Wealth and Well-being
Lessons from Indian Debt Relief*

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April 23, 2012

Abstract. A fundamental prediction of economic theory is that greater wealth causes greater well-being. This paper tests that prediction. It uses a natural experiment to estimate the causal effect of income on subjective well-being. Among a population of indebted farmers in rural India, the marginal effect of income on life satisfaction is found to be positive. However, the source of income appears to exert an important and independent effect. In this study the source is debt relief, which features a positive marginal effect but also a countervailing negative effect (perhaps due to stigma).

Keywords: Subjective well-being, welfare, wealth, income, debt relief, Easterlin paradox

JEL classification codes: O10, O12, D63, I31

*This project was funded by grants from the Weatherhead Center for International Affairs, Empowerment Lab, South Asia Initiative, Center for International Development, Institute for Quantitative Social Science, and Graduate Student Council, all at Harvard University. The author is grateful to Martin Kanz for collaboration on the survey and dataset; Maulik Chauhan for excellent field project management; Rohini Pande and Richard Zeckhauser for invaluable support and guidance throughout; Andrew Oswald for helpful suggestions; and seminar participants at Harvard University, the University of Cape Town, the BREAD-Verona-Trento Summer Workshop in Development Economics, the Association for Social Economics World Congress, and the Institute for Financial Management and Research for helpful comments. Four anonymous referees also helped to strengthen the paper. The usual disclaimer applies.

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1 Introduction

The relationship between income and experienced well-being has been the subject of debate for millenia. It has featured in Aristotle's reflections on ethics (Aristotle, 1891), in the first purported book of economics (the Indian *Arthashastra* of the fourth century B.C., as discussed in Rich, 2010), and in the discussions of philosophers, statesmen, and economists to the present day. While rhetoric and observed correlation have predominated in the discussion thus far, in this paper I take a different tack. By employing modern econometric techniques to exploit a natural experiment, I estimate the actual causal effect of income for a particular population of farmers in rural India.

My analysis considers an agricultural debt relief program launched by India in 2008, the largest such program ever undertaken. 710 billion rupees (about $14.6 billion) worth of agricultural debt was formally waived and tens of millions of Indian farmers benefited. Average relief amounted to over Rs.17,000, roughly half the annual consumption expenditure of the average rural household.

Two features of this program made it particularly well suited to analysis. First, for most beneficiaries qualification was based upon a sharp landholding cut-off. Farmers owning land below a certain threshold received 100% relief, while farmers above that threshold qualified for only 25% relief. This allows me to estimate the causal effect of 100% relief by treating it as a natural experiment and using the landholding cut-off in a regression discontinuity design. Second, the 25% relief was contingent upon repayment of the remaining 75% balance. Because a large majority of 25%-relief beneficiaries in fact repaid their remaining balances, this allows me to infer a lower-bound valuation for full relief in terms of present-value wealth. Otherwise, it might have been difficult to value relief, as the value to beneficiaries could have been substantially less than face value.

The key outcomes I consider are self-reported happiness and life satisfaction – collectively known as subjective well-being (SWB) – measured roughly 18 months post-relief.[1] I follow a growing number of economists in considering these SWB measures as broad indicators of experienced well-being (see, e.g., discussions and reviews in Frey and Stutzer, 2002; Di Tella and MacCulloch, 2006; Loewenstein and Ubel, 2008; Fleurbaey, 2009). While there has been a substantial body of empirical work on the income-SWB relationship (e.g., Easterlin, 1974; Stevenson and Wolfers, 2008), it has tended to be correlational in nature. My use of micro data from a natural experiment allows for a more focused consideration of the causal relationship.

Estimation of the income-SWB relationship proceeds here in three steps: (1) estimating the causal effect of debt relief on happiness and life satisfaction; (2) deriving conservative upper and lower bounds for the

[1] Kanz (2011) considers other effects of the same debt relief program on the same sample of farmers.

value of that relief in terms of present-value wealth and then annual income; and (3) estimating the effect of income on SWB with an instrumental variables approach, using debt relief as an instrument for post-relief income.

The remainder of this paper is organized as follows. The following section provides additional background on both the related literature and the debt relief program in question. Section 2 introduces the data and empirical strategy. Section 3 presents results, including cross-sectional relationships and possible channels of effect. Section 4 concludes with a short discussion. Appendices provide additional details on the data as well as a series of robustness checks.

1.1 Background

1.1.1 Wealth and well-being

Because the income-well-being relationship sits squarely at the crossroads of choice- and experience-based measures of welfare, it is situated within the broader debate over how to best assess welfare. In this context, Köszegi and Rabin (2008) lay out the basic case against welfare inferences drawn exclusively from choice behavior. Their conclusion that "choice behavior alone can never reveal which situations make people better off, even with unlimited data and under the maintained hypothesis of 100% rational choice" rests heavily on the theoretical implications of the empirical phenomenon that individuals' preferences are choice-set-dependent (also called menu-dependent). Along the same lines, an earlier and more thorough theoretical critique can be found in Sen (1977). Relaxing the assumption of rationality only strengthens the argument.

Köszegi and Rabin suggest, however, that choice is in fact a sufficient basis for welfare inference under certain "ancillary assumptions" that are likely to hold in many contexts. The trick is figuring out when these assumptions do or do not hold. When necessary, they suggest complementing choice-based measures with alternative approaches to the measurement of well-being, such as the measurement of happiness. They take, in fact, happiness or well-being to be the ultimate aim of public policy (p. 1824), but this is by no means an uncontested notion. Sen (2009) and Nussbaum (2008) have, for example, leveled a variety of conceptual objections. Fleurbaey (2009), Stiglitz et al. (2009), Thinley (2007), and Diener and Seligman (2004) discuss the issue from the perspective of replacing or complementing GDP as an indicator of social welfare or economic progress.[2] Broader reviews of the "happiness economics" literature include Frey and Stutzer (2002), Di Tella and MacCulloch (2006), and Graham (2005).

Rayo and Becker (2007b) have taken happiness as "the fundamental goal of individuals in making decisions" (p. 487), suggesting that utility maximization and happiness maximization are effectively the same. In later work, however, they have suggested that happiness might only be one term in the utility function, with happiness traded off against other goals (Becker and Rayo, 2008). This would seem to bring them closer to Sen and Nussbaum, both of whom suggest that there is more to consider beyond happiness.

As it turns out, the psychological literature on subjective well-being (SWB) has long posited more than a single dimension (e.g., Diener, 1984; Diener et al., 1999). One dimension, most closely associated with self-report measures of happiness, is primarily hedonic in nature; it is variously associated with "positive affect" or "emotional well-being." Another, most closely associated with self-report measures of life satisfaction, is

[2] The issue of time inconsistency, as discussed in O'Donoghue and Rabin (1999), presents a particularly thorny normative puzzle. While time-inconsistent behavior undercuts choice-based welfare inferences, it also complicates experience-based inferences. When experiences of happiness and perspectives on satisfaction change over time, when is the right time to measure them? Which "self" matters most? Thus, we have issues with *intra*personal comparisons of welfare, to add to the interpersonal variety.

more cognitive in nature; it is variously associated with "judgment" or "evaluation." In this paper I call these dimensions, simply, happiness and life satisfaction.[3]

Subjective and objective measures of well-being correlate well enough to suggest that subjective measures are fundamentally meaningful (i.e., they are more than just noise and framing effects), but not so well that they do not offer any additional information. For example, Oswald and Wu (2009) find that self-report life satisfaction data show a correlation of 0.6 with revealed-preference-based data on compensating differentials. This correlation is high enough to lend credibility to the subjective data, but the R^2 of 0.36 suggests that much variation is left unexplained.

The relationship between income and SWB has been of particular interest within the economics literature. Most modern inquiry into the subject responds to or follows from Easterlin (1974). Easterlin suggested that richer countries were no happier than poorer countries, even though richer individuals were happier than poorer ones within any given country. This stylized fact has come to be known as "the Easterlin paradox" (but not without challenge, as in Veenhoven and Hagerty, 2006; Stevenson and Wolfers, 2008). The United States has been an important case in point, where richer individuals have tended to report being happier than poorer ones, but tremendous post-war growth in per-capita income has not been matched by a corresponding rise in average happiness.

Easterlin discussed relative comparison as one plausible explanation for this pattern of evidence. If individuals judge themselves in relation to their peers, then a rising tide need not raise all happiness. Empirical studies such as Luttmer (2005) have lent support, as have theoretical exercises. Rayo and Becker (2007a), for example, show that a "happiness function" that measures success in relative terms is most efficient from an evolutionary perspective. The relativity of happiness has, of course, also been the subject of considerable study within the field of psychology (e.g., Parducci, 1995). In both the economics and the psychology, relativity with respect to one's own past, or habituation, has also been considered alongside relativity with respect to others. Habituation may also play some part in the Easterlin paradox (see, e.g. Clark et al., 2008). Alternatively, other omitted-variable problems may render the cross-sectional relationships misleading.

There is a vast and growing body of empirical work on the income-SWB relationship. Most follow Easterlin in considering cross-section or panel data, as in Di Tella and MacCulloch (2008), Layard et al. (2008), Hagerty and Veenhoven (2003), and Stutzer (2004). These studies tend to establish correlation, however, rather than causation. After all, one can tell many plausible stories for why both income level and income growth would be endogenous; plausibly exogenous variation in income is difficult to come by.

[3]Certainly other formulations are possible. For a richer discussion of happiness measures and possible sub-components, see, for example, Seligman (2002) and Schueller and Seligman (2010).

However, some have come considerably closer to causal estimates. For example, Gardner and Oswald (2007) consider longitudinal data for lottery winners in the UK. While they find a positive income-SWB relationship, it is based on a very small number of lottery winners.

Stevenson and Wolfers (2008) re-evaluate the Easterlin paradox by considering a wide range of data and methods. They present a variety of evidence against Easterlin's findings, summarizing the income-SWB relationship with a single meta-estimate of 0.4.[4] They present their estimate as broadly consistent across both within- and across-country analyses, find no evidence of a "satiation point" beyond which SWB no longer rises with income, and suggest that the relationship is similar for both happiness and life satisfaction (once these measures are standardized to a common scale). This pits their findings against a host of earlier studies, a fact that they confront in their discussion. The latest analysis by Kahneman and Deaton (2010), for example, finds important differences between happiness and satisfaction measures: the former feature satiation at a certain income level, while the latter do not.

The present analysis brings micro-level evidence to bear, using a natural experiment to provide a causal estimate of the income-SWB relationship for a particular population of farmers in rural India.

1.1.2 Debt relief

India's 2008 Agricultural Debt Waiver and Debt Relief program was partly motivated by a highly visible increase in farmer suicides, most notably in the Vidarbha region of Maharashtra, where high indebtedness among farmers was an oft-cited factor. The economic theory of *debt overhang* (Ghosh et al., 2000) may have also provided motivation, with the expectation being that lower indebtedness would increase the efficiency of investment across the agricultural sector. As a sizable transfer to over 36 million farmers before national elections, the program may have also served other political purposes. Because it reimbursed banks and cooperatives for bad loans, the program was also popular with these lenders, and may have helped to revive financially troubled institutions.

The program considered formal agricultural debt issued by cooperative, commercial, and rural banks. This included crop loans, investment credits for direct agricultural purposes, investment credits for purposes allied to agriculture (e.g., dairy), and agricultural debt restructured under prior debt restructuring programs. Debt to moneylenders, relatives, and other informal lenders, as well as any loans taken for non-agricultural purposes, was not included in the program.

To qualify for relief, a loan had to be overdue or restructured as of December 31, 2007 (well prior to the program announcement). The amount of relief depended on the location and classification of the borrower,

[4]This 0.4 estimate represents the coefficient on the logged income or per-capita GDP term when regressing SWB on logged income and a variety of other controls. Stevenson and Wolfers find similar results whether using ordered probit or OLS with a standardized SWB measure, as in the present analysis.

with farmers qualifying for either a full 100% waiver or a more limited 25% relief conditional on repayment of the remaining 75%. As shown in Table 1, "small and marginal farmers" received a full waiver, while "other farmers" received the conditional 25% relief. In drought-prone and other specially-designated districts, the partial relief was 25% or Rs. 20,000, whichever was greater.[5]

Table 1: Extent of debt relief by classification and location

	Regular districts	**Special districts**
Small/marginal farmers	100% waiver	100% waiver
Other farmers	25% relief (only if remainder repaid)	25% or Rs. 20,000 relief (whichever is greater; only if remainder repaid)

Farmer classification depended on the type of loan. For direct agricultural loans, classification was based on the total landholdings of the farmer at the time the loan was written. Farmers with two or fewer hectares of total land were classified as small or marginal; farmers with more than two hectares were classified as other farmers.[6] In practice, banks often implemented using "total landholdings on file," which could have been more or less than the total land owned by the borrower.[7] For allied-to-agriculture loans, farmers with loans Rs.50,000 and under were considered small or marginal, while farmers with larger loans were considered other farmers.

Across India, more than 36 million farmers qualified for relief totaling over 650 billion rupees (IndiaStat and Rajya Sabha, 2008). Implementation began on June 30, 2008, with full waivers being granted immediately. 25% relief was granted upon repayment of the remaining 75%, with an initial deadline of June 30, 2009. This deadline was extended by one year in order to accommodate those who had trouble repaying their 75%. The goal was 100% participation.

[5]Many districts qualified for this extra relief. In Gujarat state, 20 of 26 districts qualified.

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

[7]The estimation implications of this distinction are discussed in Appendix B.1 and Appendix B.3.

2 Empirical strategy

I employ a regression discontinuity (RD) design to identify the effect of debt relief and an instrumental variables approach to identify the effect of income more generally. I define the sample frame and relevant discontinuity using administrative data from participating rural banks, then use household survey data to assess outcomes. Finally, I use official land records to audit the accuracy of bank-reported landholdings.

2.1 Data

The analysis focuses on a particular population of farmers in rural India. These are farmers who (a) lived in a particular geographical area (the central-northwest part of Gujarat, a state in Western India); (b) held formal agricultural loans with one of the leading banks serving the area; (c) had total landholdings within a certain range; (d) held loans for which these landholdings were determinant of debt relief qualification; and (e) were overdue on their loan payments as of the end of 2007, therefore qualifying for debt relief. The landholding range is used to focus the analysis on only those farmers just above and below the cut-off for 100% relief. In all, there are 5,554 farmers within the sample frame.[8]

The analysis relies upon data from three sources. The first is the banks themselves. As an anti-corruption transparency measure, banks were required to publicly post details about all qualifying debt relief beneficiaries. This included the name, landholding, village, loan category, date of original disbursal, overdue principal and interest as of December 31, 2007, and eligible relief amount. All of this was posted to the notice boards of participating bank branches, and several banks also posted the information on their websites.

The second source is a detailed household survey conducted in late 2009, roughly one and a half years after the implementation of the debt relief program.[9] Of the 5,554 farmers in the sample frame, 2,897 were surveyed. Appendix A includes details on the survey itself as well as attrition, which appears balanced across treatment categories.

The final source is Gujarat's *e-Dhara* repository of official land records. Because of the importance of farmer landholding to the RD design, we audited the official land records of a majority of surveyed households. More details on the land audits can be found in Appendix B.

For measuring subjective well-being, the household survey employed standard survey items so that results could be compared with prior findings. The broadest happiness question is the same as the one used for decades in both the U.S. General Social Survey and the World Values Survey:

Keeping everything in mind, tell us about yourself overall: are you very happy, quite happy,

[8] Appendix A discusses the sample frame in greater detail.
[9] This survey was conducted in collaboration with Martin Kanz.

not very happy, *or* *not at all happy?*

The broadest life satisfaction question is the one recently used by the Gallup World Poll in over 150 countries, administered with the aid of a visual ladder scale and based upon "Cantril's ladder" (a variation of Cantril's self-anchoring striving scale, Cantril, 1965):

> *Please imagine a ladder with steps numbered from zero at the bottom to ten at the top. Suppose we say that the top of the ladder represents the best possible life for you and the bottom of the ladder represents the worst possible life for you.*
>
> *On which step of the ladder would you say you personally feel you stand at this time, assuming that the higher the step the better you feel about your life, and the lower the step the worse you feel about it? Which step comes closest to the way you feel?*

Figure 1 shows the distributions of happiness and life satisfaction for surveyed respondents. Figure 2 shows the joint distribution. With a coefficient of correlation of only 0.22, the two measures are not as highly correlated as might be expected,[10] which is consistent with the view that they measure distinct dimensions of SWB. Surprisingly, neither measure is significantly correlated with what the respondent happened to be doing in the 15 minutes prior to survey administration. Whether respondents had been working, playing, or relaxing does not appear to have systematically affected their SWB responses.[11]

Note that these SWB scales are ordinal in nature. To put them into common units, the SWB measures are standardized throughout the analysis. That is, each SWB variable is de-meaned and divided by its standard deviation. Thus, all measures are in standard-deviation units. The analysis presented here uses linear least-squares methods because they are more mathematically and analytically straightforward, but ordered-probit variations of key results are qualitatively similar throughout. See Stevenson and Wolfers (2008) for a discussion of the ordered-probit vs. the standardized-linear approaches.

2.2 Identification

The identification strategy employed here builds upon a regression discontinuity (RD) design in order to estimate the causal effect of debt relief, then the effect of income more generally. Landholding—defined as

[10]Economists often treat these two measures as essentially interchangeable, even within a single analysis. Also, the measures appear in close proximity on the survey, which we might expect to push toward consistency in response.

[11]Many economists are quick to object to SWB on the grounds of the so-called impossibility of interpersonal welfare comparisons. Before setting such concerns aside, I have two comments. First, interpersonal welfare comparisons are ubiquitous in economics and not nearly so impossible, as discussed in Sen (1982). Second, under certain assumptions comparing group means might be less problematic than comparing individuals, thus making inter-group comparisons somewhat less problematic than interpersonal comparisons. Imagine, for example, a distribution of types with respect to the experience or reporting of well-being. As long as treatment selection is orthogonal to that distribution and thus the same distribution obtains in both the treatment and control groups, then means can be safely compared. Interpersonal differences will tend to attenuate estimates, not bias them in a particular direction.

"hectares from cut-off" so that the discontinuity is at zero—is used as the forcing variable. Presuming that banks followed the rules of the debt relief program faithfully, this RD design is of the *sharp* variety.[12] In order to maximize power and limit bias, the sampling strategy focuses the analysis exclusively on observations close to the cut-off.

As discussed in the background above, the debt relief program design featured a strong discontinuity in relief eligibility at the statutory landholding cut-off (which was either five acres or two hectares, depending on the bank). Those to the left of the cut-off received 100% relief and those to the right qualified for only 25%, conditional on repayment of the remaining 75%. Figure 3 shows the distribution of qualifying relief, which illustrates the strong discontinuity.

The fundamental assumption of the RD approach is that potential outcomes are continuous in the forcing variable. Formally, both $E\left[Y_0|X=x\right]$ and $E\left[Y_1|X=x\right]$ must be continuous in the forcing variable X. If this assumption holds around the cut-off, then any discontinuity in outcomes observed at the cut-off can be attributed to the discontinuity induced by the treatment (in this case, debt relief). For more on the RD approach employed here, see Imbens and Lemieux (2008).

Appendix B discusses threats to the continuity assumption that obtain in the present analysis, provides empirical checks for continuity and balance, and presents robustness checks for all key results. The analysis presented there motivates a set of "other controls" that provide conditional continuity when unconditional continuity is in question.[13]

For the RD design, regression specifications are straightforward. For some outcome Y, β in the following specification is the local average treatment effect (LATE), where treatment is 100% debt relief vs. the offer of contingent 25% relief. The ATE is local because it applies only at the discontinuity. Strictly speaking, in this case the LATE applies only to farmers with around two hectares of land in the sampled districts who had overdue debts with the sampled banks and within the sampled categories.

$$Y_i = \alpha + \beta T_i + \gamma_1 H_i + \gamma_2 H_i T_i + \varepsilon_i \tag{2.1}$$

In this specification, T_i is the treatment indicator (1 if below the landholding cut-off, 0 if above), H_i is the landholding variable (hectares from cut-off), and β is the treatment effect. The γ terms capture the slopes, allowing them to differ on either side of the cut-off. When run with only observations within a narrow band around the cut-off, this regression is effectively the same as running local linear regressions on either side of

[12] Several mechanisms were meant to assure faithful implementation of the debt relief program. Banks themselves had multiple levels of auditing, then the central bank and other regulators performed additional audits. In addition, I test for robustness to corruption concerns by auditing official land documents, as discussed in Appendix B.

[13] Specifically, there are slightly more women interviewed in the treatment group than in the control, so respondent gender is one control. Also, there was a government land distribution program that issued 5-acre plots to many farmers in a single village, so a fixed effect for that village has been included. Finally, fixed effects for landholding audit outcome are included. A more complete discussion and additional robustness checks can be found in Appendix B.

the cut-off. Because the treatment definition is insensitive to whether 25%-relief beneficiaries actually avail the option for relief, the estimator can be thought of as an intent-to-treat (ITT) estimator. When adding bank $\times$ district, interviewer, and month-of-interview fixed effects in the ϕ's and a vector of other controls in X_i, the specification becomes:

$$Y_i = \alpha + \beta T_i + \gamma_1 H_i T_i + \gamma_2 H_i + \phi_{bd} + \phi_j + \phi_t + \theta X_i + \varepsilon_i \tag{2.2}$$

I present key results with and without the vector of controls, X_i. For robustness, I also present results in both unweighted and weighted forms. Weighted regressions re-weight observations based upon the original distribution of bank accounts within the sample frame, before the sample frame was restricted by landholding and attrition. For example, 43% of beneficiaries hold KDCC loans before landholding and attrition, but the proportion among surveyed farmers is 49%. The weighted regressions reduce the weight of KDCC observations and raise the weight of other observations, according to the original distribution of loans.

Importantly, there was not a single, homogeneous treatment. Rather, farmers received relief according to the widely varying sizes of their overdue balances. In order to estimate the potentially heterogeneous treatment effect, specification (2.2) is extended as follows:

$$Y_i = \alpha + \beta_1 T_i + \beta_2 lnB_i T_i + \gamma_1 H_i T_i + \gamma_2 lnB_i + \gamma_3 H_i + \phi_{bd} + \phi_j + \phi_t + \theta X_i + \varepsilon_i \tag{2.3}$$

In this specification, lnB_i is the logged balance overdue, β_1 is the base treatment effect, and β_2 is the additional marginal treatment effect. Here, the overall treatment effect can depend partially or even entirely on the magnitude of relief. This specification allows for there to be an underlying relationship between the outcome variable and the overdue balance size, identifying the heterogeneous treatment effect from the difference in that relationship above vs. below the cut-off (conditional on the forcing variable, etc.).[14] Because the balance term is logged, β_2 is most easily interpreted as the effect of proportional changes. For example, a 3x increase in balance size (roughly 1 log point) causes a β_2 shift in the outcome variable among the treated.

The above specifications consider the effect of receiving 100% relief above and beyond any effect of the 25%-relief offer (elsewhere, "the effect of debt relief"). Since many of the "untreated" farmers in fact availed the offer of 25% relief, they received many of the overall benefits of debt relief (namely, cleared balances and access to new loans). The key distinction between treated and untreated, then, was the cost at which benefits were made available. By receiving their benefits *gratis*, treated farmers experienced the equivalent of an income boost.

[14] I can further narrow estimation of the marginal effect to the point of the discontinuity by further interacting the balance term with the forcing variable (in addition to the treatment indicator). This reduces power somewhat, complicates presentation of the results, but does not alter any of the key findings. Therefore, the simpler specification is used throughout.

To consider the effect of income more directly, debt relief can be used as an instrument for post-relief income in a two-stage least squares (2SLS) set-up. The estimation procedure involves three steps: (1) valuing debt relief in income terms, (2) aggregating it into the household income measure, and (3) instrumenting that income with the variation induced by the discontinuity in debt relief. Post-relief income—the prior year's household income inclusive of debt relief—is represented by y_i. This is then instrumented with a single instrument, the lnB_iT_i term from equation (2.3), to capture the variation attributable to the effectively exogenous variation in debt relief (conditional on the forcing variable, fixed effects, and other controls).[15] The full specification is as follows, with instruments and instrumented terms in bold:

$$\begin{aligned} \mathbf{lny_i} &= \delta + \eta_1 T_i + \eta_2 \mathbf{lnB_i T_i} + \lambda_1 H_i T_i + \lambda_2 lnB_i + \lambda_3 H_i + \xi_{bd} + \xi_j + \xi_t + \omega X_i + \mu_i \\ Y_i &= \alpha + \beta_1 T_i + \beta_2 \mathbf{lny_i} + \gamma_1 H_i T_i + \gamma_2 lnB_i + \gamma_3 H_i + \phi_{bd} + \phi_j + \phi_t + \theta X_i + \varepsilon_i \end{aligned} \tag{2.4}$$

By treating the T_i and H_iT_i terms as controls rather than additional instruments, this controls for any independent effect of the debt relief itself, thus isolating the marginal effect of the debt-relief-induced income. This assures that identified effects operate exclusively through the income channel, so that the exclusion restriction holds. As for the exogeneity of the instrument, that relies upon the core identifying assumptions of the RD design, namely that treatment assignment is "as good as random" once conditioned on the forcing variable and other controls. Thus, β_2 represents the marginal effect of income (or, more accurately, the local average treatment effect (LATE), as discussed in, e.g., Angrist and Pischke (2009)).

This 2SLS approach effectively treats debt relief as a type of income shock. However, this shock is more implicit than explicit. Beneficiaries are forgiven their debts and therefore do not have to repay them. Thus, rather than being explicitly given income, they are implicitly given wealth by virtue of a reduced expense. This wealth can then be valued in income terms. Of course, valuation of the debt relief in wealth and then income terms depends upon the counterfactual of repayment, as well as the time horizon over which the wealth shock is spread. In turn, estimates for β_2 hinge critically on this valuation. Section 3.2 derives upper- and lower-bound valuations, which then bound the treatment effects estimated in Section 3.3.[16]

Three factors may attenuate the effects estimated here. First, there is measurement error, including variation in how individuals interpret the SWB questions. Second, there is the possibility of adaptation

[15] This process is conceptually and mathematically similar to the standard use of intent-to-treat (ITT) indicators as instruments for treatment status or intensity in experimental contexts. Here, I instrument a noisy indicator of treatment intensity – post-relief income – with exogenously-induced intended relief.

[16] Note that I consider household income, not personal income. In the literature, the effect of personal or per-capita income is more commonly estimated. Because y_i is logged, re-scaling by household size would only affect the intercept and error terms, not the estimated coefficient – so there is no trouble interpreting my results in per-capita terms. However, individuals may be more responsive to personal income than they are to their share of household income. For example, it could be that personal income buys bargaining power within the household (as in, e.g., Qian, 2008; Lundberg et al., 1997), so that a corresponding rise or fall in personal income has a greater effect than a corresponding rise or fall in one's share of household income. Thus, this estimate of income effects might be naturally lower than corresponding estimates that are based upon personal income.

or habituation. By the time they were surveyed, farmers had already had more than a year to adapt to their improved circumstances and adjust their frames of reference accordingly (Parducci, 1995; Rayo and Becker, 2007a). Third, there might have been general equilibrium or relative comparison effects. If the debt relief program led to a general increase in prices, this would constitute a spillover, negatively affecting members of the control group. This would be misconstrued as a positive impact on the treatment group (i.e., those receiving 100% relief). Likewise, as shown in Luttmer (2005), evaluations of SWB are sensitive to the income level of one's neighbors. If the debt relief program lifted the general level of incomes, this could also negatively affect the reported SWB of the control group, again confounding the analysis. However, given that Gujarat had only one 100% relief beneficiary for every 100 citizens, and that the face value of all relief amounted to only Rs.410/capita on average ($<$ \$10/capita), these effects are likely negligible.

3 Results

3.1 The effect of debt relief

Table 2 columns (2) and (6) estimate equation (2.2), the average effect of 100% debt relief, on happiness and life satisfaction respectively. Figures 4 and 5 present the equivalent results in graphical form. No effect is evident, which is surprising given the magnitude of debt relief.[17]

Columns (4) and (8) (as well as Figures 6 and 7) estimate equation (2.3), which considers the possibility of heterogeneous treatment effects. Here, there is a clear effect on life satisfaction. While the base effect of 100% relief is negative (row one), the marginal effect of additional relief is positive (row two). Farmers receiving relief on balances close to zero experience a roughly one standard deviation drop in life satisfaction; farmers receiving average relief experience roughly no change as the positive and negative effects just cancel out; farmers receiving above-average relief experience improvements in life satisfaction. Figure 8 summarizes this estimated treatment effect by relief amount, and also includes less precise non-parametric estimates. While the parametric estimates suggest net-negative effects for roughly 50% of full-relief beneficiaries, the non-parametric estimates are only strongly supportive of net-negative effects for the bottom 5-10% of beneficiaries. Section 3.4 discusses possible channels for both the positive and negative effects.

In contrast to these findings with respect to life satisfaction, none of the happiness effects are statistically significant. Given less variation in the happiness measure, however, and a correspondingly less precise estimate, I cannot reject a happiness effect on the same order of magnitude as the satisfaction effect. The null result is imprecise.

3.2 The value of debt relief

To connect the effect of debt relief to the broader literature on the effect of income, it is necessary to derive the income value of relief. This section does so in two steps, first deriving upper- and lower-bound valuations in terms of present-value wealth, then deriving a range of valuations in terms of annual income. Throughout, maximally-conservative bounds are employed in order to avoid reliance on any specific model of household behavior.

In terms of present-value wealth, the upper-bound valuation is simply the face value of relief. This follows from a simple thought experiment. Imagine that a beneficiary were presented with the choice of Rs.x in debt relief or Rs.y in cash. Cash equal to the face value of the relief ($y = x$) is the largest conceivable cash amount for which the beneficiary would still be willing to accept debt relief in lieu of the cash payment.

[17] These and other results hold for a series of sub-samples and specifications, as discussed in Appendix B.

Even assuming that the first x marginal rupees go toward repayment, no beneficiary would accept debt relief when $y > x$. Thus, the face value is in some sense the highest possible cash equivalent.

The lower-bound valuation can be derived from the established willingness to pay among farmers who qualified for only 25% relief. While those in the 100% relief group had their overdue balances cleared for free, those in the 25% relief group had to repay the remaining 75% in order to clear them. This provides an empirical willingness to pay (WTP) for full balance settlement. This can then be imputed to the 100% relief group under the RD assumption that observations just above the discontinuity are systematically similar, *ex ante*, to those just below (conditional on the forcing variable, as discussed in Section 2.2).

To calculate a conservative lower bound, I consider the value of loan settlement to be Rs.0 for those farmers who were offered 25% relief but chose not to pay their 75% share. This is certainly an underestimate, because many of these farmers might have been willing to pay something between 75% and Rs.0. I then consider the value of loan settlement to be the 75% rupee balance for those farmers who did pay the 75%. This is again a lower bound, because these farmers might have been willing to pay more (indeed, in the normal case where farmers must repay their full balances, the majority in fact do so).[18] When the face value of relief is V_{FV}, then, the lower-bound valuation V_{LB} is as follows:

$$V_{LB} = E\left[PAID75P|OFFERED25P\right] \cdot 0.75 \cdot V_{FV} \tag{3.1}$$

Here, $PAID75P$ is 1 if a farmer paid the 75% and 0 if he did not, for all those who were offered 25% relief. The overall propensity of farmers to repay the 75% is thus used to scale the lower bound on 100% relief.[19] As it turns out, the repayment rate among those surveyed in the 25% relief group is 75%.[20] Therefore, in this sample, the lower-bound valuation is simply:

$$\begin{aligned} V_{LB} &= 0.75 \cdot 0.75 \cdot V_{FV} \\ &= 0.5625 \cdot V_{FV} \end{aligned} \tag{3.2}$$

100% relief amounts to a sizable wealth shock, even in lower-bound terms. The first two bars in Figure 9 illustrate the lower- and upper-bound valuations for the median beneficiary (Rs.21,020 and Rs.37,075, respectively). This amounts to a wealth shock of roughly 50% to 100% of median household income (Rs.40,200).

[18] This presumes that farmers actually had to pay the full 75% to settle their balances, rather than a lower bribe amount. Given that claiming 75% repayment when less had in fact been repaid would have adversely affected bank balance sheets and been easy to detect in both internal and external audits, fraud here would have been unlikely. Still, had there been fraud, it would reduce the magnitude of the 100% treatment and increase the later estimates of income's effect on life satisfaction. For example, if 25%-relief beneficiaries only had to repay 60% on average, then the range of income-effect estimates later reported in Table 3 row 3 would go from 0.462-1.081 to 0.783-1.561.

[19] Rather than use the unconditional propensity to repay, the propensity conditional on landholding, balance size, or other farmer characteristics might be used. However, such factors turn out not to be predictive of repayment, and thus provide no empirical advantage over the unconditional propensity to repay.

[20] Follow-up interviews with bank managers eight months post-survey revealed that nearly all farmers had repaid. This suggests that the true value of debt relief is considerably higher than the lower bound employed here.

Following similar logic, the lower-bound value of 25% relief is simply Rs.0 for everybody, and the upper-bound value of 25% relief is the face value for those who took up the offer.

