Online Appendix

"What does Debt Relief do for Development?"

Martin Kanz*

A Location of Survey Districts

Figure OA.I: **Map of Survey Districts**

Notes: The figure shows the location of the survey districts. Bank branches in the sample are located in the districts of Anand, Kheda, Gandhinagar and Mehsana, in the central and northern part of Gujarat (marked in dark grey). A small number of sample housholds overlap into Ahmedabad district, which borders the sample area (marked in light gray).

B Economic Environment

This appendix provides additional evidence on the economic environment in the years prior to the program to document the absence of economic shocks that could have affected the treatment response. Data reported in this Appendix are taken from Giné and Kanz (2014).

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The main type of economic shocks affecting Indian agriculture are weather shocks, in particular the timing and abundance of the Northwest Monsoon. Table OA.I, Panel A, shows the deviation of monsoon rainfall from its long-run trend for a sample of 493 Indian districts for a window of ±4 years around the program date, based on official rainfall data from the *Indian Meteorological Department*. There is no indication that the survey districts, or India as a whole experienced any particularly bad monsoon seasons in the years leading up to the program. In fact, monsoon rainfall was significantly above its long-run average in the sample districts in the three years prior to the ADWDRS bailout.

Second, to check for the possibility of non-monsoon related shocks to agriculture at the time of the program (such as demand shocks or declines in the price of export crops around the time of the 2008-2009 global economic crisis), Table OA.I, Panel B.1, reports agricultural revenue per capita for the sample districts and India as a whole. There is again no indication of negative demand shocks ahead of the bailout: agricultural revenue was not significantly below its long-run average prior to the program.

Finally, Table OA.I, B.2, summarizes the performance of agricultural loans for all of India and the sample districts in the years prior to the program announcement. If Indian agriculture had experienced any dramatic shocks not captured by weather or agricultural revenue (such as shocks to specific input or commodity prices), one would expect this to be reflected in a spike of agricultural defaults ahead of the program. While there is evidence of a steady increase of loan defaults in India overall, there is no indication of a discontinuous change in the performance of agricultural loans prior to the ADWDRS bailout.

Table OA.I: **Weather Shocks and Agricultural Performance**

The table shows summary statistics for weather shocks, agricultural revenue and the performance of agricultural loans for the sample districts and India as a whole. Panel A reports data on monsoon precipitation at the district level, measured as a percentage of its long-run average. Panel B.1 reports data on agricultural revenue per hectare, with the year 2008 normalized to 100, and Panel B.2 reports data on the share of non-performing agricultural loans in percent.

	2004	2005	2006	2007	**2008**	2009	2010	2011	2012
Panel A: Monsoon precipitation									
India	87.14	97.71	95.27	103.83	**102.26**	79.29	105.12	102.05	87.38
Sample districts	95.34	180.43	163.10	122.78	**78.96**	57.62	109.80	90.39	74.10
Pabel B.1: Agricutural revenue									
India	86	93	98	96	**100**	104	112	102	98
Sample districts	99	99	91	107	**100**	117	121	122	102
Panel B.2: Non-performing agricultural loans									
India	-	-	9.7	10.4	**14.37**	6.8	8.9	9.1	12.1
Sample districts	-	-	4.2	3.2	**3.50**	3.0	6.5	9.1	10.8

C Integrity of the Assignment Variable

The identification strategy relies on the assumption that manipulation of the assignment variable, which would make selection to either side of the discontinuity non-random, was

not possible. This appendix reports the results of an audit of electronic land records to rule out the possibility of manipulation of treatment status. As noted in Section III, there is some evidence of bunching in the land distribution below the eligibility cutoff. This is concentrated in the subsample of accounts at cooperative banks, but may nonetheless give rise to the concern that manipulation of land records was possible.

Section III.C discusses the various features of the ADWDRS program that make manipulation of land records highly unlikely (such as the fact that program rules were implemented retroactively) and presents a number of robustness checks. Section IV additionally confirms that all results hold in a robustness sample, restricted to accounts at commercial banks, for which manipulation of land records can be ruled out.

As an additional robustness check, we conducted a large audit of bank reported land records, using data from Gujarat's centralized electronic *e-Dhara* database of land records. This database contains information on all official landholdings in the state, and is ideally suited to conduct an audit of bank-reported landholdings for several reasons. First, the *e-Dhara* database is centrally administered and controlled by an authrity separate from the institutions keeping land records at the village level. Second, each new entry or modification in the database undergoes several levels of scrutiny by local and central authorities to verify that the information provided is in fact authentic. Third, any changes in landholding status have to be confirmed and documented by independent authorities, so that even if borrowers had tried to manipulate their land records in response to the program, it is highly unlikely that they could have completed this process in the short time period between the program announcement and the implementation of debt relief.

