I. Introduction

Consumers struggle when making financial decisions (Morduch & Schneider, 2017). These difficulties often translate to costly mistakes across several domains of household finance, from investment and retirement savings decisions to mortgage choice and debt management (Benartzi & Thaler, 2001; Choi et al., 2009; Gross & Souleles, 2002; Ponce, Seira, & Zamarripa, 2017). In the context of consumer credit, one common and costly behavior is failing to make minimum payments on time. Past-due accounts can lead to a variety of downstream consequences such as penalty fees, higher interest rates, and lower credit scores. Recent estimates indicate that approximately 20% of consumer credit accounts incur late fees each quarter (CFPB, 2015), amounting to more than $11 billion per year in penalty fees for late payments (Sidell, 2016).

Given the direct implications for consumer welfare, improving financial decision making has become a focus in recent decades with actors in the public, private, and nonprofit sectors implementing a wide range of interventions. Recent legislation has focused on improving consumer decisions by providing enhanced disclosures such as those mandated in the Credit Card Accountability Responsibility and Disclosure (CARD) Act of 2009 and in state regulation of payday lending. These disclosures typically aim to simplify information about financial products such as interest rates, repayment terms, or penalty fees.

Our intervention tests a novel form of disclosure that provides borrowers with information about themselves rather than about attributes of financial products. Specifically, we provide individuals with a personalized, quantifiable, and behaviorally responsive measure of their creditworthiness: their FICO Score. We present evidence from a large-scale field experiment with over 400,000 clients of Sallie Mae, a national financial institution specializing in student loans. Beginning in June 2015, Sallie Mae offered borrowers access to unlimited views of their FICO Score. This was part of a broader initiative, the FICO Score Open Access Program, to increase consumer access to their scores through partnering financial institutions. As of 2018, more than 250 million consumer accounts included free access to FICO Scores.

We exogenously vary the likelihood of viewing one’s FICO Score page by randomly assigning borrowers to receive direct communications about the program’s availability. To estimate the effect of the intervention on financial outcomes, we examine individual-level credit report data provided by TransUnion. Borrowers assigned to the treatment group received quarterly email messages notifying them that an updated FICO Score was available to view through Sallie Mae’s website and provided instructions on how to view their score. During the first year of the intervention, 32% of treatment group members viewed their personalized page at least once, an 8 percentage point increase over the control group.

We find that the intervention led to a significant decrease in the likelihood of having a late payment one year after the intervention. Specifically, treatment group members were 0.7 percentage points less likely to have an account that was thirty days or more past due, a 4% decrease relative to the control group. This change in payment behavior is quite large, especially given that less than half of treatment group members ever opened the email and an even smaller fraction ever logged in to view their score. The intervention also led to a net positive outcome for the borrower’s creditworthiness as indicated by an increase in the borrower’s FICO Score (a statistically significant increase of 0.7 points) and reduced the proportion of subprime borrowers by 0.4 percentage points. These effects largely persist across the full two-year study period.

A key component of our intervention entails prompting individuals to view their personal FICO Score page, which is not included in the email message. This page includes the borrower’s FICO Score along with two reason codes detailing key factors contributing to their score. While the
intent-to-treat estimates are the policy-relevant estimates for financial institutions considering a similar email campaign, we also investigate the effect of viewing one’s FICO Score on financial behaviors by using treatment status as an instrument for the likelihood of viewing one’s FICO Score page. Our estimates suggest that borrowers who were induced to view their score page as a result of our intervention are 9.0 percentage points less likely to have a thirty-day late payment, contributing to an 8.2 point increase in the FICO Score itself and a 5.1 percentage point decrease in the likelihood of being classified as a subprime borrower. For comparison, removal of a bankruptcy flag leads to a FICO Score increase in the range of 10 to 15 points (Dobbie et al., 2016; Gross, Notowidigdo, & Wang, 2020). While we cannot disentangle the effects of viewing one’s score separately from viewing the associated reason codes, we provide evidence that treatment effects are not driven solely by additional information provided in the reason codes.

We complement findings from this field experiment by analyzing responses to a survey conducted by Selfie Mae one year after the start of the intervention, completed by a small subset of our borrower population. The survey asked participants questions about their FICO Score knowledge and general financial literacy. We find that treatment group members were more likely to accurately report their score. Specifically, treatment group members were less likely to overestimate their scores. This is consistent with literature on overoptimism and overconfidence (Kahneman & Tversky, 1996; Fischhoff, Slovic, & Lichtenstein, 1977; Svenson, 1981) and suggests the intervention may lead to behavior change in part by allowing people to properly calibrate their creditworthiness. In contrast, we find no differences in general financial literacy or the ability to identify actions associated with improving creditworthiness across experimental groups.

We test whether repeated email reminders are necessary to maintain the effects on financial outcomes we observe in the first year of the intervention by using a separate sample: our “discontinued sample” who received emails for only the first year of the intervention. We find no significant differences in financial outcomes between the main treatment group and the discontinued sample, evaluated a full year after the discontinued sample stopped receiving communications, suggesting no additional positive effect of repeated reminders on financial behavior. Additionally, we tested whether the content of the message affected FICO Score views or financial outcomes by varying whether the quarterly email contained (a) instructions on how to view their score; (b) instructions plus additional information about economic consequences of FICO Scores; or (c) instructions plus additional information about peer behavior. We saw no differences as a function of the specific message received.

Our intervention design builds on several promising strategies that focus on improving financial outcomes. One strategy aims to improve decision making through enhanced disclosures. In the context of payday lending, Bertrand and Morse (2011) found that disclosures clarifying interest costs reduced borrowing. Motivated by these insights, recent legislation, such as the CARD Act and modifications to the Truth-in-Lending Act (TILA), mandate the use of enhanced disclosures for a range of financial products. However, research suggests that complexity or confusion over the information provided in these disclosures inhibits their efficacy (Carpenter et al., 2017). For example, Lacko and Pappalardo (2010) find that mortgage cost disclosures required by TILA have been ineffective, with many consumers misunderstanding key terms. Similarly, Seira, Elizondo, and Laguna-Muggenburg (2017) find no evidence that TILA-type information disclosures change consumer behavior. Agarwal et al. (2014) find that the CARD Act’s 36-month disclosure requirement led to minimal changes in payment behavior overall. This is consistent with findings on the role of simplification of information in improving consumer choice in the context of Medicare prescription drug plans (Kling et al., 2012) and EITC take-up (Bhargava & Manoli, 2015).

