Can Credit Rating Affect Credit Risk? Causal Evidence from an Online Lending Marketplace

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Abstract

Credit rating is determined by credit risk, but can the rating in itself change a borrower's credit risk in an economically meaningful manner? Despite the theoretical and practical importance of this question, there is limited empirical evidence on this topic since it is difficult to obtain variation in credit rating that is independent of a borrower's underlying fundamentals. Using a regulatory change in March 2020 that provides a credibly exogenous variation in the credit ratings of household borrowers, we show that individuals with negative rating shock default at a 23 percentage point higher rate than otherwise identical borrowers in the year following the negative shock. Our findings suggest that empirical studies linking credit ratings to real outcomes should carefully consider the endogenous effect of ratings on future outcomes. These findings also show that frequent incidence of erroneous credit bureau data imposes economically large long-term costs on consumers.

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1 Introduction

Credit ratings play a vital role in the allocation and pricing of capital across individuals, households, corporations, and sovereigns. A standard mechanism through which these ratings affect borrower outcomes is the revelation of the borrower's true credit risk to relatively uninformed providers of capital. Credit ratings, therefore, are determined by the credit risk of the borrower. However, a higher credit rating by itself can facilitate better access to financing, which in turn can potentially affect the borrower's credit risk. Several theoretical papers argue that coordination cost among lenders can help borrowers with better ratings obtain external financing at attractive terms even if their underlying fundamentals remain the same (Boot et al., 2006; Manso, 2013; Opp et al., 2013). Consequently, credit rating by itself may impact a borrower's future performance and credit risk.

As shown by Manso (2013), the rating-risk relation has implications for the credit rating process itself. For example, how should a rating agency incorporate the effect of their action on rating? Does the feedback effect lead to a worse outcome for borrowers when a multiplicity of rating equilibrium is possible? Thus, whether credit rating affects future outcomes or not has implications for the efficiency of credit markets. Understanding the causal effect of ratings on credit risk is also important for the interpretation of empirical literature that studies the effect of ratings on borrower outcomes. Finally, credit reporting mistakes are fairly common for individual borrowers with millions of Americans complaining about such errors on their reports every year. A study by the Federal Trade Commission documented that 26% of randomly selected consumers reported some error in their credit report.[1] If these mistakes and the resulting deterioration in the affected consumers' rating, such as FICO score, affects their performance, then rating mistakes can have significant welfare and distributional implications for the economy. Does credit scoring error, i.e., deterioration in rating that is independent of the credit risk of the borrowers, affect the borrower's future

[1] See: https://www.ftc.gov/sites/default/files/documents/reports/section-319-fair-and-accurate-credit-transactions-act-2003-fifth-interim-federal-trade-commission/130211factareport.pdf

default risk in an economically meaningful manner?

Given the intensity of information frictions in retail lending, these issues are especially important for individual borrowers. However, there is a paucity of empirical evidence teasing out the impact of rating on credit risk, either for individual borrowers or for corporate entities. The main empirical difficulty in establishing a causal link from rating to risk comes from a lack of plausibly exogenous variation in ratings that are unrelated to the fundamental characteristics of the borrowers. How can we observe two sets of borrowers who are identical in terms of their fundamentals but have different ratings? Our paper address this issue by exploiting a unique law change in the immediate aftermath or the COVID-19 crisis that altered the credit ratings of two identical groups of borrowers in a quasi-random manner.

A provision of the Coronavirus Aid, Relief, and Economic Security (CARES) Act in March, 2020[2] changed the reporting requirement for borrowers who were late on their payments in an unexpected manner: borrowers who were late with their payments before January 31, 2020 were reported as such to the credit bureau, but borrowers who were late afterward were not. Further, the law was signed by the President on March 27, 2020, i.e., it was signed retroactively for borrowers who were late in February and most of March. Therefore, the borrowers or the lenders could not have anticipated the law change before it was passed by the President. Nor could they have changed their behavior in response to the policy change. Our identification strategy relies on the assumption that the change in reporting requirements to credit bureaus exogenously changes the borrower's rating, namely it changes their FICO scores without any concomitant changes in their fundamentals. Therefore, the reporting change shock serves as an instrument for rating changes, allowing us to tease out the rating's standalone effect on outcomes.

We use a comprehensive sample of borrowers from the online lender LendingClub and exploit an institutional feature of the lending platform along with the reporting requirement change for our empirical analysis. The online lender provides a hardship plan to their bor-

[2]https://www.consumerfinance.gov/about-us/blog/protecting-your-credit-during-coronavirus-pandemic/

rowers if the borrower faces financial difficulty. Once a borrower goes on the hardship plan, they make reduced payments for the next three months. More importantly, if a borrower went on the hardship plans before January 31, 2020, they were reported as being late on their payments to the credit bureaus. Borrowers who went on the plan after the cutoff date were not reported as being late. Therefore, we get an exogenous variation in the credit reporting for these two sets of borrowers.

We narrow our focus on comparing borrowers' outcomes for those who went on the hardship plan in a short window surrounding the cutoff date; specifically, borrowers who went on the plan between December, 2019 to early March, 2020. Borrowers who entered the hardship plan in February and early March are called the "treatment" borrower, whereas borrowers who went on the hardship plan in December and January are called the "control" borrowers. To ensure that our results are not contaminated by borrowers who became late in their payments due to the COVID-19 shock, we only include borrowers who were late in early March, i.e., before the effect of COVID-19 disruption was felt in the U.S.

We first show that the treated and control borrowers were nearly identical on several dimensions that correlate with a borrower's credit risk. The distribution of FICO scores at origination, FICO, before they become late on their payments, debt-to-income ratio, interest rates on their loans, the amount of debt they have, maturity of loans, income, and percentage utilization of their credit lines, are almost identical across the two groups. They are also similar in terms of their occupation and the internal risk assessment of LendingClub at the time they received their loans. Therefore, these two sets of borrowers are similar except for the fact that the treated borrowers were not reported late despite being late in their payments. Our empirical setting, as a result, allows us to tease out the standalone effect of credit score, i.e., credit rating, on future outcomes holding fixed the borrower's fundamental risk factors.

Our analysis shows that the treated borrowers experienced a significant increase in their FICO score soon after the reporting shock, as compared to the control borrower. While the two groups followed a parallel trend in their FICO scores before they entered the hardship plan (i.e., became late on their payments), the group that was not reported late experienced an average increase of 16 points in its average FICO score relative to the late reported borrowers. The effect corresponds to about one-third of the standard deviation of FICO scores of all the borrowers in our sample. The largest change across the treated and control borrowers occurred three-to-five months after the borrowers become late in their payments, consistent with the practice that credit bureaus incorporate the effect of late payments on FICO scores with some lag. Overall, these results show that the two identical group of borrowers ended up with materially different ratings due to an exogenous change in the reporting requirement for late payments.

What implication does the exogenous rating change have on borrower outcomes? We follow their default rate over time to answer this question. In a 10-month period following the borrower hardship, the default rate of the treated group is 23 percentage points lower than the control group.[3] Therefore, the effect of reporting requirement change on borrower default rate is economically large. Using a logistic regression model that controls for a host of borrower characteristics and loan features, we show that the odds ratio of default is between 0.37-0.39 for the treated borrower, depending on the model specification. We obtain similar results using a discrete time hazard rate model that carefully accounts for the timing of default and several other control variables. The odds ratio of default for the treated group is between 0.48-0.50 for the hazard rate specification. Overall, these results show that the treated borrowers defaulted at a much lower rate that is significant, both statistically and economically.

In our main test, we directly relate the deterioration in FICO score due to the reporting change to the default rate using an instrumental variable regression framework. In the first stage regression, we use the treatment status of a borrower as an instrument for changes in their FICO score over a period surrounding their hardship, i.e., from one month before

[3]We stop at the 10-month period because our data ends in January, 2021. Thus we are able to track all our borrowers in the treatment and control sample for a maximum period of 10 months.

they enter the hardship plan to four months after it. Consistent with our earlier result, borrowers in the treatment status experience a significant increase in their FICO score over this time period compared to the treatment set. The second stage regression shows that borrowers whose FICO scores increased by one standard deviation have 76% lower default probability. The results are robust to a host of controls including state fixed effects, indicating that differential COVID-related policies such as the variation in the length and timing of lockdown across states does not affect our results. Overall, these findings show that credit rating changes have a significant effect on the future performance of otherwise equivalent borrowers.

Our results are robust to alternative econometric specifications that allow us to model borrower default in a dynamic manner, i.e., when we exploit the timing of the borrower's default in a more granular manner in addition to whether they defaulted during the sample period or not. Since our treated borrowers went into hardship in December, 2019 and January, 2020, whereas the control borrowers in February, 2020 and early March, 2020, we ensure that our results are not driven by the proximity of hardship to the pandemic shock. Similarly, our results do not change if we control for the borrower's location or the time since the borrower entered the hardship. Unsurprisingly, our coefficients of interest remain similar, both statistically and economically in these alternative specifications since our treated and control borrowers are nearly identical on observable characteristics.

In our final test, we focus on the mechanism behind our results. Specifically, we empirically evaluate whether the treated borrowers had better access to external financing compared to the control borrowers. Although we are unable to observe all the external financing that a borrower obtains, we do observe the amount of loan balance the borrower has on the lending platform. Leading up to hardship, both sets of borrowers show a steady decline in their loan balance, capturing the fact that they have been regularly paying down their loans. Soon after getting into distress, both groups slow down their payments, again as expected due to the concessional payments they need to make once they are on the plan. However, four months after the hardship, there is a significant divergence between the two groups. The treated borrowers pay significantly higher amounts of their debt compared to the control borrowers beginning in the fifth month after the distress. Panel regression analysis shows that the treated borrowers paid off their balance at 0.87% higher rate per month compared to the control borrowers after entering into the hardship program, after soaking away the borrower and time fixed effects. The effect translates into almost $950 lower balance for these borrowers in the post-hardship period, which is approximately 5% of the original loan amount. Overall, these results show that better credit scores in itself helped the treated borrowers obtain better access to financing, which ultimately lowered their credit risk.