Evidence from the Audit of Electronic Land Records

To compare the landholding numbers reported by banks and survey respondents, we obtained official copies of the land records for 2,064 of the 2,897 survey respondents (71% of the sample). To collect electronic land records, survey teams approached the district accountant's office in each of the sample districts and collected printouts of the official land records for all sample households for which this information was available (district officials can access the *e-Dhara* database and generate printouts, but are not able to amend land records without additional cross-checks and verification). Table OA.II reports the land audit results.

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

Second, in a much smaller number of cases loans considered the landholdings of multiple individuals. Most frequently, land held by 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 own the entire listed land. In these cases, the bank-reported landholding is greater than the land owned by the farmer. This is also legitimate and does not violate the identification

assumption requiring no manipulation of landholdings in response to the program rules.

Third, rounding and conversion errors were common, as landholding was recorded in a variety of complex and region-specific units of measurement. Since official land documents

Table OA.II: **Audit of Electronic Land Records**

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

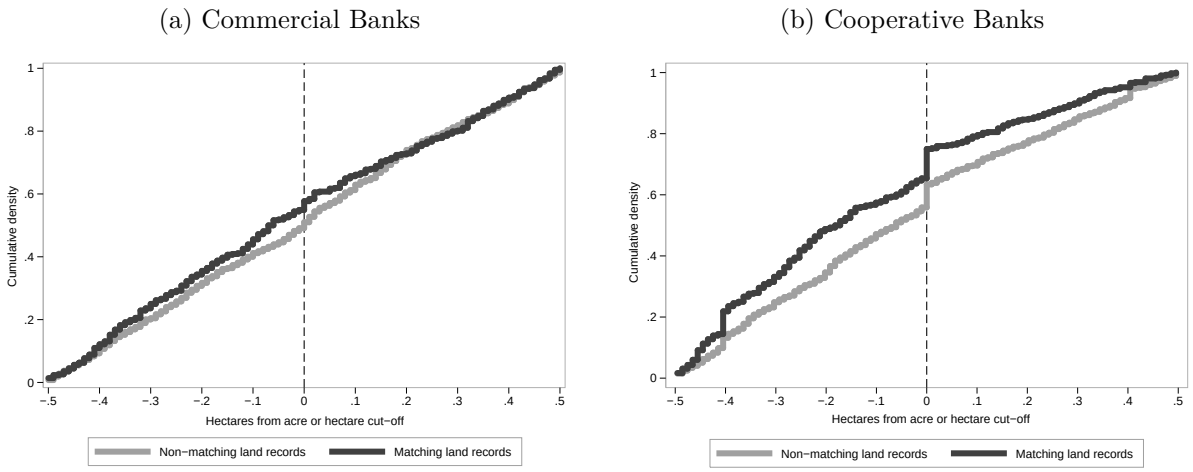
	Electronic land records				**Total**
	$\leq$ bank record (N=1,942)		**>bank record (N=122)**		
Treatment	1,112	(93%)	82	(7%)	1,194
Control	830	(95%)	40	(5%)	870
Total	1,942	(94%)	122	(6%)	2,064

almost never reported landholding in the same units as banks, there were nearly always opportunities for rounding and conversion errors. In assessing whether an official landholding record matches the corresponding bank report, we therefore allow for a $\pm 5\%$ margin for error. In addition, since landholding documents sometimes report distinct plots of land, we allow for either total-land or partial-land matches: if any combination of listed plots adds up to the size reported by the bank, within $\pm 5\%$, we consider this a match. This match protocol retains considerable power, and both excluding partial-land matches and using a $\pm 1\%$ margin of error has only negligible effects on the overall match rate. With landholding documents for 71% of surveyed households, manipulation of the forcing variable can be ruled out for 96.1% of audited households. Note that a potential case of manipulation would be one in which the land reported by the bank is *smaller* than the land reported in official documents. We find that this is the case for only 82 households or 3.9% of the audited sample. The rate of exact matches is 41.4%. Of the cases that fail to match, 83.5% fail to match because the total official landholding is too small to match with the bank report. These appear to be cases where multiple landholdings were pooled, or cases where land was misreported on the high side in order to qualify for a larger loan. In either case, note that this works *against* debt relief qualification: given that qualification depended on landholding being below the program cutoff, over-reporting land makes qualification for debt relief *less* likely, thus identifying these accounts as cases where we can rule out manipulation of land records in order to qualify for debt relief. The robustness sample used in the analysis excludes all accounts for which manipulation cannot be ruled out.

