underwriting on autopay enrollment around the two changes in underwriting, using a polynomial of order 1 with no covariates. Dashed lines represent 95% confidence intervals. Sample includes accounts originated within 10 weeks of the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

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



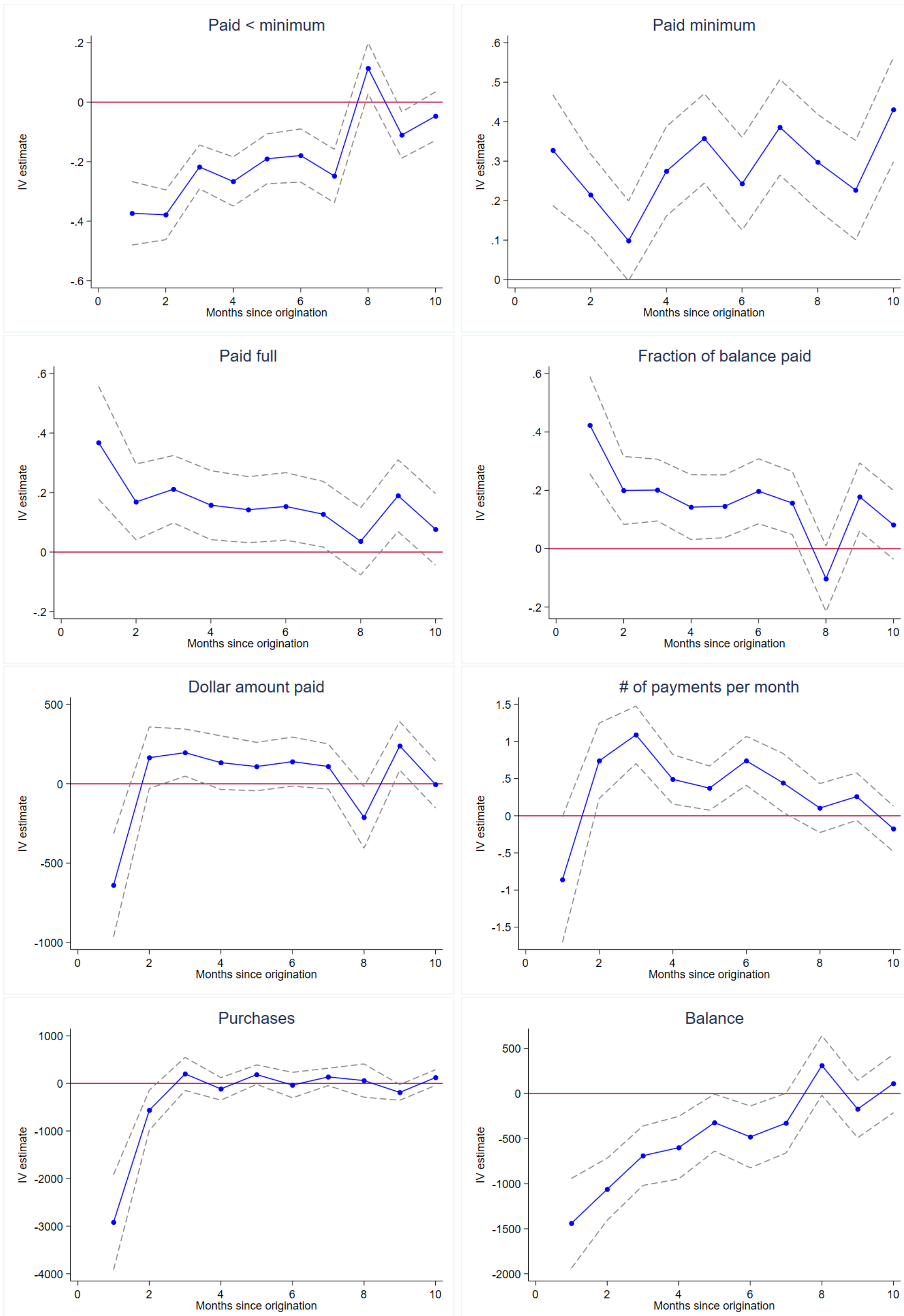
Notes: Figures plot average account outcomes by origination week for accounts originated within 10 weeks of the first change in underwriting flow. Chargeoff is a one-time outcome per account, and the other outcomes are pooled across all observations for each account.

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



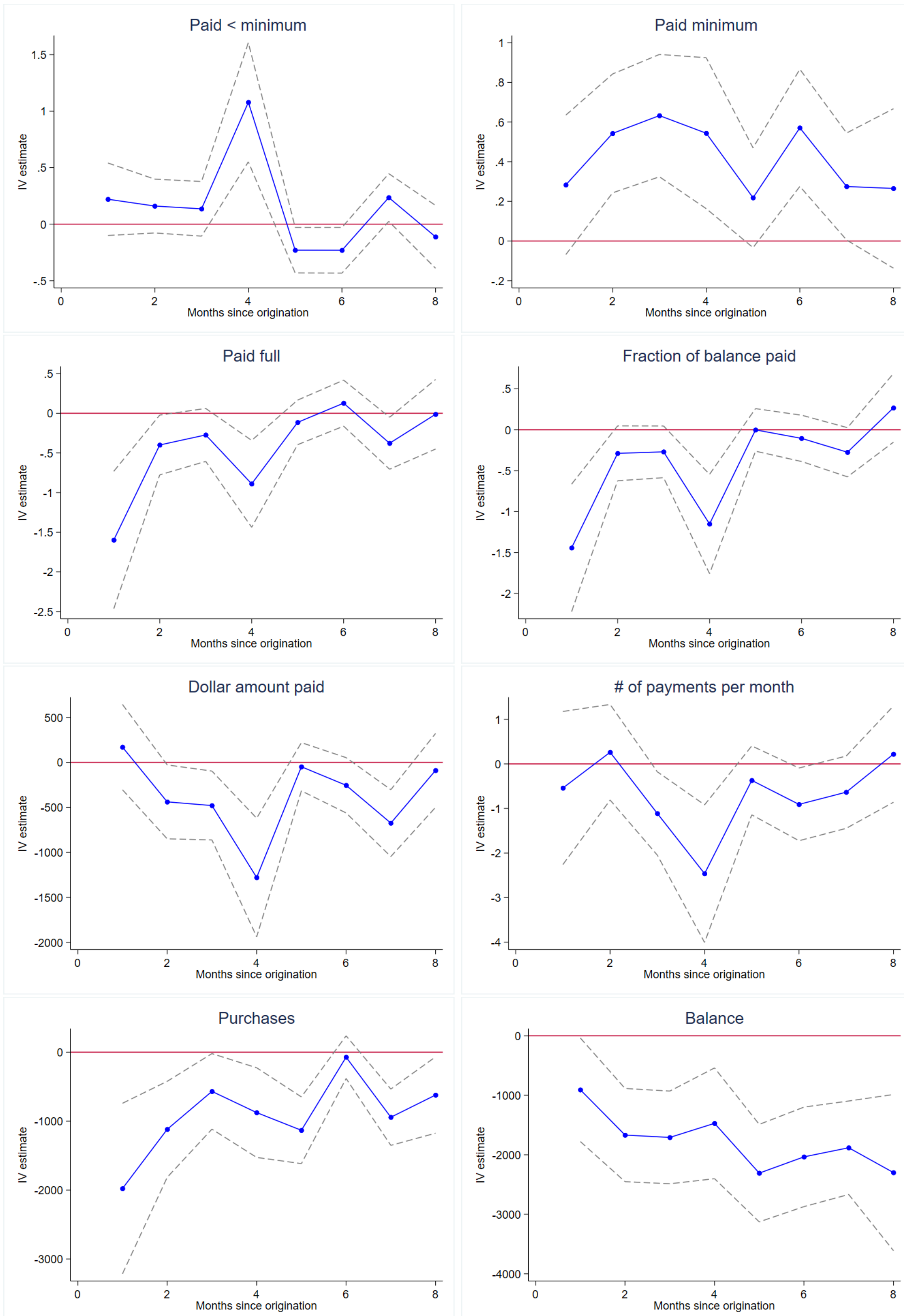
Notes: Figures plot average account outcomes by origination week for accounts originated within 10 weeks of the second change in underwriting flow. Chargeoff is a one-time outcome per account, and the other outcomes are pooled across all observations for each account.

