Identification of Loan Effects on Personal Finance: A Case for Small U.S. Entrepreneurs

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Abstract

Small entrepreneurs are vital to the U.S. economy. This paper draws information from a lender and a credit bureau to identify the causal effects of small loans on the financial health of a group of small U.S. business owners. To estimate the causal effects, the paper exploits temporal variations in the loan disbursements across borrowers and uses of a dynamic difference-in-difference estimation strategy that controls for potential biases due to treatment effect heterogeneity. We identify loan effects on widely accepted indicators of financial health, such as *Vantage Scores* (credit scores), debt-to-income ratios, and credit utilization ratios. The results suggest that even small loans are effective for generating lasting positive impacts on these indicators. The results also shed light on the loan effects on sub-prime and startup borrowers, who are known to face difficulties in securing credit.

JEL: C22, D14, G21, O16.

Keywords: Small U.S. Entrepreneurs, Identification of Loan Effects, Financial Health, Dynamic Treatment Effects, Treatment Effect Heterogeneity

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1 Introduction

According to the U.S. Small Business Administration[1], small businesses—firms with less than 500 employees—account for 99.9 percent of all businesses and 60% of these businesses employ only 1-4 employees. Small businesses account for 47 percent of private-sector employment, 32 percent of export, and 60 percent of new job creation in the United States. Despite their importance, small businesses and small startups are known to face difficulties raising capital using traditional channels and experience low survival rates[2]. To address these problems, there has been a rapid growth of a network of microcredit and microfinance programs in the U.S. with the common goal of helping small entrepreneurs with limited access to credit and other valuable services. Examples of private initiatives include Accion USA, Grameen America, and Kiva. Similarly, the U.S. Small Business Administration (SBA) has rolled out programs in which SBA acts as a guarantor for small business loans and brings together small entrepreneurs and potential lenders. These programs also offer management training, technical assistance, and contracting opportunities. This paper seeks to offer a systematic investigation based on observed data—obtained directly from a U.S. based microfinance institution and a U.S. credit bureau—to uncover the causal impacts of microloans on the financial health of small U.S. business owners.

The network of U.S. programs that we see today draws inspiration from the experimentation with microcredit and microfinance programs in developing countries. The emerging evidence seems to suggest that the impacts of micro-loans on economic and social measures in developing countries are not unconditional, but vary according to borrowers' characteristics, loan terms, and the social and institutional environment in which borrowers operate (Banerjee et al. (2015), Cull and Morduch (2018), Banerjee et al. (2019), Banerjee et al. (2017) and Banerjee (2013)). Evidently, the environment in which U.S. micro-enterprises operate differs from those in the developing world. Most small businesses in the U.S. operate in the formal sector with greater access to infrastructure, markets, and other facilities. The small U.S. enterprises face softer credit terms relative to their counterparts in the developing world, and even some small business owners in the U.S. can access alternative credit sources (e.g., credit cards, home equity credit lines, etc.) to supplement borrowed funds (Bernanke et al. (2007)). It is also possible that shared norms, personal trust, and reciprocity play a lesser role for U.S. small businesses vis-à-vis those operating in the developing world. When such differences are present, it is imprudent to draw conclusions about the impacts of small credit U.S. programs using lessons that we learned in the context of the developing world.

The existing literature, however, offers little for understanding the impacts of small loans on U.S. micro-enterprises. When it exists, the evidence is largely based on anecdotes, case studies, descriptive statistics, or an empirical strategy that fails to disentangle correlation from causation. The *Longitudinal Impact study by Accion and Opportunity Fund Small Business Lending (2018)* reports a summary of responses from a nationwide cohort of 350 borrowers. Some of these borrowers were followed for three years post loan. The report suggests a strong positive

[1] https://cdn.advocacy.sba.gov/wp-content/uploads/2021/12/06095731/Small-Business-FAQ-Revised-December-2021.pdf

[2] Between 1994-2019, on the average, only 67.6% of new small business establishments survived beyond two years.

impact on business indicators such as cash flow, employee hiring, equipment purchases, and business owners' personal and household well-being. Similarly, *Aspen Institute*[3] offer summaries of survey responses from small enterprises highlighting the short and long-run impact of microloan and services. There are a few studies (Young et al. (2014), Craig et al. (2007) and Lee (2013)) that rely on the data aggregated at the MSA level to understand the impacts of the SBA loans on employment, startups, and income growth. We differ from these existing approaches on two counts. First, neither we rely on the survey data, which are prone to inaccurate measurements and response biases nor on aggregated data, making it difficult to map microloan intervention on outcomes with precision. Instead, our data track each borrower for a period of 10 years during the pre, and post-loan periods and, we draw conclusions based on actual and measurable outcomes. This panel also helps us to control for individual-level unobserved heterogeneity. Second, we exploit temporal variation in loan disbursements across borrowers to capture the causal effects of small loans. For this purpose, we use the most recent advances in the causal inference literature that are effective in minimizing biases in a staggered treatment setting.

A part of our data comes directly from the Wisconsin Women's Business Initiative Corporation's (WWBIC) in-house data repository. WWBIC is the largest non-profit microfinance and economic development institution in Wisconsin, assisting the Wisconsin small business community since 1987. A key distinguishing feature of this organization is its focus on low-income, minority, and women business owners. We furnish more details about WWBIC in the data section. This data contain demographic and loan information on 737 borrowers who received business loans from WWBIC during 2007-2016. The data come to us in an anonymous format where each borrower is assigned a unique identifier. To this, WWBIC has added individual credit file information obtained from one of the three credit bureaus. The credit file also covers the period 2007-2016. WWBIC matches individuals across the two data sets to construct a panel of 737 individuals. We use this data to exploit the variation in the timing of loans across individuals to identify the causal effects of microloans on the personal financial indicators of these small entrepreneurs. We use information on key variables such as *Vantage Score*, debt to income ratio, and credit utilization ratio to capture borrowers' personal financial status during both pre- and post-loan periods. These variables not only are correlated with the health of the business, but also determine a borrower's present and near-future access to credit.

In our sample, the timings of loan disbursements vary across borrowers, which lends our data to a standard linear two-way fixed effect model or an event study design. However, the recent advances in the econometric literature (Goodman-Bacon (2018), Sun and Abraham (2021), Athey and Imbens (2018), Callaway and Sant'Anna (2021), Borusyak and Jaravel (2017) and De Chaisemartin and d'Haultfoeuille (2020)) offer reasons to be skeptical about causal estimates from a standard staggered DID framework. The consensus is that, even when the parallel trends assumption is satisfied and treatment assignments are random, the standard approach can lead to misleading estimates in the presence of treatment effects heterogeneity across units or over time. We take this consensus seriously and use a solution proposed by Callaway and Sant'Anna (2021), which, in addition to reducing potential biases, offers easily interpretable estimates akin to an event study for capturing both the short and long-run effects of loans. In addition, we

[3] https://www.aspeninstitute.org/programs/business-ownership-initiative/data/

subject our estimates to several robustness checks to add confidence to our conclusions.