These results are also robust to a more restrictive definition of the robustness sample that excludes non-matching land on both sides of the discontinuity. As an additional test, Figure OA.II plots the cumulative distribution functions for commercial and cooperative bank landholdings separately for matching and non-matching accounts. If the spikes in the land distribution were indeed due to the manipulation of land records, rather than rounding, we would expect to see no evidence of bunching for the distribution of audit matches.

For bank accounts, shown in Panel (a), accounts with matching and non-matching land records appear to follow very similar distributions. To a slight extent, matching land appears more heavily concentrated at the low end of the distribution. This pattern is similar but more pronounced for the cooperative landholding distributions shown in Panel (b). Note,

Figure OA.II: **Land Distributions by Bank Type and Audit Result**



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

however, that the same spike at 5 acres is equally evident in both the matching and non-matching land distributions. Taken together, this suggests bunching arising from rounding around full numbers, rather than strategic manipulation of the running variable in response to the program. As can be seen from Figure OA.II (b), the higher concentration of matching land on the low end of the distribution is a combination of two factors: a slightly higher audit rate for smaller landholdings (i.e., a higher propensity to secure the official land documents) and a slightly higher propensity for land documents to match, once secured. Note that both the audit rate and the match rate are markedly higher just to the left of the cutoff than to the right. This is precisely the opposite of what one would expect to happen in the presence of manipulation: If there were indeed significant manipulation in the vicinity of the program eligibility threshold, we should be *less* likely to locate official documents for corrupt farmers, and land should match at much lower rates below the program eligibility cutoff.

Finally, we can compare the distribution of landholdings for all holdings between 4 and 6 acres. By ignoring whether land matches or not, this allows for a comparison of the raw land distributions, as reported in bank and government sources. These distributions are visually indistinguishable, and a Kolmogorov-Smirnov test fails to reject the equality of distributions with p=0.357. This suggests that the bank-reported distribution is almost certainly a case of natural bunching, rather than the result of deliberate bank or borrower manipulation.

D A Simple Model of Debt Relief and Investment

To motivate the empirical analysis, this section develops a simple two-period model of debt, credit and investment. The model describes how a household with existing debt on its balance sheet decides to undertake an investment that requires external credit to finance. The household's access to credit, as well as the private return of the available investment opportunity depend on how much of its accumulated debt the household chooses to repay. The government can help the household repay part of or all of its debt. If a household wishes to obtain a new loan after debt relief, it faces fixed cost κ, which may be either constant $\frac{\partial \bar{\kappa}}{\partial\{(1-\delta)D\}} = 0$, or increasing in the amount of debt relief received $\frac{\partial \kappa}{\partial\{(1-\delta)D\}} > 0$.

There are two periods. In period 1, a household starts with endowment e and inherited debt D, owed to an external creditor. The household can take advantage of a government bailout program offering partial or full debt relief so that the household pays the creditor $\delta D, \delta \in [0,1]$, while the government pays $(1-\delta)D$. Assume that $\delta D \leq e < D$ so that without the bailout, the household can repay only a fraction of its accumulated debt. The household faces a profitable investment opportunity that requires external financing k, where $k > e$.

In period 2, the investment will yield a high return y_H with probability $p \in (0,1)$, and a low return y_L with probability $1-p$, where $y_H > y_L$. For simplicity, assume that the household is risk-neutral and consumes only in period 2. This yields three possible scenarios:

1. No debt relief: If the household cannot repay its debt from its own revenues, so that $\delta = 0$ in period 1, and no debt relief is available, the household cannot obtain new financing k and will not be able to invest. In this case, the creditor will will confiscate the household's endowment, leaving the household with 0 consumption.

2. Partial debt relief: If the household is able to obtain partial debt relief in period 1, it pays down a fraction δ of its accumulated debt D and keeps $e - \delta D$. The household can now decide to invest, in which case it needs, and is able to, obtain new credit $b = k - (e - \delta D) - \kappa$, where κ is the fixed cost of establishing a new credit relationship. If this cost is prohibitively high, such that $k < (e - \delta D) + \kappa$, the household will not obtain a new loan and forgo the investment opportunity. If the household invests, it borrows amount b. Let r be the rate of return demanded by the creditor, and assume that $y_L < (1+r)b < y_H$. That is, only in the good state will the household be able to fully repay b with interest. In the bad state, the creditor will confiscate y_L from the household, leaving it 0 consumption. In this case, the household's period 2 consumption will be:

$$p[y_H - (1+r)b] + (1-p) \cdot 0$$

Hence, in period 1, the household solves the optimization problem:

