What Does Debt Relief Do for Development? Evidence from India's Bailout Program for Highly-Indebted Rural Households

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Abstract

This paper studies the impact of a large debt relief program, intended to attenuate investment constraints among highly-indebted households in rural India. I isolate the causal effect of bankruptcy-like debt relief settlements using a natural experiment arising from India's Debt Relief Program for Small and Marginal Farmers –one of the largest debt relief initiatives in history. I find that debt relief has a persistent effect on the level of household debt, but does not increase investment and productivity as predicted by theories of debt overhang. Instead, the anticipation of future credit constraints leads to a greater reliance on informal financing, lower investment and a decline in productivity among bailout recipients. The results suggest that one-time settlements may be insufficient to incentivize new investment, but can have significant real effects through their impact on borrower expectations.

Keywords: Household finance, debt and poverty traps, credit constraints, investment

JEL Classification: G18, O13, O18, Q14, Q18

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1 Introduction

Extreme levels of household debt are prevalent throughout the developing world. This is especially true in rural economies, where households face highly volatile incomes, but often lack access to basic financial instruments that could mitigate the impact of recurring income shocks (Townsend 2006, Karlan and Morduch 2009).[1] The potentially far-reaching aggregate implications of extreme household indebtedness have motivated a range of large debt relief initiatives for low-income households in developing countries. Some recent examples include a US$ 2.9 billion bailout for farmers in Thailand, and the rescheduling of more than US$ 10 billion of agricultural debt in Brazil.[2]

While the benefit of debt relief to individual households can be substantial, the merit of unconditional bailouts as a tool to improve household welfare and productivity remains highly controversial.[3] Proponents of debt relief argue that extreme levels of indebtedness distort investment and production decisions, so that debt relief holds the promise of improving the productivity of recipient households. Critics of debt relief, on the other hand, worry that it is difficult to "write off loans without writing off a culture of prudent borrowing and repayment" (The Economist).[4] They question that one-time settlements can have a lasting impact, and argue that such bailouts may instead aggravate credit rationing by inducing moral hazard and stigmatizing borrowers in default.[5] While both views can appeal to a foundation in economic theory, there currently exists little empirical evidence on how indebtedness, and hence debt relief, affect economic decisions at the household level.

This paper provides direct evidence on the impact of debt relief on the economic decisions of recipient households, using a natural experiment among Indian households that benefited from a large nationwide debt relief program. In particular, I test whether –similar to a bankruptcy

[1]For evidence on barriers to household risk-management see also Cole et al. (2011) and Giné et al. (2008)

[2]USDA Economic Research Service Briefings, http://www.ers.usda.gov

[3]See also Robert (2012) who uses the natural experiment and survey data underlying this study to test the impact of wealth on subjective well-being.

[4]"Waiving, not drowning: India writes off farm loans. Has it also written off the rural credit culture?" *The Economist*, July 3, 2008.

[5]For the theory of moral hazard and credit rationing see Stiglitz and Weiss (1981) and Jaffee and Russell (1976). Karlan and Zinman (2009) provide empirical evidence on moral hazard and adverse selection in an emerging credit market.

settlement– the bailout acted to restore access to formal credit among beneficiary households and led to economically meaningful changes in investment and productivity. My analysis takes advantage of an experiment generated by India's *Debt Waiver and Debt Relief Scheme for Small and Marginal Farmers*, one of the largest household debt relief programs in history. Enacted by the Government of India in June 2008, the program waived Rs 715 billion (US$14.4 billion) of agricultural debt issued by commercial and cooperative banks between 1997 and 2007. The program covered *all* agricultural loans in India that were overdue at the end of 2007 and remained in default until February 28, 2008. The volume of debt relief granted under the program corresponded to 1.6% of India's GDP, and the program affected approximately 45 million households across the country (Government of India, 2008).

I identify the causal impact of debt relief by exploiting a unique feature of the program that induces quasi-random variation in the eligibility for debt relief: in contrast to previous debt relief initiatives, eligibility for the program depended on the amount of land pledged as collateral at the time a loan was taken out: households that had pledged *less* than 2 hectares (4.95 acres) of land at the time their loan was originated qualified for 100% unconditional debt relief, while households that had pledged *more* than 2 hectares qualified for only 25% of debt relief, conditional on settling the remaining 75% of their outstanding balance. Using this feature of the program, I estimate the causal impact of debt relief by employing a regression discontinuity design comparing economic outcomes across households in the close vicinity of the eligibility cutoff established by the program.

The results suggest that resolving investment disincentives arising from high levels of inherited debt through a debt relief settlement is insufficient to encourage significant new investment: debt relief leads to a persistent reduction in overall household debt, but does not increase investment and productivity as predicted by theories of debt overhang. The evidence suggests that this effect is largely due to program's impact on borrower expectations about future lending relationships and access to institutional credit. Specifically, I show that the program was largely unsuccessful at reintegrating households into formal credit relationships. The results suggest that one-time settlements can have important real effects through their impact on borrower expectations, but may be insufficient to incentivize new investment unless they can credibly attenuate uncertainty about the stability of future lending relationships.

The analysis yields three sets of results that suggest a cautionary tale of debt relief. First, debt relief, by and large, failed to reintegrate recipient households into formal lending relationships. Despite the fact that banks were required to make bailout recipients eligible for fresh loans, a large fraction of beneficiary households do not use their cleared collateral to obtain new formal sector loans. This leads to a persistent shift in the composition of household debt *away* from formal sector borrowing and a relative greater reliance on informal credit. Households that had all of their debt cancelled borrowed, on average, 6 percentage points less from formal sector sources than households in the control group. Evidence on loan applications after the program suggests that this result is unlikely to be explained by changes in the supply of credit.

Second, contrary to the implications of a standard model of debt overhang, I find that debt relief does not increase the investment or productivity of beneficiary households. Debt relief beneficiaries, to the contrary, reduce investment in agricultural inputs, potentially as a direct result of the shift towards more expensive sources of financing. This is also reflected in the post-program productivity of debt relief households, which declines in absolute terms and lags up to 14 percentage points behind the productivity of households in the control group. These findings contradict the standard theory of debt overhang, and it is necessary to look to the impact of debt relief on expectations to reconcile these results with a rational model of the household's investment decision.

Third, I find that debt relief strongly affects households' expectations about the reputational consequences of default and leads beneficiary households to anticipate future credit constraints. Debt relief beneficiaries are less concerned about the reputational consequences of default, which provides prima facie evidence in support of a link between debt relief and subsequent moral hazard. More importantly, however, debt relief recipients are significantly more concerned that debt relief will result in borrowing constraints in the future. This might occur due to the stigma of being singled out as a defaulter or through the termiation of ongoing lending relationships as a result of debt relief. This shift in expectations among recipients of debt relief also provides a straightforward explanation of the decline in investment expenditures among debt relief households: households appear to perceive debt relief as a short-term benefit, likely to make it more difficult to access institutional credit in the future, and suggest that the anticipation of borrowing constraints leads households to rationally reduce investment in the present period.

The results presented in this paper connect the literature on debt- and poverty traps (Banerjee and Newman, 1993; Mookherjee and Ray, 2003) to the literature on government interventions in credit markets. Economic theory suggests two channels through which extreme levels of household debt may affect household welfare and productivity. First, poverty trap models 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 (Banerjee, 2000). Second, theories of debt overhang and risk-shifting (Jensen and Meckling, 1976; Myers, 1977) emphasize the disincentive effects of extreme indebtedness and suggest that indebtedness affects both the level and risk-profile of investment: if a household's debt burden is sufficiently high that the proceeds of its investment go largely towards debt service, the household may pass up profitable (positive NPV) investment opportunities because it does not reap the full return of its investments.[6]

The literature on 'social banking' suggests that government interventions in credit markets can have a positive impact on household welfare if they provide insurance against otherwise uninsurable events (Bolton and Rosenthal, 2002), or improve access to basic financial services among marginal borrowers (Burgess and Pande, 2005; Burgess, Wong and Pande, 2005). Results from the same literature also caution, however, that targeted credit market interventions are vulnerable to political capture and may lead to substantial market distortions in the long run (Dinç, 2005; Cole, 2009). In the case of debt relief, an important concern is that unconditional bailouts may additionally induce moral hazard by altering expectations about the enforcement of debt contracts and the reputational consequences of default.

While there currently exists no direct evidence on the effect of debt relief on household be-

[6]Similarly, highly-indebted households may undertake excessively risky investments, since much of the downside-risk is borne by debt holders. Both channels would imply greater investment and productivity as a result of debt relief.

havior in developing countries, this paper also relates to the literature on personal bankruptcy in developed credit markets which, analogous to debt relief in low-income countries, aim to provide a "fresh start" to debtors in distress (Domowitz and Sartain, 1999; Campbell, 2006).[7] Han and Li (2008) find that the majority of households filing for personal bankruptcy in the United States experience renewed repayment difficulties and accumulate less wealth, even many years after a bankruptcy settlement. Gropp, Scholz and White (1997) show that lenient bankruptcy provisions affect incentives for the ex-post supply of credit, effectively worsening the financial access of poorer borrowers.[8]

Since, analogous to many households in developing countries, all households in the sample are producers as well as consumers (Banerjee and Duflo, 2007), this paper also relates to the literature on credit and investment constraints among low-income households and micro-entrepreneurs. Dupas and Robinson (2009) study the impact of access to savings accounts on microenterprise development, and provide evidence of significant barriers to saving that constrain productive investment. de Mel et al. (2008) provide unconditional cash transfers to micro-entrepreneurs in Sri Lanka and find similar evidence of significant investment constraints and high returns to capital. In an evaluation of a program that extended access to microfinance in India, Banerjee et al. (2010) find a positive but economically small impact on business creation. Karlan and Zinman (2010) study the effect of randomized access to credit on microenterprise investment in the Philippines and find no impact. Similarly, Kaboski and Townsend (2011) study a field experiment expanding access to microcredit in Thailand and find strong consumption effects but no impact on investment.

The present study differs from the literature on investment constraints in two important ways. First, in contrast to studies analyzing a credit expansion, I focus on a population of existing rather than new bank clients, who are excluded from bank credit due to high levels of pre-existing debt. Second, unlike programs expanding access to credit, debt relief

[7] An important difference between debt relief and 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.

[8] See also Djankov, McLiesh and Shleifer (2007) who show that the protection of creditor rights has important effects on *ex-ante* incentives for the provision of private credit. Visaria (2009) and von Lilienfeld-Toal, Mookherjee and Visaria (2012) provide empirical evidence on the effect of strengthening creditor rights using the introduction of debt recovery tribunals in India.

(like a bankruptcy settlement) reduced debt on the books, but did not automatically provide beneficiaries with improved liquidity.

The remainder of the paper is structured as follows. Section two provides details on the debt relief program and eligibility criteria. Section three presents a simple model of the household's investment decisio to motivate the empirical analysis. In section four, I outline the identification strategy. Section five describes the dataset and household survey. Section six presents the results and Section seven concludes.

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

The setting for my investigation into debt relief and household behavior is a natural experiment generated by India's 2008 *Debt Waiver and Debt Relief Scheme for Small and Marginal Farmers*, one of the largest debt relief programs in history. Enacted by the Government of India in June 2008, the program affected between 36 and 40 million farmers across India and covered outstanding loans worth approximately Rs 715 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 of Maharashtra, where high indebtedness among low-income farm households was a frequently cited factor. As a sizable transfer to India's important agricultural sector ahead of national elections, the program may have also served more direct political purposes.[9] Evidence of stagnating agricultural yields and economic theories of debt overhang (Myers, 1977; Ghosh, Mookherjee and Ray, 2000) and investment-driven poverty traps (Banerjee and Newman, 1993; Banerjee, 2000) provided an additional motivation, with the expectation being that a reduction in household debt would improve the efficiency of agricultural investment. Because commercial banks and cooperatives were refinanced through the central bank, the program was also popular with lenders, and may have helped to revive some financially troubled institutions. An important concern, however, even as the bailout program was being drafted, was its potentially adverse impact on borrower behavior and incentives for timely repayment.

The program, as announced in the Indian Finance Minister's budget speech on 29 February

[9] In 2009, Indian agriculture accounted for 17.12% of GDP and 66% of total employment (World Bank, 2012).

2008,[10] applied to all agricultural debt issued by commercial and cooperative banks between 1997 and 2007. This included all crop loans, investment loans for direct agricultural purposes or purposes allied to agriculture, and loans rescheduled under previous programs. Debt from lenders other than banks or credit cooperatives, and loans for non-agricultural purposes were not covered by the program. To qualify for debt relief, a loan had to be issued before December 31, 2007 (well prior to the program announcement) and remain overdue as of February 28, 2008.

In contrast to previous debt relief initiatives, eligibility for the program depended on the amount of land a borrower had pledged as collateral at the time the loan was taken out, typically several years prior to the program.[11] Borrowers who had pledged two or fewer hectares of total land qualified for unconditional 100% debt relief, while borrowers who had pledged more than two hectares of land qualified for 25% conditional debt relief granted upon the repayment of their remaining balance. An exception to this cutoff rule applied in districts that had been classified as "drought affected", where farmers above two hectares qualified for either 25% conditional debt relief or a direct disbursement of Rs 20,000 (US$ 997), whichever amount was greater. For agricultural loans that were not tied to the amount of land pledged, farmers with loans of Rs 50,000 (US$ 1,002) and under qualified for full debt relief, while farmers with larger loans were eligible for conditional debt relief. The sample in this paper includes only crop and investment loans, for which debt relief was based on land holding. All surveyed households resided in non-drought affected districts, so that the analysis is unaffected by these exceptions to the two hectare cutoff rule. Table 1 summarizes the program eligibility rules by district type and landholding category.

Implementation of the program began on June 30, 2008, with full waivers being granted immediately, and 25% conditional relief being granted upon repayment of a borrower's remaining balance, with an initial deadline of June 30, 2009. This deadline was eventually extended by one year in order to accommodate those who had trouble repaying their remaining balance.

The program had several features designed to maximize transparency and avert manipu-

[10] See http://indiabudget.nic.in/ub2008-09/bs/speecha.htm

[11] The last nationwide debt relief program in India occurred in 1986 and based on the *amount* of outstanding debt. The survey districts did not experience any regional or national debt relief programs since. The sample excludes previously restructured loans, since I do not observe their original terms and subsequent mmodifications.

lation. Each bank branch in the country was required to post a public list of all debt relief beneficiaries among its clients, along with loan and landholding details as a transparency measure. In addition to the public posting of borrower lists, accounts qualifying for debt relief underwent several rounds of audit and verification to reduce the risk of fraud. First, beneficiary lists at each bank branch had to be confirmed in several rounds of banks' annual internal audits. A number of branches were then audited by controllers from the *Reserve Bank of India*. Finally, borrower lists underwent an independent audit by the *Comptroller and Auditor General of India*. The program was widely publicized in national and regional media to ensure that borrowers were aware of their entitlements under the program. Borrowers qualifying for debt relief were notified by their bank, received a written confirmation of debt relief, and had their collateral cleared on official land documents.

3 A Simple Model of Household Debt and Investment

To motivate the empirical analysis, this section develops a simple model of household debt and investment. The model describes the household's investment decision using a simple two-period setting[12] and generates testable implications on the effect of indebtedness on investment and productivity. To focus on the investment aspect of the problem, the model abstracts from any bargaining and insurance considerations and assumes that the household maximizes the intertemporal utility

$$u = u(c_1) + \gamma \mathrm{E} u(c_2) \tag{1}$$

where E is the expectations operator and γ is the household's discount factor for consumption in period two. To simplify the exposition, I assume without loss of generalization, that the household is risk-neutral and does not discount second-period consumption, so that $\gamma = 1$.