Our results offer valuable insights. We find a noticeable positive impact of small loans on the financial health of borrowers in our sample, of which 46% of the borrowers (at the time of securing loans) were subprime borrowers (with vantage scores below 650) and 45% of borrowers were startups. For example, within 4-6 years of receiving loans, the average *Vantage Score* improves by 92 points—an effect that is strong enough to move 66% of sub-prime borrowers out of this category. We witness similar improvements in credit utilization and debt-income ratios, both of which play significant roles for the present and the future access to credit. Although loan size matters, the effects of small loans are no less visible. For example, within 4-6 year of receiving treatments, the ATT on *Vantage Score* registers 67 point improvements in a sub-sample of 433 borrowers with small loans with a mean and the median loan size of $24,000 and $20,000, respectively. Also, our analysis opens up a rare opportunity to understand the impact of loans on sub-prime and startup borrowers. These borrowers face a higher prospect of being marginalized in the formal credit market. We find that even loans of modest sizes improve the financial status of these borrowers, with effects being particularly robust for the startup borrowers.

The remainder of this paper is organized as follows. The data and its sources are described in Section 2. Section 3 offers a detailed overview of the methodology. Section 4 discusses the results and includes a series of robustness checks. Finally, Section 5 concludes with some comments.

2 Background and Data

Wisconsin Women's Business Initiative Corporation[4] (WWBIC) is a non-profit statewide economic development corporation assisting small business community in Wisconsin since 1987 by lending fair capital for business startups and expansions[5]. It is currently the state's largest micro-lender with the support from federal, state, and private donors. Since its inception, the WWBIC has disbursed $ 90 million in loans[6] to small businesses with the mission of helping borrowers who typically face difficulty in accessing capital using traditional channels. Such borrowers include sub-prime-low-income, low wealth, minority, and women borrowers. For example, across all the categories of assistance (including training and consultation), 70% of WWBIC's clients are female, 61% are people of color, and 63% come from low to moderate-income households.

Our data set consists of **737** individuals who have received business loans from WWBIC during **2007-2016**. The data comes to us in an anonymous format where each borrower is assigned a unique identifier. The data is compiled based on three sources of information. The first source is the WWBIC client information file. Individuals who wish to access loans and/or

[4]www.wwbic.com

[5]The WWBIC also offers business and financial education to its members in areas that include, but are not limited to, business planning, business accounting, access to capital, marketing strategy, personal budgeting, debt reduction, saving plans, and legal help.

[6]https://www.wwbic.com/wp-content/uploads/2022/04/2022-Data-Sheet.pdf

other WWBIC services are required to register with WWBIC as clients. The registration process requires individuals to fill in a form and share various financial and personal details with the WWBIC. This information is saved electronically as the client information file, which contains information on each borrower's age, gender, ethnicity, education, marital and minority status. In our sample, 61% of borrowers are female, 56% of borrowers are married, and 77% of borrowers have a college degree or equivalent. The age distribution of borrowers is skewed toward younger borrowers with a mean and median age of 41 and 39 years, respectively, and only 17% of borrowers are with minority status[7].

The second source of information is the WWBIC's loan file which contains details on each loan, including loan amounts and closing dates. All individuals in our sample have received at least one loan, while 17% of borrowers received a second loan during the period of analysis. The closing date of loans varies across the borrowers, and the frequency of loan disbursement is slightly skewed toward the later years: half of the borrowers in our sample received their first (second) loan before 2013 (2011). Although WWBIC normally caps the loan sizes at $250,000, the actual loan sizes are much smaller. For example, the mean and the median size of the first loan in our sample is $40,820 and $25,700, respectively. The loan sizes are even smaller for the second loans[8]. In table S1 we summarize the details on loan disbursements. The loan file also distinguishes startup borrowers from the borrowers with running businesses. In our sample, 45% of the borrowers have received a loan to start a new business.

Our third and final source of information is the credit files on the borrowers, which the WWBIC has secured from a credit bureau. The credit file is made available to us also in an anonymous format while maintaining the same borrower-specific identifiers used in the other files. Thus, we can track each borrower's financial status for a period of 10 years, which nests both the pre-and post-loan periods for borrowers. Our main outcome variable is the *Vantage (credit) Score*, which serves as a good proxy for an individual's near past and current financial health and determines a borrower's current and near-future access to credit. We also consider credit utilization ratio and debt-income ratio as supplementary outcome variables. The credit utilization ratio—defined as the ratio of the amount of revolving credit used and the amount of revolving credit available—receives a significant weight (30%) in the construction of the *Vantage Score*. This ratio conveys information about the extent to which a borrower is credit constrained. In contrast, the debt-income ratio—defined as the ratio of total recurring monthly debt (including credit payments, mortgage, and auto loan) and the gross monthly income—does not play a direct role in the construction of the *Vantage Score*, but lenders take this ratio seriously to learn about a borrower's capacity to service additional debt. We observe these variables at a two-year frequency starting December 2008. The additional details on these outcome variables are furnished in table S2.

[7]According to WWBIC, minority borrowers include African American and Hispanic.

[8]The mean and the median of the second loan are $31,000 and $22,000 respectively.

3 Empirical Methodology

For our analysis, receiving loans is synonymous with receiving treatments, which all individuals in our sample have received, but not all at once. This staggered adoption setting is suitable for using a static linear two-way fixed effect (TWFE) model or an event study design for exploiting temporal variation in treatment to identify the loan effects.[9] The recent advances in the literature have put both designs under scrutiny and came up with several valuable insights. For example, we now know that the causal parameter in a TWFE design is a weighted average of all possible 2x2 DID estimators that compare timing groups to each other. The weight assigned to each 2x2 DID is sensitive to the panel length, the groups' timing of the treatments, and the relative size of the treatment and control group in the sample (Goodman-Bacon, 2018). We are also aware that in a TWFE model, already treated units act as comparison units. Thus, even when the parallel trend assumption holds, and the assignment of treatments is random, the causal estimates in a TWFE model can be misleading due to the presence of treatment effects heterogeneity.[10] As in a static specification, the dynamic TWFE models also fail to yield sensible estimates of dynamic causal effects under heterogeneity across cohorts.[11] (Sun and Abraham (2021)).

The recent advances in the econometric literature (e.g., Goodman-Bacon (2018), Sun and Abraham (2021), Athey and Imbens (2018), Callaway and Sant'Anna (2021), Borusyak and Jaravel (2017), Imai and Kim (2020) and De Chaisemartin and d'Haultfoeuille (2020)) offer solutions to eliminate potential bias arising due to the heterogeneity in the treatment effects. A direct approach to eliminate bias would be to modify the set of effective comparison units so that units receiving treatments are not compared to previously treated units. Thus, only the 'never treated' units are allowed to act as controls. However, given a relatively small sample size, such a restriction is costly for us. As a solution, we turn to the methodology proposed by Callaway and Sant'Anna (2021) (henceforth CS 2021), which allows 'not yet treated' units to act as controls. In addition to providing sensible estimates even under arbitrary heterogeneity of treatment effects, the methodology permits us to use data to the fullest extent and report treatment effects over an extended period while maintaining the balance between the treated and control units. Below, we briefly describe the CS 2021 methodology used in this paper.

The CS 2021 takes a ground-up approach, using group-time specific average treatment effects on the treated, $ATT(g,t)$, as the building blocks. The groups are created according to when the units received (absorbing) treatments in the sample. The $ATT(g,t)$ measures the average

[9]Both these designs gained popularity over the last two decades. According to De Chaisemartin and d'Haultfoeuille (2020), 20% of the empirical papers published in the *American Economic Review* between 2010-2012 are based on the TWFE model. At the same time, the researchers struggled to obtain a clear interpretation of the treatment effect parameters.

[10]Since the estimate partly depends on the difference between the changes in the outcomes of (already treated) control units and the changes in outcomes for units that are treated later, the possibility of contamination arises in the presence of time-varying treatment effects.

[11]In addition, Sun and Abraham (2021) noted that the evaluation of pre-trends based on these coefficients could also be misleading. The treatment lead coefficients are not guaranteed to be zero even when parallel trends are satisfied in all periods.

treatment effect at time t for the group first treated in time $g < t$, and is defined as

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(0) \mid G_g = 1\right] \tag{1}$$

where G_g is a dummy variable equal to one if the unit is in treatment time group g. $Y_t(g)$ is the outcome variable at time t for treated units if they were to first become treated in time period g, and $Y_t(0)$ is the potential outcome for the treated units had they not been treated. In the absence of data on $Y_t(0)$, the identification strategy relies on control groups consisting of only those units which have not received treatment up to the time g.

According to CS 2021 and Sant'Anna and Zhao (2020), $ATT(g,t)$ can be semi-parametrically estimated using a doubly robust approach that combines the outcome regression (OR) approach of Heckman et al. (1997) and the inverse probability (IPW) approach of Abadie (2005), and is given by

$$ATT(g,t) = \mathbb{E}\left[\overbrace{\left(\frac{G_g}{\mathbb{E}\left[G_g\right]} - \frac{\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}}{\mathbb{E}\left[\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}\right]}\right)}^{\text{Inverse Probability Weight}} \overbrace{\left(Y_t - Y_{g-1} - m_{g,t}(X)\right)}^{\text{Outcome Regression}}\right] \tag{2}$$

As before, the term G_g is a binary variable that is equal to 1 when an individual is first treated in period g. The set of covariates is given by X, and the term $p_{g,t}(X)$ represents the propensity score prediction of receiving treatment (loans) at $t = g$. This prediction is based on the group of individuals receiving loans at $t = g$, and those groups who have received loans no earlier than the period $t+1$. The first expression represents the Inverse Probability Weighting (IPW) term (Abadie (2005)), and the term $m_{g,t}(X) = \mathbb{E}\left[Y_t - Y_{g-1} \mid X, D_t = 0, G_g = 0\right]$ is the estimated conditional expectation function from the outcome regression (OR) approach of Heckman et al. (1997) measuring change from $g-1$ to t in outcome Y_t for the group receiving loan no earlier than the period $t+1$, conditional on covariates X. The OR approach requires researchers to correctly model the outcome evolution of the comparison group. The IPW approach, on the other hand, requires one to correctly model the conditional probability of unit i being in group g given X. The $ATT(g,t)$ in equation (2) is the weighted average of the difference in the evolution of outcomes between the treated and the control groups, where higher weights are assigned to the difference in outcomes between the treated and control group when the control group shares similar characteristics to those found in the treated group. This weighting procedure guarantees that the covariates of the group g and the control group are balanced. According to CS 2021, the above $ATT(g,t)$ estimator is doubly robust in the sense that for the estimate to be valid, one is required to correctly specify either the outcome evolution for the comparison group or the propensity score model, but not necessarily the both.

We use the methodology outlined above to divide the sample period into five sub-intervals: 2007-2008, 2009-2010, 2011-2012, 2013-2014, and 2015-2016, and assign a group identifier to a borrower according to which sub-interval includes the borrower's first loan date. For example, we assign borrowers to group 2 if these borrowers received their first loan during the two-year interval 2009-2010. Similarly, we assign borrowers to group 5 if their loan dates belong to the interval 2015-2016. These sub-intervals and groups is not arbitrary. They are designed to utilize the maximum amount of information on key outcome variables and to capture the loan effects over a longer horizon.

As a first step, we seek to use equation (2) to estimate $ATT(g, t = g + e)$: the average loan effects on borrowers belonging to group g as a function of time e relative to the treatment period. As noted earlier, we observe outcome variables at a two-year frequency starting December 2008. These reporting dates do not necessarily align with the borrowers' loan dates. For example, borrowers with loan closing dates in 2009-2010 belong to group 2, for whom we observe the earliest post-treatment outcomes on December 2010. At this date, the length of exposure to treatment for group 2 borrowers could vary between 0 - 2 years. We face the same issue for borrowers in the other groups, requiring us to interpret $ATT(g, t = g + e(= 0))$ as the average loan effect materializing for group g within the 0 - 2 years of receiving loans. Similarly, $ATT(g, t = g + e(= 2))$ needs to be interpreted as the average loan effect for the group g in the 2 - 4 year interval, and so on.

Our next goal is to construct loan effect estimates that are well suited to capture instantaneous as well as time-varying loan effects. For each value of e, we construct a weighted average of the group $ATTs$, assigning the weights by the group sizes. We repeat this exercise for every relative period e, including the pre-treatment ($e \leq 0$) periods, to present our results in a standard event study format. Besides capturing the evolution of loan effects, this format presents an opportunity to conduct an informative test of the parallel trend assumption using formal inference based on CS 2021 recommended bootstrapping procedure which reports simultaneous confidence bands robust to multiple hypothesis testing and individual cluster errors. To create a single, overall point estimate, we take the average of these aggregated relative time estimates with $t \geq g$. Our analysis takes advantage of the R code, which has been made publicly available[12] by Callaway and Sant'Anna (2021) as a supplement to their research.

4 Loan Effects

Vantage (Credit) Score is our primary outcome variable, which serves as a good indicator of an individual's financial health and creditworthiness. Since we observe this variable starting December 2008, the pre-treatment information on *Vantage Score* on 50 borrowers in group 1 (who received loans during 2007-2008) is missing in our data. We exclude these borrowers from the analysis. The methodology also prohibits us from using already treated units as controls. Thus, the group of last treated borrowers (group 5) lack controls of their own and can only serve as comparison units for the other groups. Therefore, the reported loan effects are based on $ATTs$ that materialized for the remaining three groups.

Table 1 summarizes loan effects on the *Vantage Score*. The single estimate in the first row represents a weighted average of all group-time $ATTs$. The next row reports the weighted av-

[12]Callaway and Sant'Anna's R Package, DiD, version 2.0.1.908. See https://bcallaway11.github.io/did/ for more information on this package.

erage of the groups' treatment effects by the length of exposure to the treatment. The results suggest that the group-average treatment effect within the first two years of receiving loans ($e = 0$) is indistinguishable from zero. The effects, however, become visible with time. In the 2-4 years interval ($e = 2$), the group-average *ATT* on the *Vantage Score* records an 68 points improvement. We experience a loss of (not-yet-treated) comparison units as we move forward in time. Despite this, the loan effects on the *Vantage Score* in the 4-6 year intervals ($e = 4$) remain statistically significant and register an increase of 92 points. How significant are these effects? At the time of receiving loans, nearly 46% of borrowers in groups 2, 3, and 4 were sub-prime borrowers (with a *Vantage Score* below 650). Within the 2-4 years (4-6 years) post-treatment, the improvements in the *Vantage Score* is large enough to move 56% (66%) of these borrowers out of the subprime category with a prospect of better financial health and better access to credit. Improvements of this magnitude clearly hold the potential for transformational impacts in the lives of borrowers.

