

How do Borrowers Respond to a Debt Moratorium? Experimental Evidence from Consumer Loans in India*

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Abstract

Debt moratoria that allow borrowers to postpone loan payments are a frequently used tool intended to soften the impact of economic crises. One concern with such policies is that they might give rise to moral hazard by changing borrower beliefs about credit enforcement and the likelihood of future relief. We partner with a large consumer lender in India to issue randomized debt forbearance offers to a nationwide sample of borrowers. In the experiment, borrowers receive identical forbearance offers that are presented either as an act of generosity by the lender or as the result of government regulation. We find that delinquent borrowers who are offered a debt moratorium by their lender are 4 percentage points (6.9 percent) *less likely* to default on their loan, while forbearance has no effect on repayment if it is granted by the regulator. Borrowers receiving forbearance offers from their lender are also more likely to do future business with the lender. In a follow-up experiment we find that demand for the lender's products is 16.3 percentage points higher among customers who were offered repayment flexibility by their lender than among customers who received a moratorium offer presented as an initiative of the regulator. Overall, our results suggest that, rather than generating moral hazard, debt forbearance can improve loan repayment and lead to higher value banking relationships.

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1 Introduction

Debt forbearance policies that allow borrowers to postpone loan payments are a popular tool to soften the credit market impact of economic downturns and have been used to an unprecedented extent in recent crises. More than 80 countries enacted debt moratoria for individuals and firms in response to the Covid-19 crisis.¹ In the U.S. alone, financial institutions granted more than \$2 trillion in debt forbearance to over 60 million borrowers over this period (Cherry et al., 2021). The effects of such repayment deferrals on borrower behavior and overall credit market discipline are ambiguous, however. On the one hand, forbearance can help borrowers cope with temporary income shocks and reduce the aggregate impacts of financial distress on the economy (Ganong and Noel, 2020; Mian et al., 2013). At the same time, one common concern is that repayment deferrals—especially when they are granted by private lenders—may generate expectations of lenient credit enforcement and give rise to *moral hazard* through this channel.² This apprehension seems well-founded, especially in the market for consumer credit, where survey evidence indicates that borrowers overwhelmingly expect repayment deferrals to damage credit discipline.³

In this paper, we study the borrower response to debt forbearance using a nationwide field experiment conducted in the context of a large debt forbearance program in India. We take advantage of an unusual collaboration with a leading consumer finance lender whose late paying customers were eligible for a debt moratorium. This allows us to issue real stakes debt forbearance offers that are fully compliant with the terms of India’s national consumer loan moratorium. We first report results from our main experiment, which measures the impact of randomized forbearance offers on borrower beliefs and loan repayment. In the experiment, late paying borrowers are randomly assigned to receive debt forbearance offers that are presented either as being *granted voluntarily by a private lender* or *extended as the result of government regulation*, or to one of several placebo treatments. We observe the impact of forbearance offers on borrower beliefs as well as the decision to take up the offer and to repay one’s loan. In a second experiment, we explore the impact of forbearance offers on banking relationships and demand for future interactions with the lender. In the product adoption experiment, borrowers who received one of the forbearance offers in the main experiment are contacted with an offer for a personal loan or a non-credit financial product that is either nondescript or clearly marketed as a product of their lender. In combination, the forbearance and product adoption experiments allow us to examine how forbearance offers shape borrower expectations and moral hazard, and how they affect the willingness to deepen banking relationships and interact with the lender in the future.

We find that, rather than giving rise to moral hazard, none of the debt forbearance offers extended in our experiment have a negative effect on loan repayment. Instead, borrowers who receive a moratorium offer presented as an initiative of their lender are 4 percentage points (6.9 percent) *less* likely to default on their loan, while the offer has no impact on loan repayment if it is presented as an initiative of the regulator. Turning to the longer term impact on forbearance offers, we show that

¹ See World Bank (2022) and International Monetary Fund (2022).

² In most cases, debt forbearance is optional and thus acts as a *de facto* line of credit that borrowers can draw on when they have limited liquidity and a payment deferral is needed (Cherry et al., 2021). See also Dobbie and Song (2020) and Indarte (2022) for a discussion of the role of liquidity and moral hazard in household bankruptcy decisions.

³ In our setting, a representative survey suggests that 66% of borrowers expect forbearance to damage credit discipline.

borrowers who were granted repayment flexibility by their lender are significantly more likely to engage with the lender in the future. In the product adoption experiment, borrowers who received a moratorium offer from their lender had 16 percentage points (38.5 percent) higher demand for a non-credit product offered by the lender than participants who had received a forbearance offer with identical terms presented as an initiative of the regulator. At the same time, borrowers who received a forbearance offer presented as an initiative of their lender had the same demand for personal loans marketed by the lender as customers in other treatment conditions, which suggests that the demand for future interactions with the lender is not driven by expectations of lenient enforcement or contract renegotiation that could give rise to moral hazard. Taken together, these results suggest that allowing lenders to take credit for forbearance policies can alleviate financial distress without damaging credit discipline and lead to advantageous selection into banking relationships.

Our experiment takes advantage of an unusual policy context that enables us to identify the response to forbearance offers in a real stakes environment. Measuring the effects of debt forbearance on borrower behavior is challenging for several reasons. Payment deferrals are typically endogenous to borrower and lender characteristics, and episodes of debt forbearance also tend to coincide with large economic crises that further confound the impact of repayment flexibility on borrower behavior. We overcome these challenges by working with a leading consumer lender restructuring loans after the expiration of a national consumer debt moratorium in India. Importantly, the policy guidance at the time encouraged lenders to provide further relief but did not strictly *require* them to offer additional repayment deferrals to all customers. This feature of the policy enables us to issue moratorium offers that are fully compliant with the terms of national policy and to randomize the extent of debt relief as well as the manner in which moratorium offers are presented to customers. The offers in our experiment are fully truthful, because both the statement that the moratorium is offered as a result of regulatory guidance and the statement that it is offered by the lender are true, and we observe whether customers take up the moratorium as well as their subsequent loan repayment and interactions with the lender.

In the first part of the paper, we examine how randomized moratorium offers affect borrower expectations and loan repayment. To examine the impact of forbearance offers on *overall credit discipline* beyond the loans covered by the moratorium, we combine administrative data on loan repayment with credit bureau information that captures the universe of loan payments for customers in our sample. We establish that borrowers who receive a moratorium offer from their lender prioritize payments for loans covered by the moratorium and are significantly less likely to default, but are no more likely to become delinquent on other loan payments as a result. Hence, rather than merely reallocating payments across loans, borrowers mobilize additional liquidity to make payments. The absence of moral hazard in response to forbearance offers is consistent with the results of belief elicitation, conducted immediately after the intervention stage and several months after the experiment. Contrary to the widely held assumption that debt forbearance induces moral hazard by generating expectations of future relief or lenient credit enforcement, we find no treatment effects of moratorium offers on borrower expectations measured at different time horizons and using different elicitation methods.

We next explore alternative mechanisms that can explain *why* moratorium offers affect loan repay-

ment. Intuitively, temporary debt forbearance may have positive effects on loan repayment through two main channels that have sharply different implications for the design of debt moratoria. First, temporary payment deferrals may improve a borrower's ability to repay by relaxing liquidity constraints. We rule out this possibility by showing that forbearance offers framed as coming from the lender affect loan repayment, while identical offers framed as an initiative of the regulator have no effect on repayment despite having the same take-up rate and thus an identical effect on relaxing borrowers' financial constraints. Second, borrowers may prioritize repayments to a lender that granted them a moratorium out of a sense of reciprocity. Such reciprocal behavior could either be entirely altruistic or motivated by strategic considerations. Borrowers might, for instance, interpret the offer as an investment in the lending relationship that highlights the importance of maintaining a good repayment record that safeguards the option of doing business with the lender in the future.

To distinguish between these interpretations, we first present direct survey evidence to explore how borrowers interpret the forbearance offer and its implications for their relationship with the lender. Second, we examine the heterogeneity in the repayment response by borrower characteristics to explore which borrowers are most interested in signaling their creditworthiness to the borrower in response to a forbearance offer. We show that borrowers who received a moratorium offer framed as an initiative of the lender are more likely to believe that the offer was extended only to the most creditworthy borrowers, that accepting the offer would not send a negative signal about their creditworthiness, and that the main rationale for timely repayment is the prospect of qualifying for future loans from the lender that extended the forbearance offer (as opposed to improving one's credit score and being able to access credit from other lenders). We also show that loan repayment improves most strongly for borrowers for whom the forbearance offer is immediately valuable. This includes, for instance, borrowers who suffered large income losses, and borrowers who are especially concerned about maintaining a good credit score. Overall, these results are most consistent with the interpretation that rather than triggering a non-strategic sense of reciprocity, moratorium offers extended by the lender improve repayment because they remind borrowers of the value of a continued lending relationship.

In the second part of the paper, we examine the implications of forbearance offers for the establishment of longer term banking relationships, using a complementary product adoption experiment. One striking finding from the main experiment is that borrowers who receive a forbearance offer framed as an initiative of the lender are significantly more likely to report that they would like to do business with the lender in the future.⁴ This demand for future interaction with the lender could be explained by two different motivations with sharply different implications for the selection into longer term banking relationships. On the one hand, it could be that borrowers interpret forbearance offers from the lender as a signal that it is easier to renegotiate debts with the lender in the event of financial distress. This channel, which is distinct from moral hazard in loan repayment, could lead to the selection of borrowers with higher credit risk into repeated interactions and longer term relationships with the lender. On the other hand, it is possible that borrowers who are granted repayment flexibility

⁴ Specifically, in a survey conducted within two days of the intervention, 26 percent of borrowers who received a forbearance offer from their lender state that they would like to do business with the lender again and 41 percent of respondents state that, for an identical loan offer, they would prefer to take a loan from the borrower that offered them repayment flexibility.

have higher demand for the lender's products independently of such strategic considerations. If this mechanism is at play, it could foster the establishment of more profitable banking relationships. The product adoption experiment is designed to test these competing mechanisms.⁵

In the product adoption experiment, borrowers who had received a debt forbearance offer in the main experiment received a marketing call in which they were offered a financial product. The marketing calls took place after the moratorium had expired and between three months and a year after participants had received the forbearance offer treatment. Borrowers were randomly assigned to receive either a marketing call that invited them to apply for a consumer loan which, similar to their current loan, requires regular interaction with the lender after loan origination, or a marketing call that offered the opportunity to purchase an "enhanced credit report", a non-credit product that involves only a one-time interaction with the lender and whose terms cannot be modified. The treatments additionally varied, whether the offers were *branded* and prominently mentioned the name of the lender, or *non-branded* and omitted mentioning the name of the lender in the call. This real stakes design allows us to test whether borrowers that had been offered debt forbearance as an initiative of their lender (rather than the regulator) have higher demand for financial products that are marketed by the lender that granted them repayment flexibility and whether there is demand for products from the lender that have no scope for ex post renegotiation.

We find suggestive evidence that forbearance offers granted by the lender strengthen banking relationships and the demand for future interactions with the lender for non-strategic reasons. While demand for loans is high across the board and there are no meaningful differences between treatment conditions, borrowers who—in the main experiment—received a moratorium offer from the lender have 16 percentage points (38.5 percent) higher demand for the branded non-credit product than borrowers who received debt forbearance offers framed as an initiative of the regulator. Because there is no scope for ex post renegotiation in the case of the non-credit product, this suggests that borrowers prefer to do business with the lender that granted forbearance when they were facing financial distress for reasons other than making the inference that it is easy to renegotiate financial contracts with the lender. These findings from a fully incentivized product adoption experiment are consistent with results from belief elicitation exercises and suggest that, in our setting, forbearance offers create neither moral hazard in short-term loan repayment nor in the longer-term selection into banking relationships and demand for repeated interactions with the lender.

Our results have implications for the design of debt relief policies. Regulators and private financial institutions have often been reluctant to grant temporary repayment deferrals because of the widespread assumption that such policies can damage credit discipline. Indeed, in a survey representative of the Indian population, conducted as part of our study supports this assumption—66 percent of respondents state that they expect debt moratoria to have a negative effect on timely loan repayment. In sharp contrast to this view, we provide evidence that temporary debt forbearance has no negative effect on loan repayment and does not create expectations of future debt relief or lenient credit enforcement, which are the main channels through which one would expect repayment flexibility to

⁵ For empirical evidence on relational contracting, see also [Brown et al. \(2004\)](#) who show that repeated interactions are more likely to arise in markets with imperfect third-party enforcement, and situations where one contracting party makes an investment in the relationship in the initial period, as is the case in our setting.

generate moral hazard. Instead, we show that —if properly designed —temporary debt forbearance can improve loan repayment, generate soft information about customers’ willingness to repay and lead to advantageous selection into banking relationships.

This paper contributes to several strands of the literature. First, our paper is related to an active debate about the effects of debt relief on financially distressed borrowers. Much research on this topic has focused on the impact of debt reductions that occur through restructuring and loan modification (Agarwal et al. 2017, 2022; Piskorski et al. 2010; Piskorski and Seru 2018; Abel and Fuster 2021) or consumer bankruptcy (Dobbie and Song 2015, 2020; Auclert et al. 2019; Indarte 2022).⁶ One common concern with relief measures that include an element of debt forgiveness is that they might generate moral hazard by changing borrower expectations about the enforcement of debt contracts and the consequences of non-repayment in the future. In line with this concern, Giné and Kanz (2018) find that a large debt forgiveness program in India generated widespread moral hazard and had the unintended consequence of precipitating a reallocation of credit away from borrowers covered by the program. Our paper adds to this literature by examining whether temporary repayment deferrals that reschedule, rather than write off, debt payments might offer an alternative that can avoid some of the incentive distortions that arise when debt relief includes debt forgiveness.

Second, our paper adds to the literature on the sources of repayment delinquency and default. While several papers have documented strategic motives in the repayment decision (Guiso et al. 2013; Gerardi et al. 2018), a more recent strand of research has found short-term liquidity constraints and negative life events to be the primary drivers of non-repayment and bankruptcy filings among households (Fuster and Willen 2017, Ganong and Noel 2020, 2022). One implication of these findings is that, especially in cases where the underlying shock is transitory, temporary repayment deferrals can help households smooth consumption, reduce preventable defaults and could therefore be both socially optimal and preferable to other forms of debt relief from the perspective of lenders.⁷ Consistent with this view, Aydin (2021) tests alternative forms of debt relief in a randomized experiment and finds that forbearance is especially effective at preventing late-cycle delinquencies among liquidity constrained borrowers. Similarly, Barboni and Agarwal (2021) and Battaglia, Gulesci and Madestam (2021) study microfinance contracts with embedded repayment flexibility and show that optional repayment deferrals improve business outcomes because they provide insurance when liquidity is needed. We contribute to this line of research by exploring how these benefits of temporary forbearance compare to the potential moral hazard costs that may arise through changes in borrower expectations.

Finally, our paper connects the evidence on debt relief to a growing body of research on the role of *non-monetary* motivations in debt accumulation and repayment. A large body of research shows that household financial decisions are shaped by behavioral factors (see Andersen et al. 2020; Stango and Zinman 2022; Kuchler and Pagel 2021). Similarly, a number of recent studies have shown that ethical considerations play a role in individual financial decisions, especially so in debt repayment. Guiso et al. (2013), show that the decision to default on a mortgage is shaped by ethical and reputational considerations. In a field experiment with credit card customers in Indonesia, Bursztyn et al. (2019)

⁶ See Ponticelli and Alencar (2016) for the effects of debt reduction on firms.