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 dates.

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.
Income	$44,363	$35,000	$36,321
Vantage at application	664	658	39
Enrolled in autopay	26%		
Slider use:			
Any use	21%		
Avg use per month	7%	0%	18%
Avg # times per month	0.25	0.00	1.05
Conditional on > 0	3.57	2.00	5.18
Panel B: Monthly data			
	Mean	Median	Std. Dev.
Card and account			
Enrolled in autopay	27%		
Credit limit	$1,839	$1,500	$1,425
Retail APR	21%	22%	5%
Purchases and balances			
Utilization	60%	69%	36%
Balance	$1,075	$737	$1,364
Interest charged	$15	$9	$22
Purchase volume	$384	$126	$1,080
Purchase volume > 0	78%		
Payment and delinquency			
Fraction paid	39%	13%	43%
Minimum payment	$169	$27	$943
Actual payment	$259	$94	$542
Payment:			
< Minimum	14%		
Minimum	22%		
Intermediate	36%		
Full	27%		

Notes: Panel A shows account-level summary statistics, based on characteristics at origination and average autopay enrollment and slider use across all observations for each account. Panel B shows summary statistics based on the monthly panel of account activity.

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	4849	1.847	135.5	0.012
	(882)	(0.910)	(36.0)	(0.001)
	[0.000]	[0.042]	[0.000]	[0.000]
Percent of mean	0.11	0.00	0.07	0.05
	Observations: 31,165, Joint F-test: 2878.2, p-value: 0.000			
	Panel B: First Change, Quadratic			
Post	- 689	0.164	6.128	- 0.003
	(1335)	(1.347)	(53.9)	(0.002)
	[0.606]	[0.903]	[0.909]	[0.047]
Percent of mean	- 0.02	0.00	0.00	- 0.01
	Observations: 31,165, Joint F-test: 331.44, p-value: 0.000			
	Panel C: Second Change, Linear			
Sample Mean:	$46,063	666	$2,115	20%
Post	- 10376	- 1.961	- 584.9	- 0.001
	(1249)	(1.514)	(49.1)	(0.002)
	[0.000]	[0.195]	[0.000]	[0.686]
Percent of mean	- 0.23	0.00	- 0.28	0.00
	Observations: 26,424, Joint F-test: 184.34, p-value: 0.000			
	Panel D: Second Change, Quadratic			
Post	- 11069	3.303	- 749.5	- 0.001
	(3770)	(4.664)	(147.5)	(0.006)
	[0.003]	[0.479]	[0.000]	[0.896]
Percent of mean	- 0.24	0.00	- 0.35	0.00
	Observations: 26,424, Joint F-test: 38.70, 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 linear 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)	(9)	(10)	(11)
	First stage variables		Cross-sectional outcomes					Panel outcomes			
	Autopay	Slider	Chargeoff	Max 1-30dpd	Max 30+ dpd	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	36%	20%	10%	10%	17%	9%	28%	38%	25%	38%	$266
						Panel A: Linear					
Post	- 0.191	0.096	0.074	- 0.001	0.068	0.033	- 0.053	0.045	- 0.025	- 0.026	- 13.740
	(0.003)	(0.013)	(0.007)	(0.007)	(0.009)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.722)
	[0.000]	[0.000]	[0.000]	[0.926]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.004]
						Panel B: Quadratic					
Post	- 0.226	0.100	0.071	0.000	0.063	0.027	- 0.064	0.056	- 0.019	- 0.022	- 20.720
	(0.005)	(0.019)	(0.011)	(0.010)	(0.013)	(0.003)	(0.005)	(0.006)	(0.005)	(0.005)	(6.944)
	[0.000]	[0.000]	[0.000]	[0.994]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]
						Panel C: Cubic					
Post	- 0.213	0.121	0.053	0.000	0.046	0.020	- 0.051	0.068	- 0.037	- 0.034	- 37.410
	(0.007)	(0.024)	(0.014)	(0.014)	(0.017)	(0.004)	(0.006)	(0.007)	(0.007)	(0.006)	(8.921)
	[0.000]	[0.000]	[0.000]	[0.984]	[0.007]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
						Panel D: Quartic					
Post	- 0.208	0.150	0.029	0.000	0.025	0.016	- 0.032	0.081	- 0.065	- 0.069	- 84.380
	(0.008)	(0.030)	(0.017)	(0.017)	(0.021)	(0.006)	(0.008)	(0.009)	(0.008)	(0.008)	(10.900)
	[0.000]	[0.000]	[0.090]	[0.984]	[0.230]	[0.005]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
						Panel E: Local linear regression					
Post	- 0.194	0.096	0.073	- 0.001	0.068	0.032	- 0.054	0.046	- 0.024	- 0.026	- 14.230
	(0.003)	(0.013)	(0.007)	(0.007)	(0.009)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.701)
	[0.000]	[0.000]	[0.000]	[0.928]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]

Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for first-stage and reduced-from outcomes around the first change in undewrwriting 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)	(9)	(10)	(11)
	First stage variables		Cross-sectional outcomes					Panel outcomes			
	Autopay	Slider	Chargeoff	Max 1-30dpd	Max 30+ dpd	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	16%	8%	12%	9%	18%	11%	19%	43%	27%	40%	$293
Panel A: Linear											
Post	0.124	- 0.023	0.032	- 0.027	0.056	0.018	0.053	- 0.032	- 0.039	- 0.036	- 58.40
	(0.007)	(0.034)	(0.012)	(0.011)	(0.015)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(7.44)
	[0.000]	[0.504]	[0.006]	[0.014]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Quadratic											
Post	0.179	- 0.029	- 0.019	- 0.070	0.036	0.041	0.044	- 0.058	- 0.028	- 0.051	- 81.99
	(0.022)	(0.111)	(0.037)	(0.034)	(0.047)	(0.016)	(0.020)	(0.025)	(0.023)	(0.022)	(23.04)
	[0.000]	[0.796]	[0.603]	[0.036]	[0.451]	[0.011]	[0.031]	[0.020]	[0.220]	[0.018]	[0.000]
Panel C: Cubic											
Post	0.059	- 0.163	0.063	- 0.053	0.041	- 0.010	0.002	- 0.132	0.140	0.141	- 46.57
	(0.076)	(0.345)	(0.123)	(0.114)	(0.159)	(0.054)	(0.068)	(0.082)	(0.077)	(0.073)	(77.69)
	[0.435]	[0.637]	[0.610]	[0.644]	[0.798]	[0.853]	[0.974]	[0.108]	[0.069]	[0.052]	[0.549]
Panel D: Quartic											
Post	0.278	1.795	0.470	0.177	0.881	0.812	0.237	0.332	- 1.381	- 1.458	- 794.1
	(0.252)	(1.047)	(0.398)	(0.373)	(0.520)	(0.176)	(0.226)	(0.270)	(0.255)	(0.239)	(256.0)
	[0.270]	[0.087]	[0.238]	[0.634]	[0.091]	[0.000]	[0.294]	[0.220]	[0.000]	[0.000]	[0.002]
Panel E: Local linear regression											
Post	0.125	- 0.023	0.031	- 0.027	0.055	0.019	0.053	- 0.033	- 0.039	- 0.036	- 59.13
	(0.007)	(0.034)	(0.012)	(0.011)	(0.015)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(7.433)
	[0.000]	[0.500]	[0.008]	[0.011]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

