Design Implications of Temporary Debt Relief Programmes: Retrospective versus Prospective Moratoria*

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Abstract

We study whether the retrospective implementation of temporary debt relief affects repayment discipline. We utilise the unanticipated announcement of the Covid-19 lockdown in the last week of March 2020: borrowers with instalments due during this week missed their March repayment before the moratorium was notified. For otherwise similar borrowers with instalments due before the lockdown, the same moratorium scheme applied prospectively from April onward. This sharp due-date-based assignment enables us to measure the effect of prospective *vs.* retrospective temporary debt relief independent of the concurrent macroeconomic stress. Contrary to the view that temporary debt relief does not distort future repayment incentives, we document that the implementation design of the relief programme matters. Even temporary payment holidays distort repayment discipline if applied retrospectively. We find evidence suggesting that a retrospective moratorium induces moral hazard, which drives the observed deterioration in repayment behaviour.

Keywords: Forbearance, Household finance, Credit risk, Strategic default, Microfinance, Financial inclusion, Emerging markets, India

(JEL: G21, G23, G28, G51)

1 Introduction

The financial economics literature is debating how best to reduce delinquency among distressed borrowers. Temporary relief from debt payments is one such method (Chava et al. (2023)), especially during a sudden economic crisis. Easing the repayment burden, even for a short period, may help distressed borrowers recover, and then repay their debt obligations later (Bolton and Rosenthal (2002), Collins and Urban (2015), Fiorin et al. (2023)). It has been shown to help households (Ganong and Noel (2020), Agarwal et al. (2017)), farmers (Kanz (2016)), students (Dinerstein et al. (2024), Katz (2023)), firms (Reinhart and Rogoff (2020)), and countries (Reinhart and Trebesch, 2016, Asonuma and Trebesch, 2016).

In this paper, we ask whether the implementation design of temporary debt relief affects repayment discipline. We focus on one feature of relief delivery: whether a payment holiday is granted prospectively or retrospectively. A *prospective* moratorium is announced before the instalment falls due, so the borrower knows in advance that she may skip the payment without being marked a defaulter. A *retrospective* moratorium is granted after the fact: the lender defers an instalment the borrower has already missed.

Whether prospective and retrospective delivery affect repayment behaviour differently is not obvious ex ante, as two competing arguments point in opposite directions. One argument is that the delivery structure should not matter because the moratorium only defers an instalment. Despite the deferral, the borrower still owes in full, and hence should respond to both designs alike. This is why temporary suspension of debt obligation is usually regarded as benign (Indarte and Kanz, 2024, Ferreira et al., 2025).

The competing argument is that the delivery structure matters, because of what the relief teaches the borrower about her lender. Kanz (2016) documents that permanent debt waivers induce moral hazard, as borrowers who see their obligations forgiven repay less diligently thereafter. A retrospective deferral can carry a similar lesson: the borrower has already missed a payment, and the lender has let it pass unpunished. A prospective moratorium conveys no such lesson, since the borrower enrols before any repayment is missed. If borrowers infer future leniency from their lender's past conduct, strategic default (usually observed after permanent debt waivers) could surface even under temporary relief.

Isolating this effect is difficult for two reasons. First, moratoria are almost always granted

during a crisis, so any change in repayment could reflect the relief itself or the shock that prompted it. Second, prospective and retrospective delivery rarely coexist within a single scheme, so any comparison also picks up differences in lenders, borrowers, and local conditions. We use a natural experiment that overcomes both challenges: a single scheme in which otherwise identical borrowers receive the same relief delivered in the two ways, with assignment unrelated to how badly the crisis affected them.

Our setting is the Covid-19 lockdown imposed by the Government of India on 25 March 2020, the strictest lockdown globally[1], triggering a sudden economic crisis. A few days later, the Reserve Bank of India (RBI) issued a Regulatory Package permitting lenders to grant a moratorium on instalments due from 1 March 2020.[2] The regulation allowed the lender discretion over the implementation design of the moratorium scheme. Lenders could choose who to grant relief, and over what time period.

We obtain anonymised loan records from a large Non-Banking Financial Company (NBFC)[3] that lends primarily to low-income borrowers in India. This lender took a few additional days to formulate its own moratorium policy, and retrospectively granted moratorium to all borrowers who had missed their March instalment. This notification was communicated to each such borrower individually in the first week of April 2020. The lockdown date of 25 March 2020 served as an arbitrary cut-off that sorted otherwise similar borrowers into two groups based only on their contractual due date.

Borrowers whose instalment fell due in March before the lockdown had repaid their March obligation under the original contract, and received the moratorium only prospectively, for the subsequent months (control). In contrast, borrowers whose March instalment was due during the lockdown week (25-31 March) could not make any repayment for that month, and received a retrospective deferral of their already-missed March instalment (treated). For subsequent months, starting April 2020, both groups received the moratorium in the same way. Given that the Covid-19 pandemic was exogenous to economic systems, and the lockdown was announced suddenly, this natural experiment split all active retail borrowers

[1] See Figure 17-k of Hale et al. (2020).

[2] RBI Notification DOR.No.BP.BC.47/21.04.048/2019-20 dated 27 March 2020, "COVID-19 Regulatory Package" (https://www.rbi.org.in/Scripts/NotificationUser.aspx?Id=11835), subsequently extended by RBI Notification DOR.No.BP.BC.71/21.04.048/2019-20 dated 23 May 2020.

[3] A non-banking financial company (NBFC) cannot take deposits. It raises lending capital by issuing bonds or borrowing from banks.

in India into quasi-random treatment and control groups.

The administrative dataset obtained from the lender contained loan terms, demographic details, and monthly repayments of its 17,864 borrowers. It is apt for this study because forbearance schemes are usually availed by lower-income segments of the population (Ferreira et al., 2025, Cherry et al., 2021), and the borrowers in this dataset belong to that segment. Each of these borrowers had only one loan running and was a punctual payer in the pre-crisis period. Under the RBI's enabling permission, the lender chose to extend a moratorium of up to six months (with an option to opt out at any time) to all borrowers between March 2020 and August 2020. The loan tenure was extended by the number of months for which a borrower opted to pause repayments during the moratorium period. We collected data for six months before the start of the crisis and six months after the end of the moratorium, so the sample period spans one-and-a-half years: September 2019 to February 2021.

Using the difference-in-differences method with borrower and time fixed effects, we compare the loan repayments of borrowers in the treated and the control groups after the moratorium is withdrawn while controlling for borrower characteristics and loan terms. The Ordinary Least Squares (OLS) estimates show that a purely retrospective deferral of the March instalment lowers the treatment group's repayment rate by 8.8 percentage points compared to the control group borrowers in the months following the moratorium's expiry. This represents 9.2% of the pre-period repayment rate of 96% for this sample of borrowers.

We verify the robustness of this finding with several checks. First, we replicate our analysis using various alternative measures of delinquency. These measures include a monthly missed-instalment indicator, non-performing assets (NPA, defined as 90 days of non-repayment consistent with the RBI's definition), and various overdue thresholds. All these alternative measures of delinquency produce consistent estimates. Furthermore, upon examining loans at or past maturity, using overdue days greater than 30 or 90 days, we find that the treatment effects are not transitory.

Second, treated and control repayment trends are visually indistinguishable before the lockdown, both in raw means and in month-wise event-study coefficients across all three outcomes; the absence of pre-trends is expected since the sample contains only punctual payers in the pre-period.

Third, every specification we report includes borrower fixed effects, so the treatment coefficient is identified purely from within-borrower changes between the pre- and post-moratorium windows. Any time-invariant difference between treated and control borrowers, observable or unobservable, is differenced out by construction. Beyond this safeguard, the two groups are also similar on observable loan terms and demographic characteristics, and the few statistically significant differences are economically negligible. We further (i) control for observable loan terms and borrower characteristics in the regression and (ii) re-estimate on a nearest-neighbour-matched sample. Both produce coefficient estimates similar to the baseline.

Fourth, treated borrowers received the moratorium roughly seven days earlier than control borrowers. One might question whether such a short difference could meaningfully affect loan performance. We examine this concern by comparing repayment outcomes across borrowers who voluntarily availed different lengths of moratorium. For those who took only a one-month voluntary moratorium (e.g., April 2020), the 7-day gap represents a sizable share (7/30 days), whereas for those who took the full five-month voluntary moratorium (April–August 2020), the gap is relatively minor (7/150 days). We observe that repayment performance diverges more substantially as moratorium length increases: treated borrowers with a one-month moratorium repaid 1.1 percentage points less than those in the control group, while those with a five-month moratorium repaid 19.2 percentage points less. This pattern rules out the 7-day timing difference as the primary driver and supports our interpretation that even a short retrospective debt relief could distort borrower expectations, and hence, affect their repayment behaviour.

These tests also mitigate concerns about the self-selection of borrowers out of the moratorium scheme. Specifically, we compare borrowers who opted out of the scheme within the same month and still observe significant treatment effects. Furthermore, since the scheme ended for all borrowers in September 2020, those in the August sub-group no longer had the option to avail or withdraw from the moratorium in subsequent months. Notably, this sub-group exhibits the largest treatment effects across all dependent variables. Hence, the observed results are unlikely to be driven by endogeneity arising from borrowers' decision to opt out.

To assess the external validity of our findings, we replicate the retrospective-versus-

prospective contrast using borrower-level panel data from the nationally representative CMIE Consumer Pyramids Household Survey (CPHS), which records monthly debt repayment expenses. We classify a borrower as treated if she held an outstanding formal loan in February 2020 and skipped her March 2020 repayment, so that her missed instalment could only have been absorbed into the moratorium retrospectively. Borrowers who continued to repay in March did not receive such a retrospective deferral, and could avail the moratorium only prospectively for later months (control).

Since CPHS does not record exact moratorium participation, we define two post-periods, Moratorium Phase (March to August 2020) and Post-Moratorium (September 2020 onward), and restrict the sample to borrowers with observed repayments after the moratorium period. We then estimate a difference-in-differences specification using monthly missed payments, and non-performing status, as outcomes. The test yields effects consistent with our main results, suggesting that the retrospective-deferral effect likely generalises beyond our primary lender and geography.

We then turn to mechanism. The natural candidate is moral hazard: a borrower whose missed instalment is retrospectively deferred by her lender may revise upward the probability that the lender will be similarly lenient in the future, particularly when regulators permit such forbearance during stress episodes, and may relax her repayment discipline accordingly. We test this in two ways.

First, we exploit the joint-liability feature of the lender's loans. Joint-liability lending substitutes for physical collateral with social collateral, a bundle of related forces that strengthens with the tenure of the group (Feigenberg et al., 2013). This social collateral includes peer monitoring among co-borrowers (Stiglitz, 1990, Besley and Coate, 1995, Agarwal et al., 2025), the rising opportunity cost of losing access to a group-based credit relationship (Chowdhury, 2005), and accumulated trust and reciprocity capital. We compare borrowers in their second or later JLG cycle (*Old JLG*) with borrowers in their first JLG cycle and Individual Liability borrowers (*New*). The retrospective deferral has a markedly more negative effect on New borrowers' subsequent loan performance, consistent with joint-liability's social collateral attenuating the behavioural response to updated beliefs about future leniency. The pattern is not driven by selection of better borrowers into later JLG cycles: the treatment effect can be observed within these borrower types, i.e. borrowers with older joint-

liability group loans show a lower treatment effect, and not just lower overall default levels, compared to new groups. This disciplining force operates beyond the formal joint-liability structure: the treatment effect is also significantly muted in branches whose borrowers are more socially interconnected, measured by the within-branch concentration of pre-Covid caste, religion, and occupation, consistent with community-level peer monitoring restraining the moral-hazard response.

Second, we examine how the treatment effect varies with the length of the moratorium. Take-up was near-universal (99.6%), so the relevant variation is on the intensive margin. Using a triple-interaction specification, we find that treated borrowers who stayed in the moratorium longer show substantially larger treatment effects, consistent with the hypothesis that each additional month spent in moratorium reinforces the borrower's expectation of future lender leniency. A natural concern is that borrowers more severely affected by the crisis self-select into longer moratoria, so that the length pattern reflects crisis exposure rather than moral hazard. We therefore classify borrowers as more vs. less affected by the crisis using occupation-level income drops and household income. Contrary to the crisis-impact explanation, the length effect is *larger* for the less-affected group, supporting moral hazard as the driver.

We then rule out three alternative explanations for the treatment effect. (i) The treated group saved one March instalment in cash; this extra liquidity could have funded post-moratorium overspending. We define *Saved More* as an indicator for borrowers with above-median instalment-to-household-income ratio and find no significant interaction in a triple-difference, ruling out a liquidity-windfall channel. (ii) Borrowers in the moratorium longer accrue more interest and end with larger outstanding balances, which could mechanically raise post-moratorium default. We confirm the higher accrual but show that controlling for it leaves the treatment effect intact, likely because the lender extended loan tenors to absorb the additional interest. (iii) Borrowers with a higher propensity to default may cluster towards month-end, so that the treated group's late-month due dates pick up unobserved borrower quality rather than the deferral itself. We check this alternate mechanism by conducting symmetric within-group placebo tests.

These tests rule out anticipation and date-clustering of borrower quality in a single exercise. We re-estimate the main specification inside the original treated and control groups,

with new placebo cutoffs (27 March within the treated window, 20 March within the control window) labelled *Placebo-within-Treated* and *Placebo-within-Control*. Both panels return coefficients statistically indistinguishable from zero. The within-treated split falls on 27 March, the date of the RBI's enabling notification, so its null is especially informative for anticipation. Borrowers in the original treated group due 25 to 27 March missed payment before any RBI permission existed; borrowers due 28 to 31 March missed payment after the RBI notification but well before the lender exercised its discretion to grant the moratorium retrospectively and notified the same in the first week of April. For neither subgroup did the lender's decision predate the missed instalment, so by construction no treated borrower had any commitment of relief at the moment of non-payment. A channel that operates through ex-ante anticipation predicts a significant Placebo-within-Treated coefficient and a non-trivial Placebo-within-Control coefficient; the retrospective-deferral channel predicts neither. The data deliver the second pattern, providing direct empirical support for the framing of the natural experiment laid out earlier.

This paper contributes to the literature on debt relief and borrower behaviour. The literature has examined this relationship under many settings, and found that permanent waivers and durable rate cuts are seen as prone to strategic default, while temporary suspensions are viewed as benign because they preserve the borrower's eventual obligation to repay (Indarte and Kanz, 2024, Ferreira et al., 2025, Kanz, 2016, Mayer et al., 2014). We focus on the implementation design of the relief. The delivery structure itself can induce moral hazard even in temporary debt relief programmes. Our thesis is that the borrower's expectation of future leniency depends on whether the lender's first act of relief is prospective or retrospective. We show that the strategic-default response long associated with permanent waivers also arises under temporary relief, if that relief is delivered retrospectively.

What makes this striking is that the relief itself is economically trivial: a single instalment, deferred to the end of the loan tenor and repaid in full, with no principal forgiven and no interest on interest accruals. Even this one-month retrospective deferral durably weakens repayment, pointing to the structure of the relief, not its magnitude, as what distorts borrowers' beliefs about future lender leniency.

Our paper builds on the vast literature studying how debt relief reshapes repayment incentives. Kanz (2016) follows India's 2008 agricultural debt waiver and finds that forgive-

ness did little for investment or productivity, and that beneficiary households leaned more heavily on informal credit afterwards. Giné and Kanz (2018) trace the same programme's credit-market effects and show that defaults not only rose but became far more sensitive to the electoral cycle, as borrowers began to anticipate the next round of relief. Mayer et al. (2014) pin down the mechanism in a very different setting: once Countrywide announced mortgage modifications for delinquent borrowers, previously paying borrowers strategically stopped paying in order to qualify. O'Malley (2021) makes the same point from the enforcement side, showing that when an Irish court ruling unexpectedly removed lenders' ability to repossess certain homes, default rose sharply. The evidence is not uniform; well-targeted permanent modifications under the U.S. Home Affordable Modification Program actually reduced re-default (Agarwal et al., 2017). This work establishes that borrowers respond to the leniency they come to expect, but in each case the relief studied is permanent, unusually generous, or removes enforcement altogether.

A newer literature turns to temporary relief and reaches a gentler verdict. Dinerstein et al. (2024) study the 2020 U.S. student-loan freeze and find that borrowers largely used the freed-up cash to take on other debt, with no deterioration in repayment discipline; Kim et al. (2024) similarly find that CARES Act mortgage forbearance reduced delinquencies on borrowers' other obligations. Ganong and Noel (2020) showing that mortgage default responds to short-run liquidity rather than long-run wealth, help explain why a brief payment pause can do real good. Yet how the relief is designed turns out to matter. Aydin (2024) randomises debt relief and finds that borrowers default in response to changes in their future payments that are unrelated to their current liquidity, a signature of strategic behaviour. Closest to our setting, Fiorin et al. (2023) offer Indian microfinance borrowers a moratorium framed as coming either from their lender or from the regulator, and find that the lender-framed offer improves repayment while the regulator-framed one does not. What none of these designs varies is whether the relief arrives before or after the borrower has missed a payment. That is the margin we isolate, and we add to this stream by showing that even a single retrospective deferral is enough to shift beliefs about future leniency, in the direction the strategic-default tradition predicts but the benign view of temporary relief does not.

We make two further contributions. First, our research design separates the response to relief from the response to the crisis which necessitates the forbearance in the first place

(Kanz, 2016, Giné and Kanz, 2018, O'Malley, 2021). We exploit a contractual due-date discontinuity: borrowers are split into treated and control purely by the calendar day their instalment fell due. Within this design we estimate a dose-response on moratorium length, with the treatment effect rising monotonically with moratorium length. This pattern is consistent with belief-updating rather than crisis exposure. Second, we add more evidence to the literature that the social collateral of joint-liability lending curtails strategic response in debt repayment (Stiglitz, 1990, Besley and Coate, 1995, Feigenberg et al., 2013, Agarwal et al., 2025).

Overall, our findings can be used to refine the implementation of temporary debt relief. We show that the distortion arises not from the relief itself but from delivering it retrospectively, as an after-the-fact concession on an instalment the borrower has already missed. Relief structured prospectively, as a rule-bound liquidity facility announced before payments come due, can still serve distressed borrowers without eroding their repayment discipline (Indarte and Kanz, 2024, Ganong and Noel, 2020).

The remainder of the paper proceeds as follows. Section 2 outlines the institutional context of our study and describes the sample. Section 3 draws out the empirical research design and defines the corresponding variables. Section 4 presents the results of the empirical analysis and robustness tests. Section 6 suggests moral hazard as the potential mechanism, and Section 7 rules out alternate mechanisms. Section 8 concludes.

2 Institutional Setting and Data

2.1 Government's response to Covid-19 and Moratorium Scheme

India's Covid-19 shock began as a public-health emergency and quickly became a widespread economic crisis. Cases were rising sharply globally in March 2020, and the contagion was likely to start spreading in India if left unchecked.[4] On the evening of 24 March 2020, the Government of India announced a 21-day nationwide lockdown, effective 25 March and

[4]Figure 1 in WHO Covid-19 Situation Report 71 - https://www.who.int/docs/default-source/coronaviruse/situation-reports/20200331-sitrep-71-covid-19.pdf

scheduled to end 14 April.[5] It was later extended in phases through May, with relaxations from June. The lockdown barred all non-essential activity. Debt repayment was not exempt. The top panel of Figure 1 shows how sharply work-related mobility fell in the last week of March, and how slowly it recovered.

The lockdown halted all collections by the lender. In its microfinance model, borrowers pay their instalments in cash at in-person group meetings. The lockdown suspended all physical gatherings. A borrower whose instalment fell due that week therefore had no way to pay, however willing or able. Figure 1 shows the collapse of repayment by treated group in March.

On 27 March 2020, the regulator, i.e. the Reserve Bank of India (RBI) issued a Regulatory Package to address the mass defaults. It permitted lenders to grant a three-month moratorium on instalments due between 1 March and 31 May 2020, later extended through 31 August.[6] The circular left the implementation details to each lender. For example, the lender could decide whether to grant relief, and for how long within the permitted window. In the first week of April 2020, the lender notified that it will defer the March instalment of every borrower who is not already in default, but had missed the repayment for March. Other borrowers who were not in default as of March 2020, were also offered the moratorium from April. Treated borrowers thus had exactly one extra instalment, their already-due March EMI, deferred relative to controls.

A borrower who could not be reached, or who stayed silent, was enrolled by default. A borrower who wished to keep paying continued under the original contract. Borrowers were notified by phone message, voice call, or if they visited a branch. The scheme ran in two phases under separate policies, covering 1 March to 31 May and 1 June to 31 August 2020. A borrower could opt out or back in across phases until 31 August. The loan tenor was extended by the number of months availed. A deferred instalment added no overdue days, drew no penalties, and was not reported to credit bureaus. Each borrower's days-past-due and asset classification were frozen at their 1 March 2020 level. Only regular interest

[5] Press Information Bureau, Government of India, "PM addresses the nation for 8th time in 4 Weeks," Press Release, 24 March 2020.

[6] RBI Notification DOR.No.BP.BC.47/21.04.048/2019-20 dated 27 March 2020, "COVID-19 – Regulatory Package" (https://www.rbi.org.in/Scripts/NotificationUser.aspx?Id=11835), subsequently extended by RBI Notification DOR.No.BP.BC.71/21.04.048/2019-20 dated 23 May 2020 covering instalments falling due between 1 June 2020 and 31 August 2020.

kept accruing, recovered over the remaining tenor or as a final instalment. Take-up was near-universal, at 99.6%, as the lender had expected.

The Government of India also distributed free food grains to low-income households. Unlike many countries, it offered no direct cash transfer that might have funded repayment. Either way, this relief could not have reached the treatment and control groups differently. However, this policy could not have applied differently to the treatment and control groups.

2.2 Data and Sample Selection

We collect loan-transaction-level data from a large non-banking financial company (NBFC) in India. Unlike banks, NBFCs cannot take deposits, so they borrow to lend on, which raises their interest rates, and they face lighter regulation, notably less stringent default-reporting norms compared to the large banks (the Scheduled Commercial Banks).

The data provider operates in six Indian states: Tamil Nadu, Karnataka, Odisha, Jharkhand, Chattisgarh, and Uttarakhand. As shown in Appendix Figure A1, these states are located in different corners of India. From this lender, we collected demographic and monthly repayment data for borrowers with one loan each. We asked the lender to provide data only for those borrowers who repaid every month during September 2019 and March 2020 (good borrowers). We further restricted the sample to the borrowers in the treatment group who were punctual payers in January and February 2020 and those in the control group who were punctual payers in January, February, and March 2020 (punctual borrowers). Punctual payers are borrowers who repaid between $\pm$ 3 days of the due date. We use the three-day relaxation to provide concessions for public holidays, weekends, etc. The monthly due date of each loan is assumed to be the same day every month as the day of disbursement, e.g., a loan that was disbursed on 17 January will have its monthly repayments due on 17 February, 17 March and so on.

We put these restrictions on the borrowers as they are essential to maintain the sanctity of the research design. The first restriction is to look at only good borrowers. If we keep bad borrowers in the sample, they might end up in the treatment group disproportionately, and confound the results. To understand this point, think about a borrower who has a history of being delinquent and was not planning to repay in March 2020. When the lockdown

was implemented on 25 March 2020, all such borrowers would have automatically received a moratorium and would have ended up in the treatment group. To avoid such confounding, we restrict the sample to only good borrowers, i.e., borrowers with a recent history of not missing any repayments. The second restriction is to include only borrowers with a stable repayment date. As the exact contractual due date is unobserved, we keep borrowers who have made their payments on the same calendar day in each of the past few months. This consistency lets us infer the scheduled repayment date and assign borrowers unambiguously to treatment or control groups.

The final sample has a total of 17,864 borrowers, out of which 99% belong to the state of Tamil Nadu. 6,325 borrowers fall in the treatment group, and 11,539 borrowers in the control group. The coverage summary of this dataset is documented in Panel A of Table 1. Each borrower has one loan only. 97% of these loans are joint-liability group (JLG) loans, i.e., the borrowers form groups of four or five while borrowing. Each member in the group gets an individual loan and is responsible for repaying it fully. However, the joint-liability clause means that even if one member of the group defaults, the entire group is termed a defaulter. All banking transactions of the group occur in the monthly group meeting with the loan officer. The remaining 3% loans are individual liability loans. All loans in this sample are unsecured loans.

The loans are repayable in equated monthly instalments, and we observe each borrower's monthly repayments (and hence, non-repayments) in the data. Therefore, the final data set is organised at a loan-month level and amounts to 300,185 borrower(loan)-year-month observations. Each observation also contains information on loan terms, such as the loan amount, loan tenure, interest rates, and loan purpose. The lender also collects borrower-specific information relating to age, gender, marital status, income, and occupation. These borrowers are distributed over 206 branches of the lender.

The lender primarily operates in the micro-finance space and lends to low-income rural women for business as well as consumption purposes. We list the summary statistics of the loan terms and demographic details of the borrowers as observed in February 2020 in Appendix Table A1. The average annual individual income of the borrowers in this sample is INR 96,797 (USD 1345)[7]. This is very close to the average income of women borrowers

[7]As per February 2020 conversion rate of INR 72/USD.

in India, which is INR 94,489, calculated using incomes winsorised at five and ninety-five percent, from the CPHS survey in the same time period. 99% borrowers in the sample are females with a median age of 40 years, and 96% of them are married. The median loan amount is INR 40,000 at an interest rate of 25% (median) for a tenure of 2 years (median). 59% of these loans are borrowed for business purposes, and the remaining for consumption.

In Panel B of Table 1, we document how the borrowers and their loans differ across treatment and control groups. Notice that the two groups are quite similar in most factors. This similarity reinforces the quasi-random nature of the natural experiment we are exploiting in this study. Even for the variables where the two groups are statistically different, the economic magnitude of the difference is quite low, e.g., the monthly repayable amount across the two groups differs by INR 25, which is 0.3% of their average monthly income of $\sim$ INR 8000. The two groups are also indistinguishable in terms of joint-liability group loans and individual liability loans. Where statistically significant differences do appear, the two most pricing-relevant signals, interest rate and the business-versus-consumption-loan classification, both run in the direction of the treated group being marginally *safer* ex-ante: treated borrowers face a 14-basis-point lower interest rate and have a 9-percentage-point higher share of business loans. Observable ex-ante risk therefore biases against, rather than towards, our finding of higher post-moratorium delinquency in the treated group.

The two groups are also similar in terms of their missed-instalment rates in the period before the lockdown took effect. Missed instalments, and more severe delinquencies that follow from them, affect a borrower's credit score, as well as the propensity to receive new loans in the future. Missed instalments on joint-liability loans can also bring social sanctions to the borrower (Besley and Coate (1995)). This similarity exists because we dropped all borrowers who missed even a single repayment between September 2019 and February 2020. So, the monthly and quarterly non-repayments are precisely zero for each group in the period before the crisis. However, we document the difference in the percentage of repaid instalments during the same period and report the results in Panel C of Table 1. We find that the treatment group has a slightly higher repayment rate in the pre-moratorium period than the control group. Note that Missed Instalment and NPA are zero for all borrowers by construction.

For external validity tests, we draw on a nationally representative survey: the Centre for

Monitoring Indian Economy's (CMIE) Consumer Pyramids Household Survey (CPHS) for the period of the sample. It records respondents' monthly income, occupation, expenditures (including debt repayments), and sources of debt, among other details. We also use the CPHS data to calculate the drop in income after the lockdowns to measure the occupation-wise impact of the crisis.

3 Empirical Strategy

We want to measure how the delivery of temporary relief (retrospectively for an instalment already missed, or prospectively for one not yet due) shapes a borrower's subsequent repayment. A naive comparison would mix the effect of the design with differences in borrowers, lenders, and the severity of the shock. We overcome this with a natural experiment that holds all three fixed.

Monthly repayment dates are set at loan origination, typically months or years before the pandemic. When the Government of India imposed a nationwide lockdown on 25 March 2020, it split otherwise identical borrowers by nothing more than the contractual day on which their instalment fell due. Borrowers due in the lockdown week (25–31 March) could not pay, and the lender later absorbed their already-missed March instalment after the fact. We call these the treatment group. Borrowers due the week before (18–24 March) had paid March under the original contract, and met the scheme only prospectively, for later instalments. We call these the control group. From April onward, both groups participated in the same scheme through August 2020.

The lockdown date thus varied only the design of a borrower's first contact with relief: retrospective for the treated, and prospective for the control, while holding the crisis fixed across the two groups. We measure the effect of this difference in design on subsequent repayment.

We use loan repayment, loan terms and borrower demographics data from a large NBFC in India. This data spans a six-month period before the beginning of the moratorium scheme (Sep 2019 to Feb 2020), six months during the moratorium period (Mar 2020 to Aug 2020), and six months after the moratorium scheme ended (Sep 2020 to Feb 2021). Data is organised

at the borrower-year-month level, which is equivalent to the loan-year-month level in this case, as each borrower only has one loan.

To assess the impact of the retrospective March deferral on subsequent loan performance, we estimate the following difference-in-differences linear regression model:

$$\begin{aligned} Y_{it} =& \beta_1 \times Treated_i + \beta_2 \times Post_{it} + \beta_3 \times Treated_i \times Post_{it} \\ &+ X_{it} + X_{it} \times Post_{it} + \nu_i + \mu_t + \varepsilon_{it} \end{aligned} \tag{1}$$

We use three dependent variables (Y_{it}) in this study, *viz.*, $Repayment_{it}$, $Missed\ Instalment_{it}$, or *Non-Performing Asset* (NPA_{it}). The subscript i represents a borrower, and the subscript t represents each year-month. *Repayment* is the proportion of the monthly debt repayment obligation paid by a borrower in that month. It ranges from no payment (zero) to full payment (one). *Repayment* is adjusted to one for months where a borrower took moratorium. It proxies for full repayment for that month. *Missed Instalment* is an indicator variable that equals one if a borrower makes no repayment in a month and zero otherwise. It captures a single month of non-payment, and should be distinguished from the regulator's formal definition of default (typically 90 days past due). *Missed Instalment* also equals zero for the months when the borrower took a moratorium.

NPA is another indicator variable that equals one if a borrower misses instalments for three consecutive months, consistent with the Reserve Bank of India's 90-days-past-due definition of a non-performing asset, and zero otherwise. *NPA* is automatically adjusted for the moratorium as it is constructed from the *Missed Instalment* variable. $Post_{it}$ is an indicator variable that equals one for every month after the moratorium is over for a borrower and zero before that. This variable also has the i, t subscript as each individual borrower could opt out of the moratorium scheme in a different month during April 2020 and August 2020.