In period one, the household starts out with a stock of debt of face value D that comes due in period two, and liquid assets y_1 which it may consume or invest in a productive activity that generates revenue in period two. The household may choose to invest amount $k \in [0, \overline{k}]$ in

[12] The model is similar to Krugman (1988) and Bulow and Rogoff (1989), who study debt relief in the context of sovereign borrowing. In contrast to this literature, the model abstracts from any bargaining considerations. For an overview of the theoretical and empirical literature on sovereign debt relief see also Eaton (1990).

period one. The production technology available to the household is such, that an investment k in period one yields output $y_2 = \theta f(k)$ in period two, where θ is a stochastic productivity parameter whose distribution is described by a uniform random variable with support $\theta \in \left[\underline{\theta}, \overline{\theta}\right]$ and probability density function $\pi(\theta)$, and $f(\cdot)$ is a concave, twice differentiable production function.

If the household is unable to service its debt, creditors may confiscate a share $s\theta f(k)$ of its revenue in period two, where $s \in [0, 1)$, so that second period consumption is $c_2 = \theta f(k) - min\left[s\theta f(k), D\right]$. An indebted household therefore chooses first-period investment k to optimize

$$u(k) = y_1 - k + \theta f(k) - min\left[s\theta f(k), D\right] \tag{2}$$

where $v(D, k) = min\left[s\theta f(k), D\right]$ is the value of outstanding debt in period two. Note that the household's decision whether to default or not is stochastic and depends on the realization of the productivity shock θ. The household will repay its outstanding debt if $s\theta f(k) < \frac{D}{sf(k)}$, but will default if $s\theta f(k) < D$, so that the expected value of debt can be written as:

$$v(D, k) = \int_{\underline{\theta}}^{\frac{D}{sf(k)}} \theta\pi(\theta)d\theta + D\int_{\frac{D}{sf(k)}}^{\overline{\theta}} \pi(\theta)d\theta \tag{3}$$

where the first righ-hand side term is greater than zero and captures realizations of the productivity parameter θ for which the household is better off repaying its debt, and the second right-hand side term is negative and captures states of the world in which it is preferable for the household to default. Equation (3) illustrates the basic debt-overhang argument. Note that the probability that a borrower will default on its debt depends on its first-period investment k. From the perspective of the household's lenders, this creates a tradeoff between the benefit of debt enforcement and debt restructuring: although banks would like to enforce the debt contract as strictly as possible, this creates disincentives for investment and increases the risk of default. Reducing or restructuring the household's debt burden may therefore be preferable to stricter enforcement.

To examine how a reduction of the household's inherited debt burden D affects the its investment decision, I substitute (3) into (2) and obtain the first order condition:

$$f'(k)\left[1 - s\int_{\underline{\theta}}^{\frac{D}{sf(k)}} \theta\pi(\theta)d\theta\right] = 1 \tag{4}$$

which generates the following testable prediction regarding the effect of indebtedness and debt relief on the household's investment decision in the base case where I assume that debt relief has no simultaneous impact on expectations about future access to credit.[13]

Proposition 1 (Debt relief without borrowing constraints) *Debt relief will increase investment and productivity of households suffering from debt overhang, compared to households that receive no debt relief, since* $\frac{dk}{dD} < 0$. *Proof: See Appendix.* ■

Debt relief may, however, have very different implications if it alters borrower perceptions about future access to credit. Consider the case in which a borrower expects to be credit constrained as the result of debt relief. Without access to credit, first period investment is constrained to $k \in [0, \tilde{k}]$, and the household optimizes (2) subject to $k \leq y_1$. In this case, investment depends on initial wealth and the impact of debt relief on investment is determined by the relationship between the unconstrained investment choice k^* and $\tilde{k}$.

1. If $k^* \leq \tilde{k}$, then debt relief increases investment k, as shown above, until $\tilde{k}$.

2. If $k^* > \tilde{k}$, the household's investment is reduced relative to the base case. The household will want to invest all available resources, i.e. $\tilde{k}$ to maximize expected revenue, but is constrained to choices $k \leq y_1$, so that investment depends on initial wealth.

Thus, in the case where the household plans to borrow to finance investment, debt relief may decrease investment by altering beliefs about future access to credit: without the upper bound $\tilde{k}$ introduced by the debt relief, the household may borrow to invest an amount greater than $\tilde{k}$. Debt relief restricts investment to be no higher than $\tilde{k}$. This generates the following empirical predictions that can be tested against the basic debt-overhang hypothesis:

[13] Using the household's optimal default condition we can also derive the point at which it becomes optimal for lenders to prefer debt relief over enforcement. For a discussion of this tradeoff see Krugman (1988).

Proposition 2 (Debt relief with anticipation of future borrowing constraints) *Debt relief decreases investment if households expect to be credit constrained in the future as a result of debt relief. This reduces investment to $\tilde{k}$ for all households for whom $k^* > \tilde{k}$. Because $\tilde{k} \leq y_1$, investment depends on the household's initial wealth. Proof: See Appendix.* ■

I take these predictions to the data, using the eligibility rules of India's debt relief program for small and marginal farmers as a source of exogenous variation in the level of debt forgiveness. As we shall see, the effect of debt relief on investment and productivity is consistent with the predictions of the model for the case of debt relief with the anticipation of borrowing constaints. While there is no positive effect of debt relief on productivity, the balance of the evidence suggests that debt relief beneficiaries reduced investment expenditures in the expectation that it will become more difficult to obtain institutional credit in the future.

4 Empirical Strategy

I identify the causal effect of debt relief using a regression discontinuity design based on the program eligibility criteria and data from a household survey of 2,897 debt relief beneficiaries. The identification strategy exploits the fact that, unlike previous debt relief initiatives, eligibility for debt relief under the program depended on the amount of land pledged as collateral at the time a loan was disbursed. This creates a discontinuity in the amount of debt relief around the eligibility threshold of two hectares and induces quasi-random variation in debt relief status: households below the two hectare threshold received 100% unconditional debt relief, while those above the cutoff qualified for 25% of relief conditional on settling the remainder of their outstanding balance.

Presuming that banks followed the rules of the debt relief program faithfully, the causal effect of debt relief can be estimated using a *sharp* regression discontinuity design (Imbens and Lemieux, 2008a; Hahn, Todd and Van der Klaauw, 2001) that compares households in the immediate vicinity of the program cutoff. Identification using the sharp regression discontinuity design rests on the assumption that treatment status is determined by a cutoff score $\overline{x}$ along a forcing variable x_i and therefore quasi-randomly assigned. In the setting studied here, the forcing variable is the amount of land pledged as collateral at the time the loan was disbursed. Without loss of generality, I rescale this variable so that the program eligibility cutoff is centered at zero and use "hectares from cutoff" as the assignment variable throughout the analysis.

Because assignment to treatment ($T_i = 1$) or control ($T_i = 0$) follows the known rule $T_i = \mathbf{1}\{x_i > \overline{x}\} \; \forall \; i$, treatment effect of debt relief, τ_{RD} on an outcome y_i can be estimated as the difference between the regression functions at the discontinuity $\overline{x}$.

$$\tau_{RD} = \lim_{x \downarrow \overline{x}} E\Big[y_i | x_i = \overline{x}\Big] - \lim_{x \uparrow \overline{x}} E\Big[y_i | x_i = \overline{x}\Big] \tag{5}$$

Intuitively, if households in the immediate vicinity of the cutoff $\overline{x}$ do not differ in their observable pre-program characteristics and cannot affect their treatment status once the program has been announced, any differences in observed ex-post outcomes can be attributed to the quasi-random variation in debt relief arising from a household's documented pre-program landholding x_i.

In the literature, two alternative approaches have been used for implementing the sharp regression discontinuity design. The first, and most common estimation strategy is the parametric control function approach (Heckman and Robb, 1985), which estimates the local average treatment effect (LATE) τ_{RD} at a discontinuity point $\overline{x}$ in the forcing variable using a model of the form,

$$y_i = \alpha + \gamma T_i + f(x_i) + \varepsilon_i \tag{6}$$

where y_i is an outcome of interest, T_i is a treatment indicator and $f(x_i)$ is a polynomial function of the forcing variable x_i, such that the treatment effect τ_{RD} is estimated by the parameter γ. An alternative approach is to consider only observations in close proximity of the discontinuity and estimate $y_i = \alpha + \gamma T_i + \varepsilon_i$ in an arbitrarily small neighborhood around the cutoff $\overline{x}$, $x_i \in [\overline{x} + \delta, \overline{x} - \delta]$. In the empirical analysis, I combine the advantages of both methods by following the local linear control function approach proposed by Imbens and Lemieux (2008b), which employs a linear control function and estimates the RD treatment effect for households with landholdings within a narrow band around the program cutoff. Throughout the analysis, the preferred specification is a local linear regression of the form

$$y_i = \alpha + \gamma T_i + \vartheta_1 (T_i \cdot x_i) + \vartheta_2 x_i + \phi_{bd} + \phi_j + \phi_t + \xi' \mathbf{X}_i + \varepsilon_i \tag{7}$$

where T_i is a treatment indicator, x_i is the assignment variable, hectares from cutoff, whose effect I allow to vary to either side of the cutoff. The equation additionally includes bank*district fixed effects ϕ_{bd}, interviewer fixed effects ϕ_i, and month-of-interview fixed effects ϕ_t, and a vector of controls, $\mathbf{X}_i$. Standard errors are clustered at the bank*district level. To verify the robustness of my results, I present estimates using alternative parametric control functions and estimate all equations in three separate samples: (i) the sample of all surveyed households, (ii) a robustness sample consisting of households with audited and matching land records and (iii) a reduced bandwidth sample omitting the top and bottom quartile of the land distribution to each side of the cutoff $\overline{x}$.

The sharp regression discontinuity design relies on two identifying assumptions. The first identification assumption requires that ex-ante observables and pre-program variables are continuous in the forcing variable x_i. Intuitively, this ensures that estimates are not biased by pre-existing or contemporaneous differences between the treatment and control groups in the vicinity of the discontinuity. Formally, this requires that both $\lim_{x \downarrow \overline{x}} E\big[y_i | x_i = \overline{x}\big]$ and $\lim_{x \uparrow \overline{x}} E\big[y_i | x_i = \overline{x}\big]$ are continuous in the forcing variable x_i. If this assumption holds around the cutoff, then any discontinuity in outcomes observed at the cutoff can be attributed to the discontinuity induced by the treatment, in this case debt relief. As a test of this identifying assumption, Figure 4 plots the unconditional means of pre- program observables with accompanying local linear regressions to each side of the eligibility threshold. These graphical tests demonstrate that households in the vicinity of the program eligibility threshold are indeed similar along observable pre-program characteristics, and that there are no discontinuities in any of these variables in the neighborhood of the program threshold. Table D.3 in the Supplemental Appendix provides additional parametric tests for continuity and similarly demonstrates that there are no discontinuities in pre-program observables at the cutoff, indicating that the covariate continuity assumption is met.

The second identifying assumption is that the forcing variable, and therefore treatment status, is not subject to manipulation. Ex-ante manipulation of land status was highly unlikely for several reasons. First, the program was the first of its kind in India that made eligibility conditional on land pledged as collateral at the time the loan was disbursed, rather than the vintage or amount of outstanding debt. Second, the approved amount of agricultural loans is typically proportionate to the value of land pledged as collateral. This means that, in general, households have an incentive to over- rather than underreport their land to the bank, whereas households in the vicinity of the program cutoff would have had to manipulate their documented land downward to benefit from the program. *Third*, several mechanisms were in place to assure faithful implementation and prevent the ex-post manipulation of land documentation. As a transparency measure, all bank branches were required to publicly post the land records 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. Finally, I test for robustness to corruption concerns by auditing official land documents using a statewide electronic database of landholdings. Electronic land records are administered by a central authority and cannot be amended by local administrators. Detailed results of these land audits are reported in the Supplemental Appendix. All results are presented for the sample of all surveyed households, as well as a robustness sample of households with audited and matching land records for which land manipulation can be ruled out. In additional non-reported robustness checks, I show that all results remain qualitatively unchanged when I omit households with non-matching land documents.

Figure 2 (a) illustrates the first-stage discontinuity in implemented debt relief for all households in the survey sample frame and Figure 2 (b) confirms that the same discontinuity holds in the sample of surveyed households. As an additional robustness test, Figure 3 presents evidence from placebo discontinuity regressions. The figure shows the absolute value of t-statistics for estimates of τ_{RD} obtained at a range of placebo discontinuities $\overline{x}_i \in [-.25, .25]$, and provides evidence that the discontinuity induced by the program indeed occurs at $\overline{x} = 0$.

Table 4 reports numerical estimates of the discontinuity in implemented debt relief. On average, households marginally below the two hectare eligibility threshold received Rs 37,156 (US$ 840) *more* debt relief than households just above the cutoff. Placebo regressions reported in the subsequent panels show that the discontinuity occurs in the amount of debt relief that was granted, but not in the amount of borrowers' overdue total balance, outstanding principal or outstanding interest in the vicinity of the eligibility threshold $\overline{x}$. The summary figures also show that at Rs 44,037 (US$ 995), the discontinuity is more pronounced for commercial than cooperative bank accounts (Rs 34,339 or US$ 776), which is not surprising, given that credit cooperatives generally cater to lower-income borrowers than commercial banks. Overall, the difference in relief at the discontinuity is statistically significant and economically substantial: the estimated difference in debt relief between treatment and control households in the survey sample (Rs 34,339) corresponds to approximately 77% of India's 2010 annual per capita income of Rs 44,345 (US$ 1,002).

5 Data Description

The analysis draws on data from two main sources. The first set of data consists of bank lists on all debt relief accounts in the sample districts. As a transparency measure, all banks and credit cooperatives were required to disclose details about all accounts qualifying for full or partial debt relief. This information was posted on public notice boards of local bank branches throughout the country, and several banks also published detailed beneficiary information on their websites. The second set of data comes from a detailed household survey of debt relief beneficiaries, conducted in late 2009, approximately one and a-half years after the debt relief program. The survey covered 2,897 households in four districts of the western Indian state of Gujarat and included detailed questions on household income, production, consumption and investment, as well as a number of questions on financial decisions and expectations. Households were identified from official beneficiary lists and considerable effort was spent to locate and interview as many borrowers as possible.

5.1 Bank Data and Sampling

The sample frame was drawn from the official beneficiary lists published by banks and credit cooperatives. Bank lists typically included the name, village, pledged land, loan category, date of original disbursal, overdue principal and interest as of December 31, 2007, as well as the total amount eligible for debt relief under the program. This account level data was provided by the six largest commercial banks and the largest credit cooperative operating in the four survey districts. Together these banks account for 91% of all accounts that were eligible for the program in the survey districts and 87% of all debt relief accounts in the state. The sample frame was then restricted to accounts within a narrow band of $\pm.5$ hectares around the 100% relief cutoff. This bandwidth was chosen following the cross-validation procedure proposed by Imbens and Kalyanaraman (2011).[14]

The initial sample frame consisted of 5,554 accounts. This includes agricultural crop and investment loans, but excludes loans not directly related to agriculture, since these loans were not contingent on landholdings, so that the discontinuity induced by the program does not apply. The sample frame also omits previously restructured loans, since I observe neither the original loan size nor the vintage, or terms of restructuring for these loans. This restricts the set of loans covered to the roughly 70% of accounts for which landholding was determinant of debt relief qualification.[15]. Descriptives for the population of qualifying loans in the four sample districts are reported in Table 2. Corresponding figures for accounts in the sample frame appear in Table D.1 in the Supplemental Appendix.

Figure 1 summarizes the distribution of debt relief for treatment and control housholds. The average relief amount per beneficiary in the sample frame, Rs 33,498 (US$ 740), is higher than the state average of Rs 24,275 (US$ 540). There are sevaral reasons for this. First, landholdings in the (non-drought affected) survey district are slightly larger, the bulk of qualifying farmers

[14] The chosen range was the bandwidth that minimized the mean squared error when predicting relief amount with landholding and a 100% debt relief 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.

[15] Bank records were not perfect, and for some accounts no land data was available. Accounts without reported landholding were excluded from the sample frame. Because this was a small number of accounts falling into both the unconditional and 25% relief categories, the resulting attrition was random and unlikely to bias the analysis.

in the state, and are more likely to be irrigated than landholdings in the rest of the state. Since there is a positive relationship between landholding, crop value and loan size and also between loan size and relief amount, larger landowners will tend to get more relief. Second, some banks not included in the sample frame, such as rural regional banks and smaller credit cooperatives, are likely to issue smaller loans on average than the larger banks included in the sample frame.

5.2 The Debt Relief Survey

In total, 2,897 households were surveyed in five rural districts of the northwestern Indian state of Gujarat between October and December 2009, approximately one and a-half years after the program.[16] The four sample districts, Mehsana, Gandhinagar, Kheda and Anand[17] form a contiguous band in the central and northwestern part of the state. 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. It is also more urban than the rest of the country, 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 (US$ 540). This was 37% higher than the all-India average relief of Rs 17,712 (US$ 392). However, because Gujarat is more urban and therefore had fewer beneficiaries, the state received slightly below-average relief on a per-capita basis (Government of India, 2008).

[16] Conducting a baseline survey was not feasible, as the program was enacted immediately after its announcement to minimize manipulation. Comprehensive lists of beneficiaries were therefore not available sufficiently ahead of time.

[17] Bank branches from which survey respondents were drawn were located in these four districts, however some clients resided in Ahmedabad district, which surrounds the state's largest city and is wealthier and more urbanized.

Households were visited by survey teams between October and December 2009 and asked to participate in a comprehensive household survey. In the vast majority of cases (84% of all surveyed households), the respondent was the actual borrower identified by the bank record, as well as 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. Household members other than the borrower identified on the beneficiary list were only interviewed, once the actual borrower had been located and this borrower confirmed that another household member was both the financial decision-maker and the actual user of the loan in question. This occurred in a small number of cases, where the loan was taken out in the father's or wife's name, who legally owned the land, but the son or husband was the true financial decision-maker and user of the loan.

There were two main sources of attrition. First, although considerable effort was made to locate all borrowers identified on beneficiary lists, this proved quite difficult for loans that were disbursed several years prior to the program. Second, because only imperfectly recorded and transliterated names were available from banks, many villages had multiple individuals with the same name, which sometimes created obstacles to the identification of individuals if no additional bank data on the beneficiary account was available. No interview was conducted if a borrower could not be identified with certainty based on bank records. To verify that attrition was balanced across treatment and control groups, Table D.2 (a) compares located households with households that could not be found and shows that the probability of a household being located was indeed independent of treatment status. As an additional test, Table D.2 (b) compares basic characteristics of located and non-located households (as available from the bank data). Tested jointly, balanced attrition cannot be rejected at conventional levels of significance ($p = 0.24$). Similarly, attrition is not systematically related to landholding or relief amount.

The average surveyed household is a family of seven with total landholdings of 1.82 hectares, an annual gross income of Rs 72,429 (US$ 1,610) and total pre-program debt of Rs 92,676 (US$ 2,059). For all households in the sample, agriculture is the main source of income. Surveyed households are extremely dependent on credit to support investment in agricultural inputs, such as irrigation, fertilizer and pesticides. Households in the sample spent an annual average of Rs 13,254 (US$ 295) on agricultural inputs, and 72% of households relied on external borrowing to finance this investment. Before debt relief, 86.7% of this credit was provided by banks and credit cooperatives.

6 Main Results

The empirical analysis proceeds in two steps. I first examine the impact of debt relief on the financial position of beneficiary households and the real effect of the bailout on households' investment decisions and productivity. In the second part of the analysis, I explore potential mechanisms that can explain the observed economic behavior of beneficiary households after the program, and focus specifically on the impact of debt relief on borrower expectations.

6.1 The Level of Household Debt

Table 5 presents RD estimates of the change in total household indebtedness. Panel A reports baseline estimates using the sample of all surveyed households and employing four different RD specifications: a basic discontinuity specification without controls and bank*district fixed effects, a linear control function specification with a complete set of bank*district fixed effects ϕ_{bd}, with and without additional loan and household controls, and a quadratic control function specification with bank*district fixed effects. To validate these results, Table 5, Panel B, replicates the estimates for two restricted samples: (i) an audited robustness sample consisting of all households with audited and matching electronic land records, and (ii) a reduced bandwidth sample that omits the top and bottom quartile of the land distribution to either side of the program threshold. As in subsequent tables, the reported coefficients are local average treatment effect (LATE) estimates, measuring the effect of debt relief on households benefiting from 100% unconditional debt relief, relative to borrowers qualifying for only 25% conditional debt relief.

The point estimates in Table 5 indicate that debt relief leads to a significant and persistent reduction in the overall indebtedness of beneficiary households. The economic magnitude of this effect is substantial: on average, the total indebtedness of households that had their outstanding balance cancelled entirely declined by between Rs 24,000 (US$ 470) and Rs 26,000 (US$ 508), or approximately 30% of the pre-treatment mean of overall household debt.[18] These estimates remain stable and statistically significant in alternative samples and are robust to the inclusion of additional controls and bank and district fixed effects. Given that households were surveyed approximately one and a-half years, or two crop cycles, after debt relief, these effects represent a persistent medium-term response to debt relief.[19] They also provide evidence that the program failed to achieve its aim of reintegrating beneficiaries into the formal credit market and providing liquidity through new bank loans.

6.2 The Composition of Household Debt

Table 6 turns to the effect of the bailout on the composition of household borrowing, distinguishing between the share of financing obtained from banks and informal sector lenders.[20] The coefficient estimates reported in Columns (1)–(8) of Panel A, reveal that in addition to reducing the overall debt burden of households, the bailout also led to a persistent shift in the *composition* of household borrowing. One and a-half years after debt relief, formal sector debt among households that benefited from unconditional debt relief declined by 8-10% compared to households in the control group. Over the same period, their relative reliance on informal credit increased by 5-6%. Columns (3)–(8) show that the magnitude of these estimates is unaffected by the inclusion of additional controls and alternative specifications of the parametric control function. Taken together, these results indicate that, while debt relief led to a persistent reduction in the overall level of household debt, it did not enable recipients to substitute high-interest debt from the informal sector with cheaper bank financing. Instead, households

[18] Note that if there is measurement error in recall data, this would likely lead to downward bias in estimates of the change of overall indebtedness, so that the true effect of debt relief may be larger than the reported point estimates.

[19] These estimates are based on self-reported data, in which borrowers may tend to underreport high-interest rate informal sector loans (Karlan and Zinman, 2008). To mitigate this concern, the next section reports evidence based on questions asking about relative shares of financing obtained from different lenders rather than loan amounts.

[20] Results distinguishing between different informal sector lenders are reported in the Supplemental Appendix.

became relatively more reliant on informal credit.

Exploring this shift towards informal credit in greater detail, Table D.7 in the Supplemental Appendix disaggregates the estimates further and considers the relative share of post-program credit obtained from commercial banks, cooperative banks, moneylenders and traders, and friends and relatives, respectively. The results show that the reduction in formal sector borrowing is primarily driven by a decline in borrowing from cooperative banks, while the greater share of informal sector financing is primarily due to a greater percentage of loans obtained from friends and family, rather than high-interest loans from moneylenders or credit from shopkeepers and traders. These results indicate that, in the absence of bank credit, most households are in fact able to increase their relative reliance on internal financing through family networks, rather than having to resort to loans from moneylenders –the most expensive and most strictly enforced class of informal debt.[21]

Could the observed shift towards informal sources of financing be driven by changes in the supply of credit? Evidence from new loan applications after the program suggests that this is unlikely to be the case. To verify that there was in fact no differential discrimination against beneficiary households applying for new loans, Table D.6, Panel A, presents summary statistics on new loan applications after the program. Despite the fact that all households in the 100% relief category qualified for new bank credit, only 376 of the 1,181 households in the treatment group applied for new loan 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 regression results in Table D.6, Panel B, confirm this finding: 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. While the statistical power of the test is limited by the relatively small number of new loan applications after the program, the results provide strong suggestive evidence that the documented shift in

[21] While reliable data on the interest rate structure of such internal financing is limited, stylized evidence suggests that internal financing is both more expensive than bank credit and also poorly suited to the purpose of financing recurrent investment expenditures. One reason for this is that informal loans are generally term loans, while crop loans from institutional lenders tend to be structured as a revolving line of credit, which allows for the partial settlement of outstanding loans. In addition, the increased reliance on internal funding observed in the sample is unlikely to compensate for shortfalls in bank credit, which still accounts for nearly half of the total financing requirements of households in the treatment group.

the composition of borrowing is not driven by changes in the supply of credit or differential discrimination against debt relief recipients.

6.3 Debt Relief, Investment and Productivity

An important economic argument in favor of large-scale debt relief programs is their potential to increase productivity by reducing disincentives for investment arising from high levels of pre-existing debt. The testable implications of this debt overhang argument are two-fold: *First*, debt relief should lead to an increase in investment expenditures because households, once freed of their debts, are now the residual claimants of their investment returns. *Second*, debt relief should reduce investments in risky production technologies, since the risk of project failure is no longer borne by debt holders. As I show in this subsection, I find no evidence in favor of these debt overhang effects. Instead, households that benefited from debt relief, on average, did not access new crop and investment loans which, in turn, constrained their investment and productivity after debt relief. I present evidence in support of this potential mechanism in the next section.

Table 7 estimates the impact of debt relief on the post-program investment of bailout recipients. I measure investment as the average of a household's total gross expenditure on the four primary agricultural inputs: irrigation, fertilizer, pesticides and plowing. To account for seasonal variation in agricultural expenditure, I average total investment over two pre-program crop seasons: the last pre-program monsoon season and the last pre-program dry season. Observing an increase in investment and productivity among beneficiaries of unconditional debt relief, paired with a change in the risk-profile of investment decisions, would provide prima facie evidence in favor of the debt overhang hypothesis. However, as the RD treatment effect estimates in Table 7, Panel A indicate, no such relationship is observed. Instead, households that had their debts cleared by the program reduced their agricultural investment by between 14% (Panel A, Columns 3 and 7) and 24% (Panel A, Column 1) relative to households in the control group.[22] To account for potential heterogeneity in the investment response to

[22] All investment and productivity measures are given in logs, so that estimates of the local average treatment effect τ_{RD} may be interpreted as approximate percentage differences between treatment and control groups at

debt relief, estimates of the difference in investment at the discontinuity are reported both in absolute amounts and in terms of investment per acre of cultivated land. While the estimates indicate that the decline in household productivity is somewhat less pronounced in per acre terms (Panel A, Columns 2 to 4), the decline in input spending among households in the treatment group is unambiguous, irrespective of which measure of investment spending is employed. Table 7, Panels B.1 and B.2 provide further evidence in support of these results and show that the estimates are qualitatively similar and significant at the 5% or 1% level in alternative samples. They are also robust to the inclusion of additional controls and different specifications of the parametric control function. Taken together, this provides strong evidence against the hypothesis that the bailout resolved problems of debt overhang, as well as the hypothesis that debt relief served to attenuate liquidity constraints to household investment.

Table 8, turns to the effect of debt relief on productivity. In line with the previous results, I find that the documented reduction in input spending among bailout recipients is reflected in a corresponding decline in agricultural productivity. To account for seasonal variation in agricultural revenue, I measure agricultural productivity as a household's total revenue from all agricultural production (gross and per acre of cultivated land) averaged over the first two post-program crop seasons. The baseline RD estimates reported in Table 8, Panel A show that among households in the treatment group, agricultural revenue after debt relief decreased by between 12% (Panel A, Column 3) and 22% (Panel A, Column 1), while output per acre of cultivated land declined by approximately 12% relative to the control group. The relative decline in productivity among bailout recipient households is very similar, between 13% (Panel A, Column 8) and 19% (Panel A, Column 2), when measured in terms of revenue per acre, which is arguably a better measure of productivity than the unadjusted level of revenue.

These results, contrary to the debt overhang hypothesis, suggest that the bailout not merely failed to increase investment among program beneficiaries, but in fact led to a decline in investment and productivity. The data provide two potential explanations for this seemingly paradoxical result, which are discussed in greater detail in the next section. *First*, contrary to the program's intention of enabling borrowers to access new loans by clearing their pledged

the cutoff.

collateral, only a small fraction of borrowers in fact applied for a new loan after debt relief. The vast majority of households were therefore no less credit constrained after debt relief than before. Moreover, most agricultural loans are structured as a revolving line of credit, subject to annual or semi-annual review. By clearing a loan from the bank's books, debt relief severed existing lending relationships and eliminated the possibility of obtaining new financing through a partial settlement of an existing line of credit. *Second*, as I show in the next section, debt relief had a strong effect on borrowers' beliefs about future access to credit. By and large, borrowers perceived debt relief as a short-term benefit that might make it significantly more difficult to obtain loans from institutional lenders in the future (potentially as a result of being singled out as a defaulter without being given the opportunity to prove one's creditworthiness unlike borrowers in the 25% conditional debt relief control group). Hence, the expectation of future credit constraints may have led households to rationally reduce investment expenditures after debt relief.

6.4 Expectations and Moral Hazard

Perhaps the most serious criticism of unconditional bailouts is their potential to induce moral hazard by affecting beliefs about the enforceability of debt contracts and the consequences of default. In a setting where households rely on credit to finance productive investment, bailouts may additionally distort recipient behavior by affecting expectations about credit constraints and the enforcement of debt contracts in the future. In this section, I explore these channels in greater detail. I first focus on the effect of debt relief on reputational concerns and moral hazard and then examine how debt forgiveness affects expectations about future access to institutional credit.

Table 9 examines how debt relief affects borrowers' beliefs about 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. These responses may seem initially surprising in a heavily politicized credit market where political considerations make creditor rights particularly difficult to enforce. At the same time, they highlight how important implicit (reputational) components of the credit contract are for the preservation of credit market discipline. Did the bailout damage the credit culture by altering perceptions about the reputational consequences of default? I test this question in Table 9. The dependent variable in all columns of the table are survey responses to the question "If you were to default on a loan from this lender, how likely would this be to tarnish your reputation in your village or community?" The results support the hypothesis that debt relief might indeed induce moral hazard by attenuating borrower concerns about the reputational consequences of default. The RD estimates in Table 9, Panel A, indicate that recipients of debt relief report being significantly less concerned about the reputational consequences of defaulting on a bank loan. At the same time, households in the treatment group report being significantly *more* concerned about the reputational impact of defaulting on an informal sector loan (Table 9, Panel A, Columns 5 to 12). While the estimates are qualitatively similar in the robustness samples in Panel B.1 and B.2, they are not statistically significant at conventional levels, making this only a suggestive result.

How does debt relief affect expectations about future access to credit? To understand the impact of debt relief on household behavior, and decisions about credit-financed investment in particular, it is important to examine how the bailout to affects expectations about the future relationship with institutional lenders. To address this question, Table 10 considers answers to the survey question: "Suppose that you have been unable to repay a loan to each of the following lenders. How worried are you that this will preclude you from borrowing from this lender in the future?". The results in Columns (1)–(8) of Panel A show, that debt relief recipients are unambiguously and significantly more concerned about the effect of default status on future access to institutional credit. For each specification I present RD treatment effect estimates for expectations about future access to credit overall, access to credit from banks (commercial and cooperative banks), and informal lenders, respectively. The results show that households benefiting from unconditional debt relief believe that it will be unambiguously more difficult for defaulters to access credit in the future, and that this is true for access to credit from formal as well as from informal lenders. Table 10, Panels B.1 and B.2 again confirm that these findings are robust to alternative samples and RD specifications.

