Borrower Distress And Debt Relief: Evidence From A Natural Experiment

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Abstract

Using unique borrower-level data, we study the causal effect of debt relief on loan performance of distressed and non-distressed borrowers. We employ a regression discontinuity design that exploits exogenous cut-off dates underlying the 2008 Indian debt waiver to separate loan defaulters into waiver beneficiaries and non-beneficiaries. By identifying distress before the waiver using exogenous borrower-level shocks, we examine performance on loans originated after the waiver. Loan performance of non-distressed beneficiaries worsens while that of distressed borrowers improves. While existing studies aggregate the effects of debt relief across distressed and non-distressed borrowers, we highlight crucial differences between them.

Keywords: Bank credit, credit market intervention, debt overhang, debt relief, default, loan, moral hazard, strategic default, over-indebtedness.

JEL Classification: G21, O2, Q14

1 Introduction

In this study, we examine the differential impact of debt relief on distressed and non-distressed agricultural borrowers. We use unique data at the borrower level and utilize the natural experiment provided by the $14.4 billion debt waiver in India in 2008. We show that heterogeneity among borrowers—specifically borrower distress—affects the outcome of a debt waiver.

Across the world, governments institute programs for debt relief/forgiveness during times of economic distress. One of the first legal codes—the Code of Hammurabi enacted in 1772 B.C.—advocates such relief (Mian and Sufi 2014). Apart from the Indian debt waiver program that we study, recent examples of such interventions include the US$ 2.9 billion bailout for farmers in Thailand and the rescheduling of about US$ 10 billion of agricultural debt in Brazil.

Debt relief relies on the simple idea that debt overhang can debilitate poor households and, thereby, the overall economy through general equilibrium effects (Mian and Sufi 2014). In emerging economies, debt relief assumes particular significance as a large proportion of the households remain not only poor but also vulnerable to income shocks.[1] In India, for example, farmer suicides attributable to local rainfall shocks and high indebtedness have reached alarming levels, with more than 59,000 deaths over the last 10 years (see Carleton 2017).

When compared to fiscal transfers or tax breaks, evaluating the effects of an intervention in the credit market poses significant challenges. On the one hand, debt contracts are highly incomplete as they do not provide for contingencies arising from an adverse state that is beyond the borrowers' control (Bolton and Rosenthal 2002). On the other hand, an intervention in the credit market can potentially alter borrower behavior—specifically prudent borrowing and repayment habits—by changing borrowers' belief about future interventions. While disentangling such opposing forces is inherently difficult, the key empirical challenge stems from the net effect of these forces being heterogeneous. So, average effects estimated using aggregated data can hide important heterogeneity in the effect of debt relief. Indeed, recent work on macroeconomic models with heterogeneous agents shows that the impact of stimulus programs may be misunderstood if heterogeneity among

[1] Jacoby and Skoufias (1997) show that exposure to repeated weather shocks affects households across generations. For example, school attendance of children living in households exposed to such income shocks is smaller than that of children living in low exposure households. See also (Deaton 2016, 1989) and (Datt and Hoogeveen 2003; Burgess et al. 2011).

beneficiaries is not accounted for.[2]

We study one key aspect of borrower heterogeneity—a priori distress—in examining the effect of debt relief. We consider borrowers who default on a loan due to liquidity shocks caused by exogenous events as "distressed borrowers." Other defaulters are considered as "non-distressed." Among the non-distressed, we do not distinguish between strategic defaulters and those who have long-term balance sheet issues, plausibly caused by poor inherent quality. Crucially, we identify borrower distress prior to debt relief and then carefully compare beneficiaries of debt relief to otherwise identical borrowers who could not receive debt relief for exogenous reasons.

On 29^{th} February 2008, the Indian Government announced a debt waiver program for past defaulters. In absolute terms, this debt waiver program ranks as the largest in an emerging market and as a percentage of GDP, the program ranks as the largest ever worldwide. This program waived the debt of about 40 million farmers, a group that is greater than the population of 85% of the countries in the world including countries of the size of Canada, Iraq, Poland, et cetera. The average loan amount waived equaled approximately 60% of a farm household's annual income.

We use some distinctive features of the program to study its effects. First, as we describe in section 3 below, the waiver came as an **unanticipated event**. Second, though the waiver was announced on 29^{th} February 2008, it was awarded only to borrowers who had defaulted two months back, specifically as on 31^{st} December 2007, and continued to be in default as of 29^{th} February 2008. Also, beneficiaries could neither have defaulted in anticipation of the waiver nor have self-selected into the program. Thus, the assignment of borrowers into beneficiaries and non-beneficiaries was exogenous to the program.

We employ a unique loan-level dataset provided to us by a bank in India. The data starts from October 2005 and ends in May 2012, which provides us a good before-after sample. The data pertains to crop loans that have a tenure of exactly one year. These loans do not have any interim coupon payments; they need to be repaid in full in one installment within one year of borrowing. These bullet loans enable us to cleanly identify the due date of loan repayment and loan default. A loan is considered to be in default if it is not fully repaid as on the due date. Our data contains

[2] Recent evidence on fiscal stimulus programs in the U.S. have led to a revision of the celebrated Permanent Income Hypothesis as a large part of the transfers were spent in the same quarter, primarily on non-durables. Moreover, the response to these transfers varied significantly with zero marginal propensity to consume (MPC) for one group and 0.5 for other groups. Using this evidence, Kaplan and Violante (2014) hypothesize the existence of wealthy hand-to-mouth agents, who form a larger proportion of the economy than poor hand-to-mouth agents.

information about the date of loan issuance, loan amount, date of repayment, the exact amount to be repaid and the interest/penalty charged on the loan. We hand-collect data from 14 branches located in three large states, which account for nearly one-sixth of India's population.

We use a sharp regression discontinuity (RD) design to study the causal effects of debt relief. As Lee and Lemieuxa (2010) argue citing Hahn et al. (2001), "RD designs require seemingly mild assumptions compared to those needed for other non-experimental approaches." We exploit the fact that the waiver was awarded to only those borrowers who defaulted on a loan on or before 31^{st} December 2007 and continued to be in default until 29^{th} February 2008. 31^{st} December 2007, which has no significance for agricultural production in India, serves as the sharp cut-off date. Think of two borrowers who borrowed one-year crop loans on 30^{th} December 2006 and 1^{st} January 2007. Assume both borrowers defaulted on their loans. Also assume that both the loans remained overdue on 29^{th} February 2008. As per the rules of the waiver, the first borrower is eligible for waiver but the second borrower is not. Thus, borrowers separate into beneficiaries and non-beneficiaries based on the artificial cut-off date of 31^{st} December 2007. As we describe in section 3.3, the program was completely unanticipated. Also, in section 6.4.1, we reason that borrowers cannot manipulate into treatment and follow McCrary (2008) to provide evidence consistent with the same. As the crop loans are of one-year maturity, a borrower would have to anticipate the waiver by 30^{th} December 2006—fourteen months before the waiver was announced—so that he could insist on the loan being originated before this date. Such advanced preparation and anticipation is implausible when the borrower is a small farmer.

Using crop *loans provided after the waiver to the same set of borrowers* that had defaulted before the waiver, we compare the loan performance of beneficiaries vis-a-vis similar non-beneficiaries. By design, all the borrowers in the RD sample had defaulted on a loan before the waiver. The RD tests thus exogenously separate defaulted borrowers into treatment and control groups based on the date when the loan went into default. We examine the performance on the loans given to these borrowers after the waiver. Also, waiver beneficiaries and non-beneficiaries do not differ significantly in the probability of obtaining a loan after the waiver. So, the tests alleviate concerns that our results are driven by possible substitution effects, where a new set of borrowers gets a loan after the waiver.

We proxy distressed borrowers as those that have filed a claim for crop insurance before the waiver. As we discuss in detail in Section 3, crop insurance indemnifies loss caused to a farmer by pests and crop diseases as well as natural disasters such as natural fire, lightning, inundation, landslide, drought and dry spells (Ministry for Agriculture and Farmers Welfare 2000). We make the reasonable assumption that a borrower who files for and receives crop insurance claim is affected by one of these exogenous shocks. To avoid fake claimants, we do not classify as distressed such borrowers whose insurance claim was rejected.

The presence of crop insurance, however, cannot substitute for the effect of debt waiver to distressed borrowers. During our sample period, crop insurance was heavily subsidized in India. Hence, the government restricted significantly the sum assured. Infact, the average claim to loan value ratio in our sample is significantly lower than 10%. As well, the borrower eventually received the claim amount with a significant lag, sometimes up to two years, after filing the claim. Therefore, while filing a claim credibly signals borrower distress due to an exogenous shock, receiving an insurance claim does not ameliorate the consequences of such a shock. So, **crop insurance cannot substitute** for the possible benefits of a debt waiver to distressed borrowers. In contrast, defaulting borrowers who did not file an insurance claim before the waiver—despite possessing crop insurance—plausibly did not suffer from the exogenous shocks described above.[3] As a sanity check, we test and find (i) that borrowers who claim crop insurance default more on their bank loans when compared to other subscribers for crop insurance; (ii) no significant discontinuity in insurance claims around the exogenous cutoff date for the RD.

We find that distressed waiver beneficiaries default 10% less on the loans issued to them after the waiver when compared to other comparable (distressed) non-beneficiaries. In contrast, non-distressed waiver beneficiaries default 23% more than comparable (non-distressed) non-beneficiaries. These results indicate that a debt relief is helpful when a borrower defaults because of exogenous liquidity shocks. However, when default is likely to be caused either by structural balance sheet issues or by strategic behavior, debt relief is likely to be counter-productive as it is likely to fuel further loan default. We derive these implications from a stylized model presented in the Appendix.