Our paper complements the literature that has analyzed the effect of information sharing about borrowers on access to credit. Utilizing an Argentine law requiring the publication of banks' private assessment of their borrowers to the public, Hertzberg et al. (2011) shows that when banks' negative opinions of their borrowers are released to the public, they lend less to current borrowers in anticipation of other creditors doing the same. Almeida et al. (2017) study the effect of sovereign debt rating on corporate borrowers. Our paper is also closely related to the literature on the removal of bankruptcy flag on credit availability, entrepreneurship, and labor market outcomes (Dobbie et al., 2020; Gross et al., 2020; Bos et al., 2018; Herkenhoff et al., 2021). These papers have advanced the literature by documenting the effect of information sharing about borrower quality on their external financing and employment outcomes. Our paper differs from this literature on three key dimensions. First, our paper measures the effect of a shock to credit rating on credit risk, namely, the bankruptcy outcome. We are not aware of any paper that links the standalone effect of credit rating on credit risk in a causal manner. Second, our empirical setting is about a shock to credit rating at the time a borrower gets into distress, not several years after they enter bankruptcy. Therefore, our design allows us to tease out the impact of credit rating changes soon after the change happens, which minimizes the concern about changes in borrower or lender behavior in the years following the bankruptcy. Related, our study is not about borrowers who get into bankruptcy; thus it avoids issues related to the effect of the stigma of bankruptcy from the pure effect of credit rating shock. Third, our study provides an empirical evaluation of a specific and important aspect of the CARES Act that is of interest to future policy designs. Our findings show that the reporting change passed as per the Act helped the affected borrowers in terms of better access to financing, which in turn helped them avoid default.

Our finding that credit scores or ratings by themselves can cause real effects is relevant for several other settings in addition to household finance. For example Griffin and Tang (2012) show that rating agency subjectivity played a role in the ratings of structured financial products. Our results show that such subjectivity can endogenously change the future performance of highly rated tranche and allow them to raise even more money in good times.

Our paper is closely related to the literature on the informativeness of credit ratings and their real effects. Cornaggia et al. (2018) show that municipalities' cost of capital changed in response to a mechanical recalibration of ratings by Moody's in 2010. Their paper, therefore, provides compelling evidence that investors rely on credit ratings, even in the post-financial crisis world. Kliger and Sarig (2000) document that debt markets responded to a refinement of credit ratings by Moody's in 1982. Finally, there is a large literature relating credit ratings to security prices and financing decisions, including the possibility of biases and conflict of interest(Ashcraft et al., 2011; Holthausen and Leftwich, 1986; Kisgen and Strahan, 2010; Hand et al., 1992; Griffin et al., 2013; Becker and Milbourn, 2011; Becker and Ivashina, 2015). The key difference of our paper is that we document the effect of rating changes on the borrower's future credit risk itself, namely the default rate of the borrowers. To the best of our knowledge, such a link has not been established in the literature before.

2 Setting and Research Design

Soon after the onset of the COVID-19 pandemic in the U.S., congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act on March 25, 2020. The law was signed into effect on March 27th, 2020.[4] Under the CARES Act, some lenders were prohibited from reporting borrowers who were late on their payments to the credit bureau. We exploit this law change and an interesting institutional feature of our online lender for our research design to detect the effect of credit rating on credit risk.

In 2017, the online lender LendingClub began to offer a hardship plan to its borrowers who face financial difficulties in making the promised payments. The hardship plans allow borrowers to make interest-only payments for a period of three months to help them overcome unexpected financial difficulties. The idea behind offering this plan was the same as the rationale behind any debt restructuring for distressed borrowers, namely, to give them time to recover from financial distress. After the three month period, the borrower is expected to resume regularly scheduled payments to investors. The investors on this platform are almost all institutional investors. From the investors' viewpoint, the restructuring allows them to avoid immediate charge off of the loan, with the expectation that the borrowers will be able to overcome their financial difficulties in the meantime.

In normal times, when a borrower enters hardship he is reported as being "late" on his payments to the credit bureau. Therefore, entering the hardship plan affects the borrower's credit rating, namely, his FICO score, through the effect of "late" reporting on FICO score. The entry into a hardship plan correlates with a borrower's fundamentals: borrowers with recent declines in their FICO scores and other adverse shocks are more likely to get into a hardship plan. Therefore, "late" reporting is an informative event about the borrower's fundamentals. The CARES Act, passed on March 27, 2020, retroactively changed the late reporting requirement for borrowers entering the hardship plan. Specifically, as per the CARES Act any borrower entering into a hardship plan after January 31, 2020 was not

[4] https://home.treasury.gov/policy-issues/coronavirus/about-the-cares-act

reported as being late on their payment. Since the Act was passed unexpectedly in late March, borrowers, investors, and the lending platform could not have anticipated this rule change and changed their behavior accordingly. Further, borrowers entering the hardship plan around in a small window around the cutoff date of January 31, 2020 are unlikely to be different from each other. Indeed, as we show later in the paper, they are almost identical in several key observable metrics such as their income, FICO scores, occupation, and several other characteristics. Therefore, we have an empirical setting where two identical sets of borrowers enter into a hardship plan, but only one of them gets reported as being late on their payment. Therefore whether a borrower entered the hardship plan before or after the cutoff date provides us with an instrument for their credit score, independent of the fundamentals.

Our empirical research design exploits the effect of the reporting change on the borrowers' credit ratings and consequently its effect on their eventual default rate. We only consider borrowers who entered into hardship in a narrow window around the cutoff date of January 31, 2020. Those who entered the hardship plan in January 2020 and December 2019 are referred to as the control group. Borrowers entering hardship in February, 2020 and March, 2020 are referred to as the treated group. Therefore, the treated borrowers received an unexpected relief in the reporting of their late payments to the bureaus compared to the control borrowers.

The online lender responded quickly to the onset of the pandemic and the deterioration of the U.S. economy by introducing new COVID hardship plans in the middle of March. Our sample only includes borrowers who entered non-COVID hardship plans in early March. The sample selection criteria ensures that the two sets of borrowers entered the same hardship plan offered by the online lender, i.e., they were not treated differently due to the onset of the COVID-19 pandemic.

3 Data and Sample

Our proprietary data comes from an online peer-to-peer lender in the United States. In January 2020, the lender had over one million outstanding loans totaling about of \$18 billion in face value. More than 80% of the loans issued on their digital platform are used by borrowers for debt consolidation, and borrowers select either a 36 or 60-month loan amortization schedule. LendingClub collects extensive loan applicant information or origination that we utilize in answering our research question below. Summary statistics for continuous borrower characteristics can be found in Table 1 with categorical variables summarized in Figures 5 and 6.

In our sample, 199 borrowers entered hardship after the January 31, 2021 cutoff, forming the treatment group. The control group consists of 579 borrowers who began hardship in December 2019 and January 2020. We provide summary statistics broken down by the treatment status of the borrower. Panel A of Table 1 documents the key characteristics of borrowers who are in our control group, Panel B does the same for the treatment group. The average FICO score at origination is 694 for the treatment group, 695 for the control. The average borrower borrowed \$19,770 in the treatment group, \$18,310 in the control group. Debt-to-income ratio is 19.94% for the treatment group and 20.13% for the control. Both groups of borrowers are charged about 12.50% in interest costs. Loan interest rates are the maturity and month of issuance matched spread on the yield of three or five-year U.S. treasury notes from FRED.

Figure 4 presents kernel density plots of borrower characteristics by treatment group for all continuous borrower features. The heavy density overlap supports the claim that treatment assignment is as good as random. Two-sided Kolmogorov–Smirnov tests of all continuous variables in Figure 4 fail to reject the null hypothesis of equal distribution across treatment and control groups at a 5% level of significance.

Borrower observables for categorical features are presented in Figures 5 and 6. As shown in these figures, both groups are nearly identical in terms of home ownership status, whether their income has been verified or not, employment status, and broad occupation category (e.g., education sector, trucking, medical industry, and so on).

In addition to the data on borrower characteristics and outcomes, we collect additional data on the effect of the pandemic in the subsequent months by linking COVID cases and death counts to all state-by-month observations. The monthly data on COVID case counts and deaths by state are from the *New York Times* coronavirus GitHub page.[5] Monthly case and death counts are normalized by state populations from the United States Census Bureau.

4 Results

We present our main results in three broad steps. We first document the effect of the reporting change on FICO score, which is akin to the first stage regression of an instrumental variable model. Next, we present the reduced form estimate linking reporting change to default outcomes. Finally, we present the effect of FICO change due to the reporting change on default outcome, i.e., the two stage regression results. In subsequent tests, we provide evidence on the mechanism behind our findings.

4.1 Reporting change and FICO score:

Figure 1 provides the evolution of average FICO scores of borrowers in the treatment and control group, centered around the date they enter the hardship plan. Prior to entering the hardship plan, both groups of borrowers experience a decline in their FICO score. This is an expected outcome since these borrowers are experiencing financial distress. The treatment group has about a six point higher FICO score just before they enter the hardship status.

More important from the perspective of our empirical work, the groups show a parallel trend in the deterioration of their FICO scores. However, the trend changes across the two

[5]https://github.com/nytimes/covid-19-data

groups soon after they entered hardship, especially after 3 months. Credit bureau typically takes 3-4 months to update their credit score after a material change in a borrower's financial status. Hence the disproportionate decline in FICO after 3-months is consistent with the idea that the treated group of borrowers gets an extra effect solely due to the reporting change. Said differently, the control group of borrowers gets an additional boost in their credit score solely due to the reporting law change.

We estimate the following cross-sectional regression model to analyze the effect of reporting change on FICO scores of borrowers:

$$FICO_i^n = \alpha + \beta \times treatment_i + \epsilon_i \tag{1}$$

The dependent variable measures the change in borrower $i's$ FICO score n months after the entry into the hardship plan. The model is estimated separately for each $n \in (1, 10)$, i.e., for each month starting one month after the borrowers enter the hardship plan till 10 months after it. Table 2 presents the estimation results.

The intercept term of the regression estimates represents the decline in FICO score for the control group. The coefficient on the treatment variable represents the additional effect of credit reporting change on the treatment group. As shown in the Table, the intercept is negative and significant for every month. The steepest decline occurs between months 3 and 4, where the control borrowers experience a decline of 37 and 45 points in their FICO scores, respectively. The timing of decline is in line with the general practice of the credit bureaus who update credit scores with a lag of 2-3 months.