In Figure 1, we supplement the results with an event study plot for pre- and post-treatment estimates with 95% simultaneous confidence bands. The pre-treatment estimates offer information about the validity of the (conditional) parallel trend assumption, requiring that in the absence of the treatment, the outcome variable follows the same trend across borrowers with similar characteristics. The individual characteristics that we consider are borrowers' age, gender, minority status, level of education, marital status, and business startup status that we observe at the time of securing loans. We use these characteristics as conditional variables for the full sample. However, for some sub-samples, insufficient variation in the data prevents us from executing a meaningful outcome regression component of the equation (2). For such cases, we use a subset of the conditioning variables. In Figure 1, the *ATTs* prior to treatments are indistinguishable from 0, rendering support to the (conditional) parallel trend assumption.

The validity of our estimates also depends on the 'no anticipation of the treatment' assumption. The WWBIC receives a large number of loan applications, of which only a fraction of applications is approved. Moreover, WWBIC returns its decision on a rolling basis within a short period of receiving the applications. Under this circumstance, it is fair to assume that neither a borrower can choose the treatment status nor the treatment path is a priori known to a borrower. With this in mind, we ignore the possibility of anticipation effects contaminating our estimates.

The *Vantage Score* depends on several factors, such as an individual's credit utilization ratio, payment history, the number of new credit inquiries, etc. Information on these variables is available in the credit bureau data. But, not all variables—such as payment history and the number of new credit inquiries—are well suited for our analysis. The format in which they appear makes it difficult for us to draw a clear distinction between the pre and post-treatment observation, even if borrowers belong to the same group.[13] The data on the credit utilization ratio—a ratio

[13]The data on the payment history and inquiries come with a time reference—such as the number of new inquiries or delinquencies in the last 12 months. Whereas, we associate group identifiers based on loan dates that lie in two-year intervals. In these intervals, the data that we observe on these two variables show up as pre-treatment data for some borrowers and post-treatment data for others, even when both sets of borrowers share the same group identifier.

between credit use and available credit—is, however, suitable for our analysis. As in the case of the *Vantage Score*, we observe this variable at a two-year frequency for each individual, and we can separate pre-treatment observations from the post-treatment observations. The credit utilization ratio receives nearly 0.30 weight in constructing the *Vantage Score*. We also consider debt to income ratio—the ratio of the total recurring debt to the gross monthly income of an individual—as an outcome variable. Though not used for the *Vantage Score*, lenders pay close attention to this variable to assess a borrower's ability to service new debt obligations. Typically, a borrower is discriminated against by a lender when the borrower's debt-income ratio exceeds 0.36. Information on this variable is also available at a two-year frequency starting December 2008.

We report event study plots for these two additional variables in Figures 2a and 2b. The *ATTs* on the utilization ratio become visible in the 2-4 year interval, and the ratio registers a 0.57 point drop by the 4-6 years post treatment. In contrast, the debt-income ratio is immediately impacted by loans and experiences an increase in the 0-2 year interval. This increase is transient and is followed by a steady decline. Within the 4-6 years of receiving loans, the ratio registers a 0.16 point drop (with a borderline statistical significance). At the time of loan closing, 37% of borrowers in our sample did not meet the 0.36 debt-income threshold. By 4-6 years post-treatment, the size of the *ATT* treatment effect is large enough to improve the creditworthiness of half of these borrowers by lowering their debt-income ratios below 0.36.

Only 17% of the borrowers in our sample have received multiple loans. It is, however, possible that this small group of borrowers with multiple treatments is driving our results. To explore this, we restrict our sample to 613 borrowers with single loan. The event study plots in Figure 3 validate our full-sample results and present a more convincing case for all three outcome variables. It is also possible that significant average treatment effects among single loan recipients are driven primarily by borrowers with larger loan sizes. To examine this, we re-do the analysis after excluding the top 30% single loan recipients by loan size, leaving us with 433 borrowers with loan sizes less than or equal to $50,000. The mean and median loan sizes of this sub-sample are $24,000 and $20,000, respectively. The results (in Figure 4) suggest that while smaller loans diminish the treatment effects, their effects remain quite noticeable: in the 2-4 and 4-6 year intervals, the *Vantage Score* increases by 52 and 69 points. The credit utilization and debt-income ratios follow trends similar to those observed for the full sample.[14]. Though in some cases—as in the case of the debt-income ratio—the *ATTs* lose statistical significance due to the small sample size, the results collectively point to the fact that even small loans hold the promise of transformative impacts on entrepreneurs' financial health and creditworthiness.

The startup borrowers are known to face difficulties accessing credit and represent 45% of our sample. To understand the loan effects on this group, we restrict our sample to only startup borrowers and estimate the average treatment effects while drawing both treatment and comparison units from this sub sample. Thus, the effects that we report in Figure 5 are estimated by comparing treated startup borrowers with those startup borrowers who are yet to receive loans.

[14] We repeat but do not report the exercises where we exclude the top 10% and 20% of loans by size. The positive correlation between the loan sizes and loan effects is also transparent in these exercises.

In other words, the estimates capture the loan effects within the group of startup borrowers. Despite having a smaller sample, the loan effects in the 'startup' group show up more robustly than what we witnessed in the full sample. The estimates are also statistically significant across all categories of indicators.

The sub-prime borrowers are equally visible in our sample, with a share of 46%. Since 50% of the borrowers in the startup sample are also sub-prime borrowers, the results reported in Figure 5 inform about the loan effects on the sub-prime borrowers. To be sure, we construct a sub-sample consisting only of sub-prime borrowers and draw both treated and comparison groups from the sub-sample. The results in Figure 6 suggest that the loan effects take longer to materialize for the group of sub-prime borrowers. However, by 4-6 years following treatments, the *Vantage Score* improves by 96 points. This treatment effect is also statistically significant and large enough to move 70% of original subprime borrowers out of the subprime category. We witness similar improvements in the credit utilization ratio: within 4-6 years post-treatment, the ratio decreases by 0.60 points. The loan effects are, however, missing in the case of the debt-income ratio.

We allocate the rest of this section to check the robustness of our results. In our case, the composition of the treated groups changes with $e(= 0, 2, 4)$. For example, when $e = 0$, the estimate uses information on the treatment effects of all three groups. In contrast, for $e = 4$, the dynamic coefficient only uses information on group 2. This is not a concern if the effects are common across the groups. In practice, however, this may not be the case since groups are treated on different dates and may encounter different economic environments. To address this, we restrict the analysis only to $e = 0$ and $e = 2$. The Event Study (Balanced) row in Table 1 reports the estimates using the information on group 2 and group 3 for both periods. The Event Study and the Event Study (Balanced) use the same treatment and comparison groups for $e = 2$ and return the same treatment effect. The difference, however, appears in the case of $e = 0$ where the initial decrease in the *Vantage Score* is now more pronounced and is statistically different from zero. This result is not too far from the main result where immediate loan effects on Vantage Score were absent.