There are at least two explanations why bailout beneficiaries might have reason to be concerned about future access to institutional credit. *First*, borrowers below the two hectare program cutoff received unconditional debt relief, but were also clearly and publicly identified as defaulters without being given a chance of proving their creditworthiness. Borrowers above the two hectare cutoff, on the other hand were given access to 25% conditional debt relief as well as the chance to signal their creditworthiness by paying down the remainder of their outstanding balance. The results on borrowers' beliefs about future access to credit in Table 10 suggest that debt relief beneficiaries are concerned that being thus classified as a defaulter will worsen their access to institutional credit after debt relief. *Second*, it is worth noting that crop loans are often structured as a revolving line of credit, which allows farmers to settle a part of their balance in order to draw a new loan. While borrowers in the 25% conditional relief were given the possibility of keeping existing credit relationship intact by reaching such a partial settlement, households in the treatment group benefited from 100% unconditional debt relief but also saw an existing lending relationship severed, making it potentially more difficult for a borrower to approach his bank for a new loan after debt relief. The next section presents evidence in favor of this potential mechanism that might explain the link between debt relief and the observed decline in household investment and productivity.

6.5 Discussion of Potential Mechanisms

The findings so far provide strong prima facie evidence against theories of debt overhang, but cannot pin down the underlying mechanism linking debt relief to reduced investment. In this section, I present additional results that provide evidence in support of the hypothesis that bailout recipients reduced investment in anticipation of future credit constraints.

The model in Section 3 makes several testable predictions that allow for a more precise investigation of the channel linking debt relief to household investment. The finding that households reduce their investment after the bailout rules out the possibility that the debt overhang channel (Proposition 1) is operating in the setting studied here. Additionally, the hypothesis that households reduce their investment due to the expectation of future borrowing constraints (Proposition 2), has two testable implications. First, the reduction in investment should be greater for households expressing greater concern about future credit constraints. Second, in the presence of anticipated credit constraints, investment expenditure should depend on current wealth and liquid assets. If debt relief served to reduce credit constraints this effect would be less pronounced for debt relief households. If, instead, debt relief aggravated concerns about borrowing constraints, this effect will be more pronounced for households benefiting from full debt relief.

Table 11 presents results on the heterogeneous effect of debt relief and provides evidence in favor of this mechanism. Table 11, Panel A shows coefficient estimates for households above and below the median of the self-reported anticipation of credit constraints. The point estimates provide evidence consistent with the prediction that households that are more concerned about future borrowing constraints will reduce their investment expenditures more dramatically. The point estimates for debt relief households below the median of self-reported concerns about borrowing constraints are negative, but statistically indistinguishable from zero. The coefficient estimate for households above the median of self-reported expectations of credit constraints is negative and statistically significant at the 1% level in all specifications. Table 11, Panel B, repeats this exercise for household above and below the median of total pre-program revenue. Under the assumption that debt relief removed credit constraints among recipients, households with greater pre-program revenue should not invest more than households with lower pre-program revenue. If, on the contrary, debt relief induced concern about future borrowing constraints, households with greater pre-program revenue and liquidity will invest more. I find strong evidence of this mechanism. In contrast to the overall trend in the investment response to debt relief, households with pre-program revenue above the mean were able to increase investment in productive inputs by up to 21%. This effect remains unchanged when I condition on household characteristics proxying for overall household wealth such as education, household size and total land owned. The estimate for households below the median is indistinguishable from zero throughout.

What alternative channels might explain the relationship between debt relief and the observed decline in household investment and productivity? Duflo, Kremer and Robinson (2011) study the investment decision of farmers in Kenya and show that time-inconsistent preferences limit profitable investments. In principle, it is possible that the same mechanism is operational in the sample of farmers studied here. However, in order to explain the results reported in the preceding sections, debt relief would need to induce differences in the time-preferences between treatment and control households. In non-reported robustness checks I test the impact of debt relief on measures of time preferences and find no effect. Hence time-inconsistent preferences may constrain the investment decisions of households in the sample overall, but not differentially so among the treatment and control group. Another potential mechanism that might induce a reduction in investment expenditure after debt relief is the possibility that households perceive debt relief as a windfall and shift their expenditures away from investment and towards consumption (see e.g. a discussion in de Mel, McKenzie and Woodruff 2008).[23] Two observations make this mechanism unlikely to operate in the setting studied here. First, note that the program eliminated debt on the books, but did not involve cash payments. Additionally few households used debt relief to obtain a new loan so that the effect on household liquidity and the potential to increase consumption was limited. Second, when I examine the consumption expenditures of households after the program I find no difference between households in the treatment group. I also find no evidence of an impact of debt relief on savings, which suggests that, households lack sufficient liquidity to finance investments from precautionary savings prior to the program, and that debt relief does not lead households to increase savings ex-post.

7 Conclusion

This paper has examined the impact of a large scale debt relief program, intended to attenuate invesment constraints among highly-indebted rural households in India. I identify the causal effect of debt relief on the economic decisions of recipient households using a natural experiment

[23] The authors find that micro-enterprise owners in Sri Lanka who received unconditional cash transfers invested 58% of the grant in their business but only 5% on household consumption

generated by India's *Debt Relief Program for Small and Marginal Farmers*, one of the largest debt relief initiatives in history.

I find that the bailout has a persistent and economically large effect on recipient households: approximately one year after the program, households that received full debt relief remain significantly less indebted than households eligible only for partial debt relief. Despite the substantial benefit to individual households, the bailout did not attenuate problems of debt overhang or increase productive investment among recipient households. Instead, bailout recipients increased their reliance on informal credit, reduced their investment relative to households in the control group and suffered a corresponding decline in productivity. An important channel through which the bailout appears to affect investment and productivity is its impact on expectations about access to institutional credit. Recipients of unconditional debt relief are significantly more concerned about their future access to institutional credit and exhibit investment behavior consistent with the anticipation of future credit constraints.

These findings have important implications for the design of policy. The puzzle of underinvestment in productive inputs among poor households is usually viewed through the lens of the traditional debt overhang model. According to this view, highly-indebted households face disincentives for investment because the returns of any such investment will accrue largely to debt holders, rather than the household. Viewing low productivity among highly-indebted households through this lens suggests that a bailout which, akin to a bankruptcy settlement, clears the household's pledged collateral should remove disincentives for investment and increase productivity.

This paper provides evidence in favor of a different explanation, based on the important role of expectations in the household's investment decision. The results indicate that resolving the incentive problem arising from a large stock of inherited debt alone is not sufficient to overcome barriers to new investment. In spite of the significant amount of debt cancelled under the program, a significant share of households does not exercise the option of using cleared collateral to access new loans. The results suggest that this might be due to the disruption of ongoing credit relationships, and the stigma of being identified as a defaulter due to the program, which may make it more difficult for program beneficiaries to access institutional credit in the future. This suggests that an important part of the solution is to combine debt forgiveness with strong incentives for the re-establishment of longer-term lending relationships, rather than focusing on one-time settlements, as is often done in practice.

While the findings on moral hazard should be interpreted with caution, as they are based on self-reported attitudes rather than observed defaults, they do suggest that unconditional bailouts can significantly distort beliefs about the conditionality of debt and the reputational consequences of default. One option to reduce the adverse implications on borrower expectations and moral hazard is to strengthen elements of repeated contracting in new lending relationships after relief, as well as incentives for timely repayment among non-beneficiaries whose repayment incentives are affected by the program. This was in fact done in India, where, one year after the program, the government introduced a temporary interest rate subsidy rewarding timely repayment.

I conclude with some directions for future research. First, this paper has focused on the household response to debt relief. Many questions relating to the moral hazard effects of large bailout programs have general equilibrium implications and are therefore beyond the scope of this paper. Understanding the impact of debt relief on moral hazard and banks' willingness to lend to beneficiaries in the longer run are crucial for the welfare assessment of debt relief programs and an important area of future inquiry. Second, the impact of debt relief settlements may be heterogeneous and differ especially for households who are not engaged in production. Finally, it would be useful to understand the persistence of behavioral responses to debt relief. In contrast to the literature on personal bankruptcy, I find evidence of relatively persistent effects of the bailout, which are likely to be amplified by the impact of the program on household productivity. Exploring these questions in greater detail is a promising avenue for future research and can shed light on how to best structure programs to remove barriers to productive investment.

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Tables and Figures

Table 1: **Debt Relief by Classification and Location**

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

Table 2: **Summary Statistics** – Bank Data

This table reports summary statistics for all accounts in the sample frame based on bank data. "Total overdue" is the total amount of outstanding and overdue agricultural credit as of December 31, 2007. "Eligible debt relief" is the total amount of debt eligible to be waived under the program, which is 100% of outstanding debt for households with $\leq$ 2 hectares of pledged collateral and 25% conditional relief otherwise. Treatment effects at the program cutoff $\hat{\tau}_{RD}$ are estimated using the preferred specification with a local linear control function. Standard errors, in brackets, are clustered by bank and district. +p<0.10 ** p<0.05 *** p<0.01

	N	Mean	Median	StDev	Min	Max	$\hat{\tau}_{RD}$	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total overdue	5,524	52,915	41,427	48,538	0	882,806	127.12	[4149.9]
Principal overdue	5,514	40,627	30,000	43,351	0	830,000	533.7	[3802.1]
Interest overdue	5,414	12,595	7,193	17,009	0	319,810	1,607	[1333.5]
Loan type=crop loan	5,554	0.77	1.00	0.42	0	1.00	0.05	[0.031]
Landholding (hectares)	5,554	1.97	1.96	0.29	1.50	2.52	0.00	[0.001]
Eligible debt relief	5,554	33,498	23,801	36,823	0	751,594	41,255	[4,850]***

Table 3: **Summary Statistics** – Respondent and Loan Characteristics

This table reports summary statistics for all accounts in the sample frame. *Debt Relief* is the total amount of debt relief granted to a household. *Total Debt* is the amount of total self reported debt at the time of the survey and prior to the program. *Bank Debt* includes loans from commercial and cooperative banks, *Informal Debt* includes loans from friends and family, shopkeepers, traders, and moneylenders. *Investment* in agricultural inputs includes spending on irrigation, non-organic fertilizer, pesticides and ploughing, gross and per acre, and is averaged over two pre-program seasons. *Productivity* is measured as total revenue from all agricultural activities, gross and per acre, averaged over two pre-program seasons. Standard errors, in brackets, are clustered by bank and district. +p< 0.10 ** p<0.05 *** p<0.01

Panel A: Treatment

	N	Mean	Median	StDev	Min	Max	$\hat{\tau}_{RD}$
Log debt relief, Rs	2,897	9.96	10.08	1.02	2.4	13.53	
Treated [$\leq$ 2 ha]	1,716	10.44	10.51	0.82	3.09	13.53	1.383***
Control [$>$ 2 ha]	1,181	9.284	9.386	0.87	2.4	11.94	(0.07)

Panel B.1: Debt

	N	Mean	Median	StDev	Min	Max	$\hat{\tau}_{RD}$
Total Debt, Pre-Program							
Log total debt, Rs	2,825	10.83	10.71	1.04	6.91	15.13	
Treated	1,672	10.73	10.65	0.98	6.91	15.02	0.03
Control	1,152	10.99	10.82	1.11	6.91	15.13	(0.02)
Bank debt, %	2,732	86.71	100	28.35	0.00	100	
Treated	1,600	85.9	100	29.32	0.00	100	-1.303
Control	1,132	87.86	100	26.9	0.00	100	(1.45)
Informal debt, %	2,732	8.44	0.00	22.27	0.00	100	
Treated	1,599	9.09	0.00	23.69	0.00	100	0.325
Control	1,133	7.52	0.00	20.08	0.00	100	(1.18)

Panel B.2: Investment and Productivity

	N	Mean	Median	StDev	Min	Max	$\hat{\tau}_{RD}$
Investment, Pre-Program							
Log investment, Rs	2,571	9.29	9.33	0.91	5.74	13.82	
Treated	1,505	9.2	9.25	0.88	5.74	13.82	-0.101
Control	1,066	9.43	9.47	0.94	6.21	12.88	(0.06)
Log investment per acre	2,601	8.47	8.7	1.58	00.0	14.23	
Treated	1,521	8.46	8.7	1.61	0.00	14.23	-0.008
Control	1,080	8.49	8.7	1.54	0.00	12.9	(0.13)
Productivity, Pre-Program							
Log revenue, Rs	2,521	10.74	10.82	1.03	6.62	13.15	
Treated	1,478	10.65	10.78	1.03	6.62	13.12	-0.108
Control	1,043	10.86	10.92	1.01	7.09	13.15	(0.06)
Log revenue per acre	2,488	9.44	9.54	0.99	5.25	16.03	
Treated	1,455	9.45	9.45	1.02	5.25	16.03	-0.077
Control	1,033	9.44	9.44	0.95	5.41	14.1	(0.07)

Table 4: **Regression Discontinuity** – First Stage

This table reports evidence on the program-induced discontinuity in debt relief, based on bank data. The table additionally reports placebo tests using total eligible amount, eligible principal amount and eligible overdue interest. *Debt Relief* refers to the net amount of debt relief granted. *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 on treatment status and a local linear control function, using the sample of all surveyed households. Robust standard errors, in brackets, 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: **Regression Results** – Total Household Debt

This table reports the effect of debt relief on the total outstanding debt of surveyed households. Within a panel, each column reports results from a separate regression. The dependent variable is the change in total self-reported outstanding debt between the pre- and post-program periods in units of Rs '000 and as a percentage of agricultural revenue in the crop season before the program. Regressions in panel A are estimated using the full sample. Panels B.1 and B.2 report restricted sample robustness checks. Panel B.1 reports estimates from a robustness sample consisting of housheolds with audited and matching land records. Panel B.2 reports estimates from a reduced bandwidth sample, which excludes observations in the top and bottom 25% of observations to either side of the program cutoff. All regressions exclude households not engaged in agricultural production. Interview effects include interviewer and month-of-interview dummies. Respondent controls include gender, age, years of education, household size, log of pre-program land owned, log of pre-program total debt. Robust standard errors, in brackets, are clustered by bank and district. + $p<0.10$ ** $p<0.05$ *** $p<0.01$.

	Model 1	Model 2	Model 3	Model 4
Panel A: Full Sample	(1)	(2)	(3)	(4)
100% Relief	-26.44+	-24.98+	-24.69+	-25.94**
	[14.81]	[12.25]	[13.39]	[12.45]
Observations	2,246	2,246	2,191	2,246
R-squared	0.04	0.04	0.05	0.04
Panel B: Robustness Samples				
Audited accounts				
100% Relief	-22.39	-25.03+	-25.02+	-25.57
	[18.70]	[14.84]	[14.94]	[15.06]
Observations	1,460	1,460	1,419	1,460
R-squared	0.05	0.06	0.07	0.06
Reduced bandwidth sample				
100% Relief	-30.28+	-27.16+	-26.16	-28.77+
	[16.33]	[15.60]	[16.70]	[15.75]
Observations	1,645	1,645	1,600	1,645
R-squared	0.04	0.06	0.07	0.06
Interviewer effects	Yes	Yes	Yes	Yes
Bank*district effects	No	No	Yes	Yes
Local linear control function	Yes	Yes	Yes	No
Quadratic control function	No	No	No	Yes

Table 11: **Regression Results** – Heterogeous Effects

This table reports heterogeneous treatment effects. Each column displays results from a separate regression. The dependent variable in all regressions is the log of total agricultural investment, averaged over two post-program crop seasons, and all other variables are as previously defined. Estimates are based on the full sample and exclude household controls to rule out potential endogeneity in the investment reponse to debt relief. Panel A.1 reports treatment effects for households above and below the mean value of the survey response to the question "If you were to default on a loan from the following source, how worried are you that you will not be able to borrow from this source in the future?". Panel A.2 reports treatment effects for households above and below the median of total pre-program debt. Panels B.1 and B.2 report treatment effects for households below and above median revenue in the season prior to debt relief, and total pre-program savings. Robust standard errors, in brackets, are clustered by bank and district. + p<0.10 ** p<0.05 *** p<0.01.