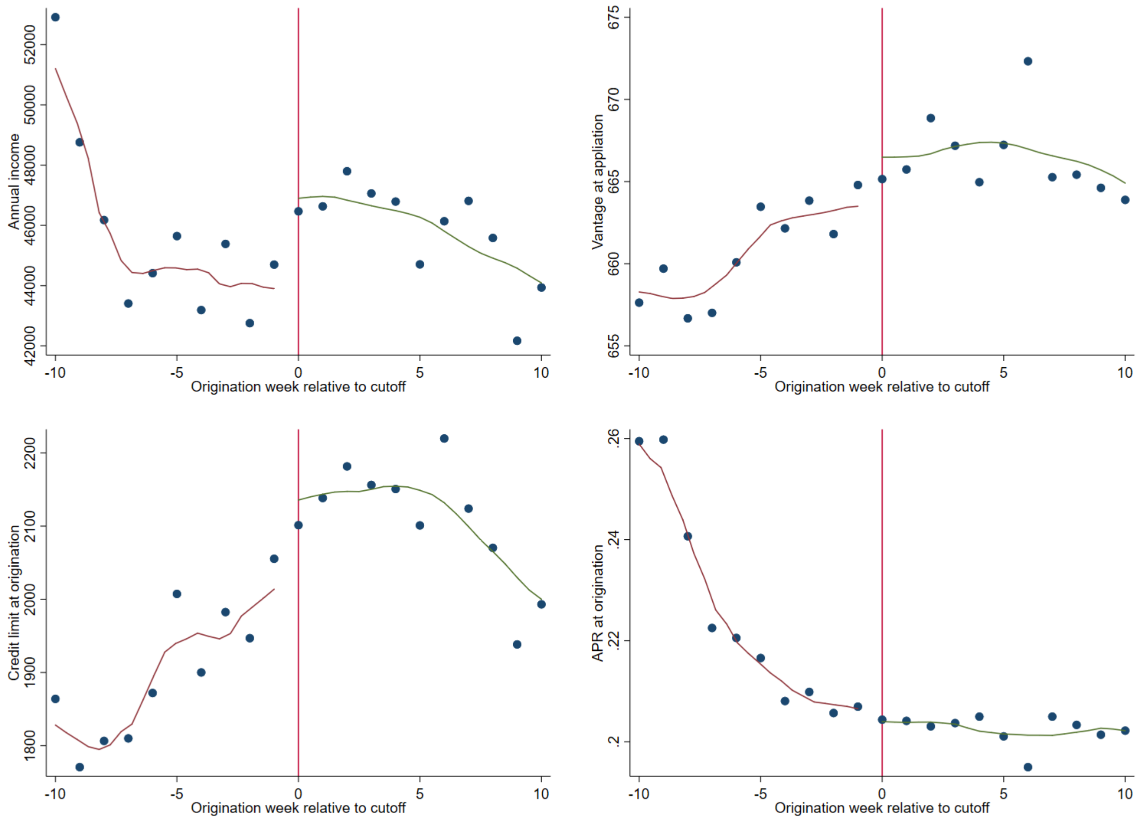
Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for first-stage and reduced-from outcomes around the second change in undewrwriting flow. Sample includes accounts originated within 10 weeks of the cutoff dates.

Table 5: **Instrumental Variables Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Cross-sectional outcomes							Panel outcomes					
	Chargeoff	Max 1-30dpd	Max 30+ dpd	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
	Panel A: First Change, Linear												
Pre-period mean:	10%	10%	17%	9%	28%	38%	25%	38%	$266	16%	1.37	$312	$1,086
Autopay	- 0.189	- 0.017	- 0.172	- 0.170	0.278	- 0.237	0.128	0.137	71.80	- 0.125	0.349	- 167.3	- 381.2
	(0.011)	(0.013)	(0.015)	(0.012)	(0.018)	(0.020)	(0.018)	(0.017)	(24.63)	(0.016)	(0.057)	(44.15)	(49.73)
	[0.000]	[0.179]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.004]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel B: First Change, Quadratic												
Autopay	- 0.127	- 0.024	- 0.085	- 0.118	0.283	- 0.247	0.083	0.096	91.89	- 0.141	0.295	46.56	- 132.1
	(0.014)	(0.017)	(0.018)	(0.015)	(0.022)	(0.025)	(0.023)	(0.022)	(30.75)	(0.020)	(0.075)	(49.55)	(61.58)
	[0.000]	[0.152]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]	[0.000]	[0.000]	[0.347]	[0.032]
	Panel C: Second Change, Linear												
Pre-period mean:	12%	9%	18%	11%	19%	43%	27%	40%	$293	20%	1.38	$382	$1,163
Autopay	0.165	- 0.236	0.408	0.148	0.426	- 0.261	- 0.313	- 0.289	- 470.2	- 0.249	- 0.727	- 693.7	- 1907
	(0.032)	(0.038)	(0.053)	(0.042)	(0.054)	(0.062)	(0.064)	(0.060)	(67.79)	0.049	0.176	(90.32)	(153.6)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel D: Second Change, Quadratic												
Autopay	0.023	- 0.431	0.365	0.232	0.247	- 0.321	- 0.157	- 0.286	- 458.6	- 0.359	- 0.790	- 636.1	- 1303
	(0.067)	(0.094)	(0.116)	(0.099)	(0.114)	(0.137)	(0.133)	(0.131)	(146.4)	(0.110)	(0.384)	(187.6)	(284.5)
	[0.732]	[0.000]	[0.002]	[0.019]	[0.030]	[0.019]	[0.235]	[0.029]	[0.002]	[0.001]	[0.040]	[0.001]	[0.000]