Our results support that the average outcomes for the treated and control groups follow parallel paths in the absence of treatment. However, the presence of parallel trends during pre-treatment does not guarantee that the time trend for untreated units is comparable to the counterfactual time trend for treated units during post-treatment periods. Some time-varying confounds or some other shocks can add biases to the estimates by causing the post-treatment trends to differ, even if pre-treatment trends were the same. In addition, the pre-tests can be under powered and may fail to detect violation of parallel trends—particularly for a small sample. To address these concerns, we turn to Rambachan and Roth (2022) which offers a formal sensitivity analysis that relates the magnitude of violations of parallel trends to the robustness of treatment estimates in post-treatment periods. We use the updated version of the `DID`[15] package to generate results, which we report in Figures 7 and 8. In Figure 7, we report the sensitivity of the 4-6 year post-treatment loan effects on Vantage score to the different degree

[15] https://github.com/bcallaway11/did

of violation of the *PTA*. The graph reports robust confidence sets under varying restrictions on the set of possible violations of parallel trends (different values of $\bar{M}$). When $\bar{M} = 0$, it implies that the parallel trend assumption holds exactly in post-treatment periods regardless of what happened in pre-treatment periods. The value of $\bar{M} = 1$ allows for violations of PTA in the post-treatment periods whose magnitude is as large as the largest violation of parallel trends on pre-treatment periods, and so on. The results indicate that the loan effects on the *Vantage Score* remain significant and statistically different from 0 even when we allow for some violation of PTA. In particular, our conclusions are robust to the violation of PTA up to 0.9 times the violation in pre-treatment periods. We obtain a very similar threshold for the utilization ratio (Figure 8).

In Figure 9, we present results from an experiment where we randomly assign borrowers to placebo treatment groups (from g=2 to g=5) without paying attention to their true loan closing dates. We consider 80 such treatment assignments and estimate the dynamic treatment effects for each assignment using the same set of covariates as in our main analysis. Figure 9 presents the distribution of the treatment effects on the *Vantage Score* in 4-6 year interval, post-treatment. The results suggest that *ATT* placebo estimates are centered approximately around 0 with an average of 2.2, and the upper bound of the distribution is significantly smaller than our original estimate. We take these results as support for our main conclusions.

5 Conclusion

Since the inception of Bangladesh Grameen Bank in 1983, the microcredit and microfinance movements have spread rapidly across the developing world. Although the United States came relatively late to the movement, experimentation in the 1980s and 1990s laid the groundwork for the network of programs that we see today in the U.S., offering small loans and other forms of assistance to small entrepreneurs. Despite the rapid growth of this network, there remains little systematic evidence to understand the impacts of such loans on small U.S. business owners. This paper seeks to fill in this gap. We do not have access to data on business indicators. However, we observe borrower-specific key variables—such as *Vantage (Credit) Score*, debt-income ratio, and credit utilization ratio—which are correlated with the business health but are also influential determinants for current and future access to credit. Even in our small sample, we find positive and statistically significant impacts of loans on these variables. Though loan sizes matter for the loan effects, small loans hold the potential for transformational impact on small business owners. We also find a significant positive effect of loans on the group of startup and sub-prime borrowers. Researchers are divided on the effectiveness of small loan programs in the developing world. In contrast, our results offer strong support for such programs for small U.S. business owners. We reconcile this difference by appealing to the view that it is not only the access to credit but its interaction with the institutional and social factors that hold the key to the success of small-loan programs.

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Table S1: Distributions of Loan Timings and Amounts

Panel A: Frequency of Loan Disbursements										
Year	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Number of borrowers	28	22	37	56	89	76	96	113	103	117

	count	mean	sd	min	max	p25	p50	p75	**Source**	Frequency
Panel B: Loan Details										
1st Loan Amount (Whole Sample)	737	40820.47	37053.17	1000	312500	15000	25661.41	50000	WWBIC	Fixed
1st Loan Amount (Single Loan Group)	613	42339.73	38493.84	2000	312500	15000	30000	55000	WWBIC	Fixed
2nd Loan Amount	124	31177.78	28797.9	1500	150000	9969.56	21781.14	43061.84	WWBIC	Fixed

Table S2: Distribution of Pre-treatment (one period before getting the loan) Outcome Variables

	count[a]	mean	sd	min	max	p25	p50	p75	**Source**	Frequency
Panel A: Outcome Variables										
Vantage Score	683	653.5608	88.22721	341	824	595	656	722	Credit Bureau	Biyearly
Debt to Income Ratio	644	30.23292	23.46053	0	101	11	27	45	Credit Bureau	Biyearly
Utilization Ratio	562	.4441608	.3453867	0	2.85125	.1512403	.4023014	.7081652	Credit Bureau	Biyearly

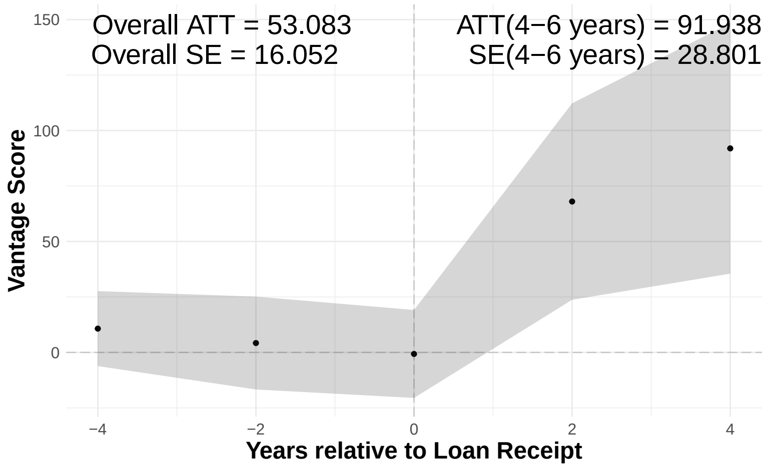
[a] Notes: Number of borrowers (count) are less than 737 because we don't have information on the pre-treatment status of Group 1. Therefore, they are not included in the analysis of the summary statistics of outcome variables based on the status prior to receiving loans.

Table 1: Loan Effect Estimates on Vantage Score

	Partially Aggregated				Single Parameters
	(1)	(2)	(3)	(4)	(5)
Conditional Parallel Trends					
Simple Weighted Average					31.9095 *
					(13.4738)
	e=0	e=2	e=4		
Event Study	-0.6837	67.9937 *	91.9384*		53.0828 *
	(10.2447)	(20.2825)	(28.759)		(17.190)
	e=0	e=2			
Event Study (Balanced Groups)	-20.2912 *	67.9937 *			23.8512 *
	(5.9970)	(20.0118)			(11.0867)

Notes: The table reports aggregated treatment effect parameters under conditional parallel trends assumptions and with clustering at the individual level. The row 'Simple Weighted Average' reports the weighted average (by group size) of all available group-time average treatment effects. The row 'Event Study' reports average treatment effects by the length of exposure to the loan; For example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. The row 'Event Study (Balanced Groups) reports average treatment effects by length of exposure while making sure that the composition of the treatment group does not change with e. The last two elements in the 'Single Parameters' column are the average of dynamic effects reported in the Event Study and Event Study (Balanced Groups) rows. '*' indicates that the results are statistically significant (p-value < 0.05).

Figure 1: Event Study plot for Vantage Score



Notes: The figure presents dynamic difference-in-difference estimates under conditional parallel trends assumptions and simultaneous 95% confidence bands. Standard errors are clustered at the individual level. The outcome variable is Vantage Score and covariates include age, gender, married, education, start-up and minority. Years relative to loan receipt capture exposure to the treatment (e); for example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. For each e, ATT is calculated by averaging dynamic treatment effects across all groups. The Overall ATT is calculated by averaging dynamic treatment effects across all event times. The average treatment effects are not significant when confidence band include 0.

Table 2: Dynamic Effect of the Loan Receipt on Outcome Variables[a]

	(1)	(2)	(3)
e	Vantage Score	DTI	Utilization Ratio (18)
-4	10.7320	-1.1812	0.0448
	(8.6211)	(2.0704)	(0.0403)
-2[b]	4.2334	-0.9328	-0.1142
	(10.6771)	(4.1376)	(0.0546)
0[c]	-0.6837	9.3118*	0.0130
	(10.1093)	(3.2789)	(0.0389)
2[d]	67.9937*	-3.7454	-0.2358*
	(22.5877)	(8.8020)	(0.0921)
4	91.9384*	-15.9220	-0.5656*
	(28.8007)	(8.2614)	(0.1360)
Overall	53.0828*	-3.4519	-0.2628*
	(16.0519)	(6.5312)	(0.0694)

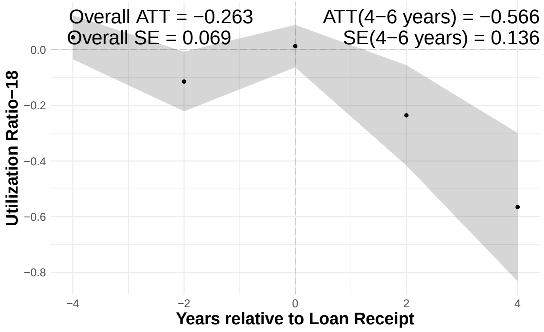
[a] Std. Errors are in parentheses and * indicates p-value < 0.05.

[b] $e = -2$ indicates the average loan effect within the interval 2-4 years prior to getting the loans

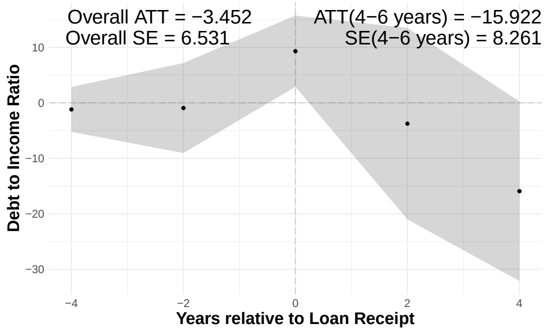
[c] $e = 0$ means the instantaneous effect of the loans (the average loan effects that materialized within the 0-2 years after receiving the treatments.)

[d] $e = 2$ indicates the effect of the loan after 2-4 years

Figure 2: Event Study plots for Utilization Ratio and Debt to Income Ratio



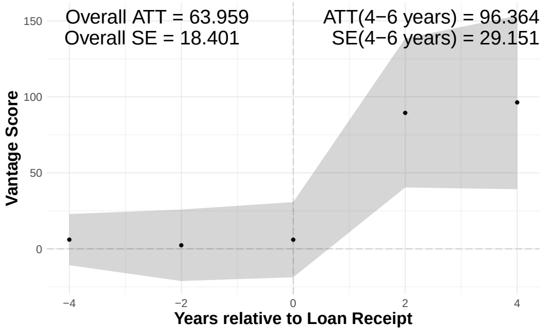
(a)



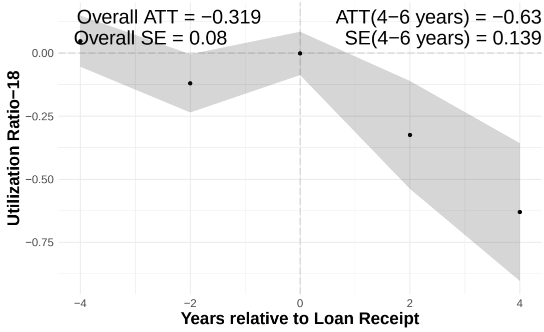
(b)

Notes: The figures present dynamic difference-in-difference estimates under conditional parallel trends assumptions and simultaneous 95% confidence bands. Standard errors are clustered at the individual level. The outcome variables include Utilization Ratio and Debt to Income Ratio and covariates include age, gender, married, education, start-up and minority. "Years relative to loan receipt" capture exposure to the treatment (e); for example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. For each e, ATT is calculated by averaging dynamic treatment effects across all groups. Overall ATT is calculated by averaging dynamic treatment effects across all event times. The Credit Bureau reports DTI as a multiple of 100, for example, DTI = 50 refers to 0.5. For our analysis, we keep the Credit Bureau data format. Therefore, the reported ATTs for DTI in the Event Study plots, needs to be divided by 100 to express it in the fraction format. The average treatment effects are not significant when confidence band include 0.

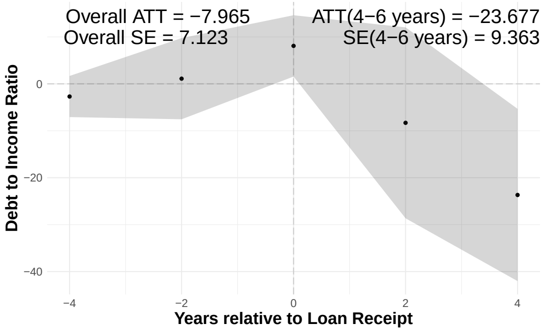
Figure 3: Single Loan Event Study Plots



(a)



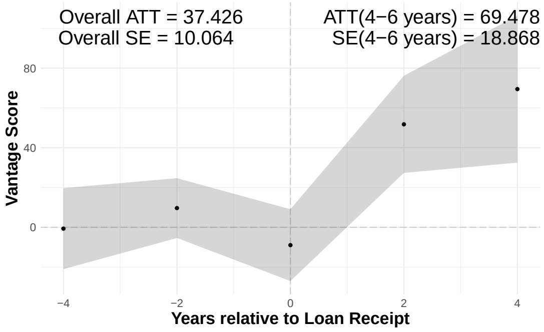
(b)



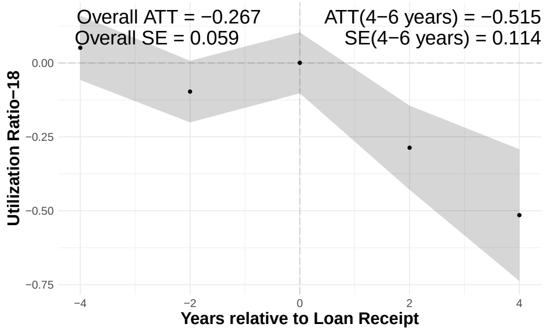
(c)

Notes: The figures present dynamic difference-in-difference estimates for the single loan group under conditional parallel trends assumptions and simultaneous 95% confidence bands. Standard errors are clustered at the individual level. The outcome variables include Vantage Score, Utilization Ratio and Debt to Income Ratio and covariates include age, gender, married, education, start-up and minority. "Years relative to loan receipt" capture exposure to the treatment (e); for example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. For each e, ATT is calculated by averaging dynamic treatment effects across all groups. Overall ATT is calculated by averaging dynamic treatment effects across all event times. The Credit Bureau reports DTI as a multiple of 100, for example, DTI = 50 refers to 0.5. For our analysis, we keep the Credit Bureau data format. Therefore, the reported ATTs for DTI in the Event Study plots, needs to be divided by 100 to express it in the fraction format. The average treatment effects are not significant when confidence band include 0.

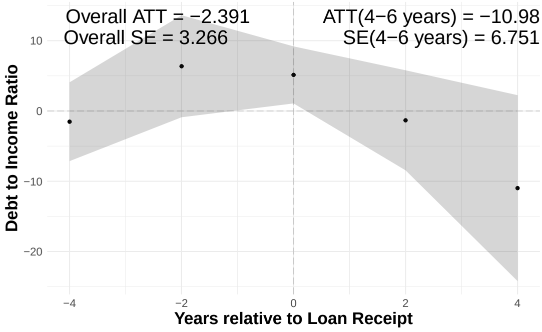
Figure 4: Event Study Plots for Single Loans less than \$ 50,000



(a)



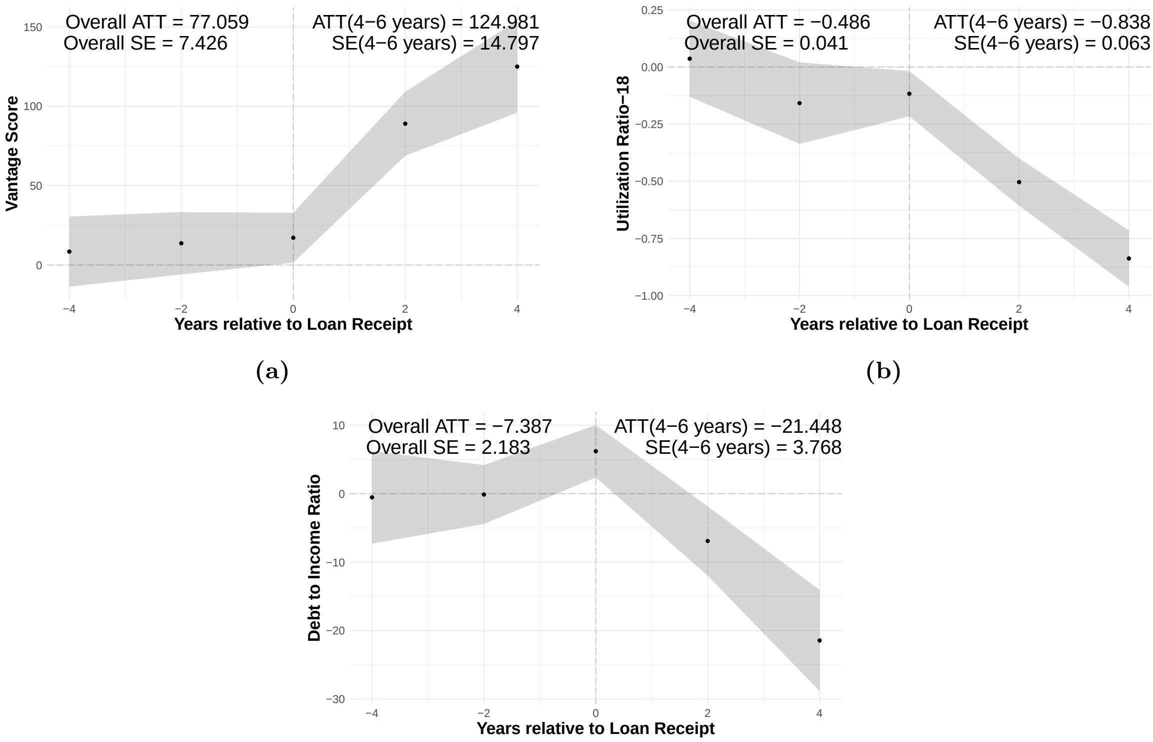
(b)



(c)

Notes: The figures present dynamic difference-in-difference estimates for the group of single loans less than \$ 50,000 under conditional parallel trends assumptions and simultaneous 95% confidence bands. Standard errors are clustered at the individual level. The outcome variables include Vantage Score, Utilization Ratio and Debt to Income Ratio and covariates include age and start-up. "Years relative to loan receipt" capture exposure to the treatment (e); for example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. For each e, ATT is calculated by averaging dynamic treatment effects across all groups. Overall ATT is calculated by averaging dynamic treatment effects across all event times. The Credit Bureau reports DTI as a multiple of 100, for example, DTI = 50 refers to 0.5. For our analysis, we keep the Credit Bureau data format. Therefore, the reported ATTs for DTI in the Event Study plots, needs to be divided by 100 to express it in the fraction format. The average treatment effects are not significant when confidence band include 0.

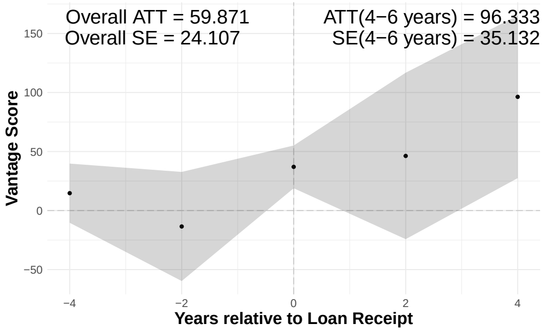
Figure 5: Start-up Event Study Plots



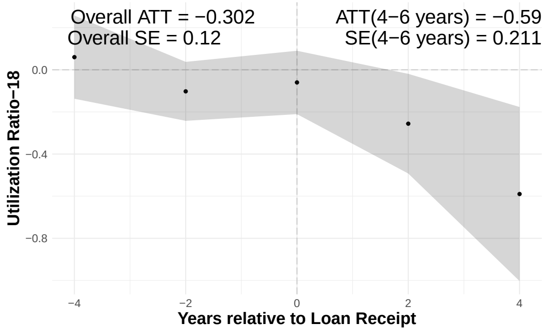
(a) **(b)** **(c)**

Notes: The figures present dynamic difference-in-difference estimates for the group of startups under unconditional parallel trends assumptions and simultaneous 95% confidence bands. Standard errors are clustered at the individual level. The outcome variables include Vantage Score, Utilization Ratio and Debt to Income Ratio. "Years relative to loan receipt" capture exposure to the treatment (e); for example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. For each e, ATT is calculated by averaging dynamic treatment effects across all groups. Overall ATT is calculated by averaging dynamic treatment effects across all event times. The Credit Bureau reports DTI as a multiple of 100, for example, DTI = 50 refers to 0.5. For our analysis, we keep the Credit Bureau data format. Therefore, the reported ATTs for DTI in the Event Study plots, needs to be divided by 100 to express it in the fraction format. The average treatment effects are not significant when confidence band include 0.

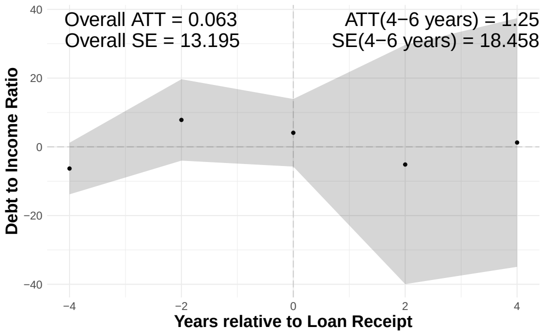
Figure 6: Subprime Event Study Plots



(a)



(b)



(c)

Notes: The figures present dynamic difference-in-difference estimates for the group of subprime borrowers under conditional parallel trends assumptions and simultaneous 95% confidence bands. Standard errors are clustered at the individual level. The outcome variables include Vantage Score, Utilization Ratio and Debt to Income Ratio and covariates include age and gender. "Years relative to loan receipt" capture exposure to the treatment (e); for example, estimates corresponding to $e = 0$, represents weighted average of group-time $ATTs$ in the 0-2 year interval from receiving loans. Similarly, $e = 2$, represents weighted average of group-time $ATTs$ in the 2-4 year interval from receiving loans and so on. For each e, ATT is calculated by averaging dynamic treatment effects across all groups. Overall ATT is calculated by averaging dynamic treatment effects across all event times. The Credit Bureau reports DTI as a multiple of 100, for example, DTI = 50 refers to 0.5. For our analysis, we keep the Credit Bureau data format. Therefore, the reported ATTs for DTI in the Event Study plots, needs to be divided by 100 to express it in the fraction format. The average treatment effects are not significant when confidence band include 0.

Figure 7: Sensitivity Analysis for Vantage Score (e=4)

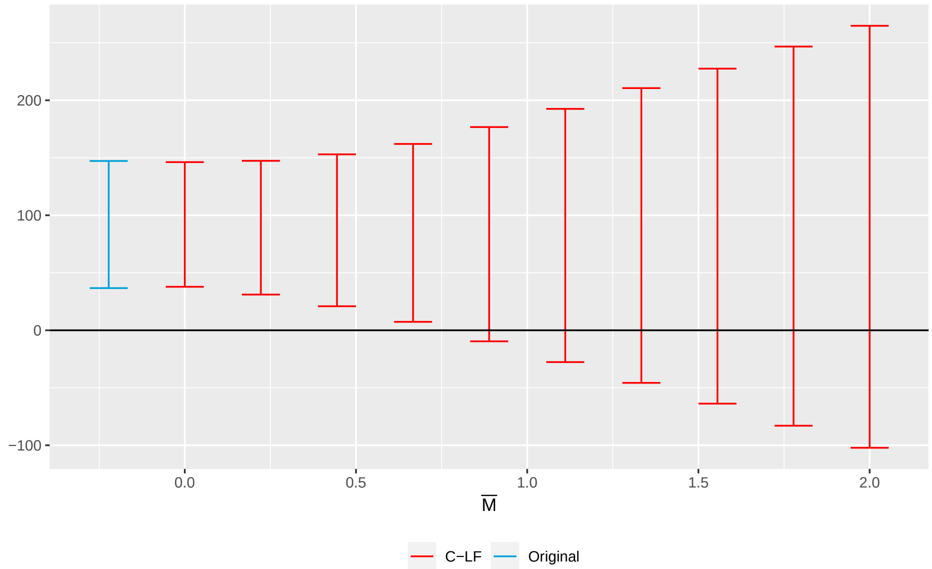
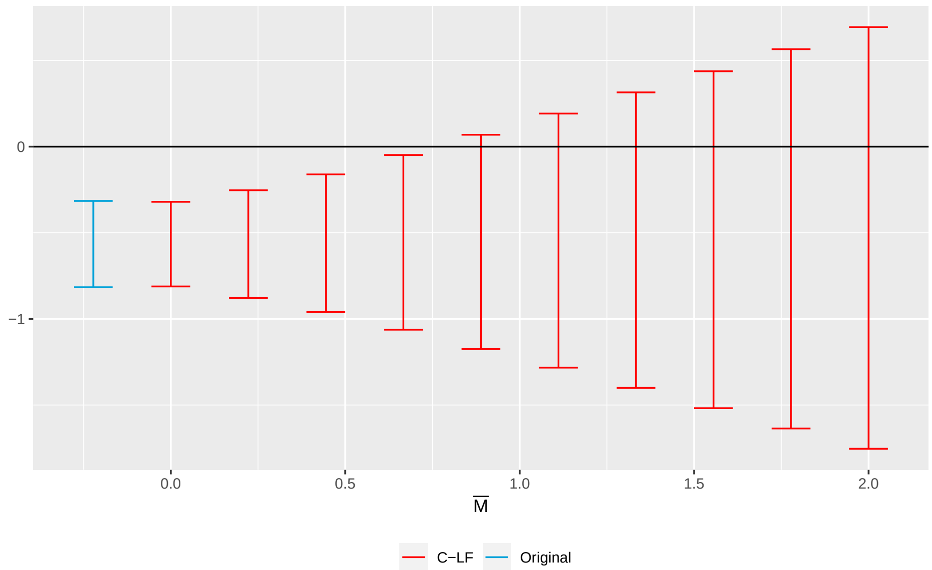
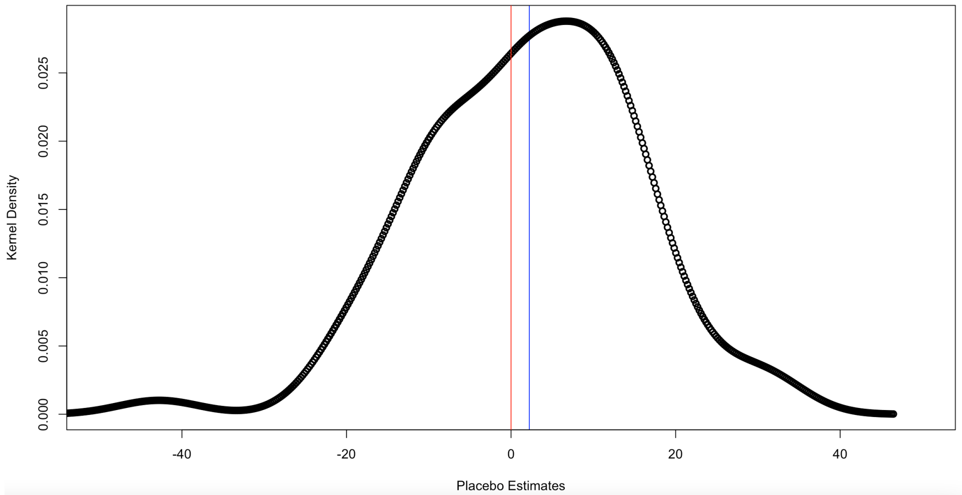


Figure 8: Sensitivity Analysis for Utilization ratio(18) (e=4)



Notes: Figures 7 and 8, offer a formal sensitivity analysis that relates the magnitude of violations of parallel trends to the robustness of treatment estimates in post-treatment periods (Rambachan and Roth (2022)). In Figure 7, we report the sensitivity of the 4-6 year post-treatment loan effects on Vantage score to the different degree of violation of the PTA. The graph reports robust confidence sets under varying restrictions on the set of possible violations of parallel trends (different values of $\bar{M}$). When $\bar{M} = 0$, it implies that the parallel trend assumption holds exactly in post-treatment periods regardless of what happened in pre-treatment periods. The value of $\bar{M} = 1$ allows for violations of PTA in the post-treatment periods whose magnitude is as large as the largest violation of parallel trends on pre-treatment periods, and so on. In Figure 8, we report the sensitivity of the 4-6 year post-treatment loan effects on Utilization Ratio to the different degree of violation of the PTA.

Figure 9: Distribution of Placebo Treatment Effects (Effect after 4-6 years) on Vantage Score



Notes: The figure plots the density distribution of the estimated aggregated dynamic treatment effects after 4-6 years from loan receipt, obtained from a randomization placebo test. We randomly select a placebo treatment group (from g=2 to g=5) without considering the true time of the loan receipt, and estimate the CS dynamic treatment effects. This estimation process is repeated 80 times, and the graph shows the distribution of placebo treatment effects on vantage score. The red vertical line is drawn at the placebo estimate value of zero and the blue vertical line corresponds to the value that indicates the mean of all aggregated dynamic placebo treatment effects(=2.2).