	Model 1	Model 2	Model 3	Model 4
Panel A: Expectation of Credit Constraints	(1)	(2)	(3)	(4)
≤median	-0.19***	-0.09	-0.09	-0.09
	[0.08]	[0.05]	[0.05]	[0.05]
>median	-0.26***	-0.15***	-0.15***	-0.15
	[0.08]	[0.07]	[0.07]	[0.07]
Obervations	2,519	2,519	2,519	2,519
R-squared	0.15	0.13	0.13	0.13
Panel B: Pre-Program Debt, Revenue and Savings				
Total debt, pre-program				
≤median	-0.26***	-0.19***	-0.20***	-0.19***
	[0.08]	[0.07]	[0.07]	[0.07]
>median	0.001	0.001	0.001	{0.03
	[0.08]	[0.07]	[0.07]	[0.07]
Obervations	2,519	2,519	2,519	2,519
R-squared	0.17	0.15	0.15	0.15
Total revenue, pre-program				
≤median	-0.19***	-0.09	-0.09	-0.09
	[0.08]	[0.07]	[0.07]	[0.07]
>median	0.14+	0.21***	0.21***	0.20***
	[0.08]	[0.07]	[0.07]	[0.06]
Obervations				
R-squared				
Total savings, pre-program				
≤median	-0.19***	-0.10	-0.10	-0.07
	[0.08]	[0.06]	[0.06]	[0.06]
>median	-0.04	0.06	0.06	0.06
	[0.08]	[0.06]	[0.06]	[0.06]
Obervations	2,519	2,519	2,519	2,519
R-squared	0.19	0.17	0.17	0.17
Bank*district effects	No	Yes	Yes	Yes
Respondent controls	No	No	Yes	No
Local linear control fctn	No	Yes	Yes	No
Quadratic control fctn	No	No	No	Yes

Table 6: **Regression Results** – Debt Relief and Sources of Credit

The table reports the effect of debt relief on the percentage of credit obtained from formal and informal sector lenders. Within a panel, each column reports results from a separate regression. The dependent variable is the change in the percentage of credit obtained from banks and informal lenders, respectively. Loans from *Banks* include commercial and cooperative banks, Loans from *Informal Lenders* include moneylenders, shopkeepers, traders, friends and relatives. All additional controls and fixed effects are as defined in Table 5. Robust standard errors, in brackets, clustered by bank and district. + $p<0.10$ ** $p<0.05$ *** $p<0.01$.

	Model 1		Model 2		Model 3		Model 4	
	Banks	Informal lenders	Banks	Informal lenders	Banks	Informal lenders	Banks	Informal lenders
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% Relief	-10.03***	6.19***	-7.94***	5.27***	-8.04***	5.30***	-8.01***	5.38***
	[2.48]	[1.47]	[2.47]	[1.46]	[2.63]	[1.44]	[2.47]	[1.46]
Observations	2,692	2,690	2,692	2,690	2,552	2,550	2,692	2,690
R-squared	0.2	0.12	0.19	0.11	0.2	0.12	0.19	0.11
Panel B: Robustness Samples								
Audited accounts								
100% Relief	-9.72**	7.22**	-7.35**	5.97**	-7.77***	6.26***	-7.43**	6.00**
	[2.93]	[2.36]	[2.95]	[2.38]	[2.73]	[2.20]	[2.96]	[2.39]
Observations	1,721	1,720	1,721	1,720	1,633	1,632	1,721	1,720
R-squared	0.21	0.15	0.2	0.14	0.21	0.14	0.2	0.14
Reduced bandwidth sample								
100% Relief	-8.95**	6.22**	-7.27***	5.03***	-7.20***	4.40**	-7.47***	5.19***
	[2.78]	[1.87]	[2.67]	[1.87]	[2.67]	[1.79]	[2.69]	[1.91]
Observations	1,994	1,992	1,994	1,992	1,877	1,875	1,994	1,992
R-squared	0.2	0.12	0.18	0.11	0.19	0.12	0.18	0.11
Interviewer effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Respondent controls	No	No	No	No	Yes	Yes	No	No
Local linear control fctn	No	No	Yes	Yes	Yes	Yes	No	No
Quadratic control fctn	No	No	No	No	No	No	Yes	Yes

Table 7: **Regression Results** – Debt Relief and Investment

This table reports the effect of debt relief on investment. Within a panel, each column reports results from a separate regression. The dependent variables are (i) total spending on agricultural inputs and (ii) spending on agricultural inputs per acre of cultivated land, both measured as averages over the first two post-program crop seasons. All additional controls and fixed effects are as defined in Table 5. Robust standard errors, in brackets, clustered by bank and district. + p<0.10 ** p<0.05 *** p<0.01.

	Model 1		Model 2		Model 3		Model 4	
	Banks	Informal lenders	Banks	Informal lenders	Banks	Informal lenders	Banks	Informal lenders
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% Relief	-0.24***	-0.20***	-0.15***	-0.14**	-0.19***	-0.17***	-0.15***	-0.15**
	[0.06]	[0.06]	[0.05]	[0.06]	[0.05]	[0.06]	[0.05]	[0.06]
Observations	2,519	2,473	2,519	2,473	2,095	2,079	2,519	2,473
R-squared	0.15	0.20	0.13	0.18	0.28	0.24	0.13	0.18
Panel B: Restricted Samples								
Audited accounts								
100% Relief	-0.26***	-0.18**	-0.16***	-0.08	-0.17***	-0.1	-0.16***	-0.09
	[0.05]	[0.06]	[0.04]	[0.06]	[0.05]	[0.07]	[0.04]	[0.06]
Observations	1,610	1,582	1,610	1,582	1,329	1,323	1,610	1,582
R-squared	0.17	0.22	0.14	0.19	0.29	0.26	0.14	0.19
Reduced bandwidth sample								
100% Relief	-0.24***	-0.17**	-0.15***	-0.12**	-0.17***	-0.15***	-0.15***	-0.13**
	[0.06]	[0.06]	[0.05]	[0.06]	[0.05]	[0.05]	[0.05]	[0.06]
Observations	1,826	1,790	1,826	1,790	1,507	1,497	1,826	1,790
R-squared	0.14	0.20	0.12	0.17	0.29	0.24	0.12	0.17
Interviewer effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Respondent controls	No	No	No	No	Yes	Yes	No	No
Local linear control fctn	No	No	Yes	Yes	Yes	Yes	No	No
Quadratic control fctn	No	No	No	No	No	No	Yes	Yes

Table 8: **Regression Results** – Debt Relief and Productivity

This table reports the effect of debt relief on productivity. Within a panel, each column reports results from a separate regression. The dependent variables are (i) total agricultural revenue and (ii) agricultural revenue per acre of cultivated land, both measured as averages over the first two post-program crop seasons. All additional controls and fixed effects are as defined in Table 5. Robust standard errors, in brackets, are clustered by bank and district. + $p<0.10$ ** $p<0.05$ *** $p<0.01$.

	Model 1		Model 2		Model 3		Model 4	
	Revenue	Revenue per acre	Revenue	Revenue per acre	Revenue	Revenue per acre	Revenue	Revenue per acre
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% Relief	-0.22***	-0.19***	-0.12**	-0.13**	-0.19***	-0.18***	-0.13**	-0.13**
	[0.07]	[0.06]	[0.06]	[0.05]	[0.05]	[0.05]	[0.06]	[0.05]
Observations	2,494	2,444	2,494	2,444	2,075	2,055	2,494	2,444
R-squared	0.13	0.16	0.12	0.15	0.29	0.2	0.12	0.15
Panel B: Restricted Samples								
Audited accounts								
100% Relief	-0.26***	-0.20***	-0.16**	-0.12+	-0.22***	-0.18***	-0.16**	-0.13+
	[0.07]	[0.06]	[0.07]	[0.07]	[0.07]	[0.06]	[0.07]	[0.07]
Observations	1,597	1,567	1,597	1,567	1,319	1,311	1,597	1,567
R-squared	0.13	0.18	0.12	0.16	0.29	0.21	0.12	0.16
Reduced bandwidth sample								
100% Relief	-0.23**	-0.15**	-0.12	-0.10+	-0.20***	-0.19***	-0.13+	-0.11+
	[0.08]	[0.06]	[0.07]	[0.06]	[0.06]	[0.06]	[0.07]	[0.06]
Observations	1,812	1,770	1,812	1,770	1,496	1,481	1,812	1,770
R-squared	0.12	0.16	0.11	0.15	0.3	0.21	0.11	0.15
Interviewer effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Respondent controls	No	No	No	No	Yes	Yes	No	No
Local linear control fctn	No	No	Yes	Yes	Yes	Yes	No	No
Quadratic control fctn	No	No	No	No	No	No	Yes	Yes

Table 9: **Moral Hazard** – Does Debt Relief Change Beliefs about the Reputational Consequences of Default?

This table reports the effect of debt relief on beliefs about the reputational consequences of default. Each column represents a separate regression. The dependent variable is 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 your village or community?'" on a scale from 0 (no effect) to 5 (severe negative effect). Regressions in panel A are estimated using the full sample of surveyed households. Panels B.1 and B.2 report restricted sample robustness checks. Panel B.1 reports estimates from the sample of housheolds with audited and matching land records. Panel B.2 reports estimates from a reduced bandwidth sample, which excludes observations outside the bottom 25% of observations below and the top 25% of observations above the program cutoff. Respondent controls are as previously defined. Loan controls include years since disbursal of the loan and its interaction with treatment status and a dummy variable equal to one for loans from cooperative banks. Standard errors, in brackets, are clustered by bank and district. + p<0.10 ** p<0.05 *** p<0.01.

	Model 1			Model 2			Model 3			Model 4		
	Overall	Banks	Informal lenders	Overall	Banks	Informal lenders	Overall	Banks	Informal lenders	Overall	Banks	Informal lenders
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
100% Relief	0.01	-0.03	0.05	-0.01	-0.12**	0.10	-0.02	-0.14**	0.10	-0.01	-0.12**	0.10
	[0.04]	[0.06]	[0.06]	[0.04]	[0.06]	[0.06]	[0.04]	[0.06]	[0.06]	[0.04]	[0.06]	[0.06]
Observations	2,792	2,822	2,795	2,436	2,459	2,439	2,316	2,338	2,319	2,436	2,459	2,439
R-squared	0.49	0.44	0.36	0.48	0.44	0.35	0.49	0.45	0.34	0.48	0.44	0.34
Panel B: Restricted Samples												
Audited accounts												
100% Relief	0.08	0.06	0.11	0.09	0.01	0.17+	0.07	-0.02	0.16+	0.08	0.01	0.16+
	[0.05]	[0.08]	[0.07]	[0.07]	[0.09]	[0.09]	[0.06]	[0.09]	[0.09]	[0.07]	[0.09]	[0.08]
Observations	1,782	1,798	1,784	1,542	1,556	1,544	1,466	1,480	1,468	1,542	1,556	1,544
R-squared	0.49	0.45	0.37	0.49	0.46	0.37	0.5	0.46	0.37	0.49	0.46	0.37
Reduced bandwidth sample												
100% Relief	0.05	0.03	0.07	0.01	-0.07	0.10	0.00	-0.09	0.09	0.01	-0.06	0.09
	[0.03]	[0.06]	[0.05]	[0.06]	[0.12]	[0.08]	[0.05]	[0.11]	[0.08]	[0.06]	[0.12]	[0.08]
Observations	2,050	2,074	2,053	1,788	1,807	1,791	1,690	1,708	1,693	1,788	1,807	1,791
R-squared	0.51	0.46	0.38	0.50	0.47	0.36	0.51	0.48	0.36	0.5	0.47	0.36
Interviewer effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent controls	No	No	No	No	No	No	Yes	Yes	Yes	No	No	No
Local linear control function	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Quadratic control function	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes

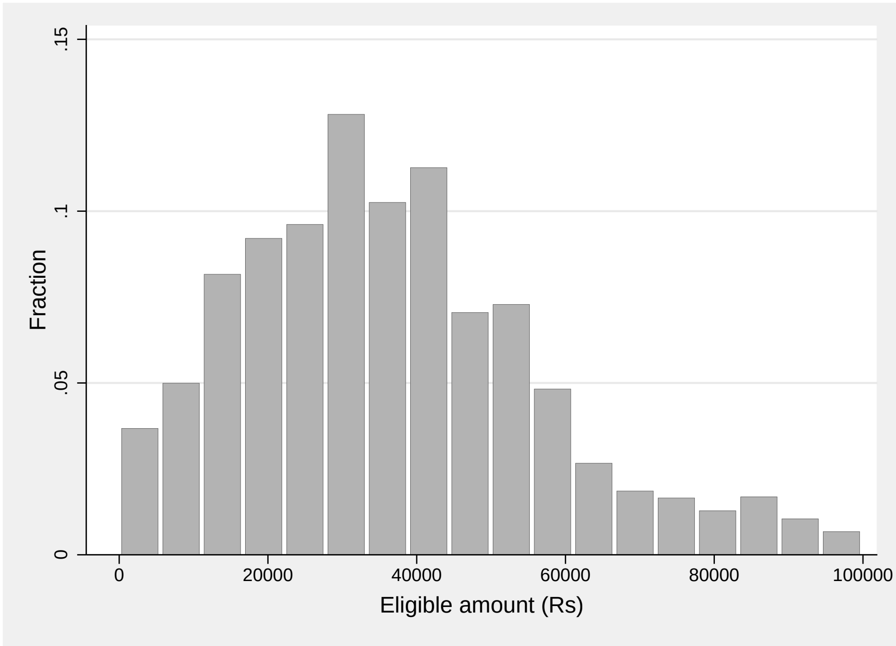
Table 10: **Expectations** – Does Debt Relief Change Expectations of Future Access to Credit?

This table reports the effect of debt relief on expectations about future access to credit. Each column represents a separate regression. The dependent variable is is based on the survey question "If a borrower like you defaulted on one of the following lenders due to a bad harvest or other unforeseen circumstances, how worried does he have to be that he will not be able to borrow from this source in the future?" on a scale from 0 (not worried at all) to 5 (very worried). All additional controls and fixed effects are as defined in Table 9. Loan controls include years since disbursal of the loan and its interaction with treatment status and a dummy variable equal to one for loans from cooperative banks. Standard errors, in brackets, are clustered by bank and district. + $p<0.10$ ** $p<0.05$ *** $p<0.01$.