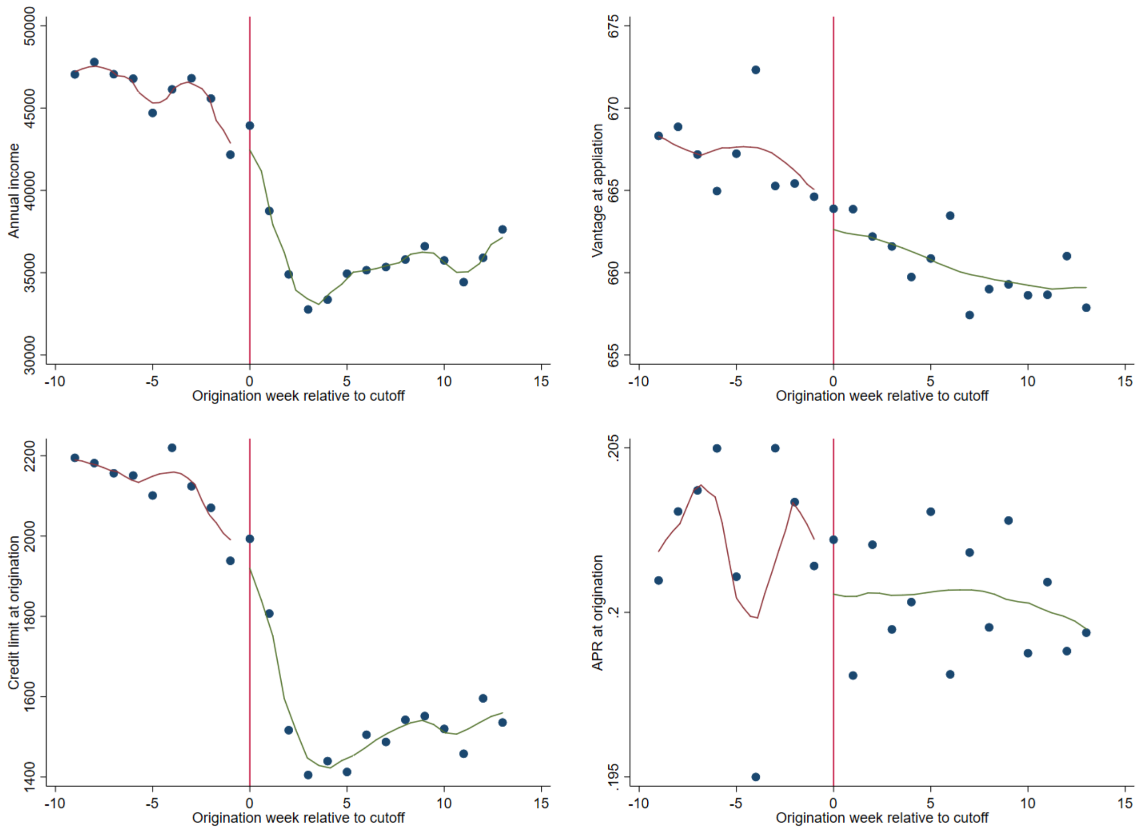
Notes: Table shows parametric RD and nonparametric local linear regression (LLR) estimates for the effect of autopay enrollment on payment outcomes, instrumenting autopay enrollment with dummy variables for account origination dates following two changes in underwriting. Sample includes accounts originated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

Figure A1: **Covariate Balance - First Underwriting Change**



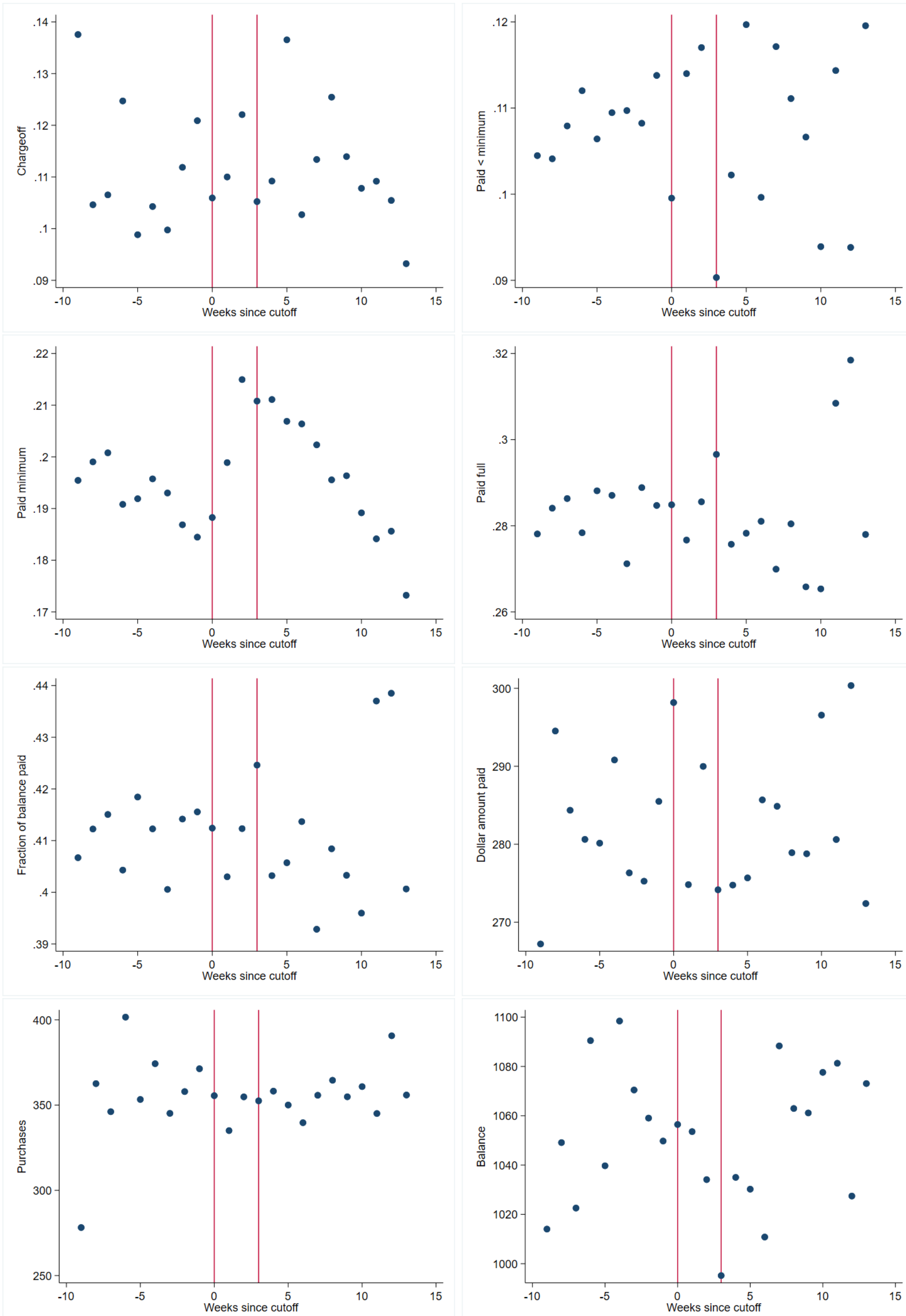
Notes: Graphs plot consumer and account characteristics by account origination date relative to the cutoff for the first change in underwriting flow. Each graph plots the average of the covariate across accounts opened in that calendar week, within a 10-week window of the cutoff.