⁷ See Bolton and Rosenthal (2002) and Eberly and Krishnamurthy (2014) for a theoretical analysis of debt relief policies.

show that moral appeals increase the repayment of credit card debt, even when neither the moral appeal nor the repayment decision are observable to others. We contribute to this literature by showing that forbearance affects loan repayment only when it is framed as an act of generosity on the part of the lender, and by exploring the mechanisms behind this result.

The remainder of the paper proceeds as follows. Section 2 outlines the institutional context of our study and provides motivating evidence on beliefs about debt forbearance in the population. Section 3 presents the design and results of the main debt forbearance experiment. In Section 4 we report the results of a complementary product adoption experiment to explore the effects of forbearance offer on repeated interactions and longer term banking relationships, and section 5 concludes.

2 Background and Motivating Evidence

2.1 Debt Forbearance for Consumer Loans in India

The experiment we describe in this paper is set in India, one of the world’s largest markets for consumer finance, and takes advantage of an unusual policy setting created by the extensive debt relief measures enacted in response to the Covid-19 pandemic. In March 2020, India experienced a first sharp rise of infections and the Indian government declared a first nationwide lockdown on March 25, 2020. The lockdown was announced at short notice and severely disrupted economic activity across the country.

To mitigate these economic effects of the pandemic, the Reserve Bank of India—similar to regulators in many other countries—announced an emergency support package that included a number of monetary and financial policy measures aimed at avoiding widespread financial distress. The package had a large debt forbearance component and included a first debt moratorium under which bank and non-bank lenders were instructed to offer payment deferrals on individual and small business loans from March 1, 2020 through May 31, 2020. The moratorium was eventually extended for another three months until August 31, 2020 and covered covered 40 percent of *all* outstanding loans in India.

Importantly, the policy did not *require* lenders to offer a moratorium to delinquent borrowers, but provided very strong incentives to do so, as it allowed them to avoid higher risk provisioning rates for delinquent loans covered by the moratorium. That is, lenders did not have to provision for possible credit losses on their balance sheets for loans covered by the moratorium, whereas risk provisioning rates (which are treated as an expense on a bank’s balance sheet) would have increased sharply with an increase in delinquencies among these loans. As a result, practically all bank and non-bank lenders in India complied with the policy and offered repayment deferrals to eligible borrowers.⁸ However, all credit bureau reporting was paused while the policy was in effect, and loans covered by the moratorium were not reclassified as restructured or non-performing.

Once the debt moratorium ended, lenders were still encouraged to grant forbearance to borrowers in need. However, the incentives for doing so were reduced, as loans covered by a moratorium now had to be classified as restructured, which entails higher risk provisioning requirements but is still

⁸ Lenders were allowed to continue charging interest. The issue of compound interest during the moratorium, i.e. interest on accrued interest, was argued before the Supreme Court of India. Ultimately, borrowers ended up paying simple interest, with the government reimbursing lenders for the difference between simple and compound interest.

preferable to the standard requirement of having to declare loans as nonperforming if they remain in arrears for more than three months. In practice, lenders in this period weighed the administrative cost of offering repayment deferrals against the credit risk among their customer population.

We conducted our main experiment after the first round of India’s consumer credit moratorium, when debt forbearance was still widely granted but had become *de facto* optional from the perspective of the lender. While banks could still avoid classifying loans as nonperforming if they granted forbearance during this time window, this now entailed a non-zero cost of marking these loans as restructured. As a result, banks exercised greater discretion in making customers eligible for repayment deferrals and generally offered forbearance only to customers with a realistic prospect of eventual repayment. This provides an ideal setting for our experiment in which we vary whether customers receive a moratorium offer as well as whether this offer is presented as an initiative of the lender or the regulator.

2.2 Beliefs about Debt Forbearance in the Population

The impact of debt relief policies has been a hotly contested topic of public debate, with proponents emphasizing their stabilizing effects and critics worrying about their potential to generate moral hazard.⁹ To provide additional motivating evidence for our exploration of the borrower response to debt forbearance, we conducted a nationally representative survey in India in which we elicited borrower opinions about the likely effect of debt forbearance policies.¹⁰ The results show that even in the case of an aggregate shock where forbearance can be reasonably assumed to benefit primarily borrowers in genuine financial distress, respondents overwhelmingly expect that repayment deferrals will generate moral hazard and damage overall credit discipline.

Figure 1 summarizes the main results of the survey.¹¹ We first asked borrowers whether they thought that forbearance extended through India’s first debt relief package, which is the setting of our study, had benefited households that were in actual financial distress and needed support. Consistent with interpreting the pandemic as an aggregate shock, respondents believe that this is the case: 51.8 percent of respondents think that most beneficiaries needed and deserved relief, and only 1.71 percent think that most beneficiaries did not need or deserve debt forbearance.¹²

Second, we asked borrowers how they thought a repayment moratorium would affect beliefs about the lender—the primary channel through which one would expect debt forbearance to generate moral hazard. We first asked respondents whether they agreed with the statement “*if a private lender gives more debt relief than others, their borrowers might take them less seriously and de-prioritize making payments to that lender*”, which captures the idea that forbearance granted by private lenders reduces loan repayment

⁹ In addition to the debate on repayment flexibility for individual borrowers, there has also been a debate on *regulatory forbearance*, which describes policies that allow banks to postpone the recognition of credit risks. Chari et al. (2021) examine the effects of such policies enacted in the aftermath of the 2007-2009 global financial crisis in India and show that they led to widespread incentive distortions, “evergreening” of de facto non-performing loans, and credit misallocation.

¹⁰ Our approach for this descriptive exercise is similar to recent work that has used online surveys with representative populations to examine how people form opinions about social issues and public policies (see, for example, Stantcheva, 2020). We report descriptive statistics for the survey population in Table B.1 in the Supplementary Appendix. Table B.2 provides summary statistics on the responses to the online survey.

¹¹ The full survey instrument and additional results are available in Table B.1 in the Supplementary Appendix.

¹² In total, 39.9 percent of respondents think that some beneficiaries needed and deserved relief, and 6.6 percent of respondents think some beneficiaries did not need or deserve debt forbearance.

because it damages the lender’s reputation and creates expectations of lenient credit enforcement. We find that 65 percent of respondents, agree with this statement. We then asked respondents whether they agreed with the statement “*widespread debt relief can discourage borrowers who are able to pay their debts from paying them in the future*” to capture beliefs about the effect of forbearance on overall credit discipline. A similarly high share (66 percent) of respondents agreed with this statement.

We additionally asked borrowers about their expectations of future debt relief, which is another channel through which debt forbearance could weaken credit discipline and generate moral hazard. Specifically, respondents were asked whether they expected that there would be another debt moratorium in the event that India would experience future waves of the pandemic similar to the events that triggered the first round of debt forbearance in India. The overwhelming majority of respondents in our representative sample of the population (72.93 percent) believe this to be “likely” or “very likely”. We then ask whether borrowers expect future forbearance to be offered by private banks or the government, and find that 63 percent of respondents believe that banks are likely or very likely to offer forbearance in the future while an overwhelming majority of 84 percent of respondents believe that future forbearance will be granted as the result of a government or regulator initiative.

In sum, the results of a survey representative of the borrower population illustrate that respondents overwhelmingly expect repayment deferrals to have negative implications on credit discipline. Moreover, respondents believe that loan repayment could be impacted negatively through two alternative channels: the anticipation of future relief and expectations of more lenient credit enforcement by lenders that grant repayment deferrals.

3 The Debt Forbearance Experiment

3.1 Setting and Sample Population

We partnered with a large consumer finance lender in India to conduct a natural field experiment (Harrison and List, 2004), in which the lender extended randomized debt forbearance offers to its customers. Our partner is one of India’s leading consumer finance lenders, has over 35 million individual customers across the country and offers a range of consumer financial products. Within the lender’s customer population, we focus on borrowers that had taken out a consumer loan to purchase a mobile phone. At the time of our experiment over 13 million loans of this type were outstanding, and we focus on loans that had missed at least two monthly payments. The loans in our sample have a mean (median) tenor of 7.8 months (8 months), a median amount of Rs 10,955 (US \$150)¹³, and a constant interest rate of 12 percent APR, which is similar to the characteristics of the typical consumer loan in India.¹⁴ The loans are collateralized by the mobile phone, and the lender has the ability to remotely restrict the phone’s ability to use data or make outgoing calls in the event that a borrower misses their monthly payments. Importantly, borrowers in this population are likely to be contactable by phone, which allows us to implement a nationwide experiment that is integrated into the lender’s

¹³ We use the Jan 1, 2021 exchange rate of Rp 73 per US\$ for all currency conversions in this paper.

¹⁴ The median (average) consumer loan recorded in credit bureau data for this population has an amount of Rs 12,800 (Rs 15,000), a median (average) interest rate of Rs 16.5% (19.5%), and a median (average) tenor of 7 months (8.9 months).

regular interaction with its customers. The forbearance offers extended in our experiment are extended through the lender's standard communication channels, have real stakes, and are fully compliant with the terms of India's national debt moratorium policy in place at the time of our experiment.

3.2 Experimental Protocol

The main experiment was implemented between November 2020 and March 2021, after India's first national debt moratorium had ended and lenders were encouraged, but not required, by the regulator to offer additional debt forbearance to their customers. This allows us to extend real stakes debt forbearance offers that are truthfully framed as being granted either as an initiative of the lender or as a result of government policy.

The experiment was conducted in several rounds, corresponding to the lender's monthly repayment cycle. Each month, the lender shared a list of borrowers that had failed to make their required monthly loan payment. The size of each monthly borrower cohort was thus determined by the size of the lender's 30-day delinquency portfolio at this point in time. This sample was further restricted to borrowers that had been making regular payments prior to this date and whose loans had not previously been restructured or modified. Through this process, the lender shared a sample of 51,660 customers that qualified for debt forbearance according to its internal criteria. We excluded from this sample any borrowers who, if they accepted a forbearance offer, would not complete repayment of their loan by April 1, 2022 even with perfect repayment. In total, the estimation sample includes 9,623 borrowers that were successfully contacted over the course of the experiment. The sample includes customers from 26 states of mainland India (see Figure 2).

We worked with the lender to randomly assign customers in the sample frame to one of the debt forbearance offer or control treatments. Professional customer service representatives from the lender's call center then attempted to reach customers on an assigned list to extend a debt forbearance offer or deliver one of the control scripts. Each customer service representative was given a randomly assigned script and list of customers to contact at the beginning of each work day. The customer service representatives made up to four call attempts for each customer in the sample, and only customers who could be reached using this protocol are included in our balance tests and estimation sample. Call recordings were audited throughout the study to ensure that the lender's customer service representatives followed the experimental protocol and assigned scripts. In total, 16 customer service representatives made an average of 122 call attempts per day for a total of 88 days over the course of the experiment.

3.3 Treatment Conditions

Borrowers in the sample were randomly and individually assigned to one of four treatment conditions, which included two variations of a debt forbearance offer, one placebo treatment and one control condition. The two debt forbearance offers extended in the experiment are identical in all terms and conditions but vary whether the offer is presented as an initiative of the lender or the regulator. The remaining treatments shed light on alternative mechanisms and allow us to benchmark the impacts of

temporary debt forbearance against standard repayment reminders and debt enforcement.

3.3.1 Debt forbearance: relationship offer

The first of our two debt forbearance treatment conditions offered customers a three-month payment deferral on their loan that was framed as an initiative of the lender. In total, 11,703 borrowers were assigned to this treatment (1,901 contacted), which presented the offer as follows:

At [lender name], we care about our customers and truly value your business. We understand that people all over India are facing great hardships due to the current economic situation. Helping our valued customers in these challenging times is our first priority at [lender name]. That is why we are offering you repayment flexibility on your loan.

Borrowers assigned to this treatment were then informed about the terms of the offer and asked if they would like to enrol in the debt forbearance program, which required payment of a small one-time processing fee of Rs 350 through the lender's payment portal. Borrowers who accepted the offer were provided with additional information about the terms and conditions of the forbearance program as part of the sign-up process. To understand how sensitive take-up of the offer is to price, we assigned 1,968 customers (177 contacted) to receive the relationship offer with a higher processing fee of Rs 500.

3.3.2 Debt forbearance: regulator offer

The second of our debt forbearance treatments, which was assigned to a total of 10,962 borrowers (1,829 contacted), is identical to the first, with the exception that the offer is framed as being extended as the result of regulatory guidance, rather than the generosity of the lender. In this treatment, the offer was introduced as follows:

We are extending an offer in accordance with guidelines issued by the Reserve Bank of India to all lending institutions to help customers manage their finances in the current economic crisis. As a result of guidance issued by the Reserve Bank of India, we are offering you repayment flexibility on your loan, comparable to programs offered by most other lenders.

The customer service representative then informed borrowers assigned to this treatment about the terms of the offer, which were identical to those in the relationship offer treatment condition, and asked whether they would like to pay the processing fee of Rs 350 and enroll in the program.¹⁵ As in the relationship offer condition, borrowers who accepted the forbearance offer were provided with additional information about the terms and conditions as part of the sign-up process. Similarly, we assigned 1,246 of these customers (146 contacted) to receive the regulator forbearance offer at a slightly higher price Rs 500 to estimate how sensitive take-up of the offer is to the processing fee.

¹⁵ For the mean loan, this processing fee is equal to an internal rate of return (IRR) of 19%, making it a cheaper form of credit than that which these borrowers have access to externally.

3.3.3 Placebo call

In addition to the forbearance conditions, 9,326 customers (2,452 contacted) were assigned to the placebo call condition, which allows us to explore whether the response to receiving a forbearance offer could be explained by customers being treated in a more friendly way. In this treatment condition, customers received a friendly call in which a customer service representative highlighted the lender's commitment to its customers using a script similar to the relationship offer condition. However, in the relationship placebo condition no forbearance offer is extended and customers are instead simply invited to access to informational resources and offered help with navigating different payment methods.

3.3.4 Control group: repayment reminder

Finally, 13,587 customers (3,441 contacted) were assigned to the control group, which was designed to replicate the lender's standard repayment reminder calls. Customers assigned to this condition received a call from a customer service representative who reminded them that their payment was past due, and asked the customer to commit to a time frame for making the payment as per the lender's standard practice. This treatment serves as the main control group in our analysis.

3.4 Data and Descriptive Statistics

To measure the impact of debt forbearance on subsequent repayment behavior and borrower outcomes, we combine data from the experiment with administrative data from the lender, as well as the detailed pre- and post-intervention credit history of all borrowers in our sample that are covered by the Indian credit bureau.¹⁶

3.4.1 Administrative Data

In preparation for the experiment, the lender shared administrative data on customer demographics, loan characteristics, as well as data on prior borrowing and repayment from the lender's data and credit bureau information at the time of loan application, for all customers in the sample frame.

We first obtained information on borrower and loan characteristics as well the full repayment records of all borrowers in the sample from the lender. We aggregate these data to monthly frequency, corresponding to the monthly repayment cycles and construct a dummy variable indicating whether a borrower has made their loan payment by the required due date. The lender additionally shared data on the origination and end date of each loan, from which we construct an indicator variable for loans that are repaid in full and on time.

In a second step, we merged the loan and repayment data with the complete credit history of all borrowers that had a record in the Indian credit bureau. The credit bureau data was accessed after

¹⁶ The Indian credit bureau, TransUnion CIBIL, was founded in 2000 and currently covers approximately 63 percent of adults in India (World Bank, 2022). Consumer credit scores in India range from 300 to 900, with scores between 700-749 labeled 'good' and scores above 750 labeled 'excellent'. In our sample, 33 percent of customers have a credit score, and 20 percent of borrowers have more than one loan outstanding.

the experiment had concluded and includes the borrower’s credit score at the time of loan origination as well as a monthly record of all loans and loan payments for each borrower. Each monthly record includes the amount of the loan, the type of the loan, the date the loan was disbursed, and the days-past-due of payments on each loan for the last 36 months. We use the credit bureau data to construct indicators of monthly loan repayment and delinquency across the credit market.

Table 1 reports summary statistics at baseline and tests of randomization balance for all contacted customers that are part of our estimation sample. Table 1, column (1), shows summary statistics for all contacted customers. The average customer in our sample is male, 33 years old, has an income of Rs 40,000 (US\$ 528), and a loan of Rs 11,810 (US\$162) with a monthly payment of Rs 1,450 (US\$ 21) outstanding. On average, borrowers in the sample had taken out 0.38 prior loans recorded in credit bureau data. However, a substantial fraction of borrowers (67 percent) has no credit record.¹⁷ The credit scores of borrowers for whom a record was available are relatively high at 731 out of 800. Table 1 columns (2) to (5) report summary statistics for the same baseline borrower and loan characteristics for each treatment condition separately, and Table 1, column (6) reports a test of equality of means across treatment groups, which confirms that the randomization was successful.

3.4.2 Measurement of Repayment Outcomes and Borrower Beliefs

The main outcomes of interest in the debt forbearance experiment are loan repayment, as observed in the lender’s administrative data and credit bureau records for all borrowers in the sample, and beliefs about credit enforcement the likelihood of future forbearance. In this section, we describe the measurement of each of these outcomes in turn.

Loan repayment. We observe repayment of the loans covered by moratorium offers in our experiment in administrative data provided by the lender. Based on this data, we construct two outcome variables: *loan repaid* and *monthly payment made*. Figure A.1 in the Supplementary Appendix shows a stylized timeline of loan repayment and the debt moratorium offers extended as part of our experiment.

Our primary measure of loan repayment is the variable *loan repaid*, which is an indicator equal to one if a loan has been repaid in full. The lender measures repayment one day after a borrower is scheduled to make their final monthly payment. If the lender receives this payment the loan is closed and reported as repaid to the credit bureau. Because borrowers who accept one of the moratorium offers in our experiment not only pause payments, but also have the tenor of their loan extended by three months, we measure repayment for all loans three months after the initial due date of the last payment for each loan. This is a conservative approach which might underestimate the true effect of moratorium offers on loan repayment, given that repayment of loans that opt into the moratorium is measured at the end date while repayment of all other loans additionally takes into account payments made in the three months after the original end date of the loan, thus potentially overstating repayment for borrowers in the control.¹⁸ We construct the outcome *loan repaid* for loans in the experiment using

¹⁷ Not all financial institutions, especially microlenders, in India report to the credit bureau. Some borrowers without a credit history may therefore have previously had a formal loan that is not recorded in credit bureau data.

¹⁸ We conduct a number of robustness checks with alternative definitions of loan repayment and find that these have no material impact on our results. The results of this exercise are available upon request.

the lender's administrative records and for all loans from other lenders that appear in credit bureau data of borrowers in our sample.

The second measure of loan repayment we construct, *monthly payment made*, is an indicator that is equal to one if a borrower makes their monthly loan payment in full and on time in any given month that their loan is active. Because we want to compare repayment for loans at the same stage of repayment, all regressions in which the outcome is monthly repayment include months-since-treatment effects, which do not increment for loans that opt into the forbearance offer while the moratorium is in effect. In practical terms, this means that monthly repayment for a loan that is one month past the end of the moratorium is compared to a control group loan that is one month past treatment. As with loan repayment, we construct the variable *monthly payment made* for loans that are part of our experiment, as well as for all loans from other lenders that are recorded in credit bureau data.

Borrower beliefs. The second group of outcomes we use in our analysis are measures of borrower beliefs. Examining the impact of moratorium offers on borrower beliefs allows us to test for the precise mechanisms through which forbearance can give rise to moral hazard. Even if the impacts of forbearance on actual loan repayment are small, a necessary condition to establish the presence of moral hazard in response to debt forbearance is that moratorium offers generate expectations of lenient credit enforcement or future forbearance.

The main survey we use for this purpose is an incentivized elicitation exercise in which borrowers who received one of our experimental treatments are asked whether they expect banks or the regulator to offer another moratorium in the event of a future crisis. Specifically, we asked a sample of 645 borrowers who had participated the experiment, whether they expected another debt moratorium to be extended by private lenders or the government in the event that India would experience another wave of the pandemic in the next 12 months as severe as the one that had triggered the initial round of debt forbearance measures.¹⁹ Borrowers were incentivized with a lottery in which they could win a cash prize of Rs 5,000 (US\$ 68) if their prediction was correct.²⁰

To measure expectations about lenient credit enforcement in the future, we additionally asked a sample of 259 borrowers who had participated in our experiment whether they expected credit enforcement to be more lenient, less lenient or about the same in the next 12 months. Since, in contrast to a debt moratorium, there is no clear trigger event for lenient credit enforcement, only the two questions about expectations of future forbearance were incentivized.

3.4.3 Descriptives: take-up of moratorium offers

Participation in debt moratoria is usually optional, so that borrowers with urgent liquidity needs can "draw" on payment deferrals if needed. In our empirical analysis, we thus rely entirely on intent-to-treat (ITT) effects. That is, we examine the effect of *receiving* a forbearance offer, rather than the effect of selecting into the offer. Nonetheless, understanding the demand for forbearance is of independent

¹⁹ Borrowers were randomly assigned to either the question eliciting incentivized expectations about a moratorium issued by banks (320 respondents) or a moratorium issued by the regulator (325 respondents).

²⁰ The question was worded as follows: "If there is another spike in Covid-19 cases with more 200,000 cases per day within the next 12 months, how likely do you think it is that the Government of India will grant another debt repayment moratorium to borrowers, similar to those that were offered in the past?"

interest. In this section, we therefore report descriptive statistics on acceptance of forbearance offers before moving to the analysis of the debt forbearance experiment.

Table B.3 in the Supplementary Appendix reports raw take-up rates for the relationship and regulator offer conditions. Overall, 13 percent of contacted customers accepted the forbearance offer and paid the processing fee to have their payment schedule adapted. At 12.5 percent versus 13.5 percent there is no statistically or economically meaningful difference in acceptance rates between the regulator and relationship offers and Table B.4 reports a test of balanced covariates, which suggests that there was no differential selection into forbearance along observable loan and borrower characteristics. The forbearance rates in our experiment are comparable to those observed in similar programs. [Cherry et al. \(2021\)](#), for example, report that in the U.S. only 10 percent of borrowers opted into forbearance in the case of mortgages eligible for repayment deferrals under the CARES Act.

Finally, Table B.5 in the Supplementary Appendix explores which borrower and loan characteristics predict acceptance of forbearance offers. The results indicate that, consistent with a liquidity constraints explanation, the size of customers' monthly payments is one of the main predictors of offer take-up across forbearance treatment conditions. One interesting difference in the predictors of take-up is that borrowers with lower baseline credit risk, as measured by prior credit delinquency, are more likely to select into the relationship offer ($b=-0.112$, $s.e.=0.033$), whereas prior delinquency does not predict acceptance in the regulator offer treatment condition.

We additionally test how price sensitive customers are with respect to the processing or restructuring fees that are typically charged to enroll in a forbearance offer. In our experiment, the baseline processing fee was Rs 350 (approximately 3 percent of the median loan amount). We tested the effect of increasing this fee to Rs 500 with a subset of customers assigned to the relationship and regulator offer treatment conditions. Table B.6 in the Supplementary Appendix reports the results. We find that borrowers in our setting are quite price sensitive: the acceptance rate of the offer drops by nearly half at the higher processing fee with an implied price elasticity of 1.2.

3.5 Empirical Specification and Results

We estimate treatment effects of debt forbearance on borrower behavior and expectations using regression equations of the general form:

$$Y_i = \phi_1 T_1 + \dots \phi_3 T_3 + \gamma X_i + \varepsilon_i \quad (1)$$

where Y_i is an outcome of interest for individual i , $T_1 \dots T_3$ are dummy variables for the three experimental treatment conditions, X_i is a matrix of controls, and ε_i is a stochastic error term.²¹ The omitted group throughout our analysis is the repayment reminder condition, in which customers received a call with the lender's standard repayment request. Regressions are estimated using the sample of all clients who received a forbearance offer, so that the parameters ϕ_k should be interpreted as intent-to-treat (ITT) effects. That is, the treatment effect coefficients measure the effect of receiving a

²¹ Our main results are unchanged when we use heteroskedasticity-robust standard errors, or cluster-robust standard errors at the level of the caller-treatment-day.

forbearance offer, rather than the effect of accepting the offer.

3.5.1 Loan Repayment

We begin by examining the effect of debt forbearance offers on loan repayment. As reflected in our survey of beliefs about debt relief in the population, reported in Section 2, there is widespread concern that granting repayment flexibility will create moral hazard in loan repayment by damaging the reputation of the lender or generating expectations of future relief. We test this hypothesis by examining whether receiving a debt forbearance offer reduces the likelihood that borrowers repay their loans fully and on time. In addition, we test whether the repayment response differs when the repayment moratorium is framed as an initiative of the lender or the regulator. Intuitively, if granting repayment flexibility during a crisis damages the reputation of the lender or generates expectations of lenient credit enforcement in the future, one would expect this effect to be *less* pronounced if forbearance is presented as an initiative of the regulator rather than the lender.

We use the lender's administrative data on loan repayment to construct a panel dataset on monthly loan repayment and amounts outstanding for all customers in our sample, as well as a dummy variable indicating whether a loan was ultimately repaid. We compare the repayment status of loans whose end date was extended by three months as a result of the offer with loans that received one of our treatments at the end date of loans in the forbearance offer treatment group. Note that this provides a lower bound estimate of the treatment effect, given that loans in the control group could have been repaid in the three month window by which loans in the forbearance groups were extended.

Table 2, reports the results. We find that none of the forbearance offers extended as part of our experiment had a negative impact on loan repayment. We find no impact of forbearance offers on loan repayment when the offer is framed as an initiative of the regulator ($b=0.003$, $s.e.=0.015$). By contrast, we find that forbearance offers *improve* loan repayment when the offer is presented as an initiative of the lender. The likelihood of loan default is 4 percentage points (6.9 percent) lower in the relationship offer treatment condition ($b=0.041$, $s.e.=0.015$) and the difference between the two offers is statistically significant at the 5 percent level ($b=0.037$, $s.e.=0.015$).

Turning to the effects on monthly repayment, in Table 2, column (2), we find that the relationship offer also increases the probability that a borrower makes a given monthly payment in full and on time. This effect is smaller in magnitude ($b=0.008$, $s.e.=0.005$), but again significantly different between the relationship and regulator offer treatment conditions ($b=0.012$, $s.e.=0.005$). These results indicate that, contrary to the hypothesis that debt moratoria give rise to moral hazard by generating expectations of lenient credit enforcement or future forbearance, neither of the debt moratorium offers extended in our setting had a negative effect on credit discipline. On the contrary, when debt forbearance offers are granted by the lender they have a positive and economically meaningful effect on loan repayment.

In Table 3, we turn to the effect of forbearance on borrower expectations. The dependent variables in the table are responses to a survey designed to test for expectations of future forbearance and lenient debt enforcement, the two principal channels through which one would expect debt moratoria to generate moral hazard. We show that, consistent with the absence of moral hazard in response to forbearance offers, repayment deferrals have no effect on borrower expectations.

In Table 3, columns (1) and (2), we first report treatment effects of debt moratorium offers on expectations of future forbearance, elicited through an incentivized survey exercise as described in Section 3.4. The point estimates are negative for both the relationship and regulator offers, suggesting that borrowers believe that receiving a debt forbearance offer in fact makes it slightly *less* likely to be offered repayment flexibility in the future from either the government or a commercial bank. In Table 3, column (3), we report treatment effects on expectations of lenient credit enforcement, using responses from a non-incentivized elicitation exercise. The point estimates are positive but close to zero, especially for the relationship offer which one would expect to have the largest impact on expectations of lenient enforcement ($b=0.049$ $s.e.=0.221$). Taken together, the results indicate that receiving a debt forbearance offer does not lead borrowers to expect additional forbearance or more lenient credit enforcement in the future. This is consistent with the absence of a negative effect of forbearance offers on loan repayment in both the regulator and relationship forbearance offer treatment conditions.

3.5.2 Overall Credit Discipline

We next examine the impact of debt performance on overall credit discipline. The results in the previous section show that none of the debt moratorium offers issued in our experiment generate moral hazard in loan repayment and, on the contrary, improve loan repayment when the lender is allowed to take credit for the forbearance offer. A natural question to ask is whether this represents an overall improvement in credit discipline or whether the improved repayment of loans covered by a moratorium offer comes at the cost of higher delinquency and default on other loans.

Intuitively, three scenarios are possible. First, it could be that moratorium offers improve repayment of the loan covered by the moratorium but do not affect the repayment of other loans leading to a net improvement of loan performance. Second, borrowers might prioritize the repayment of loans covered by a moratorium but still allocate a fixed amount to debt repayment each month, so that overall loan repayment remains unchanged. Finally, it is possible that prioritizing repayment of a loan covered by a moratorium causes borrowers to miss payments on more than one other loan, thus creating negative spillover effects and worsening overall loan repayment.

We examine the impact of debt forbearance offers on overall loan repayment using repayment-level data from the Indian credit bureau. For each borrower with a credit record, the data contain information on all active and closed lines of credit, with detailed data on each scheduled payment. Specifically, in the case of an active loan, the data contain the amount outstanding, the remaining number of payments and record each month whether the loan payment has been made in full and on time. We use this data to construct the month-level and loan-level outcomes *paid lender*, which is an indicator equal to one if a customer made their loan payment in full and on time or repaid their loan, *didn't pay others* which is an indicator equal to one if a customer missed a loan payment or defaulted on at least one other loan, *prioritized lender*, which is an indicator equal to one if the borrower made a loan payment or repaid a loan covered by the moratorium but remained delinquent on at least one other loan, and *share loans repaid* which is the number of monthly loan payments made or loans repaid as a share of total loan payments due in a given month or total loans outstanding, respectively. We construct these variables based on the sample of 9,496 customers in the sample that have a credit

history and use them to analyze the effect of our experimental treatments on overall loan repayment.

Table 4 reports treatment effect estimates of loan forbearance on all loans, including those from other lenders in the marketplace. In columns (1) and (5) we replicate our main result in this sample using the same outcomes as in Table 4. Borrowers assigned to the relationship offer treatment condition are only marginally more likely to make a given loan payment in full and on time, but they are 4.1 percentage points less likely to default than borrowers in the control group ($b=0.041$, $s.e.=0.015$), while neither the regulator offer nor the placebo condition affect loan repayment. Table 4, columns (2) and (6) report treatment effects on the repayment of loans other than those covered by the moratorium offer and show that neither the relationship offer nor the regulator offer affect the repayment of other loans. Table 4, columns (4) and (8) examine treatment effects on overall credit discipline and consider the share of monthly loan payments made in full and on time, and the share of total loans repaid in full and on time. The results show that the treatments on average have no positive effect on the likelihood that a customer makes all of their monthly payments on time. In fact, we find an economically small negative effect of the regulator offer on monthly repayment ($b=-0.014$, $s.e.=0.006$). However, as the results in Table 4, column (8) illustrate, receiving a moratorium offer framed as an initiative of one's lender significantly improves repayment and reduces loan default ($b=0.034$, $s.e.=0.014$). This improvement in overall credit discipline is entirely explained by the reduced likelihood of default on loans from the lender that offered repayment flexibility. The result that repayment of other loans is entirely unaffected by the relationship offer treatment also indicates that borrowers who prioritize the repayment of a loan covered by the moratorium offer are able to mobilize additional resources to make these payments, possibly by reducing savings or consumption.

Taken together, the results indicate that forbearance offers have no negative effects on the repayment of other loans, and that the observed improvement in overall credit discipline is explained entirely by improved repayment among borrowers in the relationship offer treatment condition.

3.6 Interpreting the Results: *Why* do Forbearance Offers Affect Loan Repayment?

In this section, we attempt to distinguish between alternative mechanisms that can explain why loan repayment improves in response to moratorium offers presented as an initiative of the lender.

The finding that borrowers prioritize payments to a lender that offered them repayment flexibility in times of financial distress is a priori consistent with a number of *strategic* and *non-strategic* motivations. One possibility is that forbearance offers affect repayment through a relaxation in financial constraints and improvements in recipients *ability to repay*. Alternatively, there are a number of mechanisms that could explain why borrowers exhibit a higher *willingness to repay* after receiving an offer from their lender. On the one hand, it could be that forbearance offers increases loan repayment because of a purely non-strategic sense of reciprocity. That is, borrowers might be grateful that their lender offered them help in difficult times and wish to return the favor by prioritizing payments to the lender after the moratorium without any expectation of future benefits from the lending relationship. On the one hand, it could be that the offers remind borrowers of the value of their relationship with the lender and highlight the importance of timely loan repayment as a signal to the lender and an investment in a continued lending relationship.

To understand which of these interpretations is most consistent with our findings, we show two sets of results. We first present results from an elicitation exercise designed to understand how borrowers in our experiment interpreted the regulator and relationship offers. Second, we examine which types of borrowers exhibit the strongest repayment response to the relationship offer.

3.6.1 Impacts on financial constraints

We first examine whether the improvement in loan repayment in response to the relationship offer could be explained by a relaxation of financial constraints and differential changes in the *ability*, rather than the willingness to repay as a result of the forbearance offers. There are several pieces of evidence that allow us to rule this out. First, note that the terms of the relationship and regulator offers are identical and that, therefore, the two offers have the same effect on a borrower's finances, if taken up. However, as Table 2 shows, repayment improves only in the relationship offer treatment condition.

Second, we show in Tables B.3 and B.4 in the Supplementary Appendix, that there is no difference in take-up between the two forbearance offers (13 percent versus 12 percent, p-value 0.47), and both financial characteristics and borrower demographics are balanced across borrowers in the two treatment groups that accept the offer. Hence, the results are also not explained by differential take-up of forbearance offers across treatment conditions.

Third, we note that our main result is an intent-to-treat estimate (which measures the effect of receiving an offer, rather than the effect of accepting the offer). In the Supplementary Appendix we show that improvements in loan repayment are concentrated among borrowers who received the relationship offer but did not take it up. In other words, our results suggest that merely receiving the relationship offer reduces the likelihood of loan default, regardless of whether the borrower accepts the offer and benefits from a temporary relaxation of financial constraints or not.²²

Taken together, this provides evidence that the improvement in repayment in response to forbearance offers framed as an initiative of the lender is not explained by the temporary relaxation of credit constraints and differential changes in the ability to repay as a result of accepting the moratorium.

3.6.2 How do borrowers interpret the offer?

Having ruled out that forbearance offers impact repayment through a change in the ability to repay, we next explore alternative channels through which the offers could affect the *willingness* to repay.

To do so, we first present evidence from a survey designed to understand how borrowers interpret the forbearance offers extended in our experiment (first-order beliefs) and what they expect the lender to infer from their response to the offer (second-order beliefs). The results are informative about alternative signaling interpretations of our main result. We collected this information using a follow-up survey with 190 borrowers who had received either the regulator or relationship forbearance offer. In the survey, respondents were asked (i) how they thought recipients of the offer had been chosen, (ii)

²² To show this empirically, Table B.14 in the Supplementary Appendix reports results of a simple prediction exercise that splits the sample based on the predicted likelihood of take-up at baseline and estimates treatment effects for the borrowers that are most and least likely to accept the debt moratorium offer. The results show that the treatment effect is in fact concentrated among borrowers with a below-median probability of accepting the offer.

what they thought their response to the offer would signal to the lender and (iii) what they thought was the main rationale for timely loan repayment.

Table 5 reports the results of this exercise by comparing responses of experimental participants assigned to the regulator and relationship offer treatment conditions, respectively. We first asked respondents about how they thought recipients of the offers were selected. Respondents in the regulator offer group were 11 percentage points (28 percent) more likely to respond that they thought offers were issued to *all borrowers* than respondents in the relationship offer group, who were 14 percentage points (48 percent) more likely to believe that forbearance offers were issued only to the lender's *most creditworthy* borrowers. This makes it unlikely that loan repayment improves because customers interpret a forbearance offer from the lender as a signal that they have been singled out as a troubled borrower and overcompensate by prioritizing payments to the lender to prove their creditworthiness. Contrary to this specific mechanism, borrowers in the relationship offer treatment assume that the lender would extend repayment deferrals only to its most creditworthy customers.

We next asked borrowers how they thought accepting the offer would affect their creditworthiness in the eyes of the lender. We find that borrowers in the relationship offer condition are 14 percentage points (82 percent) more likely to believe that accepting a forbearance offer is not interpreted as a signal of creditworthiness by the lender than borrowers in the regulator offer condition. This is consistent with the first-order belief that recipients of the relationship offer were selected from the lender's most creditworthy customers, so that—in this relatively homogeneous group—there is little additional information to be gained from observing whether a borrower accepts or declines the offer.

Finally, we asked borrowers in the relationship and forbearance offer conditions about their main motivation for timely loan repayment. In line with the interpretation that a forbearance offer issued explicitly by one's lender reminds borrowers about the value of a continued relationship with the lender, borrowers in the relationship offer treatment group are 18 percentage points more likely to respond that their primary motivation for timely repayment is the possibility of obtaining future credit from the lender. We can distinguish this channel from the possibility that the relationship offer serves as a reminder about the general benefits of timely debt repayment beyond the relationship with one specific lender: borrowers in the relationship offer condition are slightly less likely than borrowers in the regulator offer condition to state that their main motivation for timely loan repayment is to maintain a good credit score and obtain loans from other lenders in the future.

Overall, the survey evidence is most consistent with the interpretation that—rather than triggering a sense of pure, non-strategic, reciprocity—the forbearance offers remind borrowers of the value of a continued relationship with the lender and improve loan repayment because customers wish to signal their creditworthiness and safeguard the option of doing business with the lender in the future. While the survey data have a number of limitations and should be interpreted with caution, the pattern of results is overall inconsistent with pure, non-strategic reciprocity as an explanation for improved loan repayment in response to forbearance offers extended by one's lender.

3.6.3 Whose repayment responds to a forbearance offer?

We next examine which loan and borrower characteristics are associated with the largest repayment increases in response to the relationship offer. That is, we ask which types of borrowers are most eager to send a signal of creditworthiness to their lender after receiving a forbearance offer from their lender. We find that the borrowers who show the strongest improvements in repayment are those for whom the offer is most immediately valuable. This includes borrowers who suffered larger income losses as a result of the crisis, and borrowers who are especially concerned about maintaining a good credit score.

To examine treatment effect heterogeneity, we use the machine learning approach developed by Chernozhukov et al. (2018). This approach is specifically designed to provide uniform inference when there are many dimensions along which treatment effects may be heterogeneous, which is often the case in randomized experiments. When there are many potential dimensions of heterogeneity, inference typically suffers from problems of over-fitting and multiple hypothesis testing. Chernozhukov et al. (2018) solve this problem by constructing sample splits based on the combination of all dimensions of heterogeneity simultaneously. Specifically, the algorithm sorts the data into quintiles $\gamma_1 \dots \gamma_5$, using cross-validated machine learning to maximize the ability of characteristics Z to predict the conditional average treatment effect.²³ It then provides a single p -value which tests whether the Group Average Treatment Effects (GATES) of the most-affected group γ_5 and least-affected group γ_1 are significantly different. By consolidating the information content of all variables simultaneously into a single p -value, this avoids inference problems created by multiple hypothesis testing. In addition to the group treatment effects, the method additionally produces a Classification Analysis (CLAN), which summarises the characteristics of the most affected and least affected groups γ_5 and γ_1 , and tests whether each characteristic is statistically different in the most affected and least affected groups.²⁴

Table 6 reports the results of this analysis. We consider both of our main repayment outcomes; whether a customer makes a given monthly loan payment on time (columns 1 to 4) and whether a customer repays or defaults on their loan (columns 5 to 8). At the loan level, the GATES analysis finds an estimated treatment effect of 0.074 on the most-affected quintile γ_5 , nearly double the main effect on the overall population. The estimated treatment effect in the least-affected quintile γ_1 , on the other hand, is zero. Because of sample size limitations we lack sufficient statistical power in the loan level analysis to reject the null hypothesis that the treatment effect is homogeneous across the population. However, we find both stronger heterogeneity and have sufficient statistical power to reject homogeneous treatment effects with a p -value of less than .001 in the analysis of monthly repayment.

In both analyses, we find very similar results in terms of the loan and borrower characteristics along which treatment effects are heterogeneous. We first inspect heterogeneity by loan characteristics and find that the effect of the relationship offer on repayment is larger for borrowers with larger loans, and shorter remaining loan durations. Turning to heterogeneity among borrower characteristics, the

²³ This approach is a general framework which can incorporate many different machine learning methods. We use a random forest with 1,000 trees of 100 elements and two folds of cross-validation, following Breza et al. (2020) who show that these parameters work well in their setting which is similar to ours.

²⁴ Note that this method cannot test whether any one characteristic is responsible for the heterogeneity in treatment effects, given that there is no exogenous variation in characteristics. For example, the CLAN analysis may reveal that the most-affected group have higher incomes and larger loans than the least-affected group. It is not possible to determine which of these two correlated variables is responsible for the heterogeneity.

results show that the treatment effect is larger for borrowers with lower initial credit scores and a shorter credit history, borrowers who suffered especially large income losses during the pandemic, and borrowers who are especially concerned about maintaining a good credit score.

Both sets of results are consistent with the interpretation that borrowers for whom the repayment offer is immediately valuable, for example, borrowers who have larger loans outstanding, borrowers who suffered especially large income losses, or borrowers who are worried that their temporary inability to repay will tarnish their credit score, are more responsive to the relationship offer. Finally, the treatment effect of the relationship offer on loan repayment is also larger for borrowers in the who state that they would not expect banks to help customers who are facing difficulties making their payments. In other words, borrowers respond more strongly if they are surprised by an offer of repayment flexibility from their lender.²⁵

4 Debt Moratoria and Banking Relationships: The Product Adoption Experiment

In this section, we examine whether debt forbearance offers that are perceived as an act of generosity by the lender can strengthen lending relationships in the longer term. Intuitively, it is possible that customers interpret an offer of repayment flexibility as an investment in the relationship and reciprocate by choosing to do business with the bank that offered them forbearance in difficult times. If this mechanism is at play, offering repayment flexibility could be a profitable strategy, because it allows the bank to obtain soft information about its customers, identify the most creditworthy borrowers and tie them into longer-term lending relationships. On the other hand, borrowers could interpret a forbearance offer from the lender as a signal that it is easy to renegotiate with the lender, which could cause borrowers to select into continued lending relationships with the bank for strategic reasons and lower the profitability of banking relationships.

To examine the impact of debt forbearance offers on banking relationships, we conducted a complementary product adoption experiment that is designed to distinguish between these alternative mechanisms. Specifically, the experiment tests whether customers who were offered repayment deferrals framed as an initiative of the lender subsequently have higher demand for the lender’s products overall and, if so, whether there is a difference between the demand for a regular consumer loan, whose terms can—at least in principle—be renegotiated ex post, and a *non-credit financial product* that involves a one-time transaction and has no scope for renegotiation.

²⁵ In Appendix Table B.10, we supplement this analysis with a conventional heterogeneous treatment effect analysis. Specifically, for each variable in the vector of borrower and loan characteristics Z_i considered in the machine learning analysis, we construct an indicator $I(Z_i) = 1[Z_i > Z_{median}]$ equal to 1 if borrower i has an above-median value of Z_i and zero otherwise. We then estimate variations of equation (1) that include interactions between the relationship offer treatment dummy T_i^1 and the loan and borrower characteristics indicators $I(Z_i)$ (along with both terms uninteracted). While specific estimates naturally differ between the two exercises, some of the main characteristics along which treatment effects are heterogeneous are similar in the two exercises, such as the remaining duration of the loan, and the extent to which borrowers care about maintaining a good credit score. Because some borrower characteristics used in the heterogeneity analysis are elicited in an endline survey, several months after participants received one of the treatment or control messages, we verify that the treatments have no effect on survey responses. The results, reported in Appendix Table B.11, show that there are no treatment effects on any of the survey outcomes.

In the experiment, we contacted a randomly chosen subset of 1,160 borrowers who had received one of the two moratorium offers in the main experiment and randomly assigned them to receive a marketing call that offered them the opportunity to apply for a consumer loan or the opportunity to purchase a non-credit product that we designed in collaboration with the lender for the purpose of the experiment. Within each type of offer, the experimental treatments varied, whether the financial product was advertised as a product of the lender that had granted the moratorium or presented without any specific branding, so that the customer learned the identity of the lender only after accepting the offer. We use this simple experimental design to test, whether borrowers who received a moratorium offer from their lender have causally higher demand for the lender’s products.

4.1 The financial products

To ensure that our experiment is able to separately identify demand for the lender’s products stemming from consumer inferences about the ability to renegotiate financial contracts from demand due to non-strategic reasons such as reciprocity, the product adoption experiment is designed as a one-shot game in which consumers are either offered opportunity to apply for a non-collateralized consumer loan or to purchase a non-credit product which is sold at a fixed price, whose terms cannot be modified ex post, and which does not require repeated interaction with the lender. If moral hazard is at play, it would operate through the first channel described above: borrowers who believe it is more likely the lender will grant them relief will have higher demand for ex post renegotiatble loans, but not for the non-credit product (which has fixed price and does not involve repeated interaction with the lender).

Consumer loans. The consumer loan offered in the experiment is a standard Rp 30,000 (US\$ 411) uncollateralized personal loan with an annualized interest rate of 28 percent APR. These loans are paid off in monthly installments, have a tenor of 12 months, and are very similar to the lender’s most common consumer loan product. The lender’s standard procedure for offering these loans is to check a borrower’s credit history and then issue loan offers only to pre-approved customers. Because all of the borrowers in our sample have been delinquent at least once and would therefore be likely to fail the lender’s credit check, the implementation our experiment uses a slight modification to the lender’s usual procedure. Instead of pre-qualifying borrowers, we extended an offer to apply for the loan to a random sample of 528 borrowers that had participated in our experiment. The lender then evaluated the loan applications and extended a loan to the most creditworthy borrowers in this population to ensure that the exercise has real stakes. We design two variations of the loan offer, one “branded” version, in which the marketing call for the loan prominently mentions the name of the lender and one “unbranded” version, in which the identity of the lender is only revealed once a customer accepts the offer and follows the link to submit their application.²⁶

Non-credit product. In addition, we worked with the lender to design a non-credit product that is similar to the products routinely offered by consumer lenders in India but—in contrast to a loan—has no scope for ex post renegotiation. Specifically, we adapted a credit information product that explains

²⁶ The regulator requires the name of the lender to appear on the loan application. We therefore measure demand at the end of the marketing call by asking the customer whether they would like to apply for the loan. That is, *before* the identity of the lender is revealed to all applicants, including those in the “unbranded offer” control group.

the individual components of a borrower's credit report and provides actionable advice on how a borrower can improve their credit score. These customized "credit intelligence reports" were available in the Indian market prior to our experiment. They are generated by a third party firm that has agreements with many banks in India, which customize these reports and offer them to their clients for a fee. We worked with the lender and the third party firm to design two versions of these credit reports: one "branded" version of the report which is clearly identified as a product of the lender in the marketing call and the accompanying sign-up link and prominently displays the lender's name and branding and one "unbranded" version, which contains identical information but is not identified as a product of the lender in the marketing call or in the sign-up. In this second variation of the offer, the name of the lender is only revealed once a customer accepts the offer and is asked to make a payment.

4.2 Experimental protocol and design

4.2.1 Sample

To implement the product adoption experiment, the lender's sales representatives attempted to reach a sample of 3,806 customers who had received the *relationship offer* or *regulator offer* offer in the main experiment. The marketing calls took place between six months and one year after customers had received the initial offer. In total, we reached 1,160 customers and extended 528 personal loan offers and 632 offers for the non-credit product.

4.2.2 Treatment conditions

The customers in our sample were randomly assigned to one of two "cross-sell" treatment conditions, which varied whether the customer was offered a *branded* version of the product offer, which marketed the product using the lender's name and branding, and an *unbranded* version that was identical to the branded offer but was not identified with the lender.

In the unbranded offer treatments, each product was introduced with a neutral script that did not mention the lender's name. In the case of the consumer loan, the product was introduced as follows:

We are calling because we would like to invite you to apply for our new [name of product] personal loan. This convenient loan in the amount of Rs 30,000 can be used for your personal consumption or cash needs.

Similarly, the unbranded script for the non-credit product introduced the credit intelligence reports without reference to the name of the lender:

We are calling to offer you our new [product name] financial health report. This report will give you a clear overview over your credit profile that is easy to understand.

In the branded offer treatment conditions, the sales representatives used scripts that were identical, with the exception that they emphasized that the sales representative was extending the offer on behalf of the lender. The branded script for the personal loan introduced the product as follows:

We are calling from [name of lender], from whom you have previously taken out a loan. As a valued [name of lender] customer we would like to invite you to apply for our new [name of product] personal loan. This convenient loan in the amount of Rs 30,000 can be used for your personal consumption or cash needs.

The branded offer for the non-credit product correspondingly introduced the credit intelligence reports with a prominent reference to the name of the lender:

We are calling from [lender name] from whom you have previously taken out a loan. As a valued [lender name] customer, we would like to offer you our new [product name] financial health report. This report will give you a clear overview over your credit profile that is easy to understand.

The sales representative then explained the details of each offer using a script that was identical for the two offers and asked the customer whether they were interested in applying for the consumer loan or purchasing the credit intelligence report. If the customer expressed interest, the sales representative sent them a payment link. Because the payment link must, for legal reasons, disclose the identity of the lender, the main outcome of interest in our analysis is whether a customer expresses interest in purchasing the product and requests a payment link.

4.3 Results: demand for the branded product

We find that customers who were offered repayment flexibility framed as an initiative of the lender in the main experiment are significantly more likely to purchase the branded version of the non-credit product, but do not have higher demand for personal loans marketed by the lender.²⁷

Figure 5 displays unconditional means of product take-up for all treatment combinations in the forbearance and product adoption experiments. Figure 5, Panel (a), reports take-up rates for branded and unbranded personal loans, respectively. The figure shows that demand for loans is overall high, but not statistically different across treatment combinations. In particular, there are no statistically or economically significant differences in demand for the branded and unbranded versions of the personal loan, and this result does not differ between borrowers that had received the relationship moratorium offer (p-value=0.240) or the regulator moratorium offer in the main experiment (p-value=0.690).

In Figure 5, Panel (b), we plot unconditional means for the non-credit product. The figure shows that borrowers who had been offered repayment flexibility by their lender were more than 10 percentage points more likely to take up the product when it was branded as a product of the lender than when they received a nondescript offer without reference to the lender (p-value=0.048), with take-up rates of 35.7 percent and 25.2 percent for the branded and unbranded offers, respectively. Moreover, take-up of the branded product among borrowers who were offered the moratorium by the lender was also about 10 percentage points higher (p-value=0.072) than take-up among those who were offered the forbearance by the regulator (26.1 percent). In contrast, there is no similar pattern of demand for the

²⁷ We limit our analysis to borrowers for whom no more than one year has elapsed between the debt forbearance and follow-up product offers. The effect is slightly weaker but still present among customers who receive a follow-up product offer from the lender after more than one year. Results for this population are available upon request.

branded product among borrowers who received debt forbearance offers framed as an initiative of the regulator in the main experiment. In fact, at 26.1 percent, take-up of the branded product is slightly lower than take-up of the unbranded product at 30.8 percent in this group (p -value=0.347). Similarly, demand for the unbranded product is not affected by the framing of the offer (p -value=0.258).

Table 7 presents the results of the product adoption experiment in regression format, without controls in column (1) and controlling for the full set of caller and offer date fixed effects for the forbearance and product adoption experiments in column (2). The difference between take-up rates of the branded product across forbearance conditions for the non-branded products is statistically significant in both specifications and slightly larger when we control for the full set of experimental conditions ($b=0.152$, $s.e.=0.073$ and $b=0.206$, $s.e.=0.100$). By contrast, there are no meaningful differences between demand for the loan product across the different treatment combinations.

4.4 Product adoption experiment: discussion

Overall, the results of the product adoption experiment show that borrowers who are offered repayment deferrals by their lender are more willing to deepen their banking relationship with the lender, and suggest that this demand for future interactions with the lender is not explained by strategic motives or expectations of lenient credit enforcement.

The design of the product adoption experiment allows us to rule out several competing explanations that could explain the pattern of our results. First, it could be that customers who are granted repayment flexibility by the lender when they were facing financial distress are more interested in deepening their relationship with the lender because because the offer causes them to infer that the lender will be more willing to renegotiate financial contracts in the future. This possibility is ruled out by the result that borrowers who were offered repayment flexibility by the lender do not have higher demand for personal loans, whose terms can in principle be renegotiated ex post, from the lender than borrowers who were not offered repayment flexibility. Second, it could be that forbearance offers generate demand for the non-credit product—an enhanced credit report—because forbearance offers prime customers to think about the importance of maintaining a good credit score. We can also rule out this channel, given that all participants of the product adoption experiment had received a debt forbearance offer in the main experiment, yet only customers in the relationship offer treatment exhibit higher demand for the lender's products in the product adoption experiment.

We report heterogeneous treatment effects for the product adoption exercise in Table B.13 in the Supplementary Appendix. While we cannot conclusively show which borrower characteristics are most predictive of demand for future interactions with the lender due to sample size limitations, the pattern of results from this exercise is broadly consistent with the analysis of heterogeneity in the loan repayment response to repayment flexibility.

Taken together, the results from our two complementary experiments suggest that, rather than generating moral hazard, repayment flexibility offered by one's lender can improve loan repayment in the short term and create higher-value banking relationships. Importantly, we show that the demand for future interactions with the lender is not explained by strategic motives that could create moral hazard in future interactions with the lender. This provides a channel through which forbearance

offers extended by one's lender—in addition to their short-term benefits—can lead to the formation of higher-value banking relationships in the longer term.

5 Conclusion

This paper uses a nationwide randomized experiment in India to estimate the effects of debt moratorium offers on borrower beliefs and loan repayment. Contrary to widely held assumptions, we find that none of the debt forbearance offers extended as part of our experiment change borrower beliefs in a way that could give rise to moral hazard. On the contrary, when lenders are able to take credit for granting repayment flexibility and moratorium offers are framed as an initiative of the lender, loan repayment improves substantially once the moratorium expires. We can rule out a number of confounding mechanisms, including reminder effects and the relaxation of financial constraints and additionally show that forbearance offers also have no negative spillover effects on the repayment of other loans. Using data on the universe of outstanding loans for borrowers in our sample, we show that the performance of other loans is unaffected, so that the overall impact of forbearance offers presented as an initiative in the lender also lead to a moderate improvement in overall credit discipline.

Moreover, our findings suggest a mechanism through which repayment deferrals presented as an initiative of one's lender improve loan performance. Consistent with the interpretation that the offer reminds borrowers about the value of a continued relationship with their lender, borrowers in the relationship offer group are significantly more likely to state that their main rationale for timely loan repayment is to preserve the option of doing business with the lender that granted repayment flexibility in the future, rather than achieving an overall improvement in their credit score.

We show that, in addition to the immediate benefit of improved loan repayment, granting repayment deferrals in a crisis can also strengthen banking relationships in the longer term. The results from our complementary product adoption experiment show that borrowers who are offered repayment flexibility by their lender have causally higher demand for the products of a lender that offered them forbearance in difficult times. Importantly, we find that borrowers who received a forbearance offer from their lender have differentially higher demand only for a *non-credit* product marketed by the lender while demand for personal loans is overall high but does not differ depending on what type of forbearance offer customers received in the main experiment. This suggests that demand for repeated interactions and a continued relationship with the lender is not explained by expectations of renegotiation of credit contracts or weak enforcement that could give rise to moral hazard in the future. On the contrary, forbearance presented as an initiative of the lender can generate soft information and help the lender tie its most creditworthy customers into more profitable longer term banking relationships.

Our results have a number of direct implications for the design of consumer debt moratoria. First, our results suggest that the moral hazard costs of debt forbearance may have been significantly overstated relative to the benefits of such policies for consumers, at least in our setting. In our experiment, neither a moratorium offered by the government, nor a moratorium offered by an individual bank has a negative effect on loan repayment. Second, our findings suggest that allowing lenders to take credit for offering forbearance during a crisis can be an efficient strategy. While forbearance offers do

not appear to damage credit enforcement and lender reputation, they can make customers prioritize payments to a lender that offered them repayment flexibility in times of financial distress.

There are a number of areas for future research that could help to more fully explore the implications of our findings for the design of forbearance policies. First, it would be useful to assess to what extent the response to forbearance offers we document in the market for consumer credit carries over to other loan types, such as larger collateralized loans. Second, it would be interesting to explore how the identity of the lender affects the response to forbearance offers. In our analysis we show that repayment improves most strongly for borrowers who are surprised by an offer of repayment flexibility from their bank. This would have important implications in emerging markets where state-owned banks are dominant and could imply different reactions to forbearance offers from lenders with a high degree of state-ownership that borrowers would rationally expect to be more lenient in a crisis. While these questions are beyond the scope of our paper, they represent a promising direction for future work.

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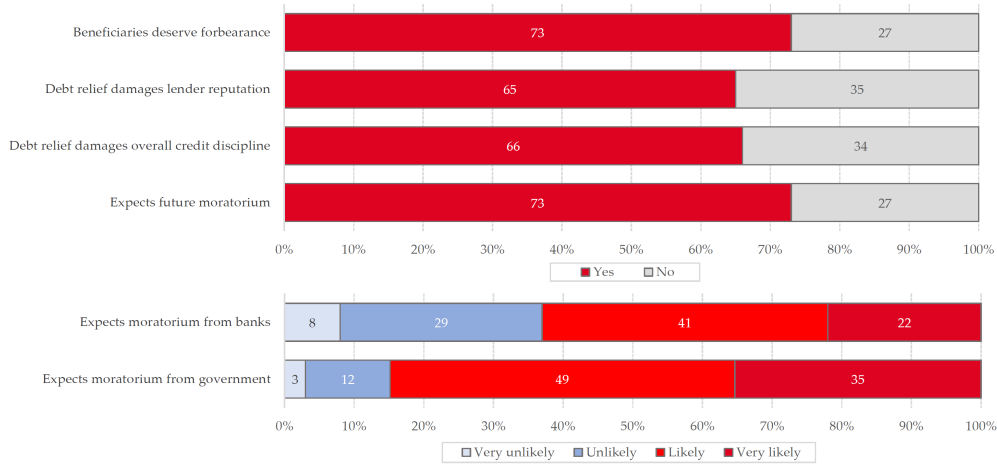
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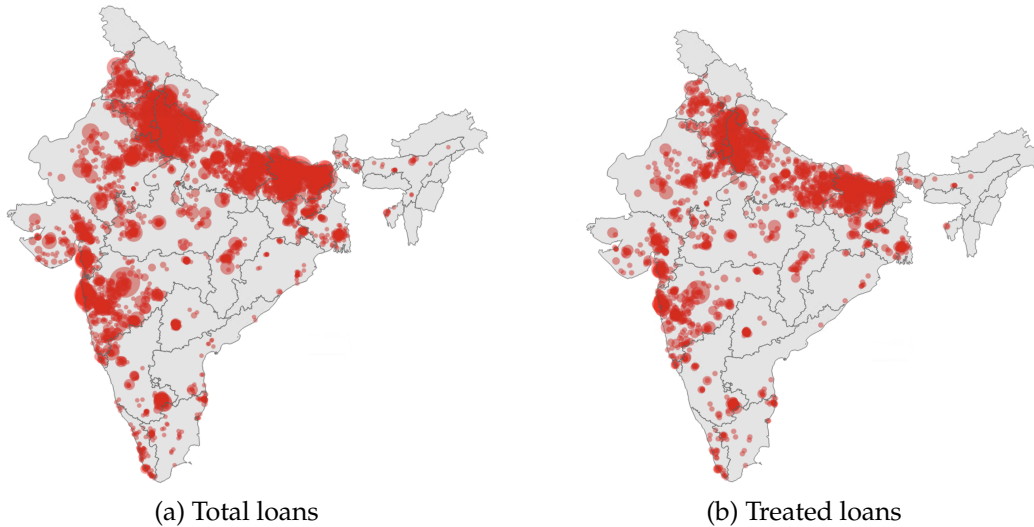
Figures

Figure 1: Beliefs about Debt Forbearance in the Population



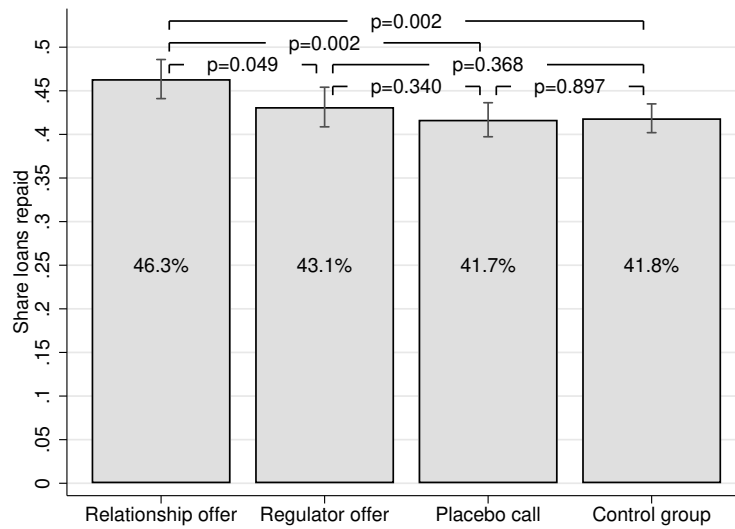
Notes: The figure summarizes responses from a nationwide online survey on beliefs about debt forbearance in the population (N=1,404). The survey was conducted with a sample of respondents representative of the population of consumer loan borrowers in terms of age, income, and gender. The sample was restricted to borrowers who had taken out at least one loan in the year prior to the survey.

Figure 2: Sample and Treated Loans



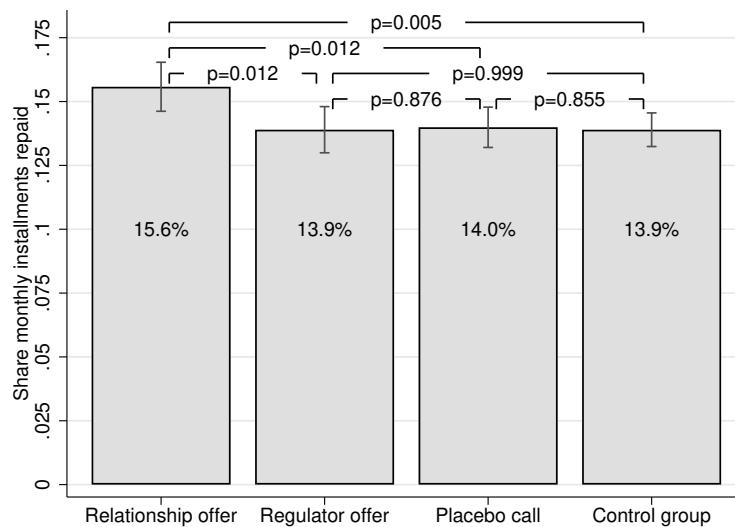
Notes: The figure shows the geographical distribution of loans assigned to one of the debt forbearance treatment conditions (N=51,660) in panel (a) and loans in the estimation sample (N=9,623) in panel (b). Each circle represents a postal code and is scaled by the number of loans in that location.

Figure 3: Treatment Effects – Main Experiment (Loan Repayment)



Notes: The figure shows loan repayment rates in each of the four treatment conditions in the main experiment. Loan repayment is measured three months after the original end date of the loan. Error bars indicate 95% confidence intervals. Horizontal bars show p -values for t -tests of equality of means across treatment conditions, estimated using robust standard errors.

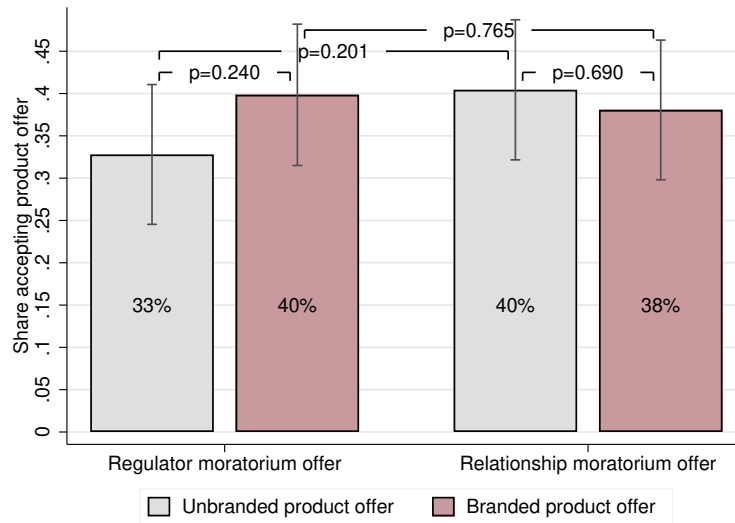
Figure 4: Treatment Effects – Main Experiment (Monthly Payment Made)



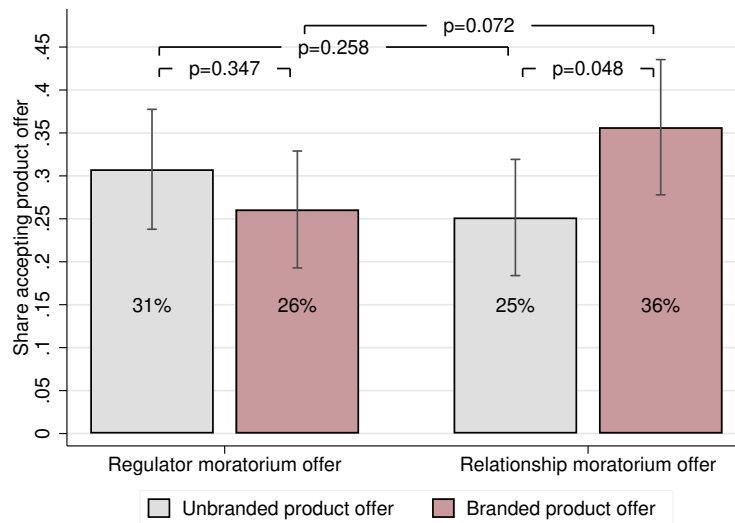
Notes: The figure shows the fraction of customers making their monthly loan payments on time in each of the four treatment conditions in the main experiment. Monthly loan repayment is measured as an indicator equal to one if a borrower makes their monthly payment in full and on time. Error bars indicate 95% confidence intervals. Horizontal bars show p -values for t -tests of equality of means across treatment conditions, estimated using standard errors clustered at the borrower level.

Figure 5: Treatment Effects – Product Adoption Experiment

Panel A: personal loan



Panel B: non-credit product



Notes: Panel A displays the results of the product adoption experiment for the credit product. Panel B displays the results for the non-credit product. The two bars on the left display product adoption rates in the branded and unbranded product offer conditions for borrowers who received the regulator moratorium offer in the main experiment. The two bars on the right display product adoption rates in the branded and unbranded product offer conditions for borrowers who received the relationship moratorium offer in the main experiment. Error bars indicate 95% confidence intervals. Horizontal bars show p -values for t -tests of equality of means across treatment conditions, estimated using robust standard errors.

Tables

Table 1: Summary Statistics and Balance of Covariates

| | Full Sample | Relationship Offer | Regulator Offer | Friendly Call | Collection Call | <i>p-value</i> |
|--|-------------------|-----------------------|--------------------|------------------|--------------------|----------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| <i>Panel A: Borrower characteristics</i> | | | | | | |
| Reported income, Rs '000 | 39.73 [58.03] | 38.49 (1.57) | 38.69 (2.17) | 40.17 (1.47) | 39.51 (1.30) | 0.86 |
| Female | 0.10 [0.30] | 0.11 (0.01) | 0.10 (0.01) | 0.11 (0.01) | 0.11 (0.01) | 0.96 |
| Age | 33.17 [8.70] | 33.86 (0.30) | 33.61 (0.27) | 33.02 (0.22) | 33.15 (0.21) | 0.07 |
| No credit history | 0.67 [0.47] | 0.69 (0.01) | 0.67 (0.01) | 0.68 (0.01) | 0.67 (0.01) | 0.45 |
| Credit score | 730.98 [51.38] | 731.25 (2.31) | 727.08 (2.09) | 729.93 (2.08) | 731.11 (1.67) | 0.42 |
| <i>Panel B: Loan characteristics</i> | | | | | | |
| Principal, Rs '000 | 11.81 [6.58] | 12.23 (0.19) | 12.08 (0.17) | 11.88 (0.14) | 11.89 (0.11) | 0.35 |
| Balance, Rs '000 | 7.05 [5.63] | 7.44 (0.14) | 7.26 (0.14) | 7.39 (0.12) | 7.43 (0.10) | 0.72 |
| Monthly payment, Rs '000 | 1.45 [0.75] | 1.50 (0.02) | 1.46 (0.02) | 1.46 (0.02) | 1.47 (0.01) | 0.48 |
| Loan duration (months) | 7.80 [0.91] | 7.76 (0.02) | 7.77 (0.02) | 7.77 (0.02) | 7.79 (0.02) | 0.81 |
| Months remaining | 5.08 [2.37] | 5.19 (0.06) | 5.15 (0.06) | 5.32 (0.06) | 5.31 (0.05) | 0.08 |
| Day called | 8.12 [4.40] | 7.99 (0.37) | 8.39 (0.36) | 8.45 (0.38) | 8.54 (0.34) | 0.70 |
| <i>Panel C: Borrower credit history</i> | | | | | | |
| Prior loans | 0.38 [1.03] | 0.37 (0.02) | 0.42 (0.03) | 0.34 (0.02) | 0.35 (0.02) | 0.06 |
| Prior defaults | 0.00 [0.03] | 0.00 (0.00) | 0.00 (0.00) | 0.00 (0.00) | 0.00 (0.00) | 0.15 |
| Observations | 9,622 | 1,901 | 1,829 | 2,451 | 3,441 | |

Notes: The table reports summary statistics on contacted borrowers and tests of covariate balance across treatment conditions. Panel A reports summary statistics on borrower characteristics from the lender's administrative data and credit reports, panel B reports summary statistics on loan characteristics observed in the lender's administrative data, and panel C reports summary statistics on borrowers' credit history based on credit bureau data. Column (1) shows summary statistics for the entire sample, columns (2) to (5) shows unconditional means for each variable and treatment conditions, with standard deviations in parentheses. column (6) reports the *p*-value of equality of means across the four main treatment conditions. Because the sampling proportions were changed after the fourth month of the experiment, all balance tests control for an indicator equal to one for all months after this date. Column (1) may differ from weighted averages of columns (2) to (5) for this same reason.

Table 2: Treatment Effects: Loan Repayment

| | Loan repaid (1) | Loan payment made (2) |
|------------------------|---------------------|--------------------------|
| [1] Relationship offer | 0.041*** (0.015) | 0.008* (0.005) |
| [2] Regulator offer | 0.003 (0.015) | -0.004 (0.005) |
| [3] Placebo call | -0.003 (0.013) | 0.002 (0.004) |
| Test: [1]–[2] | 0.037** (0.015) | 0.012** (0.005) |
| Month FE | Yes | Yes |
| Day and caller FE | Yes | Yes |
| Control group mean | 0.42 | 0.14 |
| R-squared | 0.03 | 0.21 |
| Observations | 9,623 | 96,780 |

Notes: This table shows treatment effects of debt forbearance on loan repayment. Each column shows results from a separate regression. The dependent variable in column (1) and column (2) is an indicator equal to 1 if a borrower failed to make a payment within 90 days of the due date, which is the cutoff after which the lender is required to classify the loan as nonperforming. Standard errors, clustered at the caller-date level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Treatment Effects: Borrower Expectations

| | Expects forbearance | | Expects lax enforcement |
|---------------------------|---------------------|------------------------|-------------------------|
| | from banks (1) | from government (2) | (3) |
| [1] Relationship offer | -0.074 (0.196) | -0.023 (0.122) | 0.049 (0.221) |
| [2] Regulator offer | -0.024 (0.185) | -0.030 (0.156) | 0.143 (0.298) |
| Test: [1]–[2] | -0.049 (0.191) | 0.007 (0.140) | -0.094 (0.262) |
| Test: Any– No Offer | -0.042 (0.085) | -0.026 (0.075) | 0.085 (0.114) |
| Month FE | Yes | Yes | Yes |
| Treatment day x caller FE | Yes | Yes | Yes |
| Survey day x caller FE | Yes | Yes | Yes |
| Control group mean | 0.67 | 0.68 | 0.36 |
| R-squared | 0.71 | 0.65 | 0.84 |
| Observations | 320 | 325 | 259 |

Notes: This table shows treatment effects of debt forbearance on expectations of future debt relief. Each column shows results from a separate regression. The dependent variable in column (1) is an indicator equal to 1 if a respondent answered that they expect another round of debt forbearance to be extended by private lenders. The dependent variable in column (2) is an indicator equal to 1 if a respondent answered that they expect another round of debt forbearance to be extended by the government. Both outcomes were elicited in an incentivized belief elicitation exercise. Robust standard errors, clustered at the offer date level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Treatment Effects: Debt Forbearance and Loan Repayment, All Loans

| | Month-level | | | | Loan-level | | | |
|------------------------|--------------------|--------------------------|---------------------------|-------------------------|---------------------|--------------------------|---------------------------|-------------------------|
| | Paid lender (1) | Didn't pay others (2) | Prioritized lender (3) | Share loans paid (4) | Paid lender (5) | Didn't pay others (6) | Prioritized lender (7) | Share loans paid (8) |
| [1] Relationship offer | 0.008* (0.005) | 0.005 (0.005) | 0.003** (0.001) | -0.002 (0.006) | 0.041*** (0.015) | 0.002 (0.006) | 0.003 (0.004) | 0.034** (0.014) |
| [2] Regulator offer | -0.004 (0.005) | -0.006 (0.004) | 0.000 (0.001) | -0.014** (0.006) | 0.003 (0.015) | -0.002 (0.005) | 0.000 (0.004) | -0.007 (0.014) |
| [3] Friendly call | 0.002 (0.004) | 0.000 (0.004) | 0.001 (0.001) | -0.001 (0.005) | -0.003 (0.013) | -0.001 (0.005) | 0.004 (0.003) | -0.008 (0.013) |
| Test: [1]–[2] | 0.012** (0.005) | 0.010** (0.004) | 0.002* (0.001) | 0.012* (0.006) | 0.037** (0.015) | 0.004 (0.005) | 0.003 (0.004) | 0.042*** (0.014) |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day and caller FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control group mean | 0.14 | 0.03 | 0.00 | 0.19 | 0.42 | 0.04 | 0.01 | 0.51 |
| R-squared | 0.21 | 0.02 | 0.01 | 0.26 | 0.03 | 0.01 | 0.01 | 0.03 |
| Observations | 96,780 | 96,780 | 96,780 | 96,780 | 9,623 | 9,623 | 9,623 | 9,623 |

Notes: This table reports treatment effects of debt moratorium offers on loan repayment across the credit market based on credit report data. Each column shows results from a separate regression. The analysis in columns (1) to (4) examines debt repayment data at the monthly level. The dependent variables in columns (1) to (3) are indicators equal to 1 if a borrower paid the lender that extended the debt moratorium offer, failed to make at least one payment to another lender, and made their payment to the lender that extended the moratorium offer while failing to make a payment to another lender, respectively. The dependent variable in column (4) is the total share of loan payments made in full and on time in a given month.

Table 5: How do Borrowers Interpret the Offer?

| Question | Mean | | Difference | SE | N |
|--|------------------------|---------------------------|------------|--------|-----|
| | Regulator offer (1) | Relationship offer (2) | | | |
| <i>[1] Eligibility for forbearance offers</i> | | | | | |
| Believes offer given to all customers | 0.49 | 0.39 | -0.11 | (0.07) | 190 |
| Believes offer given to least creditworthy | 0.22 | 0.18 | -0.03 | (0.06) | 190 |
| Believes offer given to most creditworthy | 0.29 | 0.43 | 0.14** | (0.07) | 190 |
| <i>[2] Does acceptance of the offer signal creditworthiness?</i> | | | | | |
| Accepting sends no signal | 0.17 | 0.31 | 0.14** | (0.07) | 158 |
| Accepting sends positive signal | 0.32 | 0.21 | -0.11 | (0.07) | 158 |
| Accepting sends negative signal | 0.51 | 0.48 | -0.03 | (0.08) | 158 |
| <i>[3] What is the main rationale for timely loan repayment?</i> | | | | | |
| Reason to repay loans is morality | 0.15 | 0.09 | -0.06 | (0.05) | 147 |
| Reason to repay loans is penalties | 0.17 | 0.11 | -0.06 | (0.06) | 147 |
| Reason to repay loans is future loans from lender | 0.03 | 0.21 | 0.18*** | (0.05) | 147 |
| Reason to repay loans is credit score | 0.65 | 0.59 | -0.06 | (0.08) | 147 |

Notes: The table shows the results from a follow-up survey that elicited beliefs about the offer and beliefs about loan repayment and creditworthiness from a random sample of survey participants who had received either the relationship or regulator forbearance offer.

Table 6: Treatment Effect Heterogeneity

| <i>Panel A: Machine Learning CLAN</i> | Monthly payment made | | | | Loan repaid | | | |
|--|------------------------|---------------------------|------------------------------|------------------------|-------------------------|--------------------------|------------------------------|------------------------|
| | Most affected | Least affected | Difference | | Most affected | Least affected | Difference | |
| | γ_5 (1) | γ_1 (2) | $\gamma_5 - \gamma_1$ (3) | <i>p</i> -value (4) | γ_5 (5) | γ_1 (6) | $\gamma_5 - \gamma_1$ (7) | <i>p</i> -value (8) |
| <i>Loan characteristics</i> | | | | | | | | |
| Loan balance | 7,698 (7333,8068) | 6,709 (6323,7079) | 1,012 (481.3,1538) | [0.000] | 7,896 (7200,8565) | 6,188 (5515,6842) | 1,589 (689,2465) | [0.001] |
| No credit score | 0.758 (0.729,0.787) | 0.549 (0.520,0.578) | 0.212 (0.171,0.252) | [0.000] | 0.761 (0.716,0.806) | 0.572 (0.525,0.618) | 0.188 (0.126,0.252) | [0.000] |
| Prime credit score | 0.186 (0.159,0.214) | 0.395 (0.367,0.422) | -0.208 (-0.245,-0.171) | [0.000] | 0.178 (0.136,0.220) | 0.365 (0.321,0.407) | -0.180 (-0.238,-0.120) | [0.000] |
| Months remaining | 4.648 (4.498,4.796) | 5.184 (5.029,5.343) | -0.575 (-0.806,-0.349) | [0.000] | 4.396 (4.161,4.625) | 4.909 (4.678,5.144) | -0.499 (-0.836,-0.175) | [0.005] |
| <i>Borrower characteristics</i> | | | | | | | | |
| Government responsible for relief | 0.762 (0.755,0.769) | 0.769 (0.762,0.776) | -0.008 (-0.018,0.003) | [0.293] | 0.770 (0.755,0.786) | 0.788 (0.772,0.803) | -0.019 (-0.040,0.003) | [0.184] |
| Banks responsible for relief | 0.839 (0.832,0.845) | 0.842 (0.836,0.849) | -0.004 (-0.013,0.006) | [0.882] | 0.812 (0.797,0.827) | 0.836 (0.821,0.851) | -0.024 (-0.045,-0.003) | [0.054] |
| Large income drop | 0.254 (0.246,0.263) | 0.230 (0.221,0.239) | 0.024 (0.013,0.036) | [0.000] | 0.227 (0.211,0.242) | 0.180 (0.164,0.195) | 0.047 (0.025,0.069) | [0.000] |
| Financial distress | 0.459 (0.447,0.471) | 0.453 (0.441,0.465) | 0.008 (-0.009,0.025) | [0.753] | 0.504 (0.481,0.527) | 0.464 (0.441,0.487) | 0.039 (0.007,0.072) | [0.035] |
| High financial literacy | 0.520 (0.509,0.531) | 0.527 (0.516,0.538) | -0.008 (-0.023,0.007) | [0.582] | 0.454 (0.434,0.474) | 0.437 (0.417,0.458) | 0.017 (-0.011,0.046) | [0.484] |
| Credit score matters | 0.830 (0.823,0.836) | 0.816 (0.810,0.823) | 0.013 (0.004,0.023) | [0.010] | 0.771 (0.756,0.785) | 0.748 (0.734,0.762) | 0.022 (0.003,0.043) | [0.051] |
| <i>Panel B: Machine Learning GATES</i> | | | | | | | | |
| Group ATE | 0.279 (0.240,0.318) | -0.225 (-0.263,-0.187) | 0.504 (0.452,0.556) | [0.000] | 0.070 (-0.027,0.167) | -0.004 (-0.100,0.091) | 0.075 (-0.061,0.211) | [0.539] |

Notes: The table shows machine learning estimates of treatment effect heterogeneity for loan repayment. The outcome variable in columns (1) to (4) is a dummy equal to one if a customer made their monthly loan payment on time. The dependent variable in columns (5) to (8) is a dummy equal to one if a loan was repaid in full and on time. For each loan and borrower characteristic, the table reports classification analysis (CLAN) coefficients for the most and least affected groups in Panel A and sorted group average treatment effects (GATES) coefficients in Panel B. The sample is divided into $K = 5$ groups, γ_K , based on the quintiles of the machine learning proxy predictor $S(Z)$. Medians over 100 splits. 90% confidence intervals in parentheses, *p*-values for the hypothesis that the parameter is equal to zero are reported in brackets.

Table 7: Treatment Effects: Product Adoption Experiment

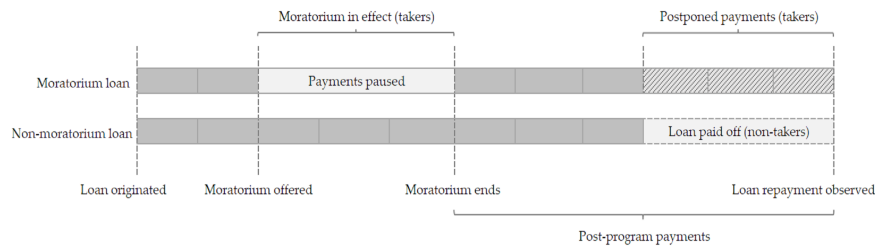
| | Personal loan | | Non-credit product | |
|-----------------------------------|-------------------|-------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) |
| | Accepted offer | | | |
| [1] Relationship moratorium offer | 0.076 (0.060) | 0.119 (0.115) | -0.056 (0.050) | -0.080 (0.076) |
| [2] Branded product offer | 0.070 (0.060) | 0.286 (0.255) | -0.047 (0.050) | -0.043 (0.108) |
| [1]x[2] | -0.094 (0.085) | -0.153 (0.133) | 0.152** (0.073) | 0.206** (0.100) |
| Month FE | No | Yes | No | Yes |
| Day and caller FE | No | Yes | No | Yes |
| R-squared | 0.004 | 0.542 | 0.008 | 0.450 |
| Observations | 528 | 528 | 632 | 632 |

Notes: This table shows treatment effect estimates from the cross-selling experiment. Columns (1) and (2) display the results of the product adoption experiment for the credit product. Columns (3) and (4) display the results for the non-credit product. Each column reports results from a separate regression. The sample in all regressions is restricted to borrowers who received a debt moratorium offer in the main experiment and were approached with a cross-sell offer no more than one year after receiving the moratorium offer. The dependent variable in all regressions is a dummy equal to 1 if a participant accepted the cross-sell offer. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Supplementary Appendix (For Online Publication)

A Appendix Figures

Figure A.1: Forbearance Experiment – Timeline and Measurement of Outcomes



Notes: The figure shows a schematic timeline of the loan repayment outcomes *monthly loan payment* and *loan repaid* and illustrates how each is observed for loans that opt into the moratorium and all other loans, respectively. Loans that opt into the moratorium have their monthly loan payments paused for three months, the tenor of their loan is extended by three months and the missed payments are added to the end of the loan. Loans are considered fully repaid if a borrower has made all payments by the end of this extended loan closing date.

B Appendix Tables

Table B.1: Summary Statistics: Debt Relief Survey

| | N | Mean | Median | StDev |
|---|-------|---------|--------|---------|
| | (1) | (2) | (3) | (4) |
| <i>Panel A: All Respondents</i> | | | | |
| Loan outstanding | 1,404 | 0.47 | 0 | 0.50 |
| Ever had loan | 1,404 | 0.65 | 1 | 0.48 |
| Male | 1,404 | 0.53 | 1 | 0.50 |
| Age | 1,404 | 35.52 | 35 | 12.32 |
| Employed | 1,404 | 0.79 | 1 | 0.41 |
| Income | 1,283 | 84,704 | 42,000 | 298,417 |
| <i>Panel B: Respondents with loan outstanding</i> | | | | |
| Male | 653 | 0.55 | 1 | 0.50 |
| Age | 653 | 36.67 | 35 | 10.61 |
| Employed | 653 | 0.92 | 1 | 0.28 |
| Income | 642 | 114,010 | 50,000 | 408,938 |

Notes: The table shows summary statistics for the respondent population of a nationally representative online survey on debt forbearance. Panel (a) shows descriptives for all respondents, panel (b) shows descriptives for respondents that had at least one loan outstanding at the time of the survey. The survey was conducted between December 1, 2021 and January 31, 2022 using an online survey platform. We restrict the sample to the 1,404 respondents who passed a standard attention check question administered as part of the survey.

Table B.2: Representative Online Survey: Responses

| | Percent | | | |
|---|---------------|----------|---------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| Widespread debt relief can discourage borrowers who are able to pay their debts from paying them in the future. | | | Yes, agree 66.10 | No, disagree 33.90 |
| If a private lender gives more debt relief than others, their borrowers might take them less seriously and de-prioritize making payments to that lender. | | | 64.96 | 35.04 |
| If there is another spike in Covid-19 cases with more 200,000 cases per day within the next 12 months, how likely do you think it is that the Government of India will grant another debt moratorium to borrowers, similar to those offered in the past. | | | 72.93 | 27.07 |
| | Very unlikely | Unlikely | Likely | Very likely |
| Suppose that a borrower who <i>is not in need</i> and has no problems making their loan payments is offered a debt moratorium (repayment holiday). How likely do you think it is that they would take up the repayment holiday, even if they do not need it? | 6.77 | 21.58 | 43.80 | 27.85 |
| Now suppose that a borrower who <i>is in need</i> and has problems making their loan payments is offered a debt moratorium (repayment holiday). How likely do you think it is that this would have a negative effect on their repayment behavior in the future? | 9.12 | 27.71 | 40.46 | 22.72 |
| Suppose there is an economic crisis and borrowers everywhere in India are having difficulties making their loan payments on time. How likely do you think that the government will offer a debt moratorium to borrowers in this situation? | 3.42 | 12.35 | 49.00 | 35.33 |
| Suppose there is an economic crisis and borrowers everywhere in India are having difficulties making their loan payments on time. How likely do you think that private banks will offer debt moratoria to borrowers in this situation? | 8.05 | 28.70 | 40.88 | 22.36 |
| Suppose there is an economic crisis and borrowers everywhere in India are having difficulties making their loan payments on time. How likely do you think that microfinance institutions will offer debt moratoria to borrowers in this situation? | 13.11 | 33.62 | 35.97 | 17.31 |

Notes: The table shows response frequencies for the main questions of a nationally representative online survey on debt forbearance. The sample is restricted to the 1,404 respondents who passed a standard attention check question administered as part of the survey.

Table B.3: Main Experiment: Treatment Cells and Takeup Rates

| Treatment (1) | Assigned (2) | Contacted (3) | Accepted offer (4) | Acceptance rate (5) |
|------------------------|-----------------|------------------|-----------------------|------------------------|
| [1] Relationship offer | 11,730 | 1,901 | 257 | 0.135 |
| [2] Regulator offer | 10,962 | 1,829 | 228 | 0.125 |
| [3] Placebo call | 9,326 | 2,452 | 0 | 0.000 |
| [4] Control group | 13,587 | 3,441 | 0 | 0.000 |
| Total | 45,605 | 9,623 | 485 | |

Notes: The table shows treatment cell sizes and takeup rates of forbearance offers in the debt forbearance experiment.

Table B.4: Balance of Covariates: Forbearance Takers

| | Offer accepted (1) | Relationship Offer (2) | Regulator Offer (3) | p-value (4) |
|--|-----------------------|------------------------------|---------------------------|----------------|
| <i>Panel A: Borrower characteristics</i> | | | | |
| Reported income ('000 INR) | 39.977 [59.798] | 41.697 (4.926) | 37.965 (4.897) | 0.592 |
| No credit history | 0.641 [0.480] | 0.634 (0.028) | 0.649 (0.032) | 0.726 |
| Credit score | 736.891 [47.479] | 737.340 (5.403) | 736.363 (5.746) | 0.902 |
| <i>Panel B: Loan characteristics</i> | | | | |
| Principal ('000 INR)) | 13.929 [8.223] | 14.153 (0.541) | 13.676 (0.567) | 0.543 |
| Balance ('000 INR) | 8.974 [7.000] | 9.097 (0.442) | 8.835 (0.474) | 0.685 |
| Monthly Payment ('000 INR) | 1.735 [0.894] | 1.760 (0.059) | 1.706 (0.059) | 0.520 |
| Loan duration (months) | 7.944 [0.935] | 7.946 (0.059) | 7.943 (0.067) | 0.977 |
| Loan age (months) | 5.686 [2.546] | 5.647 (0.169) | 5.730 (0.154) | 0.716 |
| Day Called | 9.466 [5.047] | 9.105 (0.471) | 9.873 (0.469) | 0.249 |
| <i>Panel C: Borrower credit history</i> | | | | |
| Prior loans | 3.815 [17.122] | 4.311 (1.709) | 3.276 (0.567) | 0.566 |
| Prior defaults | 0.032 [0.177] | 0.034 (0.014) | 0.031 (0.013) | 0.866 |
| Observations | 485 | 257 | 228 | |

Notes: The table shows summary statistics and tests of covariate balance for contacted borrowers who accepted one of the forbearance offers. Panel A reports summary statistics on borrower characteristics from the lender's administrative data and credit reports, panel B reports summary statistics on loan characteristics observed in administrative data from the lender, and panel C reports summary statistics on borrowers' credit history based on credit bureau data. Column (1) shows summary statistics for the entire sample, columns (2) shows summary statistics for customers who accepted the relationship offer and column (3) shows summary statistics for customers who accepted the regulator offer. Column (4) reports the p -value of equality of means across the two forbearance offer treatment conditions. At the foot of the table, we report a joint F -Test of all variables.

Table B.5: Forbearance Offer Take-up

| | Offer accepted | | |
|------------------------------|----------------------|---------------------------|------------------------|
| | Any offer (1) | Relationship offer (2) | Regulator offer (3) |
| Balance ('000s) | 0.004*** (0.002) | 0.006** (0.002) | 0.003 (0.002) |
| Monthly payment ('000s) | 0.057*** (0.009) | 0.060*** (0.013) | 0.053*** (0.013) |
| Original credit score ('00s) | -0.004 (0.005) | -0.003 (0.008) | -0.005 (0.007) |
| Prior loans | -0.003 (0.005) | -0.002 (0.008) | -0.002 (0.006) |
| Prior defaults | -0.101*** (0.026) | -0.112*** (0.033) | 0.000 (0.000) |
| Month FE | Yes | Yes | Yes |
| Day and caller FE | No | No | No |
| DV mean | 0.13 | 0.14 | 0.12 |
| R-squared | 0.03 | 0.04 | 0.03 |
| Observations | 3,730 | 1,901 | 1,829 |

Notes: This table reports reduced form effects of borrower and loan characteristics on take-up of debt forbearance offers. The outcome variable is an indicator equal to 1 if a contacted customer accepted the debt forbearance offer. The sample in column (1) consists of all contacted customers who received a debt forbearance offer. In column (2), the sample is restricted to contacted customers assigned to the *relationship offer* condition, and column (3) restricts the sample to customers in the *regulator offer* treatment condition. Standard errors, clustered at the caller-offer date level in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Forbearance Offer Take-up, by Price

| | Offer accepted | | |
|--------------------------|---------------------|---------------------------|------------------------|
| | Any offer (1) | Relationship offer (2) | Regulator offer (3) |
| High fee (Rs 500) | -0.053** (0.022) | -0.038 (0.026) | -0.069* (0.035) |
| Implied price elasticity | 1.2 | 0.8 | 1.6 |
| Month FE | Yes | Yes | Yes |
| Day and caller FE | No | No | No |
| DV mean | 0.13 | 0.14 | 0.12 |
| R-squared | 0.01 | 0.01 | 0.01 |
| Observations | 3,730 | 1,901 | 1,829 |

Notes: This table reports treatment effects of price on take-up of forbearance offers. The outcome variable in all columns is an indicator equal to 1 if a contacted customer accepted the debt forbearance offer. Take-up is regressed on a dummy equal to 1 if a customer was assigned to receive the offer at the *high fee* of Rs 500 instead of the *regular fee* of Rs 350. The sample in column (1) includes all contacted customers who received a forbearance offer. In column (2), the sample is restricted to contacted customers assigned to the *relationship offer* condition, and column (3) restricts the sample to customers in the *regulator offer* treatment condition. Standard errors, clustered at the caller-offer date level in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Treatment Effect by Predicted Likelihood of Moratorium Take-up

| | Loan repaid | | | | | | | |
|------------------------|--------------------|-------------------|--------------------|-------------------|---------------------|-------------------|------------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| [1] Relationship offer | 0.037** (0.016) | 0.050 (0.043) | 0.039** (0.016) | 0.063 (0.040) | 0.043*** (0.016) | 0.007 (0.045) | 0.038* (0.020) | 0.031* (0.018) |
| [2] Regulator offer | -0.001 (0.015) | 0.020 (0.044) | 0.004 (0.015) | -0.009 (0.040) | 0.001 (0.015) | -0.006 (0.046) | 0.013 (0.019) | -0.010 (0.019) |
| [3] Placebo call | -0.005 (0.014) | -0.009 (0.039) | 0.002 (0.013) | -0.053 (0.036) | 0.000 (0.013) | -0.036 (0.035) | 0.020 (0.017) | -0.030* (0.017) |
| Test: [1]–[2] | 0.039** (0.016) | 0.030 (0.044) | 0.035* (0.016) | 0.072 (0.040) | 0.042** (0.016) | 0.014 (0.045) | 0.025 (0.020) | 0.041* (0.019) |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day and caller FE | No | No | No | No | No | No | No | No |
| Sample split | P(Any offer paid) | | P(Rel offer paid) | | P(Reg offer paid) | | P(Control repaid loan) | |
| Offer paid | 0.11 | 0.31 | 0.11 | 0.29 | 0.11 | 0.29 | 0.15 | 0.11 |
| Control group mean | 0.43 | 0.32 | 0.43 | 0.32 | 0.42 | 0.37 | 0.27 | 0.57 |
| R-squared | 0.11 | 0.31 | 0.11 | 0.29 | 0.11 | 0.29 | 0.15 | 0.11 |
| Observations | 8,660 | 963 | 8,660 | 963 | 8,659 | 964 | 4,811 | 4,812 |

Notes: The table shows treatment effects of debt forbearance offers on total loan repayment, splitting the sample by predicted likelihood of forbearance take-up. The dependent variable in all regressions is an indicator equal to 1 if a customer completed repayment of their loan. Standard errors, clustered at the caller-offer date level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Summary Statistics: Economic Survey

| | Responses (1) | Mean (2) | Median (3) | St.dev (4) |
|------------------------------------|------------------|-------------|---------------|---------------|
| Expects govt relief | 1,334 | 0.66 | 1.00 | 0.47 |
| Expects private relief | 1,267 | 0.54 | 1.00 | 0.50 |
| Covid-19 worsened finances | 1,148 | 0.44 | 0.00 | 0.50 |
| Income losses during Covid-19 | 967 | 0.57 | 1.00 | 0.50 |
| Difficulty making payments | 967 | 0.57 | 1.00 | 0.50 |
| Feb 2020 income, Rs '000 per month | 811 | 18.56 | 15.00 | 20.88 |
| Dec 2021 income Rs '000 per month | 714 | 15.80 | 12.00 | 22.71 |
| Has bank account | 994 | 0.93 | 1.00 | 0.26 |
| Bank account age (yrs) | 906 | 8.32 | 7.00 | 6.42 |
| Used smartphone for income | 977 | 0.18 | 0.00 | 0.38 |
| Financial literacy (0-3) | 858 | 1.57 | 2.00 | 0.81 |

Notes: The table reports summary statistics for the follow-up survey that elicited borrower responses on their economic situation, and beliefs about the likelihood of future debt forbearance from banks and private lenders. A subset of beliefs elicited were incentivized and are reported in Table 3.

Table B.9: Summary Statistics: Cross-Selling Experiment

| Treatment (1) | Branding (2) | Contacted (3) | Accepted (4) | Acceptance rate (5) |
|--------------------|-----------------|------------------|-----------------|------------------------|
| Regulator offer | Unbranded | 169 | 52 | 0.308 |
| Regulator offer | Branded | 161 | 42 | 0.261 |
| Relationship offer | Unbranded | 159 | 40 | 0.252 |
| Relationship offer | Branded | 143 | 51 | 0.357 |
| All | | 632 | 185 | 0.293 |

Notes: The table shows take-up summary statistics for the cross-selling experiment. The sample for the experiment is restricted to customers who received either the regulator or relationship forbearance offers in the main experiment. “Treatment” refers to the offer extended to the participant in the main experiment, “Branding” refers to the treatments in the cross-selling experiment in which customers received either a branded or unbranded offer for a non-credit product from the lender.

Table B.10: Who Responds to Debt Moratoria? Treatment Effect Heterogeneity, OLS

| | Large loan (1) | No credit score (2) | High credit score (3) | Long loan (4) | Gov resp. (5) | Banks resp. (6) | Large income drop (7) | Fin. distress (8) | Financial lit. (9) | Credit score matters (10) |
|----------------------------|----------------------|------------------------|--------------------------|----------------------|-------------------|--------------------|--------------------------|----------------------|-----------------------|------------------------------|
| Trait | -0.217*** (0.011) | 0.032*** (0.012) | -0.064*** (0.012) | -0.260*** (0.011) | 0.002 (0.048) | -0.021 (0.050) | -0.016 (0.048) | -0.049 (0.032) | 0.033 (0.037) | -0.041 (0.057) |
| Relationship offer | 0.032* (0.018) | 0.031 (0.022) | 0.047*** (0.015) | 0.062*** (0.017) | 0.036 (0.090) | -0.050 (0.099) | -0.028 (0.050) | -0.066 (0.051) | 0.003 (0.056) | -0.284*** (0.097) |
| Relationship offer * trait | 0.0114 (0.025) | 0.0141 (0.027) | -0.0264 (0.028) | -0.0508** (0.024) | -0.152 (0.102) | -0.0514 (0.108) | 0.176 (0.113) | 0.0583 (0.073) | -0.0792 (0.082) | 0.260** (0.111) |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day and caller FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 9,623 | 9,623 | 9,623 | 9,623 | 845 | 925 | 832 | 1215 | 971 | 654 |
| R-squared | 0.074 | 0.031 | 0.034 | 0.098 | 0.074 | 0.060 | 0.048 | 0.049 | 0.054 | 0.084 |

Notes: The table shows estimates of treatment effect heterogeneity. Each column shows results from a separate regression. The dependent variable in all regressions is an indicator equal to 1 if a customer made their monthly loan payment by the due date. Standard errors, clustered at the loan level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11: Robustness – Test for Treatment Effects on Survey Outcomes

| | Large loan (1) | No credit score (2) | High credit score (3) | Long loan (4) | Gov resp. (5) | Banks resp. (6) | Large income drop (7) | Fin. distress (8) | Financial lit. (9) | Credit score matters (10) |
|------------------------|-------------------|------------------------|--------------------------|-------------------|-------------------|--------------------|--------------------------|----------------------|-----------------------|------------------------------|
| [1] Relationship offer | -0.023 (0.015) | 0.024 (0.014) | -0.024 (0.013) | -0.016 (0.014) | -0.042 (0.043) | -0.087* (0.038) | -0.025 (0.041) | -0.042 (0.043) | -0.084 (0.049) | -0.097* (0.048) |
| [2] Regulator offer | -0.025 (0.015) | 0.004 (0.014) | -0.010 (0.013) | -0.020 (0.015) | -0.034 (0.043) | -0.021 (0.036) | -0.017 (0.043) | 0.022 (0.043) | -0.041 (0.048) | -0.091 (0.050) |
| [3] Placebo call | -0.005 (0.013) | 0.010 (0.013) | -0.012 (0.012) | 0.002 (0.013) | -0.032 (0.038) | -0.048 (0.032) | 0.010 (0.039) | -0.007 (0.040) | 0.053 (0.043) | -0.018 (0.040) |
| Observations | 9,623 | 9,623 | 9,623 | 9,623 | 845 | 925 | 832 | 1,215 | 971 | 654 |
| R-squared | 0.057 | 0.014 | 0.020 | 0.086 | 0.040 | 0.057 | 0.034 | 0.034 | 0.054 | 0.058 |

Notes: The table test for the presence of treatment effects of debt moratorium offers on responses from the economic survey. Each column shows results from a separate regression. The dependent variables are responses to the question indicated at the top of each column. Standard errors, clustered at the caller-offer date level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12: Treatment Effects – Post-Program Borrowing and Loan Performance

| | Has score (1) | Score (2) | Loans (3) | Avg. Loan Amount (4) | Pct. Writeoffs (5) |
|------------------------|------------------|---------------------|-------------------|-------------------------|-----------------------|
| [1] Relationship offer | 0.001 (0.003) | 1.031 (1.408) | 0.041 (0.054) | 1.95 (2.91) | 0.017 (0.018) |
| [2] Regulator offer | 0.001 (0.003) | -3.023** (1.433) | -0.067 (0.052) | -3.41 (2.76) | -0.001 (0.014) |
| [3] Friendly call | 0.000 (0.003) | -2.692** (1.270) | -0.066 (0.049) | -4.11 (2.60) | 0.012 (0.012) |
| Test: [1]–[2] | 0.001 (0.003) | 4.055** (1.420) | 0.108* (0.053) | 5.37* (3.00) | 0.018 (0.016) |
| Month FE | Yes | Yes | Yes | Yes | Yes |
| Day and caller FE | No | No | No | No | No |
| Control group mean | 0.99 | 697 | 0.77 | 40.28 | 0.07 |
| R-squared | 0.02 | 0.01 | 0.01 | 0.02 | 0.01 |
| Observations | 9,623 | 9,496 | 9,623 | 3,471 | 3,534 |

Notes: This table reports treatment effects of debt moratorium offers on post program credit scores, borrowing, and loan performance using credit bureau data. *Has credit score* is a dummy equal to one if a borrower has a credit report with sufficient information content to allow for the calculation of a credit score. *Credit score* is the numerical credit score for borrowers with a credit score. *Loans* is the number of active loans on a borrower’s credit report taken out after the end of the debt moratorium. *Average loan* amount is the loan amount of all loans obtained after the end of the program. *Writeoffs* is the share of loans obtained after the end of the debt moratorium that are in default. Standard errors, clustered at the loan level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Treatment Effect Heterogeneity: Cross-Selling Experiment

| <i>Panel A: Machine Learning CLAN</i> | Offer accepted | | | |
|--|-------------------------|-------------------------|---------------------------|-----------------|
| | Most affected | Least affected | Difference | |
| | γ_5 | γ_1 | $\gamma_5 - \gamma_1$ | <i>p</i> -value |
| | (1) | (2) | (3) | (4) |
| Loan balance | 7.132 (5.676,8.625) | 5.621 (4.153,7.090) | 1.571 (-0.491,3.645) | [0.263] |
| No credit score | 0.573 (0.471,0.676) | 0.798 (0.696,0.900) | -0.222 (-0.369,-0.076) | [0.006] |
| Prime credit score | 0.381 (0.280,0.480) | 0.141 (0.043,0.239) | 0.231 (0.091,0.375) | [0.003] |
| Months remaining | 4.305 (3.753,4.856) | 4.867 (4.293,5.416) | -0.536 (-1.330,0.235) | [0.349] |
| Banks responsible | 0.752 (0.709,0.791) | 0.764 (0.724,0.806) | -0.019 (-0.075,0.040) | [1.000] |
| Government responsible | 0.699 (0.658,0.738) | 0.748 (0.707,0.789) | -0.055 (-0.113,-0.001) | [0.091] |
| Large income drop | 0.188 (0.153,0.223) | 0.192 (0.157,0.226) | -0.004 (-0.053,0.044) | [1.000] |
| Financial distress | 0.436 (0.380,0.492) | 0.427 (0.372,0.482) | 0.009 (-0.070,0.088) | [1.000] |
| Financial literacy | 0.501 (0.448,0.553) | 0.551 (0.499,0.603) | -0.057 (-0.129,0.017) | [0.253] |
| Credit score important. | 0.823 (0.791,0.854) | 0.823 (0.792,0.854) | 0.001 (-0.044,0.045) | [1.000] |
| <i>Panel B: Machine Learning GATES</i> | | | | |
| Offer accepted | 0.086 (-0.168,0.346) | 0.126 (-0.162,0.422) | -0.039 (-0.426,0.345) | [1.000] |

Notes: The table shows machine learning estimates of treatment effect heterogeneity for product take-up in the cross-selling experiment. The outcome variable in all columns is a dummy equal to one if a borrower assigned to the relationship offer treatment in the original experiment took up the product offer in the cross-sell experiment. For each loan and borrower characteristic, the table reports classification analysis (CLAN) coefficients for the most and least affected groups in Panel A and sorted group average treatment effects (GATES) coefficients in Panel B. The sample is divided into $K = 5$ groups, γ_K , based on the quintiles of the machine learning proxy predictor $S(Z)$. Medians over 100 splits. 90% confidence intervals in parentheses, *p*-values for the hypothesis that the parameter is equal to zero are reported in brackets.

Table B.14: Treatment Effect by Predicted Likelihood of Moratorium Take-up

| | Loan repaid | | | | | | | |
|------------------------|--------------------|-------------------|--------------------|-------------------|---------------------|-------------------|------------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| [1] Relationship offer | 0.037** (0.016) | 0.050 (0.043) | 0.039** (0.016) | 0.063 (0.040) | 0.043*** (0.016) | 0.007 (0.045) | 0.038* (0.020) | 0.031* (0.018) |
| [2] Regulator offer | -0.001 (0.015) | 0.020 (0.044) | 0.004 (0.015) | -0.009 (0.040) | 0.001 (0.015) | -0.006 (0.046) | 0.013 (0.019) | -0.010 (0.019) |
| [3] Placebo call | -0.005 (0.014) | -0.009 (0.039) | 0.002 (0.013) | -0.053 (0.036) | 0.000 (0.013) | -0.036 (0.035) | 0.020 (0.017) | -0.030* (0.017) |
| Test: [1]–[2] | 0.039** (0.016) | 0.030 (0.044) | 0.035* (0.016) | 0.072 (0.040) | 0.042** (0.016) | 0.014 (0.045) | 0.025 (0.020) | 0.041* (0.019) |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Day and caller FE | No | No | No | No | No | No | No | No |
| Sample split | P(Any offer paid) | | P(Rel offer paid) | | P(Reg offer paid) | | P(Control repaid loan) | |
| Offer paid | 0.11 | 0.31 | 0.11 | 0.29 | 0.11 | 0.29 | 0.15 | 0.11 |
| Control group mean | 0.43 | 0.32 | 0.43 | 0.32 | 0.42 | 0.37 | 0.27 | 0.57 |
| R-squared | 0.11 | 0.31 | 0.11 | 0.29 | 0.11 | 0.29 | 0.15 | 0.11 |
| Observations | 8,660 | 963 | 8,660 | 963 | 8,659 | 964 | 4,811 | 4,812 |

Notes: The table shows treatment effects of debt forbearance offers on total loan repayment, splitting the sample by predicted likelihood of forbearance take-up. The dependent variable in all regressions is an indicator equal to 1 if a customer completed repayment of their loan. Standard errors, clustered at the caller-offer date level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.