Reminders have also been shown to help people accomplish desired actions such as building savings or repaying debt (Cadena & Schoar, 2011; Karlan et al., 2016; Bracha & Meier, 2019). Consistent with an account of limited attention (Bordalo, Gennaioli, & Shleifer, 2013; Chetty, Looney, & Kroft, 2009; Malmendier & Lee, 2011), reminders operate by making previously known information salient at the right moment. In two very different contexts, Bracha and Meier (2019) and Cadena and Schoar (2011) find evidence that reminding individuals to pay their bills on time leads to reductions in delinquencies and positive financial outcomes. Karlan et al. (2016) focus on a different financial behavior, personal savings, and also find positive effects of reminder messages.

A related literature suggests that correcting for an existing bias or other cognitive limitation may improve financial decision making (Hershfield et al., 2015; Madrian et al., 2017). For example, people are often overly confident about their own knowledge and ability (Kahneman & Tversky, 1996; Fischhoff, Slovic, & Lichtenstein, 1977; Svenson, 1981). This has consequences in a variety of domains. In the context of creditworthiness, Perry (2008) finds that more than 30% of people overestimate their credit scores, suggesting that overoptimism could contribute to poor financial decision making. This miscalibration of one’s own creditworthiness may have direct consequences for consumer financial outcomes.

1This component of the CARD Act required lenders to state the amount consumers would need to pay each month to repay their bill in full in three years. Notably, changes that were observed were primarily driven by an increase in the share of accounts paying exactly the 36-month amount.

2The positive effects observed in Bracha and Meier (2019) are limited to individuals with the lowest baseline credit scores. This intervention also included reminders about creditworthiness and the consequences of low credit scores.

3For example, Biais et al. (2005) show that overconfident traders are more likely to demonstrate the winner’s curse, and Camerer and Lovallo (1999) show that overestimating chances of success in a new venture can lead to increased market entry and financial loss.
One common thread across these distinct approaches to improving outcomes for consumers is the efficacy of strategies that include personalized information. For example, Seira et al. (2017) find that among the range of disclosures considered, the only message that influenced credit behavior was a warning that highlighted borrowers’ low credit scores. Similarly, the most effective reminder message in Karlan et al. (2016) included information on the individual’s personal savings goals. This is consistent with prior research showing that individuals are responsive to negative feedback about their financial behaviors. For example, Agarwal et al. (2008) find individuals who incur credit card fees take steps that serve to reduce fees incurred over time. Moreover, Bracha and Meier (2019) show that providing individuals with delayed, and therefore potentially inaccurate, information about their creditworthiness may have unintended consequences.

By providing individuals with quarterly messages to view their current FICO Score, our intervention incorporates several of the promising elements from these studies: reminders, clearly communicated information and personalized feedback. Our intervention also highlights the promise of interventions designed to correct for cognitive biases.

The paper is structured as follows. Section II provides background on FICO Scores and the Open Access initiative. Section III presents an overview of the field experiment. Section IV provides a description of our data. Section V presents findings on the effect of the intervention on viewing behavior and financial outcomes. Section VI discusses mechanisms. Section VII concludes.

II. Background on FICO Scores and Open Access Initiative

FICO Scores, a product of the Fair Isaac Corporation, are commonly used by financial institutions to make consumer credit decisions. Scores are calculated using information collected by the major credit bureaus and are constructed using a proprietary algorithm that incorporates information about an individual’s outstanding debt, payment history, length of credit usage, mix of credit used, and applications for new credit (see appendix figure 1). Although the FICO Score is traditionally used to assess creditworthiness by lenders, it has become increasingly utilized outside the financial services sector (Bartik & Nelson, 2016; Clifford & Shoag, 2016; Dobbie et al., 2016).

In recent years there has been a push by policymakers, regulators, and financial service providers to increase consumer access to their credit information, including credit reports and credit scores. In November 2013, FICO joined this effort by launching the FICO Score Open Access Program. Through this initiative, institutions that purchase FICO Scores for use in risk management make those scores available directly to the consumers. All financial institutions that participated in the program made FICO Scores available to their customers free of charge via a password-protected website. Alongside the FICO Score itself, institutions were required to include two “reason codes” that explain the key factors contributing to the individual’s score, such as limited credit history or account delinquency. As of January 2018, FICO had partnered with eight of the top ten credit card issuers and more than 100 financial institutions, including Bank of America, Wells Fargo, Chase, and Citi, to provide free access to more than 250 million consumer credit and loan accounts in the United States (FICO, 2018).

III. Experiment Overview

On June 24, 2015, Sallie Mae, a national financial institution specializing in student loans, joined the FICO Score Open Access Program and began providing free score access to customers through their website. Consistent with requirements of the Open Access Program, clients who logged in to the website saw a visual display that included their FICO Score beside a barometer showing the range of possible FICO Scores and two reason codes (see figure 1a).

While all customers had the ability to log in and view this information, many borrowers may not have been aware of the new program. To test the effect of providing information about a borrower’s FICO Score, we experimentally vary knowledge of score availability through additional communication about the program across the 406,994 student loan borrowers who held a loan with Sallie Mae at the start of the FICO Score Open Access Program and continued to hold that loan for the following two years.

A. Experimental Conditions

Prior to the rollout of the FICO Score Open Access Program at Sallie Mae, borrowers were randomly assigned to one of four experimental groups: three treatment groups and one control group. Roughly 90% of our sample was assigned to one of the treatment groups, while the control group contained the remaining 10% of the sample.\(^4\) Borrowers assigned to the treatment groups received quarterly email communications from Sallie Mae alerting them to the availability of their FICO Score and providing instructions on how to access the information; control group members did not receive any communication about the program beyond what was stated on Sallie Mae’s website.

All emails included a short description of the FICO Score and informed borrowers that their score was available to view. The emails also included a link to log in to view the score through Sallie Mae’s website. Treatment group members received these communications once per quarter on the date that scores were updated informing them that their FICO Score had been updated and, again, providing a link to log in to

\(^4\)Sallie Mae limited the control group to 10% of the sample in an effort to maximize the number of clients receiving information about score availability while still preserving the ability to estimate the effect of the intervention.
view the score. Due to privacy considerations, no personalized information was included in the email itself.\footnote{This requirement was not specific to Sallie Mae. Our understanding is that the FICO Score Open Access Program requires FICO Score information to be displayed on a password-protected website rather than displayed in an email.}

Borrowers in the treatment group were randomly assigned to receive one of three email messages: (a) baseline, (b) economic consequences, or (c) social influence. In the baseline condition, borrowers received only the information described above (figure 1b). The two additional conditions included the same information as the baseline email as well as additional messaging. In the economic consequences condition (appendix figure 2a), clients received an email that was intended to emphasize the impact of the FICO Score on economic outcomes (e.g., “When you apply for credit—whether it’s a credit card, car loan, student loan, apartment rental, or mortgage—lenders will assess your risk as a borrower.”). Building on research demonstrating the effectiveness of messaging informing individuals of prosocial actions of their peers (Allcott, 2011; Ayres, Raseman, & Shih, 2013; Cialdini & Goldstein, 2004; Kast, Meier, & Pomeranz, 2012), the social influence condition (appendix figure 2b) included messaging informing readers that their peers were taking actions to improve their credit (e.g., “Many of your peers are building strong financial futures. You can, too, by effectively managing your student loans.”). Our main analyses focus on variation between the control group and all treatment groups...
combined. However, section VID investigates the relative effectiveness of the different treatment messages.

B. Experiment Time Line

The three treatment groups in the main sample received eight quarterly emails starting in June 2015. Each treatment group received their assigned message for three consecutive quarters (June, September, and December 2015). However, beginning in 2016, all three treatment groups received only the content included in the baseline email message. In other words, clients in the economic consequences and social influence conditions began receiving the baseline message starting in March 2016; clients in the baseline condition continued to receive the baseline message. The control group never received any direct communications about the program.

The experimental design included a separate population of 37,393 borrowers—the “discontinued sample”—that received quarterly emails for only three quarters. This sample was also split into three treatment message groups and received quarterly email communications in June, September, and December 2015. Our main analysis focuses on the 326,609 treatment group members who received quarterly communications through the end of the intervention in June 2017. We use the discontinued sample to test whether continued communication has an impact on FICO page views and on subsequent financial outcomes in section VIC.

IV. Data

A. Email and FICO Score Page View Data

Over the course of the study period, Sallie Mae tracked whether a borrower opened our treatment emails as well as each time a borrower viewed the FICO score page on the web portal that users access online by logging in with their username and password. We use this information to construct weekly and quarterly indicators for whether the borrower viewed our treatment messages or their FICO Score page throughout the study period. Our data on email open rates range from June 24, 2015 to June 9, 2016, while our FICO Score page view data range from June 26, 2015 (two days after the intervention began) to June 12, 2017.

B. Credit Bureau Data

Each quarter, Sallie Mae receives updated credit report information for each borrower as part of its routine business practice. The credit report information is provided by TransUnion, one of three major national credit reporting agencies, and is used to calculate the borrowers’ FICO Score. The score is then made available to the borrower through the FICO Score–specific knowledge. In June 2016, one year after the program began, Sallie Mae solicited survey responses from all current borrowers in the experimental sample. These data were linked to each borrower’s treatment status to evaluate the effect of the intervention on survey responses. The survey contained questions on the borrower’s awareness and use of various financial communications and products provided by Sallie Mae with a specific focus on the FICO Score Open Access Program. Questions asked each borrower

8See appendix figure 3 for a summary of the experimental timeline.

7Because the FICO Score Sallie Mae provides is based on this information from TransUnion, the score made available to the borrower does not change within each quarter.

6Responses were solicited via email, and borrowers had up to one month to participate. Sallie Mae sent email reminders encouraging borrowers to take the survey but did not provide an incentive for participating.
Importantly, these self-reported scores could then be linked to an individual’s actual FICO Score to assess the accuracy of the self-report. Additionally, the survey contained a wide variety of questions to assess the borrower’s general financial literacy, including awareness of positive credit behaviors. Additional details on these questions are in appendix C. Finally, participants responded to a series of demographic questions focusing on academic details such as college type, year and field of study, and student loan details.

Of the more than 400,000 borrowers who were asked to participate, only 3,511 individuals completed the survey. While this low response rate is in line with previous survey requests sent by the lender, it raises some questions about the external validity of this data source. Appendix table 2, panel A reveals several small but significant differences between survey respondents and nonrespondents in baseline demographic and credit data drawn from the June 2015 TransUnion credit report. For example, survey respondents were slightly older (27 versus 25), more likely to be out of school (54% versus 45%), and had higher FICO Scores (696 versus 675) than nonrespondents.

While the comparison of baseline characteristics reveals some differences between respondents and nonrespondents, an examination of treatment status by survey response shows no such differences. Appendix table 2, panel B shows that borrowers assigned to the treatment condition were equally likely to participate in the survey: 88.9% of survey respondents were assigned to the treatment condition versus 89.6% of survey respondents had higher FICO Scores (696 versus 675) than nonrespondents.

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V. Main Results

A. Dynamics of Email Open Rates and FICO Score Page Viewing Patterns

We begin our analysis by investigating whether borrowers in the treatment group opened our quarterly emails and, if so, whether these communications led to an increase in the likelihood of viewing their FICO Score page. We use administrative data from Sallie Mae on daily email open rates and FICO Score page views.

Figure 2 presents email open rates for treatment group members by week for the first year of the intervention. Quarter labels correspond to the weeks in which the intervention emails were released. Figure 2a displays email open rates by week, while figure 2b presents the percent of treatment group borrowers who had ever opened a treatment email by the week in question. Email open rates were highest in the week of the email release, with very few borrowers opening the email after two weeks of the sent date. Twenty-one percent of treatment group members opened the first email, and 48% of treatment group members opened at least one of the quarterly emails by the end of the first year of the intervention. This means that over half of borrowers in our treatment sample never received the information contained in our treatment messages.

Figures 2c and 2d mirror figures 2a and 2b but present patterns of weekly FICO Score page views rather than email open rates. Since all Sallie Mae clients had access to their FICO Score through the website regardless of treatment status, we present data for both treatment and control groups.

Figure 2c shows that less than half a percent of control group members viewed their FICO Score page in most weeks, with 12.3% of control group members viewing at least once by the end of year 1 and 19.0% by the end of the two-year intervention. This suggests that even in the absence of email communications about the program, some borrowers were aware of the availability of FICO Scores and did view them. However, the figures also show that receiving a quarterly email boosts FICO Score page views even further. Treatment group members saw a large spike in the number of FICO Score page views in the first week after each email was sent, ranging between 3% and 6% of borrowers viewing their scores in the week of the email release. Additionally, these effects do not fade over time: continued viewing is driven by a combination of borrowers who have already viewed their scores doing so again, as well as borrowers checking their score for the first time late in the study period, as shown in figure 2d. By the end of the first year of the intervention, 21.4% of treatment group members viewed their score at least once, increasing to 31.4% by the end of the second year.

Table 1 presents the corresponding regression estimates of the effect of the treatment on FICO Score page views. Columns 1 and 3 show that treatment communications led to an increase of 8.1 and 12.4 percentage points in the likelihood of viewing one’s FICO Score page by the end of years 1 and 2, respectively. Columns 2 and 4 show that the intervention also led to significant increases in the number of page views. Appendix table 3 presents corresponding estimates at the quarter level.

While these estimates suggest that our intervention led to a significant increase in the likelihood of viewing one’s score through Sallie Mae’s website, this does not necessarily tell us about the effects of the intervention on overall views. For example, treatment and control group members could be equally likely to have viewed their scores during the study period, but the intervention simply caused treatment group borrowers to

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9Possible responses included FICO Score ranges of 0–299, 300–449, 450–549, 550–649, 650–749, 750–850, and more than 850, or respondents could state that they did not know their score.

10Note that our estimates of the fraction of borrowers viewing their scores will be lower bound estimates since we did not capture score views on the first two days of the campaign, See section IV.
view their scores through Sallie Mae’s website rather than through a different source. We address this concern in appendix B using survey data on views from all sources during the first year of the intervention and find treatment effects on the likelihood of ever having viewed one’s FICO Score through any source that are nearly identical: 8.0 versus 8.1 percentage points.

B. First-Year Effects on Financial Outcomes

In this section, we examine the effect of the intervention on individual financial outcomes captured by the TransUnion credit report. For each outcome, we first estimate a reduced-form regression comparing outcomes by experimental group using first-differences to control for an individual’s credit history prior to the experiment: the intent-to-treat (ITT) estimate. The regression model is as follows:

\[ Y_i = \alpha_0 + \alpha_1 T_i + \epsilon_i, \]  

where the dependent variable is the difference in the financial outcome between the quarter prior to the experiment (June 2015) and the quarter of the intervention (June 2016).
2015) and the postintervention quarter of interest and $T_i$ is an indicator for individual $i$ being randomly assigned to the treatment condition. Therefore, the coefficient of interest, $\alpha_1$, can be interpreted as the causal impact of sending quarterly emails about FICO Score availability on the within-person change in credit record outcomes, that is, the difference-in-differences estimate comparing treatment and control groups before and after the start of the intervention. For our main specification, we consider the first-year impacts of the intervention; section VC considers longer-term impacts.

A key component of our intervention is information about the availability of one’s FICO Score. However, as detailed above, only 48% of individuals in the treatment group ever opened an email message from Sallie Mae in the first year of the intervention, and treatment group members were only 8 percentage points more likely to have ever viewed their FICO Score page than control group members. While the ITT estimates (presented in panel A of our main results tables) are the policy-relevant estimates for financial institutions considering a similar email campaign, we also present estimates from an analysis in which we use treatment status as an instrument for ever opening an email (panel B) and for ever viewing one’s FICO Score page (panel C). The former provides an estimate of the treatment-on-the-treated effects of our informational messages, while the latter aims to isolate the effect of viewing one’s FICO Score page rather than simply reading the email.

The validity of these instrumental variables (IV) estimates depends on whether the additional informational content included in the intervention has an impact on financial behavior. We investigate the potential effect of several intervention components other than the FICO Score in section VIC and find no evidence that they directly affect financial outcomes. While this does not prove the validity of the exclusion restriction, it provides suggestive evidence that (at least for the components we study) the additional financial information contained in the treatment emails did not lead to a change in financial behaviors.

Late payments. We begin our analysis by considering the effect of our intervention on the prevalence of late payments. Repayment behavior has important implications for borrowers’ creditworthiness and overall financial health. Each payment period, borrowers have the option of paying off their balance or rolling over some or all of their debt to the following period. Not all borrowers may be able to pay their full balance at each billing period, and they may not want to if the interest rate on their credit card is lower than the cost of other credit alternatives (such as payday loans). However, failing to make a minimum payment, which is typically only between 1% and 4% of the total balance (Keys & Wang, 2019), can lead to negative outcomes such as penalty fees, higher interest rates, and lower credit scores. Late fee penalties alone cost consumers more than $11 billion per year (Sidell, 2016). Therefore, many borrowers may benefit from successfully paying off their minimum balance each month.

Table 2, column 1 presents the effect of the intervention on the change in likelihood of having at least one trade account balance past due for over thirty days within the past six months. Panel A shows that treatment group members were significantly less likely to have an account that was thirty days or more past due, a 0.7 percentage point decrease. Given that only 17.5% of control group members had a balance thirty or more days past due at the end of year 1, this is a relatively large (4%) reduction.

As mentioned above, panels B and C present two alternative estimates that use treatment status as an instrument for the likelihood of opening an email and viewing one’s FICO Score page, respectively. We find that opening the treatment email is associated with a 1.5 percentage point decrease in the likelihood of having an account thirty days or more past due. Turning to panel C, we find that borrowers who were induced to view their FICO Score page as a result of the intervention are roughly half as likely as the control group to have a late payment (a reduction of 9.0 percentage points).

FICO Score. We next turn to the effect of the treatment on the FICO Score itself, a summary metric that captures the net effect of the intervention on creditworthiness. Appendix figure 1 describes some of the key components that affect an individual’s FICO Score; for example, payment history (i.e., whether balances are paid on time) accounts for 35% of the score. The significant impact of our intervention on late payments suggests that we may expect to see an increase in the FICO Score itself; however, it is possible that the positive effect of reduced late payments is offset by other unobserved negative financial behaviors, leaving the net effect ambiguous.

Table 2, column 2 presents the estimated effects of the treatment on the individual’s FICO Score. Borrowers in the control group have an average FICO Score of 676 at the end of year 1. Our results show that receiving the quarterly emails significantly increased the average FICO Score of treatment

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Source: Sallie Mae and TransUnion, June 2015 to June 2016. Outcomes: Indicator for having a balance 30+ days past due in past six months (column 1), FICO Score in points (column 2), and indicator for FICO Score of at least 620 (column 3). All outcomes are first differences between June 2015 and June 2016. Panel A: ITT estimate comparing treatment and control groups. Panel B: Instruments ever opening treatment email with treatment status. Panel C: Instruments ever viewing FICO Score page with treatment status. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$. 

Downloaded from http://direct.mit.edu/rest/article-pdf/103/2/236/1915848/rest_a_00888.pdf by guest on 22 August 2021
group members by two-thirds of a point. Our instrumental variables estimates show that opening an email is associated with a 1.4 point increase, while borrowers who were induced to view their FICO Score page saw an 8.2 point increase.

To calibrate the size of the effect, the removal of a bankruptcy flag leads to FICO Score increases on the order of 10 to 15 points (Dobbie et al., 2016; Gross et al., 2020). Separately, appendix figure 1 shows that length of credit history accounts for 15% of the FICO Score; therefore, the relationship between credit history length and FICO Score provides us with another benchmark. In our sample, we find that an additional year of credit history is associated with a 1.1 point increase in FICO Scores, an effect that is only slightly larger than our ITT estimate of the effect of the intervention.

It is important to underscore that the FICO Score is designed as a measure of credit worthiness to be used in underwriting and is therefore not necessarily an accurate measure of financial health or well-being. However, financial institutions frequently use FICO Scores when making lending decisions or determining borrowing terms. For example, Gross et al. (2020) find that a 10-point increase in credit scores is associated with a third of a percentage point decrease in the regular purchase APRs on new credit cards for consumers with credit scores between 600 and 700. Credit information is also used in other contexts. For example, credit reports are frequently used as inputs by landlords to determine eligibility for rental apartments or by employers in hiring decisions (Bartik & Nelson, 2016; Clifford & Shoag, 2016; Dobbie et al., 2016).

However, differential treatment as a function of credit scores is not always linear: banks frequently change lending terms at discrete cutoffs. For example, Federal Housing Authority loans (e.g., those issued through Fannie Mae) require a minimum credit score of 620 for most conforming fixed-rate mortgages, a common threshold used to define a subprime borrower.

Table 2, column 3 looks at the effect of the intervention on having a FICO Score above 620. The treatment led to a significant increase of just under half a percentage point in the likelihood of having a score over this threshold with a treatment-on-the-treated estimate of 0.9 percentage points. Borrowers who were induced to view their FICO Score page saw an increase in the likelihood of being above the threshold of 5.1 percentage points.

Other credit outcomes. We next explore the impact of the intervention on other outcomes reported on borrowers’ credit reports. Appendix table 4 examines the effect of our intervention on general measures of credit usage, including the likelihood of having an account, number of accounts, account balance, and credit utilization. Our analysis focuses on revolving trade activity (most commonly, credit card accounts). These are trade accounts that are plausibly easy to open or close in response to learning about one’s FICO Score, unlike, for example, a mortgage or an auto loan. The first column presents estimates of the effect of the treatment on the likelihood of having any open revolving credit account. We find that the intervention caused a small but significant increase of 0.3 percentage points in the likelihood of having at least one account (on a base of 76% among control group members), an important step toward establishing a credit history. We observe a similarly small but significant increase in the number of accounts held (an increase of 0.01 accounts) and an insignificant increase in the total balance. We also find that the effect of the treatment on credit utilization is small and not statistically significant.

We perform a multiple hypothesis correction following List, Shaikh, and Xu (2016) that includes all outcomes examined in table 2 and appendix table 4. While all of the outcomes considered in our main analysis in table 2 remain significant after this correction, the estimated effect of treatment on likelihood of having an account and number of accounts is no longer significant.

C. Heterogeneity of Treatment Effects

Baseline FICO Score. One question is whether the intervention was effective for the people who needed help the most—those with lower FICO Scores—or whether the treatment moved behavior only among those who were already performing well on this metric. In appendix figure 4, we examine heterogeneity of the treatment effect on having a late payment and on FICO Score by preintervention FICO Score decile.

Appendix figure 4a shows that, while the point estimates for the effect of the treatment on late payments are negative for all deciles, the treatment effect is largely consolidated among borrowers in the second decile (borrowers with baseline FICO Scores between 600 and 638). Specifically, while the treatment led to an average decrease in late payments of 0.4 percentage points across the other nine deciles, the treatment led to a decrease of 3.2 percentage points for borrowers in the second decile. Consistent with payment history serving as a key input for FICO Scores, appendix figure 4b also finds the largest effects on FICO Scores for borrowers in the second decile, although the differences across deciles are not as pronounced as for late payments. These patterns may be partly due to the fact that late payments are concentrated in the bottom two deciles. Fifty-five percent of borrowers in the bottom two deciles have late payments at baseline compared to only 3% in the other eight deciles combined. It is somewhat surprising that we do not see similarly large effects among borrowers in the first decile. However, one key difference between borrowers in these bottom two deciles is the propensity to have a late payment ninety days or more past due (i.e., to have a delinquent account). Two-thirds of the past-due accounts in the first decile are pronounced as for late payments.

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Figure 3.—Treatment Effects by Quarter

(a) Late Payments

(b) FICO Score

Source: Sallie Mae, June 2015 to June 2017. Timeline labels correspond to release dates of quarterly communications. Outcomes: indicator for having a balance 30+ days past due in past six months (A) and FICO Score (B). All outcomes are first-differences between June 2015 and the given quarter.

Other borrower characteristics. It is also possible that the treatment had differential effects on borrowers of different age groups. Younger borrowers are less likely to have financial experience and may be less aware of how to improve their own creditworthiness; therefore, we might expect that our intervention would be particularly successful in this population. Alternatively, older borrowers may respond more to the intervention since they have more actions available to take as a result of having more established finances. A related yet distinct categorization of borrowers is whether they are in school versus out of school. For example, borrowers who are out of school are likely to be working and may have more financial responsibilities. While these two sets of borrower characteristics are correlated, 30% of borrowers who are still in school are above the median age.

Appendix table 5, columns 1 and 2 present treatment effects by age for late payments and FICO Score, respectively, comparing borrowers who are above or below the median age of 23 years old at the start of the intervention. Columns 3 and 4 present treatment effects for the same outcomes by student status, comparing borrowers who are in school with those who are out of school. We find no significant differences in treatment effects by age or student status for either financial outcome, though the point estimates suggest that the effects are slightly larger among borrowers hypothesized to have more financial responsibilities: older borrowers and those who are out of school.

D. Long-Term Effects

The estimated treatment effects presented thus far are evaluated one year from the start of the intervention, from June 2015 to June 2016. To examine both the longer-term treatment effects and how the effects evolve over time, figure 3 presents ITT estimates quarterly for the full two-year study period from June 2015 to June 2017.

Figure 3a presents quarterly treatment effects for the likelihood having a late payment of thirty or more days past due. Our results show that the size of the treatment effect is greatest approximately twelve to fifteen months from the start of the intervention. After fifteen months, the treatment effect attenuates and by the end of the two-year period is no longer statistically significant. Figure 3b presents the estimated effect of the treatment on borrowers’ FICO Scores in each quarter. Here again we see that the estimated coefficient is largest one year from the start of the intervention; however, the effect remains fairly consistent through the end of the two-year study period.

VI. Mechanisms

The previous section shows that our informational campaign led to a reduction in late payments and an increase in FICO Scores. In this section, we investigate potential mechanisms driving these effects.

A. Personal FICO Score Knowledge

Previous research has shown that people are often overly confident about their own knowledge and ability in a range of domains (Kahneman & Tversky, 1996; Fischhoff et al., 1977), including evidence of overestimation in the context of credit scores (Perry, 2008). One potential mechanism by which the intervention could operate is by correcting biases in perceptions of one’s own FICO Score. We examine this possibility using information from our second data source, the FICO and Financial Literacy Survey, which asked respondents several questions about their knowledge of personal
financial information, specifically, their own FICO Score. Respondents were asked if they knew their FICO Score and, if so, were asked to indicate their score within a range of 100 to 150 points. Using data from our administrative credit reports, we can then verify the accuracy of these self-reported scores. Consequently, our definition of accuracy corresponds to knowing the correct 100 to 150 point range.

Column 1 of table 3 shows that treatment group members were 7.1 percentage points more likely to report an accurate FICO Score range on a base of 51.5% accuracy among control group members, a 14% increase. Column 2 looks at the effect of the intervention on the likelihood that a respondent reports knowing his or her FICO Score and finds that treatment group members were 4.3 percentage points more likely to report knowing their score. Finally, columns 3 and 4 decompose the gap between reported and accurate knowledge to examine the effects of the intervention on the likelihood of overestimating versus underestimating one’s FICO Score, respectively. We find that receiving a treatment message significantly decreased the likelihood of borrowers reporting an overestimate of their FICO Score by 3.4 percentage points but had no significant impact on the likelihood of underestimating one’s score.

These findings suggest that the intervention provided borrowers with important feedback that they could use to calibrate their personal creditworthiness. Our findings are consistent with existing evidence of overoptimism in knowledge of personal creditworthiness (Perry, 2008) and with evidence that overconfidence and overoptimism negatively affect performance in other areas (Biais et al., 2005; Camerer & Lovallo, 1999). Our evidence suggests that debiasing these misperceptions may lead to improvements in financial behaviors.

### Table 3.—Personal FICO Score Knowledge

<table>
<thead>
<tr>
<th>Measure</th>
<th>Treatment</th>
<th>Control Mean</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Range Score (1)</td>
<td>0.0712***</td>
<td>0.515</td>
<td>3,511</td>
</tr>
<tr>
<td>Did Not Know Score</td>
<td>-0.0433***</td>
<td>0.227</td>
<td>3,511</td>
</tr>
<tr>
<td>Overestimate Score (3)</td>
<td>-0.0343***</td>
<td>0.108</td>
<td>3,511</td>
</tr>
<tr>
<td>Underestimate Score (4)</td>
<td>0.0065</td>
<td>0.149</td>
<td>3,511</td>
</tr>
</tbody>
</table>

Source: FICO and Financial Literacy Survey, June 2016. Outcomes: indicators for recalling accurate personal 100–150 point FICO Score range (column 1), reporting no knowledge of one’s personal FICO Score (column 2), and reporting overestimated or underestimated FICO Score range (columns 3 and 4). Treatment group includes borrowers who received a treatment message at any point in the intervention. Robust standard errors in parentheses. ***p < 0.01, **p < 0.05, and *p < 0.1.

### Table 4.—Treatment Effects by Baseline Reason Code

<table>
<thead>
<tr>
<th>Reason Code</th>
<th>Late Payment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment</td>
<td>-0.0055***</td>
</tr>
<tr>
<td>Delinquency</td>
<td>-0.0055</td>
</tr>
<tr>
<td>Actionable</td>
<td>-0.0113***</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.175</td>
</tr>
</tbody>
</table>

Source: Sallie Mae and TransUnion, June 2015 to June 2016. Outcomes: indicator for having a balance 30+ days past due in past six months. All outcomes are first differences between June 2015 and June 2016. Delinquency Code is an indicator for having a reason code in June 2015 (the preintervention quarter) that mentions a delinquent account. Actionable Code is an indicator for having a reason code in June 2015 that mentions a direct action a borrower could take to improve their FICO Score. Robust standard errors in parentheses. ***p < 0.01, **p < 0.05, and *p < 0.1.

#### B. Reason Codes

As mentioned in section III, all financial institutions that made FICO Scores available through the Open Access initiative were required to include two reason codes that provided an explanation of the primary factors contributing to their score along with the FICO Score itself. Although our estimate of the effect of the FICO Score page as a whole is the relevant estimate for evaluating the impact of the FICO Score Open Access initiative, in this section we explore the relative effectiveness of these two components: the FICO Score and the reason codes.

In our sample of borrowers, the top three categories of reason codes shown to borrowers pertain to credit history length (e.g., “Length of time accounts have been established”), debt levels (e.g., “Proportion of loan balances to loan amounts is too high”), and late payments (e.g., “Number of accounts with delinquency”). Some reason codes refer to specific actions a borrower can take to improve her score. For example, at baseline 34% of our sample were shown a reason code directly related to late payments, one of our focal outcomes. On the other hand, 30% of borrowers received information with no direct implications for action (e.g., “Length of time accounts have been established”).

Table 4 estimates treatment effects on late payments separately for those who received (versus did not receive) a delinquency reason code (column 1) as well as for those who received any actionable (versus not actionable) reason code (column 2) at the start of the intervention. The sign on the interaction term coefficient in column 1 suggests that the treatment effects on late payments are larger for individuals with baseline delinquency reason codes. Yet the intervention also led to a statistically significant decrease in the likelihood of having a late payment at the end of the first year among treatment group members with no delinquency reason code at baseline. Column 2 also shows significant treatment effects for borrowers who did not receive any actionable reason code at baseline.
While we cannot rule out that reason codes had an independent effect on financial behavior, these findings suggest that the reason codes are not the only component of the viewing page driving behavior change. Additionally, this suggests that the decrease in late payments is unlikely to be solely driven by individuals reconciling previous past due accounts, but that the intervention reduced the likelihood that an individual would enter into delinquency going forward.12

C. Repeated Reminders

Consistent with an account of limited attention (Bordalo et al., 2013; Chetty et al., 2009; Malmendier & Lee, 2011), another possibility is that our intervention did not provide borrowers with any new information but acted as a repeated reminder (Cadena & Schoar, 2011; Karlan et al., 2016). In this section, we examine a separate sample, our “discontinued sample,” who were randomly assigned to receive quarterly email communications for only three quarters rather than throughout the two-year intervention as in our main treatment sample. This sample allows us to test the impact of additional email communications on viewing rates and financial outcomes to determine if repeated reminders led to improved outcomes.

Appendix figure 5 presents weekly FICO Score page view rates for the control group, the discontinued sample, and the main treatment sample. The figure shows that the FICO Score page view rates for the main treatment sample and the discontinued sample are virtually indistinguishable for the first three quarters of the email campaign, which is expected since the two groups received the same treatment during this time period. However, starting in March 2016, when the discontinued sample stopped receiving email communications, the discontinued group’s view rates began to closely track the control group rather than the treatment group.

Table 5 shows the regression estimates for the effect of the main versus discontinued treatment on viewing and financial behaviors. Column 1 presents treatment effects for the two treatment samples on the likelihood of viewing one’s score before March 2016, the last quarter in which the two groups had received the same treatment. Unsurprisingly, we see no difference in treatment effects between the two groups prior to March 2016. However, starting in the following quarter, we see the two groups diverge. Column 2 shows that one year after the discontinued group stopped receiving the quarterly emails, the treatment effects on viewing rates for the main sample were twice as large as those for the discontinued group: 10.9 versus 5.3 percentage points.

While our results show that individuals who continue to receive reminders to view their FICO Score page are more likely to do so than those who received reminders for a limited time, it is not necessarily true that repeated reminders will lead to larger changes in behavior. For example, borrowers may take a discrete action upon first viewing their score (e.g., signing up for automatic payment on a credit card) that could then have a persistent positive effect on financial outcomes.

Table 5, columns 3 and 4 present treatment effects on the likelihood of having a late payment and borrower FICO Score, respectively, for the two samples one year after the discontinued group stopped receiving communications. As we saw in section VC, the effects for the main treatment group are attenuated but largely persistent almost two years after the program’s inception. Similar to the estimates for the discontinued sample are only slightly smaller than those in the main treatment group: there is no statistically significant difference between the financial outcomes of those who continued to receive emails and those who stopped receiving emails a year prior.

These results are consistent with a story in which the long-term effects on financial outcomes stem from a one-time change in behavior rather than a sustained behavioral change. An alternative story that is also consistent with these results is that individuals who view their score only after receiving repeated reminders are less likely to respond to the treatment. We explore the relationship between the timing of viewing patterns and changes in financial outcomes in appendix table 6. Columns 1 and 2 exclude borrowers who viewed their FICO Score page for the first time in year 2, while columns 3 and 4 exclude those who viewed their score for the first time in year 1. We consider the effect of our treatment in these two samples on the likelihood of ever having a late payment in year 1 (columns 1 and 3) and separately in year 2 (columns 2 and 4). We find that treatment effects in year 1 are only significant for the sample that includes borrowers who viewed their FICO Score page in year 1; the results for this sample are somewhat smaller in year 2 (consistent with figure 3). However, we see small and not statistically significant results in both years for the sample that excludes borrowers who viewed their score in the first year of the intervention. This suggests that borrowers who wait to view until later in

12 A parallel analysis that considers the effect of the treatment by whether a borrower had a late payment at baseline (rather than baseline delinquency reason code) shows similar patterns: we observe significant treatment effects on late payments even among those without a late payment on their credit report at the start of the intervention.
the intervention are less likely to change their behavior in response to the information provided.

D. Additional Informational Content in Email

In addition to the ability to view one’s FICO Score page, the treatment email message includes content describing the importance of the FICO Score, as well as hyperlinks to additional information about FICO Scores and general financial literacy. If this additional information contributes to changes in financial behavior, our IV estimates will be overstated. In this section, we investigate the potential effect of this additional informational content on financial behavior.

Treatment effects by message type. The results in section V focus on the effect of receiving any treatment message. However, two experimental groups received additional information in their email messages for the first three quarters of the intervention. Borrowers in the social influence and economic consequences treatment groups received information about peer credit behavior and financial consequences of low FICO Scores, respectively. If borrowers were unaware of how FICO Scores affect the cost of credit, the economic consequences message may prompt additional changes in behavior. At the same time, borrowers may be additionally motivated to improve their FICO Score if they are told people like them are doing so (Allcott, 2011; Ayres et al., 2013; Cialdini & Goldstein, 2004; Kast et al., 2012).

Appendix figure 6 mirrors the analyses in figures 2c and 2d but displays FICO Score page view rates separately for the three treatment messages for the first year of the intervention. The figure shows that the viewing rates, both within a given week and the likelihood of ever viewing by a given week, are very similar across treatment messages. If anything, the baseline message very slightly outperformed the two messages that contained additional information, though this difference is quite small and not statistically significant.

Appendix table 7 presents treatment effects for late payments and FICO Score separately by treatment message type: baseline, economic consequences, and social influence. The $F$-test for equality of treatment effects across the three messages suggests that the estimates are not significantly different across treatment groups. This is somewhat unsurprising given the relatively similar FICO Score page view rates across the three treatment groups. While research has shown that nudges of this type can be effective in some contexts, we find no evidence that the additional message content affected behavior.

General financial information. One other possibility is that the intervention may have translated to differences in financial knowledge by providing links to general financial education resources. For example, these resources could make people more familiar with the concept of a credit score or good types of credit behavior. To the extent that borrowers were previously unaware that a metric like a credit score existed or that their behaviors were being tracked, that awareness could have, in and of itself, led them to take actions to improve it.

Appendix table 8 uses data from the FICO and Financial Literacy Survey, which contains questions on knowledge of several financial concepts, including knowledge of good credit behaviors, familiarity with FICO Scores, and a financial literacy quiz to address the effect of the intervention on general financial knowledge. We find no effects of the treatment on borrowers’ ability to correctly identify any individual credit behavior as positive or negative. It is interesting to note that the control means for accurately identifying each behavior are quite high—over 90% for all but one measure—suggesting that many respondents were already aware of the activities necessary to improve their credit. We also find no effects of the treatment on borrower’s general FICO Score knowledge (i.e., confidence they could explain what a credit score is to a friend) or performance on the financial literacy quiz.

VII. Conclusion

Findings from our field experiment indicate that viewing one’s FICO Score influences financial behaviors. Borrowers who were randomly assigned to receive communications informing them that their score was available to view were less likely to have late payments and had higher FICO Scores overall. These effects largely persisted throughout the full two-year intervention. Survey results provide evidence that borrowers in the treatment group were less likely to overestimate their score relative to those in the control group. It is particularly encouraging that this intervention appears to spur positive behavior change among a relatively young population that is new to credit and may therefore yield long-term benefits from immediate behavior change—for example, delinquent behavior remains on an individual’s credit history for up to seven years.

The FICO Score provides a single number that allows easy tracking of a disparate set of actions related to creditworthiness. This personalized, quantified, dynamic measure allows individuals to monitor and track their progress over time. This holistic financial metric may be particularly well suited for goal setting. For example, a large body of literature documents goal-setting behavior in which people try to achieve a certain level of performance as a function of a numeric cue, such as a race finishing time or personal best score in a game (Anderson & Green, 2018; Locke & Latham, 2002; Pope & Simonsohn, 2011; Allen et al., 2016). However, these types of goals can best be set and managed when they are able to be quantified through a single number. Similar metrics that summarize a broad set of outcomes may be effective in other areas as well, such as promoting overall health scores to

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13Similarly, the exclusion restriction for our estimates that instrument for ever opening an email from Sallie Mae will be violated if receipt of the message has an impact on financial behavior even if the email is never opened.
encouraging better health habits or promoting overall efficiency scores to encouraging better time management.

Our findings demonstrate the potential for targeted, low-cost, scalable interventions to have a positive impact on financial decision making and improve consumer financial welfare. More generally, our findings point to possible benefits of personalizing financial interventions, consistent with individual self-reports that personal experience is a key driver of financial learning (Hilgert, Hogarth, & Beverly, 2003) and with recent efforts to promote just-in-time interventions that are timed to personal financial events (Fernandes, Lynch, & Netemeyer, 2014).

One limitation of our experiment is that we are unable to see borrowers’ full financial pictures. Since we only observe information reported to credit bureaus, we cannot rule out the possibility that the intervention is encouraging people to prioritize financial behaviors that are directly tied to their credit score to the detriment of other aspects of their financial lives we do not observe, such as income and savings (Beshears et al., 2019; Medina, 2017; Sussman & O’Brien, 2016). While our intervention shows positive effects on behaviors recorded in credit bureau data, future work should examine the impact of viewing one’s score on other aspects of financial health.

——— “FICO Score Open Access Program Reaches Milestone of 250 Million Consumer Financial Credit Accounts” (Fair Isaac Corporation, 2017).

FICO, “US Average FICO Score Hits 700: A Milestone for Consumers”


