WHAT DOES DEBT RELIEF DO FOR DEVELOPMENT?

Evidence from a Large-Scale Policy Experiment

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Abstract

This paper analyzes the effect of a large debt relief program on investment, productivity and the subsequent financial access of beneficiary households. We carry out a survey of 2,897 households affected by the Indian Debt Waiver and Debt Relief Program for Small and Marginal Farmers –one of the largest debt relief programs in history. Using a Regression Discontinuity Design based on the program eligibility criteria, we show that debt relief does not improve the investment or productivity of beneficiary households, but leads to a strong and persistent shift of borrowing away from formal sector lenders. We further document strong effects of debt relief on beliefs about the seniority of debt and the reputational consequences of default. The results resonate with findings from the literature on personal bankruptcy and suggest that bailout programs are of limited use in addressing problems of debt overhang, but have significant behavioral implications.

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1 Introduction

Limited access to formal credit has been widely recognized as an important underlying cause of persistent poverty (see e.g., Townsend 2006). This is especially true in poor and largely agricultural economies, where bank credit serves the dual purpose of enabling productive investment and providing insurance against highly volatile income streams. However, in the absence of sophisticated instruments to manage income risk, such as insurance- and futures contracts, even households with initial access to bank credit often accumulate extreme levels of debt, factually excluding them from institutional credit in the future.

The potentially far-reaching macroeconomic implications of extreme household indebtedness have motivated a range of large-scale debt relief initiatives.[1] Some recent examples include a US$ 2.1 billion bailout program for farmers in Thailand in 2010, and the rescheduling of US$ 5 billion of agricultural household debt in Brazil since 2001.[2] While the benefit of debt relief programs to individual households is substantial, their merit as a tool to promote financial inclusion, investment and productivity remains highly controversial. Building on theories of debt overhang and risk-shifting (Myers 1977), bailout proponents argue that extreme levels of household debt distort investment and production decisions, so that debt relief holds the promise of improving productivity. This view is challenged by critics of large-scale debt relief programs, who argue that debt relief has the potential to generate substantial moral hazard problems, likely to limit the financial access of marginal borrowers in the long run.[3] While both views can appeal to a foundation in economic theory, there is surprisingly little evidence on how indebtedness and debt relief affects access to credit and economic decisions at the household level.

This paper uses a survey of 2,897 benficiaries of the Indian *Debt Relief and Debt Waiver Scheme for Small and Marginal Farmers* to address these open empirical questions. The program, which ranks among the largest debt relief initiatives in history, was enacted in the summer of 2008 and waived more than Rs 650 bn (US$14.4 bn) of overdue agricultural debt issued by commercial and cooperative banks between 1997 and 2007. The volume of the program corresponded to approximately 1.6% of India's GDP and covered more than 36 million households across the country (Government of India 2008).

In order to provide causal estimates of the effect of debt relief on the subsequent economic decisions and financial access of beneficiary households, we employ a Regression Discontinuity Design based

[1]Between 2000 and 2006, average household debt increased six-fold in India. In Mexico, outstanding consumer credit increased by 35% per year, and more than doubled in Brazil over the same time period (see e.g., Feibelman 2009).

[2]Debt relief has also regularly made available in many Latin American countries. Previous to the program studied in this paper, India enacted a US$ 3 bn nationwide debt relief program for agricultural households in 1989. This earlier debt relief program was based on outstanding debt, rather than landholding criteria (Source: USDA, Government of India)

[3]See Karlan and Zinman (2009) for evidence on moral hazard and adverse selection in an emerging credit market. See Jaffee and Russell (1976) and Stiglitz and Weiss (1981) for asymetric information and credit rationing. Karlan and Morduch (2009) and Ghosh, Mookherjee, and Ray (2000) review theories and evidence on credit rationing in developing countries.

on the program eligibility criteria. Specifically, our identification strategy exploits the feature that program eligibility was based on the amount of land hypotheticated as collateral at the time the loan was originated: households that had pledged less than 2 hectares (5 acres) of land qualified for 100% waiver of outstanding debt, while households that had pledged more than 2 hectares qualified for 25% of debt relief, conditional of settling the remaining 75% of outstanding debt. We estimate the causal effect of debt relief by comparing outcomes of beneficiary and non-beneficiary households in the vicinity of the eligibility cut-off established by the program.

Economic theory suggests two channels through which financial distress at the household level may affect investment, productivity and the aggregate economy. First, 'poverty trap' models (Banerjee and Newman 1993, Banerjee 2000, Mookherjee and Ray 2003) argue that household income net of debt service may be insufficient to cover investments in human or physical capital, causing indebted households to remain in a low-productivity equilibrium. Second, theories of debt overhang and risk-shifting (Jensen and Meckling 1976, Myers 1977) emphasize that indebtedness affects both the level and risk-profile of investment. If a household's debt burden is sufficiently high that the proceeds of any profitable investment go largely towards debt service, the household may pass up profitable investment opportunities. Similarly, heavily indebted households may undertake excessively risky investments, since much of the downside-risk is borne by debt holders. Both channels would imply improvements in investment and productivity as a result of debt relief.

There is, however, much concern that such efficiency effects of debt relief may be outweighed by moral hazard problems and behavioral responses arising from the prospect of future bailouts. Opponents of debt relief warn that unconditional bailouts may do lasting damage to the culture of prudent borrowing, in fact making banks more reluctant to lend to marginal borrowers in the long run. Indeed, debt relief has often served electoral interests[4] and survey evidence suggests that borrowers distinguish sharply between debt with 'hard' and 'soft' conditionality –occasionally to the extent that 'soft' credit from state banks is referred to by a different term than 'hard' debt issued by moneylenders or private banks.

We present three sets of results that suggest a cautionary tale of debt relief. We find, first, that debt relief leads to only a moderate improvement in the overall *level* of household debt among beneficiary households. This result is consistent with evidence from the literature on personal bankruptcy, which shows that households typically accumulate new debt very quickly after a settlement. However, we also show that debt relief leads to a significant and persistent shift in the *composition* of household debt away from formal sector borrowing. Households that benefited from full debt relief borrowed, on

[4]See La Porta, Lopez De-Silanes, and Shleifer (2002), Cole (2009), Dinç (2005), Khwaja and Mian (2005) for evidence on political lending and estimates of the costs of government interventions in credit markets.

average, 6% less from formal sector sources than households in the control group. This effect persists one and a-half years after the debt relief was implemented and is unlikely to be explained by changes in the supply of credit; banks were required to make program beneficiaries eligible for new loans and we additionally show that households that received 100% debt relief were no more likely to be turned down when applying for a new loan than households in the control group. Thus, clearing beneficiaries' collateral did not have the intended effect of increasing liquidity through access to new bank credit.

Second, we demonstrate that debt relief does not increase the investment or productivity of beneficiary households. Comparing the investment decisions of households around the eligibility threshold, we show that beneficiary households, in fact reduce investment in irrigation and agricultural inputs by as much as 7%, potentially as a direct result of the shift towards more expensive sources of financing. In contrast to the predictions of standard debt overhang models we further show that the risk composition of investment projects (as measured by the variance of realized returns after the program) does not differ between households in the treatment and control groups.

Finally, we show that debt relief has a strong effect on expectations about the reputational consequences of default and perceptions about the seniority of debt; a one standard deviation increase in the amount of debt relief increases the probability that beneficiaries would default on a formal sector loan before any other claim by 2.3%. Similarly, beneficiary households are sitgnificantly less concerned about the reputational effects of non-repayment in the case of loans issued by commercial or cooperative banks. Taken together, these results suggest that bailout programs are of limited use in addressing debt overhang problems, but have significant behavioral implications that need to be taken into account in the design of debt relief initiatives indended to improve the financial access of heavily indebted households.

The findings presented in this paper relate the literature on government intervention in credit markets (Dinç 2005, Burgess and Pande 2005, Burgess, Wong, and Pande 2005, Cole 2009) to the literature on debt- and poverty traps (Banerjee and Newman 1993, Banerjee 2000, Mookherjee and Ray 2003). Bolton and Rosenthal (2002) consider an agricultural economy exposed to recurring macroeconomic shocks. They note that, when debt contracts cannot be made contingent on aggregate shocks, ex-post government intervention in debt contracts can be beneficial by providing insurance against otherwise uninsurable events. This argument, however, abstracts from from the potential behavioral implications and moral hazard problems that may arise from the expectation of future interventions. While there exists little empirical evidence on the effect of debt relief on household behavior, this paper relates directly to a recent literature on personal bankruptcy which analogous to debt relief aims to provide a "fresh start" to debtors in distress (Domowitz and Sartain 1999, Campbell 2006).[5] Using data from the

[5]However, with respect to the effect of debt relief on credit supply, an important difference between debt relief and

United States, Han and Li (2008) find that the majority of households filing for personal bankruptcy experience renewed repayment difficulties and accumulate less wealth, even many years after a bankruptcy settlement. Gropp, Scholz, and White (1997) show that lenient personal bankruptcy provisions affect incentives for the ex-post supply of credit, effectively worsening the financial access of poorer borrowers.[6] Similar disincentives for the provision of credit may arise from moral hazard effects that change the probability of repayment, for example due to the expectation of future bailouts Gross and Souleles (2002). The behavioral implications of debt relief at the household level remain, however, poorly understood. This paper represents a first step towards closing this gap in the literature.

The remainder of the paper proceeds as follows. The next section provides an overview over the program and eligibility criteria. Section three reviews the methodology and survey design. Section four outlines the identification strategy, section five presents our results and section six concludes.

2 India's Debt Relief Program for Small and Marginal Farmers

India's 2008 *Debt Relief Scheme for Small and Marginal Farmers* ranks among the largest debt relief programs in history. Enacted by the Government of India in June 2008, the program reached an estimated 36 million farmers across India and covered outstanding debts of Rs 650 billion (US$ 14.4 billion). The program was partly motivated by a highly visible increase in farmer suicides, most notably in the Vidarbha region of Maharashtra, where high indebtedness among farmers was an oft-cited factor. As a sizable transfer to India's important agricultural sector ahead of national elections, the program may have also served other political purposes.[7] Economic theories of risk-shifting, debt overhang (Jensen and Meckling 1976, Myers 1977, Ghosh, Mookherjee, and Ray 2000) and investment-driven poverty traps (Banerjee and Newman 1993, Banerjee 2000) provided additional motivation, with the expectation being that a reduction in household debt would increase the level and efficiency of agricultural investment. Because commercial banks and cooperatives were refinanced through the central bank, the program was also popular with some lenders, and may have helped to revive financially troubled institutions. An important concern, however, was the program's impact on subsequent repayment incentives.

The program considered formal agricultural debt issued by commercial and cooperative banks. This included crop loans, investment loans for direct agricultural purposes or purposes allied to agriculture,

changes in bankruptcy laws is the extent to which creditors are refinanced by the government. While more lenient bankruptcy regulation implies a permanent redistribution away from creditors, this need not be true in the case of debt relief if banks are refinanced by the government.

[6] See also Djankov, McLiesh, and Shleifer (2007) who show that the protection of creditor rights, which may be affected by bailout programs, has important effects on *ex-ante* incentives for the provision of private credit and. Visaria (2010) provides empirical evidence on the effect of strengthening creditor rights using the introduction of debt recovery tribunals.

[7] At the end of 2009, India's agricultural sector accounted for 17.12% of GDP and approximately 66% of total employment (Source: World Bank, World Development Indicators).

and agricultural debt restructured under prior debt restructuring programs. Debt to moneylenders and other informal sources, and loans taken for non-agricultural purposes, were not included in the program.

To qualify for debt relief, a loan had to be overdue or restructured as of December 31, 2007 (well prior to the program announcement). The amount of relief depended on the location and classification of the borrower, with farmers qualifying for either a full 100% waiver or a more limited 25% relief conditional on repayment of the remaining 75%. As shown in Table 1, "small and marginal farmers" received a full waiver, while "other farmers" received the conditional 25% relief. In drought-prone and other designated districts, the partial relief was 25% or Rs 20,000 (US$ 442), whichever was greater.[8]

Table I: Debt Relief by Classification and Location

	Regular districts	Special districts
Small and marginal farmers [< 2 hectares]	100% debt waiver	100% debt waiver
Other farmers [> 2 hectares]	25% debt relief if remaining 75% settled	25% or Rs 20,000 relief whichever is greater, if remainder settled

Farmer classification depended on the type of loan. For direct agricultural loans, classification was based on the total landholdings of the farmer at the time the loan was written. Farmers with two or fewer hectares of total land were classified as small or marginal; farmers with more than two hectares were classified as other farmers.[9] For allied-to-agriculture loans, farmers with loans Rs 50,000 (US$ 1,105) and under were considered small or marginal, while farmers with larger loans were considered other farmers. Implementation began on June 30, 2008, with full waivers being granted immediately. 25% relief was granted upon repayment of the remaining 75%, with an initial deadline of June 30, 2009.[10]

[8] Many districts qualified for this extra relief. In the state of Gujarat where the survey described in this paper was carried out, 20 of 26 districts qualified. The analysis in this paper considers only accounts from bank branches not located in 'special' or drought-affected districts.

[9] For banks operating in acre units, the cut-off was five acres, which is not exactly two hectares. In the sample used here, the commercial banks operated in hectares and the cooperatives operated in acres.

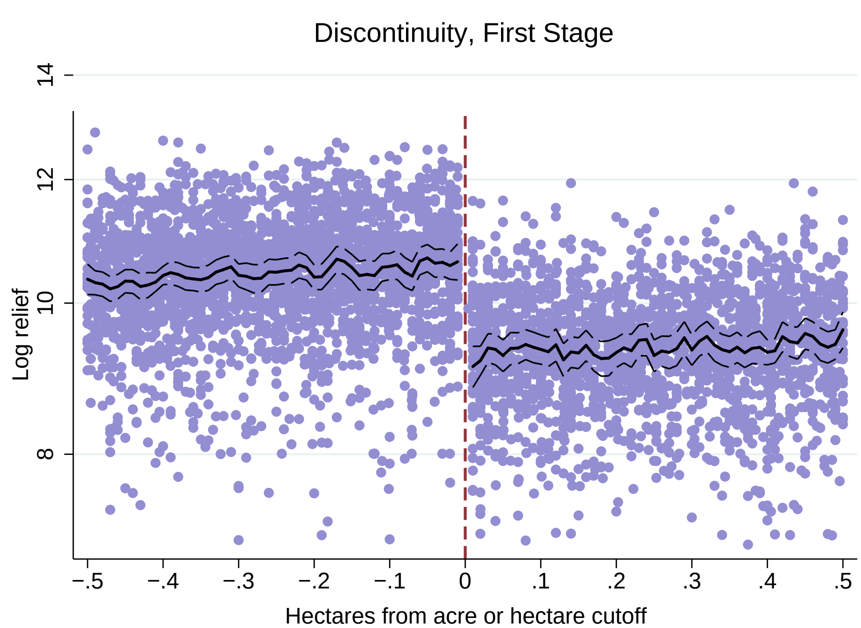
[10] This deadline was eventually extended by one year in order to accommodate those who had trouble repaying their 75%. The goal was 100% participation.

3 Empirical Strategy

We employ a regression discontinuity design (RD) to identify the effect of debt relief on consumption, investment and subsequent household-level financial decisions. The research design exploits the fact that, unlike previous debt relief initiatives, eligibility for India's *Debt Relief Scheme for Small and Marginal Farmers* was based on land holding criteria, thus creating a discontinuity in the amount of debt relief around the eligibility threshold of 2 hectares. Those to the left of the eligibility threshold received 100% relief while those to the right qualified for only 25% of conditional relief.

Figure I illustrates the strong discontinuity in debt relief around the program eligibility cutoff. In Table 4, we additionally report numerical estimates of the difference in debt relief between treatment and control. On average, households marginally below the cut-off received Rs 37,156 (US$ 840) more debt relief than households marginally above the threshold. At Rs 44,037 (US$ 995), the discontinuity is more pronounced in the subsample of commercial banks than in the sample of cooperative bank accounts (Rs 34,339 or US$ 776). Overall, the difference in relief at the discontinuity is substantial and corresponds to approximately 84% of India's 2010 annual per capita income (Rs 44,345 or US$ 1002).

Figure I: Discontinuity in Implemented Debt Relief



Notes: This figure plots the log relief amount for households benefiting from 100% debt relief (left) and conditional 25% debt relief (right). The solid lines to each side of the eligibility threshold show Epanechnikov kernel regressions with a bandwith of $\rho = 0.001$ and 99% confidence intervals marked by the accompanying dashed lines.

Presuming that banks followed the rules of the debt relief program faithfully, we can estimate the causal effect of debt relief, using a *sharp* regression discontinuity design (Imbens and Lemieux 2008a,b, Hahn, Todd, and Van der Klaauw 2001). Identification using the sharp regression discontinuity design rests on the assumption that inclusion in the program, i.e. treatment status, is determined by a cutoff score $\bar{z}$ along an assignment variable Z_i and therefore quasi-randomly assigned. In our context, the the running variable is the amount of land pledged as collateral at the time the loan was disbursed. Without loss of generality, we rescale this variable so that the program eligibility cutoff point is centered at zero and use "hectares from cutoff" as the assignment variable throughout the analysis.

The sharp RD approach relies on two fundamental identifying assumptions. The first identifying assumption is that the running variable –and therefore treatment status– is not subject to manipulation. We argue that ex-ante manipulation of land status was highly unlikely for several reasons. The program was the first of its kind in India that made eligibility conditional on collateralized land, rather than the vintage or amount of outstanding debt. In addition, several mechanisms were in place to assure faithful implementation and prevent the ex-post manipulation of land documentation. As a transparency measure, bank branches were required to publicly post the land and debt relief details of all eligible individuals. Banks themselves had multiple levels of internal audits and the central bank and local regulators performed further audits. In addition, we tested for robustness to corruption concerns by auditing official land documents and comparing them to records from a statewide database of landholdings. We present evidence from these additional robustness checks in Appendix B.

The second fundamental identifying assumption underpinning the RD approach states that potential outcomes and ex-ante observables are continuous in the forcing variable. Formally, for these variables both $E\left[Y_0|Z=z\right]$ and $E\left[Y_1|Z=z\right]$ must be continuous in the forcing variable Z. If this assumption holds around the cut-off, then any discontinuity in outcomes observed at the cut-off can be attributed to the discontinuity induced by the treatment, in this case, debt relief. As a test of this identifying assumption, Figure 2 plots the conditional means of observables around the eligibility threshold. Table A.2 provides additional empirical tests for continuity.

The sharp RD approach can be implemented in two ways. The first approach, usually referred to as the *parametric control function* approach (Heckman and Robb 1985), estimates a model of the form,

$$y_i = \alpha + \beta T_i + f(Z_i) + \epsilon_i \tag{3.1}$$

where y_i is an outcome of interest, T_i is a treatment indicator and $f(Z_i)$ is a linear or polynomial function of the running variable Z, such that the local average treatment effect (LATE) at the discontinuity is estimated by the parameter β. A second, alternative approach is to consider only observations in close proximity of the discontinuity and estimate $y_i = \alpha + \beta T + \epsilon$ in an arbitrarily small neighborhood around the cutoff $\bar{z}$, $z_i \in \{\bar{z} + \delta, \bar{z} - \delta\}$.[11]. While we follow the parametric control function approach, it is worth noting that we surveyed only households within a narrow band of ±0.5 hectares of the eligibility cutoff.[12] We first estimate basic specifications of the form,

$$y_i = \alpha + \beta T_i + \theta_1 Z_i + \theta_2 \big(T_i Z_i\big) + \epsilon_i \tag{3.2}$$

where y_i is an outcome of interest, T_i is the treatment indicator, Z_i is the running variable (hectares from cutoff, normalized such that the cutoff $\bar{z}$ is centered at zero), β is the treatment effect at the discontinuity, the coefficients θ_1 and θ_2 capture the slopes of the regression line, allowing them to differ on either side of the cutoff and ϵ_i is a stochastic error term. We choose $f(Z_i)$ to be linear, so that the function can be interpreted as measuring the average treatment effect at $Z = z$.[13]

In our preferred specification, we further add bank×district, interviewer, and month-of-interview fixed effects and a vector of other controls, $\mathbf{X}_i$, and estimate the following specification:

$$y_i = \alpha + \beta T_i + \theta_1 \big(T_i Z_i\big) + \theta_2 Z_i + \phi_{bd} + \phi_j + \phi_t + \zeta' \mathbf{X}_i + \epsilon_i \tag{3.3}$$

Importantly, there was not a single, homogeneous treatment. Rather, program beneficiaries received relief according to the widely varying sizes of their qualifying overdue balances. In order to estimate the potentially heterogeneous treatment effects, specification (3.3) is extended as follows:

$$\begin{aligned} y_i = \alpha + \beta_1 T_i + \beta_2 \big(lnB_i T_i\big) + \theta_1 \big(T_i Z_i\big) + \theta_2 \big(lnB_i\big) \\ + \theta_3 H_i + \phi_{bd} + \phi_j + \phi_t + \zeta' \mathbf{X}_i + \epsilon_i \end{aligned} \tag{3.4}$$

In this specification, lnB_i is the log eligible balance indicating the magnitude of the treatment, β_1 is the average treatment effect for farmers with average-sized overdue balances, and β_2 is the additional marginal treatment effect for farmers with larger or smaller balances. This specification allows us to identify the heterogeneous treatment effect of varying amounts of debt relief. Because the balance term is logged, β_2 is most easily interpreted as the effect of proportional changes. For example, a threefold increase in balance size (roughly 1 log point) causes a β_2 shift in the outcome variable among the treated.

[11]See for example Angrist and Lavy (1999).

[12]This corresponds to the optimal bandwith obtained using a cross-validation procedure based on the land distribution of all accounts in the sample frame.

[13]Note that when run with only observations within a narrow band around the cutoff, this regression is effectively the same as running local linear regressions on either side of the cutoff.

4 Bank Data and Debt Relief Survey

To implement the RD analysis, we draw on data from two main sources. The first source consists of administrative data from participating banks. As an anti-corruption measure, banks were required to publicly disclose details about all qualifying debt relief beneficiaries. All information was posted to the public notice boards of participating bank branches, and bank websites. This included the name, village, loan category, date of original disbursal, overdue principal and interest as of December 31, 2007, and eligible relief amount. Additionally, banks were required to disclose the amount of land pledged by the borrower at the time the loan was originated. Throughout the analysis, we use the land data as the running variable and identifier of program eligibility throughout the analysis. Because of the importance of the landholding data to the regression discontinuity design, we further audited the official land records of the majority of surveyed households using electronic records from the state of Gujarat's electronic repository of official land records. More details on the land audits are reported in Appendix B.

The second and main source of data is a detailed survey of debt relief beneficiaries, conducted in late 2009, roughly one and a-half years after the program was implemented. The survey covered 2,897 households in the western Indian state of Gujarat and included detailed questions on household income, consumption, investment and financial decisions, as well as background information about the household and its members. In this section, we define the sample frame and relevant discontinuity using administrative data from participating banks, and provide details on the survey data and procedures.

4.1 Bank Data and Sample Frame

As a transparency measure, banks were required to publicly post details about all qualifying debt relief beneficiaries. This included the name, village, loan category, date of original disbursal, overdue principal and interest as of December 31, 2007, and eligible relief amount. Some banks also included the purpose of the loan as well as the original principal amount. All of this was posted to the notice boards of participating bank branches, and several banks also posted the information on their websites. This detailed account-level data, obtained from from the six largest commerial banks and the largest cooperative bank in the state[14] provided the basis for constructing the sample frame and contained details of landholdings associated with each account, used as the running variable in the RD analysis.

The initial sample frame included 5,554 accounts, comprising all eligible borrowers from the state's largest seven banks, which accounted for 87% of debt relief in the state. The sample covers crop loans and

[14]The banks are Bank of Baroda, Bank of India, Central Bank of India, Dena Bank, State Bank of India, Union Bank of India, and Kaira District Central Cooperative Bank.

investment loans for direct agricultural purposes, but excludes loans not directly related to agriculture and loans restructured by banks previous to the program, since these loans were not contingent on landholdings, so that the discontinuity induced by the program does not apply for this subset of loans. We further excluded previously restructured loans, because we observe neither the original ticket size nor the vintage and initial terms of these loans. This restricts the class of loans covered to the roughly 70% for which landholding was determinant of debt relief qualification. Table 2 reports the number of beneficiaries in the sample frame by bank, Table 1 reports corresponding figures for the entire population of loans covered by the program, including those in banks outside the sample frame.

Table A.1 provides additional summary statistics, and Figure B.4 shows the distribution of eligible relief for all accounts included in the sample frame. The average relief per beneficiary in the sample frame, Rs 33,498 (US$ 740), is substantially higher than the Gujarat average of Rs 24,275 (US$ 540), for several reasons. First, the bulk of qualifying farmers have less land than those included in the sample frame. Since there is a positive relationship between landholding and loan size and also between loan size and relief amount, larger landowners will tend to get more relief.[15] Second, some banks not included in the sample frame, such as rural regional banks, are likely to issue smaller loans on average than the larger commercial and cooperative banks included in the sample frame.

4.2 The Debt Relief Survey

We surveyed 2,897 households in four rural districts of the western Indian state of Gujarat between October and December 2009.[16] The four sample districts, Mehsana, Gandhinagar, Kheda, and Anand, form a contiguous band in the central and northwestern part of Gujarat. These districts include relatively rich agricultural land and are slightly more rural than Gujarat as a whole, with 64-80% of households residing in rural areas. Like any of India's 28 states, Gujarat is unique in some ways and ordinary in others. It is richer than average, with a per-capita income about 26% above the all-India average (Government of Gujarat 2008a). It is also more urban than the rest of the nation, with 37% of its population living in urban areas versus 28% for India overall. Agriculture makes up about the same share of Gujarat's economy, however, as for India overall. In terms of banking, Gujarat enjoys slightly higher than average commercial bank coverage, with one commercial bank per 14,220 inhabitants, versus 15,601 for India overall (Government of Gujarat 2008a,b, Government of India 2001a,b). Nearly one million Gujarat farmers qualified for debt relief under the 2008 scheme, with average relief of Rs 24,275

[15]The banks determine a farmer's maximum loan size largely based upon the size of his land and the crops cultivated. The more land a farmer has and the more valuable the crops he grows, the more he can borrow. The relationship between loan size and relief amount is thus mechanical, since the relief is either 100% or 25% of the overdue balance.

[16]Conducting a baseline survey was not feasible, as the program was enacted very shortly after its announcement and comprehensive lists of beneficiaries were not available sufficiently ahead of its implementation.

(US$ 540). This was 37% higher than the all-India average relief of Rs 17,712 (US$ 392). However, because it is more urban and therefore had relatively fewer beneficiaries, Gujarat received slightly below-average relief on a per-capita basis (Government of India 2008).

Because the identification strategy is based on a regression discontinuity design, we surveyed only accounts within a narrow band of ± 0.5 hectares around the 100% relief cutoff. The ± 0.5 hectare bandwidth was chosen following a process similar to the cross-validation procedure described in Imbens and Lemieux (2008b). The chosen range was the bandwidth that minimized the mean squared error when predicting relief amount with landholding and a 100% waiver indicator. Because different banks implemented the program cutoff as either two hectares or five acres (2.023 hectares), the bandwidth is calculated at the bank level.[17]

Sample households were visited by survey teams between October and December 2009 and asked to participate in a comprehensive household survey. In all, 2,897 surveys were completed. Table 3 summarizes the administration results and presents tests for balanced attrition across treatment and control. Tested jointly, balanced attrition across all categories cannot be rejected at traditional levels of significance ($p = 0.24$), and neither does it appear that attrition was systematically related to either landholding or relief amount ($p = 0.68$). The relatively high refusal rate is not surprising given that the survey was lengthy, taking more than two hours to administer, and given that participants were not compensated for their time. Most households took loans in the name of the head of household, who was often the oldest male member. This helps to explain the sizable mortality rate, which increases as expected in loan age. Migration for work is not uncommon, and here migration also includes cases where the respondent had temporarily left the village or was otherwise out of town on business. Because only imperfectly recorded and transliterated names were available from the banks, many villages had multiple individuals with the same name, which created an additional obstacles to the correct identification of individuals in the sample frame.

For the vast majority of surveys (84%), we interviewed the actual borrower identified by the bank, such that the official holder of the loan was both the user of the loan and the household's main financial decision-maker. When somebody else in the household was the financial decision-maker and the loan's true user, we interviewed that individual instead. We only interviewed another household member once we verified that we had identified the actual borrower and that this borrower confirmed that the other household member was both the financial decision-maker and the actual user of the loan in question.[18]

[17] Bank records were not perfect, and landholding was not reported for some accounts. Accounts without reported landholding were excluded from the sample frame. Because this was a small number of accounts falling into both the 100% waiver and 25% relief categories, the resulting attrition was random and unlikely to introduce bias into the analysis.

[18] This typically occurred when the loan was taken out in the father's or wife's name – because he or she owned the land– but the son or husband was the true financial decision-maker and user of the loan.

5 Main Results

5.1 The Level of Household Debt

Did debt relief improve the overall financial position of beneficiary households? Table 5 presents results on the total indebtedness of beneficiary households before and after the program. Panel A presents regressions without controls and interaction terms between treatment and eligible balance, Panel B presents results using the preferred specification with controls and interactions between the treatment and the amount of relief. In columns (1) and (4) we begin by comparing debt levels between treatment and control before the program, columns (2) and (5) estimate the level of debt after the program, and columns (3) and (6) consider the change in self-reported total debt for the sample of households for which both pre- and post-data is available. While the point estimates suggest that households in the treatment group are, on average less indebted in the period after debt relief, the coefficients are not precisely estimated and the hypothesis that debt relief left the overall level of household debt unaffected cannot be rejected at conventional levels. This result resonates with evidence from the literature on personal bankruptcy (see e.g., Han and Li 2008), which shows that households often return to high levels of debt very quickly after a settlement. Does this constitute evidence that debt relief was ineffective? Not necessarily. Note that an important objective of the program was to clear the pledged collateral of marginal borrowers in order to reintegrate them into the formal financial sector. Therefore, an intended and rational response of beneficiary households would have been to use the free collateral to secure new bank loans.[19] In the next subsection we explore whether the program indeed had this effect.

5.2 The Composition of Household Debt

We next turn to the effect of debt relief on the composition of household debt. Table 6 reports the results, again distinguishing between the pre- and post-program periods. The estimates show that debt relief has a strong and persistent effect on the composition of household debt. As in the previous subsection, we compare the composition of debt before the program (columns(1) and (2)) to the composition of household debt after the program (columns (3) and (4)) and additionally present estimates of the change in borrowing between the two periods in columns (5) and (6). As one would expect, there is no significant difference in the composition of borrowing between the treatment and control group prior to the program. However, the estimates show a significant shift in the composition of borrowing

[19] An important feature of the program was that banks were required to make beneficiaries eligible for a new loan once their existing debt had been written off. Below, we provide evidence that beneficiaries of full debt relief were not more likely to be turned down when applying for a loan than households that had received no benefit under the program or paid down 75% of their outstanding balance.

among treatment households *away* from credit cooperatives and commercial banks and towards informal sector sources of credit. Approximately one and a-half years after the debt relief program was enacted, households that had benefited from 100% debt relief held on average 7.7% less formal sector debt and 5% more informal sector debt than households in the control group. This suggests that, overall, beneficiary households did not use their cleared collateral to obtain new formal sector financing. This shift in the sources of borrowing is modified by the *amount* of debt relief. While the additive effect of the amount of debt relief is only marginally significant ($p = 0.109$), a one standard deviation increase in the amount of debt relief reduces the effect on the share of bank financing to almost zero and substantially reduces the treatment effect on formal sector financing.

In Figure 3 and Table 7 we further disaggregate the shift in the composition of household borrowing, distinguishing between commercial and cooperative banks, loans from moneylenders and traders and friends and family, respectively. The results show that the reduction in formal sector borrowing is primarily due to a decline in borrowing from cooperative banks (columns (6) and (12)). Interestingly, the increase in the share of informal sector borrowing is primarily due to a higher percentage of loans from family and friends, rather than loans from moneylenders and traders. Among households in the treatment group, the percentage of financing obtained from cooperative banks declined by 5.85%, while the percentage of total credit obtained from friends and family increased by 3.5%. Both estimates are statistically significant at 5% level. This reallocation effect is again moderated by the amount of debt relief. While the percentage of total debt obtained from moneylenders increased among the treatment group, the point estimate is not significant at conventional levels.

Could the shift towards informal sector borrowing among debt relief beneficiaries be driven by changes in the supply of credit? In Table 8, we provide evidence that a supply side explanation is unlikely to explain the relative decline in formal sector borrowing among beneficiary households. Recall first that the program required banks to make beneficiaries eligible for new loans. To verify that there was in fact no differential discrimination against beneficiary households applying for new loans, we present summary statistics on new loan applications after the program at the foot of the table. Despite the fact that all households in the 100% relief category qualified for a new loan, only 31.8% applied for bank credit after the program. However, at 2.5% versus 1.92%, households in the treatment group were not significantly more likely to be denied credit than households in the control group. The estimates in columns (1) to (3) reiterates this finding in a regression framework: beneficiary households were no more likely to be turned down for a loan and, conditional on a loan being approved, interest rates did not differ between treatment and control. Taken together, these results provide strong evidence that the shift in the composition of borrowing we document is not driven by changes in the supply of credit.

5.3 Investment and Productivity

How well suited are bailout programs to address problems of debt overhang? Table 9 reports results on post-program investment and productivity. In Panel A, we first consider post-program investment in agricultural inputs including irrigation, fertilizer and pesticides (column 1), hired outside labor (column 2) and investments in capital goods such as tractors, tubewells and other agricultural equipment (column 3).[20] The results show that debt relief beneficiaries are not more likely to undertake productivity enhancing investments than households in the control group. The estimates in column (1) indicate that, in fact, households in the treatment group reduced spending on agricultural inputs by 11%. In Table 10 we look at investment in agricultural inputs in greater detail, distinguishing between total investment in agricultural inputs, investment per household member and investment per acre of cultivated farm land. The results in columns (1)-(3) and (7)-(9) show that households in the treatment group reduced their spending on agricultural inputs by approximately 6-14%. In columns (4)-(6) and (10)-(12) we use indicators for an increase in input spending between the pre-and post-program periods as the dependent variable and show that beneficiaries of full debt relief were 7-8% less likely to increase spending on agricultural inputs between the pre-and post-program period than households in the control group.

Table 9, Panel B and Table 11 report results on productivity, measured as total revenues from agricultural production, revenues per household member and revenues per acre over the first two post-program crop seasons, respectively. In line with the patterns of investment the treatment effect estimates in Table 9 columns (4)-(6) are negative throughout, indicating a decrease in output and productivity among beneficiaries of full debt relief. These estimates are significant at conventional levels only in the robustness sample but not the sample of all surveyed households, so that the hypothesis that debt relief had no impact on output and productivity can not be rejected. There is, however no indication that debt relief led to an increase in investment among any of the three primary investment categories.

Table 9 column (6) and Figure 4 presents an additional test of the debt overhang hypothesis. Because the downside of risky investments undertaken by indebted households is largely borne by debt holders, theories of debt overhang and risk-shifting imply that debt relief should reduce risk-taking among beneficiary households. In Figure 11 we use the variance of realized returns between the first two post-program monsoon seasons as a proxy for the riskiness of investment and show that the variance of output does not differ between treatment and control. Supporting this result, Table 9 reports treatment estimates from a regression in which the left hand side variable is the coefficient of variation of log output

[20] These items represent the main investment opportunities of households in the sample and account for more than 90% of investment expenditure. To account for seasonal variation, each outcome is calculated as the arithmetic mean of the first post-program summer or monsoon crop (Kharif 2008) and the first winter or dry season crop (Rabih 2008-2009). The results remain qualitatively unchanged when we restrict the sample to one post-program crop season.

in the first two post-program monsoon seasons. The results again show that there is no systematic difference in the variance of realized output between treatment and control. Thus, in contrast to the view that bailout programs represent an effective cure for problems of debt overhang, we find no support for the hypothesis that debt relief lead to a measurable increase in investment or a shift towards less risky investments as measured by a lower variance of realized returns.

5.4 Expectations

Perhaps the most serious criticism of large bailout programs is their potential to induce moral hazard by affecting beliefs about the enforceability of debt contracts and the consequences of default. We begin to explore this hypothesis in Table 12 by considering, first, the effect of debt relief on beliefs about the seniority of debt from different sources. The dependent variable in columns (1) through (6) is based on answers to the survey question "Suppose you had taken out a loan from each of the following sources and encountered financial difficulties. On which loan would you default first?" and equal to one whenever a respondent listed the respective source as the first type of loan on which she would default.

We show that debt relief increases the reported probability of default for bank loans, but not for loans obtained from the informal sector. Moreover, within formal sector loans, borrowers appear to distinguish sharply between loans originated by commercial and cooperative banks. More specifically, the estimates in 12, column (2), suggest that a one standard deviation increase in the amount of debt relief leads to a 2.6% increase in the probability of default ($p = 0.047$), while no such effect is apparent for either commercial bank ($p = 0.53$) or informal sector loans ($p = 0.81$).

The enforcement of debt contracts in emerging markets relies heavily on the reputational consequences of default. Indeed, the majority of survey respondents stated that they were either 'worried' (44%) or 'very worried' (12%) about the reputational consequences of non-repayment, irrespective of the source of the loan. Did debt relief affect these perceptions? Table 13 reports estimates of the effect of debt relief on beliefs about the reputational consequences of default. As in the previous regressions, we distinguish between the four main types of lenders (columns 1 through 4) as well as the group of formal and informal sector lenders (columns 5 and 6). Debt relief has a strong effect on expectations about the reputational consequences of non-repayment. As the estimates in columns (1) and (2) show, debt relief beneficiaries are significantly *less* concerned about the reputational consequences of defaulting on debt issued by commercial and cooperative banks. They are, however, *more* concerned about the reputational implications of defaulting on loans obtained from family and friends.

Finally, in table 14 we report results from a survey question that asked respondents about their expectations about the effect of default on future financial access. The dependent variable here is based on answers to a survey response that asked "Suppose you were unable to repay a loan to each of the following lenders. How worried would you be that this would preclude you from future borrowing from this lender?". The results in column (1) and (2) seem, at first, surprising. Households that benefited from debt relief state that they would in fact be more concerned about the effect of non-repayment on the future ability to borrow from formal sector lenders. While this result seems somewhat at odds with the result above, indicating that debt relief beneficiaries are more likely to default on formal sector loans in the future and less concerned about the reputational consequences of default, our finding suggests that debt relief reinforced the awareness of collateral constraints. Borrowers unable to repay their outstanding balance with a formal sector lender still have their collateral tied to the outstanding loan and are therefore unable to access new credit from formal sector lenders. This observation may also explain some the low demand from new loans and the shift towards informal sources of credit. If, as it appears to be the case, borrowers have sufficient access to informal sector credit, the collateral requirements tied to new bank loans may reduce demand for new bank loans –even in the face of comparatively higher informal sector interest rates.

6 Conclusion

This paper studies the effect of debt relief on the economic decisions and expectations of beneficiary households in rural India based on a survey of 2,897 households affected by the Indian *Debt Relief Program for Small and Marginal Farmers* –one of the largest debt relief programs in history. Using a regression discontinuity design based on the program eligibility criteria, we show that debt relief does little to improve the financial position of beneficiary households, but has strong effects on beliefs about the seniority of debt and the reputational consequences of default.

One and a-half years after the program was enacted, beneficiaries of full debt relief are not significantly less indebted than households in the control group. However, we document a strong and persistent shift in the composition of debt, leading to a stronger reliance on informal credit among beneficiary households. Using evidence from post-program loan applications, we show that this change in the composition of borrowing is unlikely to be explained by a reduction in the supply of bank credit. In contrast to the predictions of theories of debt overhang, debt relief does not lead to a measurable increase in investment, an improvement in the productivity or a reduction in the variance of realized returns to agricultural investment among households that had their debt cleared under the program.

Indeed, it appears that the strongest effect of debt relief is its impact on beliefs about the seniority of competing claims and the reputational consequences of default. Households in the treatment group state that they would be much more likely to default on cooperative bank loans in the future, and this propensity is increasing in the amount of debt relief they received. Similarly, recipients of full debt relief are significantly less concerned about the reputational consequences of defaulting on bank debt, which suggests that debt relief removes much of the social stigma associated with financial distress.

These results represent some of the first formal evidence on the effect of debt relief at the household level, and are largely consistent with the literature on personal bankruptcy in developed economies. Evidence from the personal bankruptcy literature shows that, in general, households build up new debt very quickly after a settlement and accumulate less wealth even many years after a bankruptcy settlement. Both debt relief and bankruptcy settlements appear to be of very limited use in improving the financial situation of marginal borrowers, but have considerable potential to alter borrower expectations.

The results do, however, point to a number of interventions that may improve the efficiency of debt relief initiatives. First, we show that, contrary to the intentions of the program, households did not use cleared collateral to obtain new bank loans. Despite evidence that there was no differential discrimination against debt relief beneficiaries, households shifted their borrowing away from bank lending (especially cooperative bank lending) towards potentially more expensive informal sector sources of finance. This may partly explain the observed decline in investment among program beneficiaries and suggests that policies intended to affect investment and truly provide a "fresh start" to beneficiary households and promote productive investment need to combine debt relief with more direct policies supporting access to bank credit. Second, and more generally, our results show significant variation in the severity of credit constraints and the impact of debt relief across households with different levels of initial productivity and asset wealth, suggesting that a narrower set of targeting criteria might have achieved a greater impact on welfare and productivity.

However, the most important point highlighted by our analysis concerns the behavioral implications of debt relief. Indeed, the finding that households that had a higher total amount of debt cleared report that they would be more likely to default on cooperative bank debt in the future appears to confirm fears that debt relief is detrimental to the culture of prudent borrowing. It is worth noting, however, that borrowers appear to distinguish between different formal sector lenders. Both the shift away from formal credit and the updating of beliefs about the seniority of debt are less pronounced for commercial than for cooperative banks, which are for example regarded as more vulnerable to local political influence. This may suggest that the ex-ante credibility of a formal sector lender may have an important effect on the ex-post effect of debt relief on borrower behavior.

In order to assess the long-term welfare implications of debt relief, it is important to understand how the credit supply response of banks is affected by debt relief and its potential impact on the repayment behavior of marginal borrowers. In this paper, we take a first step in this direction and show that, based on survey data from new loan applications after the program, it appears that banks do not discriminate against borrowers who benefited from debt relief. However, much more evidence is needed on the long-run impact of debt relief on credit supply. Examining this effect in greater depth is an important direction for future research.

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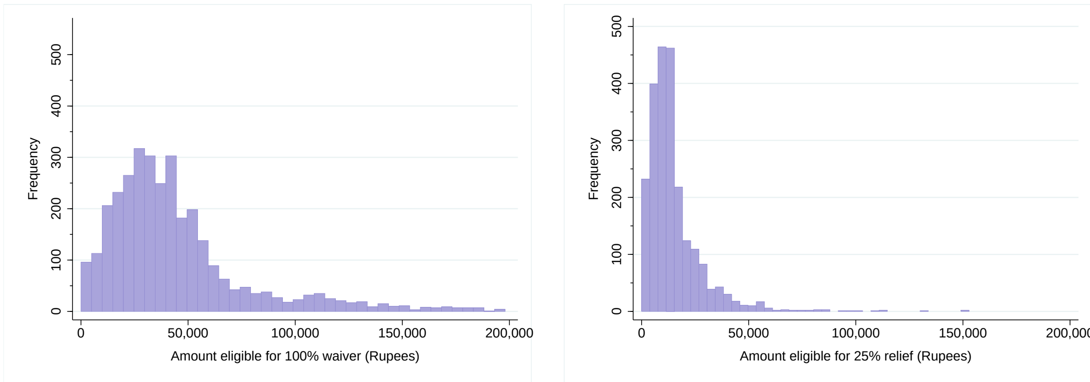
Figures and Tables

Figure 1: Distribution of Eligible Relief Amount

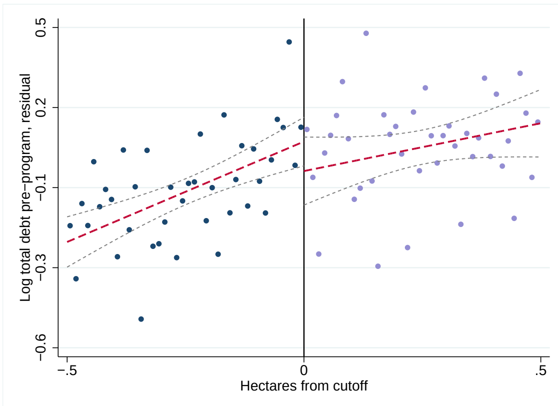
(a) 100% Debt Relief **(b)** 25% Conditional Debt Relief



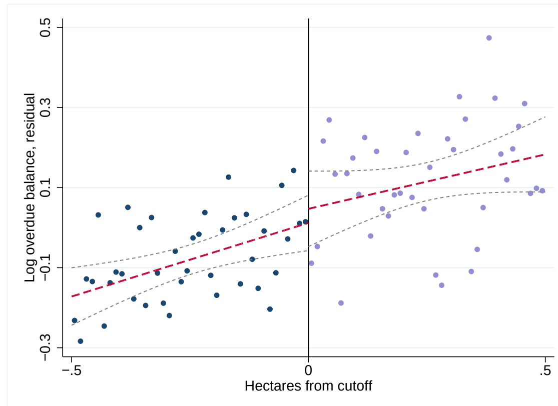
Notes: Includes all beneficiaries within sample frame, including those not surveyed. Excludes 32 observations above Rs 200,000.

Figure 2: Continuity, demographics and pre-program observables

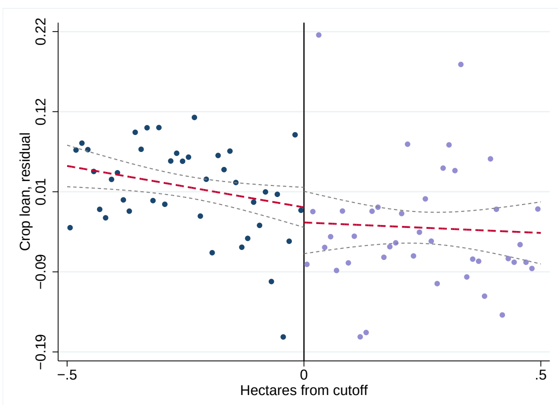
(a) Log total debt, pre-program



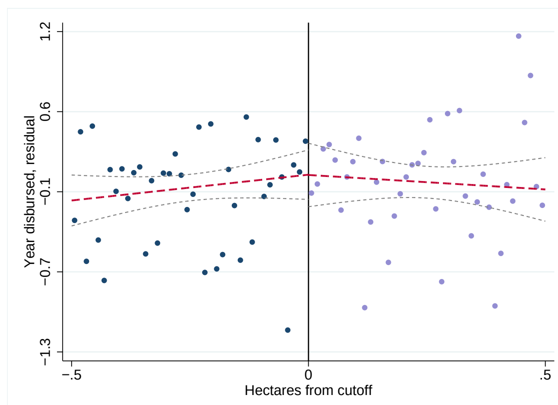
(b) Log eligible balance



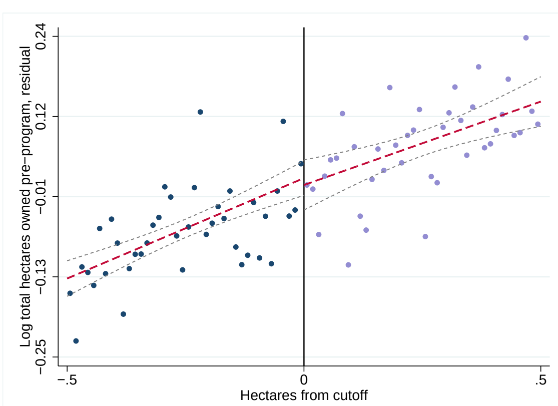
(c) Crop loan



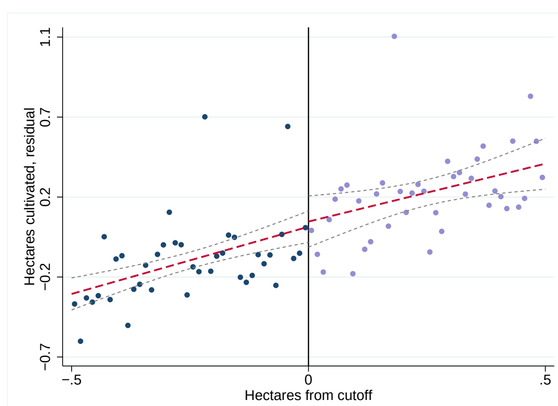
(d) Year disbursed



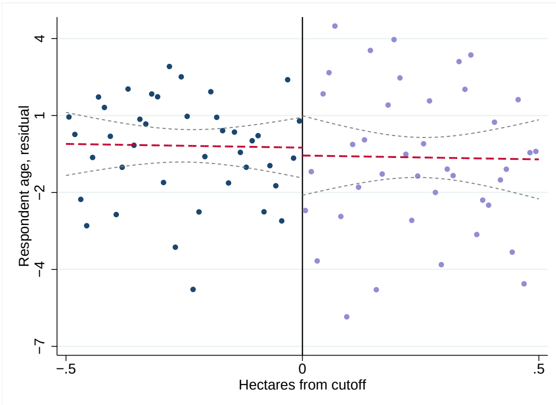
(e) Land owned, pre-program



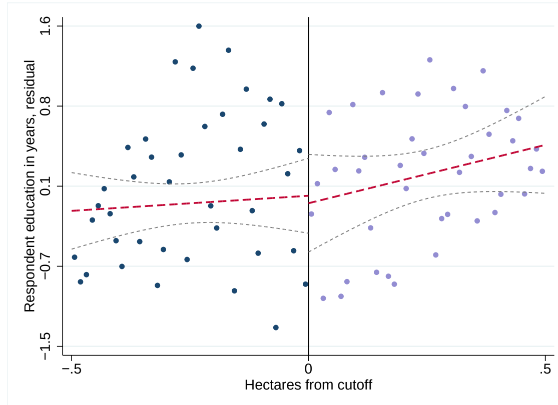
(f) Land cultivated, pre-program



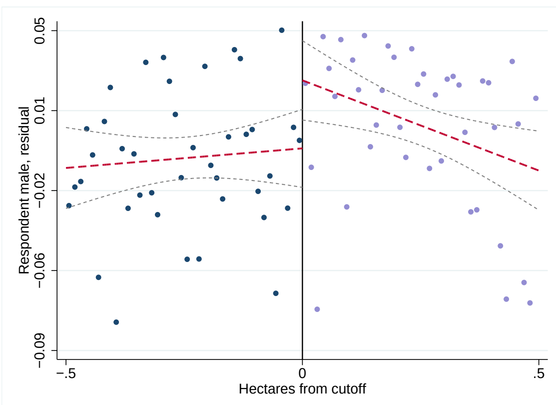
(g) Respondent age



(h) Respondent education



(i) Respondent male



(j) Household size

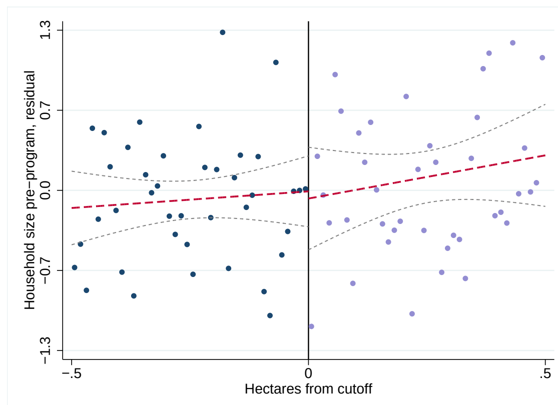
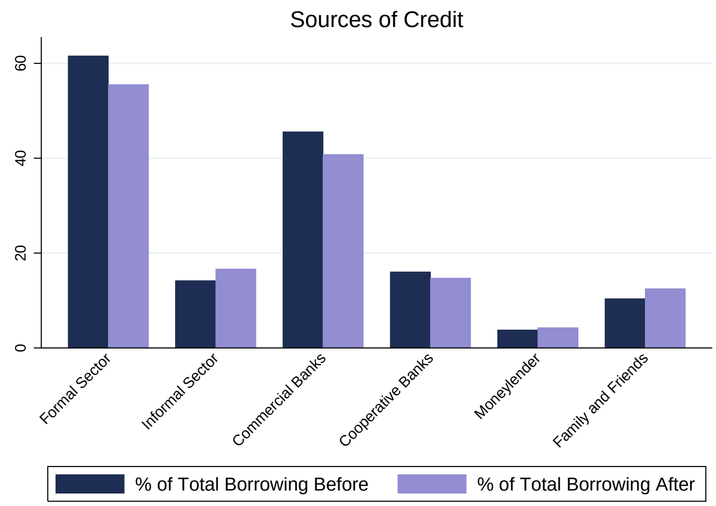
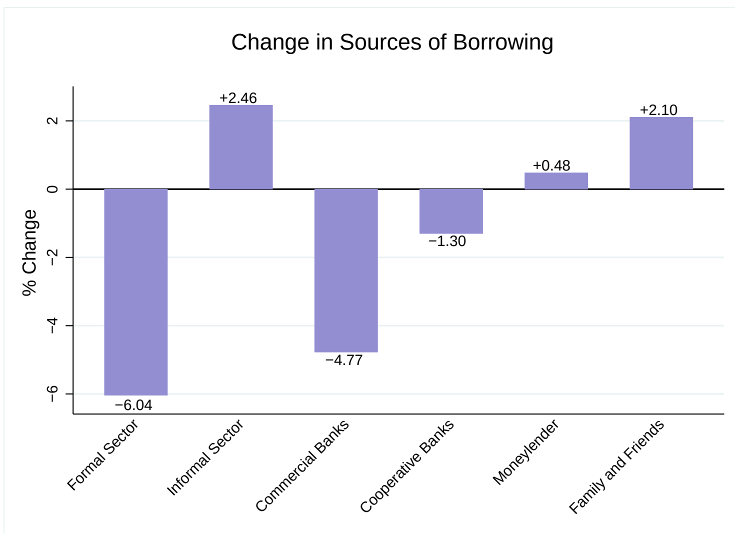


Figure 2: Sources of Credit



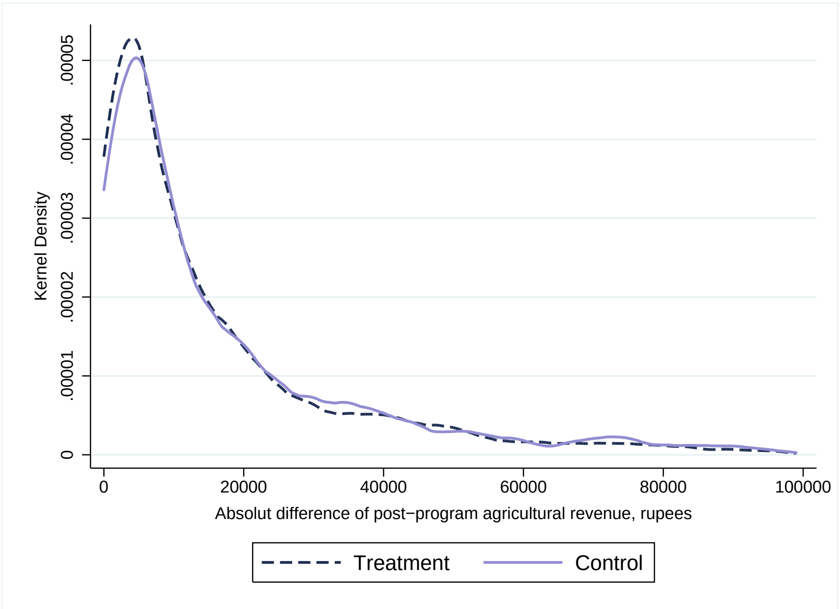
Notes: Percentages refer to the self-reported share of financing obtained from each source. *"Formal sector"* includes borrowing from commercial and cooperative banks. *"Informal sector"* includes borrowing from moneylenders, shopkeepers and traders, family and friends. *"Before"* refers to respondents' self-reported borrowing two years before the survey (November 2007) *"After"* refers to respondents' self-reported borrowing at the time of the survey (November 2009).

Figure 3: Change in Sources of Borrowing



Notes: *"Formal sector"* includes borrowing from commercial and cooperative banks. *"Informal sector"* includes borrowing from moneylenders, shopkeepers and traders, family and friends. *"Before"* refers to respondents' self-reported borrowing two years before the survey (November 2007) *"After"* refers to respondents' self-reported borrowing at the time of the survey (November 2009).

Figure 4: Dispersion of Productivity (Post-Program)



Notes: This figure plots the kernel density of the absolute difference between agricultural revenue in the first and second post-program Kharif (monsoon) season by treatment status.

Table 1: Program Beneficiaries by Bank and District

This table reports summary statistics on program beneficiaries by bank and district. Observations cover all beneficiary accounts from the largest six commercial banks and the state's largest cooperative bank, accounting for 91% of eligible accounts in the districts covered by the survey.

	District				
	Anand	Kheda	Gandhinagar	Mehsana	Total
Bank of Baroda	1,941	3,644	503	1,070	7,158
Bank of India	877	870	343	432	2,522
Central Bank of India	1,384	738	243	253	2,618
Dena Bank	654	366	794	803	2,617
State Bank of India	3,412	2,711	916	3,187	10,226
Union Bank of India	1,013	1,428	306	84	2,831
Kaira District Coop Bank	21,141		0	0	21,141
Total	40,179		3,105	5,829	49,113
Other banks	3,956		491	14,933	19,380
Total	44,135		3,596	20,762	68,493

Source: Gujarat State Level Banker's Committee.

Table 2: Sample Frame by Bank and District

This table summarizes all accounts included in the sample frame and qualifying for a 100% waiver or 25% conditional debt relief. Observations in the sample frame are drawn from administrative data published by the largest six commercial banks and the largest cooperative bank in the state of Gujarat. Percentages refer to the proportion of total beneficiaries (reported in Table 1) included in the sample frame.

	District				
	Anand	Kheda	Gandhinagar	Mehsana	Total
Bank of Baroda	276	276	35	70	657
	14%	8%	7%	7%	9%
Bank of India	84	95	33	34	246
	10%	11%	10%	8%	10%
Central Bank of India	215	39	25	16	295
	16%	5%	10%	6%	11%
Dena Bank	84	47	122	144	397
	13%	13%	15%	18%	15%
State Bank of India	216	291	159	237	903
	6%	11%	17%	7%	9%
Union Bank of India	198	199	36	11	444
	20%	14%	12%	13%	16%
Kaira District Coop Bank	1,442	1,170	—	—	2,612
	12%		—	—	12%
Total	2,515	2,117	410	512	5,554
	12%		13%	9%	11%

Source: Gujarat State Level Banker's Committee.

Table 3: Survey Coverage

This table presents summary statistics on survey coverage and tests for balanced attrition across treatment and control. "Surveyed" includes duplicates, where the same beneficiary had multiple loans in the sample frame; 2,897 surveys were administered in total. "Other" includes a small number of surveys that were not attempted and respondents outside the sample area. Standard errors in parentheses. * p<0.10 ** p<0.05 *** p<0.01

	Treatment	Control	Difference	
	100% Debt Relief	25% Debt Relief	Coefficient	SE
Surveyed	55.10%	55.48%	-0.00375	[0.01360]
Deceased	11.86%	10.26%	0.0160*	[0.00859]
Migrated	7.23%	7.99%	-0.00755	[0.00720]
Refused	3.16%	3.67%	-0.00510	[0.00492]
Not located	9.38%	10.43%	-0.0105	[0.00811]
Failed to administer	5.00%	4.50%	0.00500	[0.00582]
Other	8.27%	7.68%	0.00592	[0.00741]

Table 4: Regression Discontinuity, First Stage

This table presents evidence on the program-induced discontinuity in debt relief between treatment and control group based on bank data and placebo tests using eligible amount (as opposed to implemented debt relief). *Debt Relief* refers to the net amount of debt waived as a result of the program. *Eligible Amount, Total* (rupees) refers to the total ex-ante overdue balance to which the program criteria were applied and is the sum of *Eligible Amount, Principal* and *Eligible Amount, Interest*. Coefficient estimates are obtained from regressions of each outcome on treatment status. Robust standard errors in parentheses are clustered by bank and district. * p<0.10 ** p<0.05 *** p<0.01.

	Coefficient	*SE*	*N*
Implemented Debt Relief			
All Banks	**37,156*** **	(1,858)	2,897
Commercial Banks	**44,037*** **	(3,455)	1,475
Cooperative Banks	**34,339*** **	(1,925)	1,422
Eligible Amount, Total			
All Banks	-2,939.7	(3,843)	2,442
Commercial Banks	-5,599.5	(5,603)	1,348
Cooperative Banks	4,186.5	(4,285)	1,094
Eligible Amount, Principal			
All Banks	-4,517.2	(3,225)	2,443
Commercial Banks	-4,850.4	(4,996)	1,349
Cooperative Banks	1,432.4	(2,483)	1,094
Eligible Amount, Interest			
All Banks	1,446.8	(1,442)	2,418
Commercial Banks	-1,414.8	(1,509)	1,325
Cooperative Banks	3,375.8	(2,403)	1,093

Table 5: Total Debt

This table presents estimates of the self-reported debt level of beneficiary households. Each column reports results from a separate regression. The dependent variable in Panel A is the self-reported total debt level of surveyed households before the program in columns (1) and (3) and after the program in columns (2) and (4). The dependent variable in Panel B is the Log of self-reported total debt before the program in columns (5) and (7) and after the program in columns (6) and (8). Additional controls include respondent gender and years of education, indicator for type of loan, the log of total land owned pre-program, land audit status, bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A				Panel B			
	Total Debt		Total Debt		Log Total Debt		Log Total Debt	
	Before	After	Before	After	Before	After	Before	After
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% Relief	14.177*	6.434	0.205	-0.057	4.225	-1.617	-0.024	-0.242
	(7.54)	(13.20)	(0.16)	(0.48)	(6.95)	(11.86)	(0.04)	(0.47)
100% Relief ×Balance					5.544	13.399	-0.016	0.208
					(5.02)	(9.66)	(0.02)	(0.18)
Balance					1.683	-3.470	0.020	-0.111
					(6.64)	(6.74)	(0.02)	(0.13)
100% Relief ×Hectares from cutoff	21.111	-61.812	0.191	-0.870	13.309	-30.967	0.033	-0.574
	(52.01)	(48.31)	(0.67)	(1.44)	(45.90)	(46.25)	(0.19)	(1.72)
Hectares from cutoff	43.376	87.076**	0.571	2.514***	19.015	30.717	-0.052	1.716**
	(38.77)	(39.76)	(0.50)	(0.78)	(27.69)	(28.08)	(0.10)	(0.73)
Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,832	2,832	2,832	2,832	2,734	2,734	2,734	2,734
R-SquaredR-Squared	0.112	0.078	0.070	0.149	0.321	0.108	0.888	0.184
Mean of dep. variable treated	67.78	67.46	67.78	67.46	10.39	6.53	10.39	6.53
Mean of dep. variable control	98.09	102.28	98.09	102.28	10.67	7.77	10.39	6.53

Table 6: Sources of Credit

This taple reports estimates of the effect of debt relief on the composition of borrowing. The dependent variables are the self-reported percentages of financing obtained from banks and informal sector lenders, respectively. Each panel distinguishes between percentages of financing obtained from the formal and informal sector before the program, after the program and the change between the two time periods. Bank credit includes loans from commercial and cooperative banks, informal sector credit includes loans from moneylenders, traders and shopkeepers and family and friends. Additional controls include respondent gender and years of education, log total land owned pre-program, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A						Panel B					
	Banks			Informal Sector			Banks			Informal Sector		
	%Before	%After	%Δ	%Before	%After	%Δ	%Before	%After	%Δ	%Before	%After	%Δ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[A] 100% Relief	-2.650	-7.579***	-4.801*	1.318	5.307***	3.928**	-2.775	-7.969***	-5.054*	1.439	5.467***	3.868**
	(1.63)	(2.57)	(2.75)	(1.26)	(1.45)	(1.57)	(1.65)	(2.85)	(2.73)	(1.30)	(1.41)	(1.44)
[B] 100% Relief ×Balance							1.009	5.260**	4.360**	0.220	-1.073	-1.349*
							(1.31)	(2.01)	(1.74)	(0.69)	(0.63)	(0.67)
[C] Balance							-0.322	-3.294***	- 3.013***	-0.208	0.413	0.651
							(0.74)	(1.11)	(0.88)	(0.56)	(0.71)	(0.83)
[D] 100% Relief ×Hectares from cutoff	-18.580*	-5.767	12.171	8.230	15.097**	8.074	-15.750	-2.750	12.367	6.459	14.016*	8.290
	(9.92)	(9.45)	(9.54)	(6.91)	(7.03)	(6.19)	(10.68)	(9.35)	(10.76)	(7.46)	(8.04)	(6.94)
[E] Hectares from cutoff	7.872	-3.005	-10.478	-4.843	-0.766	3.627	6.112	-7.588	-1 3.329*	-3.837	1.214	4.549
	(5.68)	(5.70)	(7.13)	(4.23)	(4.39)	(3.66)	(5.60)	(6.83)	(7.71)	(4.12)	(4.68)	(3.92)
Additional Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test $B + C = 0$	–	–	–	–	–	–	0.36	1.34	0.80	0.00	0.86	1.12
p-value	–	–	–	–	–	–	0.552	0.259	0.379	0.981	0.363	0.230
Observations	2,697	2,692	2,690	2,695	2,690	2,688	2,604	2,599	2,597	2,603	2,597	2,596
R-Squared	0.138	0.186	0.238	0.118	0.115	0.118	0.131	0.197	0.245	0.113	0.119	0.123

Table 7: Change in Sources of Credit

This table reports estimates of the changes in borrowing by source. Each column reports results from a separate regression. The dependent variable in each regression is the change in self-reported percentages of financing obtained from the respective source. The dependent variable is the change in self-reported percentages of financing obtained from each source. Additional controls include respondent gender and years of education, log total land owned pre-program, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A %Δ				Panel B %Δ			
	Bank	Coop Bank	Money-lender	Family & Friends	Bank	Coop Bank	Money-lender	Family & Friends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[A] 100% Relief	0.950	-5.616**	0.459	3.474**	0.529	-5.448**	0.521	3.345**
	(2.87)	(2.71)	(1.38)	(1.45)	(2.97)	(2.54)	(1.41)	(1.49)
[B] 100% Relief × Balance					1.605	2.752**	0.138	-1.491*
					(1.46)	(1.23)	(0.66)	(0.80)
[C] Balance					-1.433	-1.551*	0.014	0.637
					(1.02)	(0.76)	(0.43)	(0.71)
[D] 100% Relief	-7.121	19.442**	1.680	6.415	-5.289	17.718**	1.268	7.013
× Hectares from cutoff	(9.97)	(8.10)	(3.93)	(6.00)	(10.64)	(7.86)	(4.06)	(5.86)
[E] Hectares from cutoff	5.975	-16.357**	-0.073	3.710	1.825	-15.087**	0.038	4.514
	(6.52)	(6.47)	(1.73)	(3.40)	(7.35)	(6.50)	(1.91)	(3.22)
Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test $B + D = 0$	–	–	–	–	0.03	2.55	0.27	1.13
p-value	–	–	–	–	0.873	0.123	0.610	0.299
Observations	2,700	2,693	2,691	2,688	2,607	2,600	2,598	2,596
R-Squared	0.204	0.208	0.027	0.123	0.215	0.206	0.030	0.129

Table 8: Ex-post Financial Access

This table reports evidence on the financial access of beneficiary households after the program. Each column reports results from a separate regression. The dependent variable in column (1) is a dummy indicating whether a household applied for a new loan after the program. The dependent variable in column (2) is a dummy equal to one if a new loan application was successful. The dependent variable in column (3) is the interest rate for new loans and the dependent variable in column (4) is the log approved loan amount for successful applications. Additional controls include bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Ex-post financial access			
	Applied for new loan (1)	New loan approved (2)	Interest rate (3)	Log amount approved (4)
100% Relief	0.090	0.267	0.547	-1.135
	(0.24)	(0.50)	(2.89)	(0.95)
100% Relief				
×Balance	-0.003	0.058	0.212	0.018
	(0.01)	(0.05)	(0.30)	(0.12)
×Hectares from cutoff	-0.119	-0.113	-0.655	0.294
	(0.14)	(0.17)	(1.83)	(0.29)
×Pre-program wealth	0.037*	0.033	-0.175	-0.106
	(0.02)	(0.03)	(0.17)	(0.10)
×Pre-program total debt	-0.008	-0.023	-0.101	0.075
	(0.02)	(0.05)	(0.28)	(0.09)
Balance	0.029**	-0.044	-0.092	-0.019
	(0.01)	(0.03)	(0.20)	(0.08)
Hectares from cutoff	0.094	0.118	-0.292	-0.610*
	(0.10)	(0.13)	(1.45)	(0.31)
Pre-program wealth	0.013	0.005	0.118	0.288***
	(0.02)	(0.02)	(0.15)	(0.05)
Pre-program total debt	0.015	0.003	0.111	0.313***
	(0.02)	(0.04)	(0.27)	(0.11)
Additional Controls	No	No	No	No
Fixed Effects	Yes	Yes	Yes	Yes
Observations	2,830	663	492	554
R-Squared	0.102	0.130	0.179	0.301
Treatment [N=1,181]	376	346	7.82	82,617
%	(31.8)	(92.0)	—	—
Control [N=1,716]	297	264	8.04	93,897
%	(17.3)	(88.8)	—	—

Table 9: Investment and Productivity

This table estimates the effect of debt relief on investment and productivity. The dependent variable in column (1) is the log investment in agricultural inputs, including irrigation, pesticides and fertilizer. The dependent variable in column (2) is the log expenditure on hired labor and the dependent variable incolumn (3) is the log amount of investment in capital goods including tractors, tubewells and agricultural equipment. Each of these variables is calculated as the log average of the first two post debt relief seasons. Columns (4) - (6) report per capita agricultural productivity for each season. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Investment			Production		
	Agricultural Inputs	Hired Labor	Fixed Investments	Per-capita Income	Per-capita Income, Kharif	Per-capita Income, Rabih
	(1)	(2)	(3)	(4)	(5)	(6)
[A] 100% Relief	-0.110**	0.022	0.166	-0.068	-0.018	-0.063
	(0.04)	(0.05)	(0.15)	(0.09)	(0.06)	(0.08)
[B] 100% Relief×Balance	-0.052	-0.023	0.077	-0.023	0.053	0.048
	(0.04)	(0.06)	(0.07)	(0.06)	(0.05)	(0.04)
[C] 100% Relief ×Hectares from cutoff	0.009	0.175	-0.425	-0.223	-0.267	-0.329
	(0.13)	(0.30)	(0.70)	(0.40)	(0.28)	(0.30)
[D] Balance	0.092***	0.085**	-0.060*	0.081*	0.054	0.040
	(0.03)	(0.04)	(0.03)	(0.05)	(0.05)	(0.05)
[E] Hectares from cutoff	-0.223*	-0.035	0.313	-0.031	0.029	-0.002
	(0.11)	(0.20)	(0.38)	(0.26)	(0.14)	(0.20)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,509	2,290	2,783	2,142	2,068	1,728
R-Squared	0.316	0.317	0.037	0.219	0.223	0.267

Table 10: Investment

This table presents estimates of the effect of debt relief on investment. Panel A reports estimates without covariates and interaction terms between treatment and size of outstanding balance. The dependent variables in columns (1)-(3) and (7)-(9) are the Log rupee amount of total investment in agricultural inputs, inputs per household member and inputs per acre, respectively. The dependent variables in columns (4)-(6) and (10)-(12) are indicators taking on a value of one if there was an increase in post-program spending on total inputs, inputs per household member or inputs per acre and zero otherwise. Additional controls in Panel B include respondent gender and years of education, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Investment in Agricultural Inputs											
	Panel A						Panel B					
	Log input	Log input per capita	Log input per acre	Increase in input	Increase in input per capita	Increase in input per acre	Log input	Log input per capita	Log input per acre	Increase in input	Increase in input per capita	Increase in input per acre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[A] 100% Relief	-0.117**	-0.135**	-0.075	-0.077**	-0.077**	-0.078**	-0.146***	-0.167***	-0.063	-0.069**	-0.069**	-0.068**
	(0.05)	(0.05)	(0.13)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.14)	(0.03)	(0.03)	(0.03)
[B] 100% Relief							-0.086*	-0.082*	-0.084	-0.007	-0.007	-0.009
×Balance							(0.04)	(0.05)	(0.06)	(0.02)	(0.02)	(0.02)
[C] Balance							0.053	0.062	0.014	0.023*	0.022*	0.027**
							(0.04)	(0.0 4)	(0.06)	(0.01)	(0.01)	(0.01)
[D] 100% Relief	0.192	0.106	-0.548	-0.134	-0.134	-0.173	0.279*	0.180	-0.753*	-0.169	-0.168	-0.207
×Hectares from cutoff	(0.13)	(0.19)	(0.39)	(0.13)	(0.13)	(0.13)	(0.15)	(0.20)	(0.38)	(0.13)	(0.13)	(0.13)
[E] Hectares from cutoff	0.048	0.052	0.104	-0.032	-0.032	-0.022	-0.086	-0.099	0.153	-0.022	-0.020	-0.008
	(0.08)	(0.11)	(0.37)	(0.09)	(0.09)	(0.09)	(0.10)	(0.14)	(0.38)	(0.09)	(0.09)	(0.09)
Additional Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,547	2,547	2,593	2,832	2,832	2,832	2,460	2,460	2,501	2,723	2,723	2,723
R-Squared	0.137	0.115	0.119	0.099	0.099	0.100	0.176	0.168	0.132	0.103	0.102	0.103

Table 11: PRODUCTIVITY

This table presents estimates of the effect of debt relief on productivity. Panel A presents estimates without covariates and interaction terms between treatment and size of outstanding balance. The dependent variables in columns (1)-(3) and (7)-(9) are the Log rupee amount of total agricultural output, output per household member and output per acre, respectively. The dependent variable in columns (4)-(6) and (10)-(12) are dummy variables taking on a value of one if there was an increase in the value of total agricultural output, output per household member or output per acre and zero otherwise. Additional controls in Panel B include respondent gender and years of education, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Agricultural Productivity											
	PANEL A						PANEL B					
	Log output	Log output per capita	Log output per acre	Increase in output	Increase in output per capita	Increase in output per acre	Log output	Log output per capita	Log output per acre	Increase in output	Increase in output per capita	Increase in output per acre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[A] 100% Relief	-0.037	-0.056	-0.039	0.011	0.011	0.004	-0.098	-0.129	-0.062	0.013	0.014	0.007
	(0.08)	(0.08)	(0.07)	(0.04)	(0.04)	(0.04)	(0.07)	(0.08)	(0.07)	(0.04)	(0.04)	(0.04)
[B] 100% Relief ×Balance							-0.013	-0.002	-0.038	-0.051***	-0.052***	-0.054***
							(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)
[C] Balance							0.003	-0.006	-0.006	0.020	0.020	0.020
							(0.04)	(0.05)	(0.04)	(0.01)	(0.01)	(0.01)
[D] 100% Relief × Hectares from cutoff	0.139	-0.062	-0.198	-0.021	-0.021	-0.076	-0.143	-0.334	-0.116	-0.069	-0.070	-0.090
	(0.33)	(0.37)	(0.24)	(0.13)	(0.13)	(0.14)	(0.30)	(0.32)	(0.26)	(0.13)	(0.13)	(0.14)
[E] Hectares from cutoff	0.169	0.244	-0.078	0.051	0.051	0.067	-0.155	-0.090	-0.039	0.039	0.042	0.043
	(0.21)	(0.26)	(0.17)	(0.11)	(0.11)	(0.10)	(0.16)	(0.20)	(0.15)	(0.09)	(0.09)	(0.09)
Additional Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,750	1,750	1,730	2,832	2,832	2,832	1,692	1,692	1,684	2,703	2,703	2,703
R-Squared	0.117	0.105	0.170	0.066	0.066	0.066	0.372	0.314	0.239	0.074	0.073	0.073

Table 12: Seniority of Claims

This table reports the effect on beliefs about the reputational consequences of default. Each column reports results from a separate regression. The dependent variable in each regression is based on the survey question "If you were to default on a loan from the following source, how likely would this be to tarnish your reputation in the village?" Additional controls include respondent gender, bank×district, interviewer, and month-of-survey fixed effects. Results are robust to re-weighting observations. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	"If you had to, on whom would you default first?"					
	Commercial Bank (1)	Cooperative Bank (2)	Money-lender (3)	Family & Friends (4)	**Formal Sector** (5)	**Informal Sector** (6)
[A] 100% Relief	-0.002	0.018	-0.041	0.008	0.015	-0.033
	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
[B] 100% Relief × Balance	-0.031**	0.033***	-0.014	0.006	0.002	-0.007
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
[C] 100% Relief × Hectares from cutoff	-0.050	-0.017	0.182***	0.026	-0.067	0.209
	(0.07)	(0.05)	(0.06)	(0.18)	(0.06)	(0.16)
[D] 100% Relief × Years on book	-0.002	-0.004	0.004	0.010	-0.006	0.013**
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
[E] Balance	0.018	-0.005	0.024*	-0.028	0.014	-0.004
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)
[F] Hectares from cutoff	0.026	0.020	-0.113***	0.002	0.045	-0.111
	(0.07)	(0.03)	(0.04)	(0.10)	(0.07)	(0.09)
[G] Years on book × Balance	0.000	-0.004	-0.003	0.008*	-0.003	0.005
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
[H] Years on book	0.000	0.000	-0.008**	-0.004	0.000	-0.012**
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Test B+D+G=0	0.42	4.44	0.28	0.24	0.99	0.06
p-value	0.526	0.047	0.603	0.628	0.330	0.809
Observations	2,381	2,381	2,381	2,381	2,381	2,381
R-Squared	0.115	0.127	0.086	0.308	0.199	0.282

Table 13: Repayment and Reputation

This table reports the effect on beliefs about the reputational consequences of default. Each column reports results from a separate regression. The dependent variable in each regression is based on the survey question "If you were to default on a loan from the following source, how likely would this be to tarnish your reputation in the village?" Additional controls include respondent gender, bank×district, interviewer, and month-of-survey fixed effects. Results are robust to re-weighting observations. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Bank (1)	Cooperative Bank (2)	Money- lender (3)	Family & Friends (4)	**Formal** **Sector** (5)	**Informal** **Sector** (6)
[A] 100% Relief	-0.118**	-0.108*	0.060	0.148**	-0.120**	0.104
	(0.05)	(0.06)	(0.07)	(0.07)	(0.05)	(0.07)
[B] 100% Relief × Balance	0.072**	0.065*	0.094*	0.010	0.074**	0.078
	(0.03)	(0.04)	(0.05)	(0.04)	(0.03)	(0.05)
[C] 100% Relief × Hectares from cutoff	0.026	-0.384**	-0.533	-0.437	-0.183	-0.564**
	(0.14)	(0.18)	(0.31)	(0.32)	(0.14)	(0.26)
[D] 100% Relief × Years on book	0.017	0.028**	0.007	-0.026**	0.023**	-0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
[E] Balance	0.006	-0.027	-0.046	-0.056	-0.010	-0.073
	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)
[F] Hectares from cutoff	-0.137	0.162	0.464**	0.369	0.005	0.504***
	(0.12)	(0.13)	(0.20)	(0.22)	(0.12)	(0.17)
[G] Years on book	-0.017	-0.022**	-0.016*	0.006	-0.021*	-0.005
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
[H] Years on book × Balance	-0.016	-0.001	0.002	0.009	-0.010	0.008
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Test B+D+G=0	2.51	0.22	0.68	1.20	0.98	0.10
p-value	0.128	0.646	0.420	0.286	0.333	0.760
Observations	2,732	2,722	2,706	2,723	2,722	2,699
R-Squared	0.363	0.314	0.342	0.239	0.385	0.323

Table 14: Future Access to Credit

This table reports estimates of the effect of debt relief on beliefs about future financial access. Each column reports results from a separate regression. The dependent variable in each regression is based on the survey question "If you were to default on a loan from the following sources, how worried would you be that you will not be able to borrow from this source in the future?" Additional controls include respondent gender and years of education, bank×district, interviewer, and month-of-survey fixed effects. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Bank (1)	Cooperative Bank (2)	Money-lender (3)	Family & Friends (4)	**Formal Sector** (5)	**Informal Sector** (6)
[A] 100% Relief	0.358*	0.369**	0.019	0.119	0.456**	0.156
	(0.21)	(0.14)	(0.19)	(0.18)	(0.19)	(0.18)
[B] 100% Relief × Balance	0.147	0.177	-0.120	-0.141	0.194	-0.100
	(0.14)	(0.12)	(0.16)	(0.09)	(0.19)	(0.08)
[C] 100% Relief × Hectares from cutoff	0.171	-0.375	0.745	0.486	-0.168	0.329
	(0.67)	(0.49)	(0.89)	(0.71)	(0.64)	(0.56)
[D] 100% Relief × Years on book	0.017	0.028**	0.007	-0.026**	0.023**	-0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
[E] Balance	0.201	0.039	0.072	-0.166	0.060	-0.111
	(0.12)	(0.13)	(0.22)	(0.12)	(0.15)	(0.10)
[F] Hectares from cutoff	0.237	0.480	0.357	-0.031	0.581	0.201
	(0.52)	(0.34)	(0.54)	(0.63)	(0.48)	(0.40)
[G] Years on book	0.039	0.054**	-0.057	0.020	0.059	0.012
	(0.03)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
[H] Years on book × Balance	-0.063**	-0.056***	0.056*	-0.023	-0.064**	-0.007
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,356	2,356	2,321	2,346	1,494	1,469
R-Squared	0.404	0.371	0.372	0.284	0.436	0.374

A Appendix Tables

Table A.1: Summary Statistics for Bank Data within Sample Frame

This table presents summary statistics for all qualifying accounts in the sample frame based on administrative data from participating banks.

	N	Mean	StDev	Min	Max
Principal overdue as of December 31, 2007	5,514	40,627	43,351	0	830,000
Interest overdue as of December 31, 2007	5,414	12,595	17,009	0	319,810
Total overdue as of December 31, 2007	5,524	52,915	48,538	0	882,806
Landholding (hectares)	5,554	1.97	0.29	1.50	2.52
Landholding (acres)	5,554	4.86	0.71	3.71	6.23
Eligible debt relief	5,554	33,498	36,823	0	751,594
a.) For 100% waivers	3,263	46,489	42,109	0	751,594
b.) For 25% relief	2,291	14,995	13,389	11	152,903

Table A.2: Balance Checks, Pre-Program Observables

This table reports checks for the continuity of pre-program observables and demographic characteristics around the discontinuity induced by the program. Dependent variables are a dummy for crop loan (versus investment credit), year of loan disbursal, overdue balance size (Rupees, logged), total self-reported land ownership (in hectares), total land cultivated in 2007 (in hectares), a dummy for male respondents, respondent age, respondent education (in years), and household size in 2007. Fixed effects include bank×district, interviewer, and month-of-survey. Results robust to re-weighting observations. Standard errors in parentheses, clustered at the bank × district level. * p<0.10 ** p<0.05 *** p<0.01.

	Crop Loan (1)	Year Disbursed (2)	Balance (Log) (3)	Total Land (ha) (4)	Cultivated Land (ha) (5)	Male (6)	Age (7)	Education (Years) (8)	Household Size (9)
100% waiver	0.0247	0.0711	-0.00632	0.0610	-0.0131	-0.0275*	0.390	0.0479	0.106
	(0.0261)	(0.163)	(0.0599)	(0.0839)	(0.0859)	(0.0150)	(0.678)	(0.319)	(0.223)
Hectares from cut-off	-0.0666	0.220	0.388***	0.929***	0.778***	-0.0241	-0.152	0.687	0.567
	(0.0456)	(0.308)	(0.109)	(0.156)	(0.160)	(0.0256)	(1.367)	(0.481)	(0.452)
Constant	0.788***	2004.3***	0.384*	1.855***	1.830***	1.018***	52.51***	10.89***	4.765***
	(0.0369)	(0.0672)	(0.220)	(0.109)	(0.115)	(0.0142)	(3.017)	(1.146)	(0.619)
Observations	2,887	2,519	2,887	2,858	2,835	2,886	2,885	2,827	2,886
Adjusted R^2	0.024	0.011	0.026	0.078	0.074	0.005	0.010	0.040	0.038
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.3: Total Debt, Audited Sample

This table presents estimates of the self-reported debt level of beneficiary households. Each column reports results from a separate regression. The dependent variable in Panel A is the self-reported total debt level of surveyed households before the program in columns (1) and (3) and after the program in columns (2) and (4). The dependent variable in Panel B is the Log of self-reported total debt before the program in columns (5) and (7) and after the program in columns (6) and (8). Additional controls include gender, indicator for type of loan, the log of total land owned pre-program, land audit status, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land documents. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A Total Debt		Panel B Total Debt		Panel C Log Total Debt		Panel C Log Total Debt	
	Before	After	Before	After	Before	After	Before	After
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% Relief	8.798	5.679	0.180	0.047	0.175	-0.651	-0.007	0.021
	(7.01)	(16.86)	(0.16)	(0.63)	(7.24)	(17.01)	(0.06)	(0.67)
100% Relief					9.877	21.011	-0.002	-0.098
×Balance					(7.57)	(14.23)	(0.02)	(0.23)
Balance					-0.626	-2.047	0.001	-0.012
					(8.79)	(8.06)	(0.01)	(0.19)
100% Relief	4.640	-43.329	0.366	-1.961	-9.844	-4.035	0.014	-1.933
×Hectares from cutoff	(47.75)	(81.94)	(0.61)	(2.01)	(43.04)	(60.63)	(0.17)	(2.15)
Hectares from cutoff	42.909	79.497	0.489	2.783**	26.066	13.888	0.011	2.349**
	(33.32)	(56.02)	(0.47)	(1.16)	(27.74)	(29.02)	(0.05)	(1.04)
Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,805	1,805	1,805	1,805	1,742	1,742	1,742	1,742
R-SquaredR-Squared	0.136	0.091	0.086	0.146	0.355	0.137	0.870	0.180
Mean of dep. variable treated	66.13	70.17	66.13	70.17	10.41	6.89	10.41	6.89
Mean of dep. variable control	96.13	106.03	96.13	106.03	10.71	7.98	10.71	7.98