$$\mathrm{E[U]} = max\{e - \delta D, p[y_H - (1+r)b]\}$$

If a household receives only partial debt relief and therefore carries over part of its accumulated debt D into period 2, this choice contains the standard debt overhang problem. In order to undertake a profitable investment, a household with accumulated debt $D > 0$ on its balance sheet will demand a return higher than that required to break even because part of the returns of its investment goes to creditors rather than to the household. That is, an

indebted household will give up a positive NPV investment opportunity with probability

$$py_H \geq (1+r)b - (1-p)y_L$$

unless it is the case that,

$$p[y_H - (1+r)b] \geq e - \delta D \geq 0$$

Note that the debt overhang problem is exacerbated by the upfront payment δD, required to service inherited debt. Hence, the greater the stock of inherited debt D the greater is the incentive distortion and the lower the probability that the household will invest and increase the amount the household needs to borrow to undertake the investment $k - e < b$.

More generous debt relief will reduce the fraction of debt $\delta \in [0, 1]$ required to be repaid by the household. This affects the household's investment decision on the extensive and intensive margin. On the extensive margin, debt relief makes investment a choice variable for indebted households (relaxing credit constraints). On the intensive margin, conditional on obtaining new credit, debt relief alleviates incentive distortions and encourages investment by increasing the household's expected returns from investment (removing debt overhang).

3. Full debt relief: If a household obtains full and unconditional debt relief in period 1, then $\delta = 0$ and $D = 0$. The household does not carry over any accumulated debt into period 2 and the household's period 1 optimization problem becomes:

$$\mathrm{E[U]} = max\{0, p[y_H - (1+r)b]\}$$

Hence, conditional on obtaining new credit, the household is more likely to invest under full debt relief than under partial debt relief.

Predictions: The model makes several straightforward predictions. First, the impact of debt relief on investment *incentives* is always positive, because a reduction in debt service increases the household's private return from undertaking the investment (relaxing incentive constraints). Second, the removal of the investment disincentive achieved by setting $D = 0$ will, however only achieve an increase in investment if the household is able to obtain new financing (relaxing liquidity constraints). This could fail to occur due to supply or demand side factors captured by the term κ, which denotes the cost of initiating a new lending relationship. This cost could be prohibitively high, if lenders discriminate based on debt relief status (credit supply), for example by creating red tape for bailout beneficiaries, or if households discount the benefit of the lending relationship based on negative past experiences with debt enforcement by creditors (credit demand). The model thus highlights that debt relief will increase in investment only if it resolves both incentive and liquidity constraints.

E Appendix Tables: Survey and Additional Results

Table OA.III: **Household Survey** – Eligible Population and Sample Frame

The Table reports summary statistics of the sample population and sample frame. Panel A reports summary statistics on program beneficiaries by bank and district. Observations cover all beneficiary accounts from the largest six commercial banks and the state's largest cooperative bank, accounting for 91% of eligible accounts in the districts covered by the survey. Panel B summarizes all accounts included in the sample frame and qualifying for a 100% waiver or 25% conditional debt relief. Observations in the sample frame are drawn from administrative data published by the largest six commercial banks and the largest cooperative bank in the state of Gujarat. Percentages refer to the proportion of total beneficiaries included in the sample frame.

Panel A: Program Beneficiaries by Bank and District

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

Panel B: Sample Frame by Bank and District

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

Table OA.IV: **Household Survey** – Tests for Balance and Attrition

Panel A reports tests for balanced survey coverage. Each column represents results from a separate regression based on the entire sample frame (N=5,554). "Surveyed" is a dummy variable equal to one if a beneficiary household was located and completed the entire survey. Treated is a dummy variable equal to one for all households that had pledged < 2 hectares of land as collateral and were eligible for 100% debt relief. Panel B presents tests for balanced attrition across treatment and control. "Surveyed" includes duplicates, where the same beneficiary had multiple loans in the sample frame; 2,897 surveys were administered in total. "Other" includes a small number of surveys that were not attempted and respondents outside the sample area. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

Panel A : Survey Coverage

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

Panel B: Test for Balanced Attrition

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

Table OA.V: **Density of the Assignment Variable**

This table reports the results of McCrary (2009) test for discontinuities in the density of the assignment variable. I report results for the sample of all surveyed households, the sample of accounts held at commercial banks, the sample of accounts held at cooperative banks, and the sample of accounts that underwent an audit of electronic land records and showed no sign of manipulation. The test shows some evidence of bunching below the two hectare eligibility threshold. This is concentrated among accounts held at cooperative banks. To ensure that this is not due to manipulation of land records in response to the program, all results are presented for the full sample as well as two robustness samples for which manipulation of land records can be ruled out: the sample of commercial bank accounts and the sample of accounts with audited and matching electronic land records.