We perform multiple sets of robustness tests. First, we argue that measurement error created

[3]Note that we do not claim that uninsured shocks cannot create borrower distress. However, as the insured factors dominate the risks faced in agriculture in an emerging economy, defaulting borrowers that file an insurance claim are more likely to have suffered an exogenous liquidity shock when compared to the defaulting borrowers that do not file an insurance claim. Moreover, the incidence of any uninsured shock is unlikely to vary systematically either with the cut-off date for the RD or between farmers that we categorize as distressed and non-distressed.

by our borrower-level proxy for distress do not affect our results. The measurement error would have to account *simultaneously* for the improved loan performance of distressed beneficiaries and the worse loan performance of non-distressed beneficiaries due to the waiver. This is implausible especially given the exogenous cutoff date that separates beneficiaries and non-beneficiaries. Second, we measure distress at the branch level as an orthogonal proxy to the borrower-level measure and find that our results remain unchanged. Not only do these tests alleviate residual concerns about measurement error affecting our results, but also exhibit the robustness of our results to alternative definitions of distress. Third, we provide support for an important identifying assumption underlying RD, i.e. there is no discontinuity in baseline characteristics. Finally, because we compare the performance of new loans issued after the waiver with the performance of loans before the waiver, a potential concern may be that the quality of borrowers receiving a loan after the waiver may systematically influence our results. We show that the about 90% of farmers borrow again after the waiver and that there is no discernible difference between the beneficiaries of the debt waiver program and the non-beneficiaries. We also find that the loan amount and other loan terms did not change materially after the waiver. These results mitigate the concern that our key finding stems from potential selection bias.

Our results suggest policy implications that are more nuanced than those suggested by the existing empirical studies. First, consistent with the theoretical arguments in Bolton and Rosenthal (2002) and in the poverty trap theories (Banerjee and Newman 1993; Mookherjee and Ray 2003), debt relief targeted at distressed beneficiaries is likely to improve loan performance. Thus, governments may not necessarily be wasting scarce fiscal resources to serve their narrow political interests if a debt waiver is targeted towards distressed borrowers. In fact, though the economic environment we study comprises agricultural loans in an emerging country, our findings and the attendant policy implications are similar to those in Mian and Sufi (2014), who contend that the lack of debt forgiveness on housing loans exacerbated the Great Recession. Second, a debt waiver that is granted to all borrowers—whether distressed or not—can not only waste scarce fiscal resources but also be counter-productive by increasing loan defaults.

2 Review of Literature

To the best of our knowledge, ours is the first empirical study to examine the causal effect of debt relief on distressed and non-distressed borrowers (within the same program). Our study relates closely to Kanz (2015) and Giné and Kanz (2016), who also study the Indian debt waiver program of 2008. Kanz (2015) and Giné and Kanz (2016) document the costs associated with the debt waiver program. Specifically, Kanz (2015) uses household surveys to show that the debt waiver reduced the investment and agricultural productivity of the benefiting households. Giné and Kanz (2016) use aggregate data at the (district, bank) level to show that the debt waiver decreased the loan performance of all beneficiaries, especially in those districts where program exposure was high. Our study highlights that the aggregate effects estimated by Kanz (2015) and Giné and Kanz (2016) **hide substantial heterogeneity** in the effects of a debt waiver, especially depending upon whether the borrower is distressed or not. When assessing the policy impact of a debt waiver, incorporating such heterogeneity is particularly important if the indebtedness among borrowers is widespread.

Several recent studies have examined the costs and benefits of debt relief using different types of bankruptcy laws. Demiroglu et al. (2014) show that debt relief provided by several U.S. states during the U.S. housing crisis enhanced the likelihood of default on the housing loans. Goodman and Levitin (2014) show that the modifying the principal in Chapter 13 filings increases the interest rates for consumers. Other studies examine the costs and benefits of debt relief using different types of bankruptcy laws (Dobbie and Song 2015; Athreya 2002; Chatterjee and Gordon 2012; White et al. 1998; White 2007). These studies argue that debt relief programs help achieve smoothing across different states of the world possibly at the expense of inter-temporal smoothing (Livshits et al. 2007; Dubey et al. 2005; Tabb 1995; Skeel 2001; Bolton and Rosenthal 2002; Kroszner 2003). However, not only does a borrower choose to declare bankruptcy, the decision to file for bankruptcy is also influenced significantly by credit market conditions (Cohen-Cole et al. 2009). So, in these studies, it is difficult to disentangle the impact of debt relief from the endogenous circumstances faced by the borrower (Dobbie and Song 2015; Dick and Lehnert 2010) or the endogenous market conditions.

Given these limitations, several scholars have examined large scale government debt relief programs granted during harsh economic circumstances (Rucker and Alston 1987; Agarwal et al.(2016). While some studies find that such programs result in modest benefits (Agarwal et al.(2016), others have shown that such programs induce moral hazard and do not lead to any improvements in real outcomes (Kanz 2015; Giné and Kanz 2016; De and Tantri 2013). Arguing the benefits of debt relief, Mian and Sufi (2014) in fact contend that the lack of debt forgiveness exacerbated the Great Recession. Most of these studies, however, focus either on the benefits of debt relief to "distressed" borrowers (Bolton and Rosenthal 2002) or the costs created by "strategic" borrowers (Mayer et al. 2014; Guiso et al. 2013; Kanz 2015; Giné and Kanz 2016). This is because it is difficult to separate distressed borrowers from the non-distressed/strategic ones *ex-ante*. We contribute to this literature by exploiting a natural experiment to highlight the differential effect of debt relief on distressed and non-distressed borrowers.

3 Institutional Background

3.1 Agricultural Lending in India

Three key factors—risk, scarce collateral and poor enforcement of creditors' rights, and state control of banking—characterize the agricultural credit markets in emerging economies like India.

3.1.1 Significant Exposure to Risk

Agricultural lending in a developing country like India exposes farmers to significant risks. These risks result from (i) the income stream from agriculture remaining highly uncertain (Deaton (2016, 1989)); (ii) weather shocks creating significant risks and leading to permanent, high level of distress among farmers (Jacoby and Skoufias 1997; Datt and Hoogeveen 2003; Burgess et al. 2011); and (iii) farmers being significantly under insured against crop losses caused by weather shocks.

Nearly 44.1% of small farmers in India are illiterate (Mahadevan and Suardi 2013). Thus, they are unaware of technological developments for risk mitigation in farming. The farmers in our sample are quite small: they have landholding of less than 2 hectares. Small farmers are less likely to use modern technology as these involve fixed costs in learning and in financial investment. Given the size of their landholdings, such fixed costs are disproportionately high.

3.1.2 Scarce Collateral and Poor Enforcement

A common solution to mitigate strategic default is to have the borrower post a physical asset as collateral, which can be appropriated in case of default. However, most farmers in emerging economies are too poor to post any substantial collateral other than land or the expected crop itself. Also, poorly delineated property rights over land exacerbate the problem by making it difficult for the bank to foreclose the land that has been put up as collateral for the loan. Moreover, foreclosing a farmer's land is politically sensitive as local politicians, cutting across party lines, intervene on behalf of farmers irrespective of the merits of the case.[4] In addition, the judicial process is extremely slow. [5] Effectively, farmers in India do not face the threat of their land being taken over by their lenders, which encourages strategic default.

3.1.3 State Controlled Banking System

The Government of India plays a dominant role in the banking sector: approximately 71% of the banking system (as measured by assets) is owned by the government. The Indian government nationalized many private banks in 1969 and 1980 and enacted several regulations to improve access to finance to "critical" sectors and to vulnerable sections of the population. Priority sector lending guidelines require that at least 18% of a bank's credit should be directed to agriculture.

3.1.4 Agricultural Loans in India

As agricultural loans come under the purview of priority sector lending, the rate of interest applicable for these loans is 7%, which is approximately 200 basis points lower than the risk free rate prevailing during our sample period and approximately 400 basis points lower than the bank deposit rate during our sample period. We study crop loans where the underlying crop is rice. Agricultural crop loans represent bullet loans, where the borrower repays the loan with accrued interest at the end of 12 months. In other words, no intermediate (coupon) payments are stipulated in the loan contract. Thus, a crop loan is considered overdue if such a loan remains outstanding

[4] In one such incident in Mysore, Karnataka, the lender was forced to return the tractor repossessed from a farmer as the farmer committed suicide. The local politicians alleged that the suicide was due to "arm twisting" tactics employed by the recovery agents of the bank (The Hindu 2008).

[5] World Bank's doing business survey 2012-2013 ranks India 132 out of 185 in terms of ease of doing business. In terms of enforcement of contracts India occupies 17th rank out of 185 countries surveyed. Also, in India it takes on an average 1420 days to enforce a contract. In comparison, in Singapore the same takes just 150 days.

for more than 365 days. However, every overdue loan is not considered as a non-performing asset. As per guidelines issued by The Reserve Bank of India (RBI), crop loans need to be recognized as non-performing assets only if they remain overdue for at least two crop seasons (Reserve Bank of India 2004).

3.2 Crop Insurance in India

Comprehensive Crop Insurance Scheme (CCIS) was introduced in India in the year 1985. The purpose of the scheme was to indemnify the farmers in the event of failure of crops as a result of natural calamities. The scheme was limited to farmers who availed crop loans. In the year 1999-2000, the Government of India introduced the *National Agricultural Insurance Scheme (NAIS)* that aimed to cover all farmers. The Ministry of agriculture, Government of India reports that NAIS has covered about 269.1 million farmers. Claims worth INR. 506.1 billion have been paid so far. The insurance scheme is heavily subsidized. 75% of the premium is paid by the state and federal governments. The scheme indemnifies the loss caused by natural events including natural fire and lightening, inundations and landslides caused by excessive rainfall, droughts and dry spells, pests and diseases et cetera. The state government is expected to determine the normal yield through surveys and experiments. The loss is determined by comparing the actual production with the normal production. The claim amount is lower of the loss caused and the sum assured. To avoid a burgeoning subsidy burden, the Government sets the sum assured under NAIS to be quite low. In fact, we find the insurance claim amount is less than 10% of the loan amount in our sample. As a result, the amount indemnified under NAIS does not make a meaningful difference to the farmers' liquidity condition.