In terms of income, the wealth shock can be assigned a range of valuations. At one extreme, the wealth shock could be interpreted as a one-time income shock of equivalent size. This might be appropriate if households experienced transitory income more or less the way they do permanent income.[21] At the other extreme, the wealth shock could be valued in terms of a perpetuity that pays a certain annual return. This is closer to the true annual income equivalent for a one-time wealth shock. In between the two extremes, the wealth shock might be spread out over some number of years, as a type of fixed-term annuity. For example, for a fixed rate of return r and a fixed period of n years, a wealth shock of value V would have the following income valuation:

$$V_n = \frac{V \cdot r}{1 - \frac{1}{(1+r)^n}} \tag{3.3}$$

This simplifies to $V_{\varnothing} = V$ when counting the full wealth shock as a one-time income shock. When valuing instead as a perpetuity, $n = \infty$ and so $V_\infty = V \cdot r$.

Figure 9 illustrates a range of upper- and lower-bound valuations for the median beneficiary, given a 10% rate of return and annuity periods of $\varnothing$, 5, 10, 20, and ∞. Estimates reported in the following section are not overly sensitive to other plausible choices for the rate of return, but 10% seems reasonable. These farmers carry significant debt from both formal and informal sources, with the lowest annual interest rates being 7% from the commercial and cooperative banks; returns to self-financing a portion of their most expensive debt should be at least 10%.

3.3 The effect of income

Section 3.1 and Table 2 report the reduced-form effect of debt relief on life satisfaction. This section considers the effect of income more generally, using the instrumental variables approach discussed in Section 2.2.

Table 3 reports a range of 2SLS estimates for the effect of income on life satisfaction, based on equation (2.4).[22] Figure 10 summarizes these estimates, all of which are positive and statistically significant ($p < 0.05$).

The magnitude of these estimates mechanically depend on two factors: the reduced-form effect of debt relief, and the valuation of that debt relief in income terms. The intuition is as follows. If each marginal unit of debt relief has a large effect and yet corresponds to a small amount of income, then the marginal

[21] Based on an analysis of business cycle effects, Stevenson and Wolfers (2008) suggest that subjective well-being might be even more responsive to transitory income shocks than to permanent income.

[22] The first-stage F statistic reported in Table 3 falls below 10 for some specifications. This suggests that these specifications might suffer from "weak instrument" bias, and only these specifications fail an exogeneity test with $p<0.05$. However, specifications both with and without these concerns appear consistent. Thus, the overall pattern of results seems robust.

effect of income must be very large. Likewise, if each marginal unit of debt relief has a small effect and yet corresponds to a large amount of income, then the marginal effect of income must be very small. This logic corresponds, more generally, to the logic of the Wald estimator and the method by which 2SLS effectively scales reduced-form estimates.

What varies across the columns of Table 3 is the valuation of debt relief in income terms. Annuity periods of $\varnothing$, 5, 10, 20, and ∞ are considered, as discussed in the previous section. Alternating columns estimate lower- and upper-bound coefficients for each of these periods. Because it is the contrast between the 100% (treatment) and 25% (control) groups that drives these coefficients (in concert with the 2SLS logic just described), a particular pattern of valuations leads to the most conservative lower- and upper-bound coefficients. To estimate the lower-bound coefficient, the upper-bound income valuation for those receiving 100% relief is paired with the lower-bound valuation for those receiving 25% relief; for any given annuity period, this maximizes the size of the treatment in income terms, minimizing the scaled 2SLS coefficient. To estimate the upper-bound coefficient, the lower-bound income valuation for those receiving 100% relief is paired with the upper-bound valuation for those receiving 25% relief; this minimizes the size of the treatment in income terms, maximizing the scaled 2SLS coefficient.

Absent a more precise method of valuing relief in income terms, it is not possible to derive a single estimate for the effect of income. To be safe, the estimates reported here are in a sense maximally conservative. The Stevenson-Wolfers (2008) meta-estimate of 0.4, discussed earlier, falls toward the lower end of these estimates. However, unlike Stevenson and Wolfers, I do not find any effect on happiness (not reported, but following the reduced-form results reported in Table 2).

3.4 Cross-sectional relationships and possible channels

To provide a baseline for comparison, Table 4 reports the cross-sectional relationship between subjective well-being and a series of covariates. Income (exclusive of the direct debt relief), self-perceived improvement in social status, education, and landholding all show strong positive associations with both happiness and life satisfaction. In addition, older respondents appear significantly more satisfied with their lives, recent feelings of stress are associated with markedly lower happiness and satisfaction both, and respondents in larger households report somewhat lower happiness.

If the coefficients could be taken at face value and interpreted as causal relationships, counteracting the negative effect of stress on satisfaction would require a twenty-fold increase in income (roughly 3 log points). Counteracting the negative effect on happiness would require far more (a one-hundred-fifty-fold increase in income, or 5 log points). In terms of happiness, feeling that social status has improved a lot in the past year

– the reference category, vs. feeling that it has not changed at all – is worth half a standard deviation, far more than could be achieved by way of higher income. In terms of satisfaction, the same status improvement is worth a third of a standard deviation, again extremely large when compared with the income term.

Given that SWB tends to be fairly resistant to influence – it is ultimately bounded, after all, unlike covariates like income – these are relatively sizable effects. However, the income-SWB gradient estimated here is relatively small when compared to other cross-sectional estimates. For example, Stevenson and Wolfers (2008) report a gradient many times larger, based upon a variety of different samples and datasets.

There are at least four possible explanations. First, it could be a difference in sample. Since my sample is drawn from farmers with overdue debt in a rural, low-income setting, however, one would expect my income-SWB relationship to be stronger, not weaker. Second, other highly collinear terms, such as landholding or education, could be capturing the income effect. However, the income-SWB relationship remains unchanged even when all other farmer characteristics are excluded (not reported). Third, greater measurement error could be leading to greater attenuation bias in my case. Fourth, omitted variables bias could be more severely negative in my sample, which it would be, for example, if higher rural incomes were more significantly driven by discontent. Of the four explanations, the latter two, attenuation and omitted variables, seem most likely.

For life satisfaction, the 2SLS coefficients presented in Table 3 range from 5x to 27x larger than the cross-sectional coefficient in Table 4. This suggests the presence of substantial omitted variables bias in the cross-sectional estimate.

The cross-sectional results also suggest several specific channels by which debt relief might affect SWB. For instance, relief may reduce stress or increase social status. Tables 5 and 6 estimate the effect of debt relief on these and other potential channels, treating them as outcomes in equations (2.2) and (2.3).

Results are supportive of the stress and status channels, less so of others. There is no detectable effect on past-year income or consumption, for example, but the zeros are not at all precise. Those who receive 100% relief report feeling stress for much of the prior day eight percentage points less often ("Stress 1," $p < 0.10$), and this effect does not vary by relief amount. Experiencing a particularly stressful or anxious month in the prior year, however, does vary, with a three-fold increase in relief associated with a three percentage point decrease in likelihood ("Stress 2," $p < 0.10$). This latter effect, like the satisfaction effects in Table 2, is zero on average: those with small amounts of relief are actually more likely to report a stressful month, while those with large amounts of relief are less likely. (As before, the overall effect is a combination of the base and marginal effects in rows one and two.) In addition, a roughly three-fold increase in relief causes a five percentage point increase in the likelihood of reporting an improvement in social status over the prior year ($p < 0.10$). This also is roughly zero on average: those with below-average relief are worse off in this respect (relative to the 25% control group), and those with above-average relief are better off.

The same basic pattern is reflected in several measures regarding expectations (or optimism) for the future. Greater relief causes higher expectations for future financial surplus (elasticity of 0.065, $p < 0.10$) as well as higher expected educational achievement for the respondent's children (a ten-fold increase in relief adding about one year, $p < 0.01$). Again, those with below-average relief appear more pessimistic, those with above-average relief more optimistic.

Particularly given the strong cross-sectional relationship between stress, status, and SWB, it could be that even the relatively weak treatment effects on stress and status explain the satisfaction results reported in Table 2. Table 7 explores this possibility by estimating equation (2.3) with stress and status included as control variables. If these are the primary channels of effect, then we should expect to see significant attenuation of the original results (included in column (4) for comparison). Note that controlling for potential outcome channels in equation (2.3) is an example of what Angrist and Pischke (2009) call "bad controls." However, in this case any selection effect is likely to further attenuate the treatment effect and thus bias in favor of stress and status as channels of effect.[23] Since the satisfaction effects reported in columns (5) and (6) are only slightly attenuated by the controls – selection effects and all – much of the original effect remains unexplained by stress or change in social status. While these may part of the story, they can only be a small part.

Another possible channel is relative comparison. Since bank branches were required to post lists of all debt relief beneficiaries, including quantity of relief, all farmers knew exactly how much relief they were receiving relative to others in their communities. This might have led to a disappointment or envy effect among those receiving less relief, which, if present, would help to explain why those receiving below-average relief were worse off in terms of satisfaction. To explore this possibility, Table 7 includes a term for the mean level of relief received by other beneficiaries at one's local branch (including those outside the sample frame). While the estimated effect of others' relief on life satisfaction is negative ($p<0.10$), this appears to be equally the case for 100% and 25% relief beneficiaries. The estimated treatment effect remains unaffected (as it does when others' relief is interacted with treatment, not reported).

This leaves unexplained the fixed negative effect of debt relief reported in Section 3.1. Qualitative fieldwork suggests that any channel of negative effect would have something to do with the public nature of the program. Beneficiaries often decry the government's decision to post their names publicly. One potential channel by which this would affect beneficiaries is by revealing something about their creditworthiness. Only farmers with overdue balances qualified for relief, and so being included on the list effectively outs a farmer

[23] This follows from the joint expectation that: first, the SWB of the "never stressed" population is likely to be higher, on average, than the SWB of the "not stressed with debt relief" population; and the SWB of the "social status never improves" population is lower, on average, than the SWB of the "social status fails to improve without debt relief" population. While neither of these need be true, the reverse seems unlikely in both cases.

as a so-called "irregular." However, the public lists included both 100% and 25% relief beneficiaries. Thus, the negative effect of being listed (if any) would have affected the two groups equally and so would not have been detected in the present analysis. In fact, 100% relief beneficiaries do not report lower financial access or greater financial constraint post-relief (not reported).

A more general stigma effect could be at work. Even beneficiaries of 100% relief frequently claim that it was patently unfair for government to grant such a sizable benefit to delinquent debtors (and give nothing to those who maintained their accounts in good standing). While members of the 25% relief group did receive some debt relief, they in some sense "cleared their names" by repaying the bulk of their balances. In contrast, beneficiaries of 100% relief underwent what might be considered a type of forced bankruptcy, in which there was effectively no opportunity to repay. Though they did not choose to receive relief, these beneficiaries became widely seen as having gotten away with something.

There is an intriguing parallel in the U.S. welfare system. In considering an explanation for surprisingly low welfare take-up rates, Moffitt (1983) estimates a model that suggests welfare has a positive marginal effect, but features a negative level effect due to stigma. In his case, those whose benefits would not be sufficient to overcome the negative level effect could opt not to accept welfare. Results here follow a similar pattern, but in this case farmers had no ability to opt out. Instead, they simply had to accept the well-being effects of relief, be they positive or negative. Whether farmers would have exhibited rational expectations regarding these effects is an interesting question, but one I cannot answer with the present data. I doubt that farmers would have turned down relief, but it is possible.

Regret over not having received more relief, feelings of undeservedness, or negative effects on subsequent financial access (a different type of stigma effect) could also explain similar patterns of results. However, it is difficult to imagine that regret relating to this particular program would have persisted for so long as to show up on a rating of life satisfaction 18 months after the fact. Also, data on access to credit do not suggest that beneficiaries of 100% relief suffer or even perceive lower financial access (not reported).

Finally, while there is some apparent heterogeneity in treatment effect, it does not seem conclusive with respect to either stigma or other, non-stigma explanations. The treatment effect does not vary significantly with type of loan, age of loan, total landholding, cultivated landholding, gender, age, education level, or household size (not reported). The negative treatment effect is more pronounced, however, at branches that issued 100% and 25% relief to greater numbers of beneficiaries – but this scale effect is slight and can explain less than 10% of the overall negative treatment effect (-0.0005296 per additional beneficiary, $p<0.05$). It could be that the program was more visible in those areas in which more farmers benefited, thus provoking more public outcry and a greater stigma effect. Or, it could equally be the case that branch areas with more beneficiaries were different in other ways relevant to the effect of treatment on life satisfaction.

4 Discussion

Given the magnitude of the "treatment," it is not surprising that debt relief had a lasting effect on life satisfaction. The lack of a detectable effect on happiness, however, is puzzling. It could be that happiness is more prone to adaptation (as in, e.g., Parducci, 1995; Rayo and Becker, 2007a). Alternatively, the satiation effect described by Kahneman and Deaton (2010) may kick in at lower income levels in rural India. With household incomes around \$1,000/year, however, these farmers are quite far from the \$75,000/year satiation point estimated by Kahneman and Deaton. A simpler explanation is that there is not enough variation in the happiness measure to estimate the effect with sufficient power (see Figure 2). Indeed, positive effects on the order of the life satisfaction effects cannot be rejected.

Another puzzle is the fixed negative effect that partially, fully, or more than offsets the positive marginal effect of debt relief. This effect most likely results from the public nature of the program – the posting of beneficiary lists – and may be a stigma effect akin to the one described by Moffitt (1983). However, the analysis presented here can only provide suggestive evidence on possible channels of effect; it cannot pin them down convincingly. This illustrates both the challenge and the opportunity of SWB research. SWB indicators can alert us to cases where choice-based and other measures may be missing something important and welfare-relevant, but they may leave us with mysteries that can only be resolved in future research.

The positive marginal effect of income estimated here is broadly consistent with the traditional assumption that income raises overall well-being. This is the central finding of the analysis. The Stevenson-Wolfers (2008) meta-estimate of 0.4, derived from a variety of samples and methods, falls toward the lower end of the range estimated here. One plausible explanation could be that the true income-SWB relationship is substantially stronger among the lower-income, indebted, rural population studied here. However, cross-sectional results among this same population yield estimates many times smaller, and the Stevenson-Wolfers estimates may be similarly biased downwards. Therefore, differences in these estimates may be some part differences in population and some part differences in bias.

Importantly, the estimate reported here completely leaves aside the negative fixed effect of receiving 100% relief, rendering it subject to the "all else equal" qualification. In a sense, this is precisely the effect of interest: the effect of income *per se*, leaving aside its source. Here, income is clearly good – for life satisfaction if not happiness.

And yet debt relief, which provided all beneficiaries with an income boost, was actually a net negative for those who received full but below-average relief. They ended up worse off than had they received only partial relief. Thus, the perhaps uncontroversial finding is that income is good, but then the process that generates that income might not be (for a related discussion, see Di Tella and MacCulloch, 2008). In considering the relationship between income and well-being, it should be evident that all else is rarely – if ever – equal. Since income always comes from somewhere, there is perhaps always some cost to weigh against the benefit.[24] Estimates such as those from Stevenson and Wolfers (2008) appear to measure benefits net of costs, which may also help to explain why they would be lower.

Of course, in the neoclassical view the benefits of any chosen income outweigh the costs, else free individuals would have chosen otherwise. But given positional externalities (as in Frank, 2008), or forecasting or other behavioral shortcomings (as in Tversky and Kahneman, 1974; Kahneman and Sugden, 2005; Gilbert, 2005), individuals' choices may not be utility-maximizing in the experience-utility sense. Therefore, assessment of SWB can be an important check. When SWB results differ from those predicted by neoclassical models, there may be important policy implications. In the present case, debt relief might have been structured with an explicit opt-out option, or beneficiary identity might have been kept secret in order to avoid possible consequences with respect to stigma or future financial access.

Other policy implications of the income-SWB estimate are somewhat attenuated by the cross-sectional results. These results suggest that the importance of income may be dwarfed by the importance of other related factors. For example, taking the cross-sectional results at face value, not feeling stress is worth approximately a 15-fold increase in income when it comes to life satisfaction. When it comes to happiness, not feeling stress is worth a 122-fold increase in income. Changes in social status seem to have similarly outsized importance. Even allowing for a healthy degree of reverse causality and other bias, these relationships might support the argument for policies that directly target certain non-income outcomes.

Also, different interpretations of the heterogeneous treatment effect are possible. For example, it may be that those with larger outstanding balances benefited more from debt relief for reasons other than the obvious one that they received more relief. Those with larger balances may, for instance, have been less susceptible to the stigma effects of debt relief – perhaps because they had higher social status to begin with. While I cannot rule out relevant differences in unobservables, the treatment effect does not appear to vary with the broad range of observables captured by the survey. The interpretation that the positive treatment effect followed from the actual relief seems the most plausible.[25]

Three additional qualifications are in order, all regarding external validity. First, I considered only debt relief in four districts of one Indian state. Results in other states or regions might differ, either because of differences in program implementation or differences in the underlying populations. Second, I considered only the effect of debt relief at the 5-acre/2-hectare qualification cut-off. Farmers with smaller or larger

[24] In Econ 101, this issue often arises in the context of the work-leisure trade-off.