	Discontinuity at cutoff	Standard error	Default bandwidth	Default bin size
All accounts	-.0.723	(0.118)	0.179	0.011
Commercial bank accounts	0.148	(0.144)	0.273	0.015
Cooperative bank accounts	-1.740	(0.219)	0.169	0.015

Table OA.VI: **Loan Applications and Approvals**

This table reports the effect of debt relief on new loan applications and approvals after the program. The dependent variable in column (1) is a dummy variable equal to one if a household applied for a new bank loan. The dependent variable in column (2) is a dummy equal to one if a household applied for a new loan and got turned down. The sample in column (2) is restricted to households that applied for a new loan after the program. Respondent controls include gender, age, years of education, household size, log of pre-program land owned, log of pre-program total debt. Regressions additionally include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	New loan Applied=1	Application rejected=1
	(1)	(2)
100% Relief	0.01	0.05
	[0.03]	[0.06]
Observations	2,830	523
R-squared	0.085	0.139
Polynomial order	one	one
Respondent controls	Yes	Yes

Table OA.VII: **Impact on Investment, by Type of Input**

This table reports the effect of debt relief on post-program investment by type of input. Each column reports results from a separate regression. The dependent variable is total expenditure for irrigation in column (1), expenditure on seeds in column (2), expenditure on fertilizer and pesticides in column (3), and expenditures on hired outside labor in column (4). All outcome variables are averages over the two main post-program crop seasons. Respondent controls include gender, age, years of education, household size, log of pre-program land owned, log of pre-program total debt. Regressions additionally include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	Irrigation	Seeds	Fertilizer and Pesticides	Labor
	(1)	(2)	(3)	(4)
100% Relief	-0.37*	-0.30*	-0.32*	-0.17
	[0.17]	[0.15]	[0.17]	[0.17]
Observations	2,762	2,762	2,762	2,762
R-squared	0.085	0.104	0.083	0.104
Polynomial order	one	one	one	one
Respondent controls	Yes	Yes	Yes	Yes

Table OA.VIII: **Impact on Crop Choice and Variance of Output**

This table reports the effect of debt relief on crop choice and the variance of agricultural revenue. Each column reports results from a separate regression. The dependent variable in column (1) is the standard deviation of agricultural revenue over the two main post-program crop seasons. The dependent variable in column (2) is a dummy equal to one if a household changed the composition of crops grown between summer 2007 and summer 2008, the dependent variable in column (3) is a dummy equal to one if a household changed the composition of crops grown between summer 2007 and summer 2009. Respondent controls include gender, age, years of education, household size, log of pre-program land owned, log of pre-program total debt. Regressions additionally include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	StdDev(Ag. Revenue)	Switched Crop=1	
		2007 vs 2008	2007 vs 2009
	(1)	(2)	(3)
100% Relief	-0.23	0.02	0.02
	[0.20]	[0.03]	[0.02]
Observations	2,762	2,202	2,471
R-squared	0.085	0.073	0.066
Polynomial order	one	one	one
Respondent controls	Yes	Yes	Yes

F Appendix Tables: Alternative Specification

Table OA.IX: **Regression Results** – Total Household Debt

Within a panel, each column reports results from a separate regression with the dependent variable in the column header and explanatory variables in rows. Each dependent variable is regressed on the share of debt forgiven under the program, instrumented with a indicator equal to one if the respondent household is below the two hectare collateral cutoff, and a control function of the polynomial order indicated at the foot of the table to each side of the cutoff. The dependent variable in columns (1) to (4) is the difference between total self-reported debt outstanding at the time of the survey and total debt outstanding at the time of the program eligibility date in December 2007, in units of Rs '000. The dependent variable in columns (5) to (8) is a dummy equal to one if a household applied for a new bank loan after the program. Panel A reports estimates for the entire sample. Panel B reports estimates for two robustness samples that restrict the sample to accounts with audited and matching land records, and exclude accounts at cooperative banks. Respondent controls include gender, age, years of education, household size, log land owned pre-program. All regressions include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	Δ Total debt				Applied for new bank loan=1			
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bailout_share	-45.16**	-32.33**	-29.40*	-28.80*	0.02	0.02	0.02	0.02
	(19.12)	(16.04)	(15.97)	(15.79)	(0.05)	(0.04)	(0.04)	(0.04)
Observations	1,093	2,191	2,191	2,191	1,416	2,751	2,751	2,751
R-squared	0.085	0.049	0.050	0.050	0.117	0.100	0.101	0.102
Dep. variable mean (pre)	93.20	93.20	93.20	93.20	1.77	1.77	1.77	1.77
Panel B: Robustness samples								
Audited accounts								
Bailout_share	-49.73**	-31.51*	-29.94*	-29.59	-0.03	0.00	-0.00	-0.01
	(21.55)	(17.68)	(18.03)	(18.27)	(0.06)	(0.05)	(0.05)	(0.05)
Observations	694	1,418	1,418	1,418	879	1,750	1,750	1,750
R-squared	0.101	0.075	0.076	0.076	0.141	0.119	0.119	0.121
Dep. variable mean (pre)	93.36	93.36	93.36	93.36	1.76	1.76	1.76	1.76
Bank accounts only								
Bailout_share	-8.68	-19.64	-20.10	-20.07	-0.04	-0.03	-0.03	-0.03
	(33.20)	(27.57)	(27.57)	(27.54)	(0.07)	(0.05)	(0.05)	(0.05)
Observations	583	1,092	1,092	1,092	755	1,410	1,410	1,410
R-squared	0.146	0.090	0.091	0.091	0.129	0.120	0.121	0.122
Dep. variable mean (pre)	135.18	135.18	135.18	135.18	1.72	1.72	1.72	1.72
Bandwidth	$\frac{1}{2}$	full	full	full	$\frac{1}{2}$	full	full	full
Polynomial order	zero	zero	one	two	zero	zero	one	two
Respondent controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Table OA.X: **Regression Results** – Sources of Credit

Within a panel, each column reports results from a separate regression with the dependent variable in the column header and explanatory variables in rows. Each dependent variable is regressed on the share of debt forgiven under the program, instrumented with a indicator equal to one if the respondent household is below the two hectare collateral cutoff, and a control function of the polynomial order indicated at the foot of the table to each side of the cutoff. The dependent variable in columns (1) to (4) is the share of credit obtained from banks. The dependent variable in columns (5) to (8) is the share of credit obtained from informal lenders. Banks include commercial and cooperative banks, informal lenders include moneylenders, shopkeepers, traders, friends and relatives. Panel A reports estimates for the entire sample. Panel B reports estimates for two robustness samples that restrict the sample to accounts with audited and matching land records, and exclude accounts at cooperative banks. Respondent controls include gender, age, years of education, household size, log land owned pre-program. All regressions include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	Sources of credit							
	Banks				Informal lender			
Panel A: Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bailout_share	-9.00*	-9.75**	-9.66**	-9.63**	4.98	7.39**	7.00**	6.98**
	(5.02)	(3.97)	(4.00)	(4.00)	(3.76)	(2.96)	(2.95)	(2.95)
Observations	1,355	2,617	2,617	2,617	1,354	2,615	2,615	2,615
R-squared	0.207	0.190	0.190	0.190	0.135	0.122	0.123	0.123
Dep. variable mean (pre)	86.67	86.67	86.67	86.67	8.46	8.46	8.46	8.46
Panel B: Robustness samples								
Audited accounts								
Bailout_share	-5.19	-9.80**	-9.63*	-9.62*	5.96	8.24**	8.09**	8.07**
	(6.35)	(5.00)	(5.04)	(5.04)	(4.79)	(3.65)	(3.65)	(3.65)
Observations	843	1,672	1,672	1,672	843	1,671	1,671	1,671
R-squared	0.242	0.207	0.208	0.208	0.174	0.141	0.141	0.141
Dep. variable mean (pre)	86.80	86.80	86.80	86.80	8.40	8.40	8.40	8.40
Bank accounts only								
Bailout_share	-9.17	-10.45**	-10.59**	-10.41**	0.54	6.81*	6.98*	6.95*
	(7.09)	(5.15)	(5.15)	(5.15)	(4.86)	(3.76)	(3.77)	(3.78)
Observations	730	1,369	1,369	1,369	730	1,368	1,368	1,368
R-squared	0.185	0.191	0.192	0.192	0.166	0.135	0.137	0.137
Dep. variable mean (pre)	86.97	86.97	86.97	86.97	8.83	8.83	8.83	8.83
Bandwidth	$\frac{1}{2}$	full	full	full	$\frac{1}{2}$	full	full	full
Polynomial order	zero	zero	one	two	zero	zero	one	two
Respondent controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Table OA.XI: **Regression Results** – Savings and Consumption

Within a panel, each column reports results from a separate regression with the dependent variable in the column header and explanatory variables in rows. Each dependent variable is regressed on the share of debt forgiven under the program, instrumented with a indicator equal to one if the respondent household is below the two hectare collateral cutoff, and a control function of the polynomial order indicated at the foot of the table to each side of the cutoff. The dependent variable in columns (1) to (4) is log total household savings. The dependent variable in columns (5) to (8) is log household consumption expenditure on non-durables over the previous one-month period. The dependent variable in columns (9) to (12) is log household expenditure on durables over the previous one-year period. Panel A reports estimates for the entire sample. Panel B reports estimates for two robustness samples that restrict the sample to accounts with audited and matching land records, and exclude accounts at cooperative banks. Respondent controls include gender, age, years of education, household size, log land owned pre-program. All regressions include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	Log(savings)				Log(consumption)				Log(durables consumption)			
Panel A: Full sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bailout_share	-0.29	-0.11	-0.09	-0.09	0.00	-0.03	-0.03	-0.03	-0.06	-0.24	-0.26	-0.27
	(0.36)	(0.26)	(0.26)	(0.26)	(0.07)	(0.04)	(0.04)	(0.04)	(0.39)	(0.30)	(0.30)	(0.30)
Observations	1,158	2,281	2,281	2,281	1,416	2,753	2,753	2,753	1,416	2,753	2,753	2,753
R-squared	0.186	0.176	0.177	0.177	0.203	0.379	0.380	0.380	0.399	0.411	0.411	0.411
Dep. variable mean	6.31	6.31	6.31	6.31	8.76	8.76	8.76	8.76	3.93	3.93	3.93	3.93
Panel B: Robustness samples												
Audited accounts												
Bailout_share	-0.38	0.03	0.03	0.02	-0.12	-0.10**	-0.11**	-0.11**	-0.59	-0.49	-0.52	-0.52
	(0.44)	(0.32)	(0.32)	(0.32)	(0.09)	(0.05)	(0.05)	(0.05)	(0.52)	(0.39)	(0.40)	(0.40)
Observations	725	1,461	1,461	1,461	879	1,752	1,752	1,752	879	1,752	1,752	1,752
R-squared	0.205	0.198	0.198	0.198	0.234	0.390	0.390	0.390	0.389	0.411	0.411	0.411
Dep. variable mean	6.39	6.39	6.39	6.39	8.77	8.77	8.77	8.77	3.91	3.91	3.91	3.91
Bank accounts only												
Bailout_share	-0.21	0.16	0.17	0.18	0.14	-0.00	-0.00	-0.00	0.01	-0.30	-0.30	-0.29
	(0.56)	(0.39)	(0.39)	(0.39)	(0.10)	(0.06)	(0.06)	(0.06)	(0.58)	(0.42)	(0.42)	(0.41)
Observations	628	1,180	1,180	1,180	755	1,412	1,412	1,412	755	1,412	1,412	1,412
R-squared	0.163	0.171	0.172	0.172	0.240	0.383	0.384	0.384	0.432	0.415	0.415	0.415
Dep. variable mean	6.34	6.34	6.34	6.34	8.86	8.86	8.86	8.86	4.61	4.61	4.61	4.61
Bandwidth	$\frac{1}{2}$	full	full	full	$\frac{1}{2}$	full	full	full	$\frac{1}{2}$	full	full	full
Polynomial order	zero	zero	one	two	zero	zero	one	two	zero	zero	one	two
Respondent controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Table OA.XII: **Regression Results** – Investment and Productivity

Within a panel, each column reports results from a separate regression with the dependent variable in the column header and explanatory variables in rows. Each dependent variable is regressed on the share of debt forgiven under the program, instrumented with a indicator equal to one if the respondent household is below the two hectare collateral cutoff, and a control function of the polynomial order indicated at the foot of the table to each side of the cutoff. The dependent variable in columns (1) to (4) is log total agricultural investment measured over the first two post-program crop seasons and includes expenditures for fertilizer, pesticides, irrigation and ploughing. The dependent variable in columns (5) to (8) is agricultural productivity measured as agricultural revenue per acre of cultivated land. Panel A reports estimates for the entire sample. Panel B reports estimates for two robustness samples that restrict the sample to accounts with audited and matching land records, and exclude accounts at cooperative banks. Respondent controls include gender, age, years of education, household size, log land owned pre-program. All regressions include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	Log(investment per acre)				Log(revenue per acre)			
Panel A: Full sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bailout_share	-0.20*	-0.18**	-0.18**	-0.18**	-0.17	-0.16*	-0.16*	-0.16*
	(0.11)	(0.08)	(0.08)	(0.08)	(0.13)	(0.09)	(0.09)	(0.09)
Observations	1,190	2,431	2,431	2,431	1,180	2,403	2,403	2,403
R-squared	0.155	0.256	0.256	0.256	0.159	0.205	0.205	0.205
Dep. variable mean	8.01	8.01	8.01	8.01	9.51	9.51	9.51	9.51
Panel B: Robustness samples								
Audited accounts								
Bailout_share	-0.06	-0.15	-0.14	-0.14	-0.06	-0.19	-0.18**	-0.18**
	(0.14)	(0.10)	(0.10)	(0.10)	(0.17)	(0.12)	(0.09)	(0.09)
Observations	739	1,553	1,553	1,553	734	1,539	1,539	1,539
R-squared	0.164	0.272	0.273	0.273	0.160	0.207	0.208	0.208
Dep. variable mean	8.01	8.01	8.01	8.01	9.49	9.49	9.49	9.49
Bank accounts only								
Bailout_share	-0.02	-0.14	-0.14	-0.14	-0.04	-0.15	-0.16	-0.16
	(0.15)	(0.10)	(0.10)	(0.10)	(0.18)	(0.13)	(0.13)	(0.13)
Observations	655	1,245	1,245	1,245	653	1,237	1,237	1,237
R-squared	0.205	0.276	0.277	0.277	0.188	0.202	0.202	0.202
Dep. variable mean	8.11	8.11	8.11	8.11	9.61	9.61	9.61	9.61
Bandwidth	$\frac{1}{2}$	full	full	full	$\frac{1}{2}$	full	full	full
Polynomial order	zero	zero	one	two	zero	zero	one	two
Respondent controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Table OA.XIII: **Expectations** – Reputation and Access to Credit

Within a panel, each column reports results from a separate regression with the dependent variable in the column header and explanatory variables in rows. Each dependent variable is regressed on the share of debt forgiven under the program, instrumented with a indicator equal to one if the respondent household is below the two hectare collateral cutoff, and a control function of the polynomial order indicated at the foot of the table to each side of the cutoff. The dependent variable in columns (1) to (4) uses answers to the survey question "if you were to default on a loan from a bank/informal lender, how likely would this be to tarnish your reputation in your community?" on a scale from 1 (no effect) to 5 (severe negative effect). The dependent variable in columns (5) to (8) is based on answers to the survey question "If you were to default on a loan from a bank/informal lender, how likely would this be to reduce your ability to borrow from this lender in the future?" on a scale from 1 (no effect) to 5 (severe negative effect). Panel A reports estimates for the entire sample. Panel B reports estimates for two robustness samples that restrict the sample to accounts with audited and matching land records, and exclude accounts at cooperative banks. Respondent controls include gender, age, years of education, household size, log land owned pre-program. All regressions include interviewer, month-of-interview and bank-district fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. * p< 0.10 ** p <0.05 *** p <0.01.

	Expected impact on reputation				Expected impact on access to credit			
Panel A: Full sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bailout_share	0.09	-0.16	-0.15	-0.15	0.15	0.50**	0.50**	0.49**
	(0.10)	(0.12)	(0.12)	(0.12)	(0.18)	(0.20)	(0.20)	(0.20)
Observations	1,410	2,388	2,388	2,388	1,397	2,366	2,366	2,366
R-squared	0.476	0.448	0.449	0.449	0.477	0.416	0.416	0.417
Dep. var. mean	3.78	3.78	3.78	3.78	3.93	3.93	3.93	3.93
Panel B: Robustness samples								
Audited accounts								
Bailout_share	0.14	-0.00	0.02	0.02	-0.02	0.59**	0.61**	0.59**
	(0.13)	(0.16)	(0.16)	(0.16)	(0.22)	(0.24)	(0.24)	(0.24)
Observations	876	1,510	1,510	1,510	871	1,501	1,501	1,501
R-squared	0.493	0.459	0.460	0.460	0.492	0.421	0.421	0.423
Dep. var. mean	3.78	3.78	3.78	3.78	3.94	3.94	3.94	3.94
Bank accounts only								
Bailout_share	0.17	-0.13	-0.13	-0.12	0.27	0.66**	0.66**	0.72**
Dep. var. mean	(0.14)	(0.17)	(0.17)	(0.17)	(0.24)	(0.29)	(0.29)	(0.29)
Observations	751	1,056	1,056	1,056	747	1,048	1,048	1,048
R-squared	0.469	0.487	0.487	0.487	0.511	0.493	0.493	0.496
	3.72	3.72	3.72	3.72	3.85	3.85	3.85	3.85
Bandwidth	$\frac{1}{2}$	full	full	full	$\frac{1}{2}$	full	full	full
Polynomial order	zero	zero	one	two	zero	zero	one	two
Respondent controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes