Loan Officers Impede Graduation from Microfinance: Strategic Disclosure in a Large Microfinance Institution *

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Abstract

One of the most important puzzles in microfinance is the low rate of borrower graduation to larger, more flexible loans. Utilizing observational and experimental data from a large Chilean microfinance institution, we demonstrate that loan officers impede borrower graduation due to common features of their compensation contracts. Our partner lender offers both microloans and larger, more flexible graduation loans, and relies on loan officer endorsements to determine borrower graduation. Loan officers are rewarded for the size of their portfolio and repayment, and so are implicitly penalized when good borrowers graduate. In an experiment designed to isolate strategic disclosure, we modify compensation to reduce this implicit penalty and document that loan officers withheld endorsements of their *most qualified* borrowers prior to the shift. Graduated borrowers endorsed after the shift are 34% more profitable for our partner lender than those endorsed beforehand. A back-of-the-envelope calculation suggests that strategic behavior of loan officers accounts for $4.8-29.2 billion in lost social value from forgone borrower graduations in microfinance worldwide. Our experimental design may prove useful for other experiments within firms.

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1 Introduction

Once hailed for its potential to lift the world's poorest entrepreneurs out of poverty, microfinance has fallen short of its initial promise.[1] Moreover, relatively few borrowers graduate from microfinance to larger, more formal sources of credit.[2] This is especially disappointing, and to some extent puzzling, in light of abundant evidence that outside of microcredit small-scale entrepreneurs can use capital to grow their businesses.[3] Researchers have responded to this apparent puzzle by examining alternative loan contracts and screening technologies that might raise the impact of microfinance.[4] But a complementary and somewhat less trodden approach is to examine the reasons that the microfinance industry itself has not already done this work. In particular, microfinance institutions and their employees may not be fully incentivized to support borrowers in growing their incomes and eventually graduating from microfinance (Liu and Roth, 2020).

Loan officers are often rewarded for maintaining a large borrower portfolio and high rates of repayment; 80% of MFIs represented in the MIX Market dataset use such monetary incentives.[5] These compensation schemes align the interests of loan officers with the profitability of their portfolio, but they also induce an implicit penalty when borrowers graduate to more formal sources of credit. In turn, loan officers may withhold discretionary support from borrowers when providing it would jeopardize the loan officer's compensation.[6] However, the extent to which this limits graduation rates in practice is an

[1] In a review of six experiments that randomize access to microfinance, Banerjee et al. (2015) finds at best modest impacts of microfinance on business growth. Meager (2019) confirms this conclusion in a meta analysis of these six experiments and one more. In a longer-term followup Banerjee et al. (2019) finds a positive impact of microcredit on entrepreneurs with pre-existing businesses, and Breza and Kinnan (2021) finds positive impacts of microcredit in general equilibrium, suggesting the impact of microcredit may be positive for some entrepreneurs.

[2] Karlan and Zinman (2011), Angelucci et al. (2015), Attanasio et al. (2015), Augsburg et al. (2015), Banerjee et al. (2015), Crépon et al. (2015), and Tarrozi et al. (2015) all report on experimental evaluations of microcredit, and none find evidence of that microcredit leads to an eventual increase in borrowing from formal banks.

[3] Indeed, many experiments randomizing cash grants to microentrepreneurs across the world find marginal returns to capital in excess of 5% per month. See e.g. de Mel et al. (2008), Mckenzie and Woodruff (2008) and, Fafchamps et al. (2014).

[4] e.g. Field et al. (2013), Giné and Karlan (2014), Hussam et al. (2021).

[5] Moreover, these monetary incentives are quite significant. McKim and Hughart (2005) documents that they amount to 28% of total loan officer compensation on average.

[6] This discretionary support may come in a variety of forms. For instance, loan officers are sometimes in a position to endorse high-performing borrowers for larger and more formal loans. Further, loan officers often have a measure of discretion in determining whether borrowers are offered leniency in their repayment schedule. A growing body of experimental evidence finds that allowing borrowers to match the timing of their repayments to the cash flows of their business greatly increases the impact of microfinance on business and income growth (Field et al., 2013). Finally, loan officers are often directly involved in determining the

empirical question.

We provide empirical evidence that standard loan officer compensation practices create an important misalignment of interests between loan officers and their borrowers. And we demonstrate that reforming these compensation practices has the potential to increase graduation rates out of microfinance.

Specifically, we worked with one of Chile's largest microfinance institutions. In addition to standard, joint-liability microloans, our partner lender has an internal graduation program. Borrowers who graduate from the microcredit portfolio are offered larger, more flexible, individual-liability *graduation loans*. Importantly, at the time of our study, the loan officers who managed the joint-liability loans were entirely non-overlapping with the loan officers who managed the graduation loans. Loan officers were rewarded for the size and performance of their portfolio. Thus, even though the organization benefits when high-performing borrowers move from joint-liability to graduation loans, joint-liability loan officers suffered a pecuniary penalty from borrower graduation.

Our partner lender relies on loan officer endorsements as an input into the borrower graduation process. However, prior to our study, they received few endorsements from joint-liability loan officers about borrowers who were qualified to graduate. Our partner lender hypothesized this was due to a strategic disclosure problem whereby loan officers withheld endorsements of qualified borrowers to maintain high rates of compensation.[7]

We utilize administrative data on loan officer compensation and borrower characteristics to document that there is indeed a causal relationship between the cost a loan officer suffers from losing a borrower and the likelihood she endorses that borrower for graduation. We operationalize the cost of losing a borrower in several ways; our preferred method is to estimate each borrower's *Shapley Value* – a notion from cooperative game theory that determines the portion of a loan officer's compensation attributable to each of her borrowers. Relative to more direct measures, which we also employ, the Shapley Value captures variation in the costliness of losing a borrower even for those who are not pivotal for a loan officer's compensation. Its principle draw back is that the time required to compute a borrower's Shapley Value is exponential in the number of borrowers a loan officer manages. Hence, we utilize a computationally tractable approximation described in Section 3.

borrowing limits of their clients.

[7] At the time of our study our partner lender's graduation loan portfolio was fairly new. This may explain why their compensation structure for joint-liability loan officers induced a misalignment between the interests of loan officers and growth of the graduation loan program.

To identify the causal impact of the cost of losing a borrower on the likelihood a loan officer endorses her, we exploit discontinuities in the formula by which loan officers are compensated. In effect, we compare the likelihood of endorsement for borrowers whose loan officers are far from a compensation threshold to that of borrowers whose loan officers are close to a compensation threshold, thereby isolating variation in the cost a loan officer faces of losing her borrowers. This allows us to circumvent the concern that borrowers who are costlier to lose (e.g. borrowers with larger loans) are often also more qualified for graduation. In our preferred estimate we find that, holding other borrower characteristics fixed, increasing the cost of losing a borrower by 1 standard deviation corresponds to a reduction in the likelihood of endorsement of 16% of the baseline endorsement probability.

Next, we implemented an experiment with our partner lender to quantify the impact of this strategic disclosure on forgone borrower graduations and profits. We introduced two compensation changes that reduced, and partially reversed the penalty that loan officers face when losing their borrowers. The first change, which we refer to as *Mitigation*, mitigated some of the implicit penalties that joint-liability officers incurred upon borrower graduation. Specifically, under Mitigation, loan officers were given a six month grace period during which time graduated borrowers were treated as if they were still part of the loan officer's portfolio for the purpose of determining compensation. The second change, which we refer to as *Recognition*, provided an additional reward (or recognition) for joint-liability loan officers when their borrowers graduated and performed well in the graduation loan. We also conducted several surveys eliciting endorsements from loan officers for borrowers who may be qualified for graduation loans. Our partner lender utilized these endorsements for graduation decisions, though not until after the completion of our study.

All loan officers received the compensation changes at the same time. Therefore, rather than randomizing the assignment of compensation contracts to loan officers, our experimental variation comes from randomizing the timing of surveys relative to the compensation changes. Specifically, our control loan officers received the endorsement survey one week before anyone found out about the compensation change, and therefore their endorsements were influenced by the baseline compensation contract. Our treatment loan officers received their endorsement survey immediately following the announcement of the Mitigation compensation change. As we demonstrate in our analysis, it is extremely unlikely that the one week between the two surveys was sufficient time for loan officers to gather new information about their borrowers. Hence, any difference in the endorsements between our treatment and control loan officers can be attributed to the compensation change. One month after the Mitigation contract was announced, all loan officers received news of a second change to their compensation contract – Recognition, and we conducted a final round of endorsements.[8]

This experimental design may be useful for other studies in large organizations. Managers are often reluctant to treat employees differently from one another, especially regarding the manner in which they are compensated. So, randomizing the timing of surveys relative to firm-wide changes enables researchers to evaluate the causal impact of variety of managerial practices that are too sensitive to themselves be randomized.[9]

Our experiment confirms that pecuniary penalties for losing borrowers are a substantial deterrent to loan officer endorsements. Indeed, the compensation changes resulted in several hundred new endorsements for borrowers to graduate. These represent an 11% increase in endorsements relative to those we collected in our baseline, and a far larger increase, in percentage terms, relative to those that our partner organization collected prior to our study.

The most important standard by which evaluate the compensation change, however, is not the number of additional endorsements but rather the value of the additional endorsements in predicting borrower repayment behavior. Graduated borrowers endorsed after the compensation shift exhibit significantly better repayment and are 34% more profitable for our partner lender than graduated borrowers endorsed prior to the compensation shift. This suggests that, prior to the compensation shift, not only were loan officers strategically withholding endorsements of qualified borrowers, they were withholding endorsements of their *most qualified* borrowers. Indeed, borrowers endorsed after the compensation shift also exhibited better repayment in the joint-liability portfolio, which may explain loan officers' unwillingness to endorse them under the baseline compensation scheme.

The implications of these results extend beyond microfinance institutions that have internal graduation programs. The manner in which our partner lender compensated its loan officers and the penalties loan officers suffered for borrower graduation are widespread in

[8]Due to logistical constraints we did not randomize the timing of surveys around the Recognition announcement. However we argue in our analysis that even the one month between the announcement of Mitigation and Recognition contracts is unlikely to be sufficient time for loan officers to gather meaningfully more information about their borrowers.

[9]Bassi and Rasul (2017) employ a similar design to estimate the impact of a Papal visit to Brazil on people's beliefs about fertility. But to our knowledge ours is the first study to employ this experimental design within a firm to evaluate sensitive managerial practices.

the microfinance industry. Moreover, even in microfinance institutions without internal graduation programs, loan officers have discretion over how supportive to be of their borrowers' business growth and ultimate graduation, principally through the determination of loan sizes and repayment leniency.[10] Therefore our experimental results suggest that loan officer compensation schemes may bear partial responsibility for the limited impact that microfinance has had on entrepreneurship and more broadly on borrower incomes. A back of the envelope calculation suggests that strategic behavior of loan officers may account for between USD 4.8 billion and USD 29.2 billion in lost social value worldwide. We discuss implications for policy in the conclusion.

Beyond the literature cited above, which explores the reasons underlying the low impact of microfinance, our analysis contributes to the empirical literature examining the consequences of incentive variation in firms (e.g. Baker, 1992; Shearer, 2004; Bandiera et al., 2007, 2010; Friebel et al., 2017).

We isolate and quantify the impacts of a strategic disclosure problem within a large firm. In this sense our paper complements Atkin et al. (2017), which argues that technology adoption is low amongst a set of Egyptian soccer-ball producers because of a strategic incentive of employees not to disclose the quality of the technology to their manager. The authors document a strategic disclosure problem by paying employees to demonstrate the quality of the technology to their manager, and they find that managers are more likely to implement the new technology after the demonstration. Relative to Atkin et al. (2017) we employ a more direct test of strategic disclosure and provide a richer description of its determinants.

We also contribute to the literature exploring the consequences of monetary and non-monetary incentive provision in firms and organizations with a social mission (For theory, see Bénabou and Tirole, 2006; Besley and Ghatak, 2005, 2018). This literature primarily focuses on the process of selecting intrinsically motivated workers and inducing their effort (e.g. Ashraf et al., 2014, 2019; Berg et al., 2019; Desarranno, 2019). Relative to the bulk of this literature, our paper is distinct in that we isolate a strategic disclosure problem. Rather than the question of how to motivate employees to exert the optimal level of effort, our context is one in which our partner organization wanted to elicit information already held by its loan officers. In fact, our experimental design ensures that loan officers could not exert effort to collect additional information, thereby isolating the strategic

[10]As stated in Footnote 6, a growing body of experimental evidence finds that allowing borrowers to match the timing of their repayments to the cash flows of their business greatly increases the impact of microfinance on business and income growth (Field et al., 2013).

disclosure problem.

Our paper contributes to the literature examining the decision process of loan officers and other lending agents within banks and microfinance institutions (e.g. Hertzberg et al., 2010; Cole et al., 2014; Fisman et al., 2017, 2018; Maitra et al., 2017; Vera-Cossio, 2021; Maitra et al., 2021). Most closely related are Karlan et al. (2018) and Giné et al. (2017), both of which document unintended consequences of incentive provision in microcredit institutions. In contrast to these papers, we evaluate the importance of an incentive scheme widely utilized by microfinance institutions, and we demonstrate that it leads to a substantial misalignment in the interests of loan officers and their borrowers. And once again, our emphasis on strategic disclosure is distinct within this literature.

The remainder of our paper is organized as follows. Section 2 describes the context and data. Section 3 examines the strategic determinants of loan officer endorsements and demonstrates a negative, causal relationship between the cost of losing a borrower and the likelihood a loan officer endorses her. Section 4 describes our interventions and experimental design. Section 5 documents that loan officers were withholding endorsements of qualified borrowers prior to our intervention and examines the characteristics of these borrowers. Section 6 demonstrates that borrowers endorsed after the compensation change exhibited better repayment in graduation loans than those endorsed prior to the compensation change, indicating that loan officers had been withholding their most qualified borrowers prior to the compensation change. Section 7 examines the consequence of borrower graduation on the joint-liability groups that graduates leave behind, and Section 8 concludes.

2 Context and Data

Our study was conducted in collaboration with one of Chile's largest microfinance institutions, which services more than 120,000 borrowers across the country. Their primary loan product is a joint-liability group loan. Borrowers who are geographically proximate are divided into groups of about 22 people.

The mean joint-liability loan size is USD 860, and the typical duration of a loan cycle is 4.5 months. Groups are held jointly liable for the loans of their members, such that no borrower can renew his or her loan if another group member defaults. Aside from being unable to borrow from the organization in the future, the loans of borrowers who are over

90 days late on repayments are sent to a collections agency and the central credit bureau (DICOM) is informed. These events, however, are rare. 2% of loans are 0-30 days late, 0.3% of loans are 30-60 days late, 0.2% of loans are 60-90 days late, and 0.1% of loans are over 90 days late.

While joint-liability loans constitute the majority of our partner's portfolio, they also offer a *graduation loan* product. Graduation loans are larger than the joint-liability loans, averaging 2,662 USD, are individual liability, have an average duration of 13.5 months, and repay on a monthly basis. The portion of graduation loan portfolio with 0-30 days late is 4.4%, with 30-60 days late is 1.7%, with 60-90 days late is 0.9%, and over 90 days late is 2.1%.[11]

One important feature of our partner lender is that, at the time of the study, the two loan products were housed in separate parts of the organization, supervised by different managerial hierarchies.[12] The loan officers who managed the joint-liability loans are entirely non-overlapping with the loan officers who manage the graduation loans. Typically, joint-liability loan officers are trained in social work, while graduation loan officers have backgrounds in business and engineering.

Critically, at the time of our study, joint-liability loan officers received a performance bonus based on the number of borrowers in their own joint-liability portfolio and their portfolio default rate. The average performance bonus amounted to about 25% of loan officer compensation, or about USD 330 per month. Moreover, when the number of borrowers in any of their joint-liability groups fell below 18, joint-liability loan officers were responsible for replacing lost members by the following loan cycle. Each of these features of their compensation induced penalties on joint-liability loan officers when they lost good borrowers—regardless of whether these borrowers were to leave the organization altogether or merely to graduate to graduation loans. And, at the time of our study, joint-liability loan officers were not given any reward for helping qualified borrowers to graduate out of joint-liability credit.[13] We provide a complete description of the compensation scheme employed by our partner lender prior to our intervention in Appendix Section D.

[11] We limit our graduation loan sample to borrowers who previously had a joint-liability loan and graduated after our baseline survey; whereas our joint-liability sample comprises all joint-liability borrowers in branches that offer graduation loans.

[12] In part as a result of this study, in December 2019, our partner organization combined the two loan programs under a single management structure with compensation based on the performance of both products.

[13] This compensation structure was determined before our partner lender had a graduation loan product. Our partnership began as they were trying to grow their graduation loan portfolio, which contributed to their willingness to refine loan officer compensation structure.

Data and Descriptive Statistics

Our analysis draws on a variety of survey and administrative data. Our partner lender collects data on borrower demographic and business characteristics at the first and fourth loans. We utilize administrative data on loan officer portfolio characteristics and borrower repayment at the weekly level for joint-liability loans and at the daily level for graduation loans. Further, with our guidance, our partner lender implemented a baseline survey in November 2018 during which all joint-liability loan officers were asked to endorse borrowers who are suitable for graduation. These were real-stakes endorsements; loan officers were told that their endorsements would inform the graduation process.[14] Our experiment utilizes additional surveys, described in Section 4.

Our sample comprises all loan officers and joint-liability borrowers at branches in which our partner lender offers graduation loans from October 2018 to February 2020. This represents 81,220 borrowers and 243 loan officers. Column 1 of Table A1 presents the sample descriptive statistics. The joint-liability borrowers are on average 46 years old, 39% of them are married and 63% have completed secondary school. The most common business sector is retail, representing 58% of the sample, followed by 29% in manufacturing, and 13% is services. On average, businesses in our sample earn USD 687 per month in profits, and have on average 0.12 non-household workers. The average joint-liability loan size is USD 860, and the average borrower has taken 8 loans from our partner organization.

3 Strategic Withholding of Loan Officer Endorsements

Our first exercise is to demonstrate a causal relationship between the cost of losing a borrower – in terms of forgone compensation – and the likelihood that her loan officer endorses her for graduation. This entails two challenges. The first regards determining the value that each borrower contributes to a loan officer's compensation. And the second relates to identifying the causal effect of the financial penalty of losing a borrower on the likelihood of endorsement. We discuss each of these challenges in turn.

[14]This was indeed the case, though as part of our research protocol we withheld the endorsements from our partner lender until we had enough data to judge the value of endorsements in predicting borrower repayment behavior.

Determining The Cost of Losing a Borrower

The most direct way to calculate the cost to a loan officer from losing a borrower i is by comparing the loan officer's compensation with her full portfolio to what her compensation would have been if she lost borrower i, utilizing the compensation formula in Appendix Section D. In the regressions that follow we call this $DirectCost$ and it is one of our two key independent variables.

However loan officers' compensation is a piecewise linear function with discontinuous jumps at various levels of portfolio size and risk. Therefore $DirectCost$ is 0 for most borrowers. Except in the cases where losing a borrower pushes a loan officer over a threshold, this approach disregards borrower-level variation in how close to a threshold a loan officer would be moved if she were to lose a given borrower.

We circumvent this limitation by computing a second measure of the cost of losing a borrower: the Shapley Value (Shapley, 1953). The Shapley Value is a cooperative game theoretic notion that determines the value any member adds to an arbitrary coalition. In the context of this paper, the Shapley Value for a borrower i is computed by

1. Iterating over all permutations of borrowers in borrower i's loan officer's portfolio

2. For each permutation, calculating the difference between the loan officer's compensation when she manages all borrowers who come before borrower i in the permutation *including borrower* i, and her compensation when she manages all borrowers who come before borrower i in the permutation *excluding borrower* i

3. Averaging borrower i's value-add over all permutations.

While the Shapley Value is well defined by the above formula, computing the exact Shapley Value within our sample would be computationally infeasible. There are 350 borrowers in a typical loan officer's portfolio, and therefore there are 350 factorial permutations over which the borrower's value-add must be evaluated. 350 factorial is approximately 10^{750}; for reference there are approximately 10^{82}atoms in the observable universe.

Fortunately, averages of random variables can be computed precisely with relatively few draws from their distribution. Therefore, observing that the Shapley Value for borrower i is the average value-add of borrower i to the portfolio of borrowers who come before her in a *random* permutation of all borrowers in borrower i's loan officer's portfolio, we can compute the approximate Shapley Value by taking random draws from the distribution of all borrower permutations. We compute the approximate Shapley Value to within 2.5% error by taking 500,000 draws.

In the regressions to follow, we call this $ShapleyCost$ and it is our second key independent variable.

Identifying the Causal Effect of the Cost of Losing a Borrower On Her Propensity To Be Endorsed

Next comes the question of how to identify the causal effect of the cost of losing a borrower on a loan officer's propensity to endorse that borrower. The challenge arises because features that determine the cost of losing a borrower are correlated with borrower attributes that may inform how suitable she is for a graduation loan. For instance, borrowers with larger loans are costlier to lose but may also be better candidates for graduation. The solution stems from the same observation that gave rise to the challenge above. Namely, loan officer compensation varies discontinuously around certain portfolio-level thresholds. Therefore we use discontinuities based on the number of borrowers that a loan officer manages and the average default in her portfolio to instrument for the cost of losing a given borrower.

Loan officers enjoy jumps in their compensation at 169 and 351 borrowers managed, and when their portfolio falls below 3% at risk.[15] Our instruments are (1) the distance between the number of borrowers in a loan officer's portfolio and 169 and 351 and (2) the distance between the loan officer's average default and 3%. We also include the squares of these distances. The formula by which loan officer compensation is calculated and the details of these instruments are presented in Appendix Section D.

The first stage of our instrumented regression to follow is

$$Cost_i = \alpha + \beta Z_i + \gamma X_i + \epsilon_i \tag{1}$$

Where $Cost_i$ is the November 2018 cost to borrower i's loan officer from losing borrower i – measured first as $DirectCost_i$ and second as $ShapleyCost_i$, Z_i is our vector of threshold instruments, and X_i is a vector of controls including those in Table A1, the borrower's

[15] Amount at risk is defined to be the pending amount to be repaid if the borrower is at least 7 days late in her repayments. This is the measure that our partner lender tracks to judge their own portfolio performance as well as to compute loan officer compensation.

tenure with the organization, her loan size, and the amount of her portfolio at risk, which is a summary of her default. The results are presented in Appendix Tables A2a and A2b. As expected, the closer a loan officer is to a given portfolio-size threshold, the costlier it is to lose her borrowers, and our key first-stage parameters are estimated precisely.[16] In using these instruments in the regressions to follow, our identifying assumption is that the distance between a loan officer's portfolio and a given threshold is not related to her propensity to endorse a particular borrower except insofar as it influences the cost of losing a borrower in forgone compensation.

Estimates

To estimate the causal effect of the cost of losing a borrower on a loan officer's likelihood to endorse her we run the following regression.

$$y_i = \alpha + \beta Cost_i + \gamma X_i + \epsilon_i \tag{2}$$

where y_i is an indicator for whether borrower i was endorsed, and the rest of the variables are defined as in Specification 1. To account for the endogeneity of $Cost_i$ we estimate Specification 2 via two stage least squares. Our first stage is described in the previous section. Because our sample comprises the universe of borrowers to whom our lender could offer graduation loans, we cluster our regressions at the borrower level (Abadie et al., 2017). The results are presented in Table 1. Columns 1 and 2 correspond to the ordinary least squares (OLS) estimates of Specification 2, and columns 3 and 4 correspond to the instrumented (IV) regression. Panel A corresponds to $DirectCost$ and Panel B corresponds to $ShapleyCost$. As expected, loan officers are likelier to endorse borrowers with higher loan sizes and lower default.

Most importantly, the estimates in columns 3 and 4 indicate that holding other borrowers characteristics fixed, loan officers are less likely to endorse a borrower the costlier it is for them to lose that borrower. The coefficient in column 4 Panel A implies that every additional dollar per month in $DirectCost$ that a loan officer would be penalized from losing a borrower corresponds to a reduction in the likelihood of endorsement of 0.03 [SE: 0.01] percentage points. Scaled in standard deviations, a 1 standard deviation increase in the $DirectCost$ of losing a borrower corresponds to a 1.03 percentage point decrease in

[16] Across all columns, the first-stage F statistics reject the null of weak instruments at the conventional levels suggested by Stock and Yogo (2005).

the likelihood of endorsement, relative to a 5.9% probability of endorsement across the full sample. Similarly, the coefficient in column 4 of Panel B implies that every additional dollar per month in $ShapleyCost$ that a loan officer would be penalized from losing a borrower corresponds to a reduction in the likelihood of endorsement of 0.94 [SE: 0.27] percentage points. Scaled in standard deviations, a 1 standard deviation increase in the $ShapleyCost$ of losing a borrower corresponds to a 0.92 percentage point decrease in the likelihood of endorsement.

While the $ShapleyCost$ and $DirectCost$ estimates are quite similar when scaled in standard deviations, the $ShapleyCost$ estimate is much larger when scaled in dollars per month. This is because $DirectCost$ exaggerates the difference in costliness to lose various borrowers. All borrowers assigned to loan officers who are not near a compensation threshold have a $DirectCost$ of zero, while nearly all borrowers assigned to loan officers at the cusp of a threshold have very large $DirectCost$. By construction, $ShapleyCost$ smooths out this variation.

Compared to the OLS estimates, the IV estimates on $DirectCost$ and $ShapleyCost$ are more negative. This is precisely as expected given that both measures of cost are increasing functions of attributes that are positively correlated with suitability for graduation, so that the relationship between cost of losing a borrower and likelihood of endorsing her is attenuated in the OLS estimates.

These results indicate that holding other borrower characteristics fixed, loan officers strategically withheld endorsements from the borrowers that were costliest to lose from their portfolio. To assess the cost of this strategic disclosure on our partner organization's profits and on forgone graduations we implemented an experiment, described in the following section.

4 Experimental Design

In collaboration with our partner lender, we designed two new compensation schemes meant to reduce, and partially reverse the implicit penalty that loan officers face when losing borrowers to graduation. We then conducted an experiment to assess the extent and consequences of strategic disclosure under the baseline compensation scheme. However, due to organizational constraints, we were not able to induce individual variation in loan officer compensation. This is likely a common obstacle to experiments inside firms

– managers are often reluctant to treat employees differently from one another, especially regarding matters of compensation. Therefore our experimental design may prove useful for other researchers conducting experiments inside firms.

Each of our two compensation schemes was rolled out to all loan officers at the same time. Rather than randomizing the assignment of compensation schemes, our experimental variation comes from the timing of surveys relative to the announcement of the compensation change. Namely, some loan officers were surveyed immediately prior to their discovery that their compensation scheme would be adjusted and some were surveyed immediately following the disclosure of this information. Both of the compensation changes described below were a surprise to all loan officers; no one was informed about impending changes prior to their announcement date. Figure 1 presents a timeline of the compensation changes and surveys.

Compensation Scheme Changes. With our guidance, in March of 2019 our partner lender announced the first change in the compensation scheme for joint-liability loan officers. The new compensation scheme, which we refer to as *Mitigation*, mitigated the penalty that loan officers faced from losing borrowers through graduation. Specifically, under the new incentive scheme, loan officers were given a six month grace period for each graduated borrower, during which graduated borrowers continued to be treated as part of the loan officer's portfolio for the purpose of calculating their bonus.[17] This translated to a reduction in the monetary penalty of losing a graduated borrower of about USD 6 on average, which represents about 0.5% of a loan officer's average compensation. Moreover loan officers now had a full additional loan cycle before they were required to replace lost borrowers for groups that fell below the minimum size of 18. Lastly, to maintain group cohesion as the borrower transitioned out of joint-liability, the borrower who received a graduation loan would be allowed to continue to participate in group meetings and group activities for the following year.[18]

In April of 2019 our partner organization announced the second, and final change to loan officer compensation, which we refer to as *Recognition*. In the Recognition scheme, in addition to maintaining the features of Mitigation, loan officers were rewarded (or, recognized) for endorsing borrowers that subsequently went on to receive graduation loans

[17]Recall, the details of this bonus calculation are in Appendix Section D.

[18]Loan officers told us that the people they were more likely to endorse were also individuals who participated in the leadership of the group. They worried that losing those borrowers could create internal conflicts in the group as the group members looked to replace them in the leadership structure. Allowing graduated individual liability borrowers to continue to participate in the group meetings would allow for some time to transfer knowledge to new members of the leadership.

and exhibit good repayment behavior. Rewards were calculated as a function of *points* a loan officer earned—for each borrower that was endorsed and subsequently graduated, loan officers gained three points if the borrower exhibited good repayment behavior and conversely they lost one point for endorsed borrowers who exhibited poor repayment in the graduation loan. Points could be exchanged for various rewards. To give an approximate sense of the value of a point, three points could be exchanged for a day off, or one point could be exchanged for a sleeping bag, or a pair of bluetooth headphones among many other things.

We note that neither of these two compensation schemes is likely to resemble the optimal compensation structure for loan officers. Our goal was not to evaluate the optimal compensation structure, but rather to investigate whether loan officers were strategically withholding information about qualified borrowers under the original compensation scheme. In Section 8 we discuss how our partner lender restructured its organization in response to the results of this study, in a manner that may more closely resemble the theoretically optimal organizational structure.

Surveys and Timeline. As described in Figure 1, we implemented four rounds of surveys to collect endorsements from joint-liability loan officers about which borrowers would be suitable for graduation. Specifically, loan officers were provided a form with all of their borrowers (organized by joint-liability group) and asked (a) to endorse borrowers who are suitable for graduation and (b) a strength of the endorsement on a scale of 1–5. Loan officers were informed that their endorsements would eventually be used in the graduation process.[19]

The first survey round was our baseline ($Baseline$), which occurred in November 2018 and during which all loan officers were surveyed. The second ($PreMitigation$) occurred one week before the announcement of the Mitigation incentive change in March 2019. We randomly selected half of the joint-liability loan officers to be surveyed at the $PreMitigation$ round, during which they were given the opportunity to update the endorsements they provided at baseline. The third survey ($PostMitigation$) occurred the week of the incentive change, so that there was a one week difference between the $PreMitigation$ and $PostMitigation$ survey rounds.[20] All loan officers were surveyed during $PostMitigation$ and given yet another opportunity to update their endorsements.

[19] This was indeed the case, though as part of our research protocol we withheld the endorsements from our partner lender until we had enough data to judge the value of endorsements in predicting borrower repayment behavior.

[20] Loan officers only meet all together at the branch office on Fridays. The rest of the week is spent in the field visiting borrower groups. Surveys were always implemented at the Friday meetings.

Our primary comparison of interest is between the endorsements collected by loan officers in the second survey round, and the endorsements collected from loan officers in the third survey round *who were not also surveyed in the second round.* As we discuss below, we attribute this difference to the treatment effect of changing the incentive scheme as only one week elapsed between the survey rounds and there was therefore little time for loan officers to collect new information. As a secondary estimate of the same treatment effect, we compare the number of endorsements collected from loan officers in the second survey round to the number of endorsements collected from the same loan officers in the third survey round. We make these comparisons precise in the following section.

Finally, in the week following the announcement of Recognition we implemented one final survey round ($PostRecognition$) to collect endorsements from loan officers. All joint-liability loan officers were included in this survey. Because of logistical constraints, we did not randomize the timing of this survey relative to the introduction of the Recognition scheme. Roughly one month elapsed between the $PostMitigation$ and $PostRecognition$ surveys, but we present evidence below that very few of the additional endorsements collected in the $PostRecognition$ survey are due to the elapsed time, and that the great majority of these endorsements are attributable to the compensation shift.

Table A1 presents our balance check for loan officer portfolio characteristics amongst those who were randomly selected to endorse borrowers before Mitigation versus those who were not. The only statistically significant difference is that borrowers of loan officers surveyed after mitigation have slightly fewer non-household workers (significant at the 10% level). An F-test does not reject that the two groups are drawn from the same population.

5 The Impact of the Compensation Changes on Disclosure of Endorsements

In this section we discuss the impact of the two compensation changes on loan officer willingness to endorse borrowers for graduation, and on the characteristics of endorsed borrowers.

The Impact of the Mitigation Scheme

We use two primary regression specifications to evaluate the impact of the Mitigation scheme on the number of endorsements furnished by loan officers. Our preferred specification leverages between-subject variation comparing the number of endorsements we received from loan officers who were surveyed just before the Mitigation scheme was introduced to those who were *only* surveyed just afterwards. This is a comparison of groups A and C in Figure 2. Specifically we regress

$$y_i = \alpha + \beta_1 PostMitigation_i + \gamma X_i + \mu_B + \epsilon_{it} \tag{3}$$

where y_i is the number of endorsements furnished by loan officer i, $PostMitigation_i$ is an indicator for whether loan officer i was only asked for endorsements immediately following the introduction of the Mitigation scheme, μ_B is a branch fixed effect, and X_i is a vector of loan officer controls: total endorsements given by the loan officer at baseline, size of total loan portfolio in November 2018, and number of borrowers in the loan officer's portfolio in November 2018. Standard errors are clustered at the loan officer level. β_1 is the coefficient of interest, representing the difference between the number of endorsements received by loan officers under the old incentive scheme and the number received by loan officers under the Mitigation incentive scheme.

Our second specification leverages within-subject variation and compares endorsements from groups A and B in Figure 2. Specifically, for loan officers who were randomly selected to be surveyed both one week before and immediately after the introduction of the Mitigation scheme, we regress

$$y_{it} = \alpha + \beta_2 PostMitigation_{it} + \gamma X_i + \delta_i + \epsilon_{it} \tag{4}$$

where y_{it} is the cumulative number of endorsements furnished by loan officer i in survey round t, δ_i is a loan officer fixed effect, and $PostMitigation_{it}$ is an indicator for whether loan officer i was exposed to the Mitigation scheme in survey round t. Standard errors are clustered at the loan officer level. Here β_2 represents the additional endorsements furnished by loan officers after they were exposed to the Mitigation scheme.

Finally, we combine these two sources of variation in a pooled regression specification on our full sample.

$$y_{it} = \alpha + \beta_3 PostMitigation_{it} + \gamma X_i + \mu_B + \epsilon_{it} \tag{5}$$

We therefore pool across groups B and C in Figure 2 and compare their outcomes to the outcomes of group A, and standard errors are clustered at the loan officer level.

Across all of the above specifications, we estimate the regression models using data from our $PreMitigation$, $PostMitigation$, and $PostRecognition$ survey waves

Table 2 presents our estimates of the impact of the Mitigation scheme on loan officer endorsements. Columns 1 and 2 correspond to estimates of the between-subjects Specification 3 , column 3 corresponds to the within-subjects Specification 4, and columns 4 and 5 correspond to the pooled Specification 5. When there are two columns for a specification, the second includes loan officer controls. Across all specifications, loan officers affected by the Mitigation scheme furnished between 1.1 [SE: 0.28] and 1.6 [SE: 0.53] additional endorsements. This is not only statistically significant but also economically significant. Compared to the $PreMitigation$ round, the loan officers surveyed in the $PostMitigation$ round furnished more than 300 additional endorsements. This is our first piece of experimental evidence that loan officers were strategically withholding endorsements prior to our compensation shift.

The Impact of the Recognition Scheme

Next we examine the impact of introducing the Recognition scheme. As discussed in Section 2, the Recognition scheme was introduced in April 2019 without random variation. Therefore to evaluate the impact of the Recognition scheme we estimate two regression models

$$y_{it} = \alpha + \beta_1 PostMitigation_{it} + \beta_2 PostRecognition_{it} + \gamma X_i + \delta_i + \epsilon_{it} \tag{6}$$

presented in columns 6 and 7 of Table 2, and

$$y_{it} = \alpha + \beta_1 PostMitigation_{it} + \beta_2 PostRecognition_{it} + \beta_3 PreMitigation_{it} + \gamma X_i + \delta_i + \epsilon_{it} \tag{7}$$

presented in column 8 of Table 2.

In Specification 6 we include data from three of the four survey rounds: the $PreMitigation$ survey wave immediately preceding Mitigation, the $PostMitigation$ survey wave immediately following Mitigation and the $PostRecognition$ survey wave immediately following Recognition. The omitted group is the total number of endorsements given during $PreMitigation$. We exclude data from the $Baseline$ survey wave.

Specification 7 also includes data from the $Baseline$ survey wave, which serves as the omitted group. So we can separately estimate the number of endorsements attributable to the $PreMitigation$ survey wave. In both cases standard errors are clustered at the loan officer level.

Importantly, one month elapsed between the $PostMitigation$ survey and the $PostRecognition$ survey. Therefore, we may not be able to fully attribute all additional endorsements reflected in β_2 to the impact of the Recognition scheme. Perhaps, even abstracting from our compensation changes, loan officers would anyways have accumulated new information in the elapsed month about borrowers who were qualified to graduate out of the joint-liability loan program. However, we note that between our baseline in November and our pre-Mitigation survey in February, more than three months elapsed. So the coefficient β_3 in Specification 7, corresponding to the additional endorsements we collected in our $PreMitigation$ survey, provides a conservative estimate of the number of additional endorsements from the Recognition round that can be attributed to the elapsed time. Hence, to the extent that β_2 is significantly larger than β_3, we can be confident that the Recognition scheme had an impact on loan officer willingness to endorse their borrowers.

The estimates in Table 2 imply that loan officers furnished between 2.1 [SE: 0.34] and 2.4 [SE: 0.38] additional endorsements as a result of the Mitigation and Recognition scheme jointly. In contrast, our estimates of β_3 in column 8 demonstrates that loan officers only furnished an additional 0.12 [SE: 0.24] additional endorsements in our $PreMitigation$ survey relative to $Baseline$, indicating that time trends do not account for the additional endorsements we collected $PostRecognition$. Together these comprise our second piece of experimental evidence that loan officers were strategically withholding endorsements of qualified borrowers prior to our compensation shift.

Once again, we highlight that these results are not only statistically significant but they are also economically significant. Compared to the number of endorsements that we collected at baseline, the additional endorsements attributable to the changes in compensation amount to a roughly 11% increase. This in part reflects the efficacy with which we collected endorsements at baseline. Prior to our study, our partner lender received nearly no endorsements from joint-liability loan officers, so the additional endorsements attributable to changes in compensation would amount to an enormous increase, in percentage terms, relative to the endorsements collected prior to our study.

Finally, the strongest standard by which we can judge the impact of our compensation change is by the number of additional *valuable* endorsements collected in each survey round. As we will show in Section 6, the borrowers who performed the best in the graduation loan program are those who were endorsed after Mitigation and Recognition rather than those endorsed at baseline. This suggests that it was the *most qualified* borrowers whose endorsements loan officers were strategically withholding prior to the compensation shift.

Strategic Determinants of Endorsements

In this section we examine how the characteristics of endorsed borrowers varied over our successive interventions.

The Cost of Losing a Borrower

First, we return to the relationship between the cost of losing a borrower – as of our baseline in November 2018 – and the likelihood a loan officer endorses her. Columns 1 and 2 of Table 3 replicate columns 3 and 4 of Table 1. Columns 3 and 4 (5 and 6) of Table 3 re-estimate Specification 2 but for endorsements collected after Mitigation (Recognition), rather than for endorsements collected at baseline.

As can be seen from comparing the estimates for baseline to those for Mitigation and Recognition, the relationship between the costliness to lose a borrower and the likelihood that a borrower is endorsed diminishes across the survey rounds, as the two new compensation schemes are introduced. This suggests that loan officers have internalized that our compensation shifts disproportionately reduce the cost of endorsing borrowers that were more important to their compensation in the baseline scheme. In fact, columns 5 and 6 of Panel A indicates that the relationship between $DirectCost$ and likelihood of endorsement has *reversed* by the $PostRecognition$ survey, which is consistent with possibility that loan officers were disproportionately withholding endorsements of borrowers who were costliest to lose prior to the compensation shift and therefore that these borrowers were disproportionately endorsed after the shift.

Other Borrower Characteristics

Next we examine a range of other observable characteristics of borrowers who were endorsed in each survey round; these are presented in Table 4. Column 1 presents the average characteristics across all borrowers, column 2 present the average characteristics for borrowers endorsed at baseline, and columns 3 and 4 present the difference in average characteristics between borrowers endorsed at baseline and borrowers endorsed after Mitigation, and after Recognition respectively. Relative to the whole population of borrowers, those endorsed at baseline run larger and more profitable businesses, earning approximately an additional USD 330 a month. They have been with our partner lender for approximately 2.7 additional loan cycles, their amount borrowed is about USD 347 larger , and they spend fewer days in default.

There are few differences in observable characteristics between borrowers endorsed at baseline and those endorsed after our compensation shifts. Relative to borrowers endorsed at baseline, borrowers endorsed after Mitigation are about two years younger. In terms of borrowing characteristics, they have been with the organization for 0.9 fewer loan cycles relative to a baseline of 10.7, and have slightly smaller loans – on average USD 53 less compared to a baseline of USD 1,207.The only statistically significant difference between the observable characteristics of borrowers endorsed after Recognition and those endorsed at baseline is that those endorsed after Recognition are less likely to be in agriculture. Few, if any of these differences are economically meaningful in their magnitudes.

Comparing borrowers endorsed across various survey rounds based on the size of their joint-liability group reveals a new dimension on which loan officers were strategically withholding endorsements prior to the compensation shift. Recall that when joint-liability groups fall below 18 borrowers, joint-liability loan officers face significant pressure to replace lost borrowers. Therefore loan officers may have been particularly wary to endorse borrowers from groups at or near 18 borrowers. Our Mitigation compensation scheme reduced the pressure to immediately replace lost borrowers, and our Recognition scheme increased the reward from identifying reliable borrowers regardless of their group size. Therefore both compensation schemes may have had an especially large effect in inducing loan officers to disclose their endorsements of borrowers from smaller groups. Figure 3 suggests that this was the case.

Figure 3 on the left depicts the distribution of group sizes from borrowers endorsed at baseline, and overlays the distribution of group sizes of borrowers endorsed after Mitigation. Figure 3 on the right does the same for borrowers endorsed at baseline and those endorsed after Recognition. While there is no apparent difference in the distributions for baseline and Mitigation, there is more mass in the left tail of the Recognition distribution than there is in the left tail of the baseline distribution. Specifically, a Kolmogorov-Smirnov test rejects equality of the baseline and Recognition endorsement distributions, and a t-test rejects that the two distributions have the same amount of mass to the left of both 18 and 20 borrowers at the 1% level. Therefore, borrowers from small groups—those less than 20 borrowers—had higher representation in the Recognition endorsements than in the baseline endorsements. This suggests that indeed loan officers were more likely to strategically withhold the endorsements of borrowers from smaller groups.

6 The Predictive Power of Endorsements

Our next line of inquiry regards the value of endorsements furnished across the various survey rounds in predicting the repayment behavior of borrowers. In this section we demonstrate that loan officer endorsements are valuable in predicting repayment behavior both in the joint-liability portfolio and in the graduation portfolio, and that these endorsements remain valuable even after controlling observable characteristics. Hence loan officers have valuable soft information not easily inferable from borrower characteristics.

Importantly we find evidence that borrowers endorsed after Mitigation and after Recognition exhibit better repayment behavior in both loan portfolios than borrowers endorsed at baseline. This is our final, and perhaps most striking finding regarding loan officer strategic disclosure. Not only were loan officers impeding the graduation of qualified borrowers, but they were impeding the graduation of their *most qualified* borrowers.

The fact that borrowers endorsed after Mitigation and Recognition have better performance in *both* portfolios represents an important misalignment between the interests of loan officers and those of our partner lender. These are the borrowers that our partner lender would like to graduate to larger loans, yet they are also the borrowers that loan officers would most like to keep in their portfolio. Our results in this section indicate that this misalignment of interests is important in practice.

Endorsements Predict Graduation Loan Performance

At the outset, we note that all graduated borrowers underwent a separate screening procedure managed by the set of loan officers who specialize in graduation loans. At the time of our study, we did not share the joint-liability loan officer endorsements with our partner lender.[21] Therefore, the graduation procedure was not informed by the endorsements we collected in our survey. So this section can be understood as evaluating the predictive value of endorsements over and above the information contained in our partner lender's screening procedure for graduation loans.

Specifically we estimate the following model separately for each survey wave S,

$$y_{it} = \alpha + \beta_S EndorsedInRoundS_i + \gamma X_i + \mu_B + \phi_t + \epsilon_{it} \tag{8}$$

where y_{it} is a measure of borrower i's repayment behavior in month t, and $EndorsedInRoundS_i$ is an indicator for whether borrower i was endorsed in survey round S ($Baseline$, $PostMitigation$, and $PostRecognition$).[22] We use double post lasso to select control variables X_i from the set of borrower characteristics presented in Table A1. μ_B is a branch fixed effect, and ϕ_t is a month fixed effect. Because our sample comprises the universe of borrowers in the graduation loan portfolio over the relevant time horizon, standard errors are clustered at the borrower level.

Within each regression model the sample comprises repayment data on borrowers who were endorsed in round S and subsequently graduated, and borrowers who were never endorsed in any round but who graduated after survey round S, so that they were eligible to be endorsed in survey round S. So for the baseline endorsement survey, the sample includes any borrower who was eligible to be endorsed in the baseline and received a graduation loan sometime after November 2018. For endorsements collected in the $PostMitigation$ survey it includes any borrower who was either endorsed in the $PostMitigation$ round or never endorsed, and who received a loan sometime after March 2019. And in the $PostRecognition$ survey it includes any borrower who was either en-

[21] Recall, loan officers were told that their endorsements would eventually inform the graduation process. This was indeed the case, though as part of our research protocol we withheld the endorsements from our partner lender until we had enough data to judge the value of endorsements in predicting borrower repayment behavior.

[22] For this part of the analysis, we combine endorsements given in the first survey round (baseline - November 2018) and the second survey round ($PreMitigation$- last week of February 2019) since the loan officer incentives were the same for both those rounds. As we showed in Section 5, the passage of time has no significant effect on the number of endorsements that we received.

dorsed in the $PostRecognition$ round or never endorsed, and who received a loan after April 2019. By holding fixed the comparison group to be those who were never endorsed in any round, this approach allows for evaluation of the predictive power of endorsements collected in different survey rounds S by directly comparing the coefficients β_S.

Table 5 presents the estimates of Specification 8. The four outcome variables we examine are whether a borrower is at least 15 days late (columns 1 and 2), whether she is at least 90 days late (columns 3 and 4), whether she has "defaulted" during her loan cycle (columns 5 and 6), and the total value in default (columns 7 and 8).[23] For graduation loans, default is classified as being late in repayment for 180 consecutive days, at which point borrowers are reported to the credit bureau and their debt is sold to a third party. Columns 1 - 4 represent regressions at the borrower-week level, and columns 5 - 8 represent regressions at the borrower level. Even columns include controls for observable characteristics while odd columns do not.

With few exceptions, borrowers who were endorsed – in any round – exhibit better repayment behavior than those who were not endorsed on every dimension, though none of the estimates for endorsements furnished at baseline are statistically significant. Estimates are highly stable with respect to the inclusion of controls, suggesting that loan officers have valuable information about borrower repayment capacity that is not well encoded by observable characteristics.[24]

Notably, with one exception the point estimates for endorsements furnished after Mitigation and after Recognition are always larger than the point estimates for endorsements furnished at baseline, and with one exception all of the $PostMitigation$ and $PostRecognition$ estimates are statistically significant at least at the 10% level and often at the 1% level. The one exception in both cases is the outcome variable 15 days late. And for the outcome variables reflecting 15 and 90 day lateness, the point estimates for $PostMitigation$ are significantly larger than the point estimates for baseline.

The magnitudes of the point estimates are also economically meaningful. For instance, compared to graduated borrowers who were never endorsed, graduated borrowers endorsed after Mitigation are 3.6 [SE: 1.7] percentage points less likely to be 15 days late,

[23]We selected 15 days late as an outcome variable because this is the threshold after which late payment is reported to the credit bureau. 90 day lateness is a salient metric for our partner lender, as it is the reporting threshold for default in joint-liability lending.

[24]This echoes results from Hussam et al. (2021), which finds that community members have valuable information about their entrepreneur-peers that is not well encoded by observable characteristics.

1.5 [SE: 0.5] percentage points less likely to default, and default on USD 27.8 [SE: 11.6] less, on average. Note that none of the borrowers in our sample who graduated after Mitigation or after Recognition had completed their loans by March 2020, while some borrowers who graduated after baseline had completed their loans by March 2020, so the differences in their ultimate default statistics are likely to be even larger. In fact, the estimates for columns 5 - 8 reflect that none of the borrowers endorsed after Mitigation or after Recognition exhibited any default. This is particularly significant given that at the time of our study our partner lender was struggling with default in their graduation loan portfolio.

The fact that borrowers endorsed after Mitigation and after Recognition have better repayment profiles than those endorsed at baseline, and sometimes statistically significantly so, suggests that loan officers were withholding endorsements of their best borrowers prior to the compensation shifts.

For robustness, we re-estimate Specification 8, but for each survey wave we restrict the sample to begin at the time of the survey and to end exactly 12 months later. This ensures that borrowers endorsed in each survey wave have the same amount of time to default, but does not make use of our full data. Results are presented in Table A3.

The estimates are qualitatively similar to our main estimates. With few exceptions, point estimates are larger for $PostMitigation$ and $PostRecognition$ endorsements than they are for baseline endorsements, and sometimes significantly so.

Finally, in Table A4 we re-estimate Specification 8, but replace $EndorsedInRoundS_i$ with a continuous measure of endorsement strength (recall that our loan officer survey elicited a 1-5 measure of endorsement strength). Results are qualitatively similar and suggest that the intensive margin of endorsement strength adds little predictive power over the extensive measure used in our primary analysis.

Endorsements Predict Joint-Liability Loan Performance

In this section we document the predictive power of loan officer endorsements on borrowers in joint-liability loans. Relative to graduation loans, this has the advantage that we observe the outcome for all borrowers independent of whether they were eventually selected to graduate. However the regressions on joint-liability repayment behavior also have two drawbacks. First, we asked loan officers to endorse borrowers on the basis of their suitability for graduation loans; loan officers were not asked to predict repayment behavior in joint-liability loans per se. However, this should serve to reduce the predictive power of endorsements relative to if loan officers had been asked to endorse borrowers on the basis of their suitability for joint-liability loans. As we will demonstrate, endorsements are nevertheless quite predictive of joint-liability repayment behavior. The second drawback is that within each joint-liability group, borrowers are divided into subgroups of about 4 borrowers that jointly submit their repayments. Thus, with few exceptions, repayment status is constant within each subgroup. Once again this should serve to reduce the predictive power of individual endorsements and we show below that they are nevertheless strong predictors of repayment.

We estimate regression models analogous to those in the section above. Namely, for each survey round S we estimate

$$y_{it} = \alpha + \beta_S EndorsedInRoundS + \gamma X_i + \delta_L + \phi_t + \epsilon_{it} \tag{9}$$

on the sample of borrowers who have joint-liability loans in months after their eligibility to be endorsed in a given survey round. In each regression, we restrict the sample to borrowers who were never endorsed and borrowers who were endorsed in round S. Standard errors are clustered at the borrower level. In contrast to Specification 8, our sample of joint-liability borrowers is large enough that we can include loan officer fixed effects δ_L, rather than branch fixed effects μ_B.

Results are presented in Table 6, for the same outcomes as in Table 5. In accordance with our partner lender's definitions, default, for joint-liability loans, is defined as whether a borrower is 90 days late and reported to the credit bureau. The patterns are qualitatively similar. Across all survey waves, endorsed borrowers exhibit less default than non-endorsed borrowers. For joint-liability loans, borrowers endorsed at baseline have statistically significantly lower default than those never endorsed, across all measures except amount defaulted (columns 7 and 8). Estimates are statistically significant across all outcomes for borrowers endorsed after Mitigation and after Recognition. With very few exceptions, borrowers endorsed after Mitigation and after Recognition have lower measures of default than those endorsed in baseline, and the differences are statistically significant for the outcome variables 90 days late and amount defaulted for borrowers endorsed after Mitigation, and for the outcome variables 15 days late and default for borrowers endorsed after Recognition. This further suggests that loan officers withheld endorsements of their most qualified borrowers prior to the compensation shifts.

While default is a smaller problem in our partner lender's joint-liability loan portfolio than in their graduation portfolio, the estimates on the value of endorsements in predicting default are still economically meaningful. For instance, relative to borrowers who were never endorsed, borrowers endorsed after Recognition are 1 [SE: 0.2] percentage point less likely to be 15 days off of a baseline of 1.3 percentage points, and on average default on USD 9.0 [SE: 4.5] less, off of a baseline of USD 13.5. Further, as was the case with graduation loans, the estimates are quite stable with respect to the inclusion of controls, suggesting that loan officers form their endorsements on the basis of information that is not easily observed or encoded.

As in the analysis of graduation loans, in Table A5 we re-estimate Specification 9, but replace $EndorsedInRoundS_i$ with a continuous measure of endorsement strength. Results are qualitatively similar and suggest that the intensive margin of endorsement strength adds little predictive power over the extensive measure used in our primary analysis.

Note that in contrast to the case of graduation loans, we do not re-estimate Specification 9 while restricting attention to the 12 months following each survey wave, as the joint-liability loan cycles are short enough that all borrowers had completed their loans by March 2020.

The fact that borrowers endorsed after Mitigation and Recognition exhibited better repayment in their joint-liability loans than those endorsed at baseline (and those never endorsed) is an important factor in explaining why these endorsements were withheld at baseline. Borrowers with good joint-liability repayment are exactly the ones loan officers would like to keep within their portfolios. Importantly, we found in the previous section that borrowers endorsed after Mitigation and Recognition also exhibited better repayment in the graduation portfolio, and hence these are also the borrowers that our partner lender would most like to be endorsed. This is an important conflict of interests–inherent in the baseline incentive scheme–between loan officers and our partner lender, and more broadly between loan officers and the goal of graduating qualified borrowers out of their joint-liability microloans.

Endorsements Predict Profitability of Loans

The final exercise of this section is to estimate the predictive value of endorsements on the profits that our partner lender derived from their loans. We begin with the predictive value of endorsements on the profits of graduation loans. The principle difficulty with this exercise is that the Chilean economy virtually shut down in March of 2020 in response to the COVID-19 pandemic. Therefore, for borrowers who graduated after Mitigation, we have only partial repayment data prior to the shutdown. We address this challenge by predicting what our partner lender's profits would have been, had the economy not shut down, for borrowers about whom we have only partial repayment data.

We form these predictions by estimating 15 models, one for each possible number of months of repayment data we could have for loans with incomplete data. In other words, the model we use to predict what profits would have been for the set of loans where we have only three months of repayment data is different from the model we use to predict what profits would have been for the set of loans where we have four months of repayment data and so on.

For a given number of months of repayment data n, we form these predictions by estimating

$$y_i = \alpha + \beta X_i + \epsilon_i \tag{10}$$

on the sample of borrowers who have completed their graduation loans between December 2018 and March 2020. Each observation represents an individual loan, where y_i is the ratio of the loan's realized net present value (NPV) over what the loan's NPV would have been if the borrower exhibited perfect repayment.[25] The vector X_i includes all of the control variables in Table A1, month of disbursal fixed effects, and the ratio of the loan's NPV up to month n over what the loan's NPV would have been if the borrower exhibited perfect repayment up to month n. This final control allows us to use a borrower's repayment data up until month n to predict the final NPV of the loan. We utilize the ratio of a loan's actual NPV to the NPV of perfect repayment to allow for comparability of repayment histories for loans of different sizes.

As the goal is to form an out-of-sample prediction of a loan's NPV, we estimate Specification 10 using LASSO and use the model estimated for n months of repayment data to form a prediction $\hat{y}_i$ of the final NPV for all loans i about which we only have n months of repayment data.

Next, to estimate the value of endorsements in predicting the profitability of graduation

[25] The net present value of a loan i is calculated as $NPV_i \equiv \sum_t \{\delta^t * p_{i,t}\} - P_i$, where $p_{i,t}$ is the payment received for the loan t months after the loan's origination, $\delta \equiv \frac{1}{1.0038}$ is the monthly interest rate our partner lender pays for capital, and P_i is the loan's principal.

loans we estimate the following model separately for each survey wave S,

$$y_i = \alpha + \beta_S EndorsedInRoundS_i + \gamma X_i + \epsilon_i \tag{11}$$

on the sample of borrowers who received a graduation loan after survey round S and who were either endorsed in round S or were never endorsed. For borrowers who have completed their loan before March 2020, the outcome variable y_i is the NPV of their stream of payments, and for those who have not completed their loan by March 2020 the outcome variable is their loan's predicted NPV $\hat{y}_i$. $EndorsedInRoundS_i$ is an indicator for whether borrower i was endorsed in survey round S ($Baseline$, $PostMitigation$, and $PostRecognition$), and we use double post lasso to select control variables X_i from the set of borrower characteristics presented in Table A1. We bootstrap our standard errors, clustered at the borrower level, to account for the prediction error in our outcome variable.

The results are presented in columns 1 and 2 of Table 7. The graduation loans of borrowers endorsed at baseline are only estimated to be USD 15.9 [SE: 38.7] more profitable than the graduation loans who are never endorsed. In contrast, loans given to those endorsed after the compensation changes are more profitable. Loans to graduated borrowers endorsed after Mitigation are estimated to be USD 110.2 [SE:164.9] more profitable than those who were never endorsed, and those of borrowers endorsed after Recognition are estimated to be USD 86.8 [SE: 76.8] more profitable. On average, our partner lender derives 34% higher NPV from graduation loans of borrowers endorsed after Mitigation and Recognition than from graduation loans of borrowers who were endorsed at our baseline. Further, the value of Mitigation and Recognition endorsements for predicting the NPV of graduation loans is virtually unchanged with the inclusion of borrower controls, which once again suggests that loan officers have valuable soft information that is not encoded by observable borrower characteristics.

Finally, we perform the same exercise for our partner lender's joint-liability loan portfolio. However, because joint-liability loans have a shorter duration than graduation loans, every borrower in the joint-liability portfolio who was eligible to be endorsed after Recognition completed at least one loan prior to March 2020. Therefore we do not need to predict loan profitability. The results are presented in columns 3 and 4 of Table 7. Across all rounds, the the joint-liability loans of endorsed borrowers are between USD 2 and USD 22 more profitable than the graduation loans who are never endorsed, with no significant differences across rounds. In contrast to the case of graduation loans, the value of endorsements in predicting the NPV of joint liability loans diminishes with the inclusion of borrower controls.

7 Spillovers onto Joint-Liability Borrowers when Someone Graduates

The results thus far suggest that our partner lender benefited from graduating borrowers endorsed after Mitigation and Recognition. However, one potential cost of graduating qualified borrowers, which so far we have not explored, is that there may be negative externalities on the joint-liability borrowers left behind by the borrowers who graduate. This could be the case if the best borrowers in the group provide advice, repayment discipline, or insurance to their groupmates. In this section we examine the repayment behavior in joint-liability groups before and after they lose a borrower to graduation and find no evidence of negative spillovers from graduation.

Specifically, for each survey round S, and for the population of all joint-liability borrowers who had a group member endorsed in survey round S graduate between survey round S and March 2020, we regress

$$y_{it} = \alpha + \beta_S PostS_{it} + \gamma X_i + \delta_L + \phi_t + \lambda_c + \epsilon_{it} \tag{12}$$

The level of observation is borrower by month. Here $PostS_{it}$ is an indicator variable taking the value of 1 if borrower i's group mate has already graduated by month t and 0 else, y_{it} is a measure of borrower i's repayment in month t, λ_c is a loan cycle fixed effect, and all other variables are as defined above. The sample is restricted to joint-liability borrowers who were present both before and after a borrower in their group graduated, and standard errors are clustered at the borrower level.

The coefficient β_S captures any reduction or improvement in the repayment behavior of joint-liability borrowers when one of their group mates who was endorsed in survey round S graduates. We note that we do not have random variation in whether an endorsed borrower graduates. However, our regression includes both month and loan cycle fixed effects, so β_S is unlikely to capture any secular trend. And at the time of our study, the process by which borrowers were selected to graduate was determined by a loan officer who specializes in graduation loans and had no responsibility or stake in the joint-liability portfolio. So reverse causality is unlikely to drive any observed relationships between borrower graduation and the repayment of her peers.

Results are presented in Table A6 for the full sample of borrowers who graduated (Panel A), borrowers who graduated and were not endorsed in any survey round (Panel B), and borrowers who graduated and were endorsed in baseline, after Mitigation, and after Recognition (Panels C, D, and E). Across the board the point estimates are small, and we can never reject that there are no spillovers on the borrowers who are left behind in joint-liability groups. For the case when the graduated borrower was endorsed after Mitigation or after Recognition, point estimate are precisely 0. In these cases we cannot estimate the corresponding standard errors as there is no default among anyone in the corresponding joint-liability groups.

8 Discussion

Loan officers in microfinance institutions are commonly rewarded for maintaining portfolios with large loan volume and high rates of repayment. This implicitly penalizes loan officers whose borrowers graduate from microfinance. To the extent that loan officers have some discretion in whether to support their borrowers' business growth, through the determination of loan limits, the forgiveness of late payments, and endorsements for more formal loans, this penalty may reduce the impact of microfinance.

In partnership with a large Chilean microfinance institution that offers both joint-liability and larger graduation loans, we demonstrate a causal, negative relationship between the cost that a loan officer incurs when losing a borrower and the likelihood that she endorses that borrower for graduation. In an experiment we reduced the penalty that loan officers suffered from borrower graduation. We find that loan officers with modified compensation schemes furnished several hundred more endorsements for borrower graduation, representing an increase in endorsements of about 11% relative to the number we collected at baseline and a far larger increase relative to the number of endorsements our partner lender collected prior to our study. Further, graduated borrowers endorsed after the compensation changes were 34% more profitable for our partner lender than graduated borrowers endorsed prior to the shift. This indicates that not only were loan officers strategically withholding endorsements from qualified borrowers prior to the compensation shift, but further that they were withholding endorsements from *their most* qualified borrowers. Our experimental design may also prove useful to researchers desiring to conduct experiments within large organizations, where managers may be reluctant to treat employees differently from one another.

Because of the ubiquity of incentive schemes that penalize microfinance loan officers for borrower graduation (McKim and Hughart, 2005), our results may shed new light on the limited impact that microfinance has had on entrepreneurship and business growth. Microfinance institutions and their loan officers face an inherent tension between their own profitability and supporting their borrowers' ultimate graduation out of microfinance (Liu and Roth, 2020). Our results demonstrate that this tension strongly deters loan officers from supporting their borrowers.

What is the social cost of these forgone borrower graduations? In Appendix Section E we provide a back of the envelope calculation. Our changes to the compensation scheme induced loan officers to furnish an additional 497 endorsements, representing about 0.5% of our partner's portfolio. Among these borrowers, our partner lender earned on average an additional USD 285 on loans to graduated borrowers relative to standard joint-liability loans. Estimating the impact on the borrowers is more challenging, but utilizing the lower bound of the return to flexible microcredit from Field et al. (2013) of 6% per month, the fact that graduation loans were on average about USD 1,500 larger than joint-liability loans, and that these loans carried an interest rate of about 28% APR, we conclude that these larger loans led to an increase in lifetime consumer surplus of between USD 8,630 and USD 53,705 depending on a plausible range of borrower discount factors. Therefore the social value of the additional endorsements furnished by loan officers at our partner lender ranges from USD 4.4 million to USD 26.8 million.

What about in the industry more broadly? We argued above that even in microfinance institutions that do not have a graduation program, loan officers have some discretion in determining graduation rates to competing institutions. Assuming that loan officers at other institutions are deterring graduation at the same rate as those of our partner lender implies about 650,000 forgone graduations worldwide. And scaling the social value of forgone graduations at our partner lender by the average loan size worldwide, we estimate the lost social value of forgone graduations to be between USD 4.8 billion and USD 29.2 billion.

Policies that reward loan officers and microfinance institutions when their borrowers graduate to self-sufficiency or more formal sources of credit may enhance rates of graduation and the impact of microfinance. After the completion of our study, our partner lender took this insight to heart. Rather than permanently implementing either of the compensation schemes we study in this paper, our partner undertook a more significant reorganization. Prior to our study, the joint-liability loans and graduation loans were siloed, being managed by different loan officers but also entirely different organizational hierarchies. After our study, our partner lender merged the two loan programs into one managerial hierarchy, so that at each branch, one manager oversaw the full team of loan officers across both the joint-liability and graduation loan portfolios. That branch manager was therefore able to internalize the rewards of graduating qualified borrowers as well as the costs of graduating unqualified borrowers. While this appears to be an elegant solution for organizations that house both a standard microcredit product and a graduation loan, government and other third party intervention (e.g. by donors and investors) may be required to align the incentives of microfinance institutions with the graduation of their borrowers in situations when borrower graduation necessarily implies that they lose a valuable customer.

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A Main Tables

Table 1: Impact of the Cost of Losing a Borrower on Likelihood of Endorsements (Baseline)

	Endorsed at Baseline			
	OLS		IV	
	(1)	(2)	(3)	(4)
Panel A: Direct Cost				
β: Direct Cost (USD)	-0.00008***	-0.00008***	-0.00025**	-0.00026**
	(0.00001)	(0.00001)	(0.00011)	(0.00011)
γ_1: Principal (USD)	0.00007***	0.00005***	0.00007***	0.00005***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
γ_2: Amount at Risk (USD)	-0.00009***	-0.00005***	-0.00010***	-0.00007***
	(0.00001)	(0.00001)	(0.00001)	(0.00002)
γ_3: Borrower Loan Cycle	0.00094***	-0.00021	0.00092***	-0.00026
	(0.00016)	(0.00024)	(0.00016)	(0.00024)
Panel B: Shapley Cost				
β: Shapley Cost (USD)	0.00797***	0.00824***	-0.00867***	-0.00942***
	(0.00143)	(0.00144)	(0.00263)	(0.00266)
γ_1: Principal (USD)	0.00007***	0.00004***	0.00007***	0.00005***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
γ_2: Amount at Risk (USD)	-0.00004***	0.00001	-0.00013***	-0.00011***
	(0.00001)	(0.00002)	(0.00002)	(0.00002)
γ_3: Borrower Loan Cycle	0.00099***	-0.00015	0.00089***	-0.00024
	(0.00016)	(0.00024)	(0.00016)	(0.00024)
Borrower Controls		X		X
Observations	65,127	65,127	65,127	65,127

Notes: **Specification**: This table implements Specification 2. The first stages of the IV Specifications of Panel A and B can be found on Table A2a and Table A2b, respectively. The unit of observation is the borrower. Robust standard errors are in parentheses. Direct Cost (USD) is the amount that the officer would have lost if the borrower had graduated in November 2018. Shapley Cost (USD) is an alternative measure of the value of a borrower to a loan officer's portfolio. See Section 3 for details on the construction of these Cost variables. Columns (1) and (2) report results from an OLS model, whereas columns (3) and (4) report results from a 2SLS model. For each regression, the sample comprises borrowers endorsed at Baseline and those never endorsed. We exclude borrowers from 9 loan officers who have not worked long enough with our partner lender to be eligible for a bonus. The first stage regression includes the following instruments: distance to 169 borrowers from below and its square; distance to 351 borrowers from below and its square; distance to 351 borrowers from above and its square; distance to 3% lateness indicator from below and its square; and distance to 3% lateness indicator from above and its square. Dummy variables are also included to control for loan officers to whom an instrument does not apply. The second stage regression also includes the following variables as controls: "Principal": Loan amount given to the borrower; "Amount at Risk": the complete pending amount if the borrower has 7 or more days late (and zero otherwise); "Borrower Loan Cycle": the number of cycles that the borrower has been with our partner lender; controls for average days late before Baseline, and demographic and business characteristics from Table A1. **Outcome variable**: Columns (1)-(4) report results on an indicator variable that equals 1 for being endorsed at Baseline round, and 0 if never endorsed.

Table 2: Impact of the Compensation Change on Total Cumulative Endorsements

	Total Cumulative Endorsements							
	Between Officers		Within Officers	All Officers		All Officers		All Officers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Post Mitigation	1.111***	1.086***	1.626***	1.334***	1.322***	1.309***	1.296***	1.614***
	(0.278)	(0.279)	(0.526)	(0.268)	(0.268)	(0.272)	(0.273)	(0.295)
β_2: Post Recognition						2.167***	2.145***	2.432***
						(0.335)	(0.337)	(0.376)
β_3: Pre Mitigation								0.119
								(0.236)
P-value for F Test:								
$\beta_1 = \beta_2$						0.000	0.000	0.000
$\beta_1 = \beta_3$								0.000
Mean: Endorsements pre Mitigation	0.187	0.187	0.187	0.187	0.187	0.187	0.187	
	[0.862]	[0.862]	[0.862]	[0.862]	[0.862]	[0.862]	[0.862]	
Mean: Endorsements at Baseline	20.437	20.437	21.896	20.924	20.924	20.914	20.914	20.776
	[27.007]	[27.007]	[31.571]	[28.601]	[28.601]	[28.201]	[28.201]	[27.844]
Branch FE	X	X		X	X	X	X	
Loan Officer FE			X					X
Loan Officer Controls		X			X		X	
Observations	241	241	246	364	364	592	592	821

Notes: **Specification**: Columns (1)-(2) implement Specification 3, Column (3) implements Specification 4, Columns (4)-(5) implement Specification 5, Columns (6)-(7) implement Specification 6, and Column (8) implements Specification 7. Standard errors are in parentheses, clustered at the loan officer level. Standard deviations are in brackets. Columns (1)-(5) only include the February and March survey waves, and Columns (6)-(7) include the pre-mitigation round in February, the post mitigation round in March, and the post recognition round in April. Pre-mitigation is the omitted group in all regressions in Columns (1) - (7). Column (8) includes the November Baseline, February, March, and April survey waves. The omitted group is the baseline endorsements wave. In Columns (1)-(2), there are 123 officers who submitted endorsements pre-mitigation, and 118 officers who submitted endorsements *only* after mitigation for between officer regression. In Column (3), there are a total of 246 observations from 123 officers observed twice, who submitted their endorsements both before *and* after mitigation for within officer regression. Columns (4)-(5) include 246 observations from 123 officers observed twice in the pre and post mitigation rounds, and 118 officers who are only observed once post mitigation as a part of pooled regression. Columns (6)-(7) include 246 observations from 123 officers observed twice on the pre and post mitigation rounds, 118 officers who only submitted responses post mitigation and 228 officers from the post recognition round as a part of pooled regression. Column (8) includes 229 officers from November, 123 officers from pre and 241 officers from post mitigation and 228 advisors from Recognition. Loan officer controls include the total number of endorsements made in November Baseline, size of total loan portfolio in November 2018, and number of borrowers in the loan officer's portfolio in November 2018. Columns (1),(2),(4),(5),(6),(7) have branch fixed effect while columns (3) and (8) have loan officer fixed effects. Columns (2), (5) and (7) have officer controls. **Outcome variable**: Columns (1)-(8) report results on the total cumulative number of endorsements made by a loan officer by each survey round.

Table 3: Impact of the Cost of Losing a Borrower on Likelihood of Endorsements

	Endorsed at Baseline		Endorsed at Mitigation		Endorsed at Recognition	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Direct Cost						
β: Direct Cost (USD)	-0.00025**	-0.00026**	0.00000	0.00000	0.00009***	0.00010***
	(0.00011)	(0.00011)	(0.00003)	(0.00003)	(0.00002)	(0.00002)
γ_1: Principal (USD)	0.00007***	0.00005***	0.00001***	0.00000**	0.00000***	0.00000***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
γ_2: Amount at Risk (USD)	-0.00010***	-0.00007***	-0.00001	-0.00001	0.00000	0.00001**
	(0.00001)	(0.00002)	(0.00000)	(0.00001)	(0.00000)	(0.00000)
γ_3: Borrower Loan Cycle	0.00092***	-0.00026	-0.00003	-0.00015**	0.00003	-0.00003
	(0.00016)	(0.00024)	(0.00004)	(0.00007)	(0.00004)	(0.00006)
Panel B: Shapley Cost						
β: Shapley Cost (USD)	-0.00867***	-0.00942***	-0.00168**	-0.00189**	-0.00105*	-0.00103*
	(0.00263)	(0.00266)	(0.00083)	(0.00084)	(0.00060)	(0.00062)
γ_1: Principal (USD)	0.00007***	0.00005***	0.00001***	0.00000***	0.00000***	0.00000***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
γ_2: Amount at Risk (USD)	-0.00013***	-0.00011***	-0.00002***	-0.00002***	-0.00001***	-0.00001**
	(0.00002)	(0.00002)	(0.00001)	(0.00001)	(0.00000)	(0.00000)
γ_3: Borrower Loan Cycle	0.00089***	-0.00024	-0.00004	-0.00016**	0.00002	-0.00006
	(0.00016)	(0.00024)	(0.00004)	(0.00007)	(0.00004)	(0.00006)
Borrower Controls		X		X		X
Observations	65,127	65,127	61,389	61,389	61,267	61,267

Notes: **Specification**: This table implements Specification 2. The first stages of the IV Specifications of Panel A and B can be found on Table A2a and Table A2b, respectively. The unit of observation is the borrower. Robust standard errors are in parentheses. Direct Cost (USD) is the amount that the officer would have lost if the borrower had graduated in November 2018. Shapley Cost (USD) is an alternative measure of the value of a borrower to a loan officer's portfolio. See Section 3 for details on the construction of these Cost variables. Columns (1) and (2) report results from an OLS model, whereas columns (3) and (4) report results from a 2SLS model. For each regression, the sample comprises borrowers endorsed at Baseline and those never endorsed. We exclude borrowers from 9 loan officers who have not worked long enough with our partner lender to be eligible for a bonus. The first stage regression includes the following instruments: distance to 169 borrowers from below and its square; distance to 351 borrowers from below and its square; distance to 351 borrowers from above and its square; distance to 3% lateness indicator from below and its square; and distance to 3% lateness indicator from above and its square. Dummy variables are also included to control for loan officers to whom an instrument does not apply. The second stage regression also includes the following variables as controls: "Principal": Loan amount given to the borrower; "Amount at Risk": the complete pending amount if the borrower has 7 or more days late (and zero otherwise); "Borrower Loan Cycle": the number of cycles that the borrower has been with our partner lender; controls for average days late before Baseline, and demographic and business characteristics from Table A1. **Outcome variable**: Columns (1)-(6) report results on an indicator variable that equals 1 for being endorsed at a given round, and 0 if never endorsed. Borrowers who are endorsed in other rounds are excluded from the regressions. So borrowers endorsed at Mitigation or Recognition are excluded from columns 1 and 2. Borrowers endorsed at Baseline or Recognition are excluded from columns 3 and 4. Borrowers endorsed at Baseline or Mitigation are excluded from columns 5 and 6.

Table 4: Borrower Characteristics

	All Borrowers	Endorsed at Baseline	Endorsed at Mitigation	Endorsed at Recognition
	Mean	Mean	Difference from Baseline	Difference from Baseline
	(1)	(2)	(3)	(4)
Age	45.683	47.611	-1.719***	0.194
	[13.170]	[11.562]	(0.620)	(0.911)
Married	0.391	0.463	-0.039	-0.030
	[0.488]	[0.499]	(0.028)	(0.039)
HH Size	3.664	3.691	0.042	0.071
	[1.587]	[1.565]	(0.094)	(0.139)
Education: Secondary and Above	0.630	0.673	-0.042	0.004
	[0.483]	[0.469]	(0.027)	(0.037)
No. of Non-HH Workers	0.120	0.273	-0.097**	-0.010
	[0.970]	[1.519]	(0.043)	(0.075)
Sector: Manufacturing	0.289	0.269	-0.027	0.031
	[0.453]	[0.444]	(0.024)	(0.037)
Sector: Retail	0.582	0.587	0.025	-0.019
	[0.493]	[0.492]	(0.028)	(0.040)
Sector: Services	0.125	0.140	0.002	-0.009
	[0.331]	[0.348]	(0.020)	(0.027)
Sector: Agriculture	0.004	0.003	0.000	-0.003***
	[0.065]	[0.054]	(0.003)	(0.001)
Monthly Business Revenues (USD)	1035.145	1524.038	-128.037**	-138.217
	[760.302]	[920.785]	(63.608)	(95.324)
Monthly Business Profits (USD)	687.351	1017.564	-60.521	-70.657
	[518.640]	[615.338]	(39.361)	(59.695)
Borrower Cycle	8.033	10.765	-0.936**	-0.307
	[7.189]	[7.728]	(0.433)	(0.671)
Amount Borrowed	859.673	1207.121	-53.476*	-41.507
	[524.793]	[531.659]	(30.291)	(41.581)
Days Late	0.423	0.030	0.033	0.004
	[4.345]	[0.480]	(0.040)	(0.035)
Amount Late	2.798	0.600	0.920	-0.052
	[26.658]	[8.036]	(0.652)	(0.559)
Observations	81,220	4,833	5,166	4,997

Notes: Column (1) reports average borrower characteristics as of the 1st of November 2018, for *all* borrowers who had an active loan at our partner lender and were evaluated during that month. Column (2) reports average characteristics of borrowers who were endorsed at Baseline or endorsed at Pre-Mitigation in February 2019, just before the Mitigation scheme was announced, since at Baseline and at Pre-Mitigation the compensation scheme is the same. Columns (1) and (2) report standard deviations in brackets. Column (3) reports the mean difference in characteristics of borrowers who were endorsed at Mitigation, from borrowers who were endorsed at Baseline (column (2)). Column (4) reports the mean difference in characteristics of borrowers who were endorsed at Recognition, from borrowers who were endorsed at Baseline ((2)). Columns (3) and (4) report robust standard errors in parentheses.

Table 5: Do Endorsements Predict Default on Graduation Loans?

	Late $\geq$ 15 Days		Late $\geq$ 90 Days		Defaulted		Amount Defaulted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Endorsements								
$\beta_{Baseline}$: Endorsed at Baseline	0.012	0.015	-0.003	-0.002	-0.005	-0.004	-14.929	-11.573
	(0.016)	(0.016)	(0.008)	(0.008)	(0.018)	(0.018)	(39.184)	(39.011)
Mean: Not Endorsed	0.078	0.078	0.031	0.031	0.049	0.049	102.942	102.942
	[0.268]	[0.268]	[0.173]	[0.173]	[0.216]	[0.216]	[486.837]	[486.837]
Lasso Controls		X		X		X		X
Observations	27,978	27,978	27,978	27,978	735	735	735	735
Panel B: Mitigation Endorsements								
$\beta_{Mitigation}$: Endorsed at Mitigation	-0.036**	-0.036**	-0.016***	-0.016***	-0.015***	-0.015***	-27.807**	-27.807**
	(0.017)	(0.017)	(0.004)	(0.004)	(0.005)	(0.005)	(11.587)	(11.575)
Mean: Not Endorsed	0.057	0.057	0.018	0.018	0.015	0.015	27.807	27.807
	[0.231]	[0.231]	[0.132]	[0.132]	[0.120]	[0.120]	[254.131]	[254.131]
Lasso Controls		X		X		X		X
Observations	14,692	14,692	14,692	14,692	496	496	496	496
Panel C: Recognition Endorsements								
$\beta_{Recognition}$: Endorsed at Recognition	0.032	0.032	-0.011***	-0.011***	-0.009**	-0.009**	-20.416*	-20.416*
	(0.047)	(0.047)	(0.003)	(0.003)	(0.005)	(0.005)	(11.425)	(11.412)
Mean: Not Endorsed	0.053	0.053	0.016	0.016	0.009	0.009	20.416	20.416
	[0.225]	[0.225]	[0.124]	[0.124]	[0.096]	[0.096]	[237.202]	[237.202]
Lasso Controls		X		X		X		X
Observations	11,608	11,608	11,608	11,608	442	442	442	442
P-Value for F test:								
$\beta_{Baseline} = \beta_{Mitigation}$	0.026	0.019	0.110	0.089	0.585	0.520	0.734	0.667
$\beta_{Baseline} = \beta_{Recognition}$	0.744	0.778	0.211	0.178	0.815	0.743	0.886	0.816
$\beta_{Mitigation} = \beta_{Recognition}$	0.196	0.196	0.373	0.373	0.143	0.143	0.183	0.183

Notes: **Specification**: This table implements Specification 8. Standard errors are in parentheses, clustered at the borrower level. Standard deviations are in brackets. Columns (1)-(4) are borrower-week level regressions and include a week fixed effects. Columns (5)-(8) are borrower level regressions. For all panels, endorsed borrowers in other rounds are excluded from each round regression, so the sample for each round comprises borrowers endorsed at that given round and those never endorsed. So borrowers endorsed at Mitigation or Recognition are excluded from Panel A. Borrowers endorsed at Baseline or Recognition are excluded from Panel B. Borrowers endorsed at Baseline or Mitigation are excluded from Panel C. The omitted group in all panels is borrowers who were never endorsed at any round. The sample in every panel is limited to graduation loans that are made after each round of surveys. Odd columns don't include any control variables. Even columns include double-post lasso controls for average days late before Baseline, and demographic and business characteristics from Table A1. Tests of equality of Baseline and Post Mitigation, Baseline and Post Recognition, and Post Mitigation and Post Recognition coefficients are based on the SURS framework. **Outcome variable**: Columns (1)-(2) report results on an indicator variable for being late 15 or more days on a Graduation loan in the months after each endorsement wave, up to March 2020. Columns (3)-(4) report results on an indicator variable for being late 90 or more days on a Graduation loan in the months after each endorsement wave, up to March 2020. Columns (5)-(6) report results on an indicator variable for ever defaulted on a Graduation loan in the months after each endorsement wave, up to March 2020. Columns (7)-(8) report results on total amount defaulted for each borrower in the months after each endorsement wave, up to March 2020.

Table 6: Do Endorsements Predict Default on Joint Liability Loans?

	Late $\geq$ 15 Days		Late $\geq$ 90 Days		Defaulted		Amount Defaulted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Endorsements								
$\beta_{Baseline}$: Endorsed at Baseline	-0.005***	-0.001	-0.001***	-0.000**	-0.019***	-0.006***	-2.502	-2.285
	(0.001)	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)	(1.893)	(1.951)
Mean: Not Endorsed	0.014	0.014	0.002	0.002	0.040	0.040	17.128	17.128
	[0.117]	[0.117]	[0.044]	[0.044]	[0.195]	[0.195]	[120.488]	[120.488]
Lasso Controls		X		X		X		X
Observations	3,625,092	3,625,092	3,625,092	3,625,092	75,550	75,550	75,550	75,550
Panel B: Mitigation Endorsements								
$\beta_{Mitigation}$: Endorsed at Mitigation	-0.005**	-0.004	-0.001***	-0.001**	-0.017**	-0.012*	-9.643***	-10.041***
	(0.003)	(0.003)	(0.000)	(0.000)	(0.007)	(0.007)	(2.695)	(2.738)
Mean: Not Endorsed	0.014	0.014	0.002	0.002	0.032	0.032	14.347	14.347
	[0.116]	[0.116]	[0.046]	[0.046]	[0.177]	[0.177]	[110.362]	[110.362]
Lasso Controls		X		X		X		X
Observations	2,497,640	2,497,640	2,497,640	2,497,640	65,420	65,420	65,420	65,420
Panel C: Recognition Endorsements								
$\beta_{Recognition}$: Endorsed at Recognition	-0.010***	-0.008***	-0.001**	-0.001	-0.023***	-0.020***	-8.995**	-10.119**
	(0.002)	(0.003)	(0.001)	(0.001)	(0.006)	(0.006)	(4.545)	(4.603)
Mean: Not Endorsed	0.013	0.013	0.002	0.002	0.029	0.029	13.533	13.533
	[0.115]	[0.115]	[0.046]	[0.046]	[0.168]	[0.168]	[108.206]	[108.206]
Lasso Controls		X		X		X		X
Observations	2,260,263	2,260,263	2,260,263	2,260,263	61,971	61,971	61,971	61,971
P-Value for F test:								
$\beta_{Baseline} = \beta_{Mitigation}$	0.863	0.274	0.324	0.070	0.769	0.448	0.028	0.018
$\beta_{Baseline} = \beta_{Recognition}$	0.033	0.004	0.542	0.239	0.568	0.045	0.184	0.112
$\beta_{Mitigation} = \beta_{Recognition}$	0.167	0.190	0.998	0.951	0.526	0.384	0.902	0.988

Notes: **Specification**: This table implements Specification 9. Standard errors are in parentheses, clustered at the borrower level. Standard deviations are in brackets. Columns (1)-(4) are borrower-week level regressions and include a week fixed effects. Columns (5)-(8) are borrower level regressions. For all panels, endorsed borrowers in other rounds are excluded from each round regression, so the sample for each round comprises borrowers endorsed at that given round and those never endorsed. So borrowers endorsed at Mitigation or Recognition are excluded from Panel A. Borrowers endorsed at Baseline or Recognition are excluded from Panel B. Borrowers endorsed at Baseline or Mitigation are excluded from Panel C. The omitted group in all panels is borrowers who were never endorsed at any round. The sample at every panel is limited to repayment behavior from each survey round onward. Odd columns don't include any control variables. Even columns include double-post lasso controls for average days late before Baseline, and demographic and business characteristics from Table A1. Tests of equality of Baseline and Post Mitigation, Baseline and Post Recognition, and Post Mitigation and Post Recognition coefficients are based on the SURS framework. **Outcome variable**: Columns (1)-(2) report results on an indicator variable for being late 15 or more days on joint liability loans in the months after each endorsement wave, up to March 2020. Columns (3)-(4) report results on an indicator variable for being late 90 or more days on joint liability loans in the months after each endorsement wave, up to March 2020. Columns (5)-(6) report results on an indicator variable for ever defaulted on joint liability loans in the months after each endorsement wave, up to March 2020. Columns (7)-(8) report results on total amount defaulted for each borrower in the months after each endorsement wave, up to March 2020.

Table 7: Net Present Value - Growth Loans and Joint Liability Loans

	GL Net Present Value		JL Net Present Value	
	(1)	(2)	(3)	(4)
Panel A: Baseline Endorsements				
$\beta_{Baseline}$: Endorsed at Baseline	15.918	11.515	18.354***	2.715***
	(38.696)	(36.537)	(0.990)	(0.960)
Mean: Not Endorsed	272.079	272.079	78.038	78.038
	[424.984]	[424.984]	[79.332]	[79.332]
Lasso Controls		X		X
Observations	913	913	152,389	152,389
Panel B: Mitigation Endorsements				
$\beta_{Mitigation}$: Endorsed at Mitigation	110.147	139.365	16.639***	2.354
	(164.915)	(101.265)	(2.380)	(2.256)
Mean: Not Endorsed	272.079	272.079	78.038	78.038
	[424.984]	[424.984]	[79.332]	[79.332]
Lasso Controls		X		X
Observations	693	693	144,171	144,171
Panel C: Recognition Endorsements				
$\beta_{Recognition}$: Endorsed at Recognition	86.775	82.371	21.389***	8.213*
	(76.817)	(72.938)	(4.919)	(4.815)
Mean: Not Endorsed	272.079	272.079	78.038	78.038
	[424.984]	[424.984]	[79.332]	[79.332]
Lasso Controls		X		X
Observations	686	686	143,886	143,886

Notes: **Specification**: This table implements Specification 11. Standard errors are clustered at the borrower level and shown in parentheses. In columns (1) and (2), standard errors are bootstrapped. Standard deviations are in brackets. These are loan level regressions. For the Graduation loan regressions in columns (1) and (2) we utilize data from completed loans and also from loans whose repayment was not yet completed by March 2020. In the latter case, we predict what the net present value (NPV) of these loans would have been, in the absence of the pandemic induced lockdown, by estimating Specification 10. Because the joint liability (JL) loans have a shorter repayment horizon, we do not need to utilize incomplete loans in columns (3) and (4). Data in both specifications spans from December 2018 to March 2020. In every panel the sample for each round comprises borrowers endorsed at that given round and those never endorsed. In every panel the sample is limited to loans that started after each round of surveys. Odd columns do not include any control variables. Even columns include double-post lasso controls for average days late before Baseline, and demographic and business characteristics. The omitted group in all panels is borrowers who were never endorsed at any round. **Outcome variable**: Columns (1)-(2) report results on the NPV of a Graduation Loan, whereas columns (3)-(4) report results on the NPV of a Joint Liability loan.

B Figures

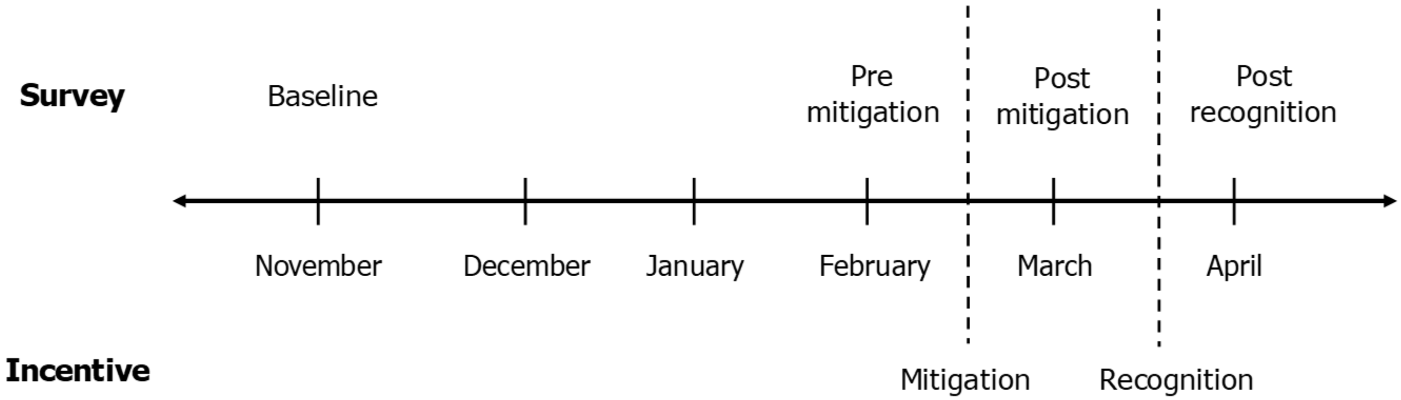

Figure 1: Intervention and Survey Timeline

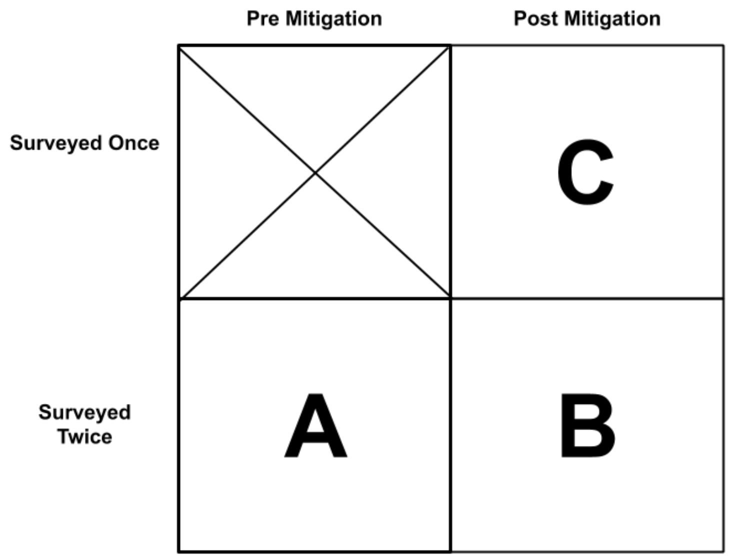

Figure 2: Randomization Design

Notes: Loan officers were randomized into two groups before the Mitigation incentive change. In the Pre Mitigation survey wave, only one group - Group A - was asked to submit endorsements. All loan officers were asked to submit endorsements in the Post Mitigation survey wave - Group B is the group of loan officers who were also surveyed in the Pre Mitigation wave, and Group C is the group of loan officers who were only surveyed in the Post Mitigation wave. Our between-person identification strategy compares loan officers surveyed just before the Mitigation incentive change (Group A), to those only surveyed immediately after the Mitigation incentive change (Group C). Our within-person identification strategy compares the responses of those surveyed just before the Mitigation incentive change (Group A) to the responses of the same loan officers surveyed once again just after the incentive change (Group B).

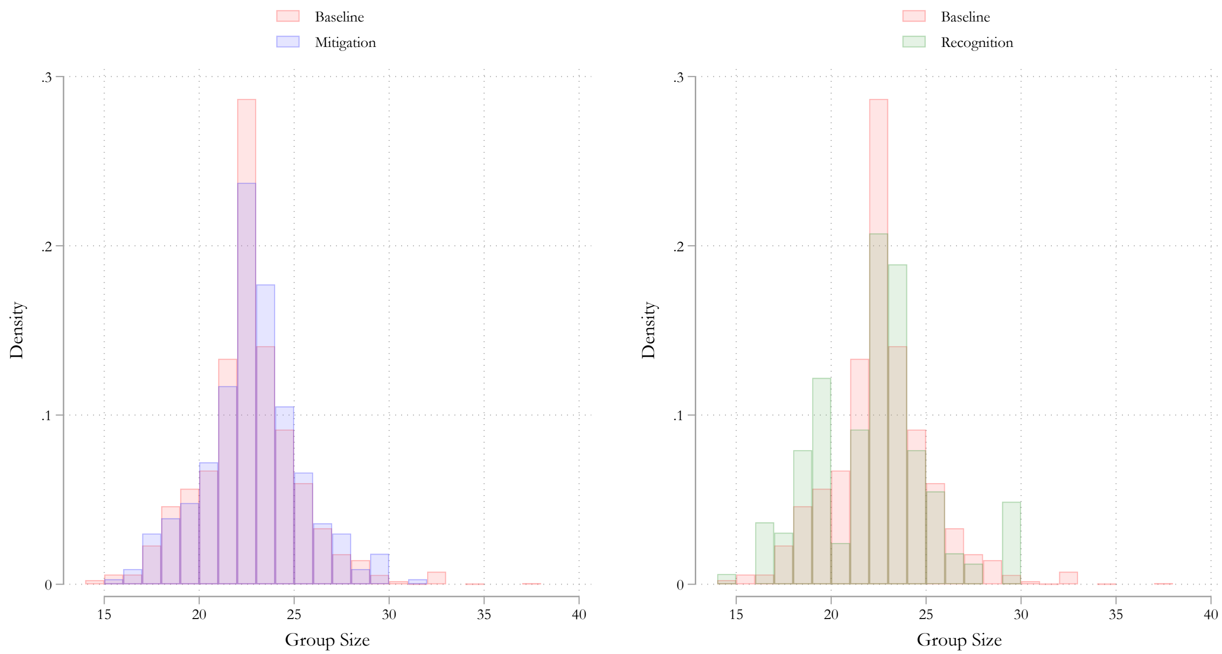

Figure 3: Histogram of Endorsed at each round by size

Notes: These figures present the distribution of borrowers endorsed at each round by group size. The Baseline endorsements round was conducted in November 2018, Mitigation in March 2019, and Recognition in April 2019. Refer to Figure 1 for the intervention and study timeline.

C Appendix Tables

Table A1: Randomization Check

	All Borrowers	Control (Pre Mitigation) Sample	Treatment Sample
	Mean	Mean	Difference from Control Sample
	(1)	(2)	(3)
Panel A: Borrower Characteristics			
Age	45.683	45.743	-0.111
	[13.170]	[13.149]	(0.262)
Married	0.391	0.388	0.005
	[0.488]	[0.487]	(0.010)
HH Size	3.664	3.643	0.040
	[1.587]	[1.591]	(0.039)
Education: Secondary and Above	0.630	0.627	0.008
	[0.483]	[0.484]	(0.009)
No. of Non-HH Workers	0.120	0.129	-0.017*
	[0.970]	[1.073]	(0.009)
Sector: Manufacturing	0.289	0.291	-0.004
	[0.453]	[0.454]	(0.005)
Sector: Retail	0.582	0.581	0.002
	[0.493]	[0.493]	(0.007)
Sector: Services	0.125	0.123	0.003
	[0.331]	[0.329]	(0.005)
Sector: Agriculture	0.004	0.004	-0.001
	[0.065]	[0.066]	(0.001)
Monthly Business Revenues (USD)	1035.145	1039.063	-5.914
	[760.302]	[757.278]	(20.722)
Monthly Business Profits (USD)	687.351	687.942	1.037
	[518.640]	[518.076]	(15.437)
Group Size	21.654	21.626	0.039
	[2.753]	[2.657]	(0.135)
Borrower Cycle	8.033	8.103	-0.126
	[7.189]	[7.176]	(0.295)
Amount Borrowed	859.673	865.402	-11.398
	[524.793]	[522.898]	(18.522)
Days Late	0.423	0.431	-0.112
	[4.345]	[4.442]	(0.106)
Amount Late	2.798	2.798	-0.658
	[26.658]	[26.996]	(0.572)
P-Value for Joint Difference F test:			0.683
Observations	81,220	39,381	77,508
Panel B: Loan Officer Characteristics			
Number of Borrowers	337.436	344.957	-6.772
	[81.951]	[74.377]	(10.109)
Portfolio (USD)	272914.844	283918.469	-14843.721
	[92280.797]	[88034.508]	(11880.571)
Total Amount Late (USD)	892.191	921.410	-243.490
	[1848.535]	[1628.729]	(182.922)
Fraction of Borrowers in Portfolio Endorsed at Baseline	0.058	0.063	-0.008
	[0.070]	[0.082]	(0.009)
P-Value for Joint Difference F test:			0.315
Observations	243	115	229

Notes: Column (1) reports average borrower and loan officer characteristics as of the 1st of November 2018, for *all* borrowers who had a loan with our partner lender and were evaluated by their officers during that month. Column (2) limits the sample and reports average borrower and loan officer characteristics only for loan officers who were selected to be surveyed in the Pre Mitigation survey round in February 2019, just before the Mitigation scheme was announced. We label these officers as our Control Sample . Columns (1) and (2) report standard deviations in brackets. Column (3) reports the mean difference in borrower and loan officer characteristics of loan officers who were not assigned to submit endorsements in the Pre Mitigation Survey (We label these officers as our Treatment Sample), from those who were assigned to Control Sample. Column (3) reports standard errors in parentheses, clustered at the loan officer level.

Table A2a: First Stage: Impact of the Cost of Losing a Borrower on Likelihood of Endorsements (Direct Cost)

	Direct Cost (USD)					
	Baseline		Mitigation		Recognition	
	(1)	(2)	(3)	(4)	(5)	(6)
γ_1: Principal (USD)	0.00029	-0.00028	0.00038	-0.00046	0.00038	-0.00047
	(0.00035)	(0.00068)	(0.00037)	(0.00071)	(0.00037)	(0.00071)
γ_2: Amount at Risk (USD)	-0.08511***	-0.10992***	-0.08684***	-0.11222***	-0.08690***	-0.11233***
	(0.01473)	(0.01848)	(0.01488)	(0.01869)	(0.01490)	(0.01871)
γ_3: Borrower Loan Cycle	-0.07096***	-0.23469***	-0.07194***	-0.24486***	-0.07222***	-0.24534***
	(0.02159)	(0.03600)	(0.02305)	(0.03872)	(0.02308)	(0.03881)
β_1: Below 169	-0.29183***	-0.32063***	-0.29425***	-0.32997***	-0.29403***	-0.32918***
	(0.03440)	(0.03653)	(0.03567)	(0.03779)	(0.03563)	(0.03781)
β_2: Below 169 Squared	0.00162***	0.00181***	0.00164***	0.00187***	0.00163***	0.00186***
	(0.00021)	(0.00022)	(0.00022)	(0.00023)	(0.00022)	(0.00023)
β_3: Below 169 Dummy	-9.83811***	-9.74171***	-9.84085***	-9.93222***	-9.89216***	-9.95400***
	(1.18799)	(1.22800)	(1.21737)	(1.26156)	(1.21996)	(1.26413)
β_4: Below 351	-0.72007***	-0.71643***	-0.73824***	-0.73429***	-0.73978***	-0.73581***
	(0.03836)	(0.03812)	(0.03975)	(0.03949)	(0.03983)	(0.03956)
β_5: Below 351 Squared	0.00453***	0.00449***	0.00463***	0.00459***	0.00464***	0.00460***
	(0.00024)	(0.00024)	(0.00025)	(0.00025)	(0.00025)	(0.00025)
β_6: Below 351 Dummy	-21.39129***	-21.03564***	-22.02407***	-21.64794***	-22.06242***	-21.68978***
	(1.16523)	(1.15776)	(1.21164)	(1.20305)	(1.21370)	(1.20543)
β_7: Above 351	-0.08871***	-0.08690***	-0.08985***	-0.08769***	-0.09033***	-0.08781***
	(0.00916)	(0.01008)	(0.00944)	(0.01040)	(0.00948)	(0.01043)
β_8: Above 351 Squared	0.00108***	0.00108***	0.00110***	0.00109***	0.00111***	0.00109***
	(0.00010)	(0.00011)	(0.00011)	(0.00012)	(0.00011)	(0.00012)
β_9: Below 3%	22.79593***	22.94017***	24.21438***	24.42300***	24.23544***	24.42191***
	(1.29168)	(1.27314)	(1.37841)	(1.36080)	(1.37975)	(1.36111)
β_{10}: Below 3% Squared	-6.00953***	-5.96168***	-6.38250***	-6.34508***	-6.38710***	-6.34384***
	(0.32439)	(0.31933)	(0.34687)	(0.34179)	(0.34715)	(0.34180)
β_{11}: Below 3% Dummy	25.04114***	25.40630***	27.62533***	28.09853***	27.57740***	28.01115***
	(3.02771)	(2.76271)	(3.18032)	(2.90994)	(3.18456)	(2.91181)
β_{12}: Above 3%	7.02559***	7.07929***	7.09421***	7.16519***	7.14245***	7.21087***
	(0.81863)	(0.74112)	(0.83782)	(0.75808)	(0.84295)	(0.76216)
β_{13}: Above 3% Squared	-0.55093***	-0.59257***	-0.55663***	-0.59984***	-0.56030***	-0.60352***
	(0.07581)	(0.07133)	(0.07779)	(0.07296)	(0.07819)	(0.07329)
Borrower Controls		X		X		X
R-Squared	0.049	0.055	0.051	0.057	0.051	0.057
Fst	30.45	30.84	29.90	30.32	29.91	30.32
Observations	65,127	65,127	61,389	61,389	61,267	61,267

Notes: **Specification**: This table implements Specification 1. This is the first stage of Panel A columns (3)-(4) on Table 1, and Panel A on Table 3. The unit of observation is the borrower. Robust standard errors are in parentheses. We exclude borrowers from 9 loan officers who have not worked long enough with our partner lender to be eligible for a bonus. All regressions include the following instruments: distance to 169 borrowers from below and its square; distance to 351 borrowers from below and its square; distance to 351 borrowers from above and its square; distance to 3% lateness indicator from below and its square; and distance to 3% lateness indicator from above and its square. Dummy variables are also included to control for loan officers to whom an instrument does not apply. Finally, the following variables are also included as controls: "Principal": Loan amount given to the borrower; "Amount at Risk": the complete pending amount if the borrower has 7 or more days late (and zero otherwise); "Borrower Loan Cycle": the number of cycles that the borrower has been with our partner lender; controls for average days late before Baseline, and demographic and business characteristics from Table A1. **Outcome variable**: Columns (1)-(6) report results on Direct Cost (USD), which is the amount that the officer would have lost if the borrower had graduated in November 2018. Borrowers who are endorsed in other rounds are excluded from the regressions. So borrowers endorsed at Mitigation or Recognition are excluded from columns 1 and 2. Borrowers endorsed at Baseline or Recognition are excluded from columns 3 and 4. Borrowers endorsed at Baseline or Mitigation are excluded from columns 5 and 6.

Table A2b: First Stage: Impact of the Cost of Losing a Borrower on Likelihood of Endorsements (Shapley Cost)

	Shapley Cost (USD)					
	Baseline		Mitigation		Recognition	
	(1)	(2)	(3)	(4)	(5)	(6)
γ_1: Principal (USD)	0.00035***	0.00038***	0.00033***	0.00037***	0.00033***	0.00037***
	(0.00001)	(0.00001)	(0.00001)	(0.00002)	(0.00001)	(0.00002)
γ_2: Amount at Risk (USD)	-0.00576***	-0.00665***	-0.00553***	-0.00637***	-0.00554***	-0.00638***
	(0.00055)	(0.00066)	(0.00053)	(0.00065)	(0.00053)	(0.00065)
γ_3: Borrower Loan Cycle	-0.00562***	-0.00373***	-0.00502***	-0.00308***	-0.00506***	-0.00309***
	(0.00047)	(0.00070)	(0.00048)	(0.00071)	(0.00048)	(0.00072)
β_1: Below 169	-0.02126***	-0.02063***	-0.02123***	-0.02053***	-0.02123***	-0.02062***
	(0.00216)	(0.00227)	(0.00215)	(0.00226)	(0.00214)	(0.00225)
β_2: Below 169 Squared	0.00010***	0.00009***	0.00010***	0.00009***	0.00010***	0.00009***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
β_3: Below 169 Dummy	0.42295***	0.40872***	0.43508***	0.42603***	0.43469***	0.42395***
	(0.08617)	(0.09014)	(0.08564)	(0.08945)	(0.08531)	(0.08919)
β_4: Below 351	-0.00735***	-0.00733***	-0.00755***	-0.00750***	-0.00746***	-0.00740***
	(0.00046)	(0.00046)	(0.00045)	(0.00045)	(0.00045)	(0.00045)
β_5: Below 351 Squared	0.00007***	0.00008***	0.00007***	0.00007***	0.00007***	0.00007***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
β_6: Below 351 Dummy	0.57467***	0.57001***	0.58016***	0.57549***	0.58304***	0.57888***
	(0.01565)	(0.01546)	(0.01572)	(0.01553)	(0.01573)	(0.01553)
β_7: Above 351	-0.00399***	-0.00394***	-0.00398***	-0.00392***	-0.00397***	-0.00392***
	(0.00053)	(0.00054)	(0.00055)	(0.00055)	(0.00055)	(0.00055)
β_8: Above 351 Squared	0.00001*	0.00001	0.00001**	0.00001*	0.00001**	0.00001*
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
β_9: Below 3%	0.57950***	0.61984***	0.59336***	0.62953***	0.58392***	0.62082***
	(0.03791)	(0.03678)	(0.03746)	(0.03645)	(0.03746)	(0.03645)
β_{10}: Below 3% Squared	-0.16177***	-0.16992***	-0.16442***	-0.17156***	-0.16182***	-0.16914***
	(0.00871)	(0.00845)	(0.00868)	(0.00843)	(0.00868)	(0.00843)
β_{11}: Below 3% Dummy	-0.27568*	-0.11579	-0.28404**	-0.13609	-0.29217**	-0.14228
	(0.14926)	(0.14720)	(0.14308)	(0.14262)	(0.14302)	(0.14256)
β_{12}: Above 3%	0.21681***	0.19734***	0.23229***	0.21323***	0.22965***	0.21027***
	(0.03826)	(0.03737)	(0.03719)	(0.03647)	(0.03722)	(0.03647)
β_{13}: Above 3% Squared	-0.01352***	-0.01267***	-0.01513***	-0.01419***	-0.01484***	-0.01388***
	(0.00371)	(0.00366)	(0.00369)	(0.00365)	(0.00368)	(0.00363)
Borrower Controls		X		X		X
R-Squared	0.246	0.255	0.254	0.263	0.254	0.262
Fst	2,017.28	1,979.10	2,002.86	1,971.80	1,993.60	1,968.18
Observations	65,127	65,127	61,389	61,389	61,267	61,267

Notes: **Specification**: This table implements Specification 1. This is the first stage of Panel B columns (3)-(4) on Table 1, and Panel B on Table 3. The unit of observation is the borrower. Robust standard errors are in parentheses. We exclude borrowers from 9 loan officers who have not worked long enough with our partner lender to be eligible for a bonus. All regressions include the following instruments: distance to 169 borrowers from below and its square; distance to 351 borrowers from below and its square; distance to 351 borrowers from above and its square; distance to 3% lateness indicator from below and its square; and distance to 3% lateness indicator from above and its square. Dummy variables are also included to control for loan officers to whom an instrument does not apply. Finally, the following variables are also included as controls: "Principal": Loan amount given to the borrower; "Amount at Risk": the complete pending amount if the borrower has 7 or more days late (and zero otherwise); "Borrower Loan Cycle": the number of cycles that the borrower has been with our partner lender; controls for average days late before Baseline, and demographic and business characteristics from Table A1. **Outcome variable**: Columns (1)-(6) report results on the Shapley Cost (USD), which is a measure of the value of a borrower to a loan officer's portfolio. See Section 3 for details on the construction of this variable. Borrowers who are endorsed in other rounds are excluded from the regressions. So borrowers endorsed at Mitigation or Recognition are excluded from columns 1 and 2. Borrowers endorsed at Baseline or Recognition are excluded from columns 3 and 4. Borrowers endorsed at Baseline or Mitigation are excluded from columns 5 and 6.

Table A3: Robustness Check: Do Endorsements Predict Default on Graduation Loans?

	Late $\geq$ 15 Days		Late $\geq$ 90 Days		Defaulted		Amount Defaulted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Endorsements								
$\beta_{Baseline}$: Endorsed at Baseline	-0.002	0.000	-0.002	-0.001	0.008	0.010	14.702	18.565
	(0.016)	(0.016)	(0.008)	(0.008)	(0.015)	(0.014)	(34.620)	(34.372)
Mean: Not Endorsed	0.067	0.067	0.024	0.024	0.021	0.021	50.476	50.476
	[0.251]	[0.251]	[0.152]	[0.152]	[0.145]	[0.145]	[358.632]	[358.632]
Lasso Controls		X		X		X		X
Observations	19,590	19,590	19,590	19,590	682	682	682	682
Panel B: Mitigation Endorsements								
$\beta_{Mitigation}$: Endorsed at Mitigation	-0.036**	-0.036**	-0.016***	-0.016***	-0.015***	-0.015***	-27.807**	-27.807**
	(0.017)	(0.017)	(0.004)	(0.004)	(0.005)	(0.005)	(11.587)	(11.575)
Mean: Not Endorsed	0.057	0.057	0.018	0.018	0.015	0.015	27.807	27.807
	[0.231]	[0.231]	[0.132]	[0.132]	[0.120]	[0.120]	[254.131]	[254.131]
Lasso Controls		X		X		X		X
Observations	14,692	14,692	14,692	14,692	496	496	496	496
Panel C: Recognition Endorsements								
$\beta_{Recognition}$: Endorsed at Recognition	0.032	0.032	-0.011***	-0.011***	-0.009**	-0.009**	-20.416*	-20.416*
	(0.047)	(0.047)	(0.003)	(0.003)	(0.005)	(0.005)	(11.425)	(11.412)
Mean: Not Endorsed	0.053	0.053	0.016	0.016	0.009	0.009	20.416	20.416
	[0.225]	[0.225]	[0.124]	[0.124]	[0.096]	[0.096]	[237.202]	[237.202]
Lasso Controls		X		X		X		X
Observations	11,608	11,608	11,608	11,608	442	442	442	442
P-Value for F test:								
$\beta_{Baseline} = \beta_{Mitigation}$	0.121	0.101	0.119	0.088	0.138	0.100	0.240	0.190
$\beta_{Baseline} = \beta_{Recognition}$	0.552	0.577	0.214	0.167	0.254	0.195	0.335	0.273
$\beta_{Mitigation} = \beta_{Recognition}$	0.196	0.196	0.373	0.373	0.143	0.143	0.183	0.183

Notes: **Specification**: This table implements Specification 8. This is a robustness check for Table 5 - the sample is additionally limited to the 12 months after each survey round. So all loans in Panels A-C are observed for the same duration. Standard errors are in parentheses, clustered at the borrower level. Standard deviations are in brackets. Columns (1)-(4) are borrower-week level regressions and include a week fixed effects. Columns (5)-(8) are borrower level regressions. For all panels, endorsed borrowers in other rounds are excluded from each round regression, so the sample for each round comprises borrowers endorsed at that given round and those never endorsed. So borrowers endorsed at Mitigation or Recognition are excluded from Panel A. Borrowers endorsed at Baseline or Recognition are excluded from Panel B. Borrowers endorsed at Baseline or Mitigation are excluded from Panel C. The omitted group in all panels is borrowers who were never endorsed at any round. The sample in every panel is limited to graduation loans that are made after each round of surveys. Odd columns don't include any control variables. Even columns include double-post lasso controls for average days late before Baseline, and demographic and business characteristics from Table A1. Tests of equality of Baseline and Post Mitigation, Baseline and Post Recognition, and Post Mitigation and Post Recognition coefficients are based on the SURS framework. **Outcome variable**: Columns (1)-(2) report results on an indicator variable for being late 15 or more days on a Graduation loan in the next 12 months after each survey round. Columns (3)-(4) report results on an indicator variable for being late 90 or more days on a Graduation loan in the 12 months after each survey round. Columns (5)-(6) report results on an indicator variable for having ever defaulted on a Graduation loan in the 12 months after each survey round. Columns (7)-(8) report results on total amount defaulted for each borrower in the 12 months after each survey round.

Table A4: Do Strength of Endorsements Predict Default on Graduation Loans?

	Late $\geq$ 15 Days		Late $\geq$ 90 Days		Defaulted		Amount Defaulted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Endorsements								
$\beta_{Baseline}$: Strength of Endorsement at Baseline	0.003	0.003	-0.001	-0.000	-0.001	-0.001	-4.864	-4.140
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)	(7.890)	(7.851)
Mean: Not Endorsed	0.078	0.078	0.031	0.031	0.049	0.049	102.942	102.942
	[0.268]	[0.268]	[0.173]	[0.173]	[0.216]	[0.216]	[486.837]	[486.837]
Lasso Controls		X		X		X		X
Observations	27,978	27,978	27,978	27,978	735	735	735	735
Panel B: Mitigation Endorsements								
$\beta_{Mitigation}$: Strength of Endorsement at Mitigation	-0.010***	-0.010***	-0.004***	-0.004***	-0.004***	-0.004***	-6.975**	-6.975**
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(2.930)	(2.924)
Mean: Not Endorsed	0.057	0.057	0.018	0.018	0.015	0.015	27.807	27.807
	[0.231]	[0.231]	[0.132]	[0.132]	[0.120]	[0.120]	[254.131]	[254.131]
Lasso Controls		X		X		X		X
Observations	14,692	14,692	14,692	14,692	496	496	496	496
Panel C: Recognition Endorsements								
$\beta_{Recognition}$: Strength of Endorsement at Recognition	0.007	0.007	-0.002***	-0.002***	-0.002**	-0.002**	-4.404*	-4.404*
	(0.010)	(0.010)	(0.001)	(0.001)	(0.001)	(0.001)	(2.470)	(2.464)
Mean: Not Endorsed	0.053	0.053	0.016	0.016	0.009	0.009	20.416	20.416
	[0.225]	[0.225]	[0.124]	[0.124]	[0.096]	[0.096]	[237.202]	[237.202]
Lasso Controls		X		X		X		X
Observations	11,608	11,608	11,608	11,608	442	442	442	442
P-Value for F test:								
$\beta_{Baseline} = \beta_{Mitigation}$	0.004	0.003	0.068	0.054	0.499	0.442	0.782	0.709
$\beta_{Baseline} = \beta_{Recognition}$	0.729	0.757	0.241	0.205	0.805	0.736	0.952	0.972
$\beta_{Mitigation} = \beta_{Recognition}$	0.114	0.114	0.100	0.100	0.073	0.073	0.071	0.071

Notes: **Specification**: This table implements Specification 8. Standard errors are in parentheses, clustered at the borrower level. Standard deviations are in brackets. Columns (1)-(4) are borrower-week level regressions and include a week fixed effects. Columns (5)-(8) are borrower level regressions. Strength of Endorsement for each round is a continuous variable that contains the confidence value selected by the loan officer for each endorsement, on a scale ranging from 0 to 5 (the higher the value, the higher the confidence on the endorsement). For all panels, endorsed borrowers in other rounds are excluded from each round regression, so the sample for each round comprises borrowers endorsed at that given round and those never endorsed. So borrowers endorsed at Mitigation or Recognition are excluded from Panel A. Borrowers endorsed at Baseline or Recognition are excluded from Panel B. Borrowers endorsed at Baseline or Mitigation are excluded from Panel C. The omitted group in all panels is borrowers who were never endorsed at any round. The sample in every panel is limited to graduation loans that are made after each round of surveys. Odd columns don't include any control variables. Even columns include double-post lasso controls for average days late before Baseline, and demographic and business characteristics from Table A1. Tests of equality of Baseline and Post Mitigation, Baseline and Post Recognition, and Post Mitigation and Post Recognition coefficients are based on the SURS framework. **Outcome variable**: Columns (1)-(2) report results on an indicator variable for being late 15 or more days on a Graduation loan in the months after each endorsement wave, up to March 2020. Columns (3)-(4) report results on an indicator variable for being late 90 or more days on a Graduation loan in the months after each endorsement wave, up to March 2020. Columns (5)-(6) report results on an indicator variable for having ever defaulted on a Graduation loan in the months after each endorsement wave, up to March 2020. Columns (7)-(8) report results on total amount defaulted for each borrower in the months after each endorsement wave, up to March 2020.

Table A5: Do Strength of Endorsements Predict Default on Joint Liability Loans?

	Late $\geq$ 15 Days		Late $\geq$ 90 Days		Defaulted		Amount Defaulted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Endorsements								
$\beta_{Baseline}$: Strength of Endorsement at Baseline	-0.001***	-0.000*	-0.000***	-0.000***	-0.004***	-0.002***	-0.640	-0.595
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.429)	(0.442)
Mean: Not Endorsed	0.014	0.014	0.002	0.002	0.040	0.040	17.128	17.128
	[0.117]	[0.117]	[0.044]	[0.044]	[0.195]	[0.195]	[120.488]	[120.488]
Lasso Controls		X		X		X		X
Observations	3,624,164	3,624,164	3,624,164	3,624,164	75,526	75,526	75,526	75,526
Panel B: Mitigation Endorsements								
$\beta_{Mitigation}$: Strength of Endorsement at Mitigation	-0.001*	-0.001	-0.000***	-0.000*	-0.005***	-0.004**	-2.461***	-2.690***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)	(0.669)	(0.680)
Mean: Not Endorsed	0.014	0.014	0.002	0.002	0.032	0.032	14.347	14.347
	[0.116]	[0.116]	[0.046]	[0.046]	[0.177]	[0.177]	[110.362]	[110.362]
Lasso Controls		X		X		X		X
Observations	2,497,640	2,497,640	2,497,640	2,497,640	65,420	65,420	65,420	65,420
Panel C: Recognition Endorsements								
$\beta_{Recognition}$: Strength of Endorsement at Recognition	-0.002***	-0.002***	-0.000	-0.000	-0.005***	-0.004**	-1.838	-2.042*
	(0.001)	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)	(1.168)	(1.180)
Mean: Not Endorsed	0.013	0.013	0.002	0.002	0.029	0.029	13.533	13.533
	[0.115]	[0.115]	[0.046]	[0.046]	[0.168]	[0.168]	[108.206]	[108.206]
Lasso Controls		X		X		X		X
Observations	2,260,263	2,260,263	2,260,263	2,260,263	61,971	61,971	61,971	61,971
P-Value for F test:								
$\beta_{Baseline} = \beta_{Mitigation}$	0.997	0.472	0.459	0.183	0.818	0.214	0.020	0.014
$\beta_{Baseline} = \beta_{Recognition}$	0.109	0.019	0.770	0.440	0.804	0.129	0.333	0.244
$\beta_{Mitigation} = \beta_{Recognition}$	0.274	0.275	0.857	0.911	0.980	0.776	0.642	0.719

Notes: **Specification**: This table implements Specification 9. Standard errors are in parentheses, clustered at the borrower level. Standard deviations are in brackets. Columns (1)-(4) are borrower-week level regressions and include a week fixed effects. Columns (5)-(8) are borrower level regressions. Strength of Endorsement for each round is a continuous variable that contains the confidence value selected by the loan officer for each endorsement, on a scale ranging from 0 to 5 (the higher the value, the higher the confidence on the endorsement). For all panels, endorsed borrowers in other rounds are excluded from each round regression, so the sample for each round comprises borrowers endorsed at that given round and those never endorsed. So borrowers endorsed at Mitigation or Recognition are excluded from Panel A. Borrowers endorsed at Baseline or Recognition are excluded from Panel B. Borrowers endorsed at Baseline or Mitigation are excluded from Panel C. The omitted group in all panels is borrowers who were never endorsed at any round. The sample in every panel is limited to graduation loans that are made after each round of surveys. Odd columns don't include any control variables. Even columns include double-post lasso controls for average days late before Baseline, and demographic and business characteristics from Table A1. Tests of equality of Baseline and Post Mitigation, Baseline and Post Recognition, and Post Mitigation and Post Recognition coefficients are based on the SURS framework. **Outcome variable**: Columns (1)-(2) report results on an indicator variable for being late 15 or more days on joint liability loans in the months after each endorsement wave, up to March 2020. Columns (3)-(4) report results on an indicator variable for being late 90 or more days on joint liability loans in the months after each endorsement wave, up to March 2020. Columns (5)-(6) report results on an indicator variable for having ever defaulted on joint liability loans in the months after each endorsement wave, up to March 2020. Columns (7)-(8) report results on total amount defaulted for each borrower in the months after each endorsement wave, up to March 2020.

Table A6: Repayment Behavior of Groups in Which Someone Graduated

	Late $\geq$ 15 Days	Late $\geq$ 90 Days	Defaulted	Amount Defaulted
	(1)	(2)	(3)	(4)
Panel A: Everyone				
β_{All}: Post	0.007	0.000	0.003	0.749
	(0.005)	(0.001)	(0.006)	(2.959)
Mean: Pre-period	0.002	0.000	0.004	1.565
	[0.043]	[0.000]	[0.065]	[28.301]
Observations	119,600	119,600	119,600	119,600
Panel B: Never Endorsed				
β_{Never}: Post	0.007	-0.000	0.005	1.401
	(0.007)	(0.001)	(0.007)	(3.927)
Mean: Pre-period	0.002	0.000	0.005	1.675
	[0.046]	[0.000]	[0.069]	[29.029]
Observations	86,167	86,167	86,167	86,167
Panel C: Endorsed at Baseline				
$\beta_{Baseline}$: Post	0.009	0.001	-0.004	-1.697
	(0.008)	(0.001)	(0.003)	(1.385)
Mean: Pre-period	0.001	0.000	0.003	1.477
	[0.035]	[0.000]	[0.057]	[28.451]
Observations	29,692	29,692	29,692	29,692
Panel D: Endorsed at Mitigation				
$\beta_{Mitigation}$: Post	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Mean: Pre-period	0.000	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	1,641	1,641	1,641	1,641
Panel E: Endorsed at Recognition				
$\beta_{Recognition}$: Post	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Mean: Pre-period	0.000	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	2,100	2,100	2,100	2,100

Notes: **Specification**: This table implements Specification 12. Standard errors are in parentheses, clustered at the joint-liability (JL) group level. Standard deviations are in brackets. These are borrower-week level regressions, including loan cycle, month and individual fixed effects for all specifications. The sample is limited to groups where just one borrower graduated to a Graduation loan, and restricted to borrowers that were in the JL group when the graduating borrower left. The main explanatory variable "Post" is a dummy variable that equals 1 for periods when the graduating borrower has graduated and left the group, and zero when the borrower is still a member of the JL group. Panel A includes all group members that were in the JL group when the graduating borrower left, whereas Panels B to E include group members that were in the JL group when the graduating borrower left, but restrict the group sample according to the endorsement round of the graduating borrower. Finally, note that the zeros in Panels D and E (variable drops) are caused by no one defaulting in those samples. **Outcome variable**: Column (1) reports results on a dummy variable that equals 1 if the borrower is 15 or more days late in their installments, and zero otherwise; Column (2) reports results on a dummy variable that equals 1 if the borrower is 90 or more days late in their installments, and zero otherwise; Column (3) reports results on a dummy variable that equals 1 if the borrower defaulted, and zero when the borrower has not defaulted yet; and Column (4) reports results on a continuous variable containing the amount defaulted by a borrower if the borrower defaulted, and equals zero when the borrower has not defaulted yet.

D Formula for Computing Loan Officer Compensation in Section 3

In this section we describe the formula by which the variable component of loan officer compensation, or bonus, was computed as of November 2018 (i.e. prior to our compensation shifts). Loan officer compensation was calculated and distributed monthly, as a function of the number of borrowers their portfolio, the total amount of capital in their portfolio, and various summaries of borrower lateness. The following steps document the exact calculation.

Step 1: Determining a Loan Officer's "Range"

Loan officers fall into one of three ranges, determined by the largest number of borrowers they have ever managed.

Range	Number of borrowers
1	0-168
2	169-350
3	$\geq$351

Step 2: Determining Whether a Loan Officer Has Access to Any Bonus

To receive a positive bonus, loan officers must meet the following three conditions.

- Condition 1: The loan officer must be in Range 2 or 3.
- Condition 2: If the loan officer is in Range 3, then either she must currently manage at least 351 borrowers, or the average number of borrowers she has managed over the last four months must be at least 351.
- Condition 3: Her three-month average portfolio at risk must not exceed 3%, where portfolio at risk in a given month is defined as $\frac{\text{Total debt of borrowers who are at least 7 days late}}{\text{Total value of portfolio}}$

Step 3: Determining The Base Bonus

If the loan officer meets all conditions in Step 2 above she is eligible for a positive bonus, which is a function of her Range and the total value of her portfolio in chilean pesos (CLP).

Step 4: Determining Compensation Multiplier Based on Lateness

Level	Ranges	Portfolio	Bonus Amount (Base Bonus)
1	2 and 3	$\geq$ CLP$20,000,000	CLP$23,543
2	2 and 3	$\geq$ CLP$40,000,000	CLP$70,628
3	2 and 3	$\geq$ CLP$50,000,000	CLP$141,256
4	2 and 3	$\geq$ CLP$70,000,000	CLP$223,655
5	3	$\geq$ CLP$85,000,000	CLP$278,863
6	3	$\geq$ CLP$100,000,000	CLP$315,236
7	3	$\geq$ CLP$130,000,000	CLP$343,219

Loan officers in Range 3 are eligible for a compensation multiplier as a function of their total portfolio at risk (defined in Step 2).

Portfolio at risk	Multiplier
0% - 0.49%	10%
0.5% - 0.99%	6%
1% - 1.49%	4%
1.5% - 1.99%	2%
$\geq$ 2%	-

A loan officer i's bonus is then Base Bonus$_i$ $*$ (1+multiplier$_i$), where Base Bonus$_i$ is computed in Step 3 and multiplier$_i$ is computed in Step 4.

Instruments for Section 3

In interviews with loan officers, it became apparent that by far the most salient threshold were those based on the number of borrowers (i.e. those that determine Range), and the 3% threshold for portfolio at risk, which determines whether loan officers have access to a bonus at all. Therefore the instruments we construct for our regressions in Section 3 are based on the distance between a loan officer's portfolio and these thresholds.

Namely these are:

- $Dist_{169} \equiv 169 - n$ if $n < 169$ where n is the number of borrowers a loan officer manages. $Dist_{169}$ takes a filler value if $n \geq 169$ as this distance is no longer relevant, and a dummy is included indicating if the filler value is used.
- ${Dist_{169}}^2$
- $Dist_{351-} \equiv 351 - n$ if $n \in [169, 350]$. $Dist_{351-}$ takes a filler value if $n < 169$ or $n \geq 351$, and a dummy is included indicating if the filler value is used.

- $Dist_{351-}{}^2$

- $Dist_{351+} \equiv n - 351$ if $n \geq 351$. $Dist_{351+}$ takes a filler value if $n < 351$, and a dummy is included indicating if the filler value is used.

- $Dist_{351+}{}^2$

- $Dist_{3\%-} \equiv 3 - r$ if $r \leq 3$ where r is the loan officer's portfolio at risk. $Dist_{3\%-}$ takes a filler value if $r > 3$, and a dummy is included indicating if the filler value is used.

- $Dist_{3\%-}{}^2$

- $Dist_{3\%+} \equiv r - 3$ if $r > 3$ where r is the loan officer's portfolio at risk. $Dist_{3\%+}$ takes a filler value if $r \leq 3$, and a dummy is included indicating if the filler value is used.

- $Dist_{3\%+}{}^2$

We utilize separate instruments for loan officers above and below 351 borrowers and above and below 3% at risk as there is an asymmetric effect of crossing these thresholds from above and below.

E Back of the Envelope Calculation for Social Value of Forgone Graduations

In this section we describe a back of the envelope calculation to determine the social value of forgone endorsements, both within our partner lender and then in the industry more broadly.

First we calculate the additional profit that our partner lender enjoyed from graduating borrowers who were endorsed after we changed loan officer compensation. On average, among borrowers endorsed after our compensation change, our partner lender earned an additional USD 285 on loans to graduated borrowers relative to standard joint-liability loans. As in Section 6, we use data on the profitability of completed loans for the joint-liability portfolio and predicted profits for graduation loans that were not completed by March 2020, and we assume a monthly borrowing rate of 0.38%.

To calculate the benefit of graduation to the borrowers, we assume a monthly return to capital of 6%, utilizing the lower bound of the estimates in Field et al. (2013). We assume this return does not compound, but lasts forever. On average among endorsed borrowers, graduation loans are about USD 1,500 larger than joint-liability loans. If borrowers have a discount rate of 5% per year, this implies a benefit of USD 54,180 from graduation. And if borrowers have a discount rate of 40% per year, this implies a benefit of USD 9,030 from graduation.

The interest rate on graduation loans at our partner lender is about 28% APR, paid monthly. The average tenure for graduation loans is about 14 months. Applying this interest rate to the loan size increase of USD 1,500, and assuming a yearly discount rate of 5%, implies that borrowers value the stream of additional interest payments at USD -475, and a yearly discount rate of 40% implies that borrowers value the additional interest payments at USD -400.

On net consumer surplus is estimated to be about USD 53,705 at a borrower discount rate of 5% and USD 8,630 at a discount rate of 40%.

The total surplus from these loans is therefore USD 8,915 to USD 53,990.

Multiplying this surplus by the 497 endorsements we received after the compensation shifts indicates that the social value of forgone graduations was USD 4.4 million to USD 26.8 million at our partner lender prior to our intervention.

We argued in Section 8 that even in microfinance institutions that do not have a graduation program, loan officers have some discretion in determining graduation rates to competing institutions. To estimate the social value of forgone graduations in the microfinance sector worldwide we assume that loan officers at other institutions deter graduation at the same rate as those of our partner lender – about 0.46%. The microfinance industry serves about 140 million borrowers worldwide (Convergences, 2019), implying about 650,000 forgone graduations from microfinance. To scale the social value of a forgone graduation to a representative borrower worldwide, we note that the average microloan worldwide is about USD 886 (Convergences, 2019), while the average loan at our partner lender is about 20% larger. Dividing our estimate of the social value of a forgone graduation at our partner lender by 1.2 yields a worldwide average social value of forgone graduation of between USD 7,400 and USD 45,000.

Therefore, the forgone social value of graduations worldwide is estimated to be between USD 4.8 billion for an annual discount rate of 40% and USD 29.2 billion for a discount rate of 5%.