[25] In addition to posing conceptual difficulties, other interpretations of the heterogeneous treatment effect would implicitly violate the exclusion restriction, thus casting the 2SLS results into question on technical grounds.

landholdings might respond differently to relief. Third, I considered only seven banks, all of which kept good records and administered the program faithfully. In aborted attempts to collect data and interview beneficiaries for several other banks, our team discovered that neither the record-keeping nor the faithfulness of implementation were universal. Since the debt relief actually seemed to reach its intended beneficiaries in the sample considered here, estimates might be a kind of best-case. In areas where debt relief does not actually reach its beneficiaries, positive effects are obviously unlikely.

Keeping these qualifications in mind, the population studied here may well respond to income changes the way that other rural, low-income populations do. There are many smallholder farmers in the developing world, many of them poor, vulnerable, and struggling financially. Ultimately, though, the question of external validity will only be resolved with additional, well-identified estimates from similar populations in other settings.

Future work on the income-SWB relationship, whether at the micro or macro level, might do well to consider both "all else equal" and "all else considered" approaches. While it is interesting to know the pure well-being effects of income, it is almost always important to jointly assess the well-being effects of whatever brings that income about.

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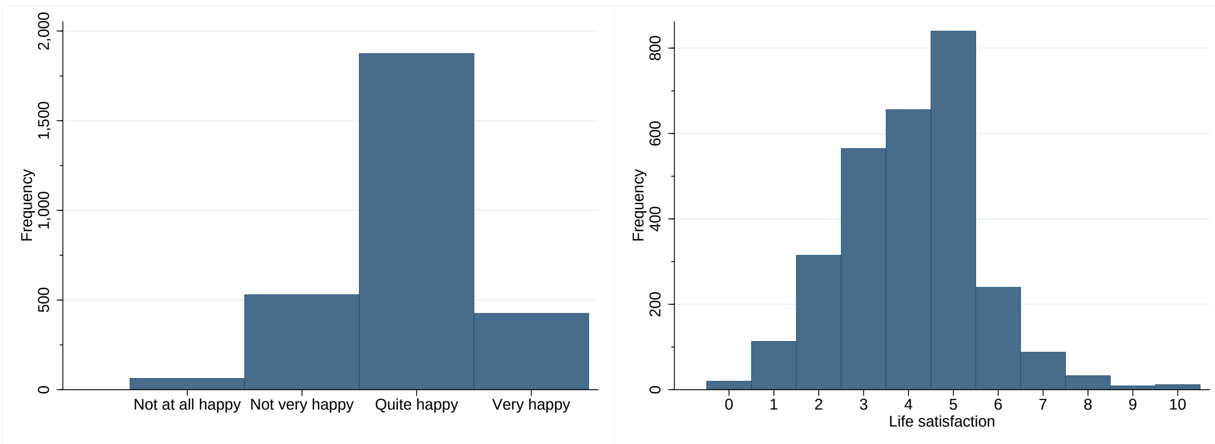
Figures

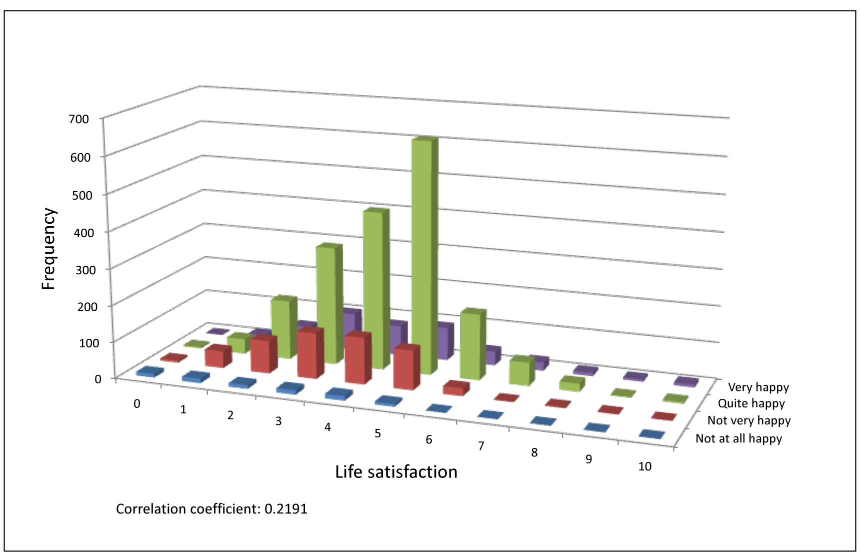
Figure 1: Distribution of happiness and life satisfaction

(a) Happiness (b) Life satisfaction



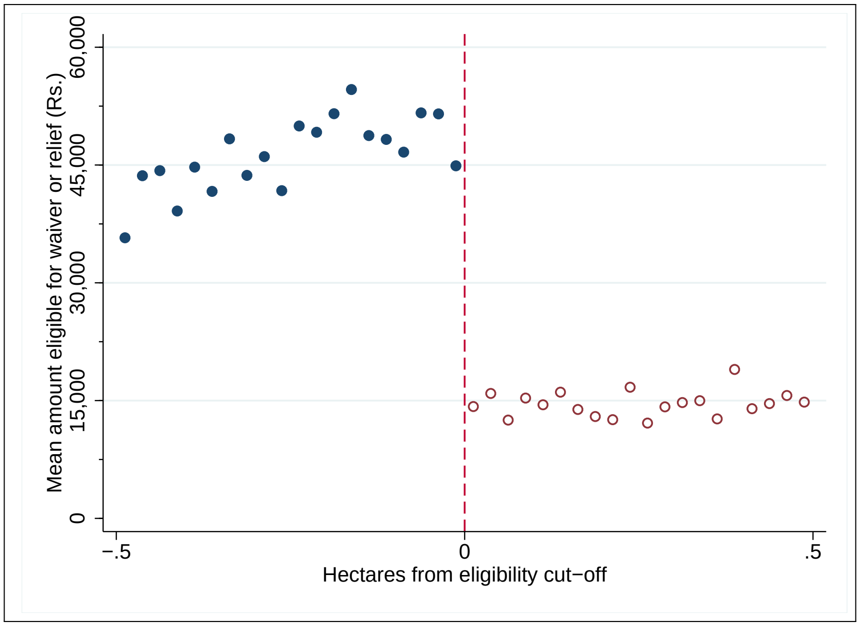
For life satisfaction, the x axis ranges from 0 (the worst possible life) to 10 (the best possible life).

Figure 2: Joint distribution of happiness and life satisfaction



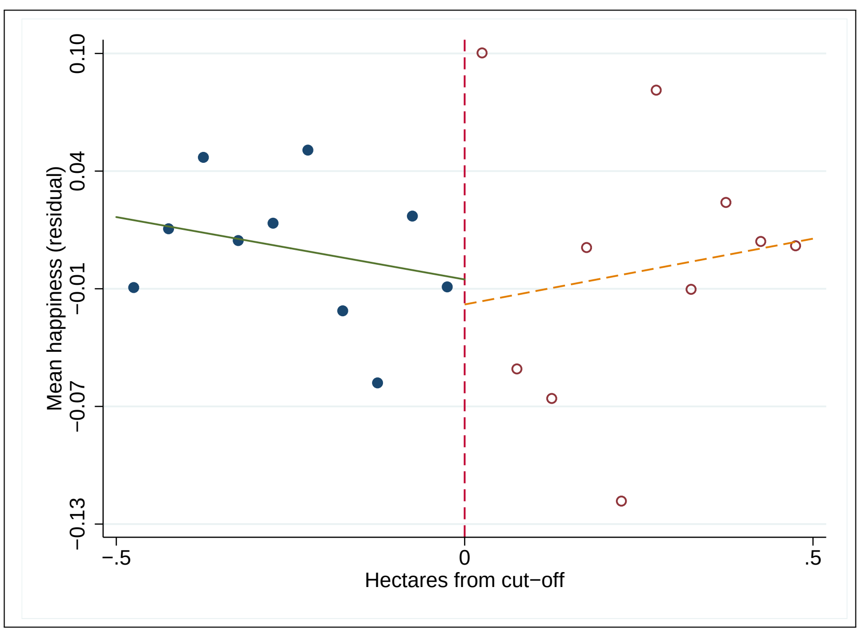
The x axis ranges from 0 (the worst possible life) to 10 (the best possible life).

Figure 3: Eligible relief amount by landholding (RD first stage)



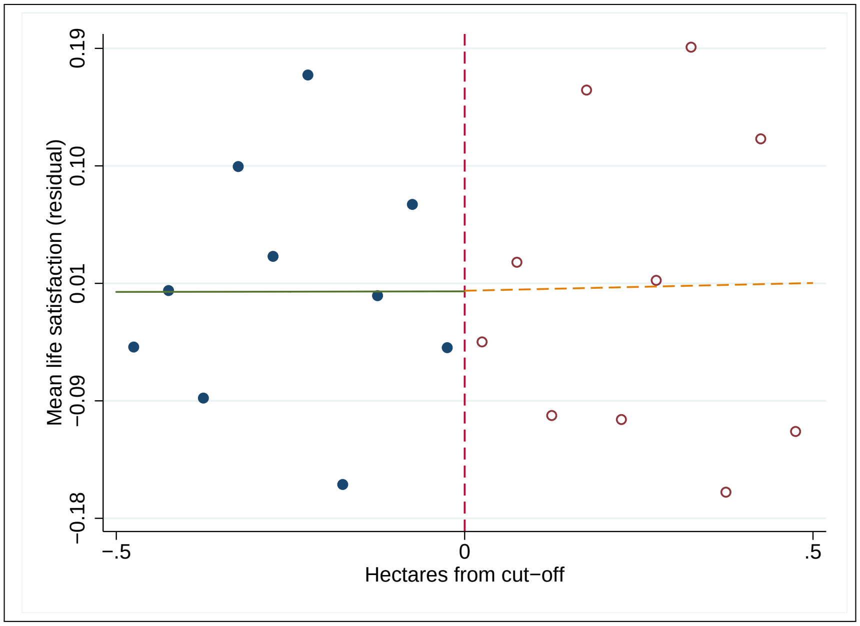
Includes only surveyed farmers. Graph looks nearly identical for the full sample frame.

Figure 4: Happiness by landholding



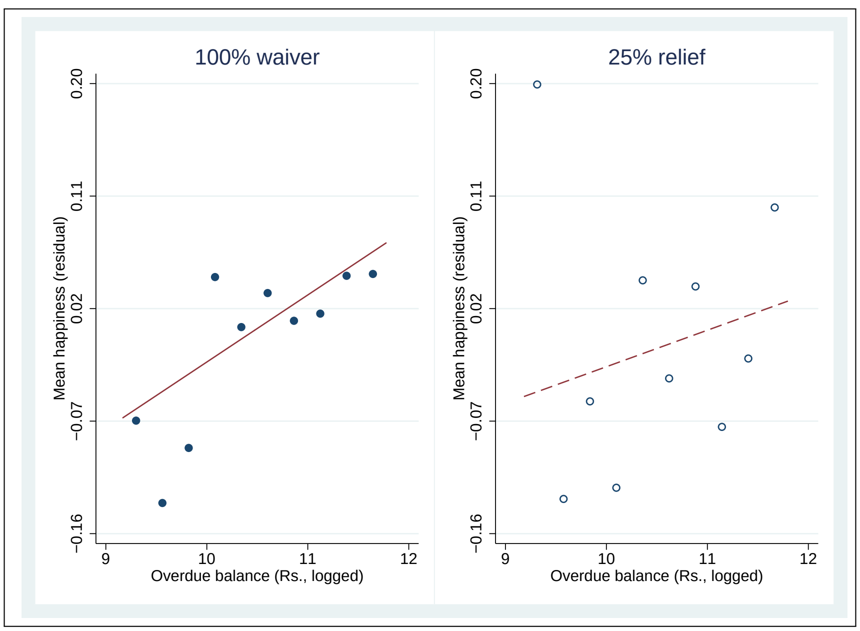
Residual on y axis is after controlling for fixed effects (bank $\times$ district, interviewer, and month-of-survey) as well as gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land redistribution. Lines of best fit indicated to either side of the landholding cut-off. Corresponding quantitative results can be found in Table 2.

Figure 5: Life satisfaction by landholding



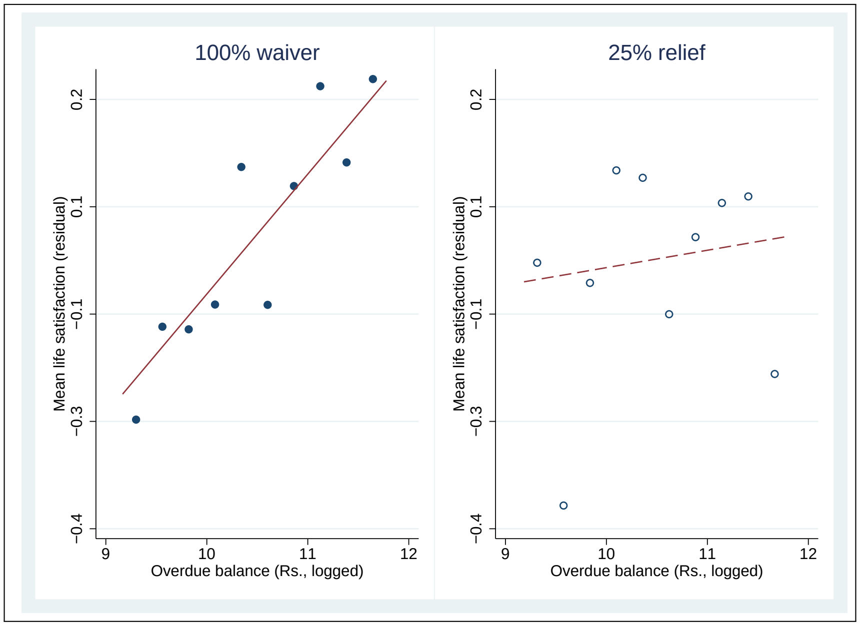
Residual on y axis is after controlling for fixed effects (bank $\times$ district, interviewer, and month-of-survey) as well as gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land redistribution. Lines of best fit indicated to either side of the landholding cut-off. Corresponding quantitative results can be found in Table 2.

Figure 6: Happiness by logged overdue balance and treatment status



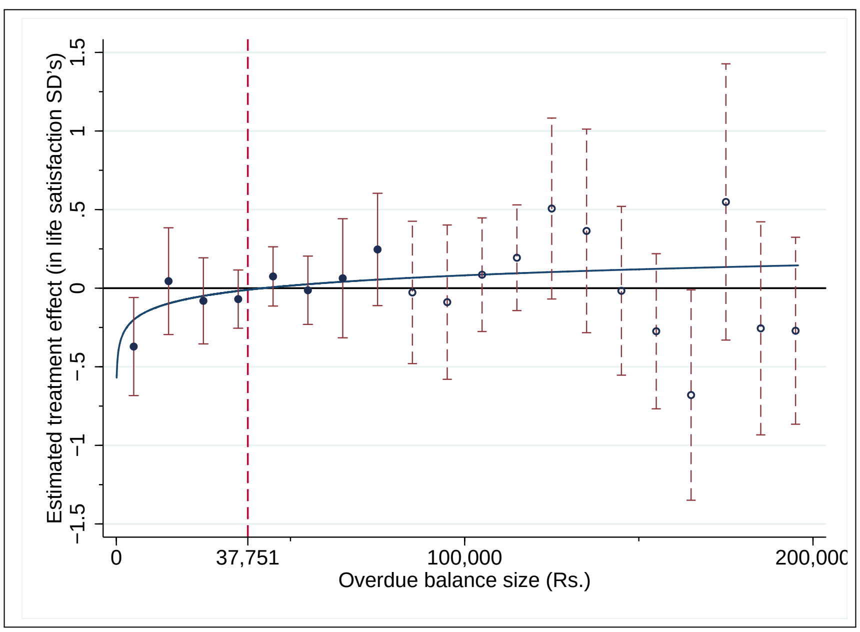
Left graph includes those farmers to the left of the landholding cut-off (who received a 100% waiver); right graph includes those to the right (who received only the contingent 25% relief). Residual on y axis is after controlling for fixed effects (bank $\times$ district, interviewer, and month-of-survey) as well as gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land redistribution. Corresponding quantitative results can be found in Table 2.

Figure 7: Life satisfaction by logged overdue balance and treatment status



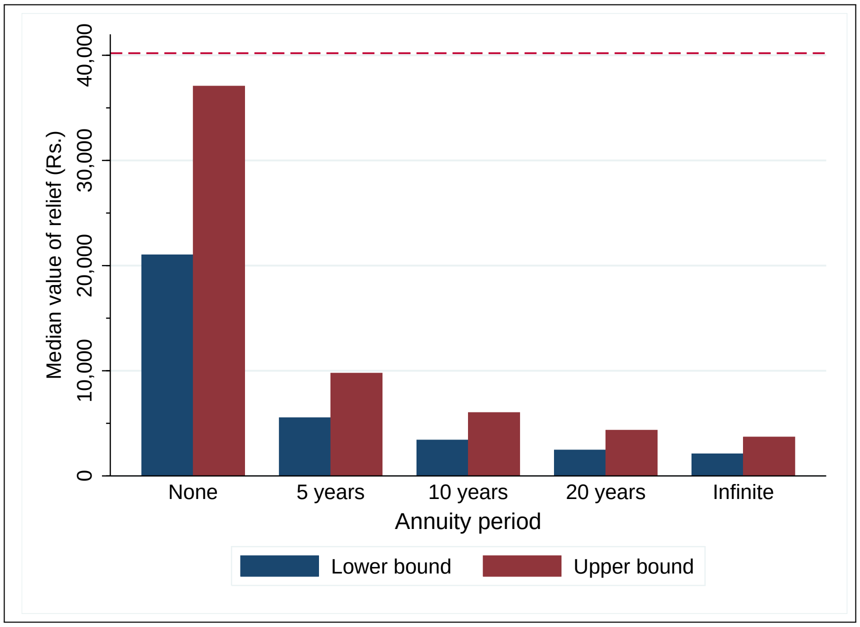
Left graph includes those farmers to the left of the landholding cut-off (who received a 100% waiver); right graph includes those to the right (who received only the contingent 25% relief). Residual on y axis is after controlling for fixed effects (bank $\times$ district, interviewer, and month-of-survey) as well as gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land redistribution. Corresponding quantitative results in Table 2.

Figure 8: Estimated effect of 100% debt relief by relief amount



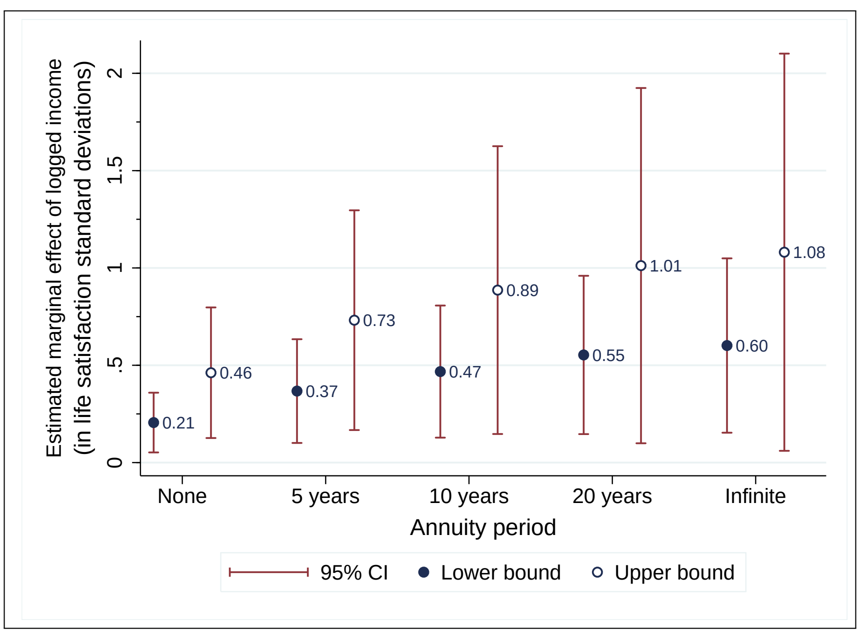
Solid line summarizes parametrically-estimated effect of 100% waiver (vs. contingent 25% relief) on life satisfaction (standard deviations), based on first two rows of Table 2 column (8). Non-parametric estimates and 95% confidence intervals also plotted for bins Rs.10,000 wide; bins above Rs.75,000 dashed since vast majority of observations $\leq$ Rs.75,000. Finally, dashed vertical line denotes mean logged balance, unlogged.

Figure 9: Median relief valuations by annuity period



Upper- and lower-bound valuations are presented for the median 100% relief beneficiary. The "None" annuity period indicates the full valuation in present-value wealth terms; the other periods scale into the equivalent annuity or perpetuity valuations, based on a 10% rate of return. The dashed line at Rs.40,200 indicates the median household income among these beneficiaries.

Figure 10: Estimated effect of income on life satisfaction, by valuation assumption



This figure summarizes the 2SLS point estimates and 95% confidence intervals from Table 3. For each annuity period, lower- and upper-bound estimates are reported. The "None" annuity period includes the full present value of relief (in wealth terms) as part of post-relief income; the other periods include only the equivalent annuity or perpetuity amount, based on a 10% rate of return.

Tables

Table 2: Debt relief's effect on happiness and life satisfaction

	Happiness				Satisfaction			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% waiver	-0.0410	-0.00812	-0.326	-0.259	-0.0486	-0.00736	-1.113***	-1.005**
	(0.0596)	(0.0691)	(0.519)	(0.511)	(0.0360)	(0.0553)	(0.390)	(0.400)
100% waiver × logged balance			0.0269	0.0238			0.101**	0.0944**
			(0.0505)	(0.0474)			(0.0365)	(0.0364)
Logged balance (mean 10.54)			0.0324	0.0273			-0.0299	-0.0218
			(0.0333)	(0.0327)			(0.0352)	(0.0343)
100% waiver × hectares from cut	-0.358	-0.265	-0.376	-0.279	-0.153	-0.0705	-0.193	-0.107
	(0.307)	(0.327)	(0.302)	(0.324)	(0.246)	(0.274)	(0.244)	(0.271)
Hectares from cut-off	0.0881	0.0997	0.0787	0.0918	0.00255	0.0197	0.0103	0.0267
	(0.194)	(0.219)	(0.191)	(0.214)	(0.179)	(0.193)	(0.179)	(0.193)
Constant	-0.218	-0.238	-0.579	-0.541	0.560**	0.491*	0.862*	0.710
	(0.133)	(0.193)	(0.423)	(0.481)	(0.253)	(0.246)	(0.442)	(0.420)
Observations	2885	2884	2885	2884	2882	2881	2882	2881
Adjusted R^2	0.165	0.167	0.166	0.167	0.290	0.296	0.291	0.298
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls		Yes		Yes		Yes		Yes
Weighted		Yes		Yes		Yes		Yes

Dependent variables are self-reported happiness and life satisfaction, standardized. Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. Key effects in column (8) nearly identical but less precise ($p<0.10$) when interacting logged balance with the forcing variable and all controls. Standard errors in parentheses, clustered at the bank × district level.
* $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 3: Income's effect on life satisfaction (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
100% waiver	-0.124	-0.0922	-0.0708	-0.0224	-0.0503	0.00323	-0.0356	0.0210	-0.0281	0.0300
	(0.0957)	(0.0913)	(0.0847)	(0.0846)	(0.0827)	(0.0874)	(0.0824)	(0.0920)	(0.0827)	(0.0954)
Post-relief income, logged	0.206^{***}		0.367^{***}		0.467^{***}		0.553^{***}		0.601^{***}	
(instrumented lower bound)	(0.0783)		(0.136)		(0.173)		(0.208)		(0.229)	
Post-relief income, logged		0.462^{***}		0.732^{**}		0.886^{**}		1.012^{**}		1.081^{**}
(instrumented upper bound)		(0.171)		(0.288)		(0.377)		(0.466)		(0.521)
Logged balance	-0.0195	-0.0885^{*}	-0.0226	-0.0706^{*}	-0.0246	-0.0670	-0.0264	-0.0655	-0.0275	-0.0650
(mean 10.54)	(0.0310)	(0.0454)	(0.0311)	(0.0414)	(0.0316)	(0.0431)	(0.0323)	(0.0457)	(0.0328)	(0.0475)
100% waiver ×	0.0609	0.0214	0.0666	0.0430	0.0694	0.0492	0.0710	0.0523	0.0716	0.0533
hectares from cut	(0.263)	(0.263)	(0.273)	(0.307)	(0.284)	(0.338)	(0.295)	(0.364)	(0.302)	(0.380)
Hectares from	0.0293	0.0446	0.0125	0.00682	0.00252	-0.0125	-0.00596	-0.0276	-0.0107	-0.0357
cut-off	(0.174)	(0.178)	(0.177)	(0.197)	(0.182)	(0.215)	(0.188)	(0.233)	(0.192)	(0.243)
Constant	-1.607^{**}	-3.717^{**}	-3.282^{**}	-6.676^{**}	-4.309^{**}	-8.309^{**}	-5.184^{**}	-9.623^{**}	-5.678^{**}	-10.34^{**}
	(0.737)	(1.527)	(1.312)	(2.777)	(1.688)	(3.685)	(2.036)	(4.576)	(2.248)	(5.128)
Observations	2300	2300	2300	2300	2300	2300	2300	2300	2300	2300
Adjusted R^2	0.318	0.286	0.284	0.084	0.237	.	0.180	.	0.141	.
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trimmed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Annuity period	None	None	5 years	5 years	10 years	10 years	20 years	20 years	Infinity	Infinity
1st stage F stat.	92.25	20.19	30.12	8.252	18.77	5.517	13.42	4.157	11.33	3.609

Dependent variable is self-reported life satisfaction, standardized. Post-relief income is net income from the prior year, plus upper- and lower-bound relief valuations for each of several possible annuity periods. Logged overdue balance, interacted with the 100% waiver (treatment) dummy, is the instrument for post-relief income in 2SLS estimation. Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land redistribution. Weighted regressions re-weight banks by their pre-survey distributions. The sample is trimmed to exclude the top and bottom 5% of observations with respect to income. (When trimming observations outside 1.5x the inter-quartile range, results only slightly attenuated and still significant $p<0.05$. When not trimming at all, results are attenuated a bit more but still $p<0.10$; Windsorized results are similar.) OLS coefficients for rows two and three range from 0.12 to 0.17 ($p<0.01$). The first-stage strength of the instrument declines from specification (1) onwards, as seen in the last row; confidence in exogeneity likewise declines from $p=0.48$ in specification (1) to $p=0.03$ in specification (10). Weakness and/or endogeneity of the instrument is of particular concern in specifications (4), (6), (8), and (10), though the pattern of results is otherwise strong and clear. Standard errors in parentheses, clustered at the bank × district level.
* p<0.10 ** p<0.05 *** p<0.01

Table 4: Happiness and life satisfaction in the cross-section

	Happiness			Satisfaction		
	(1)	(2)	(3)	(4)	(5)	(6)
Estimated annual income, logged	0.0508***			0.0369***		
	(0.00934)			(0.00866)		
Estimated annual consumption, logged		0.0416			0.0672	
		(0.0537)			(0.0733)	
Stress 1			-0.244***			-0.100**
			(0.0462)			(0.0399)
Social status: little bit better			-0.203***			-0.0222
			(0.0706)			(0.0641)
Social status: unchanged			-0.512***			-0.336***
			(0.0912)			(0.0722)
Social status: little bit worse			-0.942***			-0.635***
			(0.238)			(0.158)
Social status: a lot worse			-2.009***			-0.524
			(0.646)			(0.401)
Respondent male	0.0474	0.0306	0.0947	0.00300	-0.00982	0.0394
	(0.142)	(0.134)	(0.137)	(0.0676)	(0.0627)	(0.0691)
Respondent age	0.00201	0.00187	0.000824	0.00497***	0.00481***	0.00466**
	(0.00173)	(0.00178)	(0.00164)	(0.00171)	(0.00171)	(0.00174)
Respondent education (years)	0.0256***	0.0267***	0.0203***	0.0321***	0.0322***	0.0295***
	(0.00506)	(0.00507)	(0.00480)	(0.00370)	(0.00389)	(0.00396)
Household size (# members)	-0.0140***	-0.0130**	-0.0104***	-0.0108*	-0.0118	-0.00972*
	(0.00480)	(0.00600)	(0.00358)	(0.00546)	(0.00726)	(0.00525)
Hectares from cut-off	-0.111	-0.108	-0.120	-0.0891	-0.0869	-0.0951
	(0.0867)	(0.0875)	(0.0842)	(0.0712)	(0.0712)	(0.0688)
Self-reported total land (ha)	0.0566**	0.0668***	0.0551**	0.0720***	0.0779***	0.0702**
	(0.0243)	(0.0238)	(0.0205)	(0.0254)	(0.0255)	(0.0261)
Constant	-1.307***	-1.275**	-0.440**	-0.658***	-1.033	-0.166
	(0.180)	(0.538)	(0.211)	(0.220)	(0.880)	(0.183)
Observations	2077	2077	2049	2075	2075	2047
Adjusted R^2	0.206	0.198	0.239	0.331	0.328	0.348
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes
Trimmed	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variables are self-reported happiness and life satisfaction, both standardized. Stress 1 is a dummy for feeling stress during much of the prior day. Changes in social status are dummies, over the past year; "gotten a lot better" is the omitted category. Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. The sample is trimmed to exclude the top and bottom 5% of observations with respect to income and consumption. (The pattern of results remains substantially unchanged when trimming by inter-quartile range or not trimming at all.) Standard errors in parentheses, clustered at the bank × district level. * p<0.10 ** p<0.05 *** p<0.01

Table 5: Possible channels of SWB effect, 1 of 2

	Income (logged)		Consumption (logged)		Stress 1		Stress 2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% waiver	0.00208	0.455	-0.00547	-0.0722	-0.0821*	-0.0924	0.0196	0.350*
	(0.109)	(0.902)	(0.0344)	(0.186)	(0.0459)	(0.175)	(0.0405)	(0.179)
100% waiver × logged balance		-0.0423		0.00650		0.000970		-0.0311*
		(0.0863)		(0.0178)		(0.0138)		(0.0167)
Logged balance (mean 10.54)		0.107		0.0351**		-0.000654		0.0370**
		(0.0718)		(0.0145)		(0.00973)		(0.0157)
100% waiver × hectares from cut	-0.0738	-0.0590	0.0557	0.0528	-0.105	-0.105	0.176	0.189
	(0.441)	(0.427)	(0.173)	(0.177)	(0.134)	(0.136)	(0.174)	(0.172)
Hectares from cut-off	0.274	0.235	0.000871	-0.0120	-0.0934	-0.0932	-0.0933	-0.108
	(0.310)	(0.312)	(0.142)	(0.144)	(0.103)	(0.103)	(0.146)	(0.144)
Constant	10.15***	9.006***	11.30***	10.92***	0.0902	0.0971	0.112	-0.282
	(0.391)	(0.979)	(0.111)	(0.149)	(0.0886)	(0.143)	(0.112)	(0.221)
Observations	2135	2135	2135	2135	2108	2108	2063	2063
Adjusted R^2	0.037	0.038	0.101	0.105	0.148	0.147	0.267	0.269
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trimmed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variables are estimated annual income (Rs., logged, without the first-order effect of debt relief being added), estimated annual consumption (Rs., logged), stress 1 ("Did you experience a feeling of stress during a lot of the day yesterday?"), and stress 2 ("In the last year, has there been a period of a month or more, during which you were worried, tense, or anxious most of the time?"). Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. The sample is trimmed to exclude the top and bottom 5% of observations with respect to income and consumption. Standard errors in parentheses, clustered at the bank × district level.

* p<0.10 ** p<0.05 *** p<0.01

Table 6: Possible channels of SWB effect, 2 of 2

	Social status better		Current surplus		Future surplus		Highest grade	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
100% waiver	0.0174	-0.548*	0.228	-2.079	0.131	-0.559	-0.128	-4.632***
	(0.0385)	(0.300)	(0.333)	(2.348)	(0.0864)	(0.406)	(0.223)	(1.466)
100% waiver × logged balance		0.0534*		0.219		0.0655*		0.426***
		(0.0270)		(0.219)		(0.0370)		(0.138)
Logged balance (mean 10.54)		-0.0311		0.0689		0.00564		-0.0960
		(0.0207)		(0.183)		(0.0358)		(0.0922)
100% waiver × hectares from cut	-0.338**	-0.358**	-0.346	-0.428	0.625*	0.600	0.138	-0.0244
	(0.144)	(0.145)	(1.073)	(1.101)	(0.351)	(0.355)	(1.119)	(1.161)
Hectares from cut-off	0.236**	0.247**	0.516	0.490	0.00267	0.000693	-0.343	-0.292
	(0.103)	(0.102)	(0.685)	(0.679)	(0.286)	(0.288)	(0.647)	(0.652)
Constant	0.753***	1.084***	8.597***	7.839***	10.41***	10.35***	14.85***	15.77***
	(0.111)	(0.249)	(0.567)	(2.159)	(0.266)	(0.475)	(0.753)	(1.098)
Observations	2133	2133	2120	2120	2134	2134	1354	1354
Adjusted R^2	0.190	0.192	0.171	0.173	0.144	0.145	0.073	0.082
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trimmed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variables are a dummy for whether respondent thought his or her social status had improved over the past year, surplus money projected to be left over at end of this year (Rs., logged), surplus money projected to be left over in five years (best-case, Rs., logged), and the highest grade to which the respondent plans to send his children to school (respondents with school-age children only). Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. The sample is trimmed to exclude the top and bottom 5% of observations with respect to income and consumption. Standard errors in parentheses, clustered at the bank × district level.
* p<0.10 ** p<0.05 *** p<0.01

Table 7: Happiness and satisfaction, controlling for possible channels of effect

	Happiness			Satisfaction		
	(1)	(2)	(3)	(4)	(5)	(6)
100% waiver	-0.259	-0.326	0.180	-1.005**	-0.882**	-0.751*
	(0.511)	(0.428)	(0.502)	(0.400)	(0.396)	(0.390)
100% waiver × logged balance	0.0238	0.0271	-0.0186	0.0944**	0.0822**	0.0705*
	(0.0474)	(0.0404)	(0.0468)	(0.0364)	(0.0364)	(0.0361)
Logged balance (mean 10.54)	0.0273	0.0302	0.0502	-0.0218	-0.0124	-0.00376
	(0.0327)	(0.0277)	(0.0323)	(0.0343)	(0.0381)	(0.0352)
100% waiver × hectares from cut	-0.279	-0.188	-0.0783	-0.107	-0.0499	-0.00168
	(0.324)	(0.322)	(0.364)	(0.271)	(0.291)	(0.312)
Hectares from cut-off	0.0918	-0.0299	-0.0339	0.0267	-0.0265	-0.0450
	(0.214)	(0.204)	(0.218)	(0.193)	(0.199)	(0.212)
Mean relief (others at branch, logged)		0.0175	0.0233		-0.0604**	-0.0604*
		(0.0229)	(0.0230)		(0.0287)	(0.0304)
Stress 1		-0.358***			-0.113**	
		(0.0383)			(0.0438)	
Stress 2			-0.382***			-0.229***
			(0.0438)			(0.0297)
Social status better		0.441***	0.409***		0.396***	0.372***
		(0.0385)	(0.0415)		(0.0297)	(0.0324)
Constant	-0.541	-1.031**	-1.277**	0.710	0.944**	0.880*
	(0.481)	(0.423)	(0.502)	(0.420)	(0.443)	(0.453)
Observations	2884	2843	2793	2881	2840	2791
Adjusted R^2	0.167	0.217	0.214	0.298	0.326	0.328
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variables are self-reported happiness and life satisfaction, both standardized. Mean relief is the mean amount of relief at the same branch, logged. Stress 1 is a dummy for feeling stress during much of the prior day. Stress 2 is a dummy for feeling worry or anxiety for a month or more in the past year. "Social status better" is a dummy for feeling that social status had improved in the past year. Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. Standard errors in parentheses, clustered at the bank × district level.
* p<0.10 ** p<0.05 *** p<0.01

Appendix

A Data

A.1 Sample frame

The sample of farmers considered here was drawn exclusively from four districts of Gujarat, a state in Western India. Like any of India's states, Gujarat is unique in some ways and ordinary in others. It is richer than average, with a per-capita income about 26% higher than the all-India average (Government of Gujarat, 2008b). It is also more urban than the rest of the nation, with 37% living in urban areas (vs. 28% for India overall, Government of India, 2001). Agriculture makes up about the same share of Gujarat's economy, however, as India overall (around 18%, Government of Gujarat, 2008a; Government of India, 2007).

In terms of banking, Gujarat enjoys slightly higher than average commercial bank coverage, with one commercial bank per 14,220 people (vs. 15,601 people for India overall, Government of Gujarat, 2008a). Nearly one million Gujarat farmers qualified for debt relief under the 2008 scheme, with average relief of Rs.24,275. This was 37% higher than the all-India average relief of Rs.17,712 (IndiaStat and Rajya Sabha, 2008). However, because it is more urban and therefore had relatively fewer beneficiaries, Gujarat received slightly below-average relief on a per-capita basis.

The sample districts, Mehsana, Gandhinagar, Kheda, and Anand, form a contiguous band in the central-northwest part of Gujarat. These districts include relatively rich agricultural land and are home to numerous cooperatives, including the Amul dairy brand that fueled India's so-called "white revolution." They are slightly more rural than Gujarat as a whole, with 64-80% of households residing in rural areas (vs. 61%, Government of India, 2001). Their literacy rate, however, is higher (69-73% vs. 61%, Government of Gujarat, 2008b).

Gandhinagar, Kheda, and Anand are less industrial than Gujarat as a whole, but Mehsana has almost double the average number of factories per capita (primarily pharmaceuticals and heavy equipment, Government of Gujarat, 2008b). With respect to both banking and TV ownership, Kheda lags slightly behind Gujarat averages, but otherwise the sample districts are similar to Gujarat as a whole (Government of India, 2001).

Kheda and Anand are similar in terms of agriculture, but Anand has an especially large and productive dairy industry. Farmers in both districts cultivate primarily rice, millet, tobacco, corn, pulses, ground nuts, sesame, castor, cotton, mustard, wheat, and potato. Farmers in Mehsana and Gandhinagar cultivate primarily rice, millet, pulses, sorghum, sun hemp, wheat, mustard, cumin, cowpea, and various vegetables

(Government of Gujarat, 2010). Average rice yields are somewhat higher in Gandhinagar, but otherwise rice and millet yields are similar to all-Gujarat averages (Government of Gujarat, 2008a).

The sample frame includes farmers who qualified for debt relief, held particular loan types with certain banks, and whose landholding was recorded as being within a certain range. The six commercial banks holding the most beneficiary accounts are included, as is the largest cooperative bank: Bank of Baroda (BOB), Bank of India (BOI), Central Bank of India (CBI), Dena Bank (DENA), State Bank of India (SBI), Union Bank of India (UBI), and Kaira District Central Cooperative Bank (KDCC, which covers cooperatives throughout Kheda and Anand). Table A.1 details the number of debt relief beneficiaries by bank, including those in other banks outside the sample frame.[26]

The sample frame includes only certain categories of loan. Crop loans and investment credits for direct agricultural purposes are included, while allied-to-agriculture and previously-restructured loans are not. This restricts the class of loans to the roughly 70% for which landholding was determinant of debt relief qualification.

Finally, because the identification strategy involves a regression discontinuity design, only accounts with landholdings close to the discontinuity are considered. Thus, only accounts within a band of +/- 0.5 hectares around the 100% relief cut-off are included. Because different banks used different cut-offs – the commercial banks used two hectares and the cooperative bank used five acres (2.0234 hectares) – the band is calculated at the bank level.[27]

The final sample frame includes 5,554 accounts, as detailed in Table A.2.

A.2 Bank data

Banks were required to publicly post details about all qualifying debt relief beneficiaries. This included the name, landholding, village, loan category, date of original disbursal, overdue principal and interest as of December 31, 2007, and eligible relief amount. Some banks also included the purpose of the loan (e.g., "tractor" or "tube well") as well as the original principal amount. All of this was posted to the notice boards of participating bank branches, and several banks also posted the information on their websites. (This information was provided as an anti-corruption transparency measure.)

For the subset of farmers included in the sample frame, Table A.3 provides summary statistics, and Figure A.1 shows that the distribution of eligible relief was very right skewed: a few farmers got very large

[26] The largest gap in the sample frame is a set of 12,965 cooperative accounts in Mehsana, where bank records were too poor and bank cooperation too reluctant for inclusion in the sample.

[27] Bank records were not perfect, and landholding was not reported for some accounts. Accounts without reported landholding were excluded from the sample frame. Because this was a small number of accounts falling into both the 100% waiver and 25% relief categories, it is not likely to introduce bias into the final analysis. The +/- 0.5 hectare bandwidth was chosen following a process similar to the cross-validation procedure described in Imbens and Lemieux (2008). This was the bandwidth that minimized the mean squared error when predicting relief amount with landholding and a 100% waiver indicator.

amounts. The average relief per beneficiary in the sample frame, Rs.33,498, is substantially higher than the Gujarat average of Rs.24,275, for several reasons. First, the bulk of qualifying farmers have less land than those included in the sample frame. Since there is a positive relationship between landholding and loan size and also between loan size and relief amount, larger landowners will tend to get more relief.[28] Second, those banks not included in the sample frame, such as rural regional banks, are likely to issue smaller loans on average than the larger commercial and cooperative banks included in the sample frame.

A.3 Survey data

Between October 2009 and January 2010, we attempted to locate nearly all households within the sample frame in order to administer a comprehensive household survey.[29] In all, we administered 2,897 surveys. Table A.4 summarizes the administration results. Tested jointly, balanced attrition across all categories cannot be rejected at traditional levels of significance (p=0.24), and attrition does not seem to be systematically related to either landholding or relief amount (p=0.68).

The refusal rate is not surprising given that the survey was lengthy, taking two to three hours to administer, and given that farmers were not compensated for their time. Most households took loans in the name of the head of household, who was often the oldest male member. This helps to explain the sizable mortality rate, which increases as expected in loan age. Migration for work is not uncommon, and here migration also includes cases where the farmer was simply in the city or otherwise out of town on business. Because only imperfectly-recorded and -transliterated names were available from the banks, many villages have multiple individuals with the same name, and many village names were wrong or missing. Such factors made it difficult to locate the correct farmer in many cases.

For most surveys (84%), we interviewed the actual borrower identified by the bank. These were the cases where the official holder of the loan was both the user of the loan and the household's financial decision-maker. When somebody else in the household was the financial decision-maker and the loan's true user, we interviewed that individual instead. We only interviewed another household member once we verified that we had the right borrower and that this borrower confirmed that the other household member was both the financial decision-maker and the actual user of the loan in question. This typically happened, for instance, when the loan was taken out in the father's or wife's name – because he or she owned the land, for example – but the son or husband was the true decision-maker and user of the loan.

The survey includes modules that measure household composition and characteristics, education, animal

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

[29]The survey effort was a collaboration with Martin Kanz.

husbandry, land ownership and cultivation (including specific crop choice, input use, and yields over the last five seasons), assets, consumption, income from remittances and non-farm work, subjective well-being, community status, expectations, household debt, credit demand, financial dependence and constraints, risk and time preferences, savings and inter-household transfers, and politics. It is quite comprehensive. For the purposes of the present analysis, only a small subset of the survey variables are relevant. Summary statistics for these variables can be found in Table A.8.

Summary measures of annual household income and consumption are derived from detailed income and consumption modules. The summary measure of annual income includes agricultural income (gross yield minus expenses), income from animal products, rental and remittance income, and other sources of non-farm income. Because our survey accounts for income more comprehensively than expenses, this almost certainly over-estimates true household income. For instance, revenue from animal products is included, but the expense associated with feeding and caring for those animals is not. The summary measure of annual consumption is calculated by taking the past 30 days of reported household consumption as representative. Thus, annual consumption is derived by combining past-year consumption categories with scaled-up versions of past-month consumption categories.

For measuring subjective well-being, standard survey items were used so that results could be compared with prior findings.[30] The specific questions, as well as summary statistics, are discussed in Section 2.1.

Responses to the following questions also feature in the analysis:

> *Now I would like you to think about your status in the community. Thinking about <u>the past year</u>, would you say that your status has <u>gotten a lot better</u>, <u>gotten a little better</u>, <u>stayed about the same</u>, <u>gotten a little worse</u>, or <u>gotten a lot worse</u>?*
>
> *Did you experience a feeling of stress during a lot of the day yesterday?*
>
> *In the last year, has there been a period of a month or more, during which you were worried, tense, or anxious most of the time?*
>
> *About how much money was left over last year, after all fixed household and farm expenses?*
>
> *About how much money do you think will be left over this year (after all fixed household and farm expenses)?*
>
> *Now I would like you to think about five years from now. In five years, at the end of the year, about how much money do you think you will have left over (after all household and farm expenses)?*

[30]For those questions included in the World Values Survey and Gallup World Poll, Gujarati text was translated directly from the Hindi so that responses would be as comparable as possible.

Still talking about five years from now...In the best case, if all of your plans go perfectly and you have very good luck, about how much money do you think you might have left over (after all household and farm expenses)?

Till what grade will you send your children to school?

A.4 Audit data

Records from Gujarat's *e-Dhara* repository of official land records were used to audit bank-reported landholdings for a majority of surveyed households. These audits are discussed in Appendix B.1 below.

B Robustness

B.1 Integrity of the forcing variable

The central identification strategy employed here is a regression discontinuity (RD) design, roughly following the method as described in Imbens and Lemieux (2008). One of the chief concerns for any such design is that there was no manipulation of the forcing variable that would make selection to either side of the discontinuity non-random (as discussed in, e.g., McCrary, 2008). This appendix section, therefore, focuses on the integrity of the forcing variable, in this case borrower landholding.

Figure A.2 shows the land distribution according to bank records, for all surveyed farmers. For cooperative accounts in particular, there is a large and suspicious spike in density precisely at the cut-off for debt relief qualification. McCrary's (2008) test for discontinuity in the forcing variable correspondingly fails to reject the presence of a discontinuity with $p < 0.01$.

The McCrary test also fails to reject discontinuities at 4 and 6 acres, suggesting that bunching at whole numbers could be a part of the explanation. However, the size of the spike at 5 acres is so large as to require an additional explanation. Note that the McCrary test cannot reject continuity in the forcing variable once observations exactly at the cut-off are dropped. The question then becomes: why are there so many observations just at the cut-off?

In order to gauge the extent of potential manipulation – and provide for the possibility of a robustness check using a manipulation-free sub-sample – we audited the official landholdings of most surveyed households. In Gujarat, official landholdings are recorded in a centralized electronic system, *e-Dhara*. Manipulation of *e-Dhara* records is considered highly unlikely because of the many bureaucratic checks against corruption; even legitimate changes in landholding are difficult to record in a timely fashion. In order to audit the landholding numbers reported by both the bank and survey respondents, we obtained official copies of the relevant landholding records.

We discovered several legitimate reasons for these landholding records to differ from the landholding numbers reported by the banks. First, many banks accepted partial mortgages: to qualify for some loans, farmers were allowed to mortgage only a portion of their land. In these cases, the bank-reported landholding is less than the total land held by the farmers, and the smaller landholding amount will have been used to determine program qualification. While basing qualification on mortgaged rather than total landholding is technically at odds with the government-mandated qualification criteria, government regulators do not appear to have objected. Since banks tended not to track borrowers' total landholdings, "total landholding" was widely interpreted as "total landholding on file." This is not considered manipulation, and does not affect the validity of the RD approach.

Second, loans often considered the landholdings of multiple individuals. Most frequently, land held by multiple members of the same extended household is pooled in order to qualify for a larger loan. In many cases, the loan was recorded as having a single beneficiary and the total landholding was listed – even though the beneficiary did not himself or herself own all of the listed land. In these cases, the bank-reported landholding is greater than the total land held by the farmers. This also is considered legitimate and should not violate the fundamental RD assumption.

Third, rounding and conversion errors were common, as landholding can be recorded in a variety of complex and region-specific units. Since official landholding documents almost never reported landholding in the same units as banks, there were nearly always opportunities for rounding and conversion errors.

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

With landholding documents for 2,040 of 2,897 surveyed farmers, the match rate is 41.4%. Of the cases that fail to match, 83.5% fail to match because the total official landholding is too small to match with the bank report. These appear to be cases where multiple landholdings were pooled, or they could be cases of fraud 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 cut-off, over-reporting land makes qualification for debt relief less likely. If it is manipulation, it is in the wrong direction with respect to debt relief.

Figure A.3 plots the empirical cumulative distribution functions for commercial and cooperative landholdings, separately for matching and non-matching accounts. For commercial accounts, matching and non-matching land appear to follow very similar distributions, though the Kolmogorov-Smirnov test for equality still rejects with p=0.047. To a slight extent, matching land appears more heavily concentrated at the low end of the distribution. This pattern is similar – but much more pronounced – for the cooperative landholding distributions shown in the right panel. Note, however, that the same spike at 5 acres is equally evident in both the matching and non-matching distributions.

As shown in Figure A.4, the higher concentration of matching land on the low end of the distribution is a combination of two factors: a slightly higher audit rate for smaller landholdings (i.e., a higher propensity to secure the official land documents) and a slightly higher propensity for land documents to match, once secured. Note that both the audit rate and the match rate are markedly higher just to the left of the cut-off than to the right. This is precisely the opposite of what should happen in the presence of corruption at the cut-off: we should be less likely to locate official documents for corrupt farmers,[31] and corrupt land should match at much lower rates.

Finally, Figure A.5 plots the bank-reported and audit-derived landholding distributions, for roughly the 4 to 6 acre range of landholdings. By ignoring whether land matches or not, this allows a comparison of the raw land distributions, as considered from bank and government sources. The distributions are visually indistinguishable, and the Kolmogorov-Smirnov test fails to reject equality with p=0.357. This suggests that the bank-reported distribution is in some sense natural – spike and all – and not the result of bank or farmer manipulation.

The question of the discontinuity remains. Though almost certainly a case of natural bunching in at least part, there still seem to be too many farmers right at the cut-off. As it turns out, the single largest group of these is in one village, Mitli, in Anand district. In this village, a government land distribution scheme issued exactly five-acre plots of land to a large number of farmers, many of whom ended up in our sample.[32] So that the residents of Mitli do not bias the analysis, I include a Mitli fixed effect as part of the "other controls" throughout the analysis. Key results are robust to inclusion of a more sweeping fixed effect for all observations located right at the cut-off (not reported).

Thus, a detailed analysis of landholding distributions and audit results argues against manipulation for the sake of debt relief qualification: when audited landholding differs from reported landholding, the difference works *against* qualification in most cases; qualifying farmers agreed to and passed audits at consistently higher rates than non-qualifying farmers; and equality between the reported and true land distributions, both affected by a 5-acre land distribution in a particular sample village, cannot be rejected. Still, Appendix B.3 below conducts a series of robustness checks using sub-samples that are arguably corruption-free. Those robustness checks further suggest that key results are not significantly affected by manipulation.

B.2 Balance and continuity

The continuity assumption at the heart of the RD design implies that static and pre-program variables should exhibit neither discontinuities nor apparent treatment effects. Figure A.6 provides a graphical continuity check and Table A.5 provides a balance check (similar to a randomization check) for nine static and pre-program variables. Conditional on the forcing variable and fixed effects (bank × district, month-of-interview,

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

[32] These plots were issued to 100-200 farmers 25 years earlier. Because all of these farmers were below the poverty line at the time and were given high-salinity land not well suited to agriculture, they represent an exceptionally poor sub-sample.

and interviewer), only respondent gender reflects a difference ($p < 0.10$). There are slightly more women below the cut-off than above. In order to ensure that this does not bias results, I include gender as one of the "other controls" throughout the analysis. Key results have also been checked to ensure that treatment effects do not vary by gender (using gender×treatment interactions, not reported).

B.3 Robustness checks

Appendix B.1 argues against corruption in program implementation, but it is still worth checking key results against sub-samples that are more assuredly corruption-free. Tables A.6 and A.7 present the same specifications as Table 2, but for a series of sub-samples. All key results continue to hold when considering only commercial bank accounts, and when only considering accounts that fully passed the landholding audit described in Appendix B.1.[33]

Another identification concern has to do with the 2-hectare qualification cut-off. This cut-off corresponds to the traditional cut-off for being considered a "small farmer," and Indian agricultural policy has often favored such farmers with preferential access to loans (including quotas and specialized loan products), subsidies for agricultural and allied investments (including, for example, a subsidy for drip irrigation in the 1980's), and lower interest rates at cooperative and other banks. This threatens the overall identification strategy in two ways. First, the treatment effects estimated here could be the result of another program or benefit sharing the same 2-hectare cut-off. Second, the historical benefits of being classified as a small farmer might have led some farmers to strategically divide their land, inducing selection effects that are discontinuous at the 2-hectare cut-off. Both would threaten the causal interpretation advanced in this paper.

However, note that the key results in Tables 2 and 3 derive from the interaction term in equation (2.3). In order for other program effects to bias this term, they would have to systematically vary with overdue balance size. It would be much more likely for the overall ATE to be biased.

Also, note that other programs that benefited small farmers used *total personal land* as the criterion for qualification. Because in practice it used the landholding on record with the loan – which included many cases of partial and pooled land, as discussed in Appendix B.1 – the debt relief program was implemented differently. Thus, the landholding cut-off used for debt relief qualification was in fact novel.

There is a sub-sample of 1,330 farmers, all of whom will have qualified for any other small-farmer program or preference, but 430 of whom failed to qualify for debt relief because they clearly pooled their land with others in order to qualify for a larger loan. I call these "small landholders," because their total household land,

[33] Commercial banks appear less prone to corruption because of their prevailing reputation, a greater intensity of government oversight, and evidence such as that in Figure A.3. Audit successes, because they "check out," should be almost entirely free of corruption. Estimates robust to these sub-samples are not likely to be biased by manipulation of the forcing variable. That said, the audit-success sub-sample is small and not likely to be fully representative, due to the variations in audit and match rates discussed earlier. Therefore, the full sample is preferred.

as indicated in official land documents, falls below the 2-hectare cut-off. Many other households undoubtedly included small farmers, perhaps by means of strategic land division within the household. However, the small landholders that I consider are officially small farmers regardless of their intra-household division of land.

Table A.7 reports key results for this sub-sample of small landholders. The key treatment coefficients are strikingly similar to those in the full and commercial bank samples, and show no signs of bias. However, they do exhibit lower power, so the treatment effect is no longer statistically significant.[34]

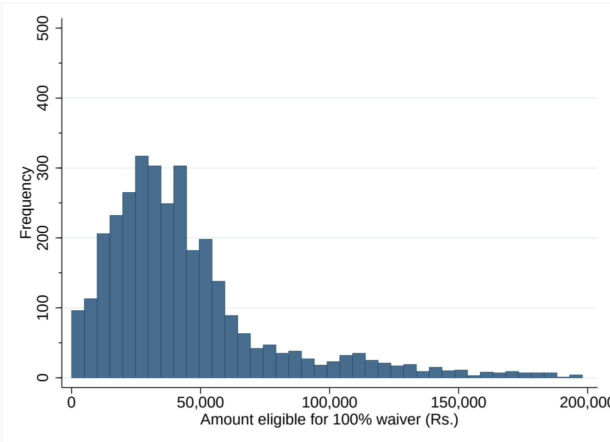
The estimated effect of debt relief on life satisfaction – and thus the estimated effect of income – remains robust to a wide variety of specifications and sub-samples. This includes specifications that include higher-order polynomial controls for landholding, as well as analyses that consider only a more narrow band around the qualification cut-off (+/- 0.25 hectares, not reported).

[34]The small landholder sub-sample suffers from the fact that all observations above the cut-off must be cases where land was pooled. Thus, there may be a new form of selection bias. However, when considering a sub-sample of only those who appear to be pooling land – 988 cases where an audit reveals lower landholding than reported by the bank – the estimated effect on life satisfaction is nearly identical. Results are also robust to defining small landholders by self-reported personal landholding rather than household landholding.

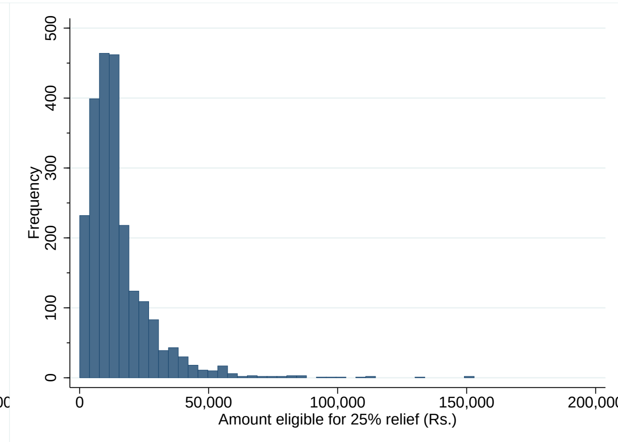
Appendix figures

Figure A.1: Distribution of eligible relief amount

(a) 100% waiver



(b) 25% relief



Includes all farmers within the sample frame, including those not surveyed. Excludes 32 observations over Rs.200,000.

Figure A.2: Beneficiary landholding distribution, from bank records

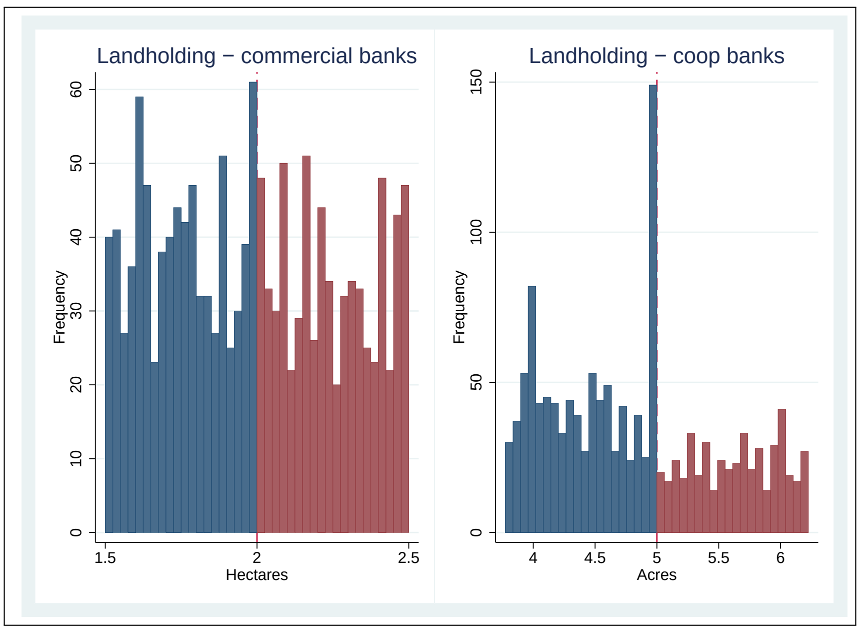


Figure A.3: Comparison of land distributions by audit result

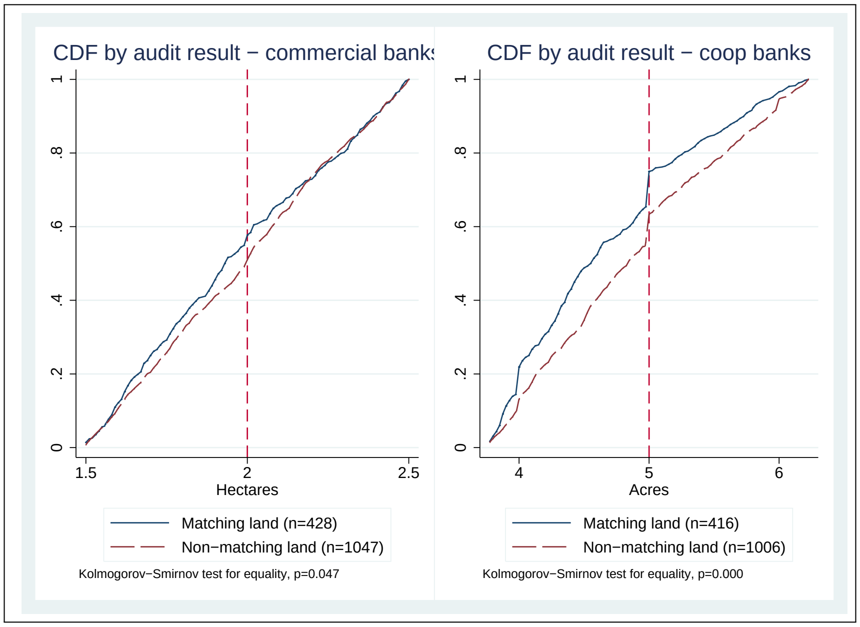


Figure A.4: Audit completion and match rates, cooperative banks

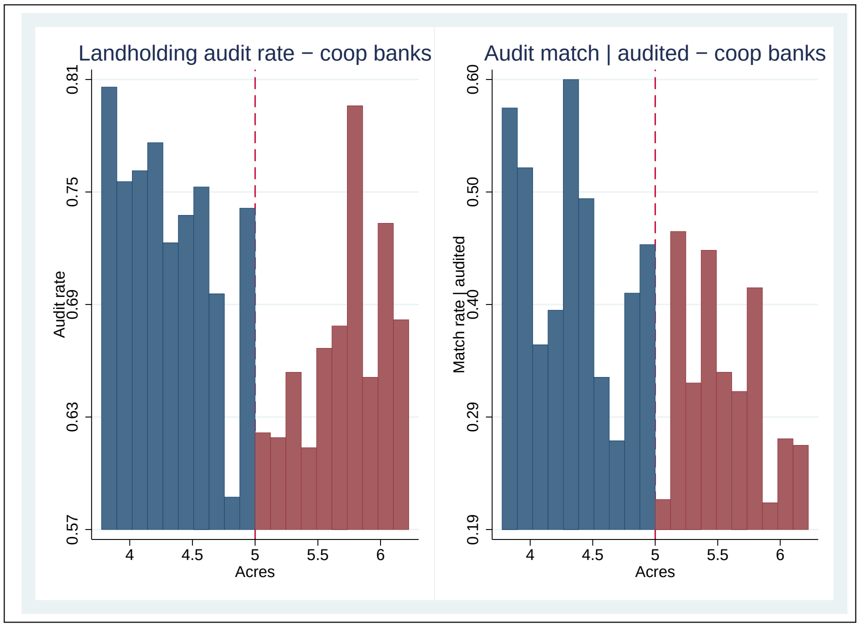
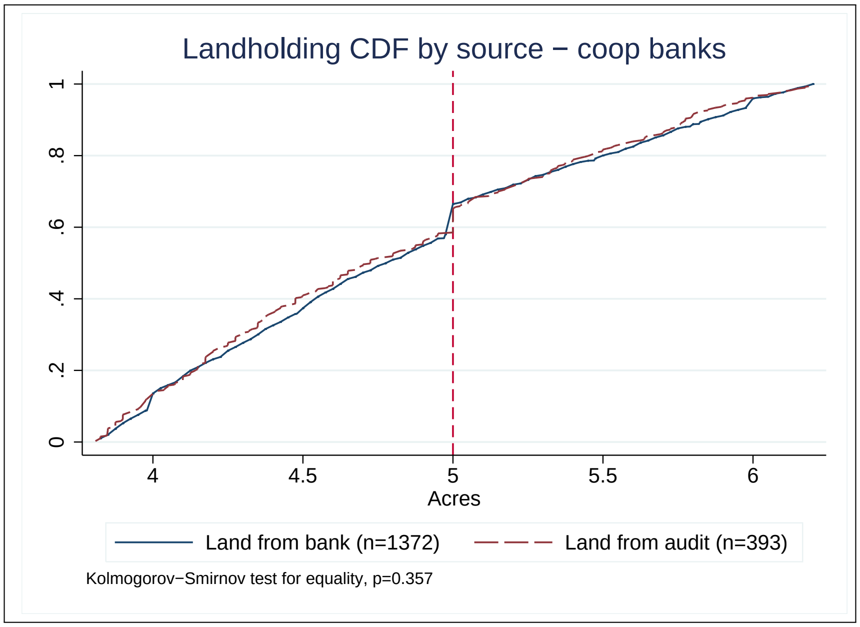
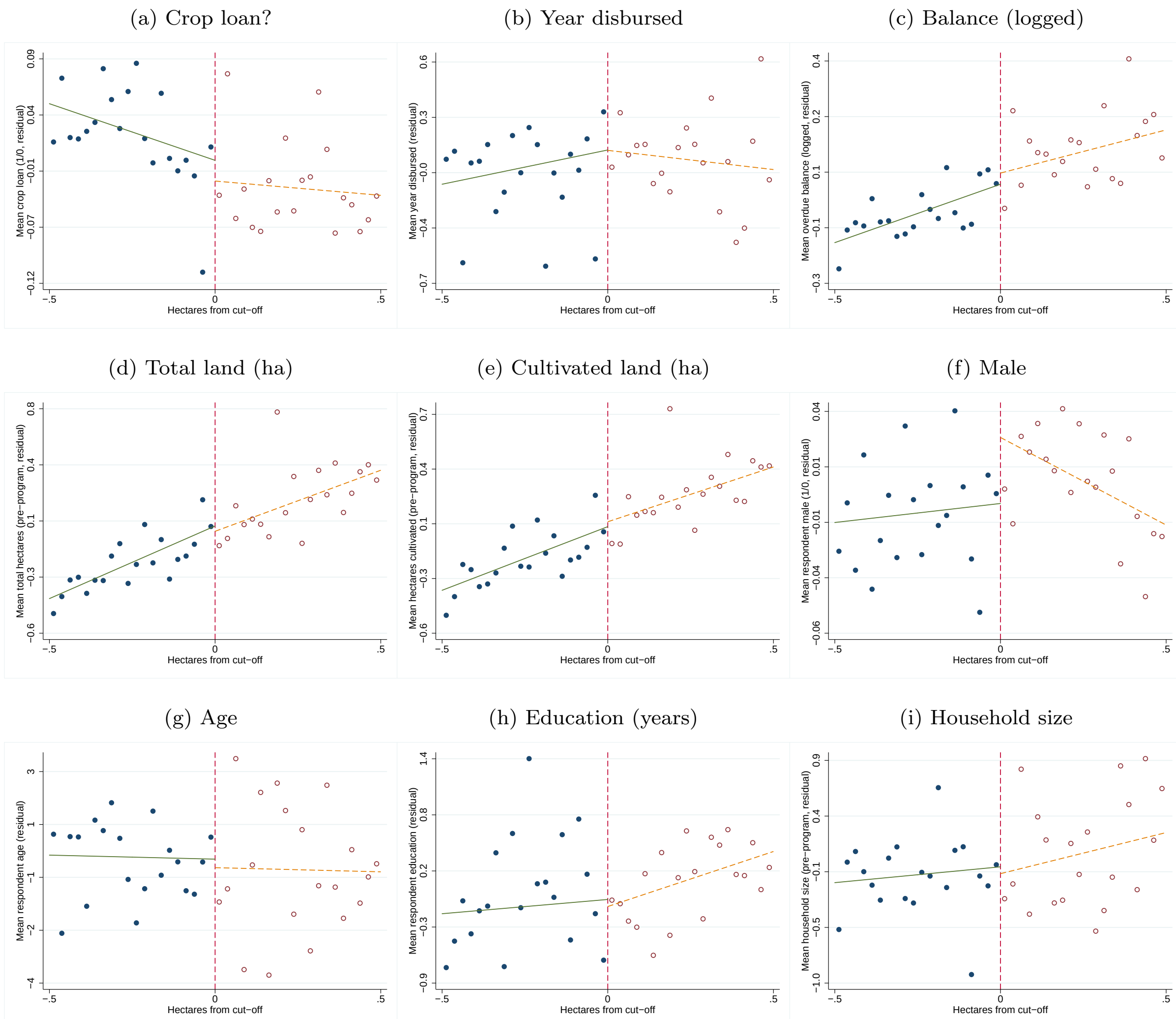


Figure A.5: Comparison of bank and audited land distributions, cooperative banks



This excludes the audited land records falling outside the acre range shown above. The full distribution of audited land records includes many smaller and larger landholdings.

Figure A.6: Continuity check for static and pre-program variables



(a) Crop loan? (b) Year disbursed (c) Balance (logged) (d) Total land (ha) (e) Cultivated land (ha) (f) Male (g) Age (h) Education (years) (i) Household size

Variables are a dummy for crop loan (vs. investment credit), year of loan disbursal, overdue balance size (Rs., logged), total self-reported land ownership (in hectares), total land cultivated in 2007 (in hectares), a dummy for male respondents, respondent age, respondent education (in years), and household size in 2007. Residual on y axis is after controlling for fixed effects (bank $\times$ district, interviewer, and month-of-survey). Lines of best fit indicated to either side of the landholding cut-off. Corresponding quantitative results can be found in Table A.5.

Appendix tables

Table A.1: Debt-relief beneficiaries by bank and district

	Anand	Kheda	Gandhinagar	Mehsana	Total
BOB	1,941	3,644	503	1,070	7,158
BOI	877	870	343	432	2,522
CBI	1,384	738	243	253	2,618
DENA	654	366	794	803	2,617
KDCC	21,141		0	0	21,141
SBI	3,412	2,711	916	3,187	10,226
UBI	1,013	1,428	306	84	2,831
Total in frame	40,179		3,105	5,829	49,113
Other banks	3,956		491	14,933	19,380
Grand total	44,135		3,596	20,762	68,493

Source: Gujarat State Level Banker's Committee

Table A.2: Sample frame beneficiaries by bank and district

	Anand	Kheda	Gandhinagar	Mehsana	Total
BOB	276	276	35	70	657
	(14%)	(8%)	(7%)	(7%)	(9%)
BOI	84	95	33	34	246
	(10%)	(11%)	(10%)	(8%)	(10%)
CBI	215	39	25	16	295
	(16%)	(5%)	(10%)	(6%)	(11%)
DENA	84	47	122	144	397
	(13%)	(13%)	(15%)	(18%)	(15%)
KDCC	1,442	1,170	0	0	2,612
	(12%)		(0%)	(0%)	(12%)
SBI	216	291	159	237	903
	(6%)	(11%)	(17%)	(7%)	(9%)
UBI	198	199	36	11	444
	(20%)	(14%)	(12%)	(13%)	(16%)
Total	2,515	2,117	410	512	5,554
	(12%)		(13%)	(9%)	(11%)

Source: Gujarat State Level Banker's Committee, bank administrative data. Percentages are proportion of total beneficiaries (reported in Table A.1) included in sample frame.

Table A.3: Summary statistics for bank data within sample frame

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

Table A.4: Survey coverage

	100% waiver	25% relief	Difference
Surveyed	55.10%	55.48%	-0.00375
			(0.0136)
Deceased	11.86%	10.26%	0.0160*
			(0.00859)
Migrated	7.23%	7.99%	-0.00755
			(0.00720)
Refused	3.16%	3.67%	-0.00510
			(0.00492)
Not located	9.38%	10.43%	-0.0105
			(0.00811)
Failed to administer	5.00%	4.50%	0.00500
			(0.00582)
Other	8.27%	7.68%	0.00592
			(0.00741)

"Surveyed" includes duplicates, where the same farmer had multiple loans in the sample frame; 2,897 surveys were administered in total. "Other" includes the few that were not attempted, those that turned out to be outside the sample area, and other exceptional cases. Standard errors in parentheses.
* p<0.10 ** p<0.05 *** p<0.01

Table A.5: Balance check for static and pre-program variables

	Crop loan? (1)	Year disbursed (2)	Balance (logged) (3)	Total land (ha) (4)	Cultivated land (ha) (5)	Male (6)	Age (7)	Education (years) (8)	Household size (9)
100% waiver	0.0247	0.0711	-0.00632	0.0610	-0.0131	-0.0275*	0.390	0.0479	0.106
	(0.0261)	(0.163)	(0.0599)	(0.0839)	(0.0859)	(0.0150)	(0.678)	(0.319)	(0.223)
Hectares from cut-off	-0.0666	0.220	0.388***	0.929***	0.778***	-0.0241	-0.152	0.687	0.567
	(0.0456)	(0.308)	(0.109)	(0.156)	(0.160)	(0.0256)	(1.367)	(0.481)	(0.452)
Constant	0.788***	2004.3***	10.92***	1.855***	1.830***	1.018***	52.51***	10.89***	4.765***
	(0.0369)	(0.0672)	(0.220)	(0.109)	(0.115)	(0.0142)	(3.017)	(1.146)	(0.619)
Observations	2887	2519	2887	2858	2835	2886	2885	2827	2886
Adjusted R^2	0.024	0.011	0.026	0.078	0.074	0.005	0.010	0.040	0.038
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variables are a dummy for crop loan (vs. investment credit), year of loan disbursal, total self-reported land ownership (in hectares), total land cultivated in 2007 (in hectares), a dummy for male respondents, respondent age, respondent education (in years), and household size in 2007. Fixed effects include bank × district, interviewer, and month-of-survey. Results robust to re-weighting observations. Standard errors in parentheses, clustered at the bank × district level.

* p<0.10 ** p<0.05 *** p<0.01

Table A.6: Debt relief's effect on happiness, sub-sample results

	Commercial banks only				Audit successes only				Small landholders only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
100% waiver	-0.0266	-0.0404	-0.451	-0.392	0.0485	0.193	-1.167	-0.848	0.0164	0.0210	0.944*	0.987
	(0.107)	(0.112)	(0.549)	(0.587)	(0.140)	(0.146)	(0.984)	(0.913)	(0.0696)	(0.0822)	(0.538)	(0.619)
100% waiver × logged balance			0.0401	0.0333			0.116	0.0995			-0.0873*	-0.0908
			(0.0511)	(0.0539)			(0.0955)	(0.0841)			(0.0495)	(0.0563)
Logged balance (mean 10.54)			0.0164	0.0113			-0.0900	-0.0691			0.128***	0.129**
			(0.0323)	(0.0356)			(0.0683)	(0.0675)			(0.0433)	(0.0470)
100% waiver × hectares from cut	-0.538	-0.499	-0.558	-0.515	-0.569	-0.392	-0.666	-0.472	0.307	0.300	0.303	0.301
	(0.408)	(0.444)	(0.405)	(0.440)	(0.566)	(0.624)	(0.574)	(0.639)	(0.336)	(0.369)	(0.343)	(0.373)
Hectares from cut-off	0.221	0.188	0.220	0.188	0.287	0.441	0.362	0.500	-0.0594	-0.0388	-0.0753	-0.0573
	(0.288)	(0.305)	(0.284)	(0.300)	(0.492)	(0.499)	(0.499)	(0.501)	(0.210)	(0.237)	(0.210)	(0.233)
Constant	0.199	0.201	0.0158	0.0737	-0.163	-0.350	0.778	0.379	0.00386	-0.0710	-1.355**	-1.420**
	(0.130)	(0.221)	(0.416)	(0.488)	(0.174)	(0.313)	(0.761)	(0.845)	(0.180)	(0.307)	(0.552)	(0.661)
Observations	1470	1469	1470	1469	838	838	838	838	1319	1319	1319	1319
Adjusted R^2	0.166	0.163	0.167	0.163	0.167	0.186	0.167	0.185	0.140	0.140	0.144	0.144
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls		Yes		Yes		Yes		Yes		Yes		Yes
Weighted		Yes		Yes		Yes		Yes		Yes		Yes

Dependent variable is self-reported happiness, standardized. Audit successes include complete and partial land audit matches within a +/- 5% threshold. Small landholders are those households with audited land ≤ 2 hectares. Fixed effects include bank × district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. Standard errors in parentheses, clustered at the bank × district level.
* p<0.10 ** p<0.05 *** p<0.01

Table A.7: Debt relief's effect on life satisfaction, sub-sample results

	Commercial banks only				Audit successes only				Small landholders only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
100% waiver	-0.0672	-0.0799	-1.321**	-1.234**	-0.103	-0.0228	-2.231***	-2.083***	0.0716	0.0779	-1.088	-1.057
	(0.0672)	(0.0680)	(0.483)	(0.490)	(0.119)	(0.153)	(0.762)	(0.737)	(0.0846)	(0.0958)	(0.884)	(0.878)
100% waiver ×			0.118**	0.109**			0.203**	0.197***			0.110	0.108
logged balance			(0.0444)	(0.0449)			(0.0739)	(0.0706)			(0.0853)	(0.0832)
Logged balance			-0.0452	-0.0364			-0.161**	-0.143*			-0.00201	0.00790
(mean 10.54)			(0.0402)	(0.0404)			(0.0667)	(0.0713)			(0.0719)	(0.0677)
100% waiver ×	-0.379	-0.426	-0.418	-0.467	0.453	0.679*	0.285	0.521	-0.159	-0.211	-0.211	-0.260
hectares from cut	(0.311)	(0.287)	(0.308)	(0.285)	(0.363)	(0.385)	(0.374)	(0.396)	(0.326)	(0.338)	(0.316)	(0.327)
Hectares from	0.0670	0.0567	0.0802	0.0720	-0.361	-0.363	-0.228	-0.242	0.183	0.194	0.187	0.200
cut-off	(0.249)	(0.256)	(0.252)	(0.259)	(0.288)	(0.324)	(0.305)	(0.333)	(0.236)	(0.264)	(0.235)	(0.262)
Constant	0.345	0.417*	0.814	0.792	0.685**	0.468**	2.372***	1.974**	0.493**	0.412**	0.487	0.317
	(0.225)	(0.234)	(0.496)	(0.513)	(0.267)	(0.213)	(0.739)	(0.798)	(0.186)	(0.162)	(0.754)	(0.705)
Observations	1468	1467	1468	1467	839	839	839	839	1319	1319	1319	1319
Adjusted R^2	0.298	0.297	0.301	0.299	0.278	0.300	0.283	0.304	0.318	0.321	0.323	0.327
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls		Yes		Yes		Yes		Yes		Yes		Yes
Weighted		Yes		Yes		Yes		Yes		Yes		Yes

Dependent variable is self-reported life satisfaction, standardized. Audit successes include complete and partial land audit matches within a +/- 5% threshold. Small landholders are those households with audited land $\leq$ 2 hectares. Fixed effects include bank $\times$ district, interviewer, and month-of-survey. Other controls include gender, audit status, and a fixed-effect for being in Mitli village, where there was a special 5-acre land distribution. Weighted regressions re-weight banks by their pre-survey distributions. Standard errors in parentheses, clustered at the bank $\times$ district level.

* p<0.10 ** p<0.05 *** p<0.01

Table A.8: Summary statistics

	N	Mean	SD	Min	Max
Cooperative?	2897	0.49	0.50	0	1
Crop loan? (otherwise investment credit)	2897	0.78	0.42	0	1
Year loan disbursed	2526	2004.28	2.12	1997	2008
Hectares of land (bank-reported)	2897	1.97	0.28	1.50	2.52
Hectares of land (bank-reported, centered at cut-off)	2897	-0.04	0.29	-0.50	0.50
Hectares of land (self-reported)	2866	1.82	1.16	0.0012	23.3
Hectares of land (from audit)	2040	1.86	1.49	0.010	15.2
100% waiver	2897	0.59	0.49	0	1
Qualifying relief amount	2897	33087.90	33398.17	11	319341
Qualifying relief amount (logged)	2897	9.97	1.02	2.40	12.7
Qualifying relief amount (others at branch, mean logged)	2894	10.09	0.67	8.08	13.9
Overdue balance	2897	50734.11	41711.44	44	611612
Overdue balance (logged)	2897	10.54	0.85	3.81	13.3
Overdue interest	2836	11839.44	15700.70	0	201074
Overdue principal	2881	39430.30	38432.04	0	830000
Relief valued as present-value wealth (lower bound)	2897	15424.51	20675.96	0	181052.3
Relief valued as present-value wealth (lower bound, logged)	2897	5.85	4.90	0	12.1
Relief valued as present-value wealth (upper bound)	2897	31745.42	34188.18	0	319341
Relief valued as present-value wealth (upper bound, logged)	2897	9.10	3.09	0	12.7
Total consumption (past year)	2897	137943.26	170643.40	0	3903950
Total consumption (past year, logged)	2897	11.57	0.71	0	15.2
Total income (past year net of ag. expenses)	2621	74521.67	135858.45	-200200	2777000
Total income (past year net, logged)	2621	9.95	2.92	0	14.8
Post-relief income (for lower-bound estimation)	2621	101677.94	142121.03	-200200	2892767
Post-relief income (for lower-bound estimation, logged)	2526	11.12	0.98	5.30	14.9
Post-relief income (for upper-bound estimation)	2621	94640.82	138534.60	-157052.8	2842634.8
Post-relief income (for upper-bound estimation, logged)	2536	11.06	0.95	3.84	14.9
Respondent male?	2895	0.97	0.18	0	1
Respondent age	2894	52.60	12.64	12	102
Respondent education (years)	2835	7.38	4.27	0	19
Household size (# members)	2896	6.60	3.48	1	35
Mitli village?	2897	0.02	0.12	0	1
Land audit matches?	2897	0.29	0.45	0	1
Land audit not done?	2897	0.30	0.46	0	1
Audited land too large?	2897	0.07	0.25	0	1
Audited land too small?	2897	0.34	0.48	0	1
Happiness (1-4)	2894	2.92	0.64	1	4
Happiness (standardized)	2894	-0.00	1.00	-2.99	1.68
Satisfaction (0-10)	2891	4.08	1.55	0	10
Satisfaction (standardized)	2891	-0.00	1.00	-2.63	3.83
Feeling of stress (prior day, "stress 1")?	2858	0.18	0.38	0	1
Anxious month (prior year, "stress 2")?	2810	0.27	0.45	0	1
Status improved a little (past year)?	2895	0.48	0.50	0	1
Status unchanged (past year)?	2895	0.29	0.45	0	1
Status worsened a little (past year)?	2895	0.01	0.11	0	1
Status worsened a lot (past year)?	2895	0.00	0.05	0	1
Status improved (past year)?	2895	0.69	0.46	0	1
Surplus at end of year (Rs., this year, logged)	2876	6.96	3.74	0	13.1
Surplus at end of year (Rs., in five years, best case, logged)	2894	10.08	1.19	0	15.4
Highest grade attainment expected for children	1776	14.72	2.13	0	22