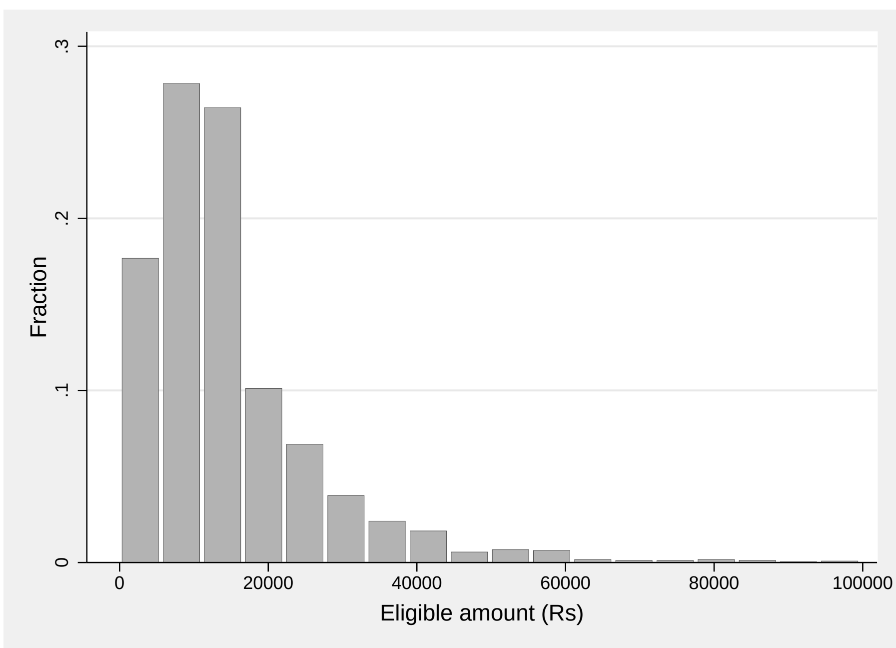
	Model 1			Model 2			Model 3			Model 4		
	Overall	Banks	Informal lenders	Overall	Banks	Informal lenders	Overall	Banks	Informal lenders	Overall	Banks	Informal lenders
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
100% Relief	0.15**	0.15	0.12**	0.24**	0.31+	0.17**	0.27***	0.39**	0.14+	0.25**	0.32+	0.17**
	[0.07]	[0.11]	[0.05]	[0.10]	[0.18]	[0.08]	[0.10]	[0.18]	[0.07]	[0.10]	[0.18]	[0.07]
Observations	2,742	2,797	2,750	2,386	2,436	2,393	2,269	2,317	2,274	2,386	2,436	2,393
R-squared	0.46	0.42	0.38	0.44	0.41	0.35	0.45	0.43	0.35	0.44	0.41	0.35
Panel B: Restricted Sample												
Audited accounts												
100% Relief	0.18**	0.17	0.15	0.31**	0.37+	0.23	0.34***	0.47**	0.18	0.31**	0.38+	0.23
	[0.08]	[0.14]	[0.12]	[0.13]	[0.19]	[0.18]	[0.13]	[0.20]	[0.19]	[0.13]	[0.19]	[0.18]
Observations	1,755	1,789	1,759	1,518	1,547	1,522	1,443	1,472	1,446	1,518	1,547	1,522
R-squared	0.48	0.43	0.39	0.46	0.42	0.37	0.48	0.43	0.37	0.46	0.42	0.37
Reduced bandwidth sample												
100% Relief	0.17**	0.13	0.20***	0.22**	0.17	0.26***	0.26**	0.26	0.23**	0.23**	0.18	0.27***
	[0.08]	[0.13]	[0.07]	[0.11]	[0.19]	[0.07]	[0.11]	[0.19]	[0.10]	[0.11]	[0.19]	[0.07]
Observations	2,017	2,058	2,024	1,752	1,791	1,759	1,656	1,693	1,661	1,752	1,791	1,759
R-squared	0.48	0.45	0.41	0.47	0.44	0.38	0.47	0.45	0.37	0.47	0.44	0.37
Interviewer effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent controls	No	No	No	No	No	No	Yes	Yes	Yes	No	No	No
Local linear control function	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Quadratic control function	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes

Figure 1: Distribution of Eligible Relief Amount

(a) Treatment: 100% unconditional debt relief



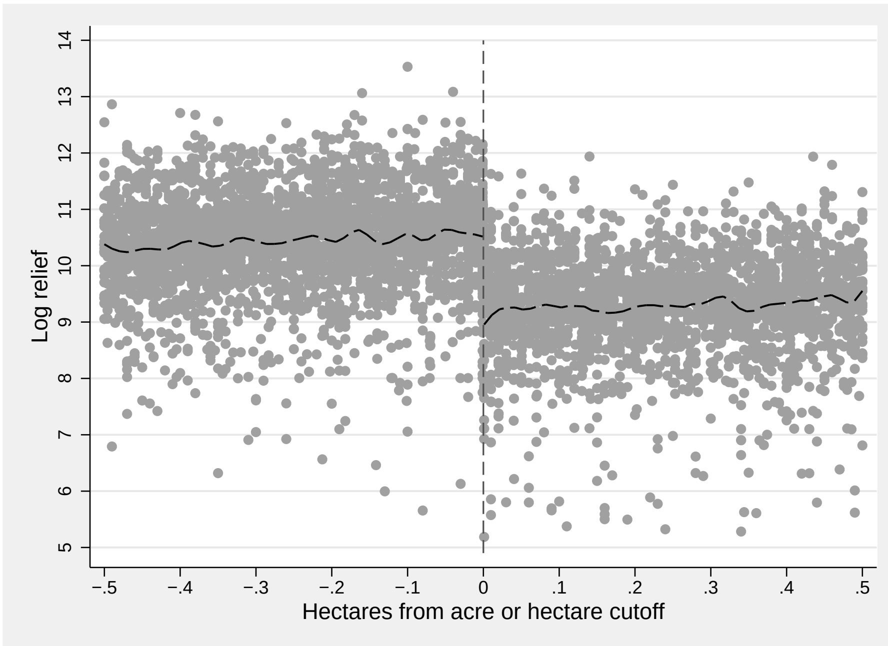
(b) Control: 25% conditional debt relief



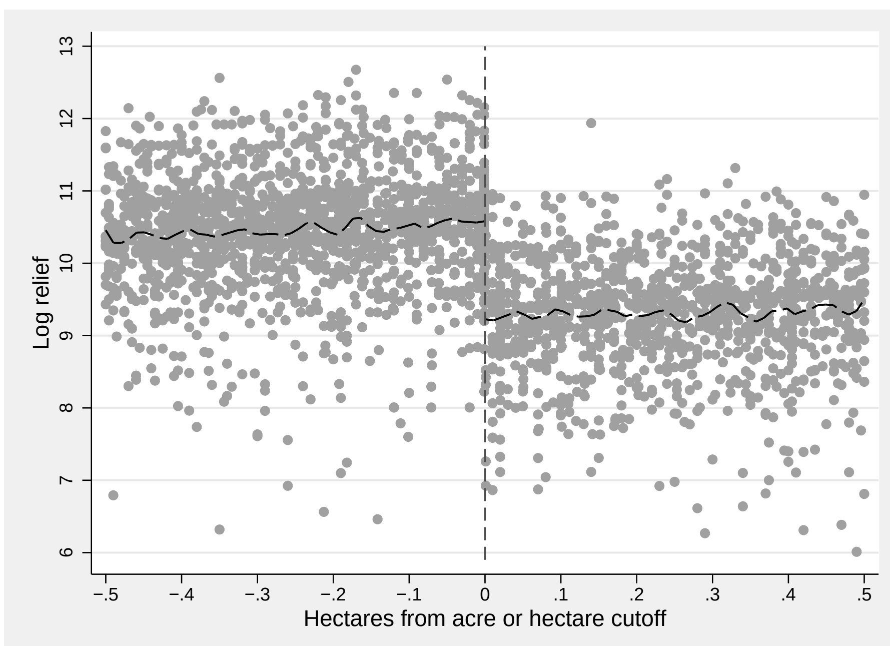
Notes: Panel (a) shows the amount of outstanding total debt eligible for 100% unconditional debt relief for households in the treatment group. Panel (b) shows the amount of total outstanding debt eligible for conditional debt relief for households in the control group. The amount of debt relief disbursed to households in the control group was 25% of the eligible amount, conditional on repayment of the remaining balance.

Figure 2: Regression Discontinuity First Stage

(a) Sample Frame (N=5,554)

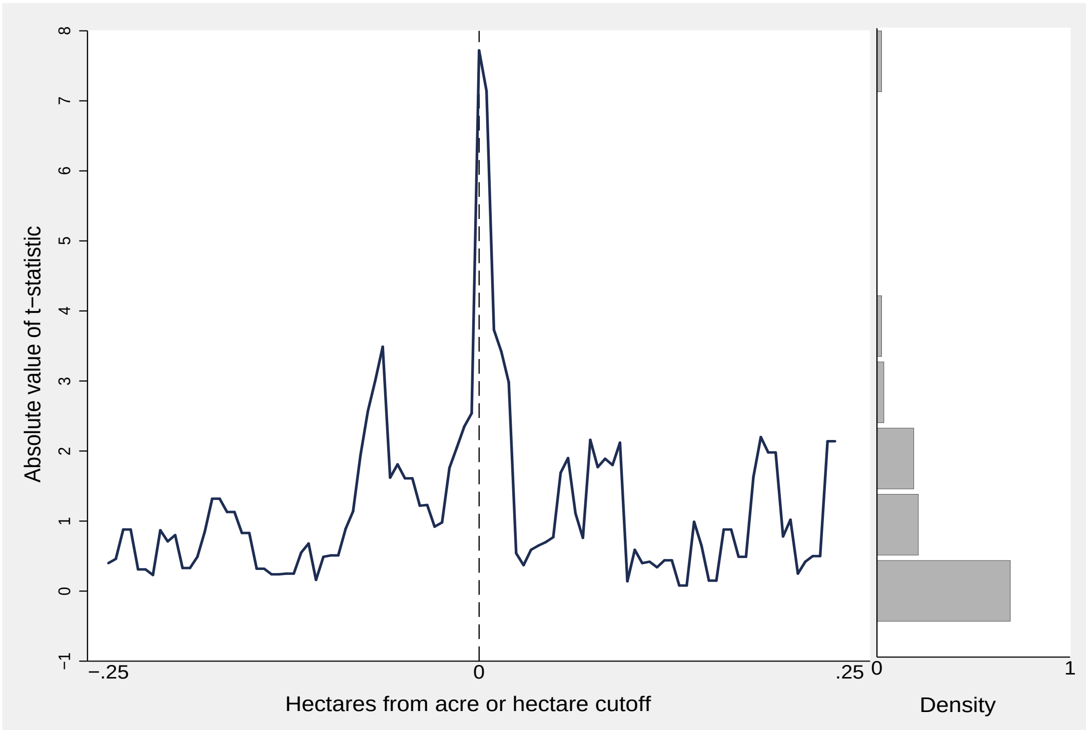


(b) Surveyed Households (N=2,897)



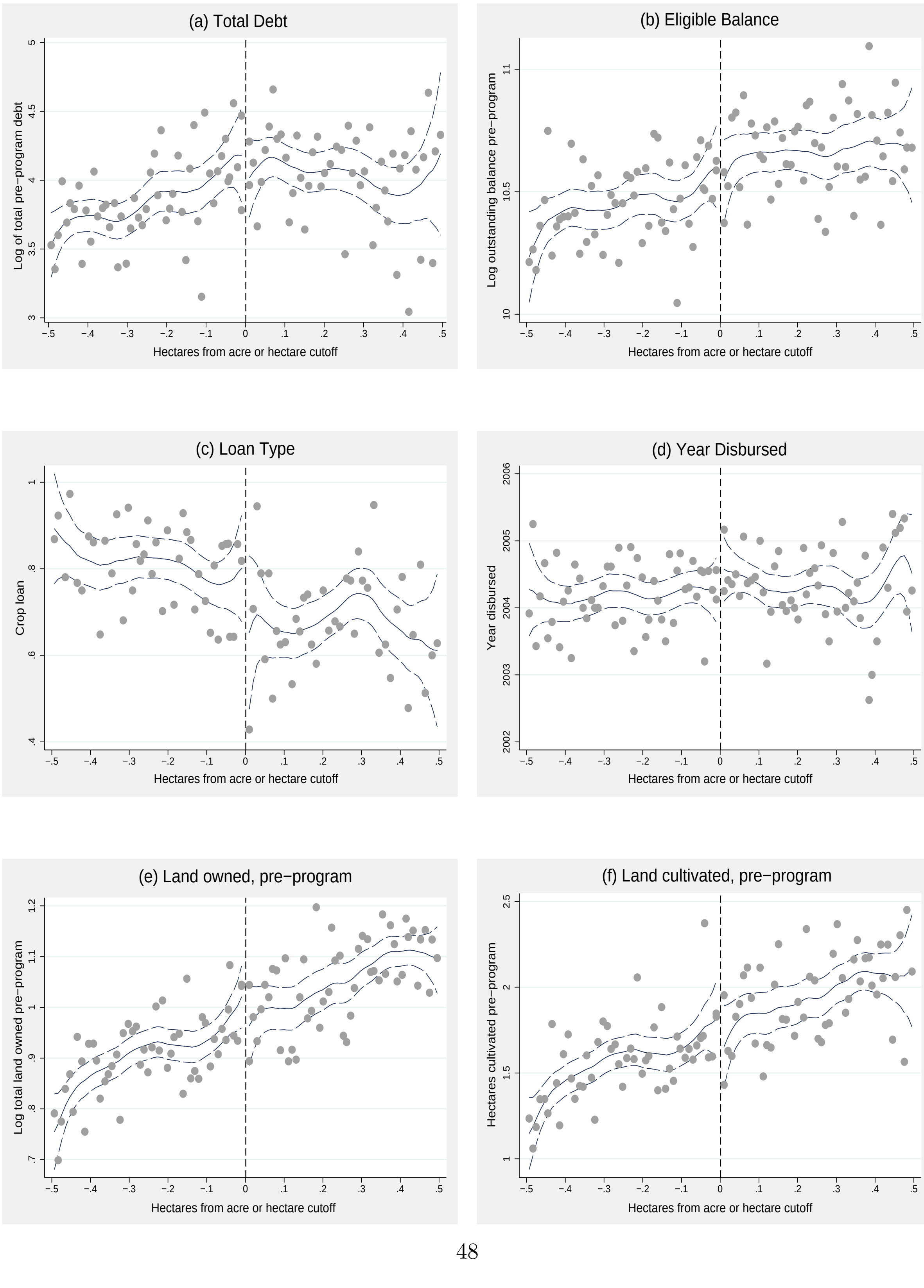
Notes: Panel (a) plots the log relief amount for households benefiting from 100% debt relief (left) and conditional 25% debt relief (right) for all 2,897 surveyed households. The estimated discontinuity at $x = 0$ is $\hat{\gamma} = 1.330$, $\text{SE}(\hat{\gamma}) = -.064$. Panel (b) plots the log relief amount for households benefiting from 100% debt relief (left) and conditional 25% debt relief (right) for all 2,897 surveyed households. The estimated discontinuity at $x = 0$ is $\hat{\gamma} = 1.330$, $\text{SE}(\hat{\gamma}) = -.064$. Dashed lines represent local linear regressions with a bandwidth of .01 ha to each side of the program cutoff.