The primary independent variable here is $Treated_i$, an indicator variable. It equals one for a borrower whose monthly repayment is due between 25 March 2020 and 31 March 2020, and zero for borrowers whose monthly repayment was due in the week before 25 March 2020 (i.e., between 18 March 2020 and 24 March 2020). X_{it} represents borrower characteristics and loan terms. Borrower characteristics are gender, marital status, age,

individual income, household income, and occupation category (agriculture, non-agriculture, salaried, and others). Loan terms include the loan amount, tenure, interest rate, and loan purpose (business and consumption). ν_i and μ_t stand for borrower fixed effects and year-month fixed effects, respectively.

We control for the time-invariant borrower and loan-level characteristics through the borrower fixed effects. To control for the time-varying effects of these factors, we interact the control vector X_{it} with the $Post_{it}$ variable and include them in the secondary regressions. The primary regressions do not include the control vector. We cluster all standard errors at the lender's branch level.

The borrower fixed effects ν_i play a substantive identification role: $Treated_i$ is collinear with ν_i and is absorbed, so the only way treatment enters the regression is through $Treated_i \times Post_{it}$, whose coefficient is identified entirely from within-borrower changes between the pre- and post-moratorium windows. Any time-invariant difference between treated and control borrowers, observable or unobservable, is therefore differenced out by construction; what remains can only be a difference in how each borrower's own outcomes evolve between the two windows.

4 Main Result

We run the difference-in-differences regression mentioned in the previous section and report the Ordinary Least Squared (OLS) coefficient estimates and their respective standard errors in Table 2. Column 1 shows the results for the dependent variable, Repayment, without including the additional control variables. This regression includes only borrower fixed effects and time (year-month) fixed effects. The main variable of interest here is $Treated \times Post$, and its coefficient estimate shows that the *Treated* group had an 8.8 percentage points lower repayment rate compared to the control group after the moratorium ended. In column 3, we show results with *Missed Instalment* as the dependent variable. The *Treated* borrowers show 8 percentage points higher missed-instalment rates compared to the control group borrowers in the post-moratorium period. Similarly, when we use NPA as the dependent variable in column 5, the *Treated* group shows a higher NPA rate by 2.2 percentage points. Note that the *Post* dummy can vary across borrowers, and hence it's coefficient is estimated despite

the year-month fixed-effects.

In columns 2, 4, and 6, we estimate regression equation 1 for Repayment, Missed Instalment, and NPA as dependent variables, respectively, after including additional controls. These controls are loan terms, borrower characteristics, and their interaction with the *Post* dummy. We also include Occupation × Post fixed effects that absorb the estimates corresponding to the *Post* dummy. The results remain similar even after including control variables. The estimates are also similar when we replace the borrower-specific $Post_{it}$ with a scheme-wide calendar $Post_t$ that equals one from September 2020 onward for all borrowers (Appendix Table A4). The results also survive a narrow-bandwidth restriction that compares only borrowers due 22–24 March to those due 25–27 March, shutting down any role for confounders that accumulate over the wider treated–control gap (Appendix Table A13).

In the following subsections, we describe several tests we ran to check the robustness of our results.

4.1 Pre Trends

As we run a difference-in-differences regression model, we must rule out the possibility of a pre-existing differential trend for the Treated and Control groups. To rule out pre-trends, we plot the average repayment rates of treated and control borrowers for each month between September 2019 and February 2021 in the bottom panel of Figure 1. Here the repayment rates are not adjusted for the moratorium. We plot moratorium-adjusted Repayment, Missed Instalment, and NPA in Appendix Figure A3.

We also see the absence of pre-trends in the month-wise coefficients plotted in Figure 2. Here, we run the full-regression specification mentioned in equation 1 after changing the Treated × Post to Treated × $\mathbb{1}$.YearMonth. This change produces month-wise coefficients of the Treated variable after controlling for loan terms, borrower characteristics, and time fixed effects. The top panel of Figure 2 has Repayment as the dependent variable. Similarly, the middle and bottom panels have Missed Instalment and NPA as the dependent variables, respectively. All of them rule out pre-trends, and confirm post-period deterioration in repayment discipline.

4.2 Matched Sample

The treated and control groups are largely similar to each other in terms of observable characteristics, as shown in Panel B of Table 1. Although the small statistically significant differences are economically minor and the two most pricing-relevant ones (interest rate and business-versus-consumption loan) run in the safer direction for the treated group (Section 2.2), we directly rule out any residual role for observable ex-ante-risk imbalance by re-estimating the main specification on a 1:1 Mahalanobis-distance propensity-score-matched sample. The covariate balance of the matched sample is reported in Appendix Table A2.

We run the difference-in-differences regressions on this matched sample as per equation 1 and report the OLS coefficient estimates in Table 3. Columns 1 and 2 show the results for the dependent variable, Repayment. Columns 3 and 4 show results with Missed Instalment as the dependent variable. Columns 5 and 6 show the result for NPA as the dependent variable. All regressions include borrower fixed effects and time (year-month) fixed effects. Columns 2, 4, and 6 have additional control variables like loan terms, borrower characteristics, and their interaction with the Post dummy, as well as Occupation $\times$ Post fixed effects. The coefficients estimated on this matched sample are quite similar to those obtained on the unmatched sample, ruling out the possibility that the headline effect reflects a differential concentration of observably riskier borrowers in the treated group.

4.3 Alternate Measures of Delinquency

For most of this study, we used dependent variables that are constructed from the monthly repayment data. The lender also provided us with its own delinquency measures: (1) *1 to 29 Days Overdue*, and (2) >= *30 Days Overdue*. We also combined these two delinquency measures to create another measure that captures >= *1 Day Overdue*. Panel A of Table 4 reports the OLS coefficient estimates of the regression equation 1 with *1 to 29 Days Overdue* as the dependent variable in columns 1 and 2. Columns 3 and 4 have >= *30 Days Overdue* as the dependent variable, and columns 5 and 6 have >= 1 *Day Overdue* as the dependent variable. Columns 1, 3, and 5 show estimates after running the regression without the control variables, and columns 2, 4, and 6 show coefficient estimates after controlling for the borrower and loan-level factors. As expected, the combined measure >= *1 Day Overdue* gives

estimates that are very close to those found with the monthly *Missed Instalment* measure we defined in Section 3.

4.4 Loan Performance At Maturity

We also check whether the delinquencies measured above are transitory or last beyond the maturity of the loan. We take a subsample of loan-level repayment data observed at maturity or after the maturity date in the post-moratorium period using the following regression.

$$Y_{it} = \beta_0 + \beta_1 \times Treated_i + X_{it} + \mu_t + \varepsilon_{it} \tag{2}$$

We use two dependent variables here, *Non-Performing Asset* (NPA_{it}), and *>= 30 Days Overdue* as defined in the previous subsection. The subscript i represents a borrower, and the subscript t represents each year-month. The primary independent variable here is $Treated_i$, and X_{it} represents borrower characteristics and loan terms. These variables are defined in Section 3. μ_t stands for year-month fixed effects. We cluster all standard errors at the lender's branch level.

The coefficient estimates for this regression are reported in Panel B of Table 4 with *Non-Performing Asset (NPA)* in columns 1 and 2, and *More Than 30 days Overdue* in columns 3 and 4 as the dependent variables. Using each dependent variable, we find that the treatment effect is not transitory and persists beyond the maturity of the loan.

4.5 Retrospective Deferral vs. Seven-Day Head Start

A reader could imagine the treatment as a quantitative head start: treated borrowers received roughly seven additional days of relief because the lender's policy absorbed their March instalment, whereas control borrowers paid in March and only entered the scheme in April. Under this interpretation, the contrast between treated and control would be one of dose, a few extra days of forbearance, rather than one of kind. We check if the data are consistent with this interpretation, or with the retrospective-deferral framing developed in Section 3.

We construct five monthly sub-groups based on the final month in which borrowers

opted for the moratorium, from April through August 2020. Within each sub-group, treated and control borrowers differ only in whether they received the moratorium in March 2020, with identical deferral patterns in subsequent months. Appendix Table A7 documents the construction and size of these sub-groups. We then estimate regression equation 1 separately for each subgroup, with results presented in Table 5. Panels A, B, and C show outcomes for Repayment, Missed Instalment, and NPA, respectively.

If the seven-day head start were the main driver, the treatment effect should shrink as total moratorium duration grows (e.g., 7/30 days for the April-only sub-group vs. 7/150 days for the April–August sub-group). Instead, we find the opposite: treatment effects grow with longer moratorium use, with the repayment gap rising from 1.1 percentage points in the April-only subgroup to 19.2 percentage points in the August subgroup. This pattern is consistent with the retrospective-deferral argument. Each additional month the treated borrower spends in moratorium compounds the salience of the deferral rather than diluting it. We compare the coefficients precisely with triple-difference regressions of treatment, post, and length of moratorium in Section 6.2.

5 External Validity

Our main sample is restricted only to punctual borrowers. We relax this constraint and run the main test on the larger sample of good borrowers obtained from the lender. We report the results in the Appendix Table A3. The coefficient estimates are similar to those obtained in the main test reported in Table 2. Appendix Figure A2 confirms that the treated and control groups follow parallel repayment trends before the moratorium in this larger sample as well.

Even this sample comes from a single lender and therefore represents only the specific borrower segments that the institution serves. To assess whether our results generalise more broadly, we draw on the nationally representative CMIE Consumer Pyramids Household Survey (CPHS), which records respondents' monthly income, expenditures (including debt repayments), and sources of debt, among other details.

In our external validity test, we replicate our primary test using CPHS data. We begin

by selecting borrowers who reported an outstanding loan from a formal lender like a bank, shadow bank or microfinance institution (MFI) in the quarter before the crisis started, and made at least one debt repayment in February 2020, just before the Covid-19 lockdown. Note that the RBI's moratorium scheme was applicable to loans taken from formal lenders. We define the Treatment group as borrowers who skipped their March 2020 payment. As in our main design, their already-due March instalment could only have entered a moratorium *retrospectively*, after it had been missed. We define the Control group as borrowers who paid in both February and March 2020, for whom any later moratorium participation was necessarily *prospective*. The CPHS data do not record the timing of each lender's moratorium decision, so this split captures the retrospective-versus-prospective contrast more coarsely than our administrative sample; still, treated borrowers are far more likely to have experienced a retrospective deferral of an already-missed instalment, and control borrowers a purely prospective one.

Unlike our main sample, we do not know when the borrowers in the survey data opted out of the moratorium. So, we classify two post periods: *Moratorium Phase*, which runs when the moratorium scheme was running, i.e. March to August 2020, and *Post Moratorium*, which runs after the moratorium scheme is over, i.e., September 2020 onwards. Moreover, it is possible that those borrowers who did not pay in March 2020 had paid off their debt with their final payment in February. To account for that, we drop the borrowers who made no repayments in the post-moratorium period. We run the following regression on this new borrower-time panel data that runs from September 2019 to December 2020:

$$\begin{aligned} \textit{Missed Instalment}_{it} = &\beta_1 \times Treated_i + \beta_2 \times MoratoriumPhase_t + \beta_3 \times PostMoratorium_t \\ &+ \beta_4 \times Treated_i \times MoratoriumPhase_t + \beta_5 \times Treated_i \times PostMoratorium_t \\ &+ \nu_i + \mu_t + \varepsilon_{it} \end{aligned} \tag{3}$$

$\textit{Missed Instalment}_{it}$ equals one if there is no debt repayment recorded in a month by the borrower. ν_i represents borrower fixed effects, and μ_t represents year-month fixed effects. We report the results of estimating Equation 3 in column 1 of Table 6, and find that treated borrowers are more likely to miss instalments after the moratorium scheme was withdrawn. Then, we run this regression after replacing the borrower fixed effects with district × year-month fixed effects, and report the coefficient estimates in column 2. The treated group

had a lower missed-instalment rate before the lockdown, which increased relative to the control group after the moratorium scheme was withdrawn. Columns 3 and 4 repeat both specifications with *NPA*: an indicator equal to one when a borrower records no repayment for three consecutive months, as the outcome, and deliver the same pattern. The results mirror our main finding and indicate that the retrospective-deferral effect generalises beyond our single lender to a nationally representative sample.

6 Potential Mechanism: Moral Hazard

The previous sections document the negative effect of the retrospective March deferral on subsequent repayment. The candidate mechanism is moral hazard: a borrower who learns that her lender will retrospectively defer a missed instalment without penalty may default more subsequently, in expectation of similar leniency. We test this in two ways. First, we compare the treatment effect across borrowers with differing exposure to joint-liability's social collateral, which should lower moral hazard. Second, we examine how the effect varies with the intensity of the crisis borrowers experienced, which should increase moral hazard.

6.1 Joint-Liability's Social Collateral

To empirically test whether moral hazard drives the results, we exploit the joint-liability feature of the lender's loans. Joint-liability lending substitutes for physical collateral with social collateral, a bundle of related forces that strengthen with the tenure of the joint-liability group. These forces are peer monitoring among co-borrowers (Stiglitz, 1990, Besley and Coate, 1995, Agarwal et al., 2025), the rising opportunity cost of losing access to a group-based credit relationship that has compounded over multiple cycles, and accumulated trust and reciprocity capital within groups that have met repeatedly (Feigenberg et al., 2013). These forces are co-moving components of the social collateral that joint-liability lending creates or harnesses, each of which attenuates the borrower's behavioural response to updated beliefs about future leniency. We therefore expect the treatment effect to be muted among borrowers whose social collateral has had longer to accumulate.

We compare borrowers in their second or later cycle of Joint-Liability Group (JLG) loans

(*Old JLG*) with borrowers in their first JLG cycle and Individual Liability (IL) borrowers (*New*). We club first-cycle JLG and IL borrowers together because both groups have low exposure to social collateral: first-cycle JLG borrowers have not yet accumulated the social collateral that a repeated group develops, and IL borrowers do not borrow under joint-liability at all.

In Table 7, we report the regression results for equation 1 for New and Old JLG borrowers separately in columns 1 and 2, respectively. The treatment effect exists for both the subsamples, but it is much lower for Old JLG borrowers. To formally compare the treatment effect across the two groups, we modify regression equation 1 to the following regression:

$$\begin{aligned} Y_{it} =& \beta_1 \times Treated_i + \beta_2 \times Post_{it} + \beta_3 \times Old\ JLG_i + \beta_4 \times Treated_i \times Post_{it} \\ &+ \beta_5 \times Old\ JLG_i \times Post_{it} + \beta_6 \times Old\ JLG_i \times Treated_i \\ &+ \beta_7 \times Old\ JLG_i \times Treated_i \times Post_{it} + X_{it} + X_{it} \times Post_{it} + \nu_i + \mu_t + \varepsilon_{it} \end{aligned} \tag{4}$$

Here, the definitions of the variables are the same as in Section 3. *Old JLG* is an indicator variable that equals one for the borrowers whose Joint-Liability Group loan is in the second cycle and zero if it is the first cycle or an Individual Liability loan. JLG Cycle is the number of joint-liability loans taken by a borrower. A first-time JLG loan borrower is in her first JLG cycle. When she borrows a second JLG loan, she enters her second JLG cycle. The JLG cycle keeps increasing with a number of subsequent JLG loans. The results of the above regression are reported in column 3 of Table 7. These regressions include loan terms, borrower characteristics (including occupation dummies), and their interactions with the Post dummy as controls. Column 4 reports results for a similar regression after replacing *Old JLG* with the exact *JLG Cycle* of the joint-liability borrower. *JLG Cycle* equals zero for individual liability borrowers, and increases by one for each subsequent JLG loan. In this sample of borrowers, the highest JLG cycle is ten.