Interestingly, we find a consistently positive coefficient on the treatment variable, indicating that the treated borrowers received a better FICO score than the control borrowers despite having similar borrower and payment characteristics. Four months after the reporting change, the treated borrowers' FICO score is relatively higher by 16 points. We attribute this additional effect on FICO score to the reporting change. This is the differential credit score change that allows us to detect the causal effect of credit score on credit risk. As shown in the Table, the decline in FICO score as well as the difference across the treated and control borrowers is persistent over the next 10 months. Again, consistent with the practice that scores are updated with a delay of 2-3 months, the coefficient on the treatment variable increases considerably in months 3 and 4 after the hardship. By the 10th month, the control group of borrowers experiences a decline of 56 points in their FICO score; the treated borrowers remain at a higher FICO score of about 18 points. These are economically large effects since the standard deviation of FICO scores at the time of origination is about 29 points.

Table 3 presents the results of an alternative empirical specification linking the treatment status to FICO score changes. In this specification, we estimate a panel data model that allows us to control for unobserved borrower quality by including borrower fixed effects. The regression model is as follows:

$$FICO_{i,t} = \alpha_i + m_t + \beta_1 \times treatment_i + \beta_2 \times PostHardship_t + \beta_3 \times treatment_i \times PostHardship_t + \epsilon_{i,t} \tag{2}$$

The dependent variable is the FICO score of borrower i in month t. α_i and m_t are the borrower and time (month) fixed effects, included in the model to soak away unobserved heterogeneity based on individual characteristics or changes in the macroeconomic environment. $PostHadship$ is an indicator variable that equals one after a borrower enters the hardship status, zero otherwise. The model is estimated with borrower-month data including observations from ten months before to ten months after each borrower starts hardship.

As documented in column (2) of Table 3, the treated borrowers have a 15-point higher FICO score as compared to the control borrowers in the post-hardship period. In Column (3), we include time fixed effects at the month-year level as well. The coefficient on the interaction term remains practically unchanged: treated borrowers have 16.18 points higher FICO score in the post-hardship period as compared to the corresponding decline for the control borrowers. These results show that our findings are not driven by any differential impact of economy wide changes, such as the effect of COVID pandemic or macroeconomic interventions, that can differentially affect the treated and control borrowers. In sum, the difference in FICO scores across the treated and control borrowers cannot be attributed to any borrower specific effects, nor can it be attributed to a time specific effect.

4.2 Reporting change and Subsequent Default:

We begin our main analysis by analyzing the effect of the treatment status of a borrower on credit risk: how frequently they defaulted on their loans. Figure 2 plots the cumulative default rate for treatment and control groups in the months after borrowers began their hardship programs. Since both groups are in distress, their cumulative default rate goes up substantially over time. In the first 4 months after the entry into the hardship plan, the cumulative default rate is roughly the same across the two groups. Beginning with the fifth month, there two rates diverge, with the treatment group exhibiting a lower default rate than the control group. By the end of month 10, borrowers in the treatment group defaulted at a rate of 38% compared to a cumulative default rate of about 61% for the control group. The unconditional default rate for borrowers who enter a hardship plan when they were introduced in 2017 through 2019 is about 60%. Therefore, the treatment status of a borrower has an economically large effect on their eventual default outcome. These findings are the reduced form estimate in an instrumental variable regression setting with the treatment status as the instrument for credit score.

To formalize our analysis of how credit reporting affects credit risk, we specify a logistic regression model of borrower default on the treatment status and borrower observables as follows:

$$P(d_i = 1) = \frac{e^{\alpha+\beta_1 \times treatment_i + \beta_2 X_i + \epsilon_i}}{1 + e^{\alpha+\beta_1 \times treatmean_i + \beta_2 X_i + \epsilon_i}} \tag{3}$$

$P(d_i = 1)$ equals one if borrower i has defaulted within ten months of starting a hardship payment program, zero otherwise. The model is estimated with cross-sectional data with one observation for each borrower. X_i is a series of control variables that measure borrower and loan characteristics. Specifically, we include all borrower traits that are described in Table 1 for summary statistics and figures 5 and 6 for categorical variables. Specifically, the model includes the borrowers' original FICO score, income, debt-to-income ratio, whether the income has been verified or not, whether the borrower owns a home, occupation category, loan maturity and so on.

The results are presented in table 4. Column (1) does not include any control variables and shows that the treated borrowers have an odds ratio of 0.387 for defaulting within 10 months compared to the control borrowers. The effect is statistically significant at the 1% level. Column (2) includes an extensive set of control variables, and Column (3) includes state fixed effects as well. The odds ratio for the treated borrowers remains stable in the range of 0.37-0.39, indicating that the effect we document are independent of other control variables or state specific factors, consistent with our earlier argument that the treatment status is an exogenous shock to an otherwise identical set of borrowers. The odds ratio translates into a cumulative default probability 23 percentage points lower for the treated borrowers compared to the control borrowers. Table 5 presents the same specification using a linear probability model, and the results are unchanged.

Treated borrowers are defaulting both unconditionally and conditionally at higher rates in the months following entering hardship. To account for time dynamics in the months leading to default, we utilize a dynamic logistic discrete time hazard rate model to account for natural variation in borrower distress throughout the life of an average loan. The monthly borrower payment schedule makes a discrete model more appropriate than continuous time methods such as a Cox proportional hazard model. Similar to Carmichael (2014), we model borrower borrowers as follows:

$$d_{i,s,t} = \alpha + \beta_1 treat_i + \beta_2 X_i + \beta_3 RV_{i,t} + \beta_4 Local\_Controls_{s,t} + \epsilon_{i,s,t} \quad (4)$$

$d_{i,s,t} = 1$ if borrower i defaults in state s at time t and is otherwise 0. Borrowers leave the sample upon default. Borrower controls X_i are identical to covariates used in equation 3. $RV_{i,t}$ stands for running variables, i.e., time-varying borrower level variables, measured for borrower i at time t. We use two variables to capture this effect: (a) the number of months since a borrower entered the hardship, and (b) each borrower's percentage of loan completed by time t. Thus, the running variables account for the effect of the loan balance and time-since-hardship on default outcomes. $Local\_Controls_{s,t}$ capture time-varying state-specific control variables; specifically, we include controls for the number of COVID cases and deaths by time t in state s to soak away any local economic effect due to the pandemic.

Table 6 presents the estimates of specification 4. Our findings remain similar: borrowers in the treated group have an odds ratio of 0.48-0.51 of default compared to the control group, depending on the model specification. The effect converts into a monthly default rate of about 0.35 percentage points lower for treated borrowers relative to control borrowers.

The dynamic model allows us to separate out the effect of the duration of default, specifically months since the hardship began. As shown in the Table, default rates increase with time. We also include the squared term of months-since-hardship in the model to account for any nonlinear effect, denoted by the variable $MonthsSinceHardship^2$. Our key finding is not sensitive to the inclusion or exclusion of these variables. Our other results show that borrowers with higher interest rates and larger loan amount default at a higher rate; however, none of these control variables change the coefficient on our variable of interest in any meaningful way. In unreported linear probability models of default with time and borrower fixed effects, the economic interpretation and large statistical significance are qualitatively unchanged. Overall, we show that the treated borrowers default at a much lower rate than the control borrowers.

While our treated and control borrowers are almost identical on borrower, loan, and repayment history characteristics before they enter the hardship plan, they differ slightly in their proximity to the COVID shock. Specifically, the treated borrowers went on the hardship plan in February and March, whereas the control group did so in January and the previous December. Thus their hardship occurred on average 2 months closer to the pandemic than the control borrowers. If the proximity to the pandemic shock makes a distressed borrower more likely to declare bankruptcy then the treated borrowers should have a higher default rate. Our results find just the opposite, mitigating these concerns. Further, our base models already control for time since a borrower enters hardship, making it unlikely that our results are driven by any differential effect that might come from proximity of a borrower's hardship to the onset of the pandemic.

Yet, to mitigate any concern about the proximity effect, we make use of the sample of non-hardship borrowers who are identical to the borrowers in our sample. The key idea of this research design is to exploit information in the time-varying differences in the baseline default rate for all borrowers, whether they enter the hardship plan or not. For each borrower in the sample, i.e., each borrower who go on the hardship plan, we find a non-hardship borrower from the online lender's database of nearly one million loans who are nearly identical based on a propensity score matching algorithm. Our propensity score matching algorithm uses the following key variables as the covariates: interest rate, income, loan amount, loan grade, state, and time remaining on the loan.

With the set of matched borrowers, we estimate the following logistic regression model:

$$P(d_i = 1) = \alpha + \beta_1 hardship_i + \beta_2 PostJan_i + \beta_3 hardship_i \times PostJan_i + \beta_4 X_{i,t} + \epsilon_{i,s,t} \quad (5)$$

$hardship_i$ equals one for all the borrowers who enter the hardship plan and hence form our main sample. It is set to zero for the matched sample of non-hardship borrowers. Thus, β_1 measures the average difference in default likelihood of borrowers who enter the hardship plan with those who do not. $PostJan_i$ equals one for borrowers who went on hardship after the reporting shock, as well as their matched counterparts who did not enter the hardship plan. Therefore, β_2 captures the difference in the 10-month default rate of an average borrower based on the proximity to the pandemic. The inclusion of this variable allows us to soak away the "proximity to the pandemic" effect. The interaction term is the estimate of interest. It gives us the estimate of changes in default rate for hardship borrowers who entered hardship after the reporting change compared to those who entered before the change, after separating out the corresponding effect for the non-hardship borrowers.

Results are provided in Table 7. As expected, borrowers on the hardship plan default at a much higher rate than the non-hardship borrowers. The coefficient estimate on the interaction term shows that the hardship borrowers have odds ratio of 0.26-0.28 if they entered the hardship after the reporting shock. Therefore, our results are unlikely to be driven by proximity to the pandemic. We repeat our analysis with a linear probability model and obtain similar results.

4.3 FICO score change and default rate

We now relate the change in credit score that arises due to the treatment status of a borrower to their default probability. Under the exogeneity assumption that the treatment status only affects a borrower's credit risk through its effect on the FICO score, we are able to tease out the causal effect of credit score on credit risk.

