Borrower Distress and Debt Relief: Evidence from a Natural Experiment

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

JEL Classification: G21, O2, Q14

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Abstract

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1. Introduction

In this study, we examine the causal effect of debt relief on both distressed and non-distressed agricultural borrowers. We study the $14.4 billion debt waiver in India in 2008 using unique loan-level data. We exploit some distinctive features of the program to study its effects using a sharp regression discontinuity analysis.

Motivation: Debt relief for distressed farmers has been advocated through the ages. For example, one of the first legal codes—the Code of Hammurabi enacted in 1772 B.C.—advocates such relief:

> "If any one owe a debt for a loan, and a storm prostrates the grain, or the harvest fail, or the grain does not growth for lack of water, in that year he need not give his creditor any grain, he washes his debt-tablet in water and pays no rent for this year." (Source: Mian and Sufi (2014))

In emerging economies, debt relief for agricultural borrowers assumes significance as a large proportion of the households engage in agriculture. Such households are not only large and poor but also remain vulnerable to income shocks. This vulnerability, in turn, results from (i) the income stream from agriculture remaining highly uncertain in developing countries (Deaton et al. (2016); Deaton (1989)); (ii) weather shocks creating significant risks and leading to permanent, high level of distress among farmers in developing countries (Jacoby and Skoufias (1997); Datt and Hoogeveen (2003); Burgess et al. (2011)); and (iii) use of agricultural insurance being limited (Cole et al. (2013)).[1] According to a U.N. report, farmer suicides originating from debt traps represent an important concern in emerging countries.[2] Given the vulnerabilities of agricultural borrowers, governments may feel the political pressure to develop mechanisms that alleviate ex-post agricultural distress (Dietrich and Ibanez (2015), Besley (1994); Bolton and Rosenthal (2002); Rucker and Alston (1987)). 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 (Kanz (2015)).

Existing empirical studies question the efficacy of such interventions. On the one hand, studies conclude that governments in emerging economies employ scarce fiscal resources to serve their narrow political interests (see Cole (2009b) and Khwaja and Mian (2005)). On the other hand, studies suggest that debt relief programs are ineffective (Kanz (2015); Giné and Kanz (2016)) because moral hazard limits their efficacy (Mayer et al. (2014); Guiso et al. (2013)).

Yet, theoretical studies advocate the need for such ex-post interventions to alleviate borrower distress. Bolton and Rosenthal (2002) contend that debt contracts are highly incomplete as they do not provide for contingencies arising from an adverse state that is beyond the borrowers' control. Therefore, adverse shocks can lead to inefficient foreclosures and thereby create significant deadweight costs. Political intervention in the form of debt moratoria can avoid inefficient foreclosures and the resultant deadweight costs (Bolton and Rosenthal (2002), Rucker and Alston (1987)).

[1] Given the limited use of insurance in agriculture in India, the current government has implemented a program for providing subsidised agricultural insurance; the program is called the Pradhan Mantri Fasal Bima Yojana, which translates into the Prime Minister's Crop Insurance program.

[2] Source: http://www.un.org/esa/sustdev/csd/csd16/PF/presentations/farmers_relief.pdf

Research question and empirical setting: In an attempt to resolve this confusion in the literature, we examine the causal effect of debt relief on the subsequent loan performance of *both distressed and non-distressed agricultural borrowers.* On 29^{th} February 2008, the Indian Government announced a debt waiver program. 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.

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. As we explain below, 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.

Data: We employ a unique loan-level dataset provided to us by a government-owned 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. Our data contains 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 transaction-level data from 14 branches located in three large states, which account for nearly one-sixth of India's population.

Identification using a regression discontinuity design: We use a sharp regression discontinuity (RD) design to study the causal effects of debt relief. As Lee and Lemieux (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. Borrowers who defaulted on the their loans just before the cut-off date of 31^{st} December 2007 form our treatment group and those borrowers who defaulted just after the cut-off date form our control group. 31^{st} December 2007 serves as the sharp cut-off date and the distance from the cut-off date serves as the running variable for the sharp RD design. Crucially, *borrowers on both sides of the cut-off are defaulters separated by an artificial cut-off date.* Also note that 31^{st} December has no significance for agricultural production in India. 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 borrowed their loan 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 in our setting. Nonetheless, following McCrary (2008), we perform tests to rule out bunching at the cut-off. In all our tests, we include fixed effects for each (branch, year) pair. Therefore, our tests exploit exogenous variation in waiver status within each (branch, year) pair. As a result, our tests control for confounding factors at multiple levels. In particular, branch level omitted variables that may affect borrower distress are not only controlled for but also cannot correlate with selection into treatment (because of the artificial cut-off date for the program).

Key Results: We find that the waiver beneficiaries, on average, default about 13.8% to 19.4% more than non-beneficiaries. This finding is broadly consistent with other studies analyzing this program (Kanz (2015); Giné and Kanz (2016)). Apart from several econometric issues that plague the analysis in these studies, which we describe in detail below, these studies do not distinguish between distressed and non-distressed borrowers.

For our salient findings, we burrow a layer further and estimate our RD regressions separately on the sample of distressed and non-distressed borrowers. As we use weather to proxy agricultural distress, we first establish that our measures of rainfall deficiency and drought positively associate with default on agricultural loans. Using these proxies, we find that distressed beneficiaries of the waiver outperform other distressed borrowers who had defaulted on their loan at about the same time but narrowly missed the waiver for exogenous reasons; the default rate of distressed waiver beneficiaries is *lower* by 16.2%-22.3% when compared to distressed non-beneficiaries. Our findings are exactly opposite for the sub-sample of non-distressed borrowers. Here, the waiver beneficiaries under-perform the comparable non-beneficiaries by 11.5% to 29.5%.

Robustness: We perform multiple sets of robustness tests. First, we perform a series of placebo and sensitivity tests to establish the robustness of the RD design. We test by altering the RD bandwidths, different forms of non-linearities, and differential slopes between the treatment and control groups. We also conduct several placebo tests for the RD design by re-estimating the RD for several false cut-off dates. Our main results remain robust. Second, we provide support for an important identifying assumption underlying RD designs, i.e. there is no discontinuity in baseline characteristics. Third, we use different measures for distress and find our results to be robust to the same. Fourth, because we examine the impact of a debt waiver and not debt relief as in Agarwal et al. (2016), we have to compare the performance of new loans issued after the waiver with the performance of loans before the waiver. Therefore, a potential concern may be that loan officers' criteria for selecting borrowers may systematically influence our results. We show that our results do not stem from such biases. Finally, to examine validity of our results outside the RD design, we perform a difference-in-difference test where we compare defaulters that did and did not receive the waiver because of the exogenous cut-off date. Our results remain robust to this larger sample.

Policy implications: 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), 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—without considering whether they are indeed 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 simultaneously.* 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. While we show that the debt waiver engenders costs when it is directed to non-distressed borrowers, we provide strong evidence that the debt waiver generates substantial benefits when it is directed to distressed borrowers. This nuance is particularly important given widespread indebtedness among agricultural borrowers in emerging economies (as described in section 1). Our study also relates to a growing literature examining the interface between law and economics in India and other emerging countries (see Chemin (2012); Peisakhin (2012); Prasad (2012); Alfaro and Chari (2014); Sukhtankar (2015)).

Several recent studies have examined the costs and benefits of debt relief using different types of bankruptcy laws. Berkowitz and Hynes (1999) make a distinction between secured and unsecured credit in examining how generous debt relief provisions affect credit markets. Lefgren and McIntyre (2009) attribute 70% of the cross-state differences in personal bankruptcy rates to variation in demographics, wage garnishment restrictions, and the fraction of bankruptcies filed under Chapter 13. Grant and Koeniger (2009) show that redistributive taxation and bankruptcy exemptions are negatively related policies that both help smooth consumption for borrowers. Traczynski (2011) shows that increases in bankruptcy exemption levels cause greater divorce rates in the U.S. Greenhalgh-Stanley and Rohlin (2013) show that the elderly are a lot more likely to file for bankruptcy in the U.S. as they face flat incomes and high medical expenses, on the one hand, and their retirement and housing assets are exempt from bankruptcy filings, on the other hand. 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 modification of principal in the case of Chapter 13 filings increase the interest rates on debt for consumers. Bhutta et al. (2016) show that restrictions on payday loans reduce payday lending while forcing consumers to shift to other high-interest credit. 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, a borrower chooses to declare bankruptcy. Moreover, the decision to file for bankruptcy is also significantly influenced by credit market conditions (Cohen-Cole et al. (2009)). So, in these studies, it is difficult to disentangle the impact of debt relief and 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 such programs resulting in modest benefits (Hembre (2014); 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)) 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 and combining the same with loan account level information to examine the causal effect of debt relief on distressed and non-distressed borrowers simultaneously.

3. Institutional Background

3.1. Agricultural Lending in India

Four key factors—significant exposure to risk, scarce collateral, state control of banking and poor legal enforcement—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. 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. Nearly 65% of the small farmers depend on rain fed irrigation (Mahadevan and Suardi (2013)). As well, more than 75% of Indian farmers are not covered by crop insurance (Mahul and Verma (2012)). The agricultural borrowers in our sample do not own a checking or savings account with the bank. This fact reflects the reality of financial exclusion in India where 51% of farmers do not even have a bank account (Karmakar (2008)).

3.1.2. Scarce Collateral

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.[3] In extreme cases, laws have been passed to render recovery

[3]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

of agricultural loans difficult; an example of this is the Andhra Pradesh Microfinance Institutions (Regulation and Moneylending) Act, 2010. 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 guidelines and branch expansion norms were among the significant regulations issued (see Burgess et al. (2005), Cole et al. (2011)). Priority sector lending guidelines require that 18%, 10% and 12% of a bank's credit should be directed respectively to agriculture, the weaker sections of society and small and medium enterprises. The Government of India introduced another set of guidelines that required the banks to open branches in four unbanked locations for every branch in a banked location. This substantially increased the branch network and improved access to finance in rural areas (see Pande and Burgess (2005)).

3.1.4. Poor Enforcement

Given state control of banking and the political economy of state controlled lending (see Cole, 2009a), recovery of loans has been a major challenge in India. Debt recovery tribunals and laws such as the "Securitization and Reconstruction of Financial Assets and Enforcement of Security Interest (SARFAESI)" Act do not apply to small agricultural loans. Thus, when it comes to agricultural loans, lenders do not have recourse to any special laws and have to rely on ordinary courts for enforcement. The slow judicial process compounds lenders' difficulties in loan recovery.[4]

3.1.5. 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 lower than the cost of funds of the banking sector. 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. The crop loans have a maturity of one year. Thus, a crop loan is considered overdue if such a loan remains outstanding for more than 365 days. However, every overdue loan is not considered as a non-performing asset. As per RBI guidelines, crop loans need to be recognized as non-performing assets only if they remain overdue for at least two crop seasons.[5]

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by the recovery agents of the bank. The Hindu, June 30, 2008.

[4] 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.

[5] Source: http://www.rbi.org.in/scripts/BS_ViewMasCirculardetails.aspx

3.2. 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). 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.[6] 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,[7] 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. But 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 loan 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 in our setting.

4. Hypotheses

In this section we lay out our empirical hypotheses.

Bolton and Rosenthal (2002) postulate that when bad economic shocks are highly likely, state-contingent debt moratoria always improve ex post efficiency and may also improve ex ante efficiency. Assuming no willful default, they show that enforcing the debt contract and seizing land when the weather conditions are adverse generate inefficiencies. These inefficiencies arise due to loss of production in the next period as the defaulting farmer no longer has the land and is unable to cultivate.

Theories of debt overhang and risk shifting (see Jensen and Meckling, 1976, Myers, 1977) also view debt relief favorably. Poverty trap theories (see Banerjee and Newman, 1993, Banerjee, 2000,

[6] 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%.

[7] 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

Mookherjee and Ray, 2003) claim that high indebtedness may not leave enough money in the hands of the households to invest in physical and human capital. Thus such households may be stuck in a low productivity equilibrium. A debt waiver will be able to pull such households out of the poverty trap and enable them to make productive investments. Kroszner (1999) presents empirical evidence highlighting the overall beneficial impact of a debt waiver. He shows that when the U.S. Government granted a large scale debt relief by making the gold indexation clauses in debt contracts unenforceable, prices of both equity and debt rose.

Our first hypothesis therefore deals with the ex-post behavior of the distressed borrowers versus non-distressed borrowers:

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

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. While Bolton and Rosenthal (2002) do not consider the costs associated with strategic default, empirically these costs may be significant. In their study of debt moratoria in the U.S. following the Great Depression, Rucker and Alston (1987) find evidence of moral hazard among borrowers. These arguments, which are more likely to apply to borrowers that are under distress, lead to our second hypothesis:

HYPOTHESIS 2: *A debt waiver program does not improve the loan performance of non-distressed borrowers.*

5. Data and Proxies

5.1. Bank Loan Data

We use *unique* loan account level information from a public sector bank in India. We hand-collected 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. The details regarding the names of districts and the location of the branches are provided in the Appendix. According to the latest Census, the three states together account for nearly one-sixth of India's population. The loan account data starts in October 2005 and ends in May 2012.

We obtain data on approximately 39,000 loans availed by more than 19,000 agricultural borrowers. 29,076 loans were lent to waiver beneficiaries and 9,914 loans were lent to non-beneficiaries. We have information on all waiver beneficiaries in the 14 branches that we cover. Among borrowers that have defaulted on their loan as of 28^{th} February 2008 but missed the waiver because they had not defaulted as of 31^{st} December 2007, we randomly select the sample of non-beneficiaries using their customer identification number.

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 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 etc. All the loans analyzed are crop loans with a one year maturity.

In our tests, 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 mentioned above, all the agricultural 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 in default.

5.2. Rainfall Data

Rainfall in a area covered by a bank branch is a variable central to our strategy for identifying distressed and non-distressed borrowers. We first identify the exact geographic location of a branch and collect data relating 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 and adverse weather variables, we follow the Percentage of Normal (PN) method as in Burgess et al. (2011). Here, we compare the actual (measured) rainfall in a particular area with its long-term average (LTA). 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.[8] Thus, the drought variable in branch k and year t takes a value one if

$$\text{Drought}_{kt} = 1 \iff r_{kt} \leq 0.8 \times \bar{r}_k, \tag{1}$$

where r_{kt} is the total kharif rainfall in branch k in year t and $\bar{r}_k$ is the long term average precipitation level:

$$\bar{r}_k = \frac{1}{N} \sum_{i=1}^{i=N} r_{k,2005-i}$$

Second, we also construct a continuous measure of rainfall deficiency, by using a standardized Kharif rainfall measure as follows:

$$\tilde{r}_{kt} = \frac{r_{kt} - \bar{r}_k}{\sigma(r_k)}, \tag{2}$$

where $\sigma(r_k)$ is the standard deviation of the kharif rainfall measure.

[8]Results are similar with an alternate drought definition of 75% of normal precipitation.

5.3. Descriptive Statistics

Table 1 reports the summary statistics for average loan size, the number of loans availed by a borrower and the probability of default. In Panel A, we provide information about the full sample and in columns B and C, we provide summary information about waiver beneficiaries and non beneficiaries. Borrowers have 1.82 loans, on an average, in the pre-waiver period. The number falls to nearly 0.7 in the post waiver period. As expected, the proportion of default is very high in the pre-waiver period. Note that the waiver was awarded to defaulting borrowers. The default rate is higher among waiver beneficiaries (0.86) when compared to non-beneficences (0.58). Note that default rates are measured loan wise and not borrower wise. This explains why waiver beneficiaries do not have 100% default rate in the pre-waiver period although the waiver was extended to defaulters only. As well, as discussed in Section 3.2, not all defaulters obtain waiver. This explains high default rate among non beneficiaries as well. The loan size of beneficiaries and non beneficiaries is similar.

[Table 1 here]

6. Proxies for agricultural distress

Distinguishing between distressed and non-distressed borrowers is key to our empirical analysis. Our setting allows us to distinguish between distressed and non-distressed borrowers ex-ante. As stated in the Introduction, we use local variation in exogenous weather shocks to distinguish between distressed and non-distressed borrowers. The fortunes of Indian farmers are heavily dependent on weather (Cole et al. (2013)). Burgess et al. (2011) show that adverse weather causes significant and *persistent* distress among Indian farmers because the infrastructure for irrigation in India is minimal. Motivated by this finding, they use deficient rain precipitation as a measure of agricultural distress. Based on the above premise, we identify borrowers who suffer from drought before the waiver. We measure drought at the mandal level; a mandal represents a geographical unit smaller than a district. If a mandal faced drought in any one of the two agricultural seasons before the waiver (i.e. 2006-07 and 2007-08), then all borrowers who borrow from bank branches located in such a mandal are deemed to be affected by drought. Distressed farmers are those who suffer from adverse weather before the waiver.

6.1. Association between adverse weather and agricultural distress

Although the association between adverse weather and agricultural distress is well established in emerging economies (Deaton et al. (2016)), it is crucial to examine this association in our sample using our measure of distress. To examine this association, we estimate the following regression:

$$Default_{ikt} = \beta_0 + \beta_t * \beta_k + \beta_1 \times AdverseWeather_{kt} + \varepsilon_{ikt} \tag{3}$$

Each observation represents a loan borrowed during year t by a farmer i located in mandal/branch k. The dependent variable is a dummy that takes the value of 1 if the the loan is in default and 0 otherwise. $\beta_t * \beta_k$ denote fixed effects for each pair of (branch k, year t); these fixed effects enable us to absorb unobserved determinants of the correlation between adverse weather and the likelihood of default for each branch in each year. The main independent variable is our measure of adverse weather.

The debt waiver may alter borrower incentives to repay their debt on time. To reduce the impact of such confounding factors on this correlation, we undertake these tests by focussing exclusively on the pre-waiver period. The standard errors are clustered at the (branch, year) level, to minimize the effect of correlated rainfall patterns across the districts and states.

The results are reported in Table 2, where we report the estimates of coefficient β_1. In column 1, the independent variable is the standardized measure of rainfall during the Kharif season. A one standard deviation decrease in kharif rainfall (about 200 mm) associates with an increase in the probability of default by 69.7%. In column 2, we use the amount of loan as a control variable, which as expected loads positively on the probability of default. However, the effect of the rainfall deficiency remains at similar levels. To ensure that the choice of rainfall months (June through October for rains in the Kharif season) is not biasing the results in our direction, we rerun the test using normalized *yearly* rainfall. As can be seen from columns 1-3, an increase in rainfall correlates with a significant reduction in the probability of default. In column 4, we use the dummy variable drought, which takes the value of 1 if the rainfall deficiency is more than 20% and 0 otherwise. Here again, we find that default is positively associated with drought. These results establish a strong positive association between adverse weather/ drought and the likelihood of default.

[Table 2 here]

Taking a cue from these regressions, we introduce two measures of cumulative borrower distress. These measures capture the fact that deficient rainfall in consecutive years leads to significantly higher distress than deficient and abundant rainfalls in alternative years. The first measure is:

$$Cumulative\ Rainfall\ Deficiency_{kt} = \sum_{s=0}^{t} \frac{r_{ks} - \bar{r}_k}{\sigma(r_k)} \tag{4}$$

where r_{ks} is the kharif rainfall mandal k in period $s \leq t$. The term inside the summation sign is simply the standardized rainfall measure used in regression 3 above. By not using the absolute value of the deviation from the long-term mean, we capture the fact that deficient rainfall in consecutive years leads to significantly higher distress than deficient and abundant rainfalls in alternative years. Similarly, the second measure counts the number of drought seasons during the pre-waiver period:

$$Cumulative\ Drought_{kt} = \sum_{s=0}^{t} \mathbf{1}(Drought_{ks} = 1) \tag{5}$$

In unreported regressions, we run regression 3 using these cumulative measures of distress. We find that they load positively and statistically significantly at 1% level on the probability of default during the pre-waiver period.

7. Empirical Strategy and Results

7.1. 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. For instance, Kanz (2015) and Giné and Kanz (2016) use variation in the intensity of treatment, i.e. percentage of borrowers that receive the waiver, at the district level to study the effects of the waiver. However, at the district level, the percentage of good versus bad quality borrowers affects (i) the intensity of treatment, i.e. percentage of borrowers receiving a waiver in the district; as well as (ii) the effect of the waiver.

7.2. Regression Discontinuity Design

To overcome the above challenges to identification, we employ a regression discontinuity (RD) analysis that exploits two unique features of the program:

1. As argued in section 3.2, the debt waiver scheme was an unanticipated event.

2. Borrowers had no opportunity to strategically manipulate into treatment. Though the waiver was announced on 29^{th} February 2008, loan status—default or no default—as on 31^{st} December 2007 was used to decide whether a borrower qualified for the loan waiver or not. To understand this clearly, consider a borrower that borrowed a crop loan on 10^{th} January 2007. Because all crop loans have a maturity of one year, this loan would be due on 9^{th} January 2008. So this borrower cannot qualify for the loan waiver even if he/she had defaulted on this loan. In contrast, consider two borrowers that borrowed a crop loan each on 10^{th} December 2006. Both these loans would be due on 9^{th} December 2007. Suppose one of these borrowers defaulted on his/her loan but the other borrower did not. The former borrower is eligible for the waiver while the latter borrower is not. Crucially, because the waiver was announced on 29^{th} February 2008 and neither borrower could have anticipated the scheme, default (full repayment) by the first (second) borrower cannot result from (no) strategic manipulation by the first (second) borrower.

In the RD design, we restrict attention to a subset of borrowers who defaulted on their existing loans during the period of December 2007 - January 2008. Thus the empirical strategy exploits the unique feature that borrowers had to be in default on their outstanding loan *as of 31^{st} 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 then creates a sharp discontinuity in the treatment status. The narrow focus of our classification scheme 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 this setup is the date on which the outstanding loan of the borrower was in default. We re-scale this variable so that the selection variable equals the number of days before or after the cut-off date (31^{st} December 2007) when the loan becomes delinquent; thus, we set the exogenous cut-off as $\bar{s} = 0$. Using the above example, consider the farmer who obtained an agricultural loan on 10^{th} December 2006. The loan is in default if it is not repaid by 9^{th} December 2007. For this loan, $s_i = -21$. Thus, loans that became delinquent before 31^{st} December 2007 (the key cut-off date for the waiver eligibility) will have a negative value for the selection variable. In contrast, those loans that defaulted in January 2008 will have a positive value. This characterization yields a simple rule for the discontinuity analysis:

$$t_i = t(s_i) = \begin{cases} 1 & \text{if } s_i \leq 0 \\ 0 & \text{if } s_i > 0 \end{cases} \tag{6}$$

It is easy to see that the treatment variable correlates perfectly with the waiver beneficiary status.

Before we proceed to estimate the local average treatment effect (LATE), it is important to ensure that the selection variable s_i has a positive density in the neighborhood of the cut-off point $\bar{s}$. This rules out the possibility of self-selection bias and potential manipulation. As argued above, the concern that beneficiaries may have manipulated into the program is significantly mitigated by the features of the program as well as the announcement of the program being unanticipated.

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

$$\tau_{RD} = \lim_{s \downarrow \bar{s}} \mathbf{E}(Y_i | S_i = s) - \lim_{s \uparrow \bar{s}} \mathbf{E}(Y_i | S_i = s) \tag{7}$$

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 using the following specification:

$$y_i = \gamma_0 + \gamma_1 t_i + \gamma_2 f(s_i) + \gamma_3 t_i \times f(s_i) + \beta_k * \beta_t + \Gamma \mathbf{X_i} + \epsilon_i \tag{8}$$

where the outcome variable of interest, y_i is the probability of default. t_i is the treatment dummy defined in (6) and $f(s_i)$ is a polynomial function of the forcing variable. $\beta_k * \beta_t$ denote fixed effects for each pair of (branch, year); these fixed effects enable us to absorb unobserved determinants of the correlation between the waiver and the likelihood of default for each branch in each year. Thus, we estimate the RD design 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 loan performance and the probability of default.

The main coefficient of interest is γ_1, which captures the LATE as defined in (7). We estimate the regression on a narrow bandwidth of length $h \in [-30, 30]$ around the cut-off point. We compute the standard errors by clustering them at the (branch, year) level.

7.3. Graphical evidence using the RD design

We first present a (non-parametric) graphical analysis of LATE. Figure 1 plots the distribution of ex-post performance on a bandwidth of 30 days around the cut-off date. The figure plots *residuals* from a regression of the binary default variable on the set of controls as follows:

$$\text{Default}_{ikt} = \beta_0 + \beta_k * \beta_t + \beta_1 \ln(Loan)_{ikt} + Weather_{kt} + \epsilon_{ikt} \tag{9}$$

The figure plots these residuals against the forcing variable on the X-axis. Each dot represents the average value of the residual in bins of 1 day, while the solid line represents the fitted values of a linear polynomial of the forcing variable in the intervals $-30 \leq s_i \leq 0$ and $0 \leq s_i \leq 30$. The 95% confidence intervals for these fitted lines are also plotted using standard errors computed by clustering at the (branch, year) level.

[Figure 1 here]

Panel A demonstrates that there is an increase of about 6.80% ($p\!-\!value = 0.006$) in the probability of default for borrowers just below the cut-off (waiver beneficiaries) relative to the borrowers just above the cut-off (non-beneficiaries). The difference is statistically significant at 1% percent level and economically large. Next, we differentiate between distressed borrowers and non-distressed borrowers. We use the cumulative distress measure during the pre-program period, as defined in equation (5). Specifically, we categorize borrowers as distressed (non-distressed) if the deficiency measure is non-positive (positive).

$$\text{Distress}_i = 1 \iff \sum_{s=0}^{t} \mathbf{1}_{(\text{CumulativeDrought}_{ks}=1)} \geq 1 \tag{10}$$

Thus, farmers are categorized as distressed if they faced at least one drought season during the pre-program period. Panels B and C present the RD design separately for the distressed and non-distressed groups. For the distressed borrowers, the probability of default for the treated group is *lower* by 16% ($p - value = 0.028$) when compared to the control group. In contrast for the non-distressed borrowers, the probability of default for the treated group is higher by 10.1% ($p - value = 0.003$) when compared to the control group.

To summarize, distressed beneficiaries perform better on their loans following the loan waiver when compared to distressed non-beneficiaries. However, the effect is exactly opposite for the non-distressed borrowers.

7.4. Regression results using the RD design

We formally test for the existence of discontinuity in ex-post performance around the cut-off date of 31^{st} December 2007, which is the key cut-off date for determining eligibility into the debt waiver program. Table 3 reports the estimates of the regression equation 8 using a quadratic polynomial:

$$f(s_i) = c_1(s_i - \bar{s}) + c_2(s_i - \bar{s})^2$$

The estimation is done with a bandwidth of $h = 30$ days around the cut-off date. The 30-day bandwidth sample has 4148 observations, with 3697 falling in the treatment group and 451 in the control group. Panel A presents the results by combining both distressed and non-distressed borrowers. The first column reports the simplest specification where we restrict the polynomial order to one (by setting $c_2 = 0$) and require the linear function $f(s)$ to have the same slope on either side of the cut-off by setting $\gamma_3 = 0$. In the remaining specifications, we allow for different slopes on either sides of the cut-off by not forcing $\gamma_3 = 0$. In models (2)-(5), we include loan size and weather in the current period as covariates. Columns (3) and (5) expand the linear model to include second-order factors of the forcing polynomial. All the tests include fixed effects for each (branch, year) pair $\beta_k \times \beta_t$.

The coefficient of the treatment variable γ_1 is statistically significant at 5% level or lower in all the specifications. The economic effect ranges between 14% - 19%. Thus, the probability of default on loans taken after the waiver is higher by about 19% for waiver recipients relative to the borrowers who are not eligible to the waiver. To place this number in context, it is crucial to note that the mean default probability is about 58% during the post-program period as seen in table 1. It is clear that this increase in default probability is economically large.

Panels B and C perform the same analysis separately for the distressed and non-distressed sub-samples. As explained before, borrower level distress is an indicator variable that equals one if the farmer experienced at least one drought episode during the pre-program period. According to this classification rule, there are 2869 distressed borrowers, while 1279 fall in the non-distressed category. The point estimates are in line with those presented in the graphical analysis. For example, in the sub-sample of distressed borrowers, the probability of default in the post-program period is lower by 16% - 22% for the treated group relative to distressed borrowers in the control group. In contrast, the effect is quite opposite for the non-distressed group of borrowers. The probability of default is about 11%-29% higher for the non-distressed waiver beneficiaries when compared to the non-distressed non-beneficiaries.

[Table 3 here]

7.5. Robustness and validity

7.5.1. Test of discontinuity in density around cut-off

The validity of the RD design rests on the assumption that borrowers are randomly assigned to treatment, so that LATE correctly estimates the causal effect of the program. In other words, there is no bunching in any direction around the cut-off point. As we have argued before, given the unanticipated nature of the waiver announcement and the choice of cut-off dates, concerns about manipulation are not significant in our setup.

Manipulating eligibility would require the borrowers to be privy to the key cut-off point (31^{st} December 2007). We test this assumption of no manipulation using the procedure proposed by McCrary (2008). This method checks for the discontinuity of the forcing variable around the cut-off point. Figure 2 plots the density of forcing variable $(s_i - \bar{s})$ around the cut-off value 0 using a bandwidth of $h = 15$ days. Interestingly, the density is actually higher at the cut-off point for $s_i > \bar{s}$, implying, if at all possible, that borrowers manipulate out of the waiver program. This is quite unlikely. Taken together, figure 2 and the discontinuity analysis presented in figure 1 and table 3 confirms our prior of no manipulation *into* treatment.

[Figure 2 here]

7.5.2. Robustness to alternative bandwidths

In this section, we verify the robustness of our main results presented in table 3 to alternate bandwidth specifications. In the main set of tables we used a bandwidth of $h = 30$ days. Table 4 report the results for alternate bandwidth choices of $h = \{10, 15, 20, 25\}$ days. Each column in table 4 reports the point estimate of the treatment variable $t_i = \mathbf{1}_{s_i \leq \bar{s}}$ using the full regression specification described in equation (8). We use a quadratic forcing function (order = 2), the full set of covariates, and the full set of fixed effects. The coefficients γ_1 of the treatment variable remains quite stable and are comparable to the results obtained in the main specification.

[Table 4 here]

7.5.3. Placebo tests

Next, we undertake placebo tests using cutoffs other than the key cut-off date of 31^{st} December 2007. If our RD specification correctly identifies the causal effect of the debt relief program on the ex-post performance of borrowers, then we should not observe such effects for other arbitrary cut-off dates. We perform this falsification test by setting cut-off dates as 30^{th} November 2007, 31^{st} January 2008 and 28^{th} February 2008. While performing these falsification tests, we only consider the last loan originated before the waiver that became delinquent at or before these cut-off dates.

To understand why these arbitrary cut-off dates do not correctly identify the causal effect of waiver program, consider 31^{st} January 2008 as the cut-off. For a bandwidth of $h = 30$ days, this cut-off identifies the loans that defaulted during the months of January and February 2008. These loans miss the key cut-off date (31^{st} December 2007) and hence are not eligible for the waiver. The same logic holds for the cut-off date of 28^{th} February 2008.

The first placebo choice is crucial to establishing our claim that the cut-off date of 31^{st} December 2007 divides borrowers *perfectly* into beneficiary and non-beneficiary groups. When we set the cut-off date as 30^{th} November 2007, loans on either side of the cut-off point potentially qualify for the waiver program. Since the amount of the loan waiver equals the loan amount for the waiver beneficiaries and zero for the non-beneficiaries, we plot the distribution of the log of the amount of waiver in figure 3. In the top and bottom panels respectively, we use 30^{th} November 2007 and 31^{st} December 2007 as the cut-off points respectively and employ a bandwidth of $h = 20$ days. In the top panel, where the cut-off point is 30^{th} November 2007, there is no discontinuity in the waiver amount on either side of the cut-off. Thus, there is no discontinuity in the distribution of waiver beneficiaries when 30^{th} November 2007 is used as the cut-off point. In stark contrast, in the bottom panel where the cut-off point is 31^{st} December 2007, we notice that there are no observations to the right of the cut-off. This is because none of the borrowers that defaulted after 31^{st} December 2007 were eligible for the waiver. Since borrowers that defaulted before 31^{st} December 2007 were eligible for the waiver, the discontinuity provided by the 31^{st} December 2007 cut-off point is sharp. Moreover, this cut-off point separates borrowers perfectly into beneficiary and non-beneficiary groups.

[Figure 3 here]

Figure 3 examines the discontinuity in probability of default for the various cut-off points. We notice that none of the cut-off dates we choose —30^{th} November 2007, 31^{st} January 2008 and 28^{th} February 2008—exhibits a discontinuity in the probability of default. Correspondingly, in table 5, we find that the coefficient γ_1 is not significantly different from zero in any of the specifications that we run. Thus, we conclude that the causal effects for distressed and non-distressed borrowers that we find in table 3 are not obtained using any of these arbitrary cutoffs. These results provide further support to our findings in table 3.

[Table 5 here]

7.5.4. Discontinuity Estimates based on Rainfall Measure

So far, we have presented results of tests examining the discontinuity in ex-post loan performance when we use drought as a measure of distress. In this section, we exploit the crude rainfall measure instead. The definition of personalized rainfall based distress measure is given in equation (4). It is important to note that the drought based distress measure is a counting variable while the standardized rainfall measure is a continuous variable. This creates a problem in mapping the distressed and non-distressed samples based on the two measures. We tackle this problem in the following way. Observe first that the definition of drought is given as

$$\text{Drought}_{kt} = 1 \iff r_{kt} \leq 0.8\bar{r}_k$$

Using this as the starting point, we use the long run mean $\bar{r}_k$ and standard deviation σ_k to compute the bound b such that

$$\frac{r_{kt} - \bar{r}_k}{\sigma_k} \leq b \implies r_{kt} \leq 0.8\bar{r}_k$$

In other words, we look for a bound b such that the distressed samples using both definitions yield a high degree of match. Thus, the rainfall based distress measure is given by

$$\text{Distress}_i = 1 \iff \frac{1}{N}\sum_{s=0}^{t}\frac{r_{ks} - \bar{r}_k}{\sigma(r_k)} < b$$

We find that using $b = -1$ creates a good match between the two samples. Based on the rainfall measure, we categorize borrowers as distressed if the pre-program rainfall is one standard deviation below the mean. Table 6 replicates the regressions described in the previous section using rainfall based distress measure.

[Table 6 here]

Using the rainfall based measure shrinks the distressed sample from 2869 to 2592. However, the discontinuity estimates obtained for the two distress measures are remarkably similar and confirms the validity of the design. In the next subsection, we perform a battery of robustness check to ensure that this result is not driven by bandwidth choices and correctly captures the effect of the waiver program on the ex-post incentives of the borrowers.

7.6. Differences in baseline characteristics?

The RD estimates presented above show that distressed beneficiaries perform better when compared to distressed non-beneficiaries, while the effect is reversed for the non-distressed borrowers. The RD estimates are robust to different measures of distress and alternate bandwidth choices. We also conduct a series of falsification tests to ensure that the local treatment effect around the cut-off point truly captures the effect of the waiver program on ex-post loan performance. However, there is a possibility that the post-program results are driven by some borrower-level unobservables and not by the waiver program itself. Specifically, could our results stem from (i) distressed beneficiaries having higher productivity when compared to the distressed non-beneficiaries? and (ii) non-distressed beneficiaries having lower productivity when compared to the non-distressed non-beneficiaries? Note that for some borrower-level unobservables—and not by the waiver program itself—to explain our results, both the above conditions should be met. Thus, the RD design somehow partitions borrowers according to their unobservable productivity types systematically based on whether they are distressed or not. In this section, we examine such concerns.

Assuming that unobserved borrower quality does not change significantly over time, we should expect similar patterns in loan performance for the treatment and control groups during the pre-program period. Thus, we should expect distressed beneficiaries to perform better relative to the non-beneficiaries if they are more productive. In contrast, the non-distressed beneficiaries should exhibit the opposite pattern if they are less productive. We perform this test in table 7. The regression specification is the same as in equation ((8)) , except that we run this test on the loans originated during the pre-program period.

These tests also enable to check an important identifying assumption underlying RD designs. For instance, in their description of the appropriate methodologies to use for RD designs, Lee and Lemieux (2010) mention: "If variation in the treatment near the threshold is approximately randomized, then it follows that all "baseline characteristics" – all those variables determined prior to the realization of the assignment variable – should have the same distribution just above and just below the cutoff."

[Table 7 here]

Columns (1) through (3) report the results for distressed borrowers, while columns (4) through (6) presents the results for the non-distressed borrowers. For each category of borrower, the first two columns report the results using drought to proxy distress while the third column reports the results using rainfall to proxy distress. We find no discontinuity around the cut-off point for either distressed or non-distressed borrowers. Taken together, we find no support that the post-period discontinuity is driven by unobserved borrower heterogeneity. This also provides strong support for an important identifying assumption underlying RD designs, i.e. there is no discontinuity in baseline characteristics.

7.7. Possible loan officer effects

We examine the effect of debt we were on distressed and non-distressed borrowers. So, unlike Agarwal et al. (2016) who examine the effects of debt relief on an existing loan, we have to compare the performance of *new* loans issued after the waiver with the performance of loans before the waiver. Therefore, a potential concern may be that loan officers' criteria for selecting borrowers may systematically influence our results. For example, loan officers may apply relatively stringent criteria for providing loans to waiver beneficiaries than to non-beneficiaries.

However, such officer fixed effects are unlikely to explain our results. First, for such effects to explain our results, the loan officer must simultaneously apply relatively stringent criteria for providing loans to distressed borrowers and relatively lenient criteria for providing loans to non-distressed borrowers.

However, none of our placebo tests show any discontinuity either among distressed or non-distressed borrowers. Moreover, any time-invariant loan officer characteristics are controlled for by our fixed effects.

Therefore, fixed effects pertaining to the criteria a loan officer uses for selecting borrowers can explain our results only if *all the following criteria are simultaneously satisfied.* First, changes in the loan officer in a branch systematically coincide with the waiver in a large number of branches. Second, the loan officer changes are such that the new loan officer simultaneously applies (i) relatively stringent criteria for providing loans to distressed waiver beneficiaries when compared to comparable distressed waiver non-beneficiaries; and (ii) relatively lenient criteria for providing loans to non-distressed waiver beneficiaries when compared to comparable non-distressed waiver non-beneficiaries. All these criteria being simultaneously satisfied is quite unlikely.

Second, all our tests include fixed effects for each (branch, year) pair. Therefore, our tests exploit variation in waiver status within each (branch, year) pair. Any systematic differences in the criteria used by the old or new loan officer for loan origination gets offset as a result. So, our results cannot be rationalized by unobserved differences in loan officers' criteria for selecting borrowers.

In order to test directly for any selection bias after the program that stems from loan officers' selection criteria, we construct two variables that capture two dimensions of credit market access in our sample. The first variable, Rationing_{ikt}, is defined as:

$$\text{Rationing}_{ikt} = \frac{\ell_{ikt}}{\frac{1}{N}\sum_{s<31Dec2007} \ell_{iks}} \tag{11}$$

The numerator is total loan size ℓ_{ikt} given to borrower i, in branch k at time t after the waiver. The denominator, which is the average loan size for the same borrower before the waiver, helps to normalize the loan amount for each borrower. The second variable captures the waiting time between repayment of one loan and the origination of the subsequent loan. This variable is defined as:

$$\Delta\text{wait}_{ikt} = \omega_{ikt} - \frac{1}{N}\sum_{s<31Dec2007} \omega_{iks} \tag{12}$$

where $\omega_{iks} = (\text{Orignation Date}_{ikt} - \text{Repayment Date}_{ik,t-1})$ is the wait period for borrower i in branch k between the date when the previous loan originated in period $(t-1)$ was repaid and the origination date of loan in period t. ω_{ikt} corresponds to loans given after the waiver. We normalize this variable as well at the borrower level by subtracting the average wait time for loans before the waiver.

Using these variables, we investigate whether there is any change in loan size and/or wait period after the program between beneficiaries and non-beneficiaries. If there is no rationing ex-post, then we should expect a value of the rationing variable to be around 1, or greater than 1. Similarly, we should expect the value of $\Delta wait$ to be around 0. We plot the means in figure 5. The top panels plot these means for rationing while the bottom panels plot the same for the waiting period. The average value of Rationing_{ikt} remains largely inside the bounds [0.97, 1.10], which indicates that there is no significant difference between the size of loans originated during before and after program. Similarly, the mean value of $\Delta wait$ remains bounded between [-10, + 20], which shows that mean waiting times are largely similar between pre- and post- periods. Moreover, there is no discontinuity in either of the two variables around the cutoff point of $\bar{s} = 0$. Thus, loan officers do not seem to be selectively discriminating after the waiver between the beneficiaries and non-beneficiaries.

[Figure 5 here]

We also examine these differences in the regression below:

$$y_{ikt} = \gamma_0 + \gamma_1 t_i + \gamma_2 f(s_i) + \gamma_3 t_i \times f(s_i) + \Gamma' \mathbf{X_i} + \epsilon_{ikt}$$

where, as before, γ_1 captures the LATE of the waiver. Possible credit rationing after the waiver is proxied by the two variables described above. We control for borrower's performance history by including three indicator variables - (i) whether the previous loan resulted in a default, (ii) was there an adverse weather shock during the previous loan and (iii) the interaction between the two. As in our previous tests, we also include branch×year fixed effects to control for potential demand and supply effects. The results are provided in table 8. We see no differences as seen in the coefficient γ_1 being statistically indistinguishable from 0. They further support our claim that our main results are not driven by potential credit rationing.

[Table 8 here]

7.8. External Validity: Difference-in-difference tests

The narrow focus of the classification scheme used in the RD design enables careful identification by reducing endogeneity concerns caused by unobserved borrower heterogeneity. However, a concern may be whether the results generalize to the full sample of borrowers. To examine this concern, we employ a difference-in-difference test using the full sample. We combine the sub-samples of distressed and non-distressed borrowers and test using a difference-in-difference if distressed borrowers indeed out perform the non-distressed borrowers. We limit our sample to post-waiver loans. We estimate the following regression equation:

$$\begin{aligned} \text{Default}_{ikt} = {} & \beta_0 + \beta_t + \beta_k + t \times \beta_k + \Gamma^{'} X_{kt} + \beta_1 \text{Beneficiary}_i + \beta_2 \text{Distressed}_{ikt} \\ & + \beta_3 \text{Beneficiary}_i \times \text{Distressed}_{ikt} + \varepsilon_{ikt} \end{aligned} \tag{13}$$

The independent variable DISTRESSED$_{ikt}$ is a dummy that takes the value of 1 if a farmer i residing near close proximity of branch k has suffered adverse weather shock previously and 0 otherwise. BENEFICIARY$_i$ is a dummy that takes the value of 1 if the borrower i is eligible for waiver and 0 otherwise. All the other variables are as defined before. The standard errors are clustered at the (branch, year) levels.

The main variable of interest is the interaction between the dummy variables BENEFICIARY$_i$ and DISTRESSED$_{tk}$. The coefficient of this variable measures a difference-in-difference:

$$\begin{aligned} \beta_3 \quad = \quad & (\overline{Y}_{\text{Waiver Beneficiaries}} - \overline{Y}_{\text{Non-beneficiaries}})\big|_{\text{Distress before the waiver}} \\ & - (\overline{Y}_{\text{Waiver Beneficiaries}} - \overline{Y}_{\text{ Non-beneficiaries}})\big|_{\text{No Distress before the waiver}} \end{aligned} \tag{14}$$

Here waiver status provides the first difference. The second difference is provided by status with respect to distress variable. The results are reported in Table 9. Here, we restrict the beneficiary sample to those who default during two months prior to December 31^{st} 2007. The non-beneficiary sample is restricted to those who default between January 1st 2008 and February 29th, 2008.

[Table 9 here]

The results in columns (1) and (2) use cumulative rainfall deficiency to measure distress whereas those in columns (3) and (4) use the drought measure. Using the continuous rainfall measure, we find that distressed waiver beneficiaries are 27.1% to 27.4% less likely to default in the post waiver period than non-distressed waiver beneficiaries in the same period. Using the drought measure, we estimate the difference-in-difference coefficient to be between 5.2% to 5.9%.

These tests also enable us to validate our estimates of the effect of the waiver on distressed and non-distressed beneficiaries separately. Using equation (13) and the results reported in columns (1) and (2), the effect of the waiver on distressed beneficiaries is given by:

$$\beta_1 + \beta_3 = (\overline{Y}_{\text{Waiver Beneficiaries}} - \overline{Y}_{\text{Non-beneficiaries}})\Big|_{\text{Distress before the waiver}} \tag{15}$$

while the effect of the waiver on non-distressed beneficiaries is given by:

$$\beta_1 = (\overline{Y}_{\text{Waiver Beneficiaries}} - \overline{Y}_{\text{Non-beneficiaries}})\Big|_{\text{No Distress before the waiver}} \tag{16}$$

Thus, the waiver improves loan performance of distressed beneficiaries by 23% - 24% and deteriorates that of non-distressed beneficiaries by 3.4% - 4.7%. These estimates are statistically significant at the 1% level and are similar economically to those obtained using the RD design. Thus, our difference-in-difference based results lead to inferences that are similar in direction to the RD based results.

8. Conclusion

We study 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. We use exogenous local weather shocks to distinguish between distressed and non-distressed borrowers. Our empirical 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 has little effect on their loan repayment behavior. Thus, if the distressed and non-distressed beneficiaries of debt relief are not carefully separated, the non-distressed borrowers are likely to impose significant costs on the program. Therefore, the success of a debt relief program crucially depends on the ability of the political executive to target the program towards distressed borrowers.

We have focused on the effects of the debt waiver on loan performance. Future studies may find it useful to carefully examine the effects of the debt waiver on consumption and investment. District level examinations of these outcomes, however, suffer from the endogeneity problem that the unobserved proportion of good versus bad borrowers in a district affects both the treatment effect as well as selection into treatment. To identify the causal effect of the waiver on such outcomes, the control group must comprise of defaulters that missed the waiver for exogenous reasons. As we have done in this study, borrower level information must be used for this purpose. Our data limitations preclude us from examining these effects.

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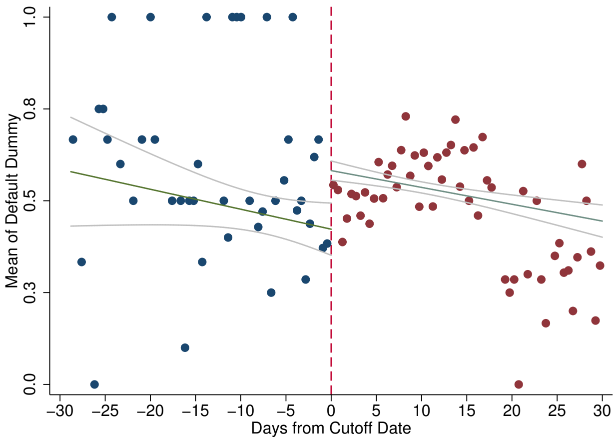
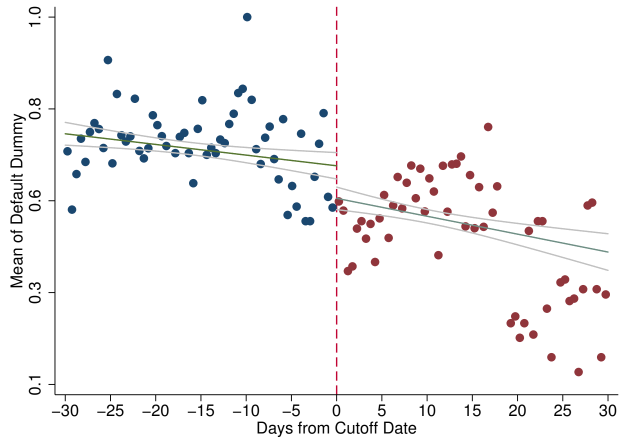
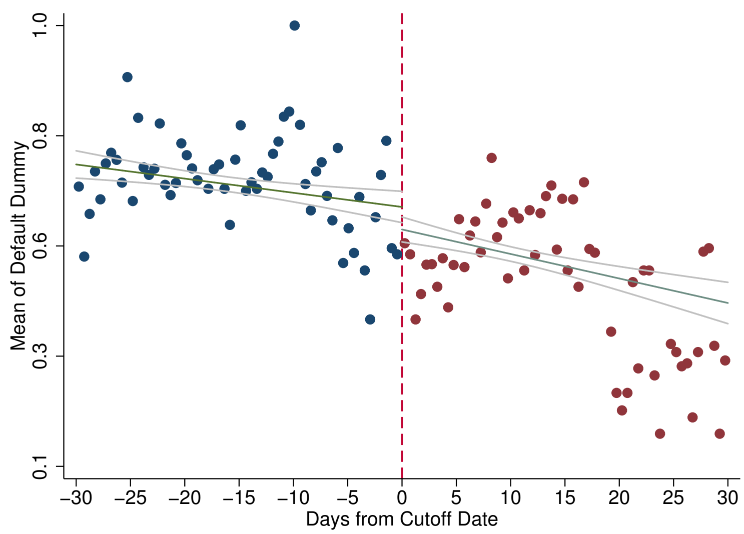
Figure 1: Ex-Post Loan Performance



(a) Distressed Borrowers, BW [-30,30]



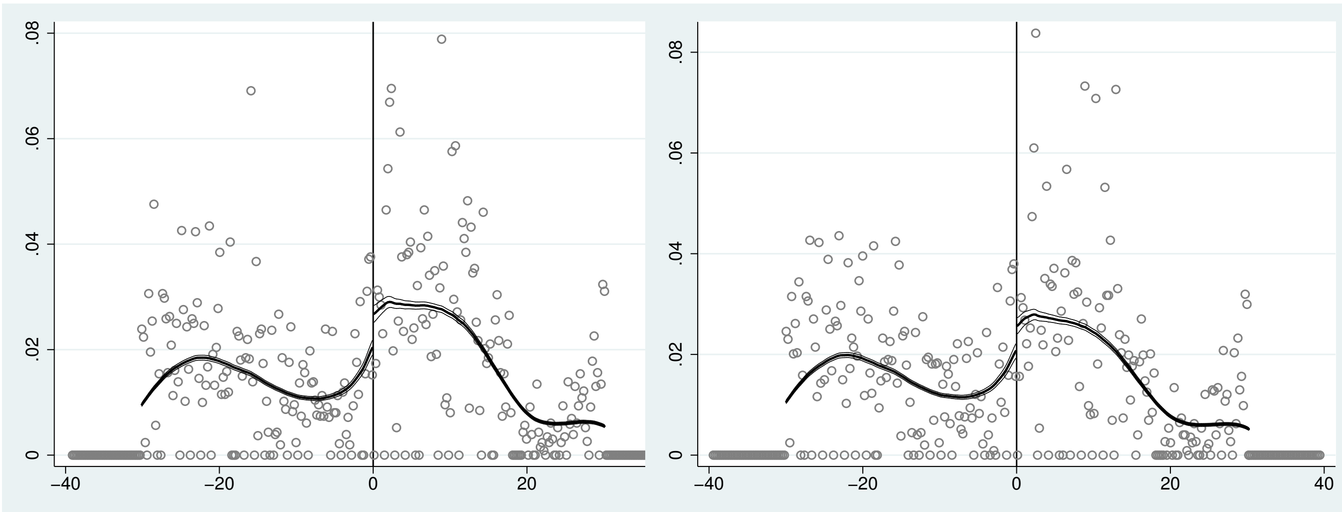
(b) Non-Distressed Borrowers, BW [-30,30]



(c) All Borrowers, BW [-30,30]

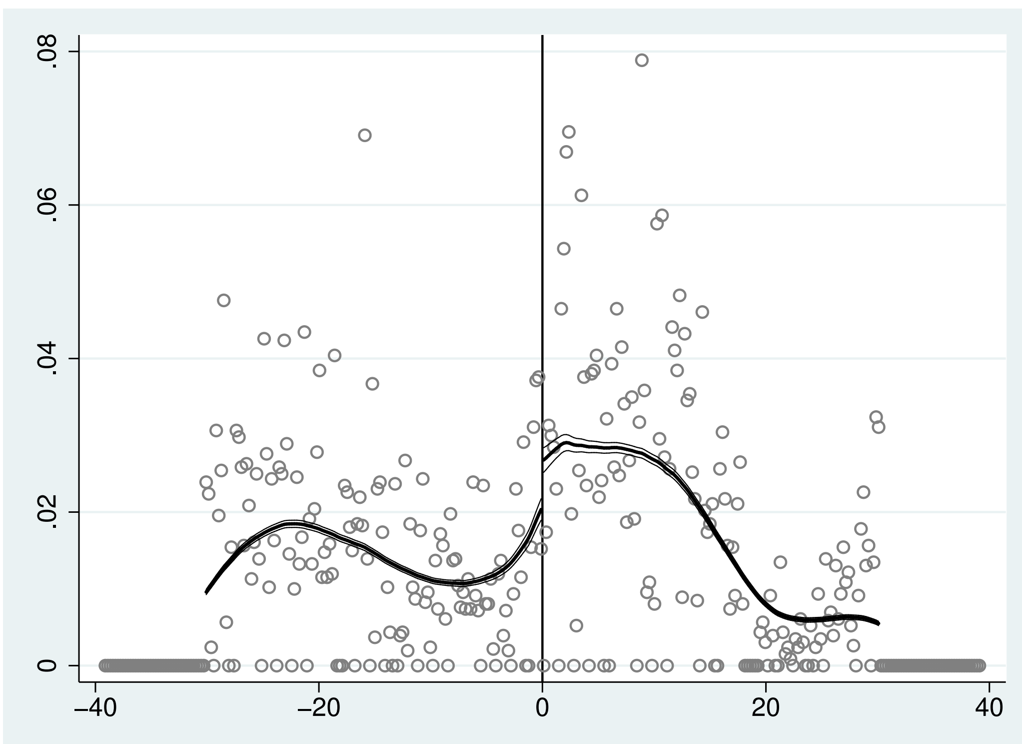
The above figure illustrates the regression discontinuity design by plotting the probability of default during the post-program period around the cutoff point. The x-axis represents the selection variable, measured as the date, relative to the cutoff date of December 31, 2007, when the last loan originated during the pre-program period becomes delinquent. In the y-axis, we plot average the probability of default for the loans originated during the post-program period. Each dot represents the average default probability in bins of 1 day. The solid line represents the fitted value of a linear function of the forcing variable. The bandwidth is [-30,30] days around the cutoff point. In panels (b) and (c), we partition the total sample into two parts based on pre-period distress measure.

Figure 2: Density Around Cutoff Point



(a) Distressed Borrowers

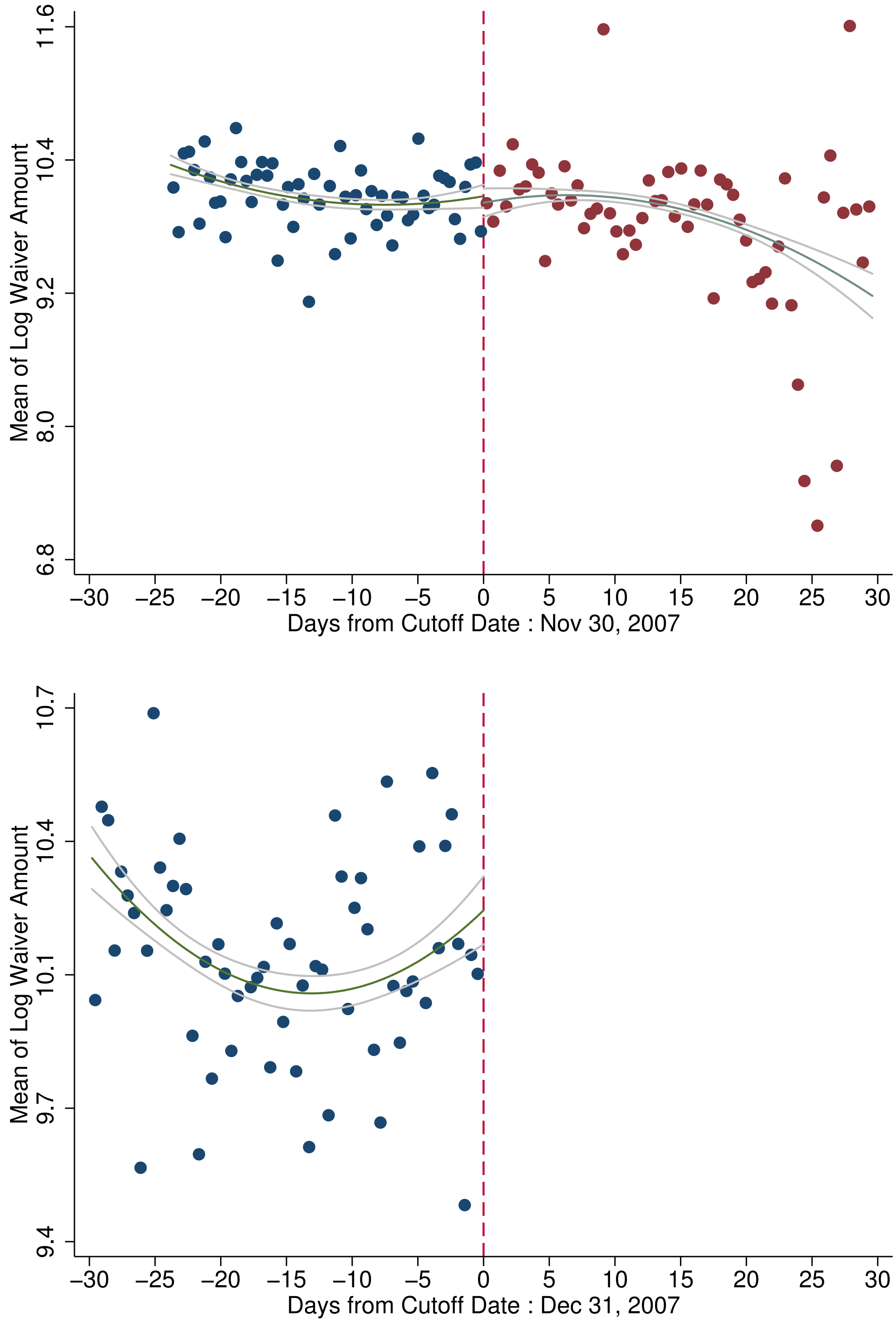
(b) Non-Distressed Borrowers



(c) All Borrowers

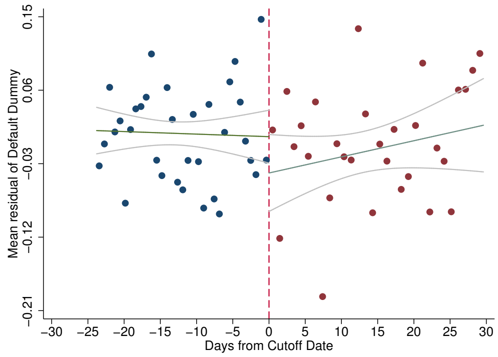
This figure plots the density around the cutoff date of December 31, 2007. The borrowers with negative value of the forcing variable were eligible for the debt waiver program. McCracy (2009) test for manipulation of the forcing variable reject the Null hypothesis of bunching and show that the distribution around the cutoff is smooth.

Figure 3: Distribution of Waiver for alternate cutoff dates

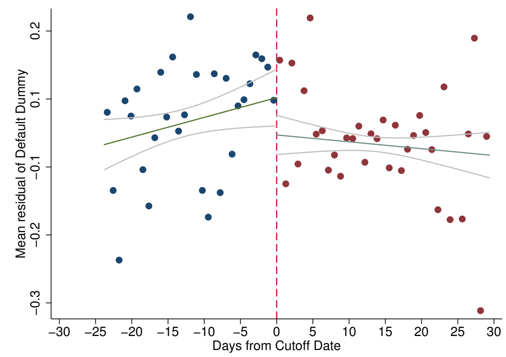


This figure plots the distribution of waiver beneficiaries for two cutoff dates. The first panel assumes a false cutoff date of November 30, 2007, which as can be seen, does not correctly identify the causal effect of waiver. The distribution of beneficiaries around the cutoff point shows no discontinuity. Contrast this with the actual cutoff date of December 31, 2007, which we show in Panel (b). There is no mass on the right side of cutoff which shows the true identification.

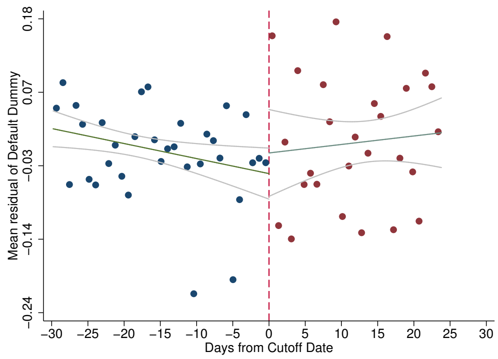
Figure 4: Falsification Tests for Different Cutoff Points



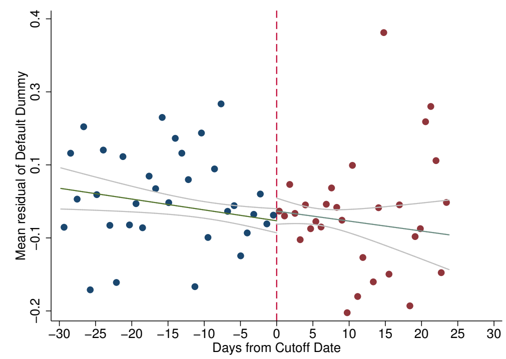
(a) Distressed, Cutoff Date : Nov 30, 2007



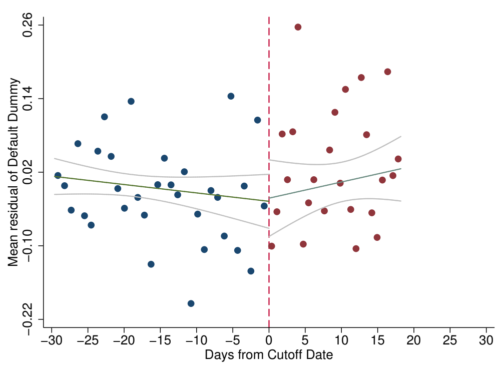
(b) Non-Distressed, Cutoff Date : Nov 30, 2007



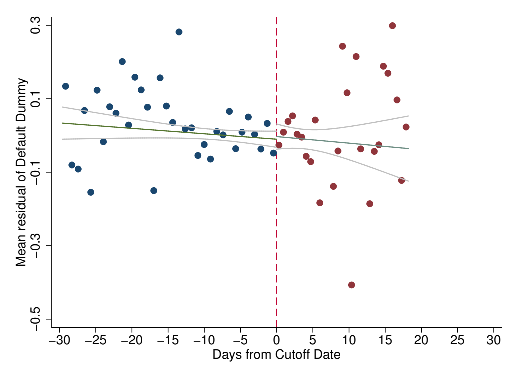
(c) Distressed, Cutoff Date : Jan 31, 2008



(d) Non-Distressed, Cutoff Date : Jan 31,2008



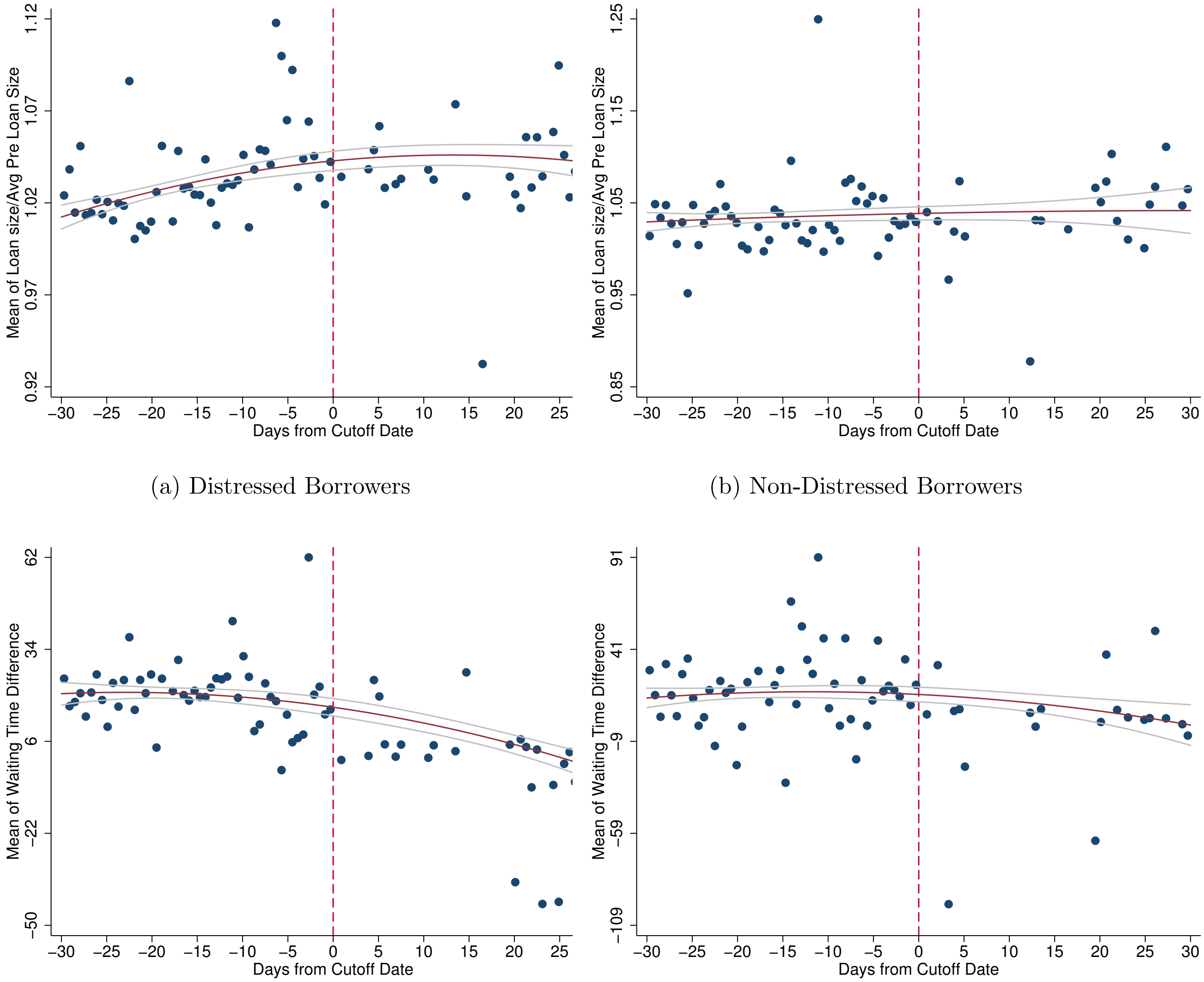
(e) Distressed, Cutoff Date : Feb 28,2008



(f) Non-Distressed, Cutoff Date : Feb 28,2008

This figure plots the post-program probabilities of default for different cutoff dates. We use alternate dates to be the last day of November 2007, January 2008 and February 2008.

Figure 5: Ex-Post Credit Rationing



(a) Distressed Borrowers

(b) Non-Distressed Borrowers

(c) Distressed Borrowers

(d) Non-Distressed Borrowers

This figure plots the means of the two different credit rationing measures introduced in the text. The first measure is the ratio of the loans originated during the post-program period, to the average loan size granted to the same borrower during the pre- period. The second measure is the incremental waiting time during the post-program period. The waiting time is defined as the number of days the borrower waits before originating a new loan since the time the last loan is repaid.

Table 1: Summary Statistics

This table presents the summary statistics for the borrower sample.

	N	mean	sd	p25	p50	p75
Panel A: Full Sample						
Log Loan Size	38990	10.14	0.88	9.70	10.21	10.72
Number of Loans (Pre-Program)	38990	1.82	0.88	1	2	2
Number of Loans (Post-Program)	38990	0.69	1.05	0	0	1
Total Number of Defaults (Pre-Program)	38990	0.86	0.62	0	1	1
Total Number of Defaults (Post Program)	38990	0.37	0.61	0	0	1
Average Default Rate	38990	0.58	0.49	0	1	1
Panel B: Waiver Beneficiaries						
Log Loan Size	29076	10.09	0.88	9.66	10.16	10.66
Number of Loans (Pre-Program)	29076	1.62	0.72	1.00	2.00	2.00
Number of Loans (Post-Program)	29076	0.72	1.05	0.00	0.00	1.00
Total Number of Defaults (Pre-Program)	29076	0.89	0.57	1.00	1.00	1.00
Total Number of Defaults (Post Program)	29076	0.43	0.65	0.00	0.00	1.00
Average Default Rate	29076	0.64	0.48	0.00	1.00	1.00
Panel C: Non-Beneficiaries						
Log Loan Size	9914	10.29	0.87	9.86	10.38	10.87
Number of Loans (Pre-Program)	9914	2.39	1.06	2.00	2.00	3.00
Number of Loans (Post-Program)	9914	0.58	1.02	0.00	0.00	1.00
Total Number of Defaults (Pre-Program)	9914	0.79	0.73	0.00	1.00	1.00
Total Number of Defaults (Post Program)	9914	0.18	0.42	0.00	0.00	0.00
Average Default Rate	9914	0.40	0.49	0.00	0.00	1.00

Table 2: EFFECT OF ADVERSE WEATHER ON DEFAULT

	Dependent Variable: Probability of Default					
	Sample : Pre-Waiver				Sample : Full	
	(1)	(2)	(3)	(4)	(5)	(6)
Standardized Kharif Rainfall	-0.697***	-0.692***			-0.389***	
	(-4.29)	(-4.37)			(-3.22)	
Standardized Yearly Rainfall			-0.666***			
			(-3.27)			
Drought				0.563***		0.563***
				(8.29)		(13.56)
Log Loan Amount		0.071***	0.075***	0.036***	0.054***	0.030***
		(3.69)	(3.26)	(5.33)	(4.46)	(5.70)
Observations	23,723	23,723	23,723	23,723	38,990	38,990
R-squared	0.412	0.426	0.419	0.400	0.393	0.441
Branch × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table tests the association between effect of our rainfall based measures and loan performance as measured using the probability of default during the pre-waiver period (September 2005 - Feb 2008). The dependent variable is a dummy that takes the value of 1 if the loan under consideration defaults and zero otherwise. Additional controls include branch fixed effects, time fixed effects, non-linear time trends, district-wise yearly food-grain production and agricultural loan disbursed. Standardized kharif rainfall is equal to $\frac{Rain_{bt}-\overline{Rain_b}}{\sigma_b}$, where $Rain_{bt}$ equals the actual rainfall in Mandal b in year t while $\overline{Rain_b}$ and σ_b equal the long-term average and the standard deviation of rainfall in the kharif season in Mandal b. Drought is a dummy variable that takes the value of 1 if the actual rainfall in a Mandal during a year is less than 80% of the long term average. The standard errors are clustered at branch-year level and adjusted t-statistics are reported in parentheses below the regression estimates.

Table 3: RD Design : Probability of Default (Drought Measure)

Dependent Variable: Post Program Probability of Default	(1)	(2)	(3)	(4)	(5)
Panel A : All Borrowers					
Treatment = 1	0.192***	0.194***	0.139***	0.138***	0.145**
	(5.40)	(5.39)	(2.62)	(4.62)	(2.17)
Log Loan Amount		0.020**	0.020**	0.015**	0.015**
		(2.55)	(2.58)	(2.14)	(2.13)
Standardized Kharif Rainfall		-0.184***	-0.184***		
		(-7.65)	(-7.63)		
Drought				0.534***	0.534***
				(22.83)	(22.87)
Observations	4,148	4,148	4,148	4,148	4,148
R-squared	0.433	0.451	0.452	0.531	0.531
Panel B : Distressed Borrowers					
Treatment = 1	-0.162*	-0.194***	-0.223**	-0.182***	-0.203**
	(-1.80)	(-3.00)	(-2.35)	(-2.71)	(-2.14)
Log Loan Amount		0.019**	0.019**	0.018**	0.018**
		(2.20)	(2.19)	(2.07)	(2.02)
Standardized Kharif Rainfall		-0.270***	-0.270***		
		(-9.99)	(-10.01)		
Drought				0.472***	0.472***
				(14.60)	(14.62)
Observations	2,869	2,869	2,869	2,869	2,869
R-squared	0.468	0.504	0.504	0.530	0.530
Panel C : Non Distressed Borrowers					
Treatment = 1	0.282***	0.295***	0.223***	0.179***	0.115*
	(5.07)	(5.35)	(3.37)	(3.89)	(1.91)
Log Loan Amount		0.027*	0.028**	0.020*	0.021*
		(1.89)	(1.97)	(1.87)	(1.93)
Standardized Kharif Rainfall		-0.044	-0.042		
		(-1.30)	(-1.26)		
Drought				0.604***	0.603***
				(21.72)	(21.72)
Observations	1,279	1,279	1,279	1,279	1,279
R-squared	0.377	0.380	0.381	0.555	0.555
Forcing Polynomial Order	1	1	2	1	2
Branch × Year FE	Yes	Yes	Yes	Yes	Yes

Notes: The above table shows that the probability of default during the post-program period increases discontinuously at the cutoff date of December 31, 2007. The outcome variable for the regression specifications is DEFAULT variable. The local average treatment effect 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 robust t-statictics are reported in parentheses. Panels B and C estimates the regression discontinuity specification separately for the distressed and the non-distressed borrower groups. Borrowers are categorized as distressed if they experienced at least one drought episode during the pre-program period.

Table 4: RD Robustness

	[-10,10] (1)	[-15,15] (2)	[-20,20] (3)	[-25,25] (4)
	Panel A : Full Sample			
Treatment = 1	0.087***	0.096*	0.113***	0.119***
	(2.20)	(1.86)	(2.76)	(3.15)
Observations	1,010	1,516	2,321	3,223
R-squared	0.567	0.551	0.514	0.516
	Panel B : Distressed Borrowers			
Treatment = 1	-0.227***	-0.231**	-0.252***	-0.226***
	(-3.32)	(-2.51)	(-3.46)	(-3.44)
Observations	551	950	1,548	2,208
R-squared	0.574	0.540	0.504	0.503
	Panel C : Non-Distressed Borrowers			
Treatment = 1 = 1	0.097**	0.105**	0.169***	0.145***
	(1.98)	(1.98)	(3.24)	(2.82)
Observations	459	566	773	1,015
R-squared	0.574	0.578	0.547	0.555
Controls	Yes	Yes	Yes	Yes
Branch × Year FE	Yes	Yes	Yes	Yes

Notes: The above table shows that the probability of default during the post-program period increases discontinuously at the cutoff date of December 31, 2007. The outcome variable for the regression specifications is DEFAULT variable. Different bandwidth is used in each column as a robustness check. The local average treatment effect 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 robust t-statictics are reported in parentheses. Panel A estimates the regression discontinuity specification for the entire sample. Panels B and C estimate the regression discontinuity specification separately for the distressed and the non-distressed borrower groups.

Table 5: Falsification Tests Based on Different Cutoff Dates

Dependent Variable: Post Program Probability of Default			
Cutoff Date	30 Nov 2007	31 Jan 2008	28 Feb 2008
Panel A : All Borrowers			
Treatment = 1	0.010	-0.124	-0.006
	(0.31)	(-0.84)	(-0.04)
Observations	3,795	2,476	1,706
R-squared	0.523	0.533	0.560
Panel B : Distressed Borrowers			
Treatment = 1	0.048	-0.195	0.042
	(0.71)	(-1.48)	(0.19)
Observations	2,574	1,689	1,104
R-squared	0.512	0.530	0.558
Panel C : Non Distressed Borrowers			
Treatment = 1	0.010	0.231	-0.166
	(0.33)	(0.99)	(-0.41)
Observations	1,221	787	602
R-squared	0.553	0.561	0.581
Forcing Polynomial Order	2	2	2
Branch × Year FE	Yes	Yes	Yes

Notes: The above table shows the results of falsification tests. In each column, we consider a false event day as indicated. The outcome variable for the regression specifications is DEFAULT variable. Different bandwidth is used in each column as a robustness check. The local average treatment effect 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 robust t-statictics are reported in parentheses. Panel A estimates the regression discontinuity specification for the entire sample. Panels B and C estimate the regression discontinuity specification separately for the distressed and the non-distressed borrower groups.

Table 6: RD Design : Probability of Default (Rainfall Measure)

Dependent Variable: Post Program Probability of Default	(1)	(2)	(3)	(4)	(5)
Panel A : Distressed Borrowers					
Treatment = 1	-0.163*	-0.194***	-0.219**	-0.182***	-0.200**
	(-1.84)	(-2.92)	(-2.28)	(-2.67)	(-2.09)
Log Loan Amount		0.029***	0.029***	0.027***	0.027***
		(3.38)	(3.34)	(3.10)	(3.02)
Standardized Rainfall		-0.264***	-0.264***		
		(-9.44)	(-9.45)		
Drought				0.475***	0.475***
				(13.45)	(13.47)
Observations	2,592	2,592	2,592	2,592	2,592
R-squared	0.478	0.514	0.514	0.539	0.539
Panel B : Non Distressed Borrowers					
Treatment = 1	0.285***	0.288***	0.220***	0.177***	0.115*
	(5.12)	(5.24)	(3.33)	(3.84)	(1.91)
Log Loan Amount		0.013	0.014	0.006	0.007
		(1.03)	(1.11)	(0.64)	(0.71)
Standardized Rainfall		-0.069**	-0.067**		
		(-2.15)	(-2.12)		
Drought				0.582***	0.582***
				(21.27)	(21.26)
Observations	1,556	1,556	1,556	1,556	1,556
R-squared	0.385	0.388	0.389	0.544	0.545
Forcing Polynomial Order	1	1	2	1	2
Branch × Year FE	Yes	Yes	Yes	Yes	Yes

Notes: The above table shows that the probability of default during the post-program period increases discontinuously at the cutoff date of December 31, 2007. The outcome variable for the regression specifications is DEFAULT variable. The local average treatment effect 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 robust t-statictics are reported in parentheses. Panels A and B estimates the regression discontinuity specification separately for the distressed and the non-distressed borrower groups. Borrower distress is measured by total rainfall deficiency during the pre-program period.

Table 7: Pre-Waiver Performance : RD Analysis

Borrower Category	Distressed			Non-Distressed		
Distress Measure	Drought	Drought	Rainfall	Drought	Drought	Rainfall
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment = 1	0.194	0.087	0.083	0.143	0.128	0.153
	(1.37)	(0.52)	(0.51)	(1.21)	(1.08)	(1.28)
Log Loan Amount	0.032***	0.029***	0.030***	0.022	0.022	0.024
	(3.27)	(3.02)	(2.78)	(1.26)	(1.29)	(1.63)
Standardized Kharif Rainfall		-1.110***	-1.110***		-0.892***	-0.891***
		(-14.47)	(-14.48)		(-8.49)	(-8.56)
Observations	1,416	1,416	1,332	543	543	627
R-squared	0.345	0.424	0.416	0.175	0.183	0.220
Branch×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The above table shows the results of tests that examine if distressed beneficiaries performed better in the pre waiver period. The outcome variable for the regression specifications is DEFAULT variable. Columns (1) through (3) report the results for distressed borrowers, while columns (4) through (6) presents the results for the non-distressed individuals. For each type of borrowers, the first two columns report the results for all the loans originated during the pre-waiver period, while the last two columns reports the results excluding the last loan. The local average treatment effect is captured by the coefficient of the TREATMENT dummy, which assumes a value of one for negative values of Standard errors are clustered at borrower level and robust t-statictics are reported in parentheses.

Table 8: Ex-post Credit Rationing : RD Estimates

Dependent Variable	Credit Volume			Origination Delay Δ		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Distressed Borrowers						
Treatment = 1	-0.150	-0.159	-0.155	1.447	-0.945	-2.377
	(-0.91)	(-1.02)	(-1.01)	(0.36)	(-0.20)	(-0.46)
Default Previous Loan?		-0.087	-0.087		-7.669	-7.850
		(-1.09)	(-1.10)		(-1.09)	(-1.12)
Drought Previous Loan?		-0.209***	-0.198***		-31.225*	-33.126*
		(-2.88)	(-2.75)		(-1.94)	(-1.93)
Default (Previous) × Drought (Previous)		-0.033	-0.044		30.436*	32.374*
		(-0.38)	(-0.49)		(1.85)	(1.82)
Observations	2,869	2,869	2,869	2,270	2,270	2,270
R-squared	0.495	0.502	0.505	0.793	0.735	0.801
Branch × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	No	No	Yes	No	No	Yes
Panel B: Non-Distressed Borrowers						
Treatment = 1	-0.097	-0.082	-0.067	12.798	16.351	16.214
	(-0.59)	(-0.49)	(-0.53)	(0.91)	(1.09)	(1.10)
Default Previous Loan?		-0.096	-0.078		13.594*	14.271*
		(-0.98)	(-1.05)		(1.81)	(1.84)
Drought Previous Loan?		0.019	0.028		-27.044**	-25.997**
		(0.17)	(0.70)		(-2.09)	(-2.01)
Default (Previous) × Drought (Previous)		0.038	0.008		-18.241*	-20.644**
		(0.30)	(0.08)		(-1.92)	(-2.01)
Observations	1,279	1,279	1,279	815	815	815
R-squared	0.421	0.423	0.426	0.585	0.605	0.607
Branch × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	No	No	Yes	No	No	Yes

Notes: The above table shows that the post waiver access to credit is unlikely to vary systematically between waiver beneficiaries and non-beneficiaries. Panels A and B present the results relating to distressed and non-distressed borrowers respectively. The volume of post waiver credit as a proportion of pre waiver credit at the borrower level is the dependent variable in columns 1 to 3. In columns 4 to 6, the dependent variable is the gap, in terms of days, between waiver and the date of disbursement of first post waiver loan. The local average treatment effect 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 robust t-statictics are reported in parentheses.

Table 9: Distress and Ex-Post Performance : DID Estimates

Dependent Variable: Probability of Default	(1)	(2)	(3)	(4)
Waiver Dummy	0.034***	0.047***	0.085***	0.098***
	(3.25)	(4.34)	(6.14)	(7.06)
Pre-Program Distress (Rainfall)	0.019	0.039		
	(0.23)	(0.44)		
Pre-Program Distress (Drought)			0.065***	0.065***
			(4.17)	(4.35)
Waiver × Distress (Rainfall)	-0.274***	-0.271***		
	(-3.07)	(-2.72)		
Waiver × Distress (Drought)			-0.052***	-0.059***
			(-2.62)	(-3.08)
Log Loan Amount		0.030***		0.030***
		(6.31)		(6.28)
Current Rainfall		-0.153***		-0.154***
		(-9.23)		(-9.32)
Observations	15,267	15,267	15,267	15,267
R-squared	0.399	0.413	0.399	0.413
Branch × Year FE	Yes	Yes	Yes	Yes

Notes: This table investigates the relative performance of the distressed and non-distressed borrowers using a difference-in-difference analysis. The first difference comes from the variation in the beneficiary status of the borrowers (waiver vs no-waiver); while the second difference pertains to per-program distress level of the borrower. Distress is measured by the cumulative rainfall deficiency (continuous variable) or the drought episodes (indicator) during the pre-waiver period. The dependent variable is the probability of default during the post period (July 2008 - May 2012). The regression specification is given by

$$\text{Default}_{ikt} = \beta_0 + \beta_k \times \beta_t + \gamma_1 \text{Distress (Pre)} + \gamma_2 \text{Waiver} + \gamma_3 \text{Waiver} \times \text{Distress (Pre)} + \epsilon_{ikt}$$

The DID estimate is captured by the interaction term γ_3. Controls include the branch and time fixed effects, as well as regional time trends. We also include loan size and current rainfall (during the post period). Standard errors are clustered at branch-year level.

A. Appendix A : Location of Bank Branches

Name of the Branch	District	State
Paloncha	Kothagudem	Andhra Pradesh
Bhadrachalam Road	Kothagudem	Andhra Pradesh
Mahabubnagar	Mahabubnagar	Andhra Pradesh
Sattupalli	Khammam	Andhra Pradesh
VM Banjara	Khammam	Andhra Pradesh
Zaheerabad	Medak	Andhra Pradesh
Kohir	Medak	Andhra Pradesh
Medak	Medak	Andhra Pradesh
Peddapally	Karim Nagar	Andhra Pradesh
Sindhanur	Raichur	Karnataka
Gangavathi	Koppal	Karnataka
Parbhani	Parbhani	Maharashtra
Nandhed	Nandhed	Maharashtra
Ramtirth	Nandhed	Maharashtra