The results in Table 7 confirm that though the treatment effect exists among both groups, it is stronger for New borrowers. The retrospective March deferral has a more negative effect on the repayments of New borrowers, who have limited exposure to joint-liability's social collateral, than on those of Old JLG borrowers, for whom this social collateral has had longer to accumulate. The pattern is consistent with social collateral attenuating the

borrower's behavioural response to updated beliefs about future lender leniency. Because the comparison identifies social collateral as a whole, the test does not pin down the share of attenuation attributable to peer monitoring *versus* the opportunity cost of losing the group *versus* accumulated trust and reciprocity. But for the purpose of identifying moral hazard as the operative behaviour, rather than decomposing the disciplining forces that constrain it, social collateral is the relevant unit of variation.

A concern can arise that, though individual-liability borrowers lack social collateral altogether and share similarities with first-time joint-liability borrowers in their limited exposure to it, one should consider splitting them separately in the analysis. By doing so, one can assess whether the results of this test are primarily driven by individual borrowers and explore potential differences between individual and joint loans that may be correlated with repayment decisions. The number of Individual Loan borrowers is small (3%), so we combine Individual Liability Loans with the first cycle of JLG Loans as a single "low social collateral" category. Regardless, Appendix Table A5 shows results for Individual *vs.* Joint Liability Group (JLG) loans separately. The results are directionally similar to those of New *vs.* Old borrowers.

A second concern could be that Old JLG borrowers are the "good type" by selection: only good borrowers graduate to the second or higher cycle of joint-liability loans, and as a result they might default less anyway. We note, however, that the treatment effect is observable *within* these borrower groups. That is, Old JLG borrowers show a lower treatment effect, and not just lower overall default levels, than New borrowers. This is consistent with social collateral disciplining the moral-hazard response, over and above any selection on borrower type.

As a complementary test of the same social-collateral logic that does not rely on the joint-liability structure, we ask whether the treatment effect is muted in branches whose borrowers are more socially interconnected. We measure branch-level social interconnectedness as the average of three within-branch Herfindahl concentration indices computed over borrowers' pre-Covid caste, religion, and occupation; higher values denote more homogeneous, and hence more tightly connected, communities in which peer monitoring and reputational concerns are stronger. Appendix Table A6 reports the results. Among treated borrowers, greater interconnectedness significantly attenuates the post-moratorium rise in

NPA, whereas the same interaction is small and statistically insignificant for the control group. The triple-difference confirms that the attenuation is specific to the treated group. The pattern mirrors the joint-liability result and reinforces the interpretation that social collateral, likely operating here through community-level peer monitoring, disciplines the moral-hazard response to the retrospective deferral.

6.2 Length of Moratorium

We further investigate the presence of moral hazard by analysing how the treatment effect varies with the duration of the moratorium. As detailed in Section 2.1, the moratorium scheme included an opt-out option, allowing borrowers to discontinue the deferral at any point until August 2020. Initially, the RBI instructed lenders to offer payment deferrals from March 2020 to May 2020 and later extended it by three months until August 2020. This extension may have led borrowers to anticipate further extensions, potentially fostering expectations that could encourage moral hazard. We hypothesise that borrowers who availed the moratorium for a longer period were more likely to form such expectations and thus exhibit a higher propensity for moral hazard.

One might expect that, because both groups ultimately participate in the same scheme, their beliefs should converge and the treatment effect should *shrink* with moratorium length. However, the two groups update on qualitatively different signals, and each additional month a treated borrower spends in moratorium reinforces that signal, whereas for a control borrower it is only an extension of an opt-in arrangement she could exit. Longer moratoria should therefore *amplify* the treated–control gap, generating a positive dose–response; the triple- and quadruple-interaction tests show this dose–response is concentrated among less-affected borrowers, for whom the effect cannot plausibly reflect liquidity-driven inability to repay.

We formalise the monthly sub-group analysis from Section 4.5 using a triple-interaction specification and report the results in Table 8. We replace *Old JLG* with *Moratorium Length* in regression equation 4, where *Moratorium Length* is a discrete variable taking values 0, 1, 2, 3, 4, or 5 corresponding to the number of months a borrower availed the moratorium, as described in Appendix Table A7. Column (1) confirms that longer moratorium uptake

is associated with larger treatment effects: each additional month of moratorium raises the treatment effect on NPA by 1.9 percentage points. Benchmarked against the post-moratorium sample mean NPA rate of 2.96% (Appendix Table A1, Panel C), a treated borrower who availed the full five-month moratorium experiences a 6.3 percentage point higher NPA rate than a comparable control borrower, more than double the post-moratorium sample mean.

A natural concern is that borrowers more severely affected by the crisis may self-select into longer moratoriums, so that the pattern in column (1) could reflect heterogeneous crisis exposure rather than moral hazard. To distinguish these explanations, we classify borrowers into *Most Affected* and *Less Affected* by the crisis. *Most Affected* is an indicator variable that equals one for borrowers with below-median household income whose CMIE Consumer Pyramids Dx occupation-level income dropped by more than the median change after March 2020, and zero otherwise; *Less Affected* complements *Most Affected*. Columns (2) and (3) re-estimate the column (1) specification on the *Less Affected* and *Most Affected* subsamples, respectively, and column (4) augments column (1) with a quadruple interaction with *Most Affected*.

Contrary to a crisis-impact explanation, the treatment effect rises more steeply with moratorium length for *Less Affected* borrowers than for *Most Affected* ones (columns 2 and 3). Column (4) confirms this: the quadruple-interaction term is negative, so each additional month of moratorium raises the treatment effect by less among *Most Affected* borrowers. Had the length pattern reflected crisis-driven inability to repay, the opposite would hold. These results reinforce the conclusion that moral hazard, rather than crisis-driven inability to repay, is the primary driver of the treatment effect.

A more general version of the selection concern is that borrowers with ex-ante riskier loan terms may disproportionately choose longer moratoria, confounding the triple-interaction in Table 8 with pre-existing borrower heterogeneity. Appendix Table A8 addresses this concern directly by regressing moratorium uptake and length on borrower and loan characteristics, with and without a *Treated* dummy interacted with every covariate. Before discussing the results, we note that 99.6% of borrowers in our sample availed the moratorium at some point, reflecting the extremely strict nationwide lockdown in March and April 2020 that made repayment mechanically difficult for almost all borrowers. Consequently, the uptake

(extensive-margin) regressions in Columns (1) and (2) are not informative.

So, we focus on length regressions in Columns (3) and (4). Column (3) shows that borrowers with smaller loans, higher interest rates, and consumption (rather than business) loans availed moratorium for longer. Smaller loans, higher interest rates, and consumption purpose indicate riskier loan. Column (4) adds the *Treated* dummy and its interactions with the covariates, so that each interaction coefficient is the difference between the Treated and Control groups. The interactions are insignificant, indicating that the mapping from ex-ante borrower characteristics to moratorium length is statistically indistinguishable across the two groups. Thus, although riskier borrowers do avail longer moratoria, they do so by the same amount in both groups. The absence of any differential loading on these ex-ante-identifiable risk characteristics rules out that the dose–response with moratorium length in Table 8 reflects group-specific selection into longer moratoria. Both mechanism tests—on joint-liability's social collateral and on moratorium length—are robust to re-estimating the triple-interaction specifications on the matched sample and to restricting the sample by loan purpose, as reported in Appendix Table A15.

7 Alternate Mechanism

7.1 Money Saved Due to March Moratorium

Another potential explanation of the observed treatment effect can be attributed to the extra money saved by the treated borrowers in the form of the instalment amount. In the month of March 2020, the treated borrowers did not repay their monthly due, and hence ended up having more money than the control group. The treated group saved their monthly debt burden for March 2020, while the control group did not.

To check whether the extra money saved by the treated borrowers could explain the observed treatment effects, we define a new variable *Saved More*; an indicator variable that equals one for borrowers whose $\frac{\text{Instalment Amount}}{\text{Household Income}}$ is greater than the median, and zero otherwise. This variable captures the importance of money saved due to the moratorium in March 2020 by the treated borrowers. We scale the instalment amount by the household income to determine the relative importance of the money saved for that household.

We run a modified version of regression equation 4 that replaces *Old JLG* with *Saved More*. The results are described in the Appendix Table A9 for all the three dependent variables: Repayment (%), Missed Instalment, and NPA. The interaction of *Saved More* with *Treated* × *Post* is statistically insignificant. Thus, saving the instalment amount of March 2020 by the treated group does not explain the treatment effect.

7.2 Larger Outstanding Loan Amount

One concern in this analysis could be that borrowers who opt into the moratorium scheme for a longer period end up with a larger outstanding loan amount, which itself may explain weaker post-moratorium repayment. In column 1 of Appendix Table A10, using OLS regression in September 2020, right after the moratorium expires, we see that a longer moratorium is indeed correlated with a larger outstanding loan amount. In subsequent columns, however, we show that controlling for the elevated outstanding loan amount does not substantially affect our primary results.

7.3 Higher Accrued Interest

A further concern is that longer moratoria mechanically generate higher accrued interest, and that this higher interest burden, rather than any change in repayment incentives, drives the post-moratorium treatment effect. To test this, we control for the borrower's accrued interest at the end of August 2020, the last month of the RBI moratorium window, scaled by the borrower's monthly instalment amount, and re-estimate regression equation 1 with this measure and its interaction with Post added to the controls. We report the results in Appendix Table A12. Column 1 confirms that longer moratoria do generate larger interest burdens by the end of the moratorium. In columns 2, 3, and 4, the *Treated* × *Post* coefficients on Repayment, Missed Instalment, and NPA remain similar to main specification after controlling for the ex-ante interest burden and its interaction with Post, ruling out higher accrued interest as the channel driving the post-moratorium treatment effect.

We use the end-of-moratorium snapshot of accrued interest rather than its contemporaneous post-moratorium value because accrued interest is a stock variable that rises mechani-

cally whenever a borrower misses a payment, so its contemporaneous post-moratorium value reflects, rather than predicts, post-moratorium repayment behaviour.

7.4 Moratorium Re-entry

A further concern is that re-entering borrowers, those who paused repayment, resumed, and paused again, may be selectively weaker than the rest of the treated group, so that the treatment effect reflects adverse selection rather than moral hazard. Within the March–August 2020 window, we code a borrower as having re-entered if the span of her moratorium months strictly exceeds their count, i.e., at least one non-moratorium month sits between two moratorium months. About 18% of borrowers meet this definition. We interact *Re-entered* with *Treated* × *Post* in a triple-difference and report the results in Appendix Table A11. Across Repayment, Missed Instalment, and NPA, with and without controls, the triple coefficient is economically small and statistically indistinguishable from zero at 5% significance level, while *Treated* × *Post* retains the magnitudes from Table 2. Re-entry therefore does not drive the treatment effect.

7.5 Anticipation and Borrower-Quality Clustering

Our identification rests on the claim that no borrower in either group could anticipate, at the moment her March instalment was due, that the lender would later choose to retrospectively defer missed instalments. The lender's discretionary decision was communicated only in the first week of April; on the due dates that define treatment status (18–31 March), no borrower had any commitment from her lender about a moratorium. Another concern is that unobservably weaker borrowers might cluster towards the end of the month, mechanically loading the treated group. Both concerns predict that effects should differ *within* each of the original groups, depending on where a borrower's due date falls inside her own window.

We test for this directly in Appendix Table A14, which has two panels. Columns (1)–(2) (*Placebo-within-Treated*) re-estimate equation 1 on the original treated group only, replacing *Treated* with an indicator for borrowers whose instalment was due after the 27$^{\text{th}}$ of the month. Columns (3)–(4) (*Placebo-within-Control*) repeat the exercise on the original control group,

replacing *Treated* with an indicator for borrowers whose instalment was due after the 20$^{\text{th}}$ within the 18–24 March window. The two panels are designed to be symmetric: if anything inside each original window correlated with subsequent default, whether through anticipation, soft information about borrower quality clustered towards month-end, or any other within-window heterogeneity, the corresponding placebo coefficient should be significantly different from zero.

Both panels return coefficients statistically indistinguishable from zero, with and without controls. The Placebo-within-Treated coefficient is the more informative for the anticipation concern: its 27 March cutoff coincides with the RBI's enabling notification, so borrowers due 25–27 March missed payment before any RBI permission existed, while those due 28–31 March missed payment after it but well before the lender's first-week-of-April communication to individual borrowers. If the effect were even partly driven by ex-ante anticipation, these subgroups should differ; the Placebo-within-Control test imposes the same discipline within the control window. Neither panel produces a significant coefficient, ruling out any role for anticipation of the RBI signal, and also the borrower-quality-clustering concern.

8 Conclusion

We examine how the implementation design of a debt moratorium, specifically whether relief is delivered retrospectively or prospectively, affects subsequent repayment behaviour. We do so by exploiting a natural experiment that assigned otherwise identical borrowers to quasi-random treatment and control groups based only on their contractually fixed monthly due date. Treated borrowers, whose March 2020 instalment had already become due in the lockdown week and was therefore retrospectively deferred by the lender's discretionary first-week-of-April decision under RBI permission, defaulted significantly more frequently in the post-moratorium period than control borrowers, whose moratorium history was exclusively prospective participation in the same scheme. The treatment effect is attenuated among borrowers in mature joint-liability groups, where joint-liability's social collateral is stronger, and is larger for borrowers who remained in the scheme longer, with both patterns concentrated in less crisis-affected borrowers. Together, these findings point to moral hazard, specifically an updated belief about future lender willingness to defer missed instalments

without penalty under regulatory cover, as the operative channel. Although moratoria can provide essential immediate relief during a crisis, our results highlight that even a single retrospective deferral can reshape repayment expectations among otherwise disciplined borrowers. Designing such interventions with built-in repayment-discipline mechanisms, such as those embedded in joint-liability lending arrangements, or as forward-looking schemes that avoid retrospective deferral of already-defaulted obligations, can help mitigate these unintended consequences.

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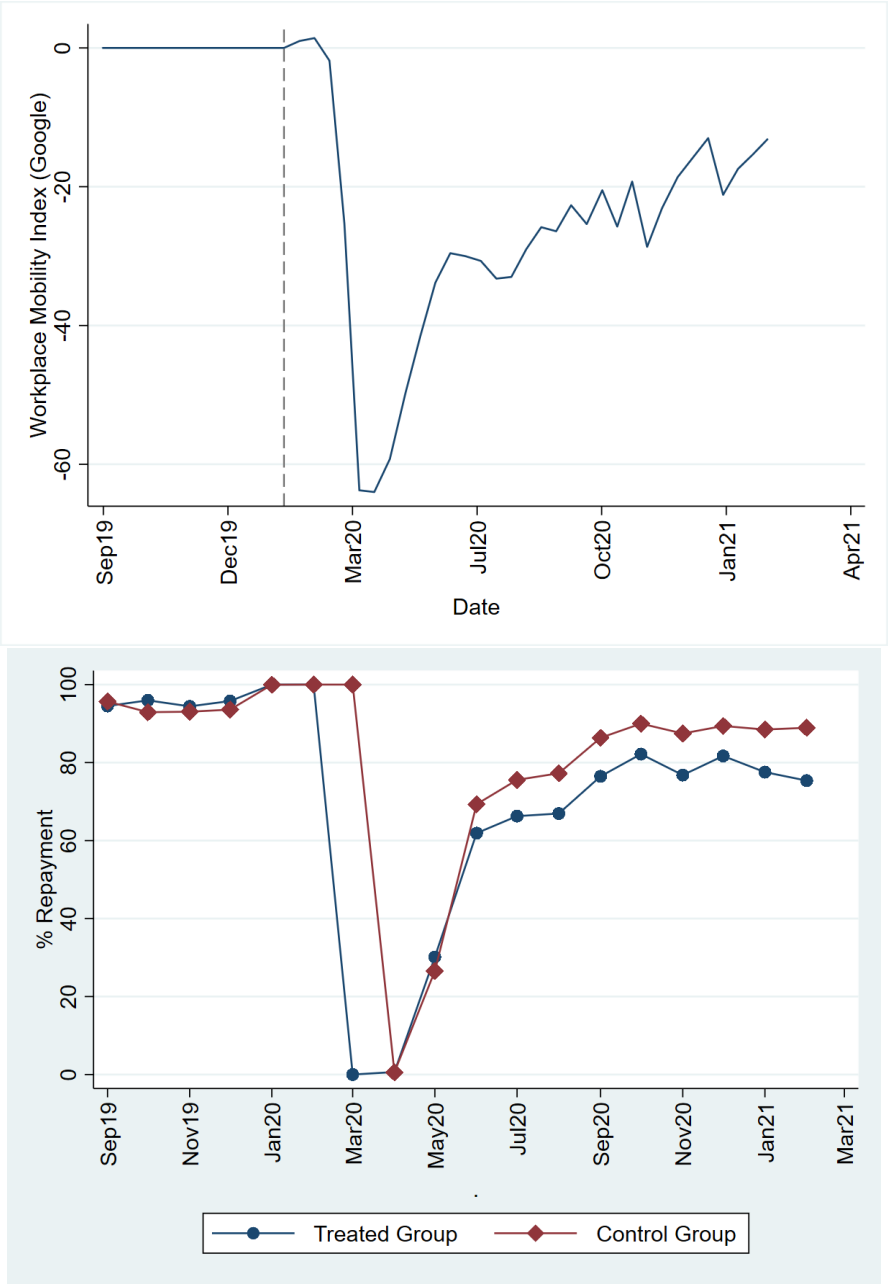


Figure 1: Lockdown and Repayment-Trend: This figure plots how the Workplace Mobility Index changed during 2020 in the **Top Panel**. This index reported by Google measures human mobility around places of work. Google started reporting this index from 15 February 2020. In the **Bottom Panel**, we report the percentage of monthly instalments paid by borrowers of treatment and control groups (as defined in Section 2.1) across time. Note that the first month of non-repayment for the treatment group was March 2020, and for the control group, it was April 2020. The government of India imposed a strict lockdown starting 25 March 2020, thus curbing the ability of borrowers to repay after this date. The borrowers who were punctual in repaying their dues and had their repayment due in the third week of March 2020 belong to the *Control* group. The borrowers who were punctual but could not repay in March 2020 because their repayment was due in the last week of March 2020 had their already-defaulted March instalment retrospectively deferred by the lender's discretionary first-week-of-April moratorium decision and belong to the *Treatment* group.

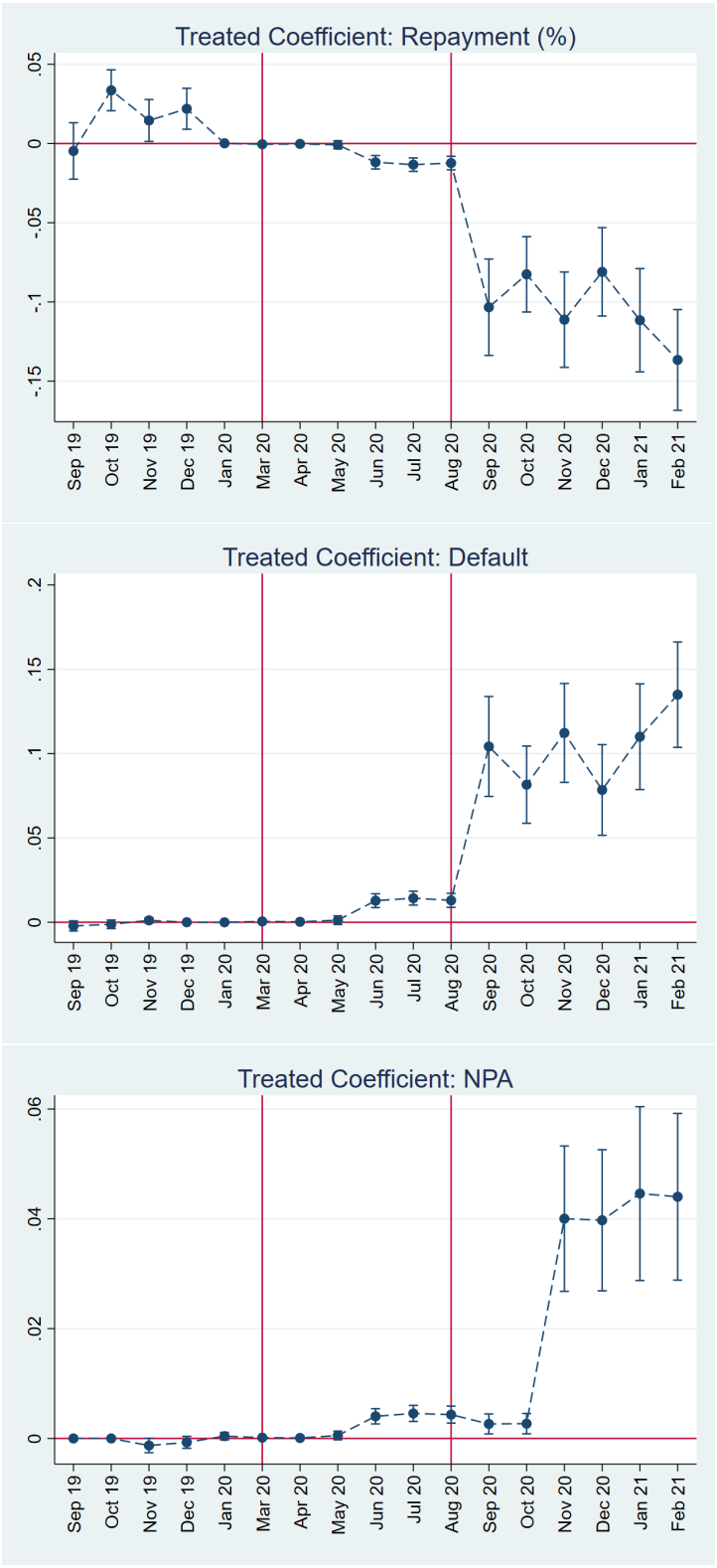


Figure 2: Coefficient Plot: This figure plots the coefficient of a regression similar to Equation 1, but after interacting *Treatment* variable with dummy variables for each month. The dependent variables are (1) *Repayment* (**Top Panel**) is the percentage of monthly instalment repaid by the borrower in a month, capped at 100%, it also equals 100% for a month in which the borrower took moratorium, (2) *Missed Instalment* (**Middle Panel**) is an indicator variable that equals one if a borrower makes no repayment in a month, and zero otherwise. Missed Instalment also equals zero for the months when the borrower took a moratorium, (3) *NPA* (**Bottom Panel**) is another indicator variable that equals one if a borrower misses instalments for three consecutive months (consistent with the RBI's 90-days-past-due NPA definition), and zero otherwise. The left vertical line indicates the beginning of the Moratorium scheme, and the right vertical line indicates its end.

TABLE 1: Data Description: Here we describe the data used for this study. Panel A lists the coverage summary of the sample. Panel B compares the treatment and control groups across several loan terms and borrower characteristics. Data used for the comparisons is restricted to February 2020, the month preceding the start of the lockdown (and the moratorium) in March 2020. Segregation of borrowers into *Treatment* and *Control* groups are defined in Section 2.1. *Treatment* group has the borrowers who did not repay in March-2020 and whose already-defaulted March instalment was therefore retrospectively deferred by the lender's discretionary first-week-of-April moratorium decision. All borrowers who made a repayment in the month of March 2020 did not get a moratorium for that month; these borrowers constitute the *Control* group. Panel C compares the repayment rates of the two groups before the moratorium started, and after it ended. *Repayment* is the percentage of monthly instalment repaid by the borrower in a month, capped at 100%.

Panel A: Coverage Summary	
Sample Period	Sep-2019 to Feb-2021
Maximum Moratorium Period	Mar-2020 to Aug-2020
Number of Branches	206
Number of Loan-Month Observations	300,185
Number of Loans	17,864
Number of Borrowers	17,864
Number of Treated Borrowers	6,325
Number of Control Borrowers	11,539

Panel B: Compare Treated and Control Groups

	Treated	Control	Difference	p-Value
Loan Terms				
Loan Amount (INR)	37,292	37,830	538***	0.00
Monthly Instalment Amount (INR)	2,082	2,108	25***	0.00
Interest Rate (%)	24.88	25.02	0.14***	0.00
Tenure (Years)	1.87	1.88	0.01***	0.00
Business Loan	0.65	0.56	-0.09***	0.00
Joint Liability Group Loan	0.97	0.97	0.00	0.63
Borrower Characteristics				
Age (Years)	40.24	40.53	0.30**	0.03
Individual Income (INR)	97,047	96,659	-388	0.66
Household Income (INR)	379,096	395,088	15,992*	0.08
Female	0.99	0.99	-0.00	0.65
Married	0.96	0.96	-0.00	0.19
Occupation Agriculture	0.12	0.13	0.01	0.07
Occupation Non-Agriculture	0.82	0.80	-0.01**	0.03
Occupation Salaried	0.04	0.04	0.00	0.71
Occupation Others	0.03	0.03	0.00	0.18
Count	6,325	11,539		

Panel C: Repayment Rates

Pre-Covid	Treated	Control	Difference	p-Value
Repayment (%)	0.9690	0.9603	-0.0087***	0.00
Missed Instalment	0.00	0.00	0.00	.
NPA	0.00	0.00	0.00	.
Post-Moratorium				
Repayment (%)	0.7840	0.8839	0.0998***	0.00
Missed Instalment	0.2097	0.1115	-0.0983***	0.00
NPA	0.0459	0.0207	-0.0253***	0.00

TABLE 2: Main Result: This table presents coefficient-estimates for regression equation 1. We show results for three dependent variables Repayment in columns (1) and (2), Missed Instalment in columns (3) and (4), and Non-Performing Asset (NPA) in columns (5) and (6). Variables used in the regression are described in Section 3. All regressions have borrower fixed effects and year-month fixed effects. Columns (2), (4), and (6) also include loan terms, borrower characteristics, and their interaction with the Post variable as controls, along with occupation × Post fixed effects. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment		Missed Instalment		NPA	
Treated × Post	-0.088***	-0.084***	0.080***	0.081***	0.022***	0.024***
	(0.011)	(0.011)	(0.011)	(0.010)	(0.004)	(0.004)
Post	-0.058***		0.062***		0.018***	
	(0.004)		(0.004)		(0.002)	
log(Loan Amount) × Post		0.136***		-0.155***		-0.057***
		(0.032)		(0.028)		(0.012)
Tenure × Post		-0.028		0.009		-0.000
		(0.034)		(0.029)		(0.014)
Interest Rate × Post		0.016		0.027***		0.012***
		(0.011)		(0.009)		(0.004)
Business Loan × Post		0.008		-0.004		-0.006
		(0.012)		(0.010)		(0.004)
Female × Post		0.064**		-0.018		0.007
		(0.027)		(0.026)		(0.011)
Married		-0.020		0.053**		0.012
		(0.023)		(0.021)		(0.013)
Married × Post		0.003		-0.012		-0.006
		(0.011)		(0.010)		(0.006)
Age (Years)		-0.004		0.006		0.001
		(0.004)		(0.004)		(0.001)
Age × Post		-0.000		-0.000		0.000
		(0.000)		(0.000)		(0.000)
log(Individual Income)		0.032***		-0.027***		-0.009***
		(0.009)		(0.007)		(0.003)
log(Individual Income) × Post		0.001		-0.000		0.001
		(0.006)		(0.005)		(0.002)
log(Household Income)		0.009*		0.005**		0.000
		(0.005)		(0.002)		(0.001)
log(Household Income) × Post		-0.000		-0.004		-0.002
		(0.003)		(0.003)		(0.001)
Observations	300185	300185	300185	300185	264457	264457
Adjusted R^2	0.183	0.191	0.224	0.232	0.148	0.154
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes

TABLE 3: Matched Sample: This table documents the coefficient estimates of regression Equation 1 for dependent variables *Repayment* in columns (1) and (2), *Missed Instalment* in columns (3) and (4), and *NPA* in columns (5) and (6) on the matched sample described in Appendix Table A2. Columns (1), (3), and (5) use the 1:1 Treatment:Control nearest neighbour matched sample. Columns (2), (4), and (6) include additional borrower and loan level controls and their interaction with *Post* dummy. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment		Missed Instalment		NPA	
Treated × Post	-0.092***	-0.092***	0.084***	0.085***	0.024***	0.025***
	(0.012)	(0.011)	(0.011)	(0.010)	(0.004)	(0.004)
Post	-0.048***		0.053***		0.016***	
	(0.005)		(0.005)		(0.002)	
log(Loan Amount) × Post		0.149***		-0.166***		-0.062***
		(0.032)		(0.029)		(0.012)
Tenure × Post		-0.037		0.013		0.001
		(0.034)		(0.030)		(0.014)
Interest Rate × Post		-0.018		0.049***		0.022***
		(0.015)		(0.013)		(0.006)
Business Loan × Post		0.007		-0.006		-0.008*
		(0.013)		(0.012)		(0.004)
Female × Post		0.032		0.003		0.016
		(0.033)		(0.032)		(0.016)
Married		-0.026		0.061**		0.013
		(0.027)		(0.025)		(0.015)
Married × Post		0.002		-0.013		-0.006
		(0.013)		(0.012)		(0.007)
Age (Years)		-0.002		0.005		-0.000
		(0.005)		(0.004)		(0.001)
Age × Post		0.000		-0.001**		0.000
		(0.000)		(0.000)		(0.000)
log(Individual Income)		0.025**		-0.021**		-0.006*
		(0.010)		(0.008)		(0.004)
log(Individual Income) × Post		0.002		-0.001		0.001
		(0.007)		(0.006)		(0.002)
log(Household Income)		0.007		0.003		-0.001
		(0.005)		(0.002)		(0.001)
log(Household Income) × Post		0.001		-0.004		-0.002
		(0.004)		(0.003)		(0.002)
Observations	213545	213545	213545	213545	188245	188245
Adjusted R^2	0.193	0.199	0.232	0.241	0.154	0.160
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes

TABLE 4: Robustness: Alternate Measures of Delinquency and Loan Performance at Maturity: This table presents coefficient estimates from regressions using alternate measures of repayment performance (Panel A) and end-of-loan outcomes (Panel B). **Panel A** reports estimates from Equation 1 for three dependent variables: 1–29 *Days Overdue* (Columns 1–2), ≥30 *Days Overdue* (Columns 3–4), and ≥1 *Day Overdue* (Columns 5–6). All regressions include borrower and year-month fixed effects; Columns (2), (4), and (6) additionally control for borrower and loan characteristics and their interaction with *Post*, along with occupation × *Post* fixed effects. **Panel B** presents results from Equation 2 for dependent variables *NPA* (Columns 1–2) and ≥30 *Days Overdue* (Columns 3–4), measured at or after loan maturity. Columns (2) and (4) include borrower and loan controls and occupation fixed effects. Standard errors are clustered at the branch level and reported in parentheses.

Panel A: Alternate Measures of Delinquency

	(1)	(2)	(3)	(4)	(5)	(6)
	1–29 Days Overdue		≥30 Days Overdue		≥1 Day Overdue	
Treated × Post	0.024***	0.023***	0.058***	0.062***	0.082***	0.085***
	(0.007)	(0.007)	(0.010)	(0.009)	(0.015)	(0.015)
Post	0.051***		0.052***		0.103***	
	(0.003)		(0.004)		(0.006)	
Observations	300185	300185	300185	300185	300185	300185
Adjusted R^2	0.180	0.182	0.266	0.276	0.359	0.369
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes
Borrower Characteristics Controls		Yes		Yes		Yes
Borrower Characteristics × Post Controls		Yes		Yes		Yes
Loan Characteristics Controls		Yes		Yes		Yes
Loan Characteristics × Post Controls		Yes		Yes		Yes

Panel B: Loan Performance at Maturity

	(1)	(2)	(3)	(4)
	NPA		≥30 Days Overdue	
Treated	0.049*	0.059**	0.052*	0.056**
	(0.027)	(0.026)	(0.028)	(0.026)
Observations	3010	3010	3010	3010
Adjusted R^2	0.214	0.223	0.389	0.398
Year × Month FE	Yes	Yes	Yes	Yes
Occupation FE		Yes		Yes
Borrower and Loan Controls		Yes		Yes

TABLE 5: Choice to Opt Out: For each sub-group defined in Table A7, we run the regression equation 1, and report the coefficient estimates. Here, we check if the results are driven by the choice of borrowers to opt out of the moratorium scheme instead of utilising it till the scheme is available. Panels A, B, and C document results using *Repayment*, *Missed Instalment*, and *NPA*, respectively, as dependent variables. Standard errors are clustered at the branch level and are reported in parentheses.

Panel A: Repayment as dependent variable					
	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.011**	-0.014**	-0.052***	-0.073***	-0.192***
	(0.005)	(0.006)	(0.017)	(0.026)	(0.022)
Observations	68229	93150	23180	16077	42544
Adjusted R^2	0.075	0.078	0.132	0.205	0.408
Borrower FE	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE	Yes	Yes	Yes	Yes	Yes
Loan and Borrower Controls	Yes	Yes	Yes	Yes	Yes
Loan and Borrower Controls × Post	Yes	Yes	Yes	Yes	Yes
Monthly Sub-group	April	May	June	July	August
Panel B: Missed Instalment as dependent variable					
Treated × Post	0.009***	0.013***	0.056***	0.066***	0.184***
	(0.003)	(0.004)	(0.016)	(0.025)	(0.021)
Observations	68229	93150	23180	16077	42544
Adjusted R^2	0.075	0.096	0.161	0.240	0.422
Borrower FE	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE	Yes	Yes	Yes	Yes	Yes
Loan and Borrower Controls	Yes	Yes	Yes	Yes	Yes
Loan and Borrower Controls × Post	Yes	Yes	Yes	Yes	Yes
Monthly Sub-group	April	May	June	July	August
Panel C: NPA as dependent variable					
Treated × Post	-0.000	-0.001	0.005	0.009	0.084***
	(0.000)	(0.001)	(0.003)	(0.008)	(0.013)
Observations	59993	81952	20430	14191	37624
Adjusted R^2	0.115	0.075	0.085	0.128	0.268
Borrower FE	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE	Yes	Yes	Yes	Yes	Yes
Loan and Borrower Controls	Yes	Yes	Yes	Yes	Yes
Loan and Borrower Controls × Post	Yes	Yes	Yes	Yes	Yes
Monthly Sub-group	April	May	June	July	August

TABLE 6: External Validity: This table presents an external validity test using nationally representative data from the CMIE Consumer Pyramids Household Survey (CPHS), replicating our main test on a broader borrower panel. We define *Treated* borrowers as those who made a formal loan repayment in February 2020 (pre-lockdown) but not in March 2020 (during the moratorium rollout), so that their missed March instalment could only have been deferred retrospectively. The control group includes borrowers who paid in both months, for whom any later moratorium was prospective. We classify two post-treatment periods: the *Moratorium Phase* (March–August 2020) and the *Post-Moratorium* Phase (September 2020 onwards). The dependent variable is *Missed Instalment*, a monthly indicator equal to one if no repayment is recorded in that month, in columns 1–2, and *NPA*, an indicator equal to one if no repayment is recorded for three consecutive months, in columns 3–4. Columns 1 and 3 include borrower and year-month fixed effects; columns 2 and 4 include district × year-month fixed effects. Standard errors are clustered at the district level and are reported in parentheses.

	Missed Instalment		NPA	
	(1)	(2)	(3)	(4)
Treated		-0.09**		-0.09**
		(0.04)		(0.04)
Treated × Moratorium Phase	0.61***	0.54***	0.40***	0.37***
	(0.06)	(0.06)	(0.04)	(0.05)
Treated × Post-Moratorium	0.16***	0.12**	0.12***	0.08**
	(0.05)	(0.05)	(0.03)	(0.03)
Observations	21,024	20,592	17,082	16,731
Adj. R-sq	0.299	0.395	0.210	0.348
Borrower FE	Yes		Yes	
Year × Month FE	Yes		Yes	
District × Year × Month FE		Yes		Yes

TABLE 7: Joint-Liability's Social Collateral: Here, we document the regression results to compare the treatment effects between borrowers who are more vs. less exposed to joint-liability's social collateral. In column (1), we report the coefficient estimates of regression equation 1 for borrowers with low exposure to social collateral. These borrowers have either individual liability (IL) loans or are in their first joint-liability loan cycle (New JLG) with the lender. Column (2) runs the same regression for the borrowers in their second or higher joint-liability loans (Old JLG). Column (3) reports coefficient estimates for regression equation 4, and column (4) does the same after replacing *Old JLG* dummy with *JLG Cycle* which represents the number of JLG loans the borrower has borrowed from this lender so far. All regressions have loan and borrower-level control variables along with their interaction with the *Post* dummy. The dependent variable is *NPA* in all columns. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)
			NPA	
Treated × Post	0.031***	0.015***	0.032***	0.028***
	(0.008)	(0.003)	(0.008)	(0.006)
Old JLG × Post			0.004	
			(0.007)	
Old JLG × Treated × Post			-0.019**	
			(0.008)	
JLG Cycle × Post				0.002*
				(0.001)
JLG Cycle × Treated × Post				-0.003**
				(0.002)
Observations	69709	194748	264457	264457
Adjusted R^2	0.218	0.152	0.177	0.176
Borrower FE	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes
Occupation × Post FE	Yes	Yes	Yes	Yes
Loan and Borrower Controls	Yes	Yes	Yes	Yes
Loan and Borrower Controls × Post	Yes	Yes	Yes	Yes
Subsample	IL and New JLG	Old JLG		

TABLE 8: Moratorium Length and Impact of Crisis: Here, we document how the non-performing-asset rates of treated and control borrowers interact with the length of moratorium participation and how this effect varies with the impact of the crisis. In column (1), we report the coefficient estimates of regression equation 4 after replacing *Old JLG* with *Moratorium Length*. Moratorium length represents the number of months for which a borrower remained in the scheme. It is a discrete variable that takes values of 0, 1, 2, 3, 4 or 5 as described in Appendix Table A7. Then, we classify borrowers into *Most Affected* and *Less Affected* by the crisis. Borrowers with below median household income whose CMIE Consumer Pyramids Dx occupation-level average income dropped more than the median change in occupation-level income after March 2020, are classified as *Most Affected*. All remaining borrowers are *Less Affected*. Columns (2) and (3) run the same regression on subsamples of the *Less Affected* and *Most Affected* borrowers, respectively. In column (4), we interact the triple interaction term of column (1) with *Most Affected*. The dependent variable is *NPA* in all columns. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)
			NPA	
Treated × Post	-0.032***	-0.041***	-0.022***	-0.041***
	(0.006)	(0.008)	(0.007)	(0.008)
Moratorium Length × Post	0.017***	0.018***	0.016***	0.018***
	(0.002)	(0.002)	(0.003)	(0.002)
Treated × Moratorium Length × Post	0.019***	0.024***	0.014***	0.024***
	(0.003)	(0.004)	(0.004)	(0.004)
Most Affected × Post				0.004
				(0.005)
Treated × Most Affected × Post				0.020**
				(0.009)
Moratorium Length × Most Affected × Post				-0.001
				(0.003)
Treated × Moratorium Length × Most Affected × Post				-0.010**
				(0.005)
Observations	264457	151499	112958	264457
Adj. R-sq	0.180	0.192	0.165	0.181
Borrower FE	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes
Occupation × Post FE	Yes	Yes	Yes	Yes
Borrower & Loan Characteristics Controls	Yes	Yes	Yes	Yes
Borrower & Loan Characteristics Controls × Post	Yes	Yes	Yes	Yes
Subsample	All	Less Affected	Most Affected	All

Appendix

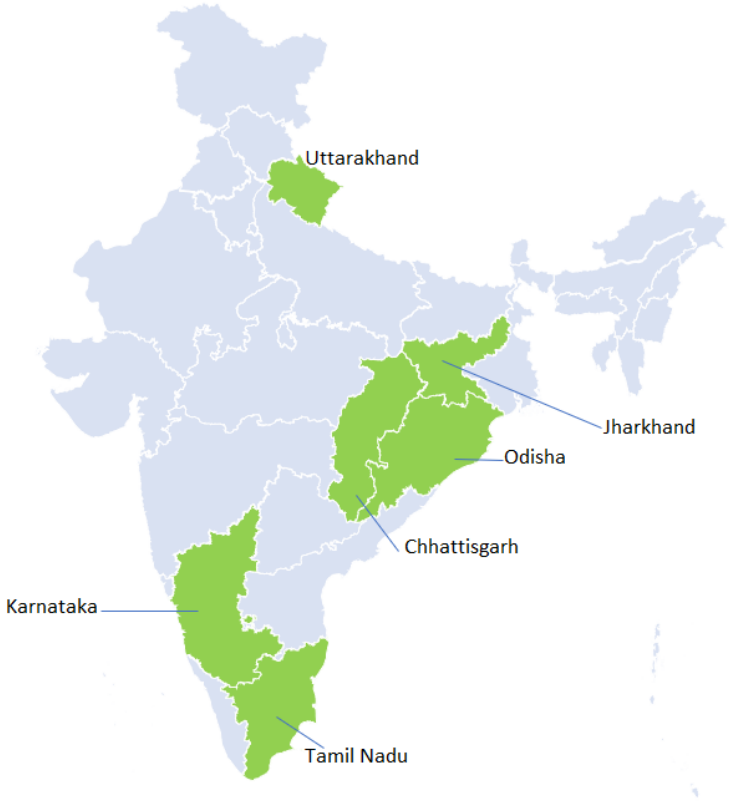


Figure A1: Map of India: States of India where the lender is active.

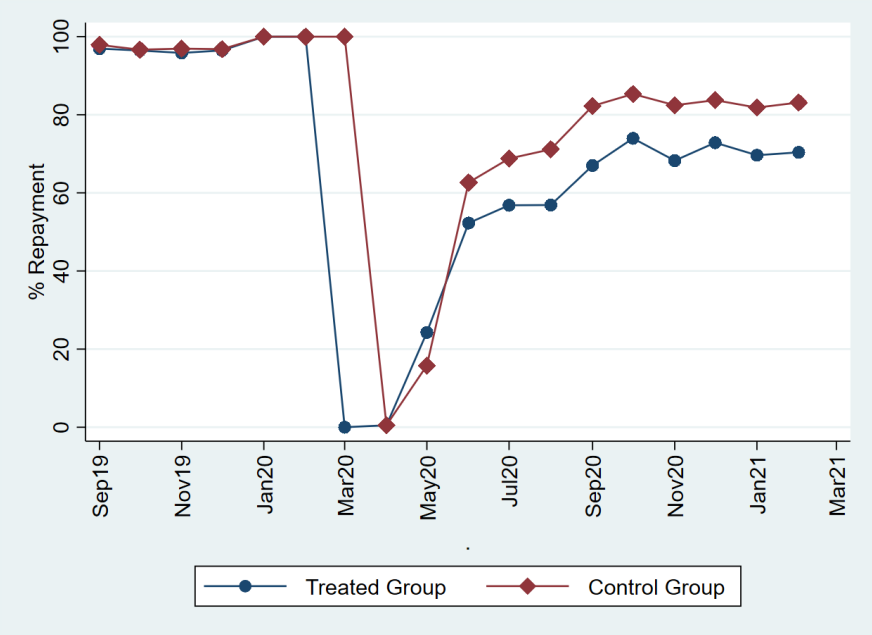


Figure A2: Repayment Trend - All Borrowers of the Lender: This figure replicates the pre-trends graph in Figure 1 for 175,578 borrowers of the lender who had no missed instalments in the pre-period. This subset does not exclude borrowers whose repayments in January and February of 2020 were not necessarily punctual.

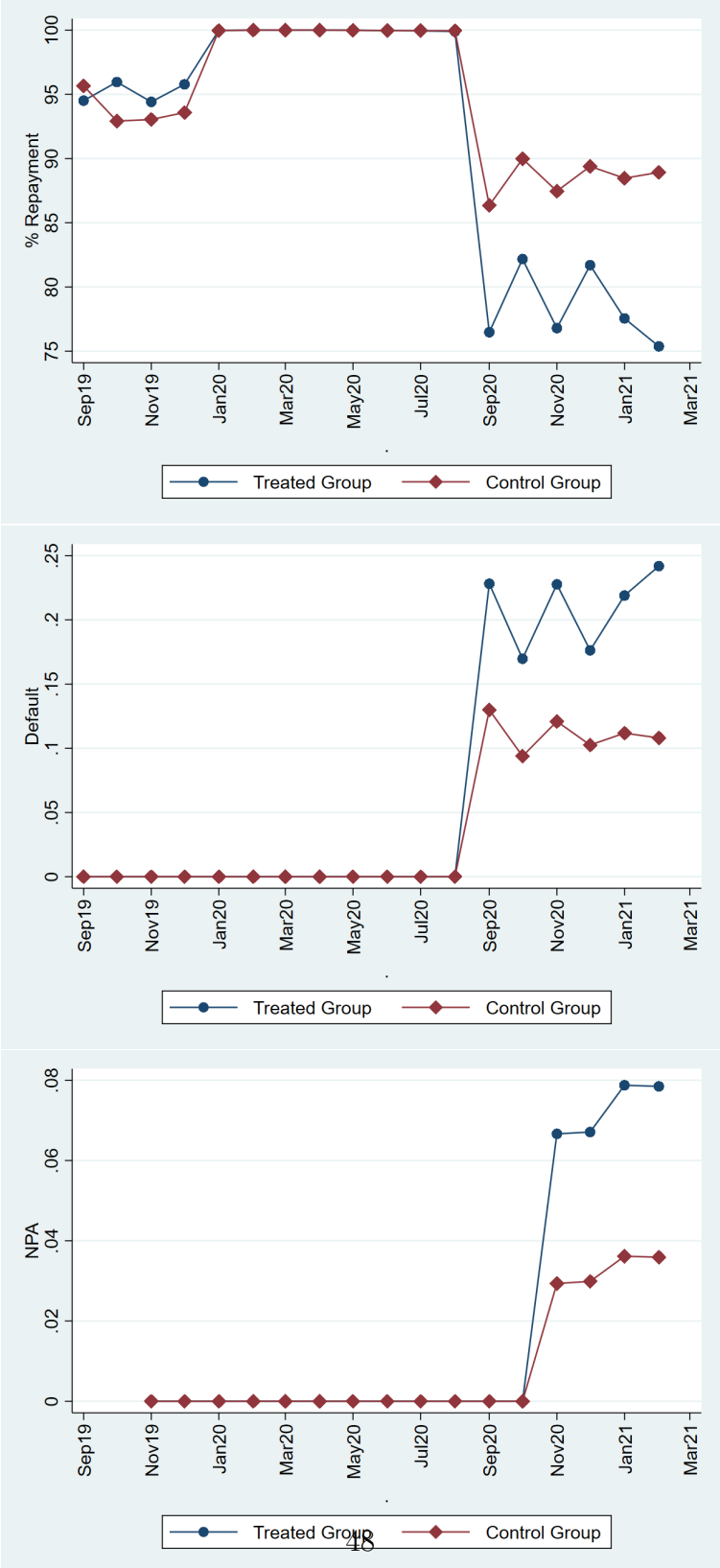


Figure A3: Repayment Trends: In the top panel, we report the percentage of monthly instalments paid by borrowers of treatment and control groups across time. The middle and bottom panels report Missed Instalment and NPA monthly averages, respectively, for Treated and Control groups. All these variables are adjusted such that repayment behaviour during the moratorium period is the same for both groups.

TABLE A1: Summary Statistics: This table lists the distribution of loan terms and demographic details for the borrowers in our sample as observed in February 2020.

Panel A - Loan Terms

	Mean	p1	p25	p50	p75	p99	Std Dev
Loan Amount (INR)	37,640	26,000	26,000	40,000	45,000	50,000	9,366
Instalment Amount (INR)	2,099	1,381	1,747	2,135	2,402	2,694	416
Interest Rate (%)	24.97	24.50	25.00	25.00	25.00	26.00	0.41
Tenure (Years)	1.87	1.50	2.00	2.00	2.00	2.00	0.22
Business Loan	0.5889	0.00	0.00	1.00	1.00	1.00	0.49
Joint Liability Group Loan	0.9731	0.00	1.00	1.00	1.00	1.00	0.16

Panel B - Borrower Characteristics

	Mean	p1	p25	p50	p75	p99	Std Dev
Age (Years)	40.42	21	34	40	48	57	9.01
Annual Individual Income (INR)	96,796	6,000	60,000	84,000	120,000	252,000	56,347
Annual Household Income (INR)	389,426	85,800	235,000	313,200	431,000	1,520,600	587,247
Female	0.9922	1.00	1.00	1.00	1.00	1.00	0.09
Married	0.9617	0.00	1.00	1.00	1.00	1.00	0.19
Occupation Agriculture	0.1223	0.00	0.00	0.00	0.00	1.00	0.33
Occupation Non-Agriculture	0.8082	0.00	1.00	1.00	1.00	1.00	0.39
Occupation Salaried	0.0415	0.00	0.00	0.00	0.00	1.00	0.20
Occupation Others	0.0280	0.00	0.00	0.00	0.00	1.00	0.16

Panel C - Repayment Variables

	Overall		Pre-Covid		Post-Moratorium	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Repayment (%)	0.9399	0.23	0.9635	0.15	0.8484	0.36
Missed Instalment	0.0464	0.21	0.0000	0.00	0.1464	0.35
NPA	0.0107	0.10	0.0000	0.00	0.0296	0.17

TABLE A2: T-test Matched Sample: We use the propensity score matching method to find the nearest neighbours of the treatment borrowers in the control group, based on Mahalanobis distance. The table reports the difference between treatment and control groups, where both groups have an equal number of borrowers. We use demographic details of borrowers and loan terms from February 2020 to get the matched sample.

	Treated	Control	Difference	p-Value
Loan Terms				
log(Loan Amount)	10.49	10.49	0.00	0.399
Interest Rate (%)	24.88	24.91	0.03***	0.000
Tenure (Years)	1.87	1.87	-0.00	0.953
Business Loan	0.65	0.65	0.00	1.000
Joint Liability Group Loan	0.97	0.98	0.00	0.250
Borrower Characteristics				
Age (Years)	40.24	40.26	0.03	0.874
Individual Income (INR)	98,746.86	98,811.79	64.92	0.947
Household Income (INR)	364,804.31	371,137.48	6333.16	0.289
Female	0.99	0.99	-0.00	0.918
Married	0.96	0.96	0.00	1.000
Occupation Agriculture	0.12	0.12	-0.00	0.956
Occupation Non-Agriculture	0.82	0.82	0.00	0.982
Occupation Salaried	0.04	0.04	0.00	0.964
Occupation Others	0.03	0.03	0.00	1.000
Observation Count	6,325	6,325		

TABLE A3: Main Result - All Good Borrowers: Here, we replicate the tests in Table 2 with 175,578 borrowers of the lender who had no missed instalments in the pre-period. This subset does not exclude borrowers whose repayments in pre-Covid months: January, February and March 2020 were not necessarily punctual.

	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment (%)		Missed Instalment		NPA	
Treated x Post	-0.109***	-0.085***	0.112***	0.091***	0.040***	0.034***
	(0.011)	(0.008)	(0.011)	(0.007)	(0.005)	(0.004)
Post	-0.090***		0.087***		0.029***	
	(0.003)		(0.003)		(0.002)	
Observations	2962276	2962273	2962276	2962273	2611120	2611120
Adjusted R^2	0.235	0.333	0.255	0.350	0.175	0.209
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Terms Controls		Yes		Yes		Yes
Loan Terms × Post Controls		Yes		Yes		Yes
Borrower Characteristics Controls		Yes		Yes		Yes
Borrower Characteristics × Post Controls		Yes		Yes		Yes

TABLE A4: Main Result with Scheme-Wide Post: This table reports the coefficient estimates of regression equation 1 after replacing the borrower-specific $Post_{it}$ with a scheme-wide calendar indicator $Post_t$ that equals one for every month from September 2020 onward, and zero for every month from September 2019 through August 2020. $Post_t$ is identical for treated and control borrowers and does not depend on any borrower's own moratorium usage or exit date. The dependent variable in columns (1) and (2) is *Repayment*, in columns (3) and (4) is *Missed Instalment*, and in columns (5) and (6) is *NPA*. Odd columns show estimates without controls; even columns add loan terms, borrower characteristics, and their interactions with $Post_t$, as well as Occupation × Post fixed effects. All regressions include borrower and year-month fixed effects, and cluster standard errors at the lender's branch level.

	Repayment (%)		Missed Instalment		NPA	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.104***	-0.073***	0.097***	0.072***	0.025***	0.019***
	(0.014)	(0.009)	(0.013)	(0.008)	(0.004)	(0.003)
Observations	300185	300185	300185	300185	264457	264457
Adj. R^2	0.179	0.268	0.218	0.313	0.145	0.178
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation x Post FE		Yes		Yes		Yes
Borrower and Loan Controls		Yes		Yes		Yes
Borrower and Loan Controls × Post		Yes		Yes		Yes

TABLE A5: Loan Types: In Panel A of this table, we document the coefficient estimates of regression equation 1 for different types of loans. Columns (1), (3), and (5) have only joint-liability loans; columns (2), (4), and (6) have only individual liability loans. The dependent variable in columns (1) and (2) is *Repayment*, in columns (3) and (4) is *Missed Instalment*, and in columns (5) and (6) is *NPA*. In Panel B, we compare the treatment effects of joint-liability group loans with individual liability loans using regression equation 5 in columns (1), (3), and (5) with the aforementioned dependent variables, respectively. *JLG* is an indicator variable that equals one if a borrower has borrowed a joint-liability group loan, and zero if the loan has individual liability. Columns (2), (4), and (6) include additional borrower-level control variables like gender, marital status, income, occupation, age, and their interactions with the *Post* dummy. The variable definitions remain the same as described in Section 3.

Panel A: Treatment Effect in Subsamples of Loan Types						
	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment		Missed Instalment		NPA	
Treated × Post	-0.086***	-0.141***	0.079***	0.126***	0.022***	0.053***
	(0.012)	(0.027)	(0.011)	(0.027)	(0.004)	(0.016)
Post	-0.058***	-0.072***	0.061***	0.082***	0.017***	0.020***
	(0.004)	(0.010)	(0.004)	(0.009)	(0.002)	(0.005)
Observations	292135	8050	292135	8050	257367	7090
Adjusted R^2	0.182	0.250	0.222	0.282	0.147	0.179
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type	JLG	IL	JLG	IL	JLG	IL
Panel B: Comparing Treatment Effect of Loan Types						
Treated × Post	-0.206***	-0.209***	0.184***	0.187***	0.062***	0.067***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.016)	(0.017)
JLG × Treated × Post	0.121***	0.124***	-0.107***	-0.110***	-0.041**	-0.045***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.016)	(0.017)
Post	-0.058***		0.062***		0.018***	
	(0.004)		(0.004)		(0.002)	
Observations	300185	300185	300185	300185	264457	264457
Adjusted R^2	0.184	0.186	0.224	0.226	0.148	0.150
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes
Borrower Characteristics Controls		Yes		Yes		Yes
Borrower Characteristics × Post		Yes		Yes		Yes

$$
\begin{aligned}
Y_{it} =& \beta_0 + \beta_1 \times Treated_i + \beta_2 \times Post_{it} + \beta_3 \times JLG_i + \beta_4 \times Treated_i \times Post_{it} \\
&+ \beta_5 \times JLG_i \times Treated_i + \beta_6 \times JLG_i \times Post_{it} + \beta_7 \times JLG_i \times Treated_i \times Post_{it} \\
&+ X_{it} + X_{it} \times Post_{it} + \nu_i + \mu_t + \varepsilon_{it}
\end{aligned} \tag{5}
$$

TABLE A6: Social Interconnectedness and Moral Hazard: This table provides an additional peer-monitoring test of the moral hazard channel. If dense social ties within a branch discipline strategic default, the moral hazard induced by the retrospective moratorium should be muted where borrowers are more socially interconnected. We measure branch-level social interconnectedness as the mean of three within-branch Herfindahl (HHI) concentration indices computed over borrowers' *caste*, *religion*, and *occupation*, using pre-Covid observations and one observation per borrower; higher values denote more homogeneous, and hence more tightly connected, communities. The measure is winsorized at the 1st and 99th percentiles. Column (1) reports the effect of *Post* × *Social Interconnect* for the control group, column (2) for the treatment group, column (3) reports the full triple-difference (*Treated* × *Post* × *Social Interconnect*), and column (4) re-estimates the column (3) specification adding loan terms and borrower characteristics, together with their interactions with *Post*, as controls. The level of *Social Interconnect* and the *Treated* main effect are absorbed by the borrower fixed effects. The dependent variable is *NPA* in all columns. All regressions include borrower and year-month fixed effects. Standard errors are clustered at the branch level and are reported in parentheses.

	NPA (Default)			
	(1)	(2)	(3)	(4)
Post	0.030	0.137***	0.028	0.154*
	(0.019)	(0.038)	(0.019)	(0.081)
Post × Social Interconnect	-0.018	-0.152***	-0.016	0.046**
	(0.029)	(0.055)	(0.029)	(0.020)
Treat × Post			0.117***	0.101***
			(0.038)	(0.033)
Treat × Post × Social Interconnect			-0.141**	-0.124***
			(0.055)	(0.047)
Observations	169,955	94,502	264,457	264,457
Adj. R-sq	0.126	0.173	0.149	0.176
Sample	Control	Treatment	All	All
Borrower FE	Yes	Yes	Yes	Yes
Year x Month FE	Yes	Yes	Yes	Yes
Borrower and Loan Controls				Yes
Borrower and Loan Controls × Post				Yes

TABLE A7: Monthly Sub-groups Construction: To precisely categorise borrowers into the treatment and control groups, we pick sub-samples of borrowers from each group such that they take moratorium in same months, differing only for the month of March 2020. For example, in the "April sub-group", the treated borrowers took moratorium only in the months of March and April, while the control borrowers took moratorium only in April. Similarly, in the "May subgroup", the treated borrowers took moratorium only in March, April and May, and the control group borrowers took moratorium only in April and May.

		Treatment			Control		
Monthly Sub-group	Voluntary Moratorium Months	Moratorium taken in months	Number of Months	Borrowers	Moratorium taken in months	Number of Months	Borrowers
April	1	Mar and Apr Only	2	1,461	Apr Only	1	2,657
May	2	Mar, Apr, and May Only	3	1,510	Apr and May Only	2	4,089
June	3	Mar, Apr, May, and Jun Only	4	419	Apr, May, and Jun Only	3	956
July	4	Mar, Apr, May, Jun, and Jul Only	5	348	Apr, May, Jun, and Jul Only	4	595
August	5	Mar, Apr, May, Jun, Jul, and Aug	6	1,074	Apr, May, Jun, Jul, and Aug	5	1,386

TABLE A8: Determinants of Moratorium Uptake and Length: This table reports OLS regressions of moratorium uptake (an indicator equal to one if the borrower availed moratorium at all) and moratorium length (number of months the borrower remained in the scheme) on borrower and loan characteristics in Columns (1) and (3), and additionally on the *Treated* dummy and its interactions with each covariate in Columns (2) and (4). All specifications include occupation and branch fixed effects, with standard errors clustered at the branch level and reported in parentheses.

	Moratorium Taken		Moratorium Months	
	(1)	(2)	(3)	(4)
log(Loan Amount)	-0.010	-0.009	-0.751***	-0.732***
	(0.007)	(0.006)	(0.143)	(0.177)
Tenure (Years)	0.008	0.011	0.212	0.176
	(0.009)	(0.011)	(0.151)	(0.199)
Interest Rate (%)	-0.002	-0.007	0.178***	0.208***
	(0.004)	(0.005)	(0.057)	(0.070)
Business Loan	-0.002*	-0.003*	-0.111**	-0.125*
	(0.001)	(0.002)	(0.056)	(0.064)
Joint Liability Group Loan	0.013**	0.012	0.047	0.137
	(0.007)	(0.010)	(0.110)	(0.142)
Female	0.008	0.014	0.042	0.198
	(0.016)	(0.023)	(0.159)	(0.178)
Married	0.003	0.003	-0.038	0.029
	(0.004)	(0.006)	(0.053)	(0.056)
Age (Years)	-0.000	-0.000	-0.004***	-0.004***
	(0.000)	(0.000)	(0.001)	(0.001)
log(Individual Income)	0.000	-0.001	-0.042*	-0.033
	(0.001)	(0.001)	(0.023)	(0.026)
log(Household Income)	-0.001	-0.001	0.009	0.019
	(0.001)	(0.001)	(0.015)	(0.017)
Treated		-0.424*		-0.844
		(0.241)		(2.932)
Treat x log(Loan Amount)		-0.007		-0.038
		(0.015)		(0.293)
Treat x Tenure (Years)		-0.010		0.060
		(0.009)		(0.328)
Treat x Interest Rate (%)		0.020*		0.106
		(0.011)		(0.110)
Treat x Business Loan		0.002		0.007
		(0.004)		(0.090)
Treat x JLG Loan		0.006		-0.215
		(0.017)		(0.216)
Treat x Female		-0.017		-0.294
		(0.026)		(0.332)
Treat x Married		-0.001		-0.222**
		(0.007)		(0.106)
Treat x Age (Years)		-0.000		0.001
		(0.000)		(0.002)
Treat x log(Individual Income)		0.004**		-0.024
		(0.002)		(0.039)
Treat x log(Household Income)		-0.002		-0.022
		(0.001)		(0.023)
Observations	17853	17853	17853	17853
Adjusted R^2	0.071	0.075	0.195	0.205
Occupation FE	Yes	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes

TABLE A9: Alternate Mechanism: Saved Money: This table documents the coefficient estimates of a modified regression equation 1 that includes triple interaction term with *Saved More*. *Saved More* is an indicator variable that equals one for borrowers whose $\frac{\text{Instalment Amount}}{\text{Household Income}}$ is greater than median, and zero otherwise. This variable captures the importance of money saved due to extra moratorium in March 2020 by the treated borrowers. We scale the instalment amount by the household income to get the relative importance of the saved money for that household. We show results for three dependent variables Repayment in columns (1) and (2), Missed Instalment in columns (3) and (4), and Non-Performing Asset (NPA) in columns (5) and (6). Variables used in the regression are described in Section 3. All regressions have borrower fixed effects and year-month fixed effects. Columns (2), (4), and (6) also include loan terms, borrower characteristics, and their interaction with the Post variable as controls, along with Occupation × Post fixed effects. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment		Missed Instalment		NPA	
Saved More	-0.011	0.010	0.030***	-0.006	0.007*	-0.005
	(0.009)	(0.011)	(0.007)	(0.009)	(0.004)	(0.006)
Post	-0.061***		0.067***		0.019***	
	(0.005)		(0.005)		(0.002)	
Treated × Post	-0.096***	-0.068***	0.087***	0.067***	0.027***	0.022***
	(0.012)	(0.009)	(0.011)	(0.008)	(0.005)	(0.004)
Treated × Saved More	-0.004	-0.012	-0.012	-0.003	0.006	0.008
	(0.018)	(0.015)	(0.017)	(0.014)	(0.009)	(0.009)
Saved More × Post	0.004	-0.007	-0.009*	0.007	-0.003	0.005
	(0.006)	(0.006)	(0.005)	(0.006)	(0.002)	(0.003)
Treated × Saved More × Post	0.014	0.009	-0.012	-0.009	-0.008	-0.007
	(0.009)	(0.008)	(0.009)	(0.007)	(0.005)	(0.004)
Observations	299389	299389	299389	299389	263834	263834
Adjusted R^2	0.184	0.260	0.224	0.304	0.149	0.177
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes
Borrower and Loan Controls		Yes		Yes		Yes
Borrower and Loan Controls × Post		Yes		Yes		Yes

TABLE A10: Outstanding Loan Amount and Moratorium: In this table, we investigate how the length of moratorium correlates with the outstanding loan amount, and whether this outstanding loan amount explains the main results. Column 1 documents the coefficient estimates after regressing log (outstanding loan amount) on length of moratorium in September 2020. Columns 2, 3, and 4 show results of the regression equation 1 after replacing *Total Loan Amount* with *Outstanding Loan Amount in September 2020*. This variable captures the outstanding loan amount right after the withdrawal of the moratorium scheme. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)
	log(Outstanding Loan)	Repayment (%)	Missed Instalment	NPA
Moratorium Length (Months)	0.133***			
	(0.008)			
Treated x Post		-0.083***	0.073***	0.023***
		(0.011)	(0.010)	(0.004)
$\ln(\text{Outstanding})_{Sep2020}$ × Post		0.001	0.044***	0.002
		(0.006)	(0.005)	(0.002)
Tenure x Post		0.077***	-0.142***	-0.046***
		(0.024)	(0.021)	(0.010)
Interest Rate x Post		0.041***	-0.019**	0.000
		(0.009)	(0.008)	(0.003)
Business Loan x Post		0.013	-0.010	-0.008**
		(0.012)	(0.011)	(0.004)
Female x Post		0.066**	-0.035	0.005
		(0.028)	(0.028)	(0.012)
Married		-0.026	0.043**	0.013
		(0.023)	(0.021)	(0.013)
Married x Post		0.008	-0.012	-0.008
		(0.011)	(0.011)	(0.006)
Age (Years)		-0.004	0.005	0.001
		(0.004)	(0.003)	(0.001)
Age x Post		0.000	-0.000*	-0.000
		(0.000)	(0.000)	(0.000)
log(Individual Income)		0.030***	-0.019***	-0.007**
		(0.009)	(0.007)	(0.003)
log(Individual Income) x Post		0.005	-0.007	-0.000
		(0.006)	(0.005)	(0.002)
log(Household Income)		0.009*	0.004**	0.000
		(0.005)	(0.002)	(0.001)
log(Household Income) x Post		-0.000	-0.004	-0.002
		(0.004)	(0.003)	(0.001)
Observations	17858	300113	300113	264397
Adjusted R^2	0.081	0.189	0.232	0.152
Borrower FE	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes	Yes	Yes
Time Period	Sep 2020			

TABLE A11: Moratorium Re-entry and Post-Period Outcomes: This table reports a triple-difference regression of Repayment (%), Missed Instalment, and NPA on *Re-entered* × *Treated* × *Post*. *Re-entered* equals one for borrowers whose span of moratorium months strictly exceeds their count of moratorium months inside the March–August 2020 window – i.e., borrowers who exited the moratorium and then re-entered. Odd columns report the bare specification with borrower and year-month fixed effects. Even columns add loan and borrower covariates interacted with *Post*, as well as Occupation × Post fixed effects, matching the covariate-augmented specification in Table 2. The coefficients on *Re-entered*, *Treated*, and *Re-entered* × *Treated* are absorbed by the borrower fixed effects and are not reported. Standard errors are clustered at the branch level and are reported in parentheses.

	Repayment (%)		Missed Instalment		NPA	
	(1)	(2)	(3)	(4)	(5)	(6)
Re-entered × Treated × Post	0.029*	0.001	-0.021	0.004	-0.012*	-0.004
	(0.017)	(0.014)	(0.016)	(0.013)	(0.006)	(0.005)
Treated × Post	-0.087***	-0.062***	0.078***	0.059***	0.024***	0.020***
	(0.012)	(0.008)	(0.011)	(0.007)	(0.004)	(0.003)
Re-entered × Post	-0.103***	-0.033***	0.097***	0.031***	0.010***	-0.007*
	(0.012)	(0.011)	(0.011)	(0.010)	(0.004)	(0.004)
Observations	300185	300185	300185	300185	264457	264457
Adjusted R^2	0.189	0.261	0.229	0.305	0.148	0.177
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation x Post FE		Yes		Yes		Yes
Borrower and Loan Controls		Yes		Yes		Yes
Borrower and Loan Controls × Post		Yes		Yes		Yes

TABLE A12: Accrued Interest at Moratorium Exit and Post-Moratorium Repayment: This table investigates whether the interest burden borrowers carried into the post-moratorium period explains the main treatment effect. The conditioning variable is the borrower-level stock of accrued interest at the end of August 2020, the last month of the RBI moratorium window, scaled by the borrower's pre-COVID monthly instalment amount in February 2020. Because this measure is fixed before the post-moratorium period begins, it cannot be mechanically inflated by post-moratorium missed payments, unlike the contemporaneous accrued-interest measure. Column 1 reports the coefficient from regressing this ex-ante measure on the length of the borrower's chosen moratorium, validating that longer moratoria produce larger ex-ante interest burdens. Columns 2, 3, and 4 report estimates from regression equation 1 after adding the ex-ante measure and its interaction with Post as controls. The level of the ex-ante measure is absorbed by the borrower fixed effects; identification of the interest channel comes from its interaction with Post. Standard errors are clustered at the branch level and are reported in parentheses.

	Accrued Interest / Instalment	Repayment	Default	NPA
	(1)	(2)	(3)	(4)
Treat × Post		-0.082***	0.079***	0.023***
		(0.010)	(0.009)	(0.004)
Accrued Interest (Aug 2020) / Instalment × Post		-0.706***	0.798***	0.175***
		(0.053)	(0.052)	(0.023)
Moratorium Length (Months)	0.042***			
	(0.001)			
Borrower FE	No	Yes	Yes	Yes
Year × Month FE	No	Yes	Yes	Yes
Occupation × Post FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Std Error Cluster	Lender	Lender	Lender	Lender
Observations	17858	300185	300185	264457
Adj. R-sq	0.345	0.210	0.262	0.160

TABLE A13: Narrow-Bandwidth and Donut Robustness: This table re-estimates the headline difference-in-differences specification (Equation 1) on a tight window straddling the 25 March 2020 cutoff, addressing the concern that confounders correlated with the elapsed time between a borrower's due date and any awareness of the moratorium scheme could drive the main result. **Panel A** restricts the sample to borrowers with disbursement-day-of-month in $[22, 27]$ – i.e., control borrowers due 22–24 March compared to treated borrowers due 25–27 March. **Panel B** additionally drops the two days immediately adjacent to the cutoff (24 and 25 March), comparing 22–23 March to 26–27 March only. Under either restriction, the gap between any control borrower's due date and any treated borrower's due date is at most six (Panel A) or five (Panel B) calendar days, which is shorter than the lag between the 27 March RBI circular and the lender's discretionary, retrospective grant of the moratorium in the first week of April. Within both panels, the dependent variable is *Repayment (%)* in Columns (1)–(2), *Missed Instalment* in Columns (3)–(4), and *NPA* in Columns (5)–(6); odd columns include only borrower and year-month fixed effects, while even columns add loan and borrower controls interacted with *Post* as well as Occupation × Post fixed effects. Standard errors are clustered at the lender level and reported in parentheses.

Panel A - Repayment Day - Treated: 22-24 Mar, Control: 25-27 Mar

	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment		Missed Instalment		NPA	
Treated × Post	-0.059***	-0.063***	0.055***	0.060***	0.019***	0.022***
	(0.014)	(0.013)	(0.013)	(0.012)	(0.006)	(0.005)
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes
Controls		Yes		Yes		Yes
Observations	127934	127934	127934	127934	112774	112774
Adj. R-sq	0.182	0.190	0.222	0.231	0.155	0.162

Panel B - Repayment Day - Treated: 22-23 Mar, Control: 26-27 Mar

	(1)	(2)	(3)	(4)	(5)	(6)
	Repayment		Missed Instalment		NPA	
Treated × Post	-0.062***	-0.064***	0.060***	0.063***	0.020***	0.022***
	(0.016)	(0.014)	(0.014)	(0.013)	(0.006)	(0.006)
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes		Yes
Controls		Yes		Yes		Yes
Observations	97831	97831	97831	97831	86259	86259
Adj. R-sq	0.185	0.193	0.223	0.234	0.155	0.163

TABLE A14: Placebo Tests: This table documents the coefficient estimates of regression equation 1 with new treatment variables: Placebo-within-Treated and Placebo-within-Control. We run these regressions on the original treatment and control group sub-samples, respectively. The new treatment variables split the sample from the midway date of the period, i.e. treatment group's (due-date is after 24th) new treatment variable (Placebo-within-Treated) has borrowers whose repayments were due after 27th of a month, and control group's (due-dates between 18th to 24th) new treatment variable (Placebo-within-Control) has borrowers whose repayments are due after 19th. We show results for Non-Performing Asset (NPA) as the dependent variable. Columns (2) and (4) have additional controls and Occupation × Post fixed effects. Variables used in the regression are described in Section 3. All regressions have borrower fixed effects, and year-month fixed effects. Standard errors are clustered at the branch level and are reported in parentheses.

	(1)	(2)	(3)	(4)
	NPA			
Placebo-within-Treated × Post	0.007	-0.001		
	(0.005)	(0.005)		
Placebo-within-Control × Post			0.003	0.001
			(0.003)	(0.002)
Post	0.032***		0.017***	
	(0.004)		(0.002)	
log(Loan Amount) × Post		-0.051***		-0.030**
		(0.016)		(0.013)
Tenure × Post		-0.016		0.002
		(0.019)		(0.015)
Interest Rate × Post		0.022**		0.002
		(0.009)		(0.003)
Business Loan × Post		-0.010		0.001
		(0.006)		(0.003)
Female × Post		0.013		0.008
		(0.032)		(0.010)
Married		0.001		0.008
		(0.022)		(0.015)
Married × Post		0.007		-0.008
		(0.011)		(0.006)
Age (Years)		-0.000		0.002
		(0.002)		(0.001)
Age × Post		0.000		0.000
		(0.000)		(0.000)
log(Individual Income)		-0.006		-0.007**
		(0.007)		(0.003)
log(Individual Income) × Post		0.002		0.002
		(0.003)		(0.001)
log(Household Income)		-0.004**		0.002
		(0.002)		(0.002)
log(Household Income) × Post		-0.002		-0.001
		(0.003)		(0.001)
JLG × Post		0.002		-0.004
		(0.020)		(0.008)
Observations	94502	94502	169955	169955
Adjusted R^2	0.173	0.213	0.126	0.145
Borrower FE	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes
Occupation × Post FE		Yes		Yes
Subsample	Original Treatment Group		Original Control Group	

TABLE A15: Robustness on Matched Sample and Loan-Type Subsamples: Columns (1) and (2) re-estimate the headline triple-interaction specifications on the Mahalanobis-distance matched sample of Section 2.2 (covariate balance reported in Appendix Table A2). Column (1) interacts *Old JLG* with *Treated* × *Post*; Column (2) interacts *Moratorium Length* with *Treated* × *Post*. Columns (3) and (4) re-estimate the headline DiD on subsamples restricted to business loans and consumption loans, respectively, directly addressing the loan-purpose imbalance. Panel A uses *Repayment*, Panel B uses *Missed Instalment*, Panel C uses *NPA*. All regressions include borrower and year-month fixed effects, occupation × post fixed effects, and the full set of loan-term and borrower-characteristic controls (and their interactions with *Post*). Standard errors clustered at the branch level in parentheses.

Panel A: Repayment

	(1)	(2)	(3)	(4)
Treated × Post	-0.089***	0.049***	-0.065***	-0.060***
	(0.016)	(0.012)	(0.009)	(0.012)
Old JLG × Treated × Post	0.030*			
	(0.017)			
Old JLG × Post	-0.014			
	(0.015)			
Mora Length × Treated × Post		-0.046***		
		(0.006)		
Mora Length × Post	-0.103***	-0.077***	-0.095***	-0.100***
	(0.005)	(0.005)	(0.006)	(0.006)
Observations	213545	213545	176637	123548
Adj. R-sq	0.274	0.278	0.248	0.279

Panel B: Missed Instalment

	(1)	(2)	(3)	(4)
Treated × Post	0.083***	-0.053***	0.061***	0.064***
	(0.015)	(0.010)	(0.008)	(0.011)
Old JLG × Treated × Post	-0.028*			
	(0.016)			
Old JLG × Post	0.015			
	(0.015)			
Mora Length × Treated × Post		0.045***		
		(0.006)		
Mora Length × Post	0.096***	0.071***	0.090***	0.093***
	(0.004)	(0.005)	(0.005)	(0.006)
Observations	213545	213545	176637	123548
Adj. R-sq	0.317	0.322	0.293	0.319

Panel C: NPA

	(1)	(2)	(3)	(4)
Treated × Post	0.033***	-0.035***	0.013***	0.028***
	(0.008)	(0.006)	(0.003)	(0.006)
Old JLG × Treated × Post	-0.020**			
	(0.008)			
Old JLG × Post	0.005			
	(0.008)			
Mora Length × Treated × Post		0.020***		
		(0.003)		
Mora Length × Post	0.027***	0.016***	0.023***	0.027***
	(0.002)	(0.002)	(0.003)	(0.003)
Observations	188245	188245	155597	108860
Adj. R-sq	0.185	0.189	0.173	0.182
Sample	Matched	Matched	Business	Consumption
Borrower FE	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes
Occupation × Post FE	Yes	Yes	Yes	Yes