3.3 India's Debt Waiver Scheme of 2008

As a part of the financial budget speech delivered on 29^{th} February 2008, the then Finance Minister of India announced an unprecedented bailout of indebted small and marginal farmers. The "Debt Waiver and Debt Relief Scheme for Small and Marginal Farmers" affected about 40 million farmers and provided subsidies worth approximately INR 715 billion (US$14.4 billion). The average loan waiver of approximately $360 amounted to 60% of a farm household's annual income.[6] All formal agricultural debt disbursed by commercial and cooperative banks between 1997 and 2007 came under the purview of this scheme. All agricultural loans that were either overdue or were restructured (after being overdue) as on 31^{st} December 2007 and continued to be overdue till 28^{th} February 2008 qualified for the debt waiver.[7] Thus, no borrower that satisfied the eligibility criteria was refused the waiver. The Government set a deadline of 30^{th} June 2008 for the implementation of the program.

The debt waiver scheme was an unanticipated event. First, concerned with the dismal performance of the agricultural sector and rising farmer suicides,[8] the Government of India set up a high powered committee (the Radhakrishna Committee) "to look into the problems of agricultural indebtedness in its totality and to suggest measures to provide relief to farmers across the country." In its report submitted in 2007, the Committee recommended setting up of a Government fund to provide loans to the farming community. However, the Radhakrishana committee *did not* recommend a loan waiver. Second, the previous national level debt waiver was announced about two decades back in 1990. Though five parliamentary elections were held between 1990 and 2008, unlike the current scheme, no waiver was announced prior to any of these elections. Finally, media reports before the 2008 budget did not mention the debt waiver as a prominent expectation.

Crucially, in our setting, borrowers could not qualify for the waiver by acting strategically after the announcement was made on 29^{th} February 2008. This is because the loan status as on 31^{st} December 2007 was used to decide whether a borrower were qualified for the loan waiver or not. As all agricultural crop loans have a maturity of one year, all those borrowers who defaulted on their loans on or before 31^{st} December 2007 should have borrowed their loans before 31^{st} December 2006—14 months before the announcement of the waiver. So, concerns about self-selection around the cut-off (Imbens and Lemieux 2008) are significantly ameliorated.

Full waiver was granted to qualifying borrowers who had pledged ≤ 2 hectares of land and a partial, limited to 25% of the outstanding amount, relief was granted to borrowers who had pledged

[6] The annual per-capita income in rural India, which provides a reasonable proxy for the per-capita income of agricultural households, equaled INR 40,772 or $630 in 2011-12 (The Times of India 2016).

[7] Large farmers—those with a landholding of more than 2 hectares—qualified for partial waiver. They were granted a waiver of 25% of the outstanding loan provided they brought in the remaining 75%.

[8] According to a UN report, more than 100,000 farmers have committed suicide since 1997, 87% of them after incurring an average debt of US dollar 835

more than 2 hectares of land. As well, partial waiver was conditional on the borrower repaying the balance 75% of the remaining balance, a difficult ask for constrained borrowers. Full waivers were implemented within 3 months of the announcement. Given the difficult conditions attached to the partial waiver, the deadline was extended several times, with only a fraction of eligible borrowers eventually participating in the waiver. Due to timing and selection issues, the inclusion of partial waiver beneficiaries is likely to significantly distort our analysis. Therefore, we drop such borrowers from the study and compare waiver beneficiaries with non-beneficiaries.

4 Hypotheses

To clearly exposit the mechanism and derive our hypotheses, we present a simple theoretical model in the Appendix. Here, we illustrate the intuition using a simple numerical example. We consider a simple two-period model where a debt waiver is implemented at $t = 0^{-}$. Borrowers differ on two dimensions in the model. First, distressed borrowers have low level of wealth at $t = 0$ as they default because of liquidity reasons. In contrast, non-distressed borrowers have high level of wealth at $t = 0$ as they default despite having the ability to repay. Second, some borrowers receive a loan waiver (waiver beneficiaries) while others do not (non-beneficiaries).

Suppose the distressed and non-distressed borrowers have a wealth of \$20 and \$70 respectively at $t = 0^{-}$. A crop loan of \$50 provided at $t = 0$ requires \$50 to be repaid in full at $t = 1$ (interest rate is normalized to zero for simplicity). Assume that the loan waiver at $t = 0$ waives the \$50 that is owed at $t = 0$ from the loan that was provided at $t = -1$. In our empirical setting, borrowers repay their loans so as to receive a new loan. Therefore, we assume that a borrower who repays her loan in full at $t = 1$ can get a new loan of up to \$75 at $t = 1$. Also, as in the costly state verification models, we assume that borrowers who default, despite having the ability to repay, are audited by the loan officer with a probability $\gamma = 0.60$. If the borrower is found to be in violation of contractual obligations, she has to pay the entire loan amount together with a 50% penalty (that is \$75 in our example). For the equilibrium to exist, we also assume that in the last period, bank audits happen with probability one , so that all borrowers with ability to pay, must do so at date $t = 2$.

The production function follows a simple Leontief technology, where the output, given an investment of k is given by

$$Y = \begin{cases} 1.6 \times \min(k, \bar{K}) & \text{with probability } p = 0.5 \\ 0.6 \times \min(k, \bar{K}) & \text{with probability } p = 0.5 \end{cases} \tag{1}$$

where the upper limit $\bar{K} = 200$ accounts for the fact that the land holdings of the borrowers are constant and can only support a maximum capital investment.

Panels A to D show the analysis for distressed beneficiaries, distressed non-beneficiaries, non-distressed beneficiaries and non-distressed non-beneficiaries respectively; the detailed calculations are presented in the appendix. Intuitively, distressed borrowers suffer from debt overhang, which reduces their investment and causes them to default because of their inability to repay. A waiver provides such borrowers liquidity and alleviates the problem of debt overhang. Higher investment increases these borrowers' ability to pay and thereby improves loan performance compared to borrowers that do not receive a waiver and continue to suffer from debt overhang. Therefore, a waiver improves loan performance of distressed borrowers. In contrast, as non-distressed borrowers are wealthier, a waiver incentivizes such borrowers to default strategically because the waiver puts enough cash in their hands so that they can invest subsequently without having to renew their loan. Non-distressed borrowers who do not receive a waiver still need a loan to invest, which incentivizes them to repay their loans.

Note that while the debt waiver is provided at t = 0, the non-distressed borrower's incentive to take a new loan at t = 1 is affected. So, the model does not predicate issues relating to borrower composition or selection at the time of the waiver, which is crucial for our empirical strategy.

5 Data and Proxies

5.1 Bank Loan Data

We use *unique* loan account level information from a bank in India. We hand-collect transaction level data for 14 branches located in four districts in the state of Andhra Pradesh, two districts in Karnataka, and three districts in Maharashtra. According to the latest Census, the three states together account for nearly one-sixth of India's population. We obtain loan-account level information on all waiver beneficiaries in 14 branches from October 2005 to May 2012.

The transaction records provided by the bank include the date of each transaction, a short description of each transaction, transaction amount, type of transaction (debit or credit), the account balance before and after the transaction and the type of balance (debit or credit). Using the account details provided to us by the bank, we obtain information on the date on which a loan was availed, date on which the loan was repaid, number of days the loan was outstanding, the interest charged et cetera. All the loans analyzed are crop loans with a one year maturity. We use the status of loan (current or default) as the dependent variable. A loan that is outstanding for 365 days or more is in default. As all the crop loans in our sample have a maturity of one year, following RBI norms, a loan that has not been repaid by the due date of maturity is considered to be in default.

5.2 Crop Insurance Data

Crop insurance transactions are also done using the loan account. We have information about premiums paid and the claims received. The Rupee value of each transaction and the dates on which these transactions were executed are also clearly mentioned. In the case of claims, the transaction descriptions state the date on which a claim is filed. We use the *date of filing* and not the date of receipt of claim to measure distress as the borrower eventually received the claim amount after a significant lag, sometimes up to two years, after filing the claim.

5.3 Proxies for Agricultural Distress

Distinguishing between distressed and non-distressed borrowers is key to our empirical design. We use local variation in shocks that are exogenous to the debt waiver program to distinguish between distressed and non-distressed borrowers ex-ante.

We consider as distressed borrowers those farmers who file for crop insurance claim during the agricultural season immediately before the waiver. Note that here we identify distress at the borrower level. As described in Section 3.2, the crop insurance scheme promises to indemnify the borrowers if the crop fails due to natural disasters or other reasons such as crop diseases and pests. Therefore, it is reasonable to infer that a borrower who has filed for crop insurance must have suffered a liquidity shock that is unrelated to the debt waiver program. To exclude fake claimants, we do not consider those borrowers whose insurance claims are eventually rejected.

We do not claim that crop insurance covers all kinds of distress. Instead, we make the reasonable assumption that a borrower who claims and eventually obtains crop insurance is likely to be distressed. While distress could be caused by other factors that are not covered by crop insurance such as personal health issues, accidents, violence in the area, our identification remains clean as long as these factors do not vary systematically (i) between crop insurance claimants and other crop insurance subscribers, *and (not or)* (ii) with the cut-off date for the waiver, which is implausible. As well, the identification strategy recognizes that different farmers could have different levels of crop realizations when faced with a similar weather shock induced by nature. The design also considers idiosyncratic shocks such as pest attack and crop diseases, which may impact some farms in an area but not others.

Some more details about the crop insurance scheme aid identification. First, to reduce the subsidy burden on the Government, the sum insured under the scheme is capped. A large portion of the insurance premium is paid by the federal and state governments. Under the scheme prevailing during our sample period, the insurance claims were paid on a pro-rata basis. Suppose a farmer, whose normal output is 100, suffers a loss of 20 units, then only 20% of the sum insured is paid (The Indian Express 2016; Firstpost 2016). This is revealed even in our sample as the ratio of average insurance claim value to loan value is less than 10%. Thus, an insurance claim does not ameliorate the distress caused by the insured event. Second, there is a significant delay in payment of claims. As a result, the indemnity amount that a borrower receives after filing for an insurance claim does not help to alleviate the liquidity crisis for the borrower. These two facts together make filing of insurance claim a credible signal of continuing borrower distress.

5.3.1 Association between Insurance Based Measure and Agricultural Distress

Although the association between insured events and agricultural distress is well established in emerging economies (Deaton (2016)), it is crucial to examine this association in our sample using our measure of distress. If indeed our measures capture distress, then it must be positively associated with loan default. To be sure that the incidence of filing a claim captures borrower distress, we run the following specification for the pre-waiver period:

$$\text{Default}_{it} = \alpha_i + \alpha_t + \beta \times \text{Claim}_{it} + \gamma \mathbf{X}_{it} + \epsilon_{it}, \tag{2}$$

where the dependent variable takes the value of one if a loan borrowed by a borrower i in a year t defaults and zero otherwise. The coefficient β captures the correlation between an insurance claim being filed by borrower i in year t and the probability of default on a loan held by the same borrower in year t; α_i and α_t denote borrower and year fixed effects. In addition, we control for the size of the loan using natural logarithm of the loan amount and total rainfall during the relevant crop season.

Table 1: Claims Filed and Probability of Default

Dependent Variable	Default Dummy (y=1 if loan defaults; 0 else)		
	(1)	(2)	(3)
Insurance Claim Dummy	0.62^{**}	0.57^{**}	0.57^{**}
	(0.11)	(0.11)	(0.11)
Log Loan Amount		0.36^{**}	0.36^{**}
		(0.02)	(0.02)
Standardized Kharif Rain			0.06^{+}
			(0.03)
Observations	4924	4924	4924
Borrower, Year FE	Yes	Yes	Yes

Notes: This table presents the correlation between our dummies for distress and default (y=1 if loan defaults; 0 else). We estimate OLS regressions. All the regressions employ borrower and year fixed effects. Standard errors are clustered at a borrower level and reported in parentheses. **, * and $^{+}$ denote statistical significance at at 1%, 5% and 10% levels.

Table 1 reports the results of the above regression, where the standard errors have been clustered at a borrower level. It is evident that the binary variable indicating whether a claim has been filed for the production period t by borrower i correlates very strongly with the borrower defaulting. Of course, we cannot control for other idiosyncratic shocks that are uncorrelated to production, but affect the loan repayment behavior of the borrowers. For example, borrowers that have suffered personal distress, such as unexpected medical expenditures, may be more likely to default on their crop loans. Identification of such shocks is impossible without being able to access the borrowers' household balance sheets, which unfortunately we do not have. Nevertheless, the high value of the coefficient β gives us reasonable confidence to use the insurance claim dummy as a measure of distress.

5.4 Descriptive Statistics

Table 2 reports the mean, median and standard deviation for loan size, the number of loans availed by a borrower, the probability of default, and the amount of insurance claim received conditional on a claim being filed. 57% of the borrowers file for an insurance claim in our sample: these constitute our distressed borrowers. Note that the claim amount is, however, 8% (5%) of the loan amount using the mean (median) values. Thus, while the insurance claim enables us to proxy borrower distress, it does not alleviate distress significantly.

Table 2: Summary Statistics

	Observations	Mean	Median	SD
Total number of Loans	10570	3.68	4.00	1.38
Loan Amount in Indian Rupees	10570	36882.55	28311.86	31465.05
Probability of Default	10570	0.62	1.00	0.49
Waiver Beneficiary Dummy	10570	0.85	1.00	0.36
Farmer Has Insurance Dummy	10570	0.57	1.00	0.49
Probability of Purchasing Insurance (Pre-Period)	10570	0.91	1.00	0.86
Probability of Filing Claim (Pre-Period)	10570	0.64	0.00	0.92
Premium Amount in Indian Rupees	10570	987.69	306.50	1853.17
Claim Amount in Indian Rupees	10570	962.84	0.00	3233.98

Notes: This table presents the summary statistics for our sample. The time period considered is 2005-2012. Note that the total number of observations pertain to borrower-loan counts for the overall Regression Discontinuity (RD) sample. As evident from the sixth row, about 91% of the borrowers have insurance during the pre-program period. The insurance RD sample excludes borrowers for whom insurance information is not available before the waiver.

6 Empirical Strategy and Results

6.1 Challenges to Identification

The key empirical challenge stems from the fact that *unobserved borrower quality* affects the likelihood of default and thereby eligibility into the waiver program. Unobserved borrower quality also influences subsequent loan performance because bad quality borrowers may either be unproductive or may divert their loans to unproductive uses. So, this omitted variable affects the likelihood of treatment as well as any outcome variable. Thus, empirical strategies that cannot control for unobserved borrower quality suffer from this endogeneity problem.

6.2 Regression Discontinuity Design

To overcome the above challenges to identification, we employ a RD that exploits two unique features of the program: First, as argued in section 3.3 and by Giné and Kanz (2016), the debt waiver scheme was *unanticipated.* Second, borrowers had no opportunity to strategically manipulate into treatment. In the RD, we restrict attention to a subset of borrowers who defaulted on their existing loans during the period of December 2007 - January 2008. As described in the introduction, the empirical strategy exploits the unique feature that borrowers had to be in default on their outstanding loan *as of 31st December 2007.* So, farmers who defaulted in the vicinity of, but *before* this cut-off date, were eligible to become beneficiaries of the program; but those who defaulted *after* the cut-off date were not. The cut-off date creates a sharp discontinuity in the treatment status. Our classification scheme thus reduces endogeneity concerns caused by unobserved borrower heterogeneity.

Identification using the RD design rests on the assumption that borrowers are assigned to the eligibility group based solely on the basis of a continuous forcing variable (or selection variable) s. The observations can then be categorized into two levels of treatments based on whether the observed value of the forcing variable exceeds an exogenous threshold $\bar{s}$ or not. The selection variable in our setup equals the number of days before or after the cut-off date (31st December 2007) when the loan becomes delinquent; thus, we set the exogenous cut-off as $\bar{s} = 0$. This characterization yields a simple rule for the discontinuity analysis:

$$t_i = t(s_i) = \begin{cases} 1 & \text{if } s_i \leq 0 \\ 0 & \text{if } s_i > 0 \end{cases} \tag{3}$$

Consider the farmer who obtained an agricultural loan on 10th December 2006. The loan is in default if it is not repaid by 9th December 2007. For this loan, $s_i = -21$. Thus, loans that became delinquent before 31st December 2007 (the key cut-off date for being eligible for the waiver) will have a negative value for the selection variable. In contrast, those loans that defaulted in January 2008 will have a positive value. Thus, the treatment variable correlates perfectly with the status as a waiver beneficiary.

The causal effect of the debt waiver on loan performance can be estimated as the discontinuity in the conditional expectations of the outcome variable at the cut-off:

$$\tau_{RD} = \lim_{s \downarrow \bar{s}} \mathbf{E}(Y_i | S_i = s) - \lim_{s \uparrow \bar{s}} \mathbf{E}(Y_i | S_i = s) \tag{4}$$

Intuitively, if the farmers who default around the cut-off date receive similar sets of shocks and do not differ in observed pre-waiver characteristics, then the difference in ex-post outcomes can be attributed to the borrower's treatment status. To estimate this causal effect, we run local linear regressions for post-waiver loan performance using the specification:

$$y_{it} = \gamma_0 + \tau_{RD} \times t_i + \gamma_1 \times f(s_i) + \gamma_2 \times [t_i \times f(s_i)] + \beta_k \times \beta_t + \Gamma \times \mathbf{X_i} + \epsilon_{it}, \tag{5}$$

where y_{it} is a dummy variable that takes the value of one if the loan under consideration issued to borrower i in year t after the waiver defaults and zero otherwise. t_i is the treatment dummy defined in (3) and $f(s_i)$ is a polynomial function of the forcing variable. $\beta_k \times \beta_t$ denote fixed effects for each pair of (branch, year). Thus, we estimate the RD by exploiting variation in waiver status within each (branch, year) pair. This variation comes about because of the differences in the default status of the loan as on 31^{st} Dec 2007 within each (branch, year) pair. $\mathbf{X_i}$ denotes a vector of controls that includes loan size and average rainfall during the loan period. We include these controls as they significantly affect the probability of default. The main coefficient of interest is τ_{RD}, which captures the LATE as defined in (4). We estimate both OLS and probit regressions on a narrow bandwidth of length $h \in [-30, 30]$ around the cut-off point. We cluster the standard errors at the (branch, year) level.

We focus on borrowers who have defaulted around the cutoff date of 31st December 2007 and consider a subset of farmers who have purchased crop insurance on their loan prior to the waiver announcement. We, however, do not require the borrowers to buy insurance during the post-waiver period. As argued above in Section 5.3.1, this could generate potential selection biases due to by moral hazard stemming from the insurance contract. Also, we cannot run specifications including borrower fixed effects because *the sample includes only the loans issued after the waiver*. These fixed effects would exclude all those borrowers who borrowed only one loan after the waiver and create potential selection problems because borrowers that receive only one loan after the waiver may be different from those that receive multiple loans after the waiver. In contrast, our sample exhibits no such differences as we highlight in Section 6.4.

6.3 RD Estimates for Effect of Waiver on Loan Repayment

We begin by presenting graphical evidence in figures **??** and **??**. The outcome variable of interest equals one if a loan defaults and zero otherwise on the loans originated after the waiver to the same set of borrowers that had defaulted before the waiver. Therefore, the outcome variable captures the likelihood of default on these loans. In figure **??**, we restrict the sample to distressed borrowers who defaulted on their pre-waiver loan. Borrowers on the left of the cut-off are waiver beneficiaries and those on the right are comparable non-beneficiaries. Notice a sharp discontinuity with a positive intercept at the cut-off. This implies that the probability of default of waiver beneficiaries is significantly *lower* than that of comparable non-beneficiaries when the sample is restricted to distressed borrowers.

Now consider the figure **??**, where the sample is restricted to non-distressed borrowers. The arrangement of borrowers exactly mimics the arrangement in figure **??**. Notice a sharp discontinuity here but in the opposite direction of the discontinuity in figure **??**. The intercept at the cut-off has a negative sign. This implies that the post-waiver default rate of waiver beneficiaries is significantly *higher* than that of non-beneficiaries when the sample is restricted to non-distressed borrowers.

We present the results of formal RD tests in Table 3. We estimate regression equation (5). In columns 1 and 2 (3 and 4), we limit the sample to distressed (non-distressed) borrowers. We present the results of a OLS model here. Following the suggestion made by Gelman and Imbens (2017), we restrict the degree of the polynomial function to two in all columns to capture any plausible non-linear effects. However, the results are robust to change in the degree of the polynomial function. We employ branch X year fixed effects in all columns. In addition, in columns 2 and 4, we include loan and branch level variables to account for the effect of rainfall and loan size.

In columns 1 and 2 and in line with figure **??**, we find that the distressed waiver beneficiaries default significantly *lower* than comparable distressed non-beneficiaries. For distressed borrowers, we find that the probability of default decreased by 10% after the waiver. Clearly the change is

Table 3: RD Design For Ex-Post Probability of Default

Dependent Variable	Post Period Default Dummy			
Group	Distressed Borrowers		Non-distressed Borrowers	
	(1)	(2)	(3)	(4)
Waiver Beneficiaries	-0.09**	-0.10**	0.23**	0.23**
	(0.008)	(0.004)	(0.079)	(0.077)
Observations	1788	1788	1565	1565
Branch × Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: This table reports the results from OLS regressions, where the dependent variable is a dummy variable that takes the value of one if the loan under consideration defaults and zero otherwise (y=1 if the loan defaults and 0 otherwise). The local average treatment effect (LATE) is captured by the coefficient of the Treatment dummy, which assumes a value of one for negative values of the forcing variable and zero otherwise. The correlation between treatment status based on RD rule and waiver status is one. Standard errors are clustered at (branch year) level and reported in parentheses. Columns 1 and 2 (3 and 4) estimate the regression discontinuity specification for the distressed (non-distressed) borrower groups. Borrowers are categorized as distressed if they receive at least one crop insurance claim during the pre-waiver period and non-distressed otherwise. **, * and $^{+}$ denote statistical significance at at 1%, 5% and 10% levels.

economically meaningful. In columns 3 and 4, we estimate the RD equation (5) using the sample of non-distressed borrowers. In line with the results presented in figure **??**, we find that the non-distressed waiver beneficiaries default 23% more when compared to comparable non-distressed non-beneficiaries. As an additional robustness, we repeat the above RD test using probit regressions and report the results in Panel A of Table A2 presented in the online appendix. The results remain largely unchanged.

These results are in line with the hypotheses presented in Section 4. Borrowers who defaulted in the pre-waiver period due to exogenous liquidity shocks outperform comparable distressed borrowers after the debt waiver. However, exactly the opposite happens when the waiver is awarded to non-distressed borrowers, who either default strategically or due to fundamental reasons (relating to credit quality).

6.4 Checking the Internal Validity of the RD Design

Validity of the RD design rests on two critical identifying assumptions. First, the selection variable should not show any discontinuous jump around the cutoff point, i.e., there is no threat of potential manipulation of treatment status around the cutoff point. Second, important observable characteristics do not show any discontinuity around the cutoff point.

6.4.1 Potential Manipulation into Treatment?

If the borrowers could have anticipated the timing and the eligibility status of the debt waiver program ex-ante, they could have timed their loan accordingly. However, for reasons already discussed in Section 3.3, we do not expect such manipulation to be a serious threat to identification. Moreover, in Panel **??** of figure **??**, which plots the density of borrowers who default around the cutoff date of 31^{st} December 2007, we do not observe higher density of borrowers on the left side of the cutoff point ($s_i \leq \bar{s}$) that corresponds to the waiver beneficiaries. Also, the McCrary (2008) test reveals no observable discontinuity around the cutoff point (see figure **??**). The proportion of borrowers on the right side of the threshold does not appear to be any different than that on the left side, which alleviates concerns about possible manipulation into the program.

6.4.2 Possible Discontinuity in Other Observable Characteristics?

We now examine if key observable characteristics prior to the waiver exhibit any discontinuity around the cutoff point. Figure **??** plots against the selection variable the means of (i) average loan size before the waiver, (ii) average loan size after the waiver, (iii) average insurance premium paid, and (iv) average insurance claims received. In panels **??**, **??**, **??** and **??**, we notice no discontinuity in any of the above variables. Crucially, it is reassuring to find no discontinuity in either the insurance premium paid or the claim received. Thus, as a proxy for borrower distress, the filing of an insurance claim does not correlate systematically with the waiver.

6.5 Corroborative Evidence using Local Area Weather Shocks

Our crop-insurance based measure of distress varies at the individual level and hence is likely to capture distress reasonably well. Even if this borrower-level proxy contains some measurement error, it is unlikely to vary systematically with the exogenous cut-off date for the waiver. Specifically, for the measurement error in the proxy to affect our results, the following conditions must simultaneously hold. First, the measurement error must have a systematically lower correlation with the probability of default by distressed waiver beneficiaries than by comparable distressed non-beneficiaries. Second, the measurement error must have a systematically higher correlation with the probability of default by non-distressed waiver beneficiaries than by comparable non-distressed non-beneficiaries. Such systematic correlations of the measurement error is implausible especially given the exogenous cutoff date that separates beneficiaries and non-beneficiaries.

Nonetheless, to ascertain further the robustness of our results, we examine our results using a measure of distress that varies geographically based the local area weather (rather than at the borrower level). We label a borrower as distressed if the local area in which the farmer resides faces drought immediately before the waiver.

In these tests, we use the incidence of drought in an area covered by a bank branch to identify distressed and non-distressed borrowers. We first identify the exact geographic location of a branch and collect data on rainfall in that location. The monthly precipitation data comes from “Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series” collected by Willmott and Matsuura at the University of Delaware, Center for Climatic Research. The data provides long-term monthly rainfall data on a 0.5×0.5 latitude-longitude grid for the years 1900-2014. The rainfall data is then matched to the branch locations using the latitude and longitude data from the GIS. To construct the drought variable, we follow the Percentage of Normal (PN) method as in Burgess et al. (2011). Here, we compare the actual average rainfall a particular area in the first six months after a loan is granted with its long-term average (LTA). We use the first six months after a loan is granted because the weather during the time when the crop is sowed matters most for the productivity of the crop. The LTA values are calculated using the rainfall data for the past 30 years (1975 - 2005). If the measured value falls short of a certain cutoff percentage of the LTA, the area is said to be suffering from drought. Following Pai et al. (2011), we use 80% as the designated cutoff. Thus, the drought variable for a loan originated in branch k in month m and year t takes a value one if

$$\text{Drought}_{kmt} = 1 \iff \bar{r}_{kmt} \leq 0.8 \times \bar{r}_k, \quad \bar{r}_{kmt} = \frac{1}{6}\sum_{i=1}^{i=6} r_{k,m+i,t}; \quad \bar{r}_k = \frac{1}{N}\sum_{i=1}^{i=N} r_{k,2005-i} \tag{6}$$

where $\bar{r}_k$ and $\bar{r}_{kmt}$ are the long-term average and average kharif rainfall in branch k in month m and year t. We report the results in Table 4. The organization of the table exactly mimics Table 3. We estimate OLS regressions. We find similar results using the branch-level measure for distress. These tests include (branch, year) fixed effects to absorb all omitted variables at the branch, year level. Note that our branch level measure of distress exhibits variation within a (branch, year) as we use the average rainfall in the first six months after a loan is originated. This allows us to estimate the results using (branch, year) fixed effects. These results provide further comfort that our results for the differing effects on distressed and non-distressed borrowers are unlikely to stem from measurement error in our insurance-based proxies.

Table 4: RD Design : Ex-Post Probability of Default Using Exogenous Drought Shocks

Dependent Variable	Post Period Default Dummy			
Group	Distressed Borrowers		Non-distressed Borrowers	
	(1)	(2)	(3)	(4)
Waiver Beneficiaries	-0.22**	-0.22**	0.20**	0.21**
	(0.018)	(0.018)	(0.049)	(0.046)
Observations	2959	2959	1219	1219
Branch × Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: This table shows marginal effects from OLS regressions, where the dependent variable is a dummy variable that takes the value of one if the loan under consideration defaults and zero otherwise (y=1 if the loan defaults and 0 otherwise). The local average treatment effect (LATE) is captured by the coefficient of the Treatment dummy, which assumes a value of one for negative values of the forcing variable and zero otherwise. The correlation between treatment status based on RD rule and waiver status is one. Standard errors are clustered at branch year level and are reported in parentheses. Borrowers are categorized as distressed if they experienced at least one drought episode during the pre-waiver period. Columns 1 and 2 (3 and 4) estimate the regression discontinuity specification for the distressed (non-distressed) borrower groups. **, * and $^+$ denote statistical significance at at 1%, 5% and 10% levels.

7 Discussion about Possible Selection Effects

As with the existing studies (Kanz 2015; Giné and Kanz 2016; De and Tantri 2013), we observe equilibrium outcomes. Therefore, it is crucial to examine the possible effects of sample selection on our main results. Two forms of selection concern us. First, in general, waiver beneficiaries and non-beneficiaries may differ in the probability of obtaining a fresh loan after the waiver, on the extensive margin, and the loan amount, on the intensive margin. As these differences do not pertain differentially to distressed or non-distressed borrowers, we refer to this form of selection as "general selection." However, as our key results pertain to the differential impact of a debt waiver on distressed and non-distressed borrowers, the existence of "general selection" is not sufficient to spoil the interpretation of our results. Consider the effects from the supply side. If a loan officer is relatively more stringent with waiver beneficiaries while granting loans after the waiver, such "general selection" can only explain relatively lower defaults among distressed borrowers but not the relatively higher defaults among non-distressed borrowers. Therefore, for selection effects to spoil the interpretation of our results, it has to manifest in opposite directions for the distressed borrowers and the non-distressed ones. Thus, the loan officer must be more stringent with waiver beneficiaries than non-beneficiaries among distressed borrowers and less stringent with beneficiaries than non-beneficiaries among non-distressed borrowers. Similar arguments can be provided from the demand side as well. To distinguish such selection from "general selection", we label it "differential selection." We alleviate concerns about such selection by appealing to the institutional setting and by conducting RD tests.

Two institutional features noted in Section 3.1.3 and 3.1.4 alleviate concerns relating to selection. First, crop loans are lent at a rate that is lower than the risk free rate (and so significantly lower than normal bank lending rates). Therefore, loan demand is unlikely to differ between waiver beneficiaries and non-beneficiaries. Second, under priority sector lending mandated by law, Indian banks are required to allocate at least 18% of their credit to agriculture. Failure to comply with this requirement not only leads to penalty for the bank but also negatively impacts loan officer assessment (Bhowal et al. 2013). Therefore, "general selection" is unlikely in our sample.

Nevertheless, we perform an RD test to examine both forms of selection. To test whether a borrower receives a loan after the waiver, we organize the data at the borrower level and not at the borrower-loan level. The null hypothesis here is that the likelihood of obtaining a loan and the loan amount are identical for the waiver beneficiaries and the non-beneficiaries. Thus, a precisely estimated difference of zero between beneficiaries and non-beneficiaries would provide evidence that selection is not operative in our setting. We report the results in Table 5. In column 1, we consider the entire sample of borrowers to examine "general selection." We cannot reject the hypothesis that the difference between waiver beneficiaries and non-beneficiaries in the probability of obtaining a loan after the waiver is zero. Given the mean and standard error of -3% and 5% respectively, the 95% confidence interval equals [-12.8%, 6.8%]. Admittedly, the large confidence intervals indicate that this test does not provide a precise estimate of zero difference between the beneficiaries and non-beneficiaries.

In columns 2 and 3, we examine the above differences for the sample of distressed and non-distressed borrowers based on the status of their insurance claim (as in our main analysis). In both sub-samples, we are unable to reject the null hypothesis that the difference between the beneficiaries and non-beneficiaries is zero. Given the respective means and standard errors, the 95% confidence intervals equal [-2.8%, 8.8%] and [-4.8%, 14.8%] in columns 2 and 3 respectively. While these confidence intervals are large, "differential selection" can spoil the interpretation of our results if—in the extreme case—distressed waiver beneficiaries are 2.79% (slightly more than the 95% confidence interval of -2.8%) *less* likely to have a loan after the waiver while, at the same time, non-distressed waiver beneficiaries are 14.79% *more* likely to obtain a loan, both when compared to identical non-beneficiaries. These estimates of the extreme values assume that the following phenomena manifest. If selection manifests from the supply side, the loan officer is more stringent in providing loans after the waiver to distressed beneficiaries and simultaneously less stringent to non-distressed beneficiaries (both when compared to similar non-beneficiaries). If selection manifests from the demand side, lower quality distressed borrowers and higher quality non-distressed borrowers are less likely to approach the bank for a loan after the waiver (again when compared to similar borrowers that did not receive the waiver). While the large confidence intervals render the RD tests as suggestive evidence, taken together with the institutional setup and the need for "differential selection," we conclude that selection may not be accounting for our results.[9]

Further, we also examine the differences at the intensive margin, i.e loan amount for loans taken after the waiver. We report the results in Table A3 of the online appendix. Here too, we do not find any significant difference between the waiver beneficiaries and non-beneficiaries. Finally, we repeat the RD test examining the probability of a loan being granted after the waiver using probit regressions and find similar results in Table A4 in the online appendix. These results further support our hypothesis that selection of borrowers after the waiver is unlikely to impact our results significantly.

[9]In column 4, we repeat the test using the entire sample of 2,255 borrowers that subscribe to crop insurance. For completion, in column 5, we use the sample of borrowers who do not subscribe to crop insurance, and hence, are excluded from our main tests. These borrowers, however, are included in tests reported in Table 4, where we proxy distress using branch-level rainfall. Our main tests are based on the loan performance of these borrowers. We find no significant difference between waiver beneficiaries and non-beneficiaries.

Table 5: Probability of Borrowing Post Waiver

	Post Period Loan Dummy(y=1 if loan the farmer borrows in the post period; 0 else)				
	(1)	(2)	(3)	(4)	(5)
Sample	Full RD	Has Pre-Period Insurance			No Pre-Insurance
		Distressed	Non-Distressed	Full	
Waiver Beneficiaries	-0.03	0.03	0.05	0.04	-0.02
	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)
N	5795	1758	497	2255	3540

Notes: This table shows results from OLS regressions, where the dependent variable is a dummy variable that takes the value of one if the borrower under consideration obtains a loan in the after the waiver period and zero otherwise (y=1 if the loan defaults and 0 otherwise). The local average treatment effect (LATE) is captured by the coefficient of the Waiver Beneficiaries dummy, which assumes a value of one for positive values of the forcing variable and zero otherwise. The correlation between treatment status based on RD rule and waiver status is one. Standard errors are clustered at branch year level and reported in parentheses. We consider the full sample in column 1 and borrowers who did not subscribe to insurance in the pre period in column 5. In columns 2-4, we consider borrowers who subscribed to insurance in the pre waiver period. In columns 2 and 3, we consider the sample of distressed and non-distressed borrowers, classified as such based on insurance claim status. In column 4, we consider all insurance subscribers. **, * and $^{+}$ denote statistical significance at at 1%, 5% and 10% levels.

8 Conclusion

We examine the causal effect of debt relief on the loan performance of distressed and non-distressed borrowers by utilizing the $14.4 billion debt waiver in India in 2008. We combine unique loan-level data with a regression discontinuity design that exploits exogenous cut-off dates to compare waiver beneficiaries with similar non-beneficiaries. Our results are consistent with the hypothesis that debt relief improves the loan repayment behavior of distressed borrowers. However, debt relief extended to non-distressed borrowers adversely impacts their loan repayment behavior. Thus, our results highlight the need for: (i) separating distressed and non-distressed beneficiaries when assessing the effects of a debt waiver; and (ii) carefully targeting debt relief programs to benefit distressed borrowers.

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Online Appendix

A A Simple Model to Derive Hypotheses

We construct a stylized model to formalize the theoretical arguments and derive our hypotheses.

The Basic Setup

To understand the borrowers' incentives post the debt-waiver, we consider a model with three dates $t \in 0, 1, 2$. Assume a continuum of risk-neutral entrepreneurs of measure one, who choose actions at dates $t = 0$ and $t = 1$ to maximize their second period wealth w_2. Assume that the entrepreneurs enter date $t = 0$ with wealth $\tilde{w}$, which is drawn from a continuous distribution $G(\cdot)$ with support $[0, W], W \in \mathbb{R}_+$ and probability density function $g(w)$. The heterogeneity in the wealth level may stem from heterogeneity in default decisions in the previous period $t = -1$. Two types of defaults are of particular interest in the context of our empirical analysis. On the one hand, liquidity defaults occur when the borrower does not have enough resources to repay the loan, even after pooling all her assets. These types of defaults are typically associated with adverse idiosyncratic shocks and/or failure of the production process. On the other hand, strategic defaults occur if the entrepreneur chooses not to repay the loan despite having the ability to do so. Through out the exposition we will continue to use the terms borrowers and entrepreneurs interchangeably, since they refer to the same set of agents.

Let $\tilde{\ell}$ denote the amount of legacy debt that a borrower carries into time $t = 0$. We restrict $\tilde{\ell} \in \{0, \ell\}$ to a two-point support to focus on the distinction between waiver beneficiaries ($\tilde{\ell} = 0$) and non-beneficiaries ($\tilde{\ell} = \ell$). The entrepreneurs have access to a private production technology:

$$y = \varepsilon f(k) = \varepsilon A \min(k, K), \quad A > 1$$

where y is the output, k and K denote respectively the capital invested and the threshold level of capital beyond which the returns to capital investment are zero. The idiosyncratic productivity shock $\varepsilon \in [0, 1]$ follows the distribution function $P(\cdot)$. Production requires a linear input cost $C(k) = c_f k$, where $c_f > 0$ is the unit cost of production. We also assume that on average, production is more profitable than cash earning a risk-free rate $r_f = 0$:

$$\mathbb{E}(\varepsilon) > \frac{c_f}{A}. \tag{7}$$

The total profit from the production process is given by:

$$\Pi(k) = (A\varepsilon - c_f)k. \tag{8}$$

Since the profit function is linear in k and $\mathbb{E}\Pi(k) > 0$, the entrepreneur's optimal strategy is to invest the maximum wealth $\min(K, w/c_f)$ into production and keep the residual, if any, as cash.

Bank Actions at $t = 0, 1$ and 2

At time $t = 0$, the representative bank issues a new loan $\ell_0 > \ell$ to a borrower, which she can use for purchasing the factor input required for production. However, since a borrower starts with an outstanding loan $\tilde{\ell}$, this amount goes towards repayment of the outstanding debt and the net inflow from the bank to the entrepreneur is only $\ell_0 - \tilde{\ell}(1 + r_b)$. Here r_b is the interest rate on the credit extended by the bank, and is assumed to be exogenously specified. We assume that the bank cannot directly observe and verify the output y bank without paying an audit cost c_A. This creates an incentive for the borrowers to default even if they have the resources to repay the outstanding debt. In the event of a default, the bank audits the borrowers with probability $\gamma_1 \in (0, 1)$. Audit reveals the exact level of the shock ε, and if the borrower is found to be a repudiator, the bank imposes a fine ϕ_1 on the borrower and he is shut out of the credit market for the next period. If the bank chooses not to audit (with probability $(1 - \gamma_1)$), it can extend a second loan $\ell_1 > \ell_0$ to the borrower with a probability λ. After the second round of production, the bank audits the borrowers with probability $\gamma_2 = 1$, and all those who are held in contempt of the lending contract are imposed a harsher fine $\phi_2 = w_2$. Thus, their entire wealth is confiscated and the entrepreneurs are left with a net wealth of 0. This stark assumption, in essence, rules out strategic default for the loan in the second period as audit certainty and the harsh penalty creates an incentive for the borrowers to behave. We make this assumption because we only want to model the incentive to default strategically, which is obtained by the possibility of the bank not auditing in the first period.

Borrower Beliefs about a Future Waiver

Assume that at time $t = 0$, the borrowers believe that another round of debt relief will be institutionalized at time $t = 1$ with a probability θ. In the event of a waiver, the outstanding loan ℓ_0 for the entrepreneurs will be forgiven by the government and the banks will issue a new loan ℓ_1 with certainty. The true probability of the debt waiver for periods $t = 1$ and $t = 2$ is zero. Since we are interested in the incentives and repayment behavior for the period $t = 1$, we will abstract from the beliefs about waiver at $t = 2$ and assume, without loss of generality, that all the borrowers know there is no waiver at time $t = 2$ when the world ends.

Optimal Borrower Decisions at Time $t = 1_+$

To make the terminology clear, we split the date $t = 1$ into two sub-periods, where the first sub-period $t = 1_-$ pertains to the optimal default decision by the entrepreneur and bank audit/lending decisions; and the second sub-period $t = 1_+$ corresponds to the optimal investment decision for the second round of production.

Assume that entrepreneurs enter period $t = 1_+$ with wealth w_1 and outstanding loan ℓ_1. The total profit from the production process, given a capital level k_1 is $\Pi_2(k_1) = (A\varepsilon_2 - c_f)k_1$, and the total wealth evolution equation is given by:

$$w_2 = \left[w_1 + \Pi_2(k_1) - \ell_1(1+r_b)\right]^+. \tag{9}$$

The entrepreneur chooses the optimal capital k_1 to maximize utility:

$$\max_{k_1 \le \min\left(\frac{w_1}{c_f}, K\right)} \mathbb{E}_1[w_2] = \max_{k_1 \le \min\left(\frac{w_1}{c_f}, K\right)} \mathbb{E}_1\left[w_1 + \Pi_2(k_1) - \ell_1(1+r_b)\right]^+. \tag{10}$$

Since c_f is the unit cost of production, the total amount of capital the entrepreneur (with wealth w_1) can raise is $\frac{w_1}{c_f}$. Since the marginal productivity of capital falls to zero for all $k > K$, it is clear that $k_1 \le K$. Combining the two inequalities gives the subscript under the max term. The residual wealth is invested in cash. Finally, since the bank audits the second period output with certainty, the entrepreneur receives only the residual outflow after the outstanding liability to the bank is met. This is accounted for by the term $f^+ := \max(f, 0)$. The optimization program is linear in capital employed k and by our assumption $A\bar{\varepsilon} > c_f$. This implies that the optimal capital is always at the upper corner and $k_1^* = \min\left(\frac{w_1}{c_f}, K\right)$.

Since the bank audits the second period output with probability 1, there is no incentive to willfully default. So, default only occurs due to lack of liquidity, i.e. when the total existing assets of the entrepreneur are not enough to repay the loan. The probability of such liquidity default in period 2 is given by:

$$\Phi_2^d(w_1, \ell_1) = \int_0^{\bar{\epsilon}_2} dP(x) = P(\bar{\epsilon}_2), \quad where \quad \bar{\epsilon}_2 = \frac{\ell_1(1+r_b) - w_1}{Ak_1^*} + \frac{c_f}{A}. \tag{11}$$

The expected value function of the entrepreneur at period $t = 1_+$ is then simply:

$$V_{1_+}(w_1, \ell_1) = \int_{\bar{\epsilon}_2}^1 w_2(x)p(x)dx. \tag{12}$$

Optimal Borrower Action at $t = 1$

Before moving to the optimal default decision at $t = 1$, let us set up the problem by examining optimal borrower behavior at time $t = 0$. As explained in the previous subsection, the entrepreneur enters period $t = 0$ with wealth $\tilde{w} \sim G_{[0,W]}$ and legacy debt $\tilde{\ell} \in \{0, \ell\}$. The total wealth of the borrower including the net credit inflow is:

$$w_0 = \tilde{w} + \ell_0 - \tilde{\ell}(1+r_b),$$

of which $\min(w_0/c_f, K)$ is invested in production, and the residual $\max(w_0 - c_f K, 0)$ is saved as cash. Between $t = 0$ and $t = 1$, the productivity shock ε_1 hits and the net assets of the entrepreneur at the beginning of time $t = 1$ is:

$$w_1 = w_0 + \Pi_1(k_0) = \tilde{w} + \ell_0 - \tilde{\ell}(1 + r_b) + \left[A\varepsilon_1 - c_f\right] \min(w_0/c_f, K). \tag{13}$$

The outstanding liability of the entrepreneur is $(1+r_b)\ell_0$. Again, liquidity default occurs when the total assets of the entrepreneur are not enough to repay the outstanding liability:

$$\Phi_1^d(w,\ell) = P(\bar{\epsilon}_1), \qquad where \qquad \bar{\epsilon}_1 = \frac{\ell_0(1 + r_b) - w_0}{Ak_0^*} + \frac{c_f}{A}. \tag{14}$$

Strategic default probability at $t = 1$

To focus on the more interesting case of strategic default, assume that the first period productivity shock lies in the interval $[\bar{\epsilon}_1, 1]$. Since the probability of liquidity default is increasing in ℓ, this restriction ensures that the *ability-to-pay* constraint is never violated, irrespective of the level of legacy debt. The optimal value function for the borrower under repayment obtains as

$$V_1^{ND}(w_1, \ell_0) = V_{1_+}(w_1 - \ell_0(1 + r_b) + \ell_1, \ell_1), \tag{15}$$

where the value function V_{1_+} is defined in equation 12. On the other hand, if the borrower defaults, the following sequence of events manifest

- With probability θ, the agent believes that the government will announce a waiver program (even though there is no bailout in reality). In this case, the entire credit $\ell_0(1+r_b)$ is waived off and the borrower receives a fresh loan ℓ_1 with certainty. The total wealth of the borrower in this case is:

$$w_1^{\text{waiver}} = w_1 + \ell_1,$$

  and the optimal value function of the borrower is $V_{1_+}(w_1^{\text{waiver}}, \ell_1)$.

- With probability $(1-\theta)$, the agent believes that the government will not announce a waiver. In this case, the bank may audit the borrower with probability γ. Since we are only considering the incentive to default strategically, the borrower has the resources to repay the outstanding debt $\ell_0(1 + r_b)$. In the event of an audit, the bank seizes the amount $\ell_0(1 + r_b)$ and imposes a penalty ϕ on the borrower. Additionally, the borrower is shut off from the credit markets. The total wealth entering into the period $t = 1_+$ in this case is

$$w_1^{\text{audit}} = w_1 - \ell_0(1 + r_b) - \phi$$

  and the value function of the borrower is $V_{1_+}(w_1^{\text{audit}}, 0)$.

- If the bank does not audit the borrower, with a probability λ the bank rolls over the existing debt by providing an additional inflow of $\ell_1 - \ell_0(1 + r_b)$. The borrower wealth and value functions are respectively,

$$w_1^{\text{rollover}} = w_1 - \ell_0(1 + r_b) + \ell_1$$

and $V_{1_+}(w_1^{\text{rollover}}, \ell_1)$. This happens with probability $(1-\theta)(1-\gamma)\lambda$.

- Finally with residual probability $(1-\theta)(1-\gamma)(1-\lambda)$, the bank does not audit and does not rollover the existing debt. The existing debt stays in the books of the borrower for another period. The total wealth of the borrower in this case is

$$w_1^{\text{no rollover}} = w_1$$

  and the optimal value function is $V_{1_+}(w_1^{\text{no rollover}}, \ell_0)$.

Collecting all the terms above, the optimal value function under default is given by

$$\begin{aligned} V_1^D(w,\ell) = {} & \theta V_{1_+}(w_1^{\text{waiver}}, \ell_1) + (1-\theta)\gamma V_{1_+}(w_1^{\text{audit}}, 0) \\ & + (1-\theta)(1-\gamma)\lambda V_{1_+}(w_1^{\text{rollover}}, \ell_1) + (1-\theta)(1-\gamma)(1-\lambda) V_{1_+}(w_1^{\text{no rollover}}, \ell_0) \end{aligned} \tag{16}$$

The optimal default decision obtains from the simple incentive compatibility condition:

$$\mathbf{1}_D = \begin{cases} 1 & \text{if } V_1^D(w,\ell) > V_1^{ND}(w,\ell) \\ 0 & \text{if } V_1^D(w,\ell) \leq V_1^{ND}(w,\ell) \end{cases} \tag{17}$$

Numerical Simulations

The above model does not admit a closed form solution for the optimal default policy. To analyze the model, we thus resort to numerical simulations. The following table A1 gives the values of the various parameters of the model used in simulating optimal borrower behavior.

First consider the optimal borrower decision at time $t = 1$. Figure **??** depicts the optimal default and repayment regions for different values of the wealth vector w_1. The y-axis plots the incremental value from defaulting on the $t = 0$ loan $V_1^D(w,\ell) - V_1^{ND}(w,\ell)$. The figure features the two default thresholds and the intermediate inaction region, where repayment of the loan is optimal. The lower threshold corresponds to liquidity default when the starting wealth w_1 is low enough for the borrower to find it optimal to default. The upper boundary mainly stems from strategic concerns where the borrower has the resources to repay the loan $w_1 > \ell_0(1+r_b)$ but finds it optimal to default.

Finally, we move back to period $t = 0$, to consider separately the ex-ante probabilities for two types of borrowers: (i) borrowers who have received a waiver at $t = 0$ and the previous liability has been reset to 0; and (ii) borrowers who start with outstanding debt with face value ℓ. Each borrower starts with an ex-ante wealth w drawn from a distribution $G([0,W])$. For the simulations we assume $G \sim U[0,W]$. The productivity shocks are also assumed to be drawn from the uniform distribution $\varepsilon \sim U[0,1]$. Figure **??** captures the ex-ante probability of default on the loan originated at time $t = 0$.

Table A1: Parameter Values

Parameter	Description	Values
A	Productivity	2.2
c_f	Production unit cost	1
K	Maximum Production Limit	2
W	Maximum Value of Wealth Distribution	4
r_b	Credit Interest Rate	0.03
ℓ	Outstanding loan coming into $t = 0$	0.5
ℓ_0	New loan originated at $t = 0$	1.0
ℓ_1	New loan originated at $t = 1$	1.5
θ	Borrowers' belief about waiver at $t = 1$	0.3
γ	Probability of bank audit at $t = 1$	0.6
λ	Probability of Rolling over outstanding debt at $t = 1$ without auditing	0.5
ϕ	Punishment for delinquents in audit	0.5

Discussion and Hypotheses

Consider the first panel **??** of figure **??**. Consider a subset of borrowers with low levels of initial endowment $w \in [0, 1]$. In a repeated-interaction setting, this low wealth stems from adverse production shocks leaving the borrowers distressed. The solid gray line plots the ex-ante probability of default on the time $t = 0$ loan for the set of borrowers who receive waiver $\tilde{\ell} = 0$ at time $t = 0$. The black dashed line plots the corresponding probability of default for the group who enter period $t = 0$ with an existing debt $\tilde{\ell} = \ell$. Our first hypothesis therefore deals with the ex-post behavior of the distressed beneficiaries versus distressed non-beneficiaries.

Hypothesis 1. *A debt waiver program improves loan performance of distressed borrowers.*

To understand the underlying mechanism, consider the vicinity of the lower (*liquidity*) default threshold in figure **??**. Borrowers starting with lower levels of wealth w are much more likely to end up in the vicinity of this region. The waiver beneficiaries benefit from the positive liquidity shock ℓ_0 much more than the non-beneficiaries, who suffer from debt overhang and receive an incremental inflow of only $\ell_0 - \ell(1+r_b)$. The debt overhang faced by non- beneficiaries reduces their investment. Hence, they are more prone to liquidity driven defaults when an adverse shock leads to a violation of the *ability-to-pay* constraint. The waiver provides liquidity to the waiver beneficiaries and leads to higher investment (and potential holding of cash), which shields them better from loan default.

On the other hand, a debt waiver can engender costs due to borrower moral hazard and strategic default by borrowers that are not under distress. Bad quality borrowers, who are either unproductive or divert their loans to unproductive uses, may continue to exhibit similar behavior after the debt waiver. In this case, the debt waiver is unlikely to improve the loan performance of such borrowers. Also, borrowers may default strategically following the debt waiver. For example, Mayer et al. (2014); Guiso et al. (2013) show that when the U.S. home prices fell sharply, even those borrowers who had the resources to be current on their home loan obligations defaulted strategically. Similarly, anticipating another waiver–though the probability of the same was quite low in our setting–borrowers may exhibit moral hazard and default strategically. The effect of heightened moral hazard is shown in panel **??** of figure **??**. The moral hazard stems from an expectation of future waivers θ and unaudited loan ever-greening λ. Higher initial wealth pushes borrowers far into the strategic default boundary (see the higher cutoff point in figure **??**), where the borrower finds it optimal to repudiate on the existing lending contract in the hopes of a future waiver or a costless debt roll-over by the loan officer. The incremental liquidity provided by the waiver, $\Delta = \ell(1 + r_b)$, pushes the beneficiaries more into the strategic default region when compared to the non-beneficiaries who do not receive such liquidity. The probability of default is also increasing in wealth, which one should expect given moral hazard problems. This leads to our second testable hypothesis:

Hypothesis 2. *A debt waiver program leads to a deterioration in loan performance of non-distressed borrowers.*

B Additional Details for the Numerical Example

Consider distressed borrowers first. At $t = 0$, the waiver beneficiaries can invest \$120 (=\$20 wealth + \$50 liquidity from the waiver + \$50 from the new loan) while the non-beneficiaries can only invest \$70 (=\$20 wealth + \$50 from the new loan). As distressed borrowers do not strategically default, the waiver beneficiaries produce enough to repay in both good and bad states while non-beneficiaries produce enough to repay only in the good state and default in the bad state due to their inability to repay. This can be readily checked by writing down the incentive compatibility condition. Consider the beneficiaries first. In the good state, the output from first period production is $Y_1 = 1.6 \times 120 = 192$. If they repay and take a new loan of \$58 from the bank, their expected payoff from period 2 production is $Y_2 = 1.1 \times (192 - 50 + 58) - 58 = 162$. With the loan amount of \$58, the borrower hits the upper Leontief threshold of $\bar{K} = 200$ and does not need to borrow more. The expected period 2 payoff is the net of production output and repayment of the loan to the bank. If they default, with probability $\gamma = 0.60$, the bank audits and the borrower loses \$75 in principal and penalty, and they can only invest ($192 - 75 = 117$). With residual probability $(1 - \gamma) = 0.40$, the bank does not audit and the borrower invests the full amount \$192. The expected second period payoff is $0.60 \times 1.1 \times 117 + 0.40 \times 1.1 \times 192 = 161.70$. The other cases are solved in an identical manner.

Next consider non-distressed borrowers. At $t = 0$, the waiver beneficiaries can invest \$170 (=\$70 wealth + \$50 liquidity from the waiver + \$50 from the new loan) while the non-beneficiaries can invest \$120 (=\$70 wealth + \$50 from the new loan). When the cash flow is good for the non-distressed beneficiary, she does not need a fresh loan at $t = 1$ as the maximum she can invest at $t = 1$ is \$200. If she defaults, she gets audited with probability of 0.60 and has to pay a penalty of 50%. Yet she prefers to default because she does not need the loan at $t = 1$ and the retained cash from defaulting dominates the expected penalty. To see this, consider the incentive compatibility condition for the non-distressed beneficiary in the good state. The output from the first period production is \$272 which is larger than the maximum capital investment level of \$200. Consider first the case of repayment. In this case, the borrower repays \$50 to the bank and does not borrow again. This gives her a total second period payoff of $1.1 \times 200 + (272 - 50 - 200) = 242$. The farmer simply invests maximum $k = 200$ into second period production, and keeps the remaining amount of \$22 as cash. Consider next the scenario under default. With probability 0.6 the bank discovers her perjury and collects \$75 from her, leaving a net period 1 wealth of \$197 which is then invested in the second period production. With residual probability 0.4, the bank does not audit and the farmer invests \$200 in production while keeping \$72 as cash, giving her a total expected period 2 payoff of $\mathbb{E}(Y_2) = 0.6 \times 1.1 \times 197 + 0.4 \times (1.1 \times 200 + 72) = 246.82$. In contrast if the cash flow is bad for the non-distressed beneficiary, then she benefits from availing the new loan at $t = 1$ and decides to repay. The non-distressed non-beneficiary does not have the level of wealth to turn down the loan at $t = 1$. Therefore, she finds it incentive compatible to repay both when the cash flow is good and when it is bad. Thus, among the non-distressed borrowers, a waiver dampens loan performance.

C Additional Tables

Table A2: Ex-Post Probability of Default (Probit Regressions)

Dependent Variable	Loan Default Dummy			
Group	Distressed Borrowers		Non-Distressed Borrowers	
Panel A: Insurance Claim Based Distress Measure				
Waiver Beneficiaries	-0.06**	-0.06**	0.24**	0.26***
	(0.01)	(0.00)	(0.05)	(0.07)
N	1585	1585	1321	1308
Panel B: Local Area Drought Based Distress Measure				
Waiver Beneficiaries	-0.23**	-0.23**	0.22***	0.21**
	(0.02)	(0.02)	(0.05)	(0.05)
N	2010	2010	745	733
Branch × Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: This table reports marginal effects from probit regressions, where the dependent variable is a dummy variable that takes the value of one if the loan under consideration defaults and zero otherwise (y=1 if the loan defaults and 0 otherwise). The local average treatment effect (LATE) is captured by the coefficient of the Treatment dummy, which assumes a value of one for negative values of the forcing variable and zero otherwise. The correlation between treatment status based on RD rule and waiver status is one. Standard errors are clustered at (branch year) level and reported in parentheses. In Panel A, we use the measure of distress based on individual insurance claim and in Panel B, we use the measure based on rainfall at the branch level. Columns 1 and 2 (3 and 4) estimate the regression discontinuity specification for the distressed (non-distressed) borrower groups. **, * and $^{+}$ denote statistical significance at at 1%, 5% and 10% levels.

Table A3: Ex-Post Credit Rationing-Intensive Margin

Dependent Variable : Log(Loan Size)	(1)	(2)	(3)
Panel A: Distressed Borrowers			
Waiver Beneficiary	-0.02	-0.00	-0.05
	(0.04)	(0.05)	(0.03)
N	1995	1995	1995
R^2	0.669	0.670	0.670
Panel B: Non-Distressed Borrowers			
Waiver Beneficiary	0.06	0.11	0.01
	(0.15)	(0.21)	(0.20)
N	1541	1541	1541
R^2	0.633	0.635	0.636
Branch × Year FE	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes
Polynomial Order	Zero	One	Two

The above table reports the estimates from OLS regressions with the dependent variable being the log of the loan size during the post-waiver period. The local average treatment effect (LATE) is captured by the coefficient of the Treatment dummy, which assumes a value of one for negative values of the forcing variable and zero otherwise. The correlation between treatment status based on RD rule and waiver status is one. Standard errors are clustered at (branch year) level and reported in parentheses. Panels A and B estimates the regression discontinuity specification separately for the distressed and the non-distressed borrower groups. Borrowers are categorized as distressed if they receive at least one crop insurance claim before the waiver. **, * and $^+$ denote statistical significance at at 1%, 5% and 10% levels.

Table A4: Probability of Borrowing Post Waiver

	(y=1 if loan the farmer borrows in the post period; 0 else)				
	Post Period Loan Dummy				
	(1)	(2)	(3)	(4)	(5)
Sample	Full RD	No Pre-Insurance	Has Pre-Period Insurance		
			Full	Distressed	Non-Distressed
Waiver Beneficiaries	-0.09	-0.06	0.20	0.18	0.27
	(0.14)	(0.11)	(0.21)	(0.24)	(0.26)
N	5795	3540	2255	1758	497
Dependent Variable Mean	0.69	0.56	0.89	0.90	0.88

Notes: This table shows marginal effects from probit regressions, where the dependent variable is a dummy variable that takes the value of one if the borrower under consideration obtains a loan in the after the waiver period and zero otherwise (y=1 if the loan defaults and 0 otherwise). The local average treatment effect (LATE) is captured by the coefficient of the Waiver Beneficiaries dummy, which assumes a value of one for positive values of the forcing variable and zero otherwise. The correlation between treatment status based on RD rule and waiver status is one. Standard errors are clustered at branch year level and reported in parentheses. We consider the full sample in column 1 and borrowers who did not subscribe to insurance in the pre period in column 2. In columns 3-5, we consider borrowers who subscribed to insurance in the pre waiver period. In column 3, we consider all insurance subscribers. In columns 4 and 5, we consider the sample of distressed and non-distressed borrowers, classified as such based on insurance claim status. **, * and $^{+}$ denote statistical significance at at 1%, 5% and 10% levels.