Table A.4: Sources of Credit, Audited Sample

This taple reports estimates of the effect of debt relief on the composition of borrowing. The dependent variables are the self-reported percentages of financing obtained from banks and informal sector lenders, respectively. Each panel distinguishes between percentages of financing obtained from the formal and informal sector before the program, after the program and the change between the two time periods. Bank credit includes loans from commercial and cooperative banks, informal sector credit includes loans from moneylenders, traders and shopkeepers and family and friends. Additional controls include gender, log total land owned pre-program, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land documents. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A						Panel B					
	Banks			Informal Sector			Banks			Informal Sector		
	%Before	%After	%Δ	%Before	%After	%Δ	%Before	%After	%Δ	%Before	%After	%Δ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[A] 100% Relief	-6.153	-4.053	2.460	4.965	8.271*	3.599	-4.428	-4.288	0.528	5.335	9.373**	3.964
	(1.63)	(2.57)	(2.75)	(1.26)	(1.45)	(1.57)	(1.65)	(2.85)	(2.73)	(1.30)	(1.41)	(1.44)
[B] 100% Relief							4.549**	3.332	-1.190	-3.019**	-3.635	-0.566
×Balance							(2.05)	(3.69)	(3.97)	(1.45)	(2.17)	(1.69)
[C] Balance							-2.339	-1.244	1.020	1.365	1.430	0.030
							(1.95)	(2.84)	(2.98)	(1.33)	(1.49)	(1.36)
	(5.07)	(6.79)	(7.06)	(4.05)	(4.05)	(4.49)	(4.89)	(7.89)	(7.33)	(3.67)	(3.95)	(4.48)
[D] 100% Relief	-27.273*	-11.276	16.419	13.826	9.101	-3.215	-29.932*	-15.749	14.630	15.681	20.576*	5.353
×Hectares from cutoff	(14.66)	(19.09)	(20.47)	(10.19)	(11.95)	(11.39)	(15.14)	(21.69)	(23.84)	(11.20)	(10.93)	(12.90)
[E] Hectares from cutoff	9.431	10.052	0.835	-4.641	4.269	9.118	13.805	8.207	-5.457	-5.779	-0.127	5.389
	(12.10)	(17.58)	(13.22)	(8.92)	(11.49)	(8.83)	(13.29)	(23.07)	(17.24)	(9.05)	(9.78)	(7.26)
Additional Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test $B+C=0$	–	–	–	–	–	–	2.39	0.55	0.00	1.41	2.30	0.23
p-value	–	–	–	–	–	–	0.135	0.466	0.954	0.246	0.142	0.639
Observations	2,697	2,692	2,690	2,695	2,690	2,688	2,604	2,599	2,597	2,603	2,597	2,596
R-Squared	0.138	0.186	0.238	0.118	0.115	0.118	0.131	0.197	0.245	0.113	0.119	0.123

Table A.5: Change in Sources of Credit, Audited Sample

This table reports estimates of the changes in borrowing by source. Each column reports results from a separate regression. The dependent variable in each regression is the change in self-reported percentages of financing obtained from the respective source. Additional controls include respondent gender and years of education, log total land owned pre-program, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land records. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A %Δ				Panel B %Δ			
	Bank	Coop Bank	Money-lender	Family & Friends	Bank	Coop Bank	Money-lender	Family & Friends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[A] 100% Relief	1.465	-4.414	1.066	3.070*	0.560	-4.162	1.249	3.354**
	(3.39)	(2.79)	(1.43)	(1.62)	(3.16)	(2.54)	(1.53)	(1.53)
[B] 100% Relief × Balance					0.167	2.742**	-0.035	-1.147
					(2.19)	(1.20)	(0.84)	(1.00)
[C] Balance					-0.339	-1.353*	0.138	0.393
					(1.36)	(0.78)	(0.48)	(0.96)
[D] 100% Relief × Hectares from cutoff	0.240	10.656	-3.417	1.644	-2.243	8.020	-3.949	7.053
	(14.83)	(8.78)	(4.29)	(5.75)	(17.23)	(8.44)	(4.51)	(7.15)
[E] Hectares from cutoff	0.463	-10.163*	3.574	6.423	-3.401	-8.817	3.722	5.474
	(6.62)	(5.61)	(2.76)	(4.07)	(7.78)	(6.06)	(3.25)	(4.45)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test $B + D = 0$	–	–	–	–	0.01	3.77	0.04	0.50
p-value	–	–	–	–	0.912	0.064	0.837	0.487
Observations	1,726	1,723	1,721	1,719	1,667	1,664	1,662	1,661
R-Squared	0.220	0.229	0.049	0.131	0.230	0.227	0.052	0.136

Table A.6: Investment and Productivity, Audited Sample

This table estimates the effect of debt relief on investment and productivity. The dependent variable in column (1) is the log investment in agricultural inputs, including irrigation, pesticides and fertilizer. The dependent variable in column (2) is the log expenditure on hired labor and the dependent variable incolumn (3) is the log amount of investment in capital goods including tractors, tubewells and agricultural equipment. Each of these variables is calculated as the log average of the first two post debt relief seasons. Columns (4) - (6) report per capita agricultural productivity for each season. The sample is restricted to accounts with matching land records. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Investment			Production		
	Agricultural Inputs	Hired Labor	Fixed Investments	Per-capita Income	Per-capita Income, Kharif	Per-capita Income, Rabih
	(1)	(2)	(3)	(4)	(5)	(6)
[A] 100% Relief	-0.110*	0.020	0.151	-0.085	-0.141*	-0.120
	(0.05)	(0.06)	(0.14)	(0.10)	(0.07)	(0.08)
[B] 100% Relief×Balance	-0.077*	-0.015	0.087*	-0.017	-0.003	0.049
	(0.04)	(0.04)	(0.05)	(0.08)	(0.06)	(0.05)
[C] 100% Relief ×Hectares from cutoff	-0.266	0.213	-0.290	-0.265	-0.258	-0.341
	(0.22)	(0.36)	(0.54)	(0.56)	(0.31)	(0.39)
[D] Balance	0.101**	0.103***	-0.015	0.125**	0.118	0.084
	(0.04)	(0.04)	(0.02)	(0.06)	(0.07)	(0.06)
[E] Hectares from cutoff	-0.102	-0.060	0.162	-0.027	-0.139	-0.020
	(0.17)	(0.25)	(0.36)	(0.38)	(0.25)	(0.30)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,602	1,466	1,775	1,359	1,337	1,111
R-Squared	0.318	0.326	0.053	0.227	0.233	0.275

Table A.7: Investment, Audited Sample

This table presents estimates of the effect of debt relief on investment. Panel A reports estimates without covariates and interaction terms between treatment and size of outstanding balance. The dependent variables in columns (1)-(3) and (7)-(9) are the Log rupee amount of total investment in agricultural inputs, inputs per household member and inputs per acre, respectively. The dependent variable in columns (4)-(6) and (10)-(12) are indicators taking on a value of one if there was an increase in post-program spending on total inputs, inputs per household member or inputs per acre and zero otherwise. Additional controls in Panel B include respondent gender and years of education, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land documents. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A						Panel B					
	Log input	Log input per capita	Log input per acre	Increase in input	Increase in input per capita	Increase in input per acre	Log input	Log input per capita	Log input per acre	Increase in input	Increase in input per capita	Increase in input per acre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[A] 100% Relief	-0.136**	-0.171**	-0.003	-0.064	-0.064	-0.059	-0.179**	-0.219***	0.046	-0.056	-0.055	-0.049
	(0.06)	(0.06)	(0.13)	(0.04)	(0.04)	(0.04)	(0.07)	(0.0 8)	(0.13)	(0.04)	(0.04)	(0.04)
[B] 100% Relief							-0.095	-0.066	-0.228**	-0.002	-0.003	-0.005
×Balance							(0.06)	(0.06)	(0.08)	(0.03)	(0.03)	(0.03)
[C] Balance							0.048	0.052	0.078	0.030**	0.029**	0.032**
							(0.05)	(0.07)	(0.05)	(0.01)	(0.01)	(0.01)
[D] 100% Relief	-0.206	-0.282	-1.227*	-0.218	-0.218	-0.268	-0.101	-0.239	-1.426**	-0.218	-0.216	-0.265
×Hectares from cutoff	(0.24)	(0.31)	(0.60)	(0.18)	(0.18)	(0.19)	(0.26)	(0.35)	(0.53)	(0.19)	(0.19)	(0.20)
[E] Hectares from cutoff	0.176	0.166	0.481	0.004	0.004	0.034	0.026	0.017	0.616	-0.000	0.003	0.032
	(0.16)	(0.24)	(0.46)	(0.13)	(0.13)	(0.14)	(0.17)	(0.26)	(0.42)	(0.13)	(0.13)	(0.14)
Additional Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,628	1,628	1,654	1,805	1,805	1,805	1,572	1,572	1,596	1,737	1,737	1,737
R-Squared	0.141	0.127	0.140	0.100	0.100	0.101	0.182	0.178	0.155	0.104	0.102	0.104

Table A.8: Productivity, Audited Sample

This table presents estimates of the effect of debt relief on productivity. Panel A presents estimates without covariates and interaction terms between treatment and size of outstanding balance. The dependent variables in columns (1)-(3) and (7)-(9) are the Log rupee amount of total agricultural output, output per household member and output per acre, respectively. The dependent variable in columns (4)-(6) and (10)-(12) are dummy variables taking on a value of one if there was an increase in the value of total agricultural output, output per household member or output per acre and zero otherwise. Additional controls in Panel B include respondent gender and years of education, log total self-reported debt pre-program, dummy variables for the type of loan and land audit status, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land records. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Panel A						Panel B					
	Log input	Log input per capita	Log input per acre	Increase in input	Increase in input per capita	Increase in input per acre	Log input	Log input per capita	Log input per acre	Increase in input	Increase in input per capita	Increase in input per acre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
[A] 100% Relief	-0.135	-0.163**	-0.096	0.017	0.017	0.010	-0.175	-0.215**	-0.120	0.015	0.017	0.008
	(0.10)	(0.08)	(0.09)	(0.05)	(0.05)	(0.05)	(0.10)	(0.08)	(0.10)	(0.05)	(0.05)	(0.05)
[B] 100% Relief							-0.001	0.016	-0.045	-0.055**	-0.057**	-0.061**
× Balance							(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)
[C] Balance							0.013	0.018	0.018	0.030	0.030	0.030
							(0.05)	(0.05)	(0.04)	(0.02)	(0.02)	(0.02)
[D] 100% Relief	-0.087	-0.179	-0.351	-0.116	-0.116	-0.167	-0.247	-0.385	-0.285	-0.150	-0.149	-0.160
× Hectares from cutoff	(0.35)	(0.43)	(0.37)	(0.15)	(0.15)	(0.15)	(0.34)	(0.40)	(0.35)	(0.16)	(0.17)	(0.16)
[E] Hectares from cutoff	0.166	0.166	-0.031	0.089	0.089	0.110	-0.164	-0.173	0.004	0.070	0.074	0.068
	(0.20)	(0.29)	(0.24)	(0.11)	(0.11)	(0.10)	(0.24)	(0.28)	(0.22)	(0.10)	(0.10)	(0.10)
Additional Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,126	1,126	1,113	1,805	1,805	1,805	1,087	1,087	1,082	1,724	1,724	1,724
R-Squared	0.140	0.124	0.202	0.089	0.089	0.089	0.372	0.319	0.270	0.096	0.094	0.094

Table A.9: Seniority of Claims, Audited Sample

This table reports the effect of debt relief on beneficiaries' perception of the seniority of debt. Each column reports results from a separate regression. The dependent variable in each regression is a dummy variable based on the survey question "If you had a loan of Rs 10,000 from each of the following sources and had to default on *one* of these loans due to a bad harvest, on which loan woud you default first?" The dependent variable takes on a value of one if the respondent ranks the respective source of credit first. Additional controls include respondent gender, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land records. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	"If you had to, on whom would you default first?"					
	Commercial Bank (1)	Cooperative Bank (2)	Money-lender (3)	Family & Friends (4)	**Formal Sector** (5)	**Informal Sector** (6)
[A] 100% Relief	-0.013	0.036	-0.073	-0.030	0.023	-0.102*
	(0.03)	(0.02)	(0.05)	(0.05)	(0.04)	(0.05)
[B] 100% Relief × Balance	-0.020**	0.037***	-0.009	0.004	0.018	-0.005
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
[C] 100% Relief × Hectares from cutoff	-0.138	-0.020	0.082	0.397**	-0.157*	0.479***
	(0.09)	(0.05)	(0.10)	(0.19)	(0.09)	(0.16)
[D] 100% Relief × Years on book	0.000	-0.009*	0.011	0.016**	-0.009	0.027**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
[E] Balance	0.010	-0.007	0.012	-0.046**	0.003	-0.034
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
[F] Hectares from cutoff	0.073	0.017	-0.081	-0.217	0.090	-0.298**
	(0.08)	(0.03)	(0.08)	(0.13)	(0.08)	(0.11)
[G] Years on book × Balance	0.003	-0.005**	-0.002	0.009	-0.002	0.007
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
[H] Years on book	-0.001	0.007	-0.016**	-0.001	0.005	-0.018**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Test B+D+G=0	0.50	11.44	0.01	2.06	4.40	1.75
p-value	0.487	0.003	0.935	0.166	0.048	0.200
Observations	1,508	1,508	1,508	1,508	1,508	1,508
R-Squared	0.121	0.155	0.116	0.345	0.208	0.313

Table A.10: Repayment and Reputation, Audited Sample

This table reports the effect on beliefs about the reputational consequences of default. Each column reports results from a separate regression. The dependent variable in each regression is based on the survey question "If you were to default on a loan from the following source, how likely would this be to tarnish your reputation in the village?" Additional controls include respondent gender, bank×district, interviewer, and month-of-survey fixed effects. Results are robust to re-weighting observations. The sample is restricted to accounts with matching land records. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Bank	Cooperative Bank	Money-lender	Family & Friends	**Formal Sector**	**Informal Sector**
	(1)	(2)	(3)	(4)	(5)	(6)
[A] 100% Relief	-0.022	0.006	0.139	0.224**	-0.009	0.170*
	(0.07)	(0.11)	(0.10)	(0.10)	(0.07)	(0.10)
[B] 100% Relief × Balance	0.063	0.033	0.121	0.005	0.052	0.095
	(0.05)	(0.06)	(0.07)	(0.08)	(0.05)	(0.08)
[C] 100% Relief × Hectares from cutoff	-0.232	-0.392	-0.827**	-0.577*	-0.330	-0.780**
	(0.23)	(0.23)	(0.35)	(0.32)	(0.20)	(0.30)
[D] 100% Relief × Years on book	0.005	0.035**	0.011	-0.018	0.020	0.005
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
[E] Balance	-0.029	-0.061	-0.066	-0.078	-0.047	-0.112
	(0.06)	(0.08)	(0.08)	(0.07)	(0.06)	(0.07)
[F] Hectares from cutoff	0.082	0.427*	0.764**	0.583**	0.261	0.784***
	(0.16)	(0.21)	(0.28)	(0.27)	(0.17)	(0.25)
[G] Years on book	-0.004	-0.023	-0.015	-0.000	-0.014	-0.007
	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
[H] Years on book × Balance	-0.012	0.007	0.002	0.009	-0.003	0.011
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Test B+D+G=0	0.22	0.12	0.63	1.35	0.00	0.01
p-value	0.640	0.736	0.436	0.259	0.97	0.912
Observations	1,506	1,503	1,495	1,500	1,503	1,491
R-Squared	0.458	0.389	0.354	0.344	0.465	0.370

Table A.11: Future Access to Credit, Audited Sample

This table reports estimates of the effect of debt relief on beliefs about future financial access. Each column reports results from a separate regression. The dependent variable in each regression is based on the survey question “If you were to default on a loan from the following sources, how worried would you be that you will not be able to borrow from this source in the future?” Additional controls include respondent gender and years of education, bank×district, interviewer, and month-of-survey fixed effects. The sample is restricted to accounts with matching land documents. Standard errors in parentheses are clustered at the bank×district level. * p<0.10 ** p<0.05 *** p<0.01.

	Bank (1)	Cooperative Bank (2)	Money-lender (3)	Family & Friends (4)	**Formal Sector** (5)	**Informal Sector** (6)
[A] 100% Relief	0.518**	0.365*	0.305	0.232	0.456**	0.156
	(0.23)	(0.20)	(0.31)	(0.31)	(0.19)	(0.18)
[B] 100% Relief × Balance	0.152	0.209	-0.147	-0.206	0.194	-0.100
	(0.20)	(0.19)	(0.16)	(0.12)	(0.19)	(0.08)
[C] 100% Relief × Hectares from cutoff	0.166	-0.555	0.220	0.477	-0.168	0.329
	(0.87)	(0.72)	(1.27)	(0.76)	(0.64)	(0.56)
[D] 100% Relief × Years on book	-0.086**	-0.040	0.075	-0.049	-0.064**	-0.007
	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)
[E] Balance	0.151	-0.054	0.178	-0.175	0.060	-0.111
	(0.15)	(0.17)	(0.23)	(0.15)	(0.15)	(0.10)
[F] Hectares from cutoff	0.432	0.731	1.035	-0.107	0.581	0.201
	(0.65)	(0.45)	(0.75)	(0.75)	(0.48)	(0.40)
[G] Years on book	0.070	0.048*	-0.064*	0.045	0.059	0.012
	(0.05)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)
[H] Years on book × Balance	-0.022	-0.025	-0.013	0.061**	-0.024	0.038**
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,497	1,496	1,476	1,490	1,494	1,469
R-Squared	0.412	0.380	0.406	0.291	0.436	0.374

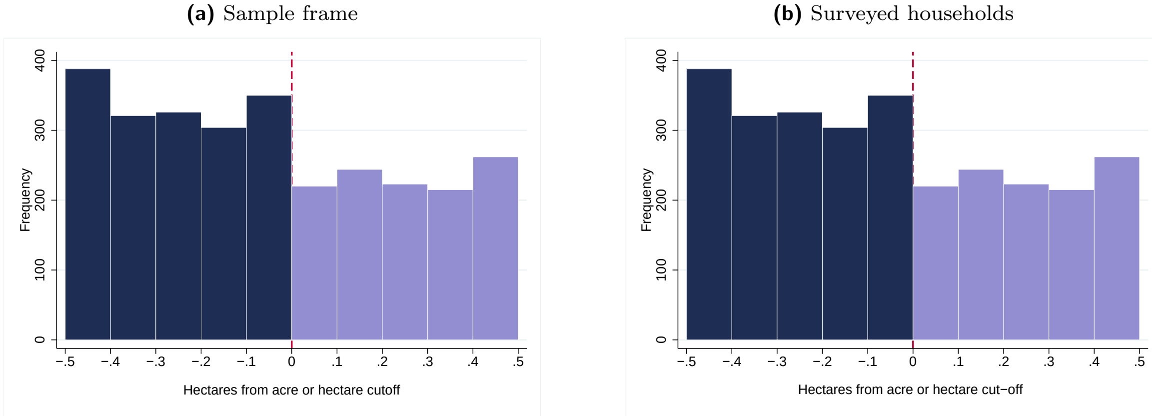
B Integrity of the Assignment Variable

The RD identification strategy we pursue in this paper relies on the assumption that there was no manipulation of the forcing variable, which would make selection to either side of the discontinuity non-random (as discussed in, e.g. McCrary 2008). This appendix therefore, presents several tests verifying the integrity of the forcing variable, in this case land pledged as collateral when the loan was originated.

Figure B.1 plots the density of the running variable according to bank records, for all surveyed farmers within bins of 0.1 hectares. The plots indicate that both for the entire sample frame and the sample of surveyed households, there is notable bunching at the cutoff but also –to a lesser extent– around 1.5 and 2.5 hectares. When we plot the distribution separately for commercial and cooperative banks (Figure B.2), we see that this bunching is driven largely by the subsample of cooperative accounts, where there is a large and spike in density precisely at the cut-off for debt relief qualification. McCrary's (2008) test for discontinuity in the forcing variable correspondingly fails to reject the presence of a discontinuity with $p < 0.01$. The McCrary test also fails to reject discontinuities at 4 and 6 acres, suggesting that bunching at whole numbers could be a part of the explanation. However, the size of the spike at 5 acres is large enough to require an additional explanation.[21] Note that the McCrary test cannot reject continuity in the forcing variable once observations exactly at the cutoff are dropped. The question then becomes: why are there so many observations exactly at the cutoff?

Figure B.1: Density of the forcing variable

(a) Sample frame **(b)** Surveyed households



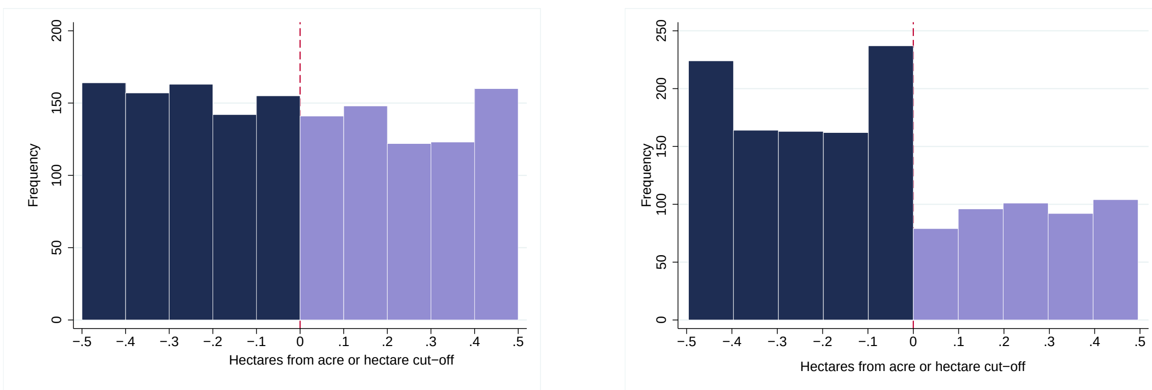
[21] As it turns out, a large group of accounts exactly at the cutoff is concentrated in one village, Mitli, in Anand district. In this village, a government land distribution scheme issued exactly five-acre plots of land to a large number of farmers, many of whom ended up in our sample. These plots were issued to 100-200 farmers 25 years earlier. Because all of these farmers were below the poverty line at the time and were given high-salinity land not well suited to agriculture, they represent an exceptionally poor sub-sample. To ensure that the residents of Mitli do not bias the analysis, we include a Mitli fixed effect as part of the "other controls" throughout the analysis. While not reported, key results are robust to inclusion of a more sweeping fixed effect for all observations located right at the cutoff.

In order to gauge the extent of potential manipulation –and provide for the possibility of a robustness check using a manipulation-free sub-sample– we audited the official land records of surveyed households, taking advantage of the state of Gujarat's electronic repository of land records. In Gujarat state, all official landholdings are recorded in the centralized electronic *e-Dhara* system. Manipulation of *e-Dhara* records is considered highly unlikely. Electronic land records are centrally administered by an authority separate from the institutions keeping land records at the village level. There are several additional checks against manipulation and any changes in landholding status have to be cleared and verified by independent authorities. In order to audit the landholding numbers reported by both the bank and survey respondents, we obtained official copies of the land records of 2,040 of 2,897 survey respondents.

Figure B.2: Density of forcing variable by bank type

(a) Commercial banks

(b) Cooperative banks



We discovered several legitimate reasons for these landholding records to differ from the landholding numbers reported by the banks. First, many banks accepted partial mortgages: to qualify for some loans, farmers were allowed to mortgage only a portion of their land. In these cases, the bank-reported landholding is less than the total land held by the farmers, and the smaller landholding amount will have been used to determine program qualification. This does not constitute manipulation, and does not affect the validity of the RD approach.

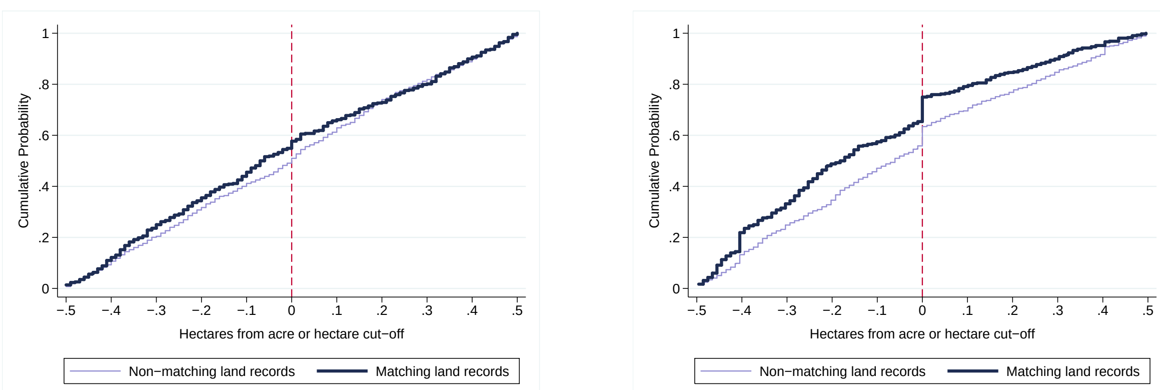
Second, in some cases loans considered the landholdings of multiple individuals. Most frequently, land held by multiple members of the same extended household is pooled in order to qualify for a larger loan. In many cases, the loan was recorded as having a single beneficiary, and the total landholding was listed –even though the beneficiary did not himself or herself own all of the listed land. In these cases, the bank-reported landholding is greater than the total land held by the farmers. This also is considered legitimate and should not violate the fundamental RD assumption. Third, rounding and conversion errors were common, as landholding can be recorded in a variety of complex and regionspecific units. Since official landholding documents almost never reported landholding in the same units as banks, there were nearly always opportunities for rounding and conversion errors.

In assessing whether an official landholding record matches the corresponding bank report, we allow a ± 5% margin for error. In addition, since landholding documents sometimes report distinct plots of land, we allow for either total-land or partial-land matches: if any combination of listed plots adds up to the size reported by the bank, within ± 5%, then we consider it a match. This match protocol retains considerable power, and both excluding partial-land matches and using a ± 1% margin of error does not dramatically affect the match rate.

With landholding documents for 71% of the surveyed households, the match rate is 41.4%. Of the cases that fail to match, 83.5% fail to match because the total official landholding is too small to match with the bank report. These appear to be cases where multiple landholdings were pooled, or cases where land was misreported on the high side in order to qualify for a larger loan. In either case, note that this works *against* debt relief qualification: given that qualification depended on landholding being below a certain cutoff, over-reporting land makes qualification for debt relief *less* likely, thus identifying these accounts as cases where we can rule out manipulation of land records in order to qualify for relief.

Figure B.3: Comparison of land distributions by audit result

(a) Commercial banks **(b)** Cooperative banks



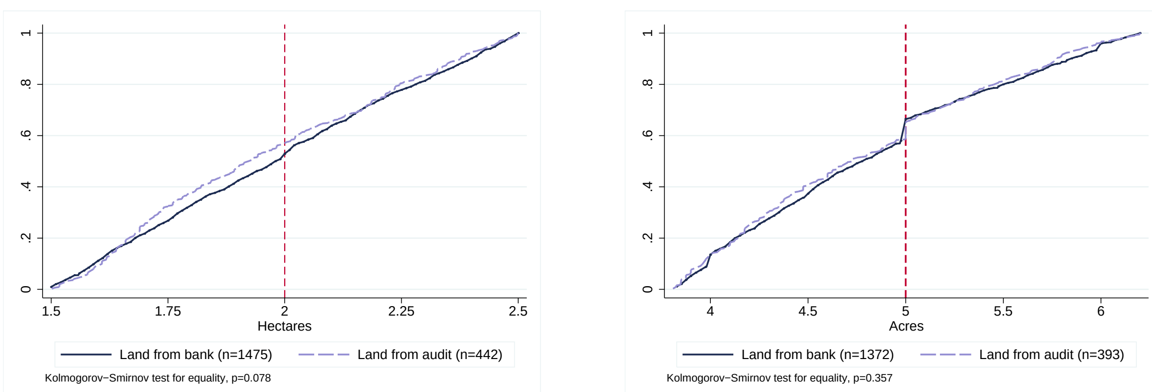
As an additional test, Figure B.3 plots the empirical cumulative distribution functions for commercial and cooperative landholdings, separately for matching and non-matching accounts. If the observed spikes in the land distribution were indeed due to manipulation of land records, rather than e.g. rounding, we would expect to see no evidence of bunching for the distribution of audit matches. For commercial accounts, matching and non-matching land appear to follow very similar distributions. To a slight extent, matching land appears more heavily concentrated at the low end of the distribution. This pattern is similar but more pronounced for the cooperative landholding distributions shown in the right panel. Note, however, that the same spike at 5 acres is equally evident in both the matching and non-matching distributions. Taken together, this suggests bunching arising from rounding around full numbers, rather than manipulation of the running variable.

As can be seen from Figure B.3, the higher concentration of matching land on the low end of the distribution is a combination of two factors: a slightly higher audit rate for smaller landholdings (i.e., a higher propensity to secure the official land documents) and a slightly higher propensity for land documents to match, once secured. Note that both the audit rate and the match rate are markedly higher just to the left of the cut-off than to the right. This is precisely the opposite of what should happen in the presence of corruption at the cut-off: we should be less likely to locate official documents for corrupt farmers,[22] and corrupt land should match at much lower rates.

Figure B.4: Comparison of landholding cdf by source of land data

(a) Land cdf by source, commercial banks

(b) Land cdf by source, cooperative banks



Finally, Figure B.4 plots the bank-reported and audit-derived landholding distributions, for roughly the 4 to 6 acre range of landholdings. By ignoring whether land matches or not, this allows a comparison of the raw land distributions, as considered from bank and government sources. The distributions are visually indistinguishable, and the Kolmogorov-Smirnov test fails to reject equality with $p = 0.357$. This suggests that the bank-reported distribution is almost certainly a case of natural bunching, rather than the result of deliberate bank or farmer manipulation.

[22] In order for us to locate official land documents, farmers had to reveal their unique farmer ID numbers, explicitly for this purpose. In fact, the higher audit rate below the cut-off might have resulted from greater investigator effort to audit those farmers who actually received relief.