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



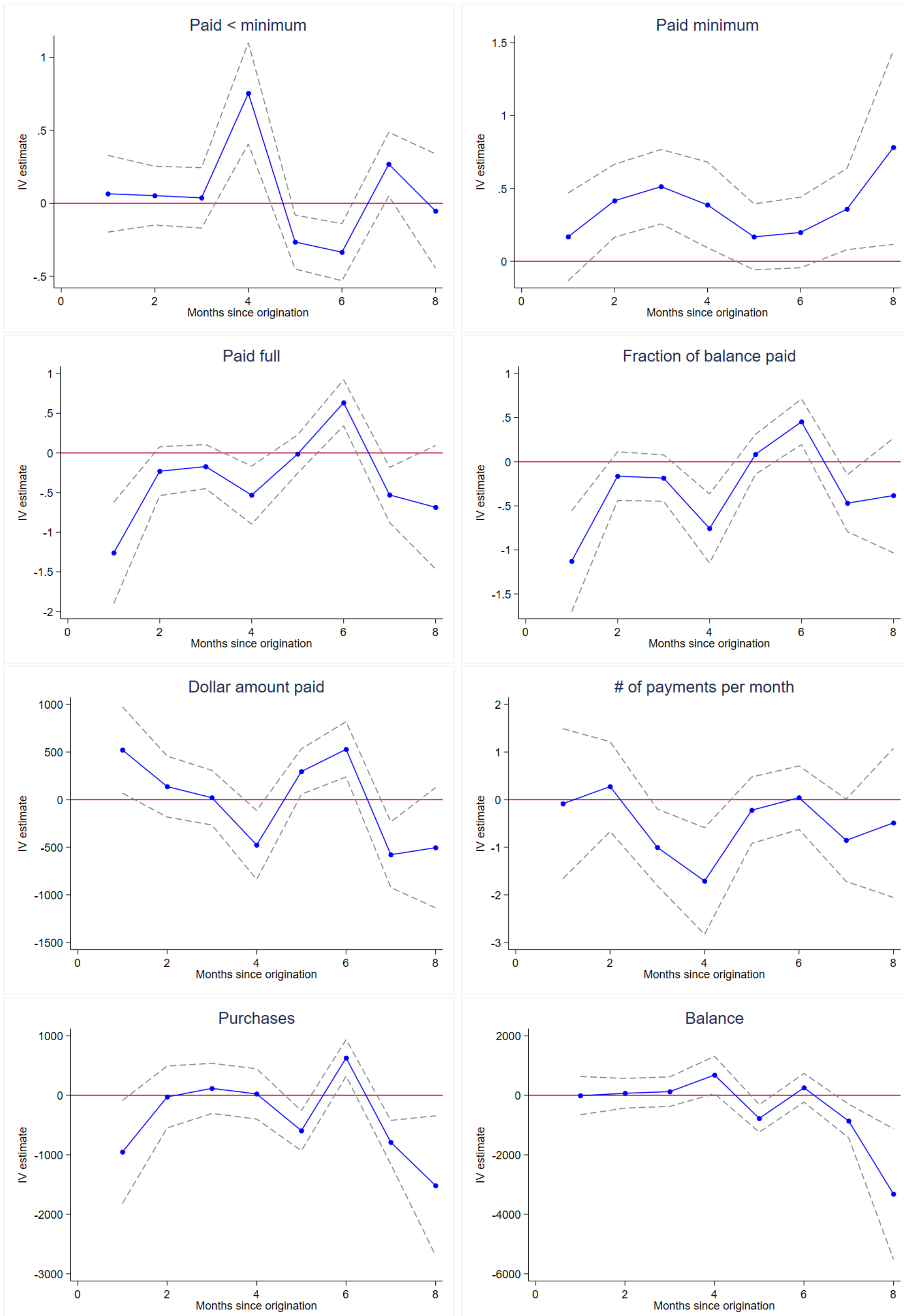
Notes: Graphs plot consumer and account characteristics by account origination date relative to the cutoff for the second change in underwriting flow. Each graph plots the average of the covariate across accounts opened in that calendar week, within 10-week windows of the cutoff dates.

Figure A3: **Reduced Form Outcomes - Second Underwriting Change with Controls**



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

Figure A4: **IV by Account Age - Second Change With Controls**



Notes: Figures plot parametric IV RD estimates of the effects of autopay enrollment on account outcomes, using a polynomial of order 1 with 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 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
	1228.7	(1.252)	(50.2)	(0.002)
	[0.050]	[0.131]	[0.058]	[0.363]
	Observations: 16,793, Joint F-test: 48.89, p-value: 0.000			
	First Change: (Bandwidth 10 weeks)			
Post	4515.8	1.768	128.7	0.011
	886.0	(0.910)	(36.1)	(0.001)
	[0.000]	[0.052]	[0.000]	[0.000]
	Observations: 31,165, Joint F-test: 2545.17, 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: 36,739, Joint F-test: 3329.46, p-value: 0.000			
	Second Change: (Bandwidth 5 weeks)			
Sample Mean:	$46,063	666	$2,115	20%
Post	- 22057.8	- 9.930	- 630.9	0.009
	9820.3	(12.590)	(372.0)	(0.016)
	[0.025]	[0.430]	[0.090]	[0.556]
	Observations: 11,770, Joint F-test: 10.98, p-value: 0.2026			
	Second Change: (Bandwidth 10 weeks)			
Post	- 10992.1	0.040	- 638.9	- 0.001
	1709.7	(2.076)	(65.3)	(0.003)
	[0.000]	[0.985]	[0.000]	[0.566]
	Observations: 24,580, Joint F-test: 126.33, p-value: 0.000			
	Second Change: (Bandwidth 15 weeks)			
Post	- 10380.9	- 1.839	- 587.3	- 0.001
	1246.2	(1.512)	(48.8)	(0.002)
	[0.000]	[0.224]	[0.000]	[0.688]
	Observations: 26,424, Joint F-test: 188.38, 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 date. 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)	(9)	(10)	(11)
	First stage variables		Cross-sectional outcomes					Panel outcomes			
	Autopay	Slider	Chargeoff	Max 1-30dpd	Max 30+ dpd	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	36%	20%	10%	10%	17%	9%	28%	38%	25%	38%	$266
Panel A: Linear											
Post	- 0.180	0.064	0.064	- 0.003	0.059	0.028	- 0.057	0.036	- 0.007	- 0.008	- 1.204
	(0.004)	(0.013)	(0.007)	(0.007)	(0.009)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.628)
	[0.000]	[0.000]	[0.000]	[0.708]	[0.000]	[0.000]	[0.000]	[0.000]	[0.040]	[0.009]	[0.795]
Panel B: Quadratic											
Post	- 0.222	0.095	0.066	0.000	0.057	0.022	- 0.063	0.047	- 0.005	- 0.008	- 13.660
	(0.005)	(0.020)	(0.011)	(0.010)	(0.013)	(0.003)	(0.005)	(0.006)	(0.005)	(0.005)	(6.738)
	[0.000]	[0.000]	[0.000]	[0.979]	[0.000]	[0.000]	[0.000]	[0.000]	[0.295]	[0.107]	[0.043]
Panel C: Cubic											
Post	- 0.210	0.111	0.047	0.001	0.041	0.017	- 0.050	0.059	- 0.027	- 0.024	- 35.440
	(0.007)	(0.024)	(0.014)	(0.014)	(0.017)	(0.004)	(0.006)	(0.007)	(0.007)	(0.006)	(8.598)
	[0.000]	[0.000]	[0.001]	[0.971]	[0.015]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel D: Quartic											
Post	- 0.200	0.147	0.030	0.000	0.026	0.014	- 0.038	0.064	- 0.040	- 0.044	- 57.100
	(0.008)	(0.031)	(0.017)	(0.017)	(0.021)	(0.006)	(0.008)	(0.009)	(0.008)	(0.008)	(10.530)
	[0.000]	[0.000]	[0.086]	[0.994]	[0.216]	[0.011]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel E: Local linear regression											
Post	- 0.183	0.066	0.064	- 0.003	0.059	0.027	- 0.057	0.037	- 0.007	- 0.009	- 2.147
	(0.004)	(0.013)	(0.007)	(0.007)	(0.009)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(4.588)
	[0.000]	[0.000]	[0.000]	[0.708]	[0.000]	[0.000]	[0.000]	[0.000]	[0.039]	[0.008]	[0.640]

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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	First stage variables		Cross-sectional outcomes					Panel outcomes			
	Autopay	Slider	Chargeoff	Max 1-30dpd	Max 30+ dpd	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount
Pre-period mean:	16%	8%	12%	9%	18%	11%	19%	43%	27%	40%	$293
					Panel A: Linear						
Post	0.142	- 0.020	0.028	- 0.031	0.047	0.008	0.049	- 0.028	- 0.029	- 0.027	- 4.626
	(0.007)	(0.035)	(0.012)	(0.011)	(0.015)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(7.201)
	[0.000]	[0.560]	[0.016]	[0.005]	[0.002]	[0.134]	[0.000]	[0.000]	[0.000]	[0.000]	[0.521]
					Panel B: Quadratic						
Post	0.203	- 0.077	- 0.003	- 0.073	0.050	0.045	0.046	- 0.091	- 0.001	- 0.028	- 38.50
	(0.022)	(0.111)	(0.037)	(0.034)	(0.047)	(0.016)	(0.020)	(0.025)	(0.022)	(0.021)	(22.04)
	[0.000]	[0.489]	[0.927]	[0.031]	[0.285]	[0.005]	[0.024]	[0.000]	[0.976]	[0.183]	[0.081]
					Panel C: Cubic						
Post	0.049	- 0.294	0.152	- 0.040	0.148	0.033	0.055	- 0.205	0.118	0.122	41.60
	(0.075)	(0.344)	(0.124)	(0.114)	(0.159)	(0.054)	(0.068)	(0.082)	(0.075)	(0.071)	(75.05)
	[0.511]	[0.393]	[0.219]	[0.725]	[0.351]	[0.547]	[0.423]	[0.013]	[0.117]	[0.084]	[0.579]
					Panel D: Quartic						
Post	- 0.057	2.397	0.370	0.073	0.700	0.370	0.066	0.548	- 0.985	- 1.006	- 364.4
	(0.264)	(1.128)	(0.397)	(0.373)	(0.517)	(0.189)	(0.243)	(0.291)	(0.268)	(0.251)	(283.6)
	[0.829]	[0.034]	[0.352]	[0.846]	[0.175]	[0.051]	[0.785]	[0.060]	[0.000]	[0.000]	[0.199]
					Panel E: Local linear regression						
Post	0.143	- 0.022	0.028	- 0.031	0.047	0.009	0.049	- 0.029	- 0.028	- 0.027	- 5.671
	(0.007)	(0.034)	(0.012)	(0.011)	(0.015)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)	(7.176)
	[0.000]	[0.530]	[0.018]	[0.004]	[0.002]	[0.088]	[0.000]	[0.000]	[0.000]	[0.000]	[0.429]

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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A4: **Additional Outcomes - First Underwriting Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No controls				With controls			
	Paid round	# of payments	Purchases	Balance	Paid round	# of payments	Purchases	Balance
Pre-period mean:	16%	1.37	$312	$1,086	16%	1.37	$312	$1,086
	Panel A: Linear							
Post	0.024	- 0.067	32.040	72.980	0.018	- 0.039	32.770	73.460
	(0.003)	(0.011)	(8.418)	(9.474)	(0.003)	(0.011)	(8.013)	(8.309)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel B: Quadratic							
Post	0.032	- 0.067	- 10.500	29.790	0.025	- 0.048	7.391	14.960
	(0.004)	(0.017)	(11.180)	(13.900)	(0.004)	(0.017)	(10.700)	(12.160)
	[0.000]	[0.000]	[0.348]	[0.032]	[0.000]	[0.004]	[0.490]	[0.219]
	Panel C: Cubic							
Post	0.035	- 0.123	- 65.260	16.900	0.028	- 0.103	- 54.780	- 11.090
	(0.006)	(0.023)	(13.940)	(17.720)	(0.006)	(0.023)	(13.410)	(15.560)
	[0.000]	[0.000]	[0.000]	[0.340]	[0.000]	[0.000]	[0.000]	[0.476]
	Panel D: Quartic							
Post	0.045	- 0.192	- 152.500	- 15.280	0.037	- 0.134	- 79.750	54.860
	(0.007)	(0.029)	(16.180)	(21.710)	(0.007)	(0.029)	(15.550)	(19.080)
	[0.000]	[0.000]	[0.000]	[0.481]	[0.000]	[0.000]	[0.000]	[0.004]
	Panel E: Local linear regression							
Post	0.025	- 0.067	29.870	71.220	0.019	- 0.039	31.280	70.780
	(0.003)	(0.011)	(8.287)	(9.498)	(0.003)	(0.011)	(7.792)	(8.330)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[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 linear discontinuity estimate as a percentage of the pre-period mean. Samples include accounts originated within 10 weeks of the cutoff dates.

Table A5: **Additional Outcomes - Second Underwriting Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No controls				With controls			
	Paid round	# of payments	Purchases	Balance	Paid round	# of payments	Purchases	Balance
Pre-period mean:	20%	1.38	$382	$1,163	20%	1.38	$382	$1,163
	Panel A: Linear							
Post	- 0.031	- 0.090	- 86.15	- 236.9	- 0.020	- 0.089	- 28.12	- 35.29
	(0.006)	(0.021)	(9.765)	(13.87)	(0.006)	(0.021)	(9.38)	(12.72)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.003]	[0.006]
	Panel B: Quadratic							
Post	- 0.064	- 0.141	- 113.70	- 233.0	- 0.071	- 0.090	- 16.38	- 98.83
	(0.019)	(0.065)	(29.12)	(41.35)	(0.019)	(0.065)	(27.77)	(37.75)
	[0.001]	[0.030]	[0.000]	[0.000]	[0.000]	[0.167]	[0.555]	[0.009]
	Panel C: Cubic							
Post	- 0.139	- 0.592	- 166.70	- 514.5	- 0.152	- 0.535	55.64	- 178.30
	(0.062)	(0.221)	(95.50)	(136.4)	(0.063)	(0.224)	(93.490)	(126.20)
	[0.025]	[0.007]	[0.081]	[0.000]	[0.015]	[0.017]	[0.552]	[0.158]
	Panel D: Quartic							
Post	0.023	- 2.108	- 660.900	685.0	0.209	- 0.182	- 301.9	245.8
	(0.205)	(0.727)	(309.30)	(440.6)	(0.223)	(0.817)	(391.8)	(482.2)
	[0.913]	[0.004]	[0.033]	[0.120]	[0.349]	[0.824]	[0.441]	[0.610]
	Panel E: Local linear regression							
Post	- 0.032	- 0.092	- 86.52	- 235.9	- 0.021	- 0.090	- 27.18	- 35.73
	(0.006)	(0.021)	(9.675)	(13.83)	(0.006)	(0.021)	(9.255)	(12.66)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.003]	[0.005]

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 linear discontinuity estimate as a percentage of the pre-period mean. Samples include accounts originated within 10 weeks of the cutoff dates.

Table A6: **Reduced Form Estimates Conditional on No Chargeoff - First Underwriting Change, No Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
Pre-period mean:	5%	29%	40%	27%	40%	$277	17%	1.41	$299	$1,021
Panel A: Linear										
Post	0.004	- 0.049	0.062	- 0.017	- 0.014	- 17.440	0.031	- 0.043	- 15.380	- 4.591
	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(4.378)	(0.003)	(0.010)	(5.190)	(8.424)
	[0.017]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]	[0.586]
Panel B: Quadratic										
Post	0.001	- 0.059	0.070	- 0.012	- 0.011	- 17.730	0.039	- 0.043	- 31.430	- 18.430
	(0.003)	(0.005)	(0.006)	(0.006)	(0.005)	(6.693)	(0.005)	(0.016)	(7.643)	(12.610)
	[0.716]	[0.000]	[0.000]	[0.030]	[0.031]	[0.008]	[0.000]	[0.009]	[0.000]	[0.144]
Panel C: Cubic										
Post	- 0.001	- 0.047	0.080	- 0.032	- 0.026	- 31.430	0.041	- 0.086	- 52.650	- 7.743
	(0.003)	(0.007)	(0.008)	(0.007)	(0.007)	(8.838)	(0.01)	(0.02)	(9.87)	(16.22)
	[0.763]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.633]
Panel D: Quartic										
Post	0.001	- 0.029	0.091	- 0.063	- 0.064	- 77.080	0.050	- 0.154	- 121.300	- 37.970
	(0.004)	(0.008)	(0.010)	(0.009)	(0.008)	(11.080)	(0.008)	(0.029)	(12.180)	(19.990)
	[0.873]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.058]
Panel E: Local linear regression										
Post	0.004	- 0.050	0.062	- 0.016	- 0.014	- 17.580	0.032	- 0.043	- 16.310	- 4.861
	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(4.405)	(0.003)	(0.010)	(5.218)	(8.456)
	[0.023]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]	[0.565]

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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A7: **Reduced Form Estimates Conditional on No Chargeoff - First Underwriting Change, With Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
Pre-period mean:	5%	29%	40%	27%	40%	$277	17%	1.41	$299	$1,021
					Panel A: Linear					
Post	0.003	- 0.055	0.050	0.003	0.005	- 1.755	0.024	- 0.007	2.285	8.629
	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(4.279)	(0.003)	(0.010)	(4.962)	(7.365)
	[0.155]	[0.000]	[0.000]	[0.440]	[0.121]	[0.682]	[0.000]	[0.507]	[0.645]	[0.241]
					Panel B: Quadratic					
Post	- 0.003	- 0.060	0.060	0.002	0.004	- 10.330	0.032	- 0.022	- 9.899	- 34.200
	(0.003)	(0.005)	(0.006)	(0.005)	(0.005)	(6.467)	(0.005)	(0.016)	(7.158)	(10.950)
	[0.317]	[0.000]	[0.000]	[0.673]	[0.459]	[0.110]	[0.000]	[0.187]	[0.167]	[0.002]
					Panel C: Cubic					
Post	- 0.003	- 0.047	0.072	- 0.023	- 0.016	- 29.050	0.033	- 0.065	- 38.750	- 29.420
	(0.003)	(0.007)	(0.008)	(0.007)	(0.006)	(8.514)	(0.006)	(0.023)	(9.274)	(14.110)
	[0.424]	[0.000]	[0.000]	[0.001]	[0.015]	[0.001]	[0.000]	[0.004]	[0.000]	[0.037]
					Panel D: Quartic					
Post	- 0.003	- 0.035	0.075	- 0.037	- 0.038	- 48.930	0.044	- 0.097	- 59.050	18.960
	(0.004)	(0.008)	(0.010)	(0.009)	(0.008)	(10.670)	(0.008)	(0.029)	(11.460)	(17.420)
	[0.457]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.276]
					Panel E: Local linear regression					
Post	0.002	- 0.056	0.051	0.003	0.005	- 2.438	0.025	- 0.008	1.630	7.072
	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(4.308)	(0.003)	(0.010)	(4.987)	(7.403)
	[0.202]	[0.000]	[0.000]	[0.481]	[0.147]	[0.571]	[0.000]	[0.453]	[0.744]	[0.339]

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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A8: **Reduced Form Estimates Conditional on No Chargeoff - Second Underwriting Change, No Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
Pre-period mean:	5%	20%	46%	29%	43%	$305	21%	1.44	$348	$1,063
Panel A: Linear										
Post	0.003	0.059	- 0.026	- 0.035	- 0.031	- 58.62	- 0.029	- 0.081	- 79.39	- 217.4
	(0.004)	(0.007)	(0.008)	(0.008)	(0.007)	(7.659)	(0.006)	(0.021)	(8.711)	(13.65)
	[0.541]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Quadratic										
Post	0.030	0.048	- 0.050	- 0.029	- 0.055	- 94.33	- 0.062	- 0.180	- 152.2	- 234.3
	(0.013)	(0.021)	(0.026)	(0.024)	(0.022)	(24.03)	(0.020)	(0.066)	(28.10)	(41.75)
	[0.024]	[0.024]	[0.053]	[0.232]	[0.015]	[0.000]	[0.002]	[0.006]	[0.000]	[0.000]
Panel C: Cubic										
Post	- 0.101	0.041	- 0.114	0.174	0.179	- 32.43	- 0.133	- 0.525	- 126.20	- 481.80
	(0.045)	(0.071)	(0.085)	(0.081)	(0.075)	(80.980)	(0.07)	(0.22)	(94.17)	(137.30)
	[0.025]	[0.565]	[0.183]	[0.031]	[0.017]	[0.689]	[0.042]	[0.019]	[0.180]	[0.000]
Panel D: Quartic										
Post	0.522	0.348	0.470	- 1.339	- 1.383	- 674.200	0.057	- 1.443	- 378.40	725.80
	(0.146)	(0.236)	(0.280)	(0.266)	(0.246)	(265.40)	(0.215)	(0.733)	(302.10)	(444.70)
	[0.000]	[0.140]	[0.094]	[0.000]	[0.000]	[0.011]	[0.791]	[0.049]	[0.210]	[0.103]
Panel E: Local linear regression										
Post	0.003	0.058	- 0.027	- 0.035	- 0.031	- 59.69	- 0.030	- 0.084	- 81.07	- 217.30
	(0.004)	(0.007)	(0.008)	(0.008)	(0.007)	(7.657)	(0.006)	(0.021)	(8.671)	(13.640)
	[0.444]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A9: **Reduced Form Estimates Conditional on No Chargeoff - Second Underwriting Change, With Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
Pre-period mean:	5%	20%	46%	29%	43%	$305	21%	1.44	$348	$1,063
Panel A: Linear										
Post	0.000	0.052	- 0.025	- 0.027	- 0.024	- 7.176	- 0.019	- 0.086	- 26.54	- 49.56
	(0.004)	(0.007)	(0.008)	(0.007)	(0.007)	(7.364)	(0.006)	(0.021)	(8.143)	(12.22)
	[0.920]	[0.000]	[0.002]	[0.000]	[0.000]	[0.330]	[0.002]	[0.000]	[0.001]	[0.000]
Panel B: Quadratic										
Post	0.022	0.051	- 0.081	0.008	- 0.021	- 46.190	- 0.068	- 0.117	- 69.37	- 139.40
	(0.013)	(0.021)	(0.026)	(0.023)	(0.022)	(22.960)	(0.020)	(0.065)	(26.30)	(37.52)
	[0.091]	[0.018]	[0.001]	[0.736]	[0.341]	[0.044]	[0.001]	[0.073]	[0.008]	[0.000]
Panel C: Cubic										
Post	- 0.099	0.104	- 0.169	0.165	0.180	66.720	- 0.139	- 0.454	33.24	- 253.10
	(0.045)	(0.072)	(0.086)	(0.079)	(0.073)	(78.030)	(0.066)	(0.226)	(89.090)	(125.10)
	[0.027]	[0.146]	[0.048]	[0.037]	[0.013]	[0.393]	[0.035]	[0.045]	[0.709]	[0.043]
Panel D: Quartic										
Post	0.169	0.128	0.613	- 0.910	- 0.905	- 122.900	0.193	0.535	386.000	976.70
	(0.156)	(0.254)	(0.302)	(0.279)	(0.257)	(281.600)	(0.234)	(0.819)	(323.10)	(452.60)
	[0.281]	[0.613]	[0.042]	[0.001]	[0.000]	[0.663]	[0.409]	[0.513]	[0.232]	[0.031]
Panel E: Local linear regression										
Post	0.000	0.052	- 0.027	- 0.026	- 0.024	- 8.280	- 0.020	- 0.087	- 26.99	- 50.85
	(0.004)	(0.007)	(0.008)	(0.007)	(0.007)	(7.353)	(0.006)	(0.021)	(8.085)	(12.210)
	[0.954]	[0.000]	[0.001]	[0.001]	[0.000]	[0.260]	[0.001]	[0.000]	[0.001]	[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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A10: **IV Estimates with Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Cross-sectional outcomes			Panel outcomes									
	Chargeoff	Max 1-30dpd	Max 30+ dpd	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
	Panel A: First Change, Linear												
Pre-period mean:	10%	10%	17%	9%	28%	38%	25%	38%	$266	16%	1.37	$312	$1,086
Autopay	- 0.163	- 0.006	- 0.145	- 0.153	0.315	- 0.201	0.039	0.047	6.692	- 0.102	0.214	- 182.1	- 408.1
	(0.012)	(0.014)	(0.015)	(0.013)	(0.019)	(0.021)	(0.019)	(0.018)	(25.71)	(0.017)	(0.060)	(44.70)	(46.29)
	[0.000]	[0.648]	[0.000]	[0.000]	[0.000]	[0.000]	[0.039]	[0.009]	[0.795]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel B: First Change, Quadratic												
Autopay	- 0.122	- 0.020	- 0.077	- 0.098	0.284	- 0.210	0.024	0.035	61.630	- 0.114	0.217	- 33.340	- 67.49
	(0.014)	(0.017)	(0.018)	(0.016)	(0.023)	(0.025)	(0.023)	(0.022)	(30.370)	(0.020)	(0.076)	(48.300)	(54.770)
	[0.000]	[0.227]	[0.000]	[0.000]	[0.000]	[0.000]	[0.294]	[0.105]	[0.042]	[0.000]	[0.004]	[0.490]	[0.218]
	Panel C: Second Change, Linear												
Pre-period mean:	12%	9%	18%	11%	19%	43%	27%	40%	$293	20%	1.38	$382	$1,163
Autopay	0.098	- 0.246	0.280	0.053	0.346	- 0.197	- 0.203	- 0.190	- 32.7	- 0.139	- 0.631	- 198.6	- 249.3
	(0.027)	(0.034)	(0.043)	(0.036)	(0.046)	(0.054)	(0.053)	(0.049)	(50.950)	0.042	0.152	(67.12)	(89.47)
	[0.000]	[0.000]	[0.000]	[0.138]	[0.000]	[0.000]	[0.000]	[0.000]	[0.521]	[0.001]	[0.000]	[0.003]	[0.005]
	Panel D: Second Change, Quadratic												
Autopay	0.057	- 0.375	0.357	0.222	0.226	- 0.445	- 0.003	- 0.137	- 189.5	- 0.347	- 0.442	- 80.6	- 486.4
	(0.058)	(0.078)	(0.098)	(0.086)	(0.100)	(0.123)	(0.110)	(0.106)	(111.1)	(0.096)	(0.326)	(137.1)	(188.4)
	[0.325]	[0.000]	[0.000]	[0.009]	[0.023]	[0.000]	[0.976]	[0.194]	[0.088]	[0.000]	[0.175]	[0.557]	[0.010]

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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A11: **IV Estimates Conditional on No Chargeoff without Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
					Panel A: First Change, Linear					
Pre-period mean:	5%	29%	40%	27%	40%	$277	17%	1.41	$299	$1,021
Autopay	- 0.006	0.323	- 0.284	- 0.033	- 0.050	- 57.83	- 0.196	0.214	- 107.80	- 294.5
	(0.008)	(0.016)	(0.018)	(0.017)	(0.016)	(19.28)	(0.014)	(0.045)	(22.920)	(37.68)
	[0.405]	[0.000]	[0.000]	[0.050]	[0.001]	[0.003]	[0.000]	[0.000]	[0.000]	[0.000]
					Panel B: First Change, Quadratic					
Autopay	- 0.022	0.256	- 0.377	0.143	0.135	162.40	- 0.174	0.258	202.100	182.7
	(0.012)	(0.024)	(0.027)	(0.025)	(0.024)	(30.03)	(0.021)	(0.072)	(35.110)	(57.32)
	[0.065]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
					Panel C: Second Change, Linear					
Pre-period mean:	5%	20%	46%	29%	43%	$305	21%	1.44	$348	$1,063
Autopay	0.019	0.441	- 0.198	- 0.262	- 0.230	- 441.4	- 0.218	- 0.612	- 597.7	- 1637
	(0.031)	(0.053)	(0.060)	(0.061)	(0.056)	(64.43)	0.047	0.162	(75.5)	(135.7)
	[0.542]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
					Panel D: Second Change, Quadratic					
Autopay	0.158	0.255	- 0.262	- 0.152	- 0.288	- 497.9	- 0.327	- 0.951	- 803.2	- 1237
	(0.075)	(0.113)	(0.133)	(0.131)	(0.127)	(145.9)	(0.108)	(0.370)	(185.5)	(270.4)
	[0.034]	[0.024]	[0.049]	[0.245]	[0.024]	[0.001]	[0.002]	[0.010]	[0.000]	[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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.

Table A12: **IV Estimates Conditional on No Chargeoff with Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	< Min payment	Minimum payment	Min to full	Full	Fraction	Payment amount	Paid round	# of payments	Purchases	Balance
Panel A: First Change, Linear										
Pre-period mean:	5%	29%	40%	27%	40%	$277	17%	1.41	$299	$1,021
Autopay	- 0.010	0.348	- 0.277	- 0.060	- 0.083	- 24.470	- 0.173	0.054	- 72.72	- 66.14
	(0.008)	(0.016)	(0.018)	(0.016)	(0.015)	(18.870)	(0.014)	(0.045)	(22.060)	(33.030)
	[0.189]	[0.000]	[0.000]	[0.000]	[0.000]	[0.195]	[0.000]	[0.234]	[0.001]	[0.045]
Panel B: First Change, Quadratic										
Autopay	- 0.005	0.311	- 0.296	- 0.011	- 0.015	32.020	- 0.137	0.056	24.73	64.25
	(0.013)	(0.026)	(0.029)	(0.026)	(0.025)	(31.500)	(0.023)	(0.078)	(35.72)	(53.85)
	[0.699]	[0.000]	[0.000]	[0.689]	[0.533]	[0.309]	[0.000]	[0.469]	[0.489]	[0.233]
Panel C: Second Change, Linear										
Pre-period mean:	5%	20%	46%	29%	43%	$305	21%	1.44	$348	$1,063
Autopay	- 0.003	0.350	- 0.169	- 0.178	- 0.165	- 48.34	- 0.129	- 0.580	- 178.8	- 333.8
	(0.028)	(0.046)	(0.054)	(0.052)	(0.048)	(49.72)	0.042	0.144	(55.71)	(82.52)
	[0.920]	[0.000]	[0.002]	[0.001]	[0.001]	[0.331]	[0.002]	[0.000]	[0.001]	[0.000]
Panel D: Second Change, Quadratic										
Autopay	0.101	0.229	- 0.366	0.036	- 0.093	- 209.0	- 0.309	- 0.530	- 313.8	- 630.9
	(0.062)	(0.096)	(0.115)	(0.105)	(0.099)	(106.6)	(0.092)	(0.303)	(124.1)	(177.5)
	[0.100]	[0.017]	[0.001]	[0.735]	[0.347]	[0.050]	[0.001]	[0.080]	[0.011]	[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 date. The first row presents sample means in the 10 weeks before the cutoff dates for each variable.