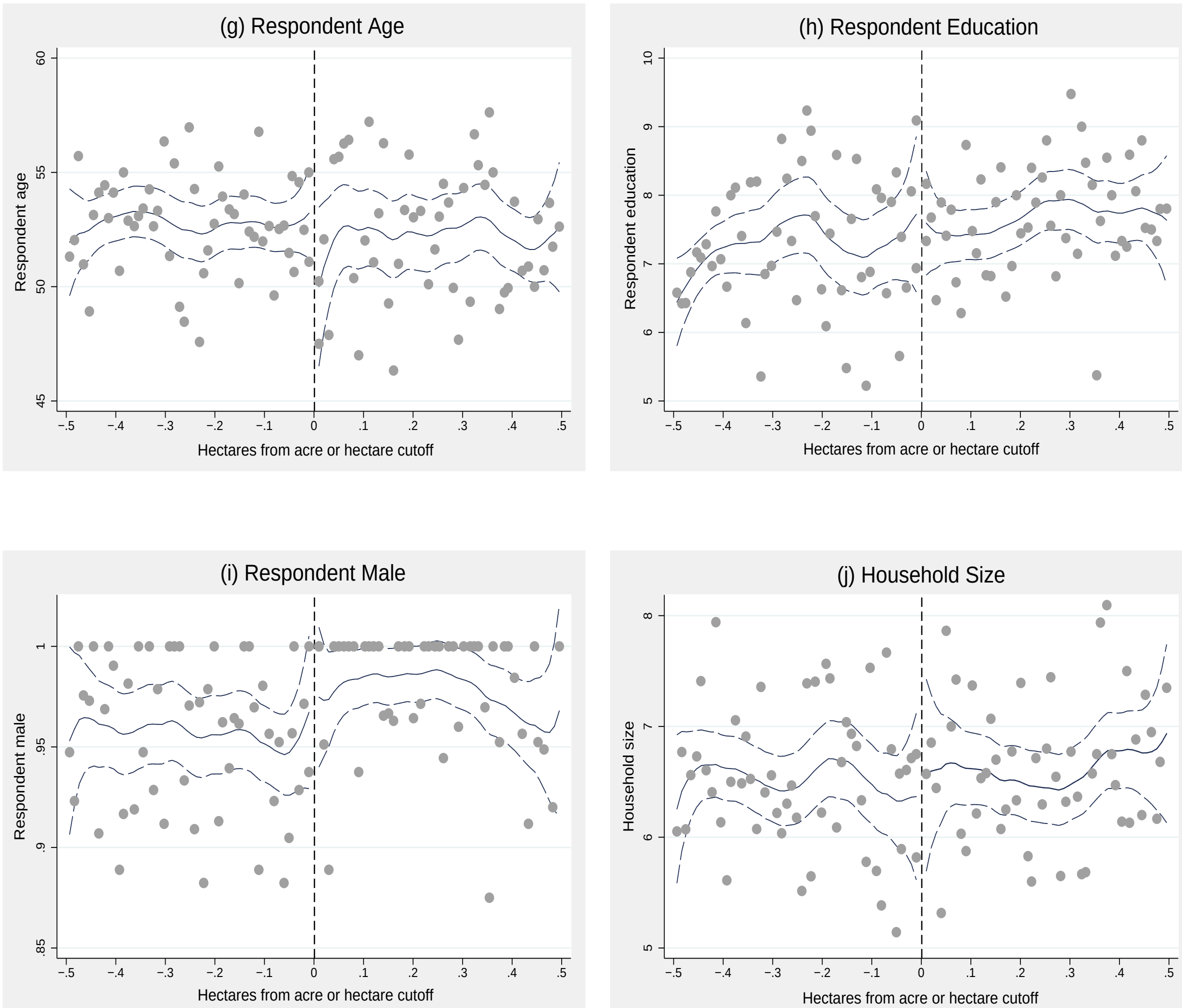
Figure 3: Placebo tests for alternative discontinuities



Notes: The figure shows placebo tests for alternative discontinuities in the forcing variable. The left-hand side shows absolute values of t-statistics obtained from placebo RD regressions at various potential discontinuities. Statistics are averages of the four specifications used in the empirical analysis, estimated within a band of 0.1 ha around each placebo cutoff. The distribution of t-statistics is shown on the right-hand side.

Figure 4: Contiunity of Pre-Program Variables





Notes: Panels (a) – (j) show graphical tests for the continuity of pre-program variables in the vicinity of the program eligibility threshold. Solid lines represent local linear regressions for observations above and below the eligibility cutoff with corresponding 95% confidence intervals.

Supplemental Appendix

Supplemental Appendix

A Data Appendix

Debt Relief: 'Debt Relief' refers to the total amount of debt relief granted as a result of the program. 'Eligible Amount' refers to the total outstanding balance to which the program rules were applied, denominated in Indian Rupees and published on bank beneficiary lists as of February 28, 2008. 'Eligible Amount, Total' is the sum of 'Eligible Amount, Principal' and 'Eligible Amount, Interest'.

Landholdings: Land pledged as collateral at the time of loan origination (a) Bank recorded: land pledged as collateral at the time of loan origination from published beneficiary lists for 5,554 households within $\pm .5$ hectares of the program cutoff (b) Audited land records: printouts of electronic (e-Dhara) land records for a sub-sample of 2,064 surveyed households.

Investment: 'Total investment' includes investment in irrigation, pesticides, non-organic fertilizer and plowing, averaged over one dry season and one monsoon season before the program (pre-program investment) and one dry season and one monsoon season after the program (post-program investment), and winsorized at the 99th percentile. 'Investment per Acre' is calculated as productivity per self reported acres of land prior to the program.

Productivity: Revenue from sale of crops, averaged over one dry season and one monsoon season prior to the program the program (pre-program productivity) and one dry season and one monsoon season after the program (post-program productivity) and winsorized at the 99th percentile. 'Revenue per acre' is measured as total revenue per self-reported acres of land prior to the program.

Total Debt: 'Total debt' refers to self-reported debt from formal and informal sources, at the time of the survey and prior to the program. Formal credit includes loans from private and public banks. Informal credit includes loans from family and friends, moneylenders, traders and shopkeepers.

Sources of Credit: Responses to the question "Of the amount you borrowed for agricultural production, what percentage comes from each of the following sources" (in the current year and one year prior to the program. Formal credit includes loans from private and public commercial banks. Informal credit includes loans from family and friends, moneylenders, traders and shopkeepers.

Expectations: (a) Reputational consequences of default: responses to survey question "If you defaulted on a loan from the following lender, how likely would this be to tarnish your reputation in your community?" (b) access to credit: "If a borrower like you defaulted on one of the following lenders due to a bad harvest or other unforeseen circumstances, how worried does he have to be that he will not be able to borrow from this source in the future?"

Savings and Consumption: 'Savings' refers to total households savings at the time of the survey and in the year prior to the program. Consumption: (a) Total consumption expenditure on staples over the 30-day period prior to the survey (b) Total consumption expenditure on durable goods in the year since the program.

B Theory Appendix

Proof of Proposition 1

In the absence of credit constraints, the household optimizes the first order condition

$$\max_k U(k) = y_1 - k + \theta f(k) - min[s\theta f(\theta), D] \tag{B.1}$$

Noting that the last term depends on the realization of the stochastic productivity parameter θ, the last term of this expression can be written as

$$\upsilon(D,k) = \int_{\underline{\theta}}^{\frac{D}{sf(k)}} \theta\pi(\theta)d\theta + D\int_{\frac{D}{sf(k)}}^{\overline{\theta}} \pi(\theta)d\theta \tag{B.2}$$

Substituting and differentiating yields the first order condition

$$\frac{dU}{dk} = -1 + f'(k)\left[1 - s\int_{\underline{\theta}}^{\frac{D}{sf(k)}} \theta\pi(\theta)d\theta\right] + \left(sf(k)\frac{D}{sf(k)} - D\right)\pi\left[\frac{D}{sf(k)}\right]\frac{Df'(k)}{sf(k)^2} \tag{B.3}$$

Noting that the integration limits are chosen optimally, and using the envelope theorem eliminates the last term. Setting the first order condition to zero yields

$$f'(k)\left[1 - s\int_{\underline{\theta}}^{\frac{D}{sf(k)}} \theta\pi(\theta)d\theta\right] = 1. \tag{B.4}$$

Implicit differentiation of this expression shows that $\frac{dk}{dD} = \frac{Df'(k)\pi\left[\frac{D}{sf(k)}\right]}{U''(k)sf(k)^2} < 0$, since $U''(k) < 0$. This shows that investment is decreasing in the level of inherited debt. ■

Proof of Proposition 2

Suppose now that the household anticipates to face credit constraints as the result of debt relief. This implies that $\overline{k} \leq y_1$, so that the household now solves

$$\max_k U(k) = y_1 - k + \theta f(k) - min[s\theta f(\theta), D] \tag{B.5}$$

$$\text{s.t. } k \leq y_1$$

Following the same steps as above, this yields the first order condition

$$f'(k)\left[1 - s\int_{\underline{\theta}}^{\frac{D}{sf(k)}} \theta\pi(\theta)d\theta\right] - \lambda = 1 \tag{B.6}$$

where λ is the Lagrange multiplier associated with the household's anticipated borrowing constraint. Comparing this to the first-order condition from the unconstrained problem, we see that the anticipation of a future budget constraint will reduce investment. ■

C Integrity of the Assignment Variable

The identification strategy relies on the assumption that there was no manipulation of the assignment variable (land pledged as collateral at the time of loan origination), which would make selection to either side of the discontinuity non-random. This appendix presents several tests verifying the integrity of the assignement variable. As a first test, Figure C.1, Panel (a) plots the density of the forcing variable according to bank records for all surveyed households within a band of ± 1 hectares of the program eligibility threshold in 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 the distribution is plotted separately for commercial and cooperative banks in Figure C.1, Panel (b), we see that this pattern is almost entirely due to the subsample of cooperative bank accounts. McCrary's (2008) test for discontinuity in the forcing variable correspondingly fails to reject the presence of a discontinuity with $p < 0.01$. It is worth noting that the McCrary test also fails to reject discontinuities at 4 and 6 acres, suggesting that bunching at whole numbers is likely to be an important part of the explanation.

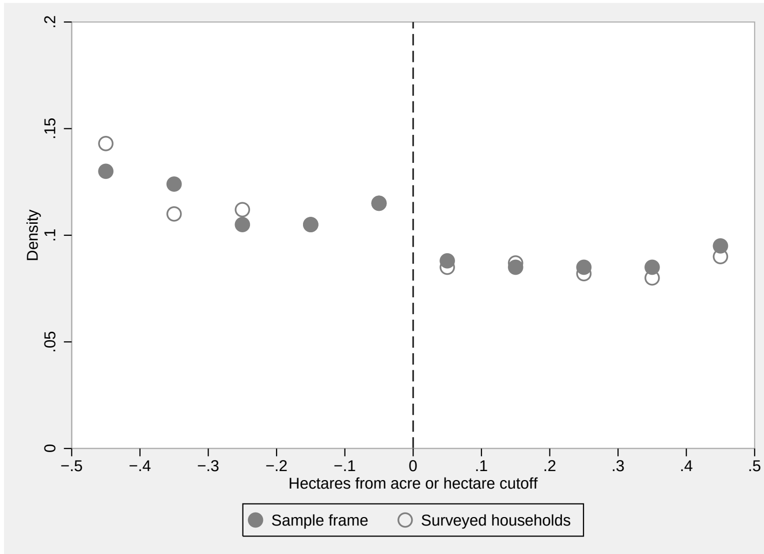
In order to gauge the extent of potential manipulation, and to provide for the possibility of a robustness check using a manipulation-free sub-sample, we audited the official land records of surveyed households, taking advantage of the state of Gujarat's electronic repository of land records. In the state of Gujarat, all official landholdings are recorded in the centralized electronic *e-Dhara* system. Manipulation of *e-Dhara* records is considered highly unlikely for several reasons: 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.

C.1 Evidence from the Audit of Electronic Land Records

To compare the landholding numbers reported by banks and survey respondents, we obtained official copies of the land records of 2,064 of 2,897 survey respondents or 71% of the sample. Table C.1 reports the land audit results. There are several legitimate reasons for electronic 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 identification strategy.

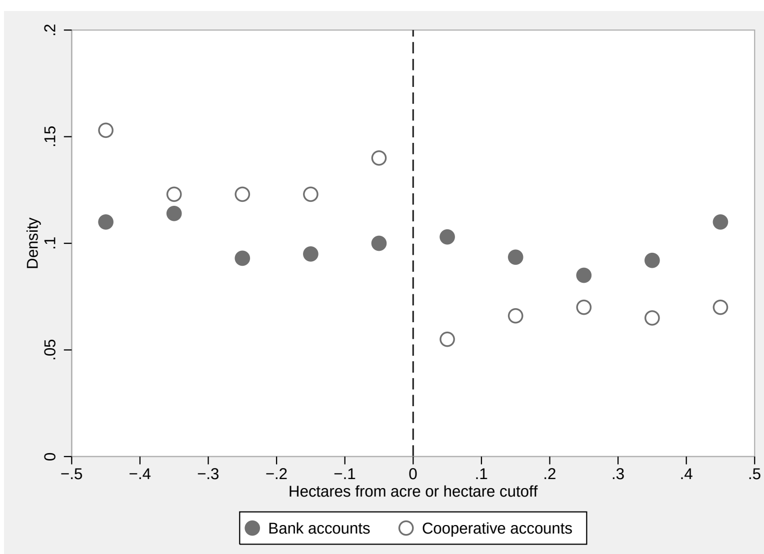
Second, in a smaller number of cases loans considered the landholdings of multiple individuals. Most frequently, land held by multiple members of the same extended household is pooled in order to qualify for a larger loan. In many cases, the loan was recorded as having a single beneficiary, and the total 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

Figure C.1: **Density of the Assignment Variable by Survey Status** – Sample Frame and Surveyed Households



Notes: The figure plots the density of the forcing variable (bank-recorded landholdings) for (a) all households in the sample frame and (b) the sample of surveyed households, in bins of 0.1 hectares within ± .5 hectares around the eligibility cutoff.

Figure C.2: **Density of the Assignment Variable by Bank Type** – Commercial Banks and Credit Cooperatives



Notes: The figure plots the density of the forcing variable (bank-recorded landholdings) for all surveyed households and separately for (a) accounts at commercial banks (b) accounts at credit cooperatives, in bins of 0.1 hectares and a band of ± .5 hectares around the program eligibility threshold.

Table C.1: **Integrity of the Assignment Variable** – Audit of Electronic Land Records

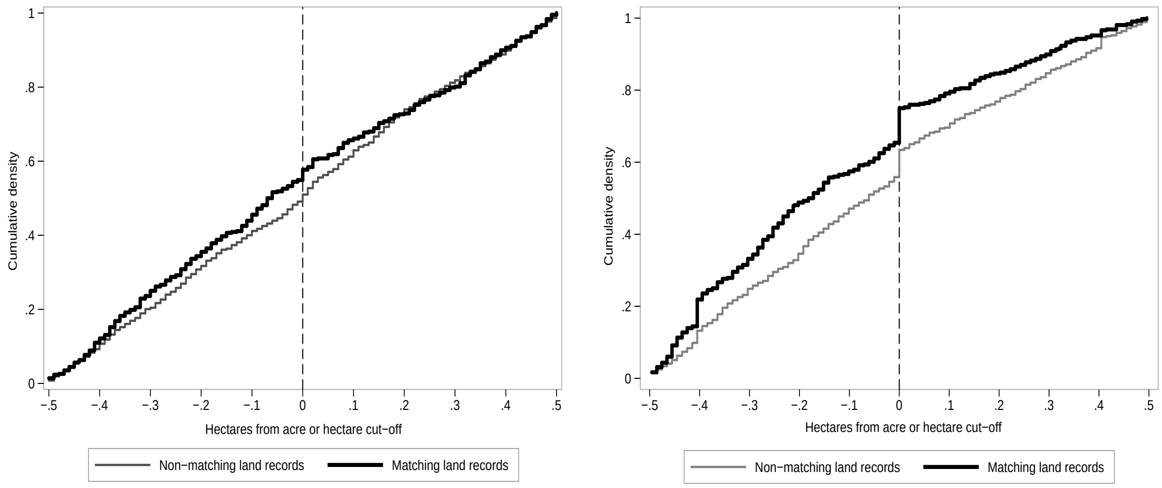
The table reports summary statistics for the sample of 2,064 audited electronic land records by treatment status. The first column reports number and percentages (in brackets) for accounts with audited landholdings greater than the land reported in bank lists. The second column reports on cases where land holdings reported in bank beneficiary lists are lower than the land reported in the household's electronic land record.

	Electronic land record		Total
	≤ bank record (N=1,942)	>bank record (N=122)	
Treatment	1,112	82	1,194
%	[.93]	[.07]	.58
Control	830	40	870
%	[.95]	[.05]	.42
Total	1,942	122	
	[.94]	[.06]	2,064

than the total land held by the farmers. This also is considered legitimate and does not violate the fundamental identification assumption. Third, rounding and conversion errors were common, as landholding can be recorded ina variety of complex and region-specific units of measurement. Since official land 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 therefore allow for 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%, we consider this a match. This match protocol retains considerable power, and both excluding partial-land matches and using a ±1% margin of error has only negligible effects on the overall match rate.