Our model is cross-sectional, with one observation for each borrower in the sample. In the first stage regression, we instrument changes in the FICO score of a borrower with their treatment status. We pick the change in FICO score in the first four months, i.e., from one month before the hardship to four months after the hardship, as our benchmark case. We focus on this time window since there is typically a delay of 2-3 months by the credit bureaus in incorporating the effects of borrowers' payment history on scores. We use the log transformed value of changes in FICO score over this time window as our dependent variable in the first stage regression model. Further, we standardize the dependent variable, by subtracting the sample mean and scaling the difference by the sample standard deviation, for easier economic interpretation. Our results remain similar for specifications that consider changes in FICO score over other reasonable time windows such as up to three or five months after the hardship.

Regression results are provided in Column (1) of Table 8. Consistent with the earlier findings, treated borrowers have a significantly higher change in FICO score over the four month period. The coefficient estimate of 0.303 shows that the change in FICO score was about 1/3rd standard deviation higher for the treated borrowers. The first stage regression model has an F-statistics of 13.76, showing that our instrument is strong.

Using the fitted value of the FICO change from the first stage regression, we estimate the effect of credit risk changes on default rate. Column (2) presents the base case result. One standard deviation decrease in FICO score change causes an increase of 77% in default rate. The estimate translates into a 25% lower default probability for the treated borrowers. Columns (3) and (4) of the Table provide alternative specifications, controlling for a host of borrower characteristics and state level fixed-effects. The coefficient estimate remains similar. Depending on the model specification, the effect of one standard deviation decrease in FICO score ranges from 56% to 68%. Overall, there is strong evidence in support of a causal link from credit rating to credit risk. The economic magnitude of the effects are large as well.

These findings have important implications for the cost of credit bureau reporting errors to the affected borrowers. In a 2012 audit study of $1,001$ borrowers reviewing $2,968$ credit reports, Commision (2022) found that 20% of sampled credit reports had a "material error", and 8.5% of credit reports had mistakes large enough for borrowers to experience drops in FICO score by 10 or more points. 5% of household credit reports had mistakes so severe to cause decreases in FICO credit scores by more than 25 points. More recently, a reporting error by *Equifax* resulted in millions of borrowers having incorrect credit scores sent to auto, home, and credit card lenders. Several thousand households experienced erroneous changes of 25 points or more.[6] Our findings show that these borrowers are likely to experience larger cost in terms of their future default probability due to reporting mistakes unrelated to their fundamental credit quality.

4.4 Loan Repayment

We now focus on the economic channel behind our findings. Higher FICO scores can affect future outcomes in several ways. Such borrowers can have higher access to financing as well as cheaper cost of borrowing compared to otherwise identical low FICO score borrowers. In addition, these borrowers can also benefit in terms of securing a job or obtaining a rental, which in turn can improve their future income. Our dataset does not allow us to tease out the effect of score change on non-financing outcomes such as access to rental or new jobs. However, we are able to observe the payment history of each borrower in our sample over time. If the improved FICO score allows the treated borrowers to obtain future financing at an attractive term, then we expect them to pay off their balances at a faster pace. For example, a borrower may be able to obtain a higher limit on their existing credit card or obtain a new credit card if their FICO score is higher. Access to these sources of financing will allow them to pay down their loan balance on the online lender's account at a relatively faster rate. Our empirical test is motivated by this idea.

We obtain the outstanding principal balance data for every borrower in the sample starting ten months before they enter the hardship plan and ending ten months after it. Figure 3 plots the evolution of the remaining balance over time across the treated and control sample. Before entering the hardship plan, both groups show a gradual reduction in loan balance in a parallel manner. Soon after they enter the hardship plan the slope of the loan balance line, which captures the repayment made by the borrower, flattens. This is expected since these borrowers are on a reduced payment plan by construction. More interestingly, the slope of the outstanding balance line begins to diverge significantly four months after the entry into

[6] https://www.wsj.com/articles/equifax-sent-lenders-inaccurate-credit-scores-on-millions-of-consumers-11659467483

the hardship plan. The treated borrowers pay off their loans at a much higher rate than the control borrowers. These results support the notion that the treated borrowers are able to access external financing relatively easily as compared to the control borrower.

More formally, we estimate the following regression model with the panel data for loan balance for each borrower every month in (-10,+10) month window:

$$bal_{i,t} = \alpha_i + m_t + \beta_1 \times treatment_i + \beta_2 \times hardship_t + \beta_3 \times treatment_i \times hardship_t + \epsilon_{i,t} \quad (6)$$

$bal_{i,t}$ is the amount of loan balance left outstanding at month t for borrower i. We include borrower fixed effects to account for any person specific features affecting our results. Since each borrower has one loan in the sample, the borrower fixed effects also soak away the time-invariant features of the loan contract such as its maturity and interest rates on repayment behavior. If the treated borrowers have better access to financing, then we expect them to have a lower balance going forward i.e., a negative coefficient on the interaction term.

Table 9 presents the results. Column (3) presents the results with borrower and time fixed effects and shows that the treated borrower had an average balance that is \$947.80 lower than the control borrowers in the post hardship period, consistent with the view that they face lower financing constraints compared to the control borrowers. In our sample, the average loan balance is about \$20,000. Therefore the extra balance of the control borrowers amounts to about 5% of the initial loan balance, which is an economically meaningful difference between the two groups of borrowers.

We supplement the above test with an analysis of the percentage change in loan balance as the dependent variable in the regression model. The test allows us to assess the differential rate of repayment across the two groups, which in turn allows us to comment on the economic magnitude of the effect of treatment status on repayment rates more precisely. Table 10 reports the results. Since the dependent variable is the percentage change in loan balance, the coefficient estimates represent the negative of repayment rate. As shown in Column (3) of the Table, the treated borrowers are able to pay down their debt at 0.87% higher rate compared to the control borrower. The coefficient estimate on the post-hardship indicator variable represents the repayment rate by the borrowers during the post-hardship period as compared to before. As expected, borrowers repay their debt at 2.64% lower rate in the post-hardship period. Therefore the treatment status changes the repayment rate by a meaningful 33% (i.e., 0.87/2.64).

Said differently, the treated borrowers paid their loan balances at a 0.873% higher rate on a monthly basis. Overall, our findings provide evidence supporting the claim that FICO scores by themselves can allow borrowers to obtain external funding even when their fundamental risk factors remain the same. In turn, better rating can improve credit risk going forward.

4.5 Cross-sectional results

We now provide some cross-sectional evidence relating borrower characteristics to future outcomes. If the advantage of higher FICO score comes from the coordinated beliefs of the lenders that the borrower will continue to obtain future financing from other sources, then our results should be stronger for the treated borrowers with relatively better ratings before the distress. Said differently, for such borrowers, losing a high credit rating is relatively more costly compared to a borrower with lower scores.

To implement this test, we estimate the base regression model similar to equation 3, but we include an interaction term between the treatment status and dummies for higher quality borrowers. We use three measures of borrower quality: (a) their FICO score one month before hardship, (b) whether the borrower was rated to be in the top rating group by the lender at origination or not, and (c) whether the borrower paid a relatively lower interest rate or not. For the FICO and interest rate-based measures, we consider borrowers in the top quartile of FICO pre-hardship or the bottom quartile of interest rate as the highest quality borrowers.

Table 11 presents the results. The odds ratio for the estimated coefficient on the interaction of the treatment status and highest quality indicator variable is less than one. The effects are economically large in all three specifications. For example, the odds ratio on the interaction term is 0.582 when we measure quality based on the interest rate paid by the borrower. This is an economically large effect, when seen in the light of the unconditional odds ratio of about 0.38 on the treatment status in the base model of Table 4. The result, however, is statistically weak. A similar pattern holds for the other two measures of quality. Our measure of borrower quality is imperfect. Hence, we create an additional test in which a borrower is considered to be of the highest quality if they meet all three criteria: high pre-hardship FICO score, high internal loan grade, and low interest rate. Results are provided in the last column of the Table and show a large economic effect with an odds ratio of 0.239 and p-value of 0.1147.

Higher credit scores can facilitate better employment outcomes and they can also help with securing rental housing. We do not have data on each individual borrower's employment history or rental decisions. Thus we are limited in teasing out the effect of the law change on these outcomes. However, we do have information on whether a borrower is a renter or a property owner at the time of loan origination. We also observe how long the borrower has been employed at the time of loan origination. In unreported tests, we do not find any meaningful difference in default outcomes across renters versus homeowners and varying employment lengths We also do not find any meaningful difference in outcome based on the occupation of the borrowers.

Overall, we find a consistent pattern that the lower default rate occurs due to improved FICO score and better access to financing for the treated borrowers.

5 Conclusion

We show that borrowers' credit score affects their subsequent default rate, even if the score change is unrelated to changes in fundamental factors that affect credit risk. Our findings highlight the causal effect of credit rating on credit risk: higher credit rating, as measured by the borrower's FICO score, causes them to default at a lower rate. The standalone effect of rating on default risk is economically large. We show that a higher credit rating allows the borrower to obtain more external financing, which in turn allows them to pay down their debt at a faster rate compared to an otherwise identical borrower with a lower rating. The causal effect of rating on risk is more pronounced for relatively higher quality borrowers, consistent with the view that lenders coordinated beliefs about a borrower's credit risk result in a causal link from rating to risk. At a practical level, our findings suggest that credit reporting errors can impose large economic costs on the affected borrowers.

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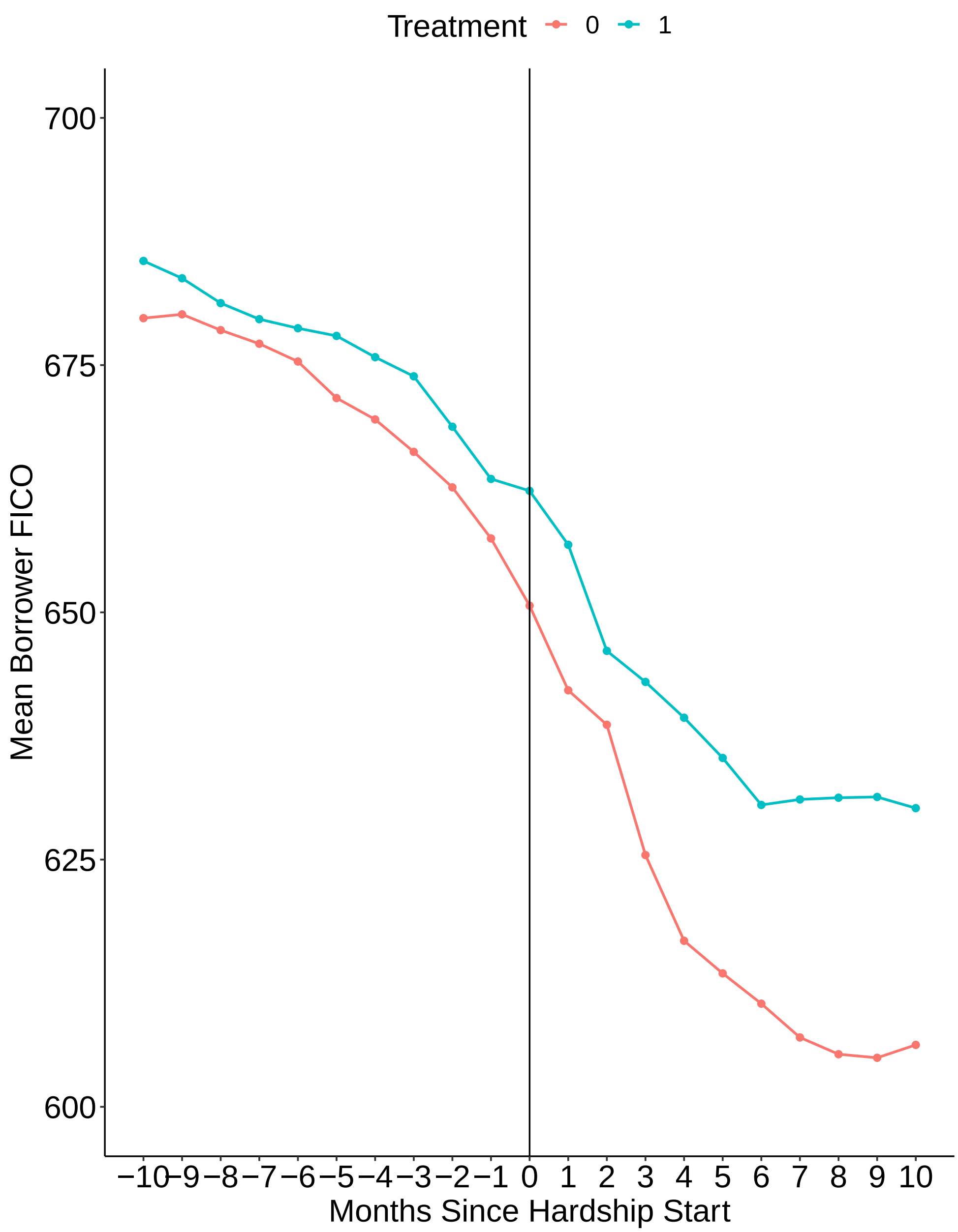
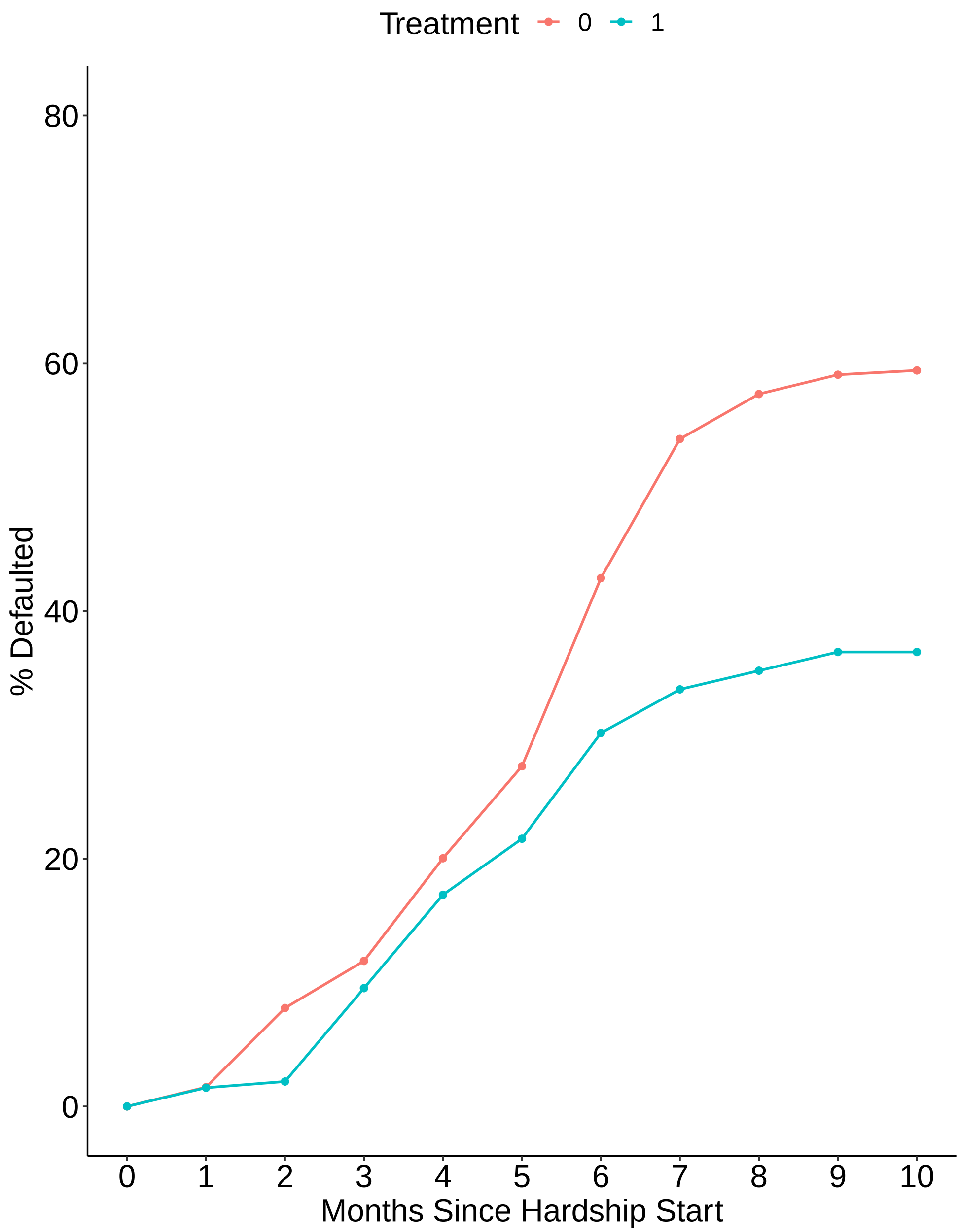
A Figures

Figure 1: Time Series of Mean Borrower FICO by Treatment Status



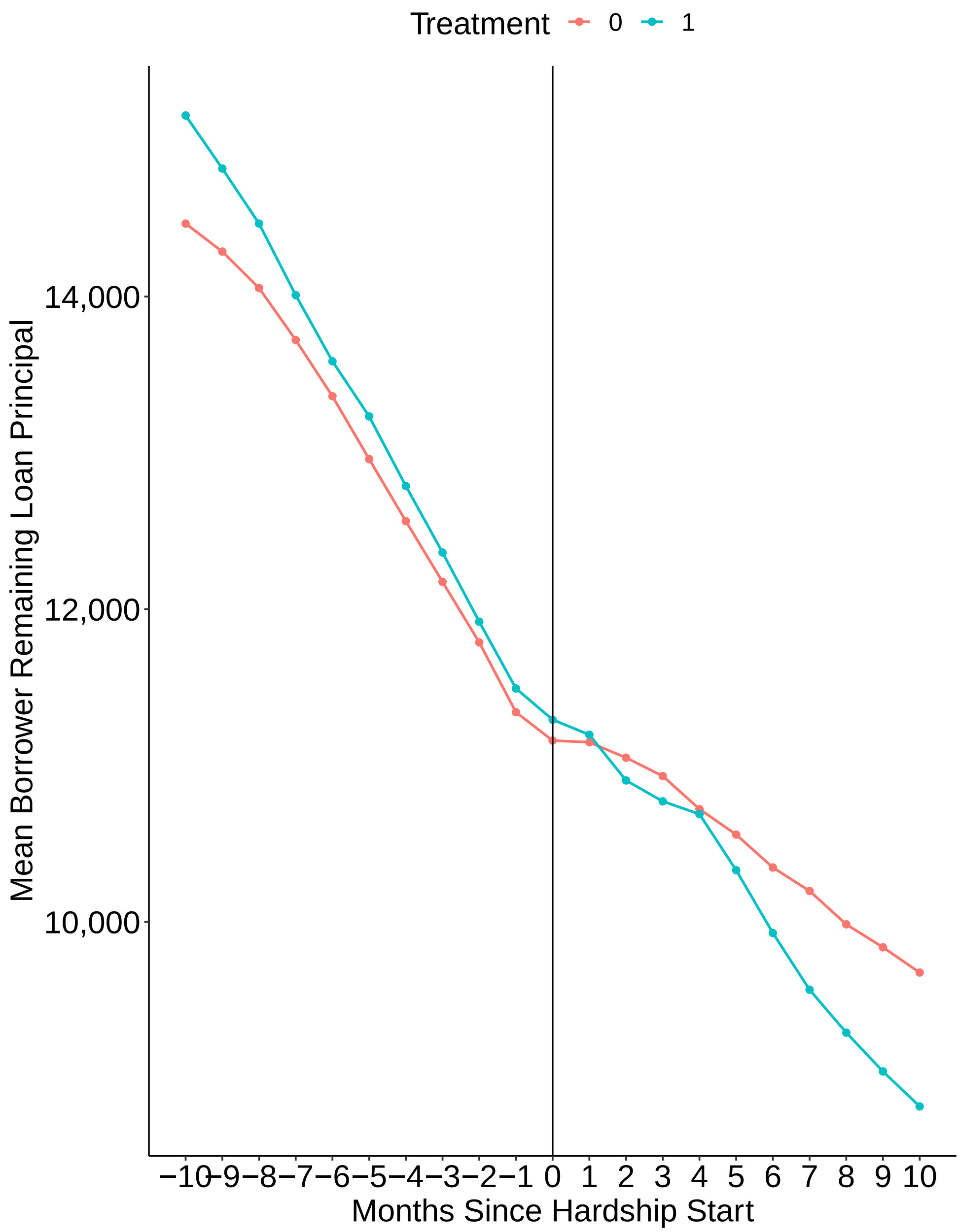
Notes: Figure 1 plots a time series of mean borrower FICO score by Treatment status. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. The x-axis is the number of months before and after a borrower entered hardship, and the y-axis plots the average FICO score by treatment group. Borrowers who default are retained in the sample at their last known FICO score. FICO scores are not always retrieved by the lender each month, so we use the borrower's previous month's FICO score for missing observations.

Figure 2: Cumulative Default Rate by Borrower Treatment Status



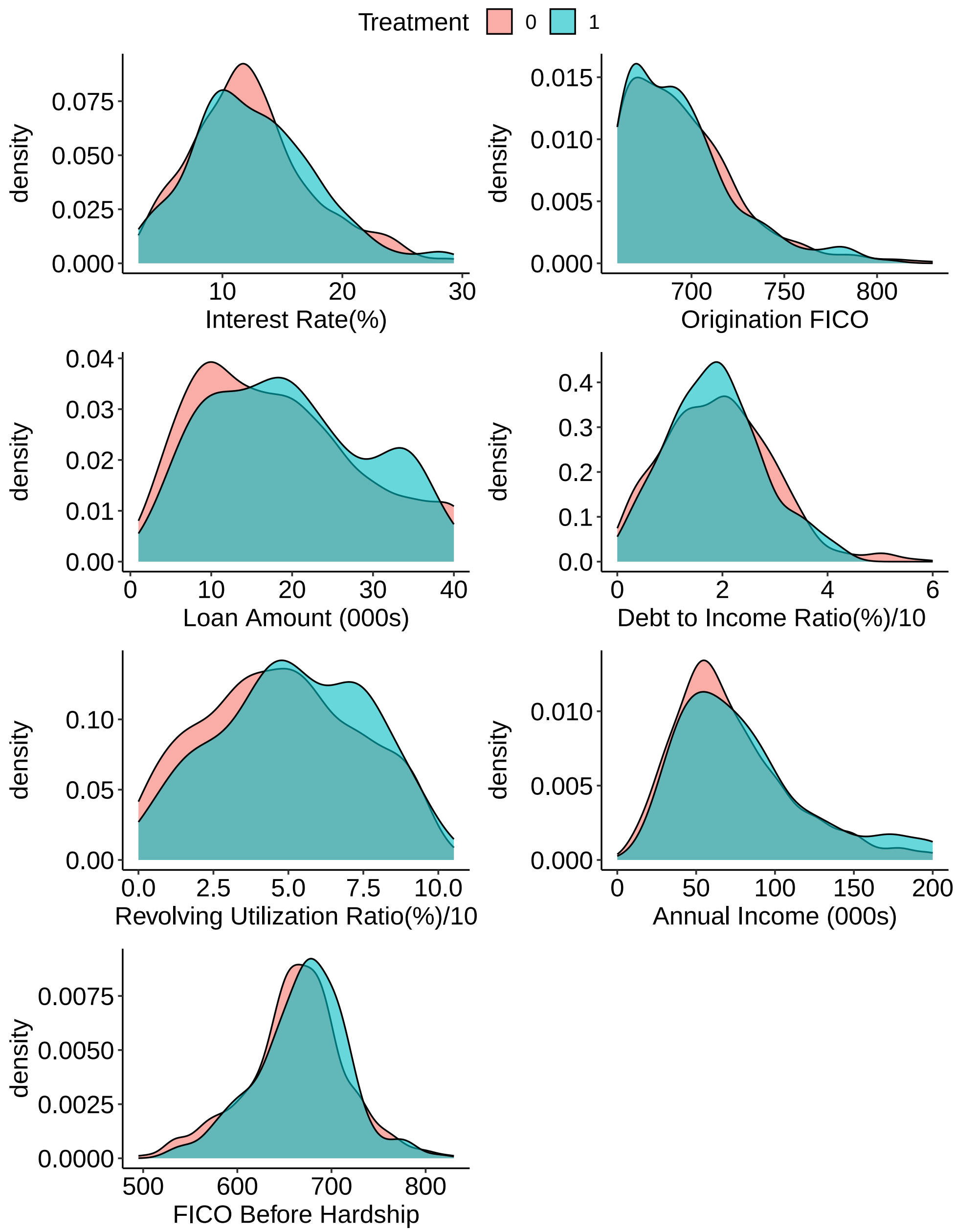
Notes: Figure 2 plots the cumulative loan default percentage of the treated and control borrowers of our sample. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. The x-axis is the number of months since a borrower entered hardship, and the y-axis plots the cumulative percentage of borrowers who have defaulted at x months since their hardship origination date.

Figure 3: Time Series of Mean Borrower Loan Principal Remaining by Treatment Status



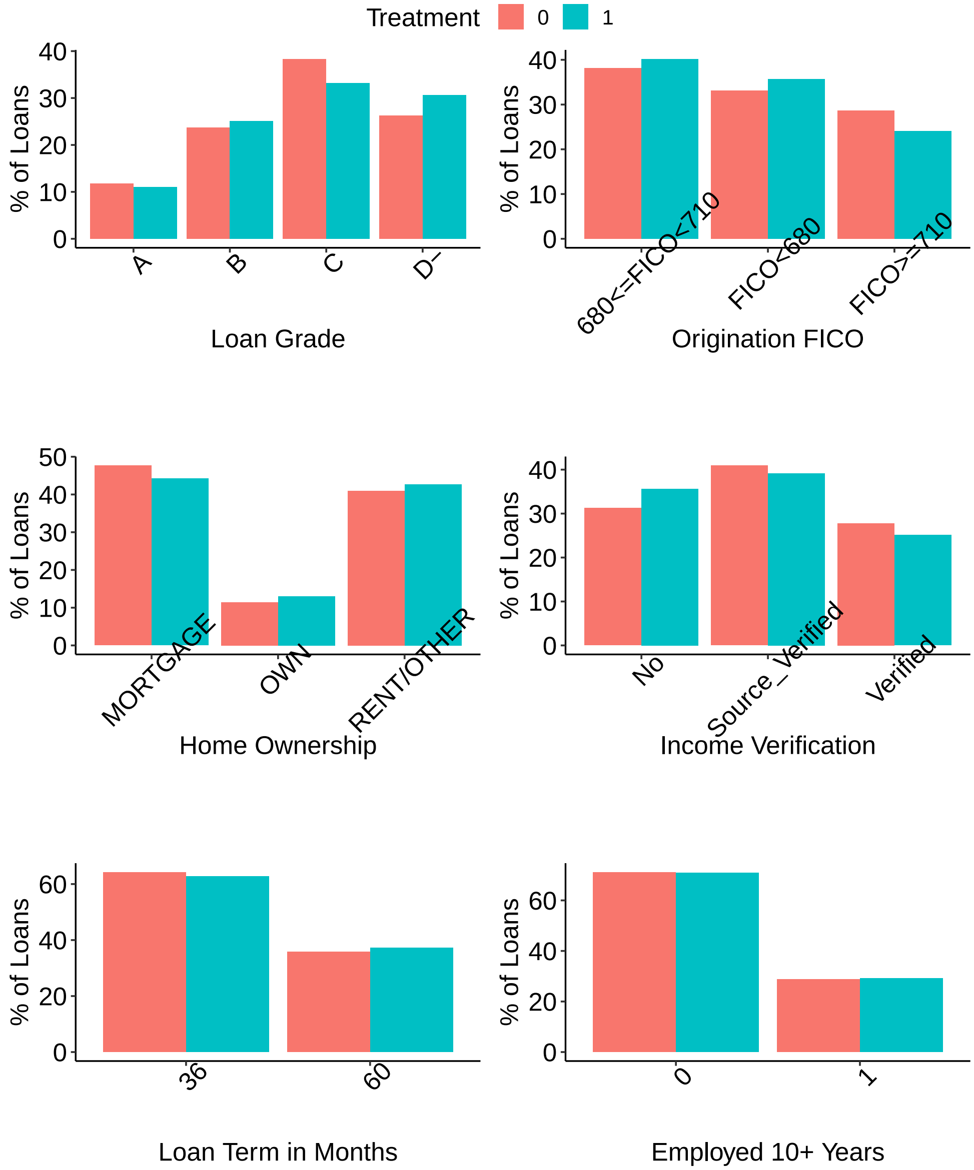
Notes: Figure 3 plots a time series of mean borrower loan principal remaining by treatment status. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. The x-axis is the number of months before and after a borrower entered hardship, and the y-axis plots the average remaining loan principal by treatment group. Borrowers who default are retained in the sample at their last known principal balance.

Figure 4: Continuous Borrower Covariates Kernel Density Plots by Treatment Status



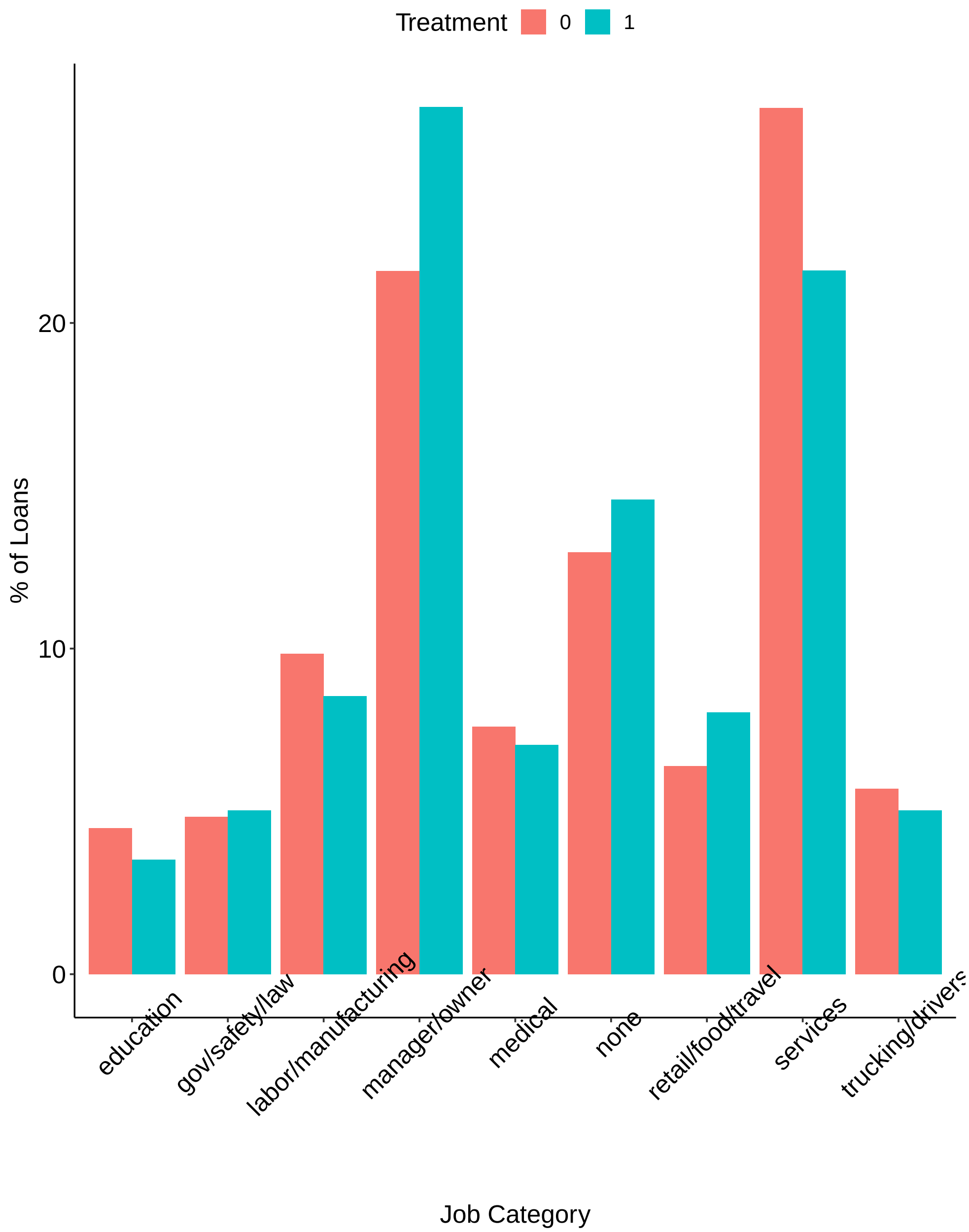
Notes: Figure 4 plots kernel density estimations for continuous borrower observable covariates by treatment status. Variables are defined and explained in Table 1.

Figure 5: Categorical Borrower Covariate Frequency Plots by Treatment Status



Notes: Figure 5 plots categorical borrower observable traits by treatment status. The x-axis is the characteristic of interest, and the y-axis is the percentage of borrowers by treatment status in each respective characteristic category.

Figure 6: Job Category Frequency Plot by Treatment Status



Notes: Figure 6 plots the percentage of loans within each borrower job category by treatment status. Borrowers list their job titles when applying for a loan, and we have subjectively categorized borrowers into the categories listed. Borrowers labeled as "none" did not clearly fall into one of our chosen job categories.

B Tables

Table 1: Summary Statistics

Panel A: Control Covariates						
Statistic	N	Mean	St. Dev.	Min	Median	Max
Interest Rate(%)	579	12.41	5.03	3.20	11.79	29.30
Origination FICO	579	695.19	29.82	660	690	830
Joint Application	579	0.14	0.35	0	0	1
Loan Amount (000s)	579	18.31	10.23	1.00	16.00	40.00
Debt to Income Ratio(%)	579	20.13	13.45	0.00	19.30	153.39
Revolving Utilization Ratio(%)	579	47.33	25.63	0.00	47.50	101.60
Employed 10+ Yrs	579	0.29	0.45	0	0	1
Annual Income (000s)	579	77.11	49.12	8.82	65.00	400.00
FICO Before Hardship	579	662.56	53.12	495	665	830
Panel B: Treatment Covariates						
Interest Rate(%)	199	12.64	5.17	3.00	12.03	28.92
Origination FICO	199	694.37	29.41	660	690	805
Joint Application	199	0.17	0.37	0	0	1
Loan Amount (000s)	199	19.77	9.76	2.00	20.00	40.00
Debt to Income Ratio(%)	199	19.94	14.00	0.64	18.41	142.68
Revolving Utilization Ratio(%)	199	51.95	24.78	0.10	50.60	105.20
Employed 10+ Yrs	199	0.29	0.46	0	0	1
Annual Income (000s)	199	89.82	64.70	16.00	72.00	500.00
FICO Before Hardship	199	668.77	48.29	535	675	815

Notes: Table 1 presents summary statistics for our sample of borrowers from LendingClub. The sample contains all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. Interest Rate is the spread on each borrower's loan interest rate relative to the yield on a maturity-matched U.S. treasury note of the month of loan issuance. Origination FICO is the borrower's FICO score at loan origination. Joint Application is an indicator equal to one when the loan has multiple applicants. Loan Amounts (000s) is the size of each loan in thousands of dollars. Debt to Income Ratio(%) is a ratio calculated at origination using the borrower's total monthly debt payments on the total debt obligations, excluding mortgage and the requested loan, divided by the borrower's self-reported monthly income. Resolving Utilization Rato(%) is the amount of credit the borrower was using relative to all available revolving credit at origination. Employed 10+ Years is a dummy for borrowers who have been employed for 10 years or more. Annual Income (000s) is the borrower's self-reported annual income in thousands of dollars. FICO Before Hardship is the borrower's FICO score 1-month before entering hardship.

Table 2: OLS Change In FICO n Months From One Month Before Hardship Start

	Dependent variable:									
	Change in Borrower FICO									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	8.584***	1.342	11.376***	16.426***	15.651***	13.965***	17.934***	19.806***	20.244***	17.808***
	(2.767)	(2.737)	(3.516)	(3.556)	(3.910)	(4.788)	(4.775)	(5.007)	(5.307)	(5.521)
Constant	−20.519***	−24.005***	−37.180***	−45.848***	−49.144***	−52.206***	−55.623***	−57.318***	−57.682***	−56.375***
	(1.461)	(1.679)	(2.209)	(2.779)	(2.822)	(2.919)	(3.169)	(3.200)	(3.312)	(3.291)
Observations	777	777	777	777	777	777	777	777	777	777
R^2	0.013	0.0002	0.011	0.017	0.014	0.010	0.016	0.019	0.020	0.015
Note:										*p<0.1; **p<0.05; ***p<0.01

Notes: Table 2 shows the estimated coefficients of an ordinary least squares model of specification 1 of the change in each borrower i's FICO score one month before entering hardship to n months after hardship on borrower i's treatment status. Each column represents the number n months after hardship started for the outcome variable Change in Borrower FICO. All borrower data comes from our proprietary online lender. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. Borrowers who default are retained in the sample at their last known FICO score. FICO scores are not always retrieved by the lender each month, so we use the borrower's previous month's FICO score for missing observations. Standard errors are clustered by borrower state residence.

Table 3: OLS Panel Regression of Borrower FICO

	Dependent variable:		
	Monthly FICO		
	(1)	(2)	(3)
Origination FICO	0.542***		
	(0.065)	(0.000)	(0.000)
Treatment	6.065		
	(4.253)	(0.000)	(0.000)
Post-Hardship Dummy	−52.825***	−52.904***	−10.827***
	(2.213)	(2.213)	(1.876)
Treatment * Post-Hardship Dummy	14.828***	14.685***	16.183***
	(3.756)	(3.673)	(3.939)
Constant	292.971***		
	(45.164)		
Borrower Fixed Effects		x	x
Month Fixed Effects			x
Observations	16,227	16,227	16,227
R^2	0.225	0.691	0.719
Note:			*p<0.1; **p<0.05; ***p<0.01

Notes: Table 3 shows the estimated coefficients of an ordinary least squares model of specification 2 of borrower i monthly FICO at time t on FICO at origination, Treatment status, a Post-Hardship Dummy variable equal to one for the months after each borrower's hardship start date, and an interaction between Treatment and Post-Hardship Dummy. Column 2 includes borrower i fixed effects, and Column 3 includes month t fixed effects. All borrower data comes from our proprietary online lender. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. We use a time period of ten months before and after each borrower began hardship. Borrowers who default are retained in the sample at their last known FICO score. FICO scores are not always retrieved by the lender each month, so we use the borrower's previous month's FICO score for missing observations. Standard errors in parentheses are clustered by borrower state residence.

Table 4: Logit Model Cumulative Default within 10 Months

	Dependent variable:		
	Default		Default
	logistic		*conditional logistic*
	(1)	(2)	(3)
Treatment	0.387***	0.392***	0.376***
	(0.169)	(0.180)	(0.191)
Interest Rate		1.131***	1.115***
		(0.039)	(0.042)
Loan Amount (000s)		1.020**	1.015
		(0.009)	(0.009)
Constant	1.562***	29.369**	
	(0.085)	(1.678)	
Conditional Logit by State			x
Extensive Borrower Controls		x	x
Observations	778	778	778
Note:	*p<0.1; **p<0.05; ***p<0.01		

Notes: Table 4 shows the estimated odds ratio coefficients of a logistic model of specification 3 of borrower default within ten months of entering a hardship plan on borrower i treatment status. Standard errors are in parentheses. The outcome variable is equal to one if the borrower has defaulted within ten months of their hardship start date. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. Our sample time period is ten months after hardship starts for every borrower. All borrower data come from our proprietary online lender, and U.S. treasury yields are from the Federal Reserve Economic Database. Columns 2 and 3 report coefficients while including all continuous covariates from Table 1 along with categorical borrower traits in Figures 5 and 6. Column 3 presents coefficient estimates of conditional logistic specification at state level s.

Table 5: Linear Probability Model Cumulative Default within 10 Months

	Dependent variable:		
	Default		
	(1)	(2)	(3)
Treatment	-0.233^{***}	-0.214^{***}	-0.223^{***}
	(0.043)	(0.046)	(0.051)
Interest Rate		0.026^{***}	0.023^{***}
		(0.009)	(0.008)
Loan Amount (000s)		0.004^{***}	0.003^{**}
		(0.002)	(0.002)
Constant	0.610^{***}	1.251^{***}	
	(0.022)	(0.309)	
State Fixed Effects			x
Extensive Borrower Controls		x	x
Observations	778	778	778
R^2	0.042	0.106	0.167
Adjusted R^2	0.040	0.069	0.074
Note:	*p<0.1; **p<0.05; ***p<0.01		

Notes: Table 5 shows the estimated coefficients of an ordinary least squares model with the identical dependent and covariates as table 4. The outcome variable is equal to one if the borrower has defaulted within ten months of their hardship start date. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. Our sample time period is ten months after hardship starts for every borrower. All borrower data come from our proprietary online lender, and U.S. treasury yields are from the Federal Reserve Economic Database. Columns 2 and 3 report coefficients while including all continuous covariates from Table 1 along with categorical borrower traits in Figures 5 and 6. Column 3 presents coefficient estimates while including state fixed effects. Standard errors in parentheses are clustered by borrower state residence.

Table 6: Dynamic Default Logit Model

	Dependent variable:		
	Default		Default
	logistic		*conditional logistic*
	(1)	(2)	(3)
Treatment	0.492***	0.509***	0.485***
	(0.130)	(0.150)	(0.171)
Months Since Hardship	3.012***	3.238***	3.301***
	(0.081)	(0.122)	(0.123)
Months Since Hardship^2	0.910***	0.908***	0.908***
	(0.007)	(0.010)	(0.011)
Payment Relief		1.139	1.139
		(0.229)	(0.230)
Interest Rate		1.073***	1.074***
		(0.023)	(0.025)
Loan Amount (000s)		1.016***	1.014**
		(0.006)	(0.006)
Constant	0.007***	0.061***	
	(0.217)	(1.009)	
Conditional Logit by State			x
Extensive Borrower/Covid Controls		x	x
Observations	7,183	7,183	7,183
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 6 shows the estimated odds ratio coefficients of a dynamic logistic model of specification 4 of borrower i default on borrower Treatment status. Standard errors are in parentheses. The outcome variable is equal to one in the month a borrower defaults, and borrowers who have defaulted leave the sample. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. All borrowers are followed for up to 10 months after entering hardship. Borrower data comes from our proprietary online lender, and U.S. treasury yields are from the Federal Reserve Economic Database. Data on covid cases and deaths by state are from *The New York Times* GitHub Repository. The running variable in the dynamic logistic regression is Months Since Hardship. Payment Relief is a variable equal to to one for the three months after hardship begins when the borrower is allowed to make reduced loan payments. Columns 2 and 3 report coefficients while including all continuous covariates from Table 1, categorical borrower traits in Figures 5 and 6, and covid case and death counts by state s in month t. Column 3 presents coefficient estimates of conditional logistic specification at state level s.

Table 7: Cumulative Default with Non-Hardship Borrowers

	Dependent variable:		
	Default		Default
	logistic		*conditional logistic*
	(1)	(2)	(3)
Hardship Dummy	23.559***	28.239***	27.556***
	(0.192)	(0.203)	(0.205)
Post January Dummy	1.409	1.356	1.375
	(0.306)	(0.312)	(0.316)
Hardship Dummy * Post January Dummy	0.275***	0.276***	0.261***
	(0.350)	(0.358)	(0.360)
Interest Rate		1.088***	1.081**
		(0.033)	(0.034)
Loan Amount (000s)		1.021**	1.017**
		(0.008)	(0.008)
Constant	0.066***	0.011***	
	(0.172)	(0.778)	
Conditional Logit by State			x
Extensive Borrower Controls		x	x
Observations	1,556	1,556	1,556
Note:			*p<0.1; **p<0.05; ***p<0.01

Notes: Table 7 shows the estimated odds ratio coefficients of a logistic model of specification 5 of borrower default within ten months of entering a hardship plan or ten months from a non-hardship borrower's matched hardship date. Standard errors are in parentheses. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic along with a propensity score matched borrower who was not in hardship when matched. Hardship Dummy is equal to one for all borrowers entering hardship from December 2019 to early March 2020. Post January Dummy equals one for borrowers who entered hardship after January 2020 along their non-hardship matched borrowers. All borrower data come from our proprietary online lender, and U.S. treasury yields are from the Federal Reserve Economic Database. Columns 2 and 3 report coefficients while including all continuous covariates from Table 1 along with categorical borrower traits in Figures 5 and 6. Column 3 presents coefficient estimates of conditional logistic specification at state level s.

Table 8: Cumulative Default within 10 Months, FICO Change Instrumented by Treatment Dummy

	Dependent variable:			
	$log\left(\frac{FICO_{t+4}}{FICO_{t-1}}\right)$	Default	Default	Default
	1st Stage	IV	IV	IV
	(1)	(2)	(3)	(4)
Treatment	0.303***			
	(0.077)			
$log\left(\widehat{\frac{FICO_{t+4}}{FICO_{t-1}}}\right)$		−0.767***	−0.566***	−0.681***
		(0.186)	(0.187)	(0.167)
Constant	−0.077*	0.550***	2.510***	2.463***
	(0.042)	(0.026)	(0.517)	(0.579)
Extensive Borrower Controls			x	x
State Fixed-Effects				x
Observations	777	777	777	777
F Statistic	13.769*** (df = 1; 775)			
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: Table 8 shows the coefficient estimates from a two-stage least squares estimation described in section 4.3. The first stage regresses the log change in borrower FICO from one month before hardship to four months after hardship on borrower Treatment status. Columns 2 through 4 present coefficient estimates for ordinary least squares regressions of borrower default within ten months after borrowers began hardship on the instrumented log change of borrower FICO. The log change variable is standardized to have a mean of zero and a standard deviation of one. All borrower data come from our proprietary online lender, and U.S. treasury yields are from the Federal Reserve Economic Database. Columns 3 and 4 show coefficient estimates including all continuous covariates from Table 1 along with categorical borrower traits in Figures 5 and 6. Column 4 includes state fixed effects. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treatment borrowers entered hardship before January 31, 2020. Standard errors in parentheses are robust to heteroskedasticity.

Table 9: OLS Panel Regression of Borrower Loan Balance

	Dependent variable:		
	Monthly Loan Balance		
	(1)	(2)	(3)
Originiation Loan Amount (000s)	658.14***		
	(14.76)	(0.00)	(0.00)
Treatment	−689.76**		
	(309.84)	(0.00)	(0.00)
Post-Hardship Dummy	−2,443.98***	−2,473.50***	587.02***
	(93.75)	(92.51)	(71.70)
Treatment * Post-Hardship Dummy	−659.35***	−680.79***	−947.80***
	(231.90)	(236.91)	(237.81)
Constant	839.23***		
	(269.60)		
Borrower Fixed Effects		x	x
Month Fixed Effects			x
Observations	16,253	16,253	16,253
R^2	0.68	0.94	0.95
Note:		*p<0.1; **p<0.05; ***p<0.01	

Notes: Table 9 shows the estimated coefficients of an ordinary least squares regression of specification 6 of borrower i's dollar reduction in loan principal on Treatment status, a Post-Hardship Dummy variable equal to one for the months after each borrower's hardship start date, and an interaction between Treatment and Post-Hardship Dummy. All borrower data comes from our proprietary online lender. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. We use a time period of ten months before and after each borrower began hardship. Borrowers who default or pay off their loan are retained in the sample at their last loan principal balance. Standard errors in parentheses are clustered by borrower state residence s.

Table 10: OLS Panel Regression of Borrower Percentage Loan Paid each Month

	Dependent variable:		
	Percentage Change in Loan Balance		
	(1)	(2)	(3)
Treatment	0.04		
	(0.13)	(0.00)	(0.00)
Post-Hardship Dummy	1.20***	0.91***	2.64***
	(0.20)	(0.22)	(0.20)
Treatment * Post-Hardship Dummy	−1.36***	−1.59***	−0.87**
	(0.30)	(0.34)	(0.37)
Constant	−3.46***		
	(0.11)		
Borrower Fixed Effects		x	x
Month Fixed Effects			x
Observations	15,862	15,862	15,862
R^2	0.01	0.15	0.16
Note:			*p<0.1; **p<0.05; ***p<0.01

Notes: Table 10 shows the estimated coefficients of an ordinary least squares regression of specification 6 of borrower i's percentage reduction in loan principal on Treatment status, a Post-Hardship Dummy variable equal to one for the months after each borrower's hardship start date, and an interaction between Treatment and Post-Hardship Dummy. All borrower data comes from our proprietary online lender. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. We use a time period of ten months before and after each borrower began hardship. Borrowers who default on their loans are retained in the sample at their last loan principal balance, and borrowers who pay off their loans leave the sample. Standard errors in parentheses are clustered by borrower state residence s.

Table 11: Logit Model Cumulative Default within 10 Months Quality Channel

	Dependent variable:				
	Default				
	(1)	(2)	(3)	(4)	(5)
Treatment	0.387***	0.431***	0.444***	0.415***	0.413***
	(0.169)	(0.193)	(0.195)	(0.179)	(0.173)
Top FICO Quartile		0.790			
		(0.193)			
Treatment * Top FICO Quartile		0.589			
		(0.419)			
Lowest Quartile Interst Rate Dummy			1.096		
			(0.197)		
Treatment * Lowest Quartile Interst Rate Dummy			0.582		
			(0.396)		
Top Grade Dummy				1.389	
				(0.275)	
Treatment * Top Grade Dummy				0.538	
				(0.556)	
Highest Quality					1.464
					(0.434)
Treatment * Highest Quality					0.239
					(0.906)
Constant	1.562***	1.660***	1.526***	1.505***	1.537***
	(0.085)	(0.099)	(0.098)	(0.090)	(0.087)
Observations	778	778	778	778	778
Note:					*p<0.1; **p<0.05; ***p<0.01

Notes: Table 11 shows odds ratio the estimated coefficients of a logistic model of borrower default within ten months of entering a hardship plan. Standard errors are in parentheses. The outcome variable is equal to one if the borrower i has defaulted within ten months of their hardship start date. Top FICO Quartile is a dummy variable equal to one for borrowers having a FICO score one month before hardship in the top quartile of borrowers. Lowest Quartile Interest Rate Dummy is a dummy variable equal to one for borrowers with interest rates in the lowest quartile. Top Grade Dummy is a dummy variable equal to one for borrowers having a proprietary rating grade in category A from Figure 5. Highest Quality is a dummy variable equal to one for borrowers with a high pre-hardship FICO score, high internal loan grade, and low interest rate. The sample consists of all borrowers entering hardship in December 2019 and the first three months of 2020 before the WHO declared a world pandemic. Treated borrowers entered hardship in February and early March of 2020, while control borrowers experienced hardship in December of 2019 and January of 2020. Our sample time period is ten months after hardship starts for every borrower. All borrower data comes from our proprietary online lender.