With landholding documents for 71% of surveyed households, manipulation of the forcing variable can be ruled out for 96.1% of audited households. Note that a potential case of manipulation would be one in which the land reported by the bank is *smaller* than the land reported in official documents. We find that this is the case for only 82 households or 3.9% of the audited sample. The rate of exact matches 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 debt relief. The robustness sample used in the analysis excludes all accounts for

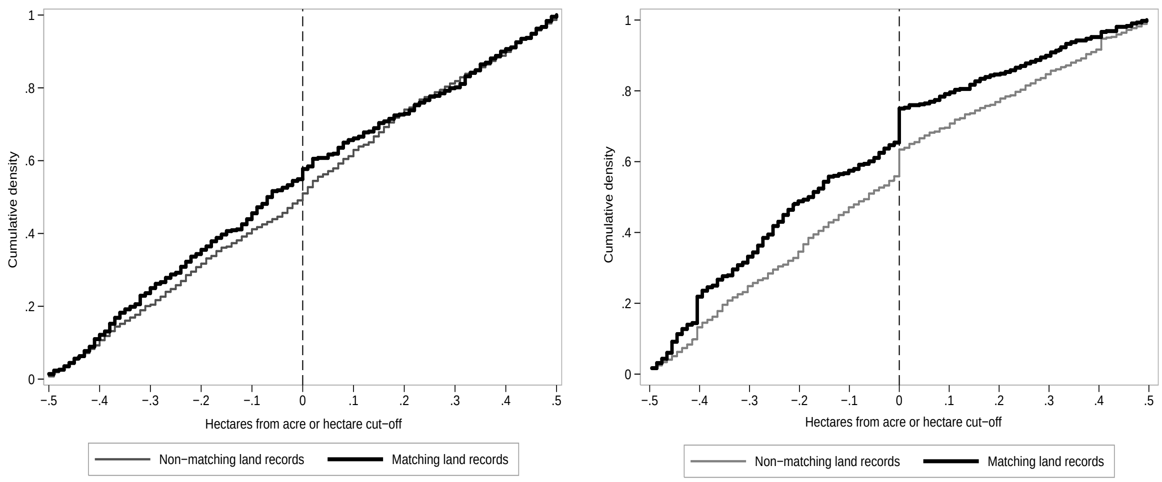
Figure C.3: **Land Distributions by Bank Type and Audit Result** (Audited Accounts, N=2,064)



Notes: Panel (a) plots the cdf of land records by audit result for the subsample commercial bank accounts. Panel (b) plots the cdf of land records by audit result for commercial bank accounts.

which manipulation cannot be ruled out. The reported results are also robust to a more restrictive definition of the robustness sample that excludes non-matching land on both sides of the discontinuity. As an additional test, Figure C.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 bank accounts, shown in Panel (a), 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 Panel (b). 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 strategic manipulation of the running variable in response to the program. As can be seen from Figure C.3 (b), 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 cutoff than to the right. This is precisely the opposite of what one would expect to happen in the presence of manipulation: If there were indeed significant manipulation in the vicinity of the program eligibility threshold, we should be *less* likely to locate official documents for corrupt farmers, and land should match at much lower rates below the cutoff.

Figure C.4: **Land Distributions by Bank Type and Source of Landholding Data**



Notes: Panel (a) plots the cumulative density of land records by source of land data (bank data or audit of electronic land records) for all commercial bank accounts in the sample. Panel (b) plots the cumulative density of land records by source of land data for all cooperative bank accounts in the sample.

Finally, Figure C.4 plots bank-reported and audit-derived landholding distributions for landholdings between 4 and 6 acres. By ignoring whether land matches or not, this allows for a comparison of the raw land distributions, as considered from bank and government sources. The distributions are visually indistinguishable, and a Kolmogorov-Smirnov test fails to reject the equality of distributions 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 borrower manipulation.

D Additional Tables

Table D.1: **Household Survey** – Eligible Population and Sample Frame

The Table reports summary statistics of the sample population and sample frame. Panel A 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. Panel B 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 included in the sample frame.

Panel A: Program Beneficiaries by Bank and District

	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
District total	44,135		3,596	20,762	68,493

Panel B: Sample Frame by Bank and District

	District				
	Anand	Kheda	Gandhinagar	Mehsana	Total
Bank of Baroda	276	276	35	70	657
Bank of India	14%	8%	7%	7%	9%
Central Bank of India	84	95	33	34	246
Dena Bank	10%	11%	10%	8%	10%
State Bank of India	215	39	25	16	295
Union Bank of India	16%	5%	10%	6%	11%
Kaira District Coop Bank	1,442	1,170			2,612
	12%				12%
Total	2,515	2,117	410	512	5,554
	12%			13%	9%

Table D.2: **Household Survey** – Tests for Balance and Attrition

Panel A reports tests for balanced survey coverage. Each column represents results from a separate regression based on the entire sample frame (N=5,554). "Surveyed" is a dummy variable equal to one if a beneficiary household was located and completed the entire survey. Treated is a dummy variable equal to one for all households that had pledged < 2 hectares of land as cllateral and were eligible for 100% debt relief. Panel B presents 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.

Panel A : Survey Coverage

	Surveyed=1	
	(1)	(2)
Treated	0.010	0.024
	[0.01]	[0.03]
Log eligible balance		0.00
		[0.01]
Coop bank loan		0.051
		[0.04]
Crop loan		0.028
		[0.05]
Total land		0.027
		[0.06]
Year disbursed		0.023**
		[0.01]
Observations	5,554	4,808
R-squared	0.001	0.011

Panel B: Test for Balanced Attrition

	Treatment	Control	Difference	
	100% Relief	25% Relief	Coefficient	*SE*
Surveyed	0.551	0.5548	-0.00375	[0.01360]
Deceased	0.1186	0.1026	0.0160+	[0.00859]
Migrated	0.0723	0.0799	-0.00755	[0.00720]
Refused	0.0316	0.0367	-0.0051	[0.00492]
Not located	0.0938	0.1043	-0.0105	[0.00811]
Failed to administer	0.05	0.045	0.005	[0.00582]
Other	0.0827	0.0768	0.00592	[0.00741]

Table D.4: **Regression Results** – Household Savings

This table reports the effect of debt relief on total household savings. Within a panel, each column reports results from a separate regression. The dependent variable is total self-reported savings at the time of the survey. Regressions in panel A are estimated using the full sample of surveyed households. Panels B.1 and B.2 report restricted sample robustness checks. Panel B.1 reports estimates from a robustness sample consisting of housheolds with audited and matching land records. Panel B.2 reports estimates from a reduced bandwidth sample, which excludes observations in the top and bottom 25% of observations to either side of the program cutoff. Interview effects include interviewer and month-of-interview dummies. Respondent controls include gender, age, years of education, household size, log of pre-program land owned, log of pre-program total debt. Robust standard errors, in parentheses, are clustered by bank and district. + p<0.10 ** p<0.05 *** p<0.01.

	Model 1	Model 2	Model 3	Model 4
Panel A: Full Sample	(1)	(3)	(5)	(7)
100% Relief	-0.25	-0.06	-0.12	-0.07
	[0.17]	[0.16]	[0.17]	[0.16]
Observations	2,346	2,346	2,225	2,346
R-squared	0.134	0.122	0.139	0.122
Panel B: Robustness Samples				
Panel B.1: audited sample				
100% Relief	-0.12	0.02	-0.05	0.02
	[0.32]	[0.25]	[0.27]	[0.25]
Observations	1,510	1,510	1,510	1,510
R-squared	0.152	0.128	0.146	0.128
Panel B.2: reduced bandwidth sample				
100% Relief	-0.20	-0.02	-0.03	-0.03
	[0.20]	[0.20]	[0.21]	[0.21]
Observations	1,710	1,710	1,611	1,710
R-squared	0.146	0.135	0.160	0.135
Interviewer effects	Yes	Yes	Yes	Yes
Bank*district effects	No	No	Yes	Yes
Local linear control function	Yes	Yes	Yes	No
Quadratic control function	No	No	No	Yes
Households with positive savings [treatment/control]			2,112	[1,248/864]
Households with zero savings [treatment/control]			279	[166/113]

Table D.3: **Parametric Balance Checks** – Respondent Demographics and Pre-Program Observables

This table reports checks for the continuity of pre-program loan characteristics and demographics at the program cutoff. Dependent variables are the total overdue balance (Rupees, logged) to which the 100% or 25% debt relief was applied, a dummy for crop loans (versus investment loans), year since the loan was disbursed, total self-reported land ownership and total land cultivated before debt relief (both in hectares), respondent age, respondent education (in years), respondent gender and household size. The change between the treatment and control groups at the cutoff is estimated using the baseline specification with a local-linear control function and the standard set of fixed effects, but without additional controls. Robust standard errors, in parentheses, are clustered at the bank*district level. + $p<0.10$ ** $p<0.05$ *** $p<0.01$.

	Loan balance	Loan type	Loan age	Land total	Land cultivated	Age	Education	Male	Household size
100% Relief	-0.019	0.027	0.01	0.012	-0.052	0.311	0.154	-0.030+	0.067
	[0.06]	[0.03]	[0.14]	[0.02]	[0.22]	[0.73]	[0.29]	[0.02]	[0.24]
Observations	2,832	2,832	2,468	2,809	2,793	2,830	2,777	2,831	2,832
R-squared	0.05	0.049	0.038	0.119	0.098	0.034	0.062	0.028	0.054
Interview effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local linear control fctn	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table D.6: **Supply Side** – Ex-Post Access to Credit

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.

Panel A: Summary Statistics on New Loan Applications

	Applied for new loan (1)	New loan approved (2)	Interest rate (3)	Log amount approved (4)
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]		

Panel B: Ex-post Access to Institutional Credit

	Applied for new loan (1)	New loan approved (2)	Interest rate (3)	Log amount approved (4)
100% Relief	0.09	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	0.13	0.179	0.301

Table D.5: **Regression Results** – Household Consumption Expenditure

This table reports treatment effects of debt relief on household consumption. The dependent variables are (a) consumption of staples over the last 30-day period and (b) consumption of durable goods over the last 12 month period after debt relief. Regressions in panel A are estimated using the full sample of surveyed households. Panels B.1 and B.2 report restricted sample robustness checks. Panel B.1 reports estimates from the sample of housheolds with audited and matching land records. Panel B.2 reports estimates from a reduced bandwidth sample, which excludes observations outside the bottom 25% of observations below and the top 25% of observations above the program cutoff. Respondent controls are as previously defined. Loan controls include years since disbursal of the loan and its interaction with treatment status and a dummy variable equal to one for loans from cooperative banks. Standard errors, in brackets, are clustered by bank and district. + $p<0.10$ ** $p<0.05$ *** $p<0.01$.

	Model 1		Model 2		Model 3		Model 4	
	Basics [30-day]	Durable goods	Basics [30-day]	Durable goods	Basics [30-day]	Durable goods	Basics [30-day]	Durable goods
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% Relief	-0.08+	-0.54**	-0.02	-0.21	-0.04	-0.24	-0.02	-0.21
	[0.05]	[0.25]	[0.04]	[0.22]	[0.03]	[0.26]	[0.05]	[0.22]
Observations	2,832	2,832	2,832	2,832	2,687	2,687	2,832	2,832
R-squared	0.155	0.403	0.15	0.382	0.373	0.404	0.15	0.382
Panel B: Restricted Sample								
Audited accounts								
100% Relief	-0.14**	-0.80**	-0.08	-0.44	-0.10**	-0.47	-0.08	-0.43
	[0.06]	[0.31]	[0.07]	[0.37]	[0.05]	[0.37]	[0.07]	[0.37]
Observations	1,805	1,805	1,805	1,805	1,713	1,713	1,805	1,805
R-squared	0.156	0.397	0.151	0.373	0.377	0.394	0.152	0.373
Reduced bandwidth sample								
100% Relief	-0.08	-0.47+	0.00	-0.12	-0.02	-0.2	0.00	-0.12
	[0.05]	[0.28]	[0.05]	[0.21]	[0.04]	[0.25]	[0.05]	[0.21]
Observations	2084	2084	2084	2084	1964	1964	2084	2084
R-squared	0.156	0.389	0.16	0.374	0.379	0.397	0.159	0.374
Interviewer effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Respondent controls	No	No	No	No	Yes	Yes	No	No
Local linear control fctn	No	No	Yes	Yes	Yes	Yes	No	No
Quadratic control fctn	No	No	No	No	No	No	Yes	Yes

Table D.7: **Regression Results** – Sources of Credit, Detail

This table reports the effect of debt relief on the composition of borrowing. The dependent variables are changes in the self-reported percentage of financing obtained from each source. Within a panel, each column reports results from a separate regression. Regressions in panel (A) use the sample of all surveyed households, panel (B.1) is estimated using the of housheolds with audited and matching land records. Estimates in panel (B.1) use a reduced bandwidth sample, which excludes the bottom 25% of observations below and the top 25% of observations above the program cutoff. All additional controls are as previously described. Robust standard errors, in parentheses, are clustered by bank and district. + p<0.10 ** p<0.05 *** p<0.01.

	Model 1				Model 2				Model 3				Model 4			
	Bank	Coop	Money-	Family	Bank	Coop	Money-	Family	Bank	Coop	Money-	Family	Bank	Coop	Money-	Family
Panel A:		Bank	lender	Friends		Bank	lender	&Friends		Bank	lender	&Friends		Bank	lender	&Friends
Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
100% Relief	4.80+	–11.32***	0.74	3.35**	0.88	–5.98**	0.33	3.24**	0.46	–6.17**	0.47	3.22+	0.83	–5.90**	0.35	3.25**
	[2.58]	[2.50]	[1.27]	[1.62]	[2.80]	[2.59]	[1.29]	[1.45]	[2.83]	[2.49]	[1.45]	[1.67]	[2.79]	[2.54]	[1.32]	[1.47]
Observations	2,700	2,693	2,691	2,688	2,700	2,693	2,691	2,688	2,560	2,553	2,551	2,549	2,700	2,693	2,691	2,688
R-squared	0.2	0.25	0.03	0.13	0.20	0.21	0.03	0.12	0.21	0.21	0.03	0.13	0.20	0.21	0.03	0.12
Panel B: Robustness Samples																
Audited accounts																
100% Relief	5.27	–11.58***	1.52	3.19+	1.36	–5.29+	0.93	2.91+	0.36	–4.99**	1.24	3.16+	1.4	–5.29+	0.94	2.86+
	[3.16]	[3.00]	[1.29]	[1.67]	[3.30]	[2.69]	[1.34]	[1.60]	[2.93]	[2.48]	[1.50]	[1.62]	[3.31]	[2.70]	[1.37]	[1.60]
Observations	1,726	1,723	1,721	1,719	1,438	1,436	1,434	1,434	1,438	1,436	1,434	1,434	1,370	1,363	1,370	1,363
R-squared	0.24	0.26	0.05	0.14	0.27	0.32	0.07	0.16	0.24	0.25	0.07	0.15	0.10	0.10	0.10	0.10
Reduced bandwidth sample																
100% Relief	7.24**	–11.64***	0.29	2.53	2.99	–5.97**	–0.36	2.20	2.35	–6.00**	–0.26	2.11	2.82	–5.94**	–0.33	2.24
	[2.82]	[2.60]	[1.55]	[1.80]	[3.00]	[2.68]	[1.57]	[1.70]	[3.02]	[2.73]	[1.86]	[1.72]	[2.98]	[2.69]	[1.63]	[1.74]
Observations	2,002	1,995	1,993	1,991	2,002	1,995	1,993	1,991	1,885	1,878	1,876	1,874	2,002	1,995	1,993	1,991
R-squared	0.21	0.23	0.05	0.14	0.22	0.23	0.05	0.13	0.23	0.23	0.05	0.14	0.22	0.23	0.05	0.13
Interview FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank*district FE	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	No	No	No	No
Local linear	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Quadratic	No	No	No	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes