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The Economic Effects of a Borrower Bailout

Evidence from an Emerging Market

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Abstract

This paper studies the credit market implications and real effects of one the largest borrower bailout programs in history, enacted by the government of India against the backdrop of the 2008–2009 financial crisis. The study finds that the stimulus program had no effect on productivity, wages, or consumption, but led to significant changes in credit allocation and an increase in defaults. Post-program loan performance declines faster in districts with greater exposure to the program, an effect that is not driven by greater risk-taking of banks. Loan defaults become significantly more sensitive to the electoral cycle after the program, suggesting the anticipation of future credit market interventions as an important channel through which moral hazard in loan repayment is intensified.

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Produced by the Research Support Team

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1 Introduction

Since the Great Depression, economic stimulus programs have been ubiquitous during times of economic crisis in advanced and developing economies. In their simplest form, such programs provide direct subsidies or income support to households through the tax code. In many cases, however, economic stimulus programs operate through the credit market, typically in the form of debt restructuring and debt relief programs designed to encourage investment and consumption during economic downturns.

Stimulus programs that operate through the credit market remain controversial among economists and policy makers for at least two reasons. First, as in the case of fiscal stimulus programs more generally, critics question whether ex-post credit market interventions can affect real economic activity (Agarwal et al. [2005], Mian and Sufi [2012]). Second, while proponents argue that stimulus programs through the credit market may strengthen household balance sheets and prevent excessive deadweight losses in times of economic crisis (Bolton and Rosenthal [2002], Mian et al. [2013], Mian and Sufi [2014]), opponents argue that credit market interventions are a particularly harmful way of implementing an economic stimulus, as they change the contracting environment and may generate bank and borrower moral hazard. Although credit market-led stimulus programs are common, there is surprisingly little evidence on how they affect credit market outcomes and the real economy.

We address these questions by evaluating one of the largest borrower bailout programs in history, enacted by the government of India against the backdrop of the global financial crisis of 2008–2009. The program, known as the "Agricultural Debt Waiver and Debt Relief Scheme" (Adwdrs), consisted of unconditional debt relief for more than 60 million rural households across India, amounting to a volume of more than US$ 16 billion (approximately 1.7% of GDP). We exploit a natural experiment generating variation in bailout exposure to estimate the effect of the bailout on the credit market and real economic activity.

The key challenge in identifying the causal effect of the bailout is the difficulty of constructing a valid counterfactual. Because cross-sectional exposure to the debt relief is a function of loan delinquencies prior to the program, one might worry that estimates of program impact are endogenous to pre-existing economic trends. To address this concern, our identification strategy takes advantage of plausibly exogenous cross-sectional variation in program exposure, generated by the ADWDRS program eligibility rules. We additionally validate our credit market estimates using bank-district data, which allows us to exploit within-district variation to explicitly control for local shocks to credit demand.

We find that the bailout had a significant and economically large effect on post program credit allocation and loan performance. A one standard deviation increase in bailout exposure led to a 4-6% decline in the number of loans and a 13-16% decline in the amount of credit outstanding. The results also reveal a significant reallocation of credit away from districts with high exposure to the bailout. While districts with high (above median) program exposure received 36 cents of new lending for every 1 dollar of credit written off under ADWDRS, districts with low (below median) program exposure received 4 dollars for every 1 dollar of debt relief. Despite this reallocation of new bank lending to observably "less risky" districts, we document a significant decline in loan performance in districts with high bailout exposure, concentrated among borrowers that had previously been in good standing.

In the second part of our analysis, we examine the effect of debt relief on the real economy. Supporters of ADWDRS, including India's government at the time, cited debt overhang and investment constraints as one of the key motivations for the program and argued that debt relief could not only provide immediate relief to households, but also improve agricultural investment, with positive implications for rural productivity, wages and consumption. We use detailed, regionally disaggregated data to test for the effect of the bailout on rural productivity, wages and employment. Our results identify a precise zero for each of these outcomes. This is consistent with household-level evidence on the impact of ADWDRS (see

Kanz [2012]), implies a low spending multiplier of debt relief, and suggests that the program did not achieve the impact on real economic activity claimed by its proponents.

There are several reasons that make India's bailout for rural households an especially attractive setting to explore the impact of a credit market-led stimulus program. First, ADWDRS is representative of a wide range of stimulus programs executed through interventions into the credit market. In the United States, federal and state governments have frequently intervened into debt contracts, with some of the largest credit market interventions occurring in the aftermath of the Great Depression (Rucker and Alston [1987]). More recently, in the context of the 2008 financial crisis, it has been argued that the weakness of household balance sheets, rather than the breakdown of financial intermediation, precipitated the crisis and remains the main obstacle to economic recovery (see, for example, Mian and Sufi [2011], Mian and Sufi [2014], Mian et al. [2014]). This "household balance sheet view" of the crisis implies that debt relief for mortgage holders could have positive effects (Agarwal et al. [2013], Guiso et al. [2013]). Stimulus programs enacted through the credit market have been comparatively more frequent in developing economies, where debt relief and restructuring programs have often targeted the economically and politically influential rural sector. Recent examples include a US$ 2.9 billion bailout for farmers in Thailand and the restructuring of more than US$ 10 billion of household debt in Brazil.

Second, India's ADWDRS program was a one-off initiative that left the institutional environment unchanged, thus allowing us to isolate the effect of debt relief. Beneficiaries were identified, settlements were made and lenders were recapitalized in a way that modified existing loans but did not affect the rules and regulations governing new lending.

Third, unlike any previous debt relief initiative in India, eligibility for the program depended on the amount of land pledged at the time of loan origination, typically many years prior to the program. This rule, which applied retrospectively, provides a source of exogenous variation in program exposure as it implies that the share of credit that could qualify for the program is a function of the historically determined land distribution in a given district.

To the best of our knowledge, this paper provides the first empirical evidence on the effects of large-scale debt relief on credit market outcomes and the real economy and thus contributes to several strands of the finance literature. The "household balance sheet view" of the financial crisis suggests that the strengthening of households' balance sheet through debt relief should have an unambiguously positive effect on the real economy.[1] India's ADWDRS bailout followed this policy recommendation closely, by waiving household debts at a time when credit markets were not in distress and before bad debts in the country's important rural sector could impair financial intermediaries. Hence, India's ADWDRS offers an unusual opportunity to assess the importance of the household balance sheet channel.

Most closely related to our paper, Agarwal et al. [2013] study the Home Affordability Modification Program (HAMP), a subsidized mortgage restructuring program enacted in the United States in the wake of the financial crisis. Although the scale of the program remained relatively small, they find that the program led to a modest reduction in foreclosures but had no effect on durable and non-durable consumption. Alston [1983], Alston [1984] and Rucker and Alston [1987] study debt moratoria in the United States in the aftermath of the Great Depression. Their results suggest that the short-term benefits of debt relief may have come at the cost of moral hazard and credit rationing in the longer run.

Our results are also complementary to, but distinct from, a growing literature on the effects of economic stimulus programs more broadly. Mian and Sufi [2012] study the impact of the "Cash for Clunkers" stimulus program in the Unites States, which offered consumers direct subsidies for new car purchases. They find that the stimulus shifted the timing of new car purchases but did not affect employment, or default rates in cities with higher exposure to the stimulus. Chodorow-Reich et al. [2014] study the American Recovery and Reinvestment

[1] There are several reasons why this policy was not actively followed in the United States, including fears of moral hazard and a possible reduction of future bank lending. See, for example, "Lawrence Summers on House of Debt" *The Financial Times*, June 6, 2014 for a discussion.

Act (Arra) and find positive employment effects of the program. These studies relate to the literature on government spending and the Ricardian Equivalence (Barro [1989], Agarwal et al. [2007], Nakamura and Steinsson [2014]). While our analysis differs from this literature by focusing specifically on the impact of a credit market-led stimulus program, our results suggest a low spending multiplier from debt relief.

Because the bailout affected not only borrowers but was also tied to a recapitalization of banks by the Reserve Bank of India, our results are also related to the literature on bank recapitalizations and the transmission of financial shocks to the real economy (Peek and Rosengren [2000], Khwaja and Mian [2008], Paravisini [2008], Schnabl [2012], Lin and Paravisini [2013]), as well as more recent work on bank recapitalizations (Philippon and Schnabl [2013] and Gianetti and Simonov [2013]). In line with this literature, we find evidence of important changes in bank lending and credit allocation. Reflecting a long history of directed lending policies, Indian banks faced significant incentives to lend to sub-prime borrowers and engage in "evergreening" of de facto non-performing loans (see Peek and Rosengren [2005]).[2] The introduction of Adwdrs partly removed this incentive distortion, so that one would expect the bailout to change both the level as well as the geographical allocation of post-program lending. Consistent with this prediction, we find evidence of a shift in post-program lending away from districts with high exposure to the bailout. Importantly, this also suggests that the bailout did not encourage greater risk-taking by banks and thus enables us to distinguish the effect of Adwdrs on bank risk-taking from its impact on borrower behavior.

Finally, we contribute to the literature on the political economy of credit in emerging markets (Dinç [2005], Cole [2009b], Agarwal et al. [2012]). We find that loan performance responds to the electoral cycle, and that this effect is magnified in the period after the bailout is enacted. This finding underscores the concern that the anticipation of future credit market

[2]See Banerjee et al. [2009] and Banerjee and Duflo [2014] for a discussion of the history and economic impact of India's directed lending policies. See Burgess and Pande [2005] and Burgess et al. [2005] for an analysis of specific features of India's directed lending policies.

interventions generated by the bailout is an important channel through which moral hazard in loan repayment is intensified. It also suggests that the moral hazard costs of credit market stimulus programs are likely to be particularly severe in economies with weak institutions and a history of politically motivated credit market interventions.

The remainder of the paper proceeds as follows. In Section 2, we provide an overview of India's ADWDRS bailout program for rural households. Section 3 describes the data. Section 4 discusses the effect of the bailout on credit supply and loan performance, while Section 5 documents the real effects of the program. Section 6 presents additional robustness checks, and 7 concludes.

2 India's Bailout Program for Rural Households

India's ADWDRS bailout for highly indebted rural households was announced in March 2008, against the backdrop of the global financial crisis of 2007–2008.[3] The goal of ADWDRS was twofold. First, owing to a long history of directed lending to the rural sector, Indian banks had accumulated significant amounts of non-performing loans. The bailout was intended to strengthen Indian banks by eliminating substantial non-performing assets from their books and recapitalizing the banks in the process. Second, the significant reduction of household debt as a result of ADWDRS was intended to strengthen household balance sheets and act as a direct stimulus for investment and consumption in India's important rural sector.[4] Introduced a year ahead of national elections, the program also represented a significant transfer from urban to rural voters.

The rules for program eligibility were kept deliberately simple to expedite the processing of claims, and to minimize opportunities for leakage and corruption at local bank branches

[3]The Indian economy remained relatively unaffected by the global financial crisis, and credit spreads indicate that Indian credit markets were not in distress at the time of the program announcement.

[4]In 2008, agriculture accounted for approximately 15% of India's GDP and 55% of total employment.

tasked with identifying eligible borrowers. In contrast to prior initiatives, individual eligibility depended on the amount of land pledged as collateral at the time a loan was originated, typically several years before the program. Borrowers who had pledged less than two hectares of land were eligible for full debt relief, while those that had pledged more than two hectares of collateral qualified for 25% conditional debt relief if they repaid the remaining balance. Loans that (i) had been originated between December 31, 1997 and December 31, 2007, (ii) were 90+ days past due as of December 31, 2007 and (iii) remained in default until February 28, 2008, qualified for the program. Importantly, the eligibility rules, including the collateral cutoff, applied retrospectively, so that there was no scope for manipulation around program dates. In addition, ADWDRS was the first debt relief program in India's history to use landholdings as a basis for eligibility and thus the rules were unanticipated.

Implementation of the program began in June 2008. Every bank branch in the country was asked to identify all loans and borrowers on its books that met the bailout eligibility criteria. As a transparency measure, branches were required to publicly post these beneficiary lists, including the identity of the borrower, the details of the qualifying loan and collateral pledged at the time of loan origination. Borrowers who qualified for debt relief had their collateral cleared through a verifiable entry in their land documents, so that they were free to use their collateral documents to access new loans. Banks were, in principle, required to make ADWDRS beneficiaries eligible for new loans, although anecdotal evidence and our results below suggest that many banks did not follow this directive.

Borrower lists underwent independent audits at the branch and bank level, and a formal audit and redress mechanism was put in place by the regulator. Banks were recapitalized by the central government through the Reserve Bank of India for the full amount of credit written off under the program. Because eligibility rules were straightforward, reporting was standardized, and implementation as well as audits were overseen by the same regulator, enforcement of the program was remarkably uniform, both geographically and across banks.

Unconditional debt relief, which accounted for approximately 81% of claims, was processed immediately so that virtually all claims had been settled by the end of June 2008. The deadline for settling claims under the partial debt relief scheme for loans with collateral of more than two hectares of land was extended several times because of slow take-up –first to December 2009, and subsequently to December 2010. To ensure that we accurately capture the total amount of debt relief granted in a district, our analysis relies on data collected in December 2011, when the program was closed and all claims had been settled.

The ADWDRS program received significant media attention and was the center of an intense political debate. Proponents of the program heralded the bailout as a cure for endemic problems of debt overhang and poor investment incentives in the rural sector. Responses to the program from the financial sector were more wary and warned, in particular, about the potentially detrimental effects of the bailout for credit discipline and future access to credit among borrowers benefiting from the bailout. *The Economist*, for example, noted "Some fear that the government's largesse will do lasting damage to a culture of prudent borrowing, productive investment and prompt repayment."[5]

3 Data and Descriptive Statistics

We examine the impact of India's bailout for rural households using data at the district and bank-district level. Our main dataset is a panel covering 489 (of 593 total) districts of India from 2001 to 2012.[6] Our primary unit of analysis is an Indian census district, an administrative unit roughly comparable to a U.S. county. In the base year 2001, India had 593 districts with an average population of 1,731,897 inhabitants. In that year, the dis-

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

[6] Between 2001 and 2012, 47 new districts were created, typically by bifurcating existing ones. In our analysis, we aggregate all data to the level of India's 2001 census districts.

tricts in our data set account for approximately 95% of the Indian population and 89% of total bank credit.[7] The dataset contains first, a measure of the intensity of cross-sectional exposure to the ADWDRS bailout, measured as the share of agricultural credit eligible to be written off in each district as a result of the ADWDRS program; second, detailed data on credit market outcomes for each district, including the number of loans, lending volume and loan performance; and third, credit market data merged with district-level information on real economic outcomes, including agricultural productivity, rural wages and per capita consumption. In this section, we describe each set of variables in turn.

3.1 Measuring Program Exposure

To measure a district's exposure to the ADWDRS bailout, we collected data on the amount of debt relief granted under the program from each state's State Level Bankers' Committee (SLBC), the administrative body responsible for maintaining data on publicly supported credit market programs within each state. We were able to obtain this information for 23 of India's 28 states, including all major states of mainland India.[8] For each district in the data, we observe the amount of credit eligible for the bailout, consisting of overdue principal and accumulated interest, and the amount of debt relief actually disbursed.

Using this dataset, we construct our measure of program exposure. Because settlement was voluntary for borrowers above the two hectare collateral cutoff, our preferred measure of program exposure uses the share of credit *eligible* to be forgiven under the ADWDRS program. Letting $credit_i$ denote the total amount of outstanding agricultural credit in district i at the

[7]Bank lending in India is typically concentrated within a district, and very little lending takes place across district boundaries. This is the result of India's long-standing branch banking regulations, under which branch licenses and lending targets are typically assigned at the sub-district or "block" level. We exclude branches located in India's largest metropolitan areas, Ahmedabad, Bangalore, Chennai, Jaipur, Hyderabad, Kolkata, Mumbai, and New Delhi, where the geographical mapping between bank and borrower location is less likely to hold.

[8]Data were not available for the Northeastern states Arunachal Pradesh, Assam, Meghalaya, Nagaland and Mizoram. In the base year, these states account for less than 10% of total credit outstanding.

time of the program deadline (February 28, 2008), with superscript S denoting the share of debt owed by households below the two hectare eligibility cutoff and superscript L denoting debt owed by households above the cutoff, and letting s_i denote the share of credit that was current at the time of the program deadline (and hence unaffected by the bailout), the program exposure of district i can be written as

$$\text{Bailout\_share}_i = \frac{(1 - s_i)\left[credit_i^{\text{S}} + .25\bar{\kappa}_i \text{credit}_i^{\text{L}}\right]}{\text{credit}_i^{\text{S}} + \text{credit}_i^{\text{L}}} \tag{3.1}$$

where $\bar{\kappa}_i$ denotes the fraction of loans settled under the partial debt relief option for households above the two hectare cutoff. Because settlement was optional for households above the two hectare threshold, we assume $\bar{\kappa}_i = 1$ for all i (full compliance among households above the cutoff), which is equivalent to estimating the intent-to-treat effect for households with more than two hectares of land pledged as collateral.

Table I reports summary statistics for the intensity of program exposure and highlights significant variation in the share of credit forgiven as a result of the bailout. At the time the program came into effect, the median district in our sample had approximately US$ 47 million of agricultural credit outstanding and saw approximately one third of this amount written off as a result of the ADWDRS bailout (28.4%, s.d.=22.4).

Figure 1, Panel [b], plots the geographical distribution of program exposure and illustrates the significant cross-sectional variation in program exposure, which is key to our identification strategy. It is worth noting that bailout exposure does not appear to correlate significantly with state boundaries or the distribution of credit prior to the program (see Figure 1, Panels [a] and [b]). This suggests that program exposure is driven primarily by variation in economic shocks ahead of the bailout, rather than longer-term differences in state-level institutions or the development of local credit markets.

3.2 Lending and Loan Performance

Our credit market dataset combines district-level information on lending by all commercial banks in India, provided by the regulator, with proprietary data on lending and loan performance at the bank-district level, obtained from India's largest commercial banks.

(a) *District-level.* Our main source of credit market data is the Reserve Bank of India's Basic Statistical Returns of Commercial Banks in India (BSR) dataset, which is an annual panel of bank lending at the district level. The BSR data are based on annual "census" of credit and cover the lending activities of all commercial banks in India at more than 100,000 bank branches across the country. Unless otherwise indicated, we focus on direct agricultural credit, the type of credit that was most directly affected by the ADWDRS program, and use data on *Total credit* (*district*) and *Total loans* (*district*) for the years 2001 to 2012. Summary statistics for these variables, which are our primary measure of credit allocation, are reported in Table II. In the base year 2001, the median district had a total of 22,744 agricultural loans outstanding with a an average (median) loan size of US$ 515 (US$ 379).

(b) *Bank-district level.* We augment our credit market dataset with information on lending and loan performance at the bank-district level. Because data at this level of disaggregation is not made available by the regulator, we construct a new dataset based on proprietary data from India's four largest commercial banks. This data set contains information on lending and loan performance for 1,783 bank-district pairs observed annually between 2006 to 2012. In the base year 2001, the data cover 27,678 bank branches, accounting for approximately 40% of India's network of bank branches and 62% of total credit. We again focus on direct agricultural loans and construct the variables *Total credit* (*bank-district*) and *Total loans* (*bank-district*).

Because data on loan performance at the sub-national level is not disclosed by the regulator, the bank-district panel is also our main source of information on loan performance.

We use the data set to construct the outcome variables *Non-performing loans* (*number of accounts*) and *Non-performing loans* (*share of credit*). The data, summarized in Table II, reveal significant cross-sectional and time series variation in loan performance. The median bank branch in the sample records a non-performing loan share of 6% (s.d.20). In the pre-program period, the median non-performing loan share stands at 13.6% (s.d.=12.3). This figure gets reduced to 6.25% (s.d.=7.3) in the first year after the ADWDRS bailout.

3.3 Productivity, Wages, and Employment

Our primary source for data on agricultural productivity is the Indian Agricultural Statistics database,[9] which contains district-level information on crop yields and area planted for the 25 most common crops grown in India. We combine this data with commodity prices to calculate the variable *Productivity*, which measures the value of agricultural production per hectare in each district and year in the dataset. Because we wish to focus on the component of productivity that can be affected by households, we use constant commodity prices for the base year 2001, so that our measure of productivity may be interpreted as a measure of quantity TFP (TFPQ).

The Indian Agricultural Statistics database also provides district-wise wages for unskilled rural labor. Wages are reported seasonally, for every district in India and for a range of agricultural occupations. We focus on unskilled daily wages for field and non-field agricultural labor and calculate the variable *Real wage*, which measures a district's rural wage as the average of these two wage groups over all crop seasons for which data are available, to account for seasonal variation in labor demand and rural wages.

Data on household consumption are calculated from the Indian National Sample Survey

[9]The dataset is published by the Indian Ministry of Agriculture and consists of the database of Agricultural Wages in India (AWI), as well as the database on Agricultural Prices and Crop Yields (APY). Appendix E provides additional information on the construction of variables based on the dataset.

(NSS). The NSS is conducted annually as a repeated cross-sectional survey of approximately 75,000 rural and 45,000 urban households. We use data from four NSS survey rounds, two prior to the bailout and two after the bailout,[10] and calculate the variable *Consumption*, which measures monthly per capita consumption expenditure (MPCE). This measure of household consumption is collected consistently across survey rounds and widely used in practice, for example in the calculation of poverty lines.[11]

3.4 Additional Variables

Our dataset contains a number of additional variables, which we use as controls and to construct the instrumental variable described below. Specifically, we measure weather shocks using local variation in monsoon rainfall. Rainfall data are taken from the Indian Meteorological Department and measure total monthly precipitation at the district level. Based on these data, we construct the variable *Rain monsoon*, which measures total monsoon rainfall between July and September for each year as a fraction of the district's long-run rainfall average over the same period.[12] Based on this dataset, we also construct a dummy variable indicating years in which a district was exposed to a drought shock. This variable takes on a value of one for any year in which a district's total monsoon rainfall between the months of July and September was below 75% of the district's long run precipitation average, measured over the same time period. This definition matches the threshold used by Indian state and local authorities to declare a district as "drought affected".[13]

[10] We use data from NSS rounds 63 and 64 for the period prior to the bailout and NSS rounds 66 and 67 for the period after the bailout.

[11] One possible limitation of using NSS data to estimate household consumption is that the NSS survey is representative at the "survey unit," rather than the district level (a survey unit typically consists of several census districts). We therefore restrict the sample to districts with at least 50 NSS households.

[12] We use 50-year averages for June-September, taken from the *Indian Meteorological Department*'s dataset "Long Run Averages of Climatological Normals."

[13] Data and definitions are available from the *India Meteorological Department* at http://www.imd.gov.in

Previous work has shown that lending of state banks in India tracks the electoral cycle.[14] Hence, we control for a district's temporal distance to the next scheduled state election to account for the resulting fluctuations in credit supply. Electoral data come from the Election Commission of India's publicly available election statistics database. Finally, we use data on district characteristics from the Census of India and the Indian Agriculture Census. These data include the population and land distribution of each district.

4 The Credit Market Effects of the Bailout

Estimating the credit market impact of the bailout poses two main identification challenges. First, program exposure is a function of loan defaults ahead of the program, and therefore potentially endogenous to pre-existing economic trends at the district level. Second, estimating the causal effect of the program requires us to distinguish changes in credit supply from contemporaneous shocks to credit demand.

We address this identification problem in two steps. First, we estimate the credit market impact of the bailout at the district level, using a difference-in-differences specification, in which we instrument for program exposure. Since we observe both program exposure and real outcomes at the district level, we use this approach as our benchmark identification strategy. Second, we verify the robustness of our credit market estimates by replicating the analysis with data at the bank-district level. This allows us to isolate the impact of the bailout on credit supply from any contemporaneous shocks to credit demand using a fixed effects strategy similar to Khwaja and Mian [2008].[15] As we shall see, both identification strategies yield similar point estimates.

[14] Cole [2009b] shows that agricultural lending by state-owned banks in India increases by 5-10 percentage points in election years.

[15] This approach been used in a number of studies that identify the transmission of credit shocks through the bank lending channel. See, for example, Schnabl [2012], Paravisini et al. [2014] and others.

We begin by estimating the impact of the bailout on credit market outcomes, using difference-in-differences specifications of the form

$$\ln(C_{it}) = \alpha_i + \beta_t \cdot d_r + \gamma E_{it} + X_{it}'\zeta + \varepsilon_{it} \tag{4.1}$$

where C_{it} is a credit market outcome for district i (in region r) and year t, the variable E_{it} ($Bailout\_share_i \times Post_t$) measures a district's exposure to the bailout program, α_i are district fixed effects and β_t are time fixed effects, which we allow to vary across the four administrative regions d_r, $i \in r$ of the Indian central bank to account for heterogeneity in local business cycles. X_{it} is a vector of observable determinants of credit market conditions, which always includes lagged monsoon rainfall and a full set of electoral cycle dummies.[16] The coefficient of interest γ measures the effect of program exposure on credit market outcomes, and the error term ε_{it} captures all omitted factors, including any deviations from linearity. We drop all observations for 2008, the year in which the program took effect, to rule out any mechanical correlation between treatment intensity and observed credit market outcomes, and estimate equation 4.1 using data for 489 districts observed between 2001 and 2012, for a total of 5,511 observations.

Equation 4.1 will consistently estimate γ if $Cov(E_{it}, \varepsilon_{it})$=0. This covariance restriction may, however, not hold if exposure to the program is correlated with unobserved trends in credit market outcomes. To address this concern, we rely on an instrumental variables strategy based on the rule that a loan had to be both in default *and* backed by land collateral of less than two hectares to qualify for unconditional debt relief. This eligibility rule allows us to use two sources of exogenous variation to instrument for program exposure. The first source of variation is the time series of weather shocks[17] experienced by a district prior to

[16] See Cole [2009b] for evidence that lending by Indian state banks co-moves with the electoral cycle for local (state assembly) elections.

[17] We use years in which total monsoon rain between June and September was below 75% of the district's long-run average rainfall as our definition of weather shocks. This definition is consistent with the threshold

the program, which is a strong predictor of loan defaults. The second source of variation is a district's land distribution, which determines the fraction of households that fall below the two hectare cutoff and could have been eligible for unconditional debt relief.

We thus use the number of drought years, w_{it}, experienced by a district in the period prior to the bailout, interacted with the share of households below the eligibility cutoff, ℓ_i, to obtain a cross-sectional predictor of the share of credit bailed out in each district:[18]

$$Z_i = \sum_{t=1}^{\bar{t}} w_{it} \cdot \ell_i \tag{4.2}$$

We then interact this variable with an indicator that marks the beginning of the program, to construct a time-varying instrument for program exposure:

$$Z_{it} = \left(\sum_{t=1}^{\bar{t}} w_{it} \cdot \ell_i \right) \times Post_t \tag{4.3}$$

It is important to stress that even if one of these two plausibly exogenous sources of variation were to affect outcomes through a channel other than program exposure, the *interaction* between weather shocks and the collateral cutoff should become relevant only as a result of the program. We verify this empirically and provide additional identification tests in Appendix B. In particular, we first check that there is no correlation between the instrument and any of our credit market outcomes prior to the program. Second, we show that the instrument is also uncorrelated with observable district characteristics in the year the program came into effect. Our instrumental variables strategy yields the first-stage specification

$$E_{it} = \delta_i + \theta_t \cdot d_r + \gamma^{fs} Z_{it} + X_{it}' \zeta^{fs} + \epsilon_{it} \tag{4.4}$$

used by Indian state and local authorities to declare a district as "drought affected."

[18] The present day land distribution of India's districts is largely the result of land reforms enacted under British colonial rule (see Banerjee and Iyer [2005] for a detailed discussion) and therefore plausibly exogenous to economic trends over the comparatively short time period we study.

where, as in equation 4.1 above E_{it} is the program exposure of district i at time t in region r, X_{it} is a matrix of observable determinants of credit market conditions and we control for year and district fixed effects. Table III presents estimates of the first stage and demonstrates that the instrument is a relevant predictor of program exposure. The first stage coefficient γ^{fs} is positive and statistically significant at the 1% level ($\hat{\gamma}^{fs}$=0.257, s.e.=0.049), indicating that districts with greater exposure to pre-program drought shocks and a greater share of households below the eligibility cutoff saw a significantly higher share of credit waived as a result of the program. An F-Statistic of 23.16 for the regression corresponding to our preferred specification (Table III, column [4]) implies a strong first stage.

One potential concern with our identification strategy is that that the treatment effect estimated in equation 4.1 confounds the impact of the bailout on credit supply with possible contemporaneous shocks to credit demand. To rule out this possibility, we use an alternative identification strategy based on Khwaja and Mian [2008]. In particular, we derive an estimating equation at the *bank-district* level from a simple model of credit supply and demand. This approach has several advantages. First, taking the identification from the district to the bank-district level allows us to isolate the effect of the bailout from shocks to credit demand by controlling for district-time fixed effects. Second, we can identify the aggregate bank lending channel separately from the effect that the bailout has on the within bank reallocation of credit across districts with differential program exposure.[19] The derivation, described in Appendix C, yields the following estimating equation

$$\ln(C_{ijt}) = \xi_{it} + \gamma_E E_{jt} + \gamma_B B_{ijt} + \chi_{ijt} \tag{4.5}$$

[19]The model underlying our estimating equation makes several testable predictions. First, banks experiencing a larger equity shock as a result of greater overall exposure to the bailout should increase lending (γ_E>0). At the same time, banks should redistribute lending away from districts with greater program exposure, as the program allows them to clean troubled assets from their books and thus reduces incentives that may have existed to "evergreen" loans close to default prior to the program (γ_B<0). This can be tested against the alternative hypothesis that the program encouraged banks to engage in riskier lending. If this were the case, then one would expect γ_E>0 and γ_B>0.

where ξ_{it} are district-time fixed effects, E_{jt} denotes the equity shock received by bank j as a result of the bailout and B_{ijt} denotes program exposure (the share of total credit written off under the program) at all branches of bank j in district i.

As we show in more detail below, the estimates from the two alternative identification strategies are quantitatively similar, although standard errors are expectedly narrower when we use bank-district level data.

4.1 Impact on Credit Allocation

In this subsection, we use the methodology described above to estimate the effect of the bailout on credit market outcomes. We begin by estimating the impact of the bailout on credit allocation and present evidence to corroborate the hypothesis that the patterns we observe are driven by changes in the supply of credit that occur in response to the bailout.

Table IV reports the results, using the specification in equation 4.1. The dependent variable in columns [1] to [4] is the log number of loans, while in columns [5] to [8] the dependent variable is the log amount of credit. For each dependent variable, the first two columns report OLS estimates while the second set of columns present two-stage least squares (2SLS) results using the instrumental variable strategy described in the previous section. Within each set of estimates, the first column controls for district fixed effects as well as time varying controls. The second column presents results from a more flexible specification that allows each district to follow its own linear time trend in the pre-program period.

We find that that bank lending becomes more conservative as a result of the bailout. While overall lending increases after the program, the results indicate that banks reallocate credit away from districts with greater program exposure. The magnitude of credit reallocation in response to the bailout is economically large. Estimates from our preferred specification in Table IV, columns [4] and [8] indicate that a one standard deviation increase in the share of credit covered by the program leads to a 3.6% decrease in new loans made after the bailout ($\hat{\gamma}$=-0.036, s.e.=0.068). The shift of bank lending away from high bailout districts is both larger and more precisely estimated when the amount of credit is used as the dependent variable. Our results indicate that a one standard deviation increase in program exposure leads to a 15% decrease in new lending after the program ($\hat{\gamma}$=-0.150, s.e.=0.068).

We also estimate a modified version of our baseline specification that uses the total amount of credit as the outcome of interest and replaces our measure of program exposure by the total amount of credit eligible for the program in a given district. In this specification, the difference-in-differences estimate $\hat{\gamma}$ can be interpreted as the amount of new lending *per dollar of bailout exposure*. The results, reported in Appendix A, suggest an asymmetric reallocation of credit away from high bailout districts as a result of ADWDRS. While districts with high (above median) program exposure receive only 36 cents of new lending for every 1 dollar of credit written off under the program, districts with low (below median) bailout exposure receive an average of 4 dollars of new lending for every 1 dollar of debt relief.

The finding that banks channel new lending to observably less risky districts after the bailout may seem counterintuitive, given that the empirical literature on bank recapitalizations has generally found a positive correlation between bailouts and bank risk-taking. The pattern of reallocation we find is, however, consistent with the hypothesis that the bailout affected incentives for "evergreening" (Peek and Rosengren [2005]), that is, to keep lending to borrowers close to default in an attempt to avoid marking these loans as non-performing. Reflecting a long history of directed lending (see Burgess and Pande [2005], and Banerjee and Duflo [2014]), all banks in India are required to allocate 40% of their net credit to "priority sectors", which include agriculture and small scale industry. While this mandate forces the allocation of a significant share of credit to sub-prime borrowers, local branch managers also face strong penalties for realizing credit losses. This creates a significant incentive to keep lending to borrowers close to default. The introduction of ADWDRS removed this incentive distortion. Consistent with the "evergreening" hypothesis, we find evidence of a shift in post-program bank lending away from districts with greater bailout exposure.

Table VI implements the specification in equation 4.5, which uses bank-district level data and controls for district-time fixed effects to verify that the results from our preferred specification are not driven by changes in credit demand. As in Table IV, column [1] uses the log number of accounts as the dependent variable, and column [2] uses log credit as the outcome of interest. To allow for a meaningful comparison, we restrict the sample in Table VI to the districts included in our estimation of equation 4.1. We find that banks experiencing a larger equity shock as a result of their bailout increase their overall lending, but reallocate credit away from districts with greater exposure to the bailout. The point estimates are negative and significant for both the number of loans ($\hat{\gamma}_B$=-0.210, s.e.=0.033) and the amount of credit ($\hat{\gamma}_B$=-0.142, s.e.=0.045), and the size of these effect is even larger than in the district-level estimates reported in Table IV. Taken together, these results indicate a strong supply-side response that is not confounded by changes in credit demand.

4.2 Impact on Loan Performance and Moral Hazard

We next examine the effect of the bailout on ex-post loan performance. Table V presents the results, using the basic specification described in equation 4.1. The dependent variable columns [1] to [4] is a dummy equal to one if the branches in a given district experienced an increase in the share of non-performing loans between year $t-1$ and t, while the dependent variable in columns [5] to [8] is a dummy equal to one if bank branches located in a given district experienced an increase in the share of non-performing credit over the same time period. As before, our preferred specification includes a full set of fixed effects, time-varying controls and linear pre-trends for each district (columns [4] and [8]).

We find evidence of a strong negative effect of the bailout on loan performance. The coefficient estimate from our preferred specification indicates that a one standard deviation increase in bailout exposure leads to a 69% increase in the probability that a district experiences the share of non-performing loans ($\hat{\gamma}$=0.691, s.e.=0.385), and a 64% increase in the probability that a district experiences an increase in the share of non-performing credit ($\hat{\gamma}$=0.642, s.e.=0.355) in the post-program period.

Table VI, columns [3] and [4] verifies that the results on loan performance also hold at the bank-district level using the fixed effects specification described in equation 4.5. The results are again consistent with those found in our preferred specification. Banks that received a greater share of bailout funds are significantly more likely to experience an increase in defaults after the program, and districts in which bank branches were more exposed to the bailout experience a decline in loan performance after the program. It is important to note that this decline in loan performance happens after loans that were in default have been written off and cleared from banks' books. Given that Table IV columns [1] to [4] show no evidence of new lending, we conclude that the negative effect of the bailout on loan performance is driven by defaults among borrowers that were previously in good standing. Table VI, columns [5] and [6] estimate the size of this effect using bank-district level data and suggest that it is substantial. The point estimates indicate a one standard deviation increase in bailout exposure is associated with a 1.6% increase in the share of non-performing loans ($\hat{\gamma}_B$=0.016, s.e.=0.003) and a 2.4% increase in the share of non-performing credit ($\hat{\gamma}_B$=0.024, s.e.=0.003) over the sample mean.

Taken together, these results point to substantial borrower moral hazard. First, loan performance after the program deteriorates in districts with greater bailout exposure even though banks reallocate credit towards observably less risky districts. Second, because no new lending took place and non-performing loans covered by the program were removed from banks' books, post-program defaults must be concentrated among borrowers that were previously in good standing (and therefore not covered by the bailout). The program thus created perverse incentives among non ADWDRS beneficiaries to default strategically.

One possible concern with this interpretation is that higher default rates in high-bailout districts might be explained by other changes in credit conditions, such as a change in average loan sizes that would have impacted the repayment capacity. In unreported results, we find no evidence of a systematic change in average loan sizes as a result of the bailout. Furthermore, Appendix B presents an additional that exploits the exogenous timing of Indian state elections. We show that electoral cycles in loan defaults are magnified in the post-program period. On average, defaults in pre-election years increase by approximately 5 percentage points over the post-program sample mean of 7.25% ($\hat{\gamma}$=0.131, s.e.=0.076). In addition, average loan sizes do not vary around elections prior to the bailout or after the bailout. This indicates that the anticipation of future credit market interventions generated by the bailout is a key channel through which moral hazard in loan repayment is intensified.

Another interpretation, perhaps less plausible, is that the tightening of credit in high-bailout districts led to a slowdown of economic activity thus causing the increase in defaults among non ADWDRS beneficiaries. The next section studies the real effects of the bailout and finds little support for this interpretation.

5 Real Effects of the Bailout

The principal aim of economic stimulus programs is to stabilize output, and to prevent distortions in investment and consumption during economic downturns. In the case of India's ADWDRS bailout, proponents of the program argued that debt relief could stimulate productive investment by alleviating problems of endemic debt overhang in India's important agricultural sector. Given the scale of the bailout, it was thought that the program might not only restore access to institutional credit for rural households, but also provide a direct stimulus to household consumption and the rural labor market.

In this section, we evaluate this hypothesis by exploring the impact of the ADWDRS program on real outcomes. We are interested in estimating the elasticity of non-financial outcomes with respect to program exposure (the share of credit forgiven as a result of the program). To do so, we use district-level data on agricultural productivity, real wages and household consumption and estimate equations of the form

$$\ln(R_{it}) = \alpha_i + \beta_t \cdot d_r + \omega Z_{it} + X_{it}'\zeta + \nu_{it} \tag{5.1}$$

where R_{it} is a real outcome of interest for district i observed at time t, α_i are district fixed effects, and β_t are time fixed effects, which we again allow to vary across India's four central bank regions d_r, $i \in r$ to account for local variation in business cycles. The variable Z_{it} measures the instrumented district's exposure to the bailout, and X_{it} is a vector of time varying controls. In this equation, the coefficient of interest ω measures the elasticity of real outcomes with respect to program exposure. We estimate this equation using data at the district level, the most granular level at which information on real outcomes is available. Hence, we cannot use district-year fixed effects to separately identify exposure to the bailout from time-varying demand shocks, as in equation 4.5 above. As in equation 4.4 before, bailout exposure is instrumented using the interaction between the number of drought years prior to the bailout and the share of households below the ADWDRS collateral threshold.

We first explore the effect of debt relief on agricultural productivity. We focus on agricultural productivity as the outcome that we would expect to be most directly affected if the bailout alleviated problems of debt overhang, as claimed by its proponents. Indeed, household indebtedness is high in India's large agricultural sector, and producers rely heavily on external financing, usually provided by commercial banks, to undertake productive investments. Proponents of the ADWDRS bailout generally argued that the debt relief initiative would create incentives for productive investment and free collateral so that rural households could access new credit. If the program had this effect, we would expect this to be reflected in greater investment and increased agricultural productivity. To test whether this is the case, we take advantage of a district level panel on crop yields and commodity prices from the Indian Department of Agriculture. The dataset, which we describe in Section 3 and Appendix E, contains detailed information on agricultural revenue and area cultivated, which allows us to construct time series of agricultural productivity over the time period 2001-2011 for 387 districts in our sample. Table VII, columns [1] to [4], estimate the impact of the stimulus program on agricultural productivity. The results show that the program had no discernible effect on agricultural productivity. Using our preferred specification, the estimated effect is a precise zero ($\hat{\gamma}$=-0.006, s.e.=0.010), suggesting that the stimulus did not create investment incentives of a magnitude sufficient to affect agricultural productivity.

Second, we investigate the effect of debt relief on real wages, to test for a possible labor market impact of the bailout. Unskilled labor is an important input to agricultural production. Hence, if the bailout had a positive impact on households' ability to invest in productive inputs, one might expect to find this effect reflected in rural employment. To test for this channel, we construct a panel of real wages for the period 2006-2011. The data capture monthly wages for unskilled agricultural labor in a sample of 327 districts, which we average for all agricultural occupations and over all reported crop seasons to account for seasonal fluctuations in labor demand. In columns [5] to [8], we test for an impact of the bailout on rural wages using the same regression specifications as in columns [1] to [4], and do not find a discernible effect of the bailout using. The coefficient estimate from our preferred specification is negative, close to zero ($\hat{\gamma}$=-0.07, s.e.=0.099) across all specifications. The result also remains unchanged when we restrict the sample to non-urban districts with a rural population share of 75 percent and above.

Finally, we turn to the effect of the bailout on household consumption. If households perceived the bailout as a permanent change in their access to credit and more productive inputs, they may have increased consumption. To test for an effect on household consumption, we construct a panel of mean per capita expenditure (Mpce), the standard measure of household consumption, using NSS micro-data as described in Section A.I and Appendix E. We estimate the impact of debt relief on consumption in Table VII, columns [9] to [12], and again find that the bailout had no measurable positive effect on household consumption.

The government believed that a recapitalization of banks would encourage new lending and that this, in turn, would stimulate new investment through the removal of disincentives for investment and the strengthening of households balance sheets. These positive effects, however, were not realized as lenders reallocated credit away from bailout districts driven, in part by the anticipation of borrower moral hazard as a result of the program.

6 Robustness Tests

In this section, we present additional identification and robustness tests in two steps: we first report several tests of the difference-in-differences identification assumption, requiring that outcomes follow parallel trends before the program. Second, we check that our results are not confounded by a differential response to weather shocks in districts with different land distributions (e.g. a greater share of large landholdings).

The difference-in-differences strategy underlying our empirical approach relies on the assumption that outcomes for the treatment and control groups –in our case districts with different levels of bailout exposure– do not follow differential trends in the pre-program period and would have followed identical trends in the absence of the intervention. We now present three tests of this assumption.

We begin by presenting graphical tests of the parallel trends assumption for our main outcomes of interest. Figure 2 and Figure 3, plot lending and loan performance for high-bailout (above median) and low-bailout (below median) districts. The graphs show no indication that the parallel trends assumption is violated for our main credit market outcomes. Figure 4 shows parallel trends plots for real outcomes, again differentiating between districts above and below the median program exposure. There is again no indication that the parallel trends assumption is violated for these outcomes.

However, given that our treatment variable (the share of credit forgiven in each district) is continuous, one may be concerned that these simple graphical tests may mask deviations from the parallel trends assumption among a subset of districts. We address this possibility by presenting additional parametric tests of the parallel trends assumption.

In the first set of parametric tests of the parallel trends assumption, we estimate regressions in which we allow each district to follow its own linear time trend in the pre-program period. These estimates are reported as the second specification for each group of regressions in Tables IV to VII. The results indicate that the inclusion of linear time trends improves the precision of the point estimates, but does not qualitatively affect our main findings.

In the second set of parametric tests, we explore the possibility that our estimates are affected by the presence of non-linear trends. To do so, we perform two placebo experiments. In the first placebo experiment, we restrict our sample to the pre-bailout period and move the timing of the bailout from 2008 to a hypothetical program date in 2005. The results, reported in Table VIII, columns [1] to [5], show that the treatment coefficients for the hypothetical program date are close to zero and, in several cases have the opposite sign, indicating that the treatment effect at the actual program date is not driven by unobserved time trends.

The second placebo experiment tests for the presence of non-linear time trends by randomly reassigning treatment levels among the cross-section of districts in our sample. We do this using a bootstrap procedure, in which we draw N=1,000 random assignments of our treatment vector, estimate treatment coefficients using our preferred specification, and report bootstrap coefficients and standard errors obtained from this exercise. The estimated treatment effects, reported in Table VIII, columns [6] to [10], are again close to zero and statistically insignificant. In sum, these results make it unlikely that our results are biased as a result of deviations from the parallel trends assumption.

Finally, we explore the possibility that our results are driven by differential responses to weather shocks at the district level. In Section 4 and Appendix B, we test the plausibility of the exclusion restriction for our instrumental variables estimates. In particular, we show that there is no correlation between the interaction of drought shocks and a district's land distribution before the bailout. In this section, we test for the possibility that the bailout shock had a differential *post-program* impact in districts with a different land distribution. If this were the case, our estimates of the post-program path of credit market variables and real effects would be biased by underlying differences in district characteristics, and provide an inaccurate estimate of the credit shock. We test this possibility by estimating a version of equation 4.1 in which we control for the share of landholdings in district i that are larger than four hectares, interacted with the variable *post*, which takes on a value of one for all years after 2008. This effectively restricts identification of the program impact to a band of $\{Cutoff-2ha,\ Cutoff+2ha\}$, thus controlling for the possibility that shock responses differ in "landlord districts" with a significant share of large landholdings. Table IX presents the results. With the exception of the extensive margin of credit, for which the effect is weaker and less precisely estimated, we do not find any significant changes relative to our baseline estimates. This makes it unlikely that our results are driven by differential shock responses arising from underlying differences in a district's land distribution.

7 Conclusion

The world over, governments have routinely intervened in credit markets in an effort to stimulate economic activity. While credit market led stimulus programs are extremely common, they are often thought to have negative implications for credit allocation and borrower discipline, without generating sufficiently large offsetting effects on real economic activity. There exists however surprisingly little evidence to evaluate these claims. This paper uses a natural experiment arising from one of the largest borrower bailouts in history –India's ADWDRS debt relief program for highly indebted rural households– to estimate the causal effect of a large credit market stimulus and makes two main contributions.

First, we show that the bailout led to a significant reallocation of bank lending away from districts with greater exposure to the bailout. A one standard deviation increase in the share of credit waived under the program leads to a 13-16% decline in new bank lending in the district after the program. This reallocation of new credit towards observably better performing districts is prima facie evidence that the bailout removed incentives to "evergreen" (Peek and Rosengren [2005]), thus allowing for a more efficient allocation of credit.

Second, we find that the program had no positive impact on productivity, consumption or labor market outcomes, but led to significant moral hazard in loan repayment. These results indicate that the program had a significant moral hazard cost that is not offset by a positive impact on productivity, consumption or the rural labor market. Importantly, we show that the increase in defaults is concentrated among borrowers that were previously in good standing, and is not driven by greater bank risk-taking or a change in the debt levels of existing borrowers. Moreover, the relationship between defaults and the electoral cycle –which exists in normal times and has been documented in earlier studies– is magnified by the bailout, suggesting that the anticipation of future credit market interventions is an important channel through which moral hazard in loan repayment is intensified.

The results also shed light on the importance of the household balance sheet channel in crisis resolution. In the case of the United States it has been argued that high levels of accumulated household debt –rather than the breakdown of financial intermediation– were an important factor precipitating the financial crisis of 2008 (see Mian and Sufi [2011], Mian and Sufi [2014], and Mian et al. [2014]). This view would suggest that a program similar to that implemented in India under the ADWDRS bailout should have an unambiguously positive effect on the real economy. In contrast, we find no evidence of greater investment, consumption or positive labor market outcomes in areas where debt relief led to a significant reduction of household debt. It is not surprising that, in the case of India, government efforts to stimulate the real economy through debt relief were largely in vain given that the bailout also led lenders to reallocate credit away from districts with high program exposure.

While our results do not dispute the potentially important role of the household balance sheet channel, they highlight the difficulty of designing debt relief programs in a way that ensures the transmission of the credit market stimulus to the real economy. In particular, our findings underscore the importance of taking into account the impact of debt relief on post-program credit supply. The reallocation effect we find is likely exacerbated by two features of the program. First, ADWDRS covered primarily term loans with short maturity, which were fully written off and eliminated from banks' balance sheets as soon as the program came into effect. Hence, Indian banks were free to immediately reallocate credit away from regions with high bailout exposure. This contrasts with the partial write down of longer-term mortgage debt proposed in the United States, which would have presumably not terminated lending relationships entirely. Second, the ADWDRS bailout made debt relief mandatory and treated willful defaulters and genuinely distressed borrowers alike. This is likely to give rise to significant ex-post moral hazard among borrowers who could have repaid but were bailed out and borrowers who did not qualify for debt relief because they had remained current on their loan payments throughout. The results suggest that this moral hazard cost of debt relief is fueled by the expectation of future government interference in the terms of existing credit contracts, and is thus likely to be especially severe in weak institutional environments with a history of politically motivated credit market interventions.

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Data Sources

Table A.I Description of Variables

Variable	*Description*	*Sources*
Drought years	The number of "drought years" experienced by a district prior to the program. A drought year is defined as a year in which total monsoon rainfall between June and September was below 75 percent of a district's 50-year average. The variable counts the number of drought years in the pre-program period between the years 2001 and 2007. Rainfall data comes from the *Indian Meteorological Department* (IMD), Long-run normals are taken from *India Meteorological Department Long Run Averages of Climatological Normals*, CD-ROM.	India Meteorological Department (IMD)
Electoral cycle	Five dummy variables indicating the temporal distance to the next scheduled state assembly election. State assembly elections are scheduled every five years and are staggered over time.	Election Commission of India, available at http://eci.nic.in.
Household consumption	Mean per capita household expenditure (MPCE), calculated from household-level data of the Indian *National Sample Survey*.	India National Sample Survey (various years)
Land distribution	Share of landholdings in the district that are smaller than two hectares (approximately 5 acres) in size.	India Agriculture Census, *District Tables* (2001)
Non-performing loans, amount	This information is not publicly available. We have calculated it from proprietary data obtained from India's four largest commercial banks. (a) At the bank-district level, the data consist of annual information on the amount of outstanding rural credit and the amount of rural NPAs (both denominated in units of Rs 100,000) and cover the years 2006-2012. (b) At the district level, our measure of loan performance is the arithmetic mean of loan performance for all banks with branches in the district. To ensure consistency with the credit data, we exclude districts for which we have no information on program exposure.	Proprietary bank data.
Non-performing loans, number of loans	This information is not publicly available. We have calculated it from proprietary data obtained from India's four largest commercial banks. (a) At the bank-district level, the data cover the years 2006-2012 and consist of annual information on the number of outstanding agricultural loans and the number of loans in default, defined as 90+ days past due (b) At the district level, our measure of loan performance is the arithmetic mean of loan performance for all banks with branches in the district. To ensure consistency with the credit data, we exclude districts for which we have no information on program exposure.	Proprietary bank data.
Rainfall	Total monsoon rainfall as a share of the 50 year district-level average. Monsoon rainfall is defined as total rainfall between June and September. Long-run normals are taken from *India Meteorological Department Long Run Averages of Climatological Normals*, CD-ROM.	India Meteorological Department (IMD)
Rural wage	Real unskilled wage for all agricultural occupations, measured at the end of the main crop season each year. Data are available for the years 2001-2011.	Indian Ministry of Agriculture
Total agricultural productivity	Revenue per hectare derived from the sale of 32 main crops. Data on agricultural yields is available from the Indian Ministry of Agriculture, revenues are calculated using constant 2001 commodity prices. Data are available for the years 2001-2011.	Indian Ministry of Agriculture, Indiastat

Table A.I: Description of Variables (cont'd)

Variable	*Description*	*Sources*
Total credit, amount	The natural logarithm of outstanding agricultural credit in units of Rs 100,000 for each district of India. (a) The district-level data are constructed from the Reserve Bank of India's BSR dataset, and cover all agricultural loans made by private, public, cooperative and regional rural banks for the years 2001-2012. (b) The bank-district level data are taken from proprietary data on agricultural lending at all branches of India's four largest commercial banks and covers the years 2006-2012.	Reserve Bank of India, *Basic Statistical Returns of Scheduled Commercial Banks in India*, (2001 -2012); proprietary bank data.
Total credit, number of loans	Total number of agricultural loans outstanding. (a) District-level data are constructed from the Reserve Bank of India's Basic Statistical Returns of commercial Banks in India (BSR) dataset, and include all agricultural loans made by private, public, cooperative and regional rural banks. We use data for the years 2001-2012 (b) At the bank-district level, this variable is constructed from proprietary data on agricultural lending at all branches of India's four largest commercial banks. The dataset covers the period 2006-2012 and contains district level aggregates of approximately 27,678 branches in all states and Union Territories of India (Reserve Bank of India, A Profile of Banks, 2012).	Reserve Bank of India, *Basic Statistical Returns of Scheduled Commercial Banks in India*, (2001 -2012); proprietary bank data.

Tables and Figures

Table I
Program Exposure

This table reports summary statistics for the variable *Bailout_share*, our main measure of program exposure. The variable measures the total amount of credit eligible for the bailout as a share of total outstanding agricultural credit on March 31, 2008, the date when the bailout program was enacted. To facilitate the interpretation of our estimates, the variable is normalized to have mean zero and standard deviation one in all subsequent tables.

	Bailout share (N=489)
Mean	.326
Median	.284
StDev	.224
Min	.002
Max	.991

Table II
Summary Statistics

This table presents summary statistics for the datasets used in the analysis. Panel A summarizes the district-level dataset, containing information for 489 districts and the years 2001 to 2012. *Total credit* is the log amount of total agricultural lending (in Rs million). *Total loans* is the log number of total agricultural loans outstanding. Loan performance at the district level is aggregated from the bank-district dataset described in Panel B. *Population* is the log of a district's total population. Rural share is the share of a districts population living in rural areas. *Productivity per hectare* is the total revenue from all crops produced in a district at 2001 prices, divided by the total area planted. *Rural wage* is the log of the average daily wage for agricultural labor. *Per capita consumption* is the log of monthly per capita household consumption. *Land holdings below* 2 *ha* measures the share of total recorded landholdings in a district that are smaller than two hectares. *Monsoon rain* is the average precipitation recorded in a district between June and September as a share of the district's 50-year rain average over the same period. *Drought years* is the number of years between 2001 and the 2007, in which a district experienced a drought, defined as a year in which monsoon rainfall was less than 75 percent its long-run average. *Time to election* measures the number of years remaining until the next scheduled state election. Panel B summarizes the bank-district level dataset. All variables in this dataset are based on proprietary data from India's four largest banks, available for a panel of 569 districts for the years 2005 to 2012. *Total credit* is the log of total agricultural lending at these banks, and *Total loans* is the log number of agricultural loans. All monetary values are in nominal Indian Rupees. Table A.I and Appendix E provide additional details on data sources and the definition of variables.

	(1)	(2)	(3)	(4)	(5)	(6)
	Obs	Mean	Median	StdDev	Min	Max
Panel A: District-level data						
I. Lending and loan performance						
Total credit	6,952	9.20	9.46	1.80	1.75	14.52
Total loans	6,952	10.10	10.36	1.52	2.08	14.71
Non-performing loans, share of ag credit	6,574	0.14	0.06	0.20	0.00	1.00
Non-performing loans, share of accounts	6,437	0.18	0.10	0.22	0.00	1.00
II. District characteristics						
Total population	7,553	14.00	14.21	1.02	10.35	16.08
Rural share	7,553	0.78	0.82	0.17	0.00	1.00
III. Real outcomes						
Productivity per hectare	5,840	0.17	0.14	0.10	0.00	0.74
Rural wage	1,031	4.36	4.32	0.43	3.29	5.75
Per capita consumption	2,478	11.96	11.85	0.53	10.59	13.43
IV. Additional controls						
Land holdings below 2ha	6,669	0.53	0.56	0.24	0.00	.99
Monsoon rain, % of 50-year mean	6,094	97.05	88.46	50.71	8.36	1062
Drought years	7,008	0.87	0.00	1.48	0	7
Time to election	7,540	2.01	2.00	1.41	0	4
Panel B: Bank-district level data						
I. Lending and loan performance						
Total credit	15,003	6.44	6.88	2.80	-4.61	12.75
Total loans	10,170	6.13	6.48	2.74	0.00	12.56
Non-performing loans, share of ag credit	14,987	0.11	0.04	0.17	0.00	1.00
Non-performing loans, share of ag accts	10,763	0.15	0.08	0.20	0.00	1.00

Table III
First Stage – Weather Shocks and Program Exposure

The regressions in this table estimate the first stage relationship between weather shocks and program exposure. The coefficient of interest measures how the interaction between the number of drought years experienced by a district in the period between 2001 and 2007 and the share of households below the two hectare eligibility cutoff affects program exposure. The dependent variable in all regressions is the share of credit eligible for the program. We define a "drought year" as any year in which total precipitation during the monsoon months of June to September was less than 75 percent of a district's long-run average. All regressions control for lagged monsoon rain, and a set of dummy variables indicating the number of years until the next schduled state election. Estimates in columns [2] to [4] add interactions between time and region fixed effects to control for heterogeneity in business cycles across regions and between urban and rural areas. Standard errors, in brackets, are calculated using the Huber-White correction for arbitrary heteroskedasticity and clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)
Dependent variable	**Program exposure**			
Drought_years*Below_cutoff	0.237***	0.237***	0.247***	0.257***
	[0.046]	[0.046]	[0.047]	[0.049]
Observations	4096	4096	4096	4096
R-squared	0.061	0.061	0.101	0.774
Joint F-Statistic	5.21	7.63	9.41	23.16
Region*Year FE	No	Yes	Yes	Yes
District FE	No	No	Yes	Yes
District trends	No	No	No	Yes
Clustered SE	district	district	district	district

Table IV
Credit Supply – Effect on Post-Program Lending (District)

The regressions in this table estimate the impact of debt relief on post-program credit supply. For each dependent variable, each column reports results from a separate regression. The first two columns (columns [1]-[2] and [5]-[6]) report OLS estimates, the second two columns (columns [3]-[4] and [7]-[8]) report instrumental variables regressions using the first stage relationship reported in Table III. The dependent variable in columns [1]-[4] is the log number of loans outstanding. The dependent variable in columns [5]-[8] is the log amount of credit outstanding. In addition to the fixed effects reported in the table, all regressions control for the deviation of lagged monsoon rainfall from its long run average, a full set of electoral cycle dummies indicating the number of years until the next scheduled state election. Standard errors, in brackets, are heteroskedasticity robust and clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	**Log(Loans)**				**Log(Amount)**			
	OLS	OLS	2SLS	2SLS	OLS	OLS	2SLS	2SLS
Bailout_share*post	-0.022	-0.058**	-0.019	-0.036	-0.035***	-0.138***	-0.176***	-0.150**
	[0.014]	[0.028]	[0.037]	[0.063]	[0.013]	[0.023]	[0.045]	[0.068]
observations	4,553	4,553	4,096	4,096	4,553	4,553	4,096	4,096
# clusters	489	489	433	433	489	489	433	433
R-squared	0.735	0.805	0.735	0.798	0.93	0.951	0.92	0.950
Region*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	No	Yes	No	Yes	No	Yes	No	Yes
Clustered SE	district	district	district	district	district	district	district	district

Table V
Moral Hazard – Effect on Post-Program Loan Performance (District)

The regressions in this table estimate the effect of debt relief on loan performance. For each dependent variable, each column reports results from a separate regression. The first two columns (columns [1]-[2] and [5]-[6]) report OLS estimates, the second two columns (columns [3]-[4] and [7]-[8]) report instrumental variables regressions, using the first stage relationship reported in Table III. The dependent variable in columns [1]-[4] is a dummy variable equal to one if a district experienced an increase in the share of non-performing loans. The dependent variable in columns [5]-[8] is a dummy variable equal to one if a district experienced an increase in the share of non-performing credit. In addition to the fixed effects indicated in the table, all regressions contain the full set of controls described in Table IV. Standard errors, reported in brackets, are heteroskedasticity robust and clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	**Non-performing loans (number of loans)**				**Non-performing loans (share of credit)**			
	OLS	OLS	2SLS	2SLS	OLS	OLS	2SLS	2SLS
Bailout_share*post	0.070***	0.039	0.513**	0.691*	0.094***	0.028	0.464**	0.642*
	[0.024]	[0.062]	[0.219]	[0.385]	[0.023]	[0.060]	[0.203]	[0.355]
observations	2,246	2,246	2,023	2,023	2,278	2,278	2,055	2,055
# clusters	460	460	412	412	460	460	412	412
R-squared	0.091	0.303	0.081	0.231	0.172	0.42	0.036	0.358
Region*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	No	Yes	No	Yes	No	Yes	No	Yes
Clustered SE	district	district	district	district	district	district	district	district

Table VI
Effects on Credit Supply and Loan Performance (Bank-District)

The regressions in this table estimate the impact of debt relief on credit supply and loan performance, using panel data at the bank-district level. The dataset is based on proprietary data from India's four largest commercial banks and covers agricultural lending and loan performance at 27,678 bank branches between 2006 and 2012. To allow comparability, the sample is restricted to the districts contained in the district level dataset used in Tables IV and V. The first two columns examine the effect of the bailout on credit supply. The dependent variable in column [1] is the log number of agricultural loans outstanding. The dependent variable in column [2] is the log amount of agricultural credit outstanding. The second two columns explore the effect of the bailout on loan performance. The dependent variable in column [3] is the share of non-performing agricultural loans. The dependent variable in column [4] is the share of non-performing agricultural credit. The variable $Bailout\_share_{ij}$ is a district's exposure to the bailout program. $Bailout\_share_j$ measures the mean bailout exposure of each bank across all districts in which it is present. To account for time-varying credit demand shocks, all regressions control for district-time fixed effects. Standard errors, in brackets, are calculated using the Huber-White correction for arbitrary heteroskedasticity and clustered at the district-year level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)
Dependent variable	**Lending**		**Loan performance**	
	Log(Loans)	Log(Amount)	NPL(Loans)	NPL(Amount)
$\text{Bailout\_share}_{ij}$*post	-0.193***	-0.240***	0.018***	0.027***
	[0.048]	[0.036]	[0.004]	[0.003]
Bailout_share_{j}*post	1.805***	0.661***	0.014***	0.005***
	[0.077]	[0.025]	[0.004]	[0.001]
observations	6,205	9,750	6,206	9,741
# districts	489	489	489	489
# clusters	2,918	3,006	2,918	3,006
R-squared	0.612	0.427	0.556	0.393
District*Year FE	Yes	Yes	Yes	Yes
Clustered SE	district-year	district-year	district-year	district-year

Table VII
Real Effects of the Bailout – Productivity, Wages and Employment

The regressions in this table estimate the effect of debt relief on real outcomes, using a district-level data for the years 2006 to 2012. For each dependent variable, each column reports results from a separate regression. The first two columns (columns [1]-[2], [5]-[6]) and [9]-[10] report OLS estimates, the second two columns (columns [3]-[4], [7]-[8] and [11]-[12]) report instrumental variables estimates based on the first stage regressions in Table III. The dependent variable in columns [1]-[4] is district level productivity, calculated as agricultural revenue per hectare at constant 2001 commodity prices. The dependent variable in columns [5]-[8] is the district level real wage for unskilled agricultural occupations. The dependent variabe in columns [9]-[12] is the district level average of monthly per capita consumption expenditure (Mpce), calculated from Indian National Sample Survey (NSS) microdata. Details on data sources and the construction of these variables are available in Table A.I and Appendix E. In addition to the fixed effects reported in the table, all regressions control for the deviation of lagged monsoon rainfall from its long run average, electoral cycle dummies indicating the number of years until the next scheduled state election. All regressions additionally control for time*region fixed effects to account for business and credit cycle heterogeneity across regions. Standard errors, in brackets, are calculated using the Huber-White correction for arbitrary heteroskedasticity and clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable	**Log(Productivity)**				**Log(Real wage)**				**Log(Consumption)**			
	OLS	OLS	2SLS	2SLS	OLS	OLS	2SLS	2SLS	OLS	OLS	2SLS	2SLS
Bailout_share*post	0.001	0.000	-0.006	0.006	0.020	0.023	-0.018	-0.070	0.000	0.011	-0.053	-0.256**
	[0.002]	[0.002]	[0.010]	[0.010]	[0.016]	[0.017]	[0.074]	[0.099]	[0.008]	[0.008]	[0.075]	[0.104]
observations	1,621	1,621	1,530	1,530	751	751	685	685	1,928	1,928	1,727	1,727
R-squared	0.116	0.264	0.104	0.259	0.114	0.152	0.119	0.129	0.916	0.927	0.911	0.846
Region*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Clustered SE	district	district	district	district	district	district	district	district	district	district	district	district

Table VIII
Robustness – Placebo Experiments

This table reports the results of two placebo experiments testing for the presence of non-linear time trends in outcomes of interest. The first placebo experiment, reported in columns [1]-[4], restricts the sample to the period prior to the program date and estimates treatment effects for a counterfactual program date. The second placebo experiment, reported in columns [5]-[8], tests for the presence of unobserved non-linear time trends. We generate N=1,000 vectors containing randomly reassigned values of the treatment variable and use these placebo treatment vectors to estimate counterfactual program effects and bootstrap standard errors. Treatment effect estimates are obtained from regressions using the same set of controls listed in tables IV and VI. * p<0.10 ** p<0.05 *** p<0.01.

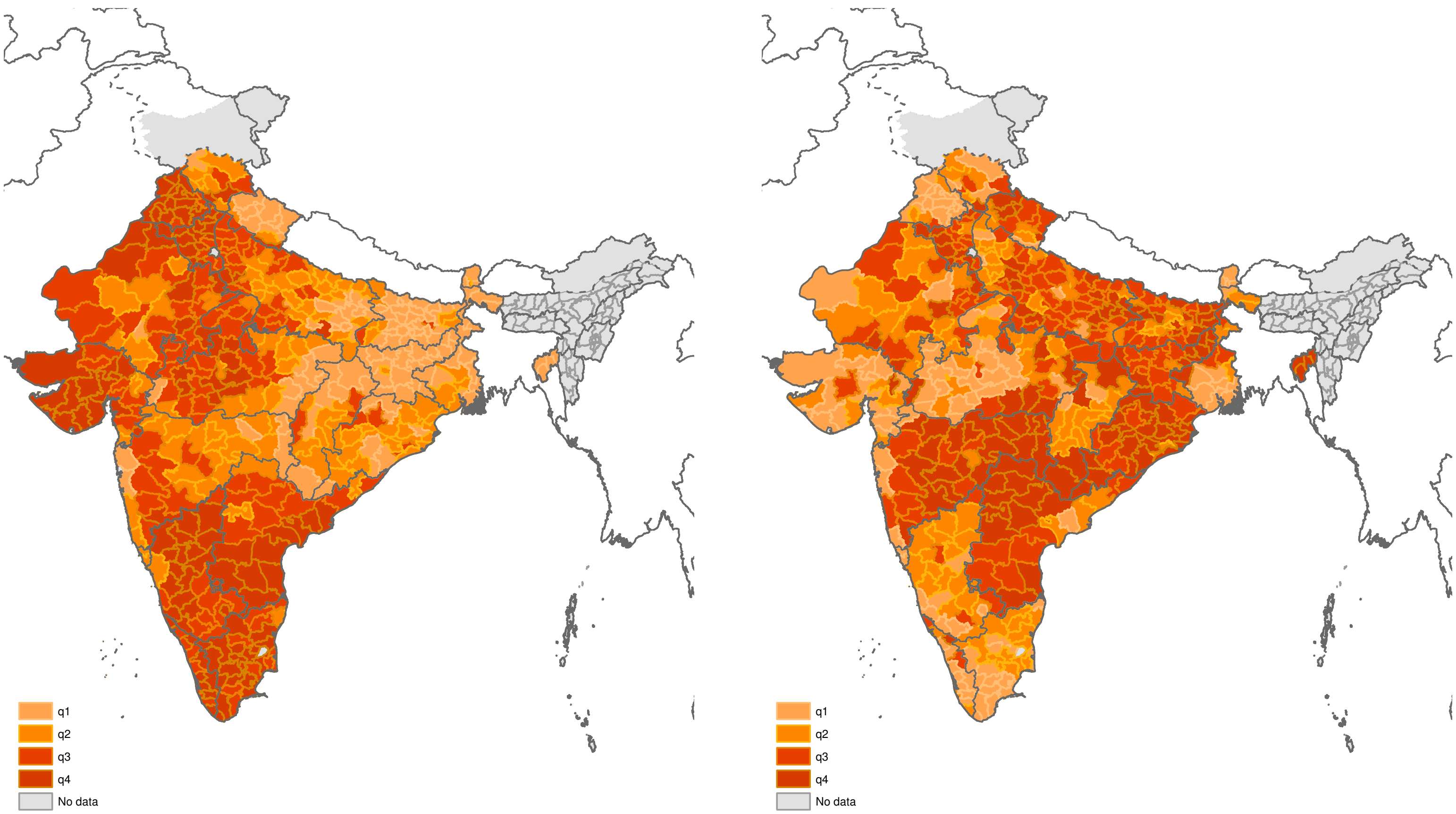
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Placebo [post=1(t≥2007)]				**Placebo [reassigned treatment]**			
Dependent variable	Log(Loans)	Log(Amount)	NPL(Loans)	NPL(Amount)	Log(Loans)	Log(Amount)	NPL(Loans)	NPL(Amount)
PANEL A: Treatment effect estimates								
Bailout_share*post	-0.036	-0.150**	0.691*	0.464**	-0.036	-0.150**	0.691*	0.464**
	[0.063]	[0.068]	[0.385]	[0.203]	[0.063]	[0.068]	[0.385]	[0.203]
PANEL B: Placebo estimates								
Bailout_share^{Placebo}*post	-0.007	0.025	-0.109*	-0.023	-0.010	-0.026	0.029	0.010
95% Confidence interval	[-0.125	[-0.113	[-0.223	[-0.136	[-0.079	[-0.138	[-0.001	[-0.008
	0.111]	0.163]	0.005]	0.090]	0.059]	0.086]	0.056]	0.028]
Region*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	district	district	district	district	district	district	district	district

Table IX
Robustness – Heterogeneous Response to Shocks

This table reports results of a robustness test that checks that results are not driven by a heterogeneous program response to shocks in districts with a significant share of large landholdings. In particular, we augment our preferred specification with the term $bigland \times post$, where *bigland* is a dummy variable equal to one in districts where more than five percent of all landholdings are larger than four hectares. Treatment effect estimates are obtained from regressions using the same set of controls listed in tables IV and VI. Panel A reports coefficient estimates form our preferred specification as a basis for comparison. Panel B reports the results of the robustness test. Heteroskedasticity robust standard errors, reported in brackets, are clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Log(Loans)	Log(Amount)	NPL(lLoans)	NPL(Amount)	Log(Productivity)	Log(Wage)	Log(Consumption)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
PANEL A: Treatment effect estimates							
Bailout_share$_i$*post	-0.036	-0.150**	0.691*	0.642**	0.006	-0.070	-0.256**
	[0.063]	[0.068]	[0.385]	[0.355]	[0.010]	[0.099]	[0.104]
PANEL B: Controlling for post*bigland							
Bailout_share$_i$*post	-0.032	-0.121***	0.345	0.289*	0.015	0.096	-0.189**
	[0.038]	[0.044]	[0.211]	[0.168]	[0.010]	[0.109]	[0.077]
observations	4,096	4,096	2,055	2,398	1,875	829	1,727
R-squared	0.731	0.926	0.118	0.085	0.143	0.101	0.881
Region*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	district	district	district	district	district	district	district

Figure 1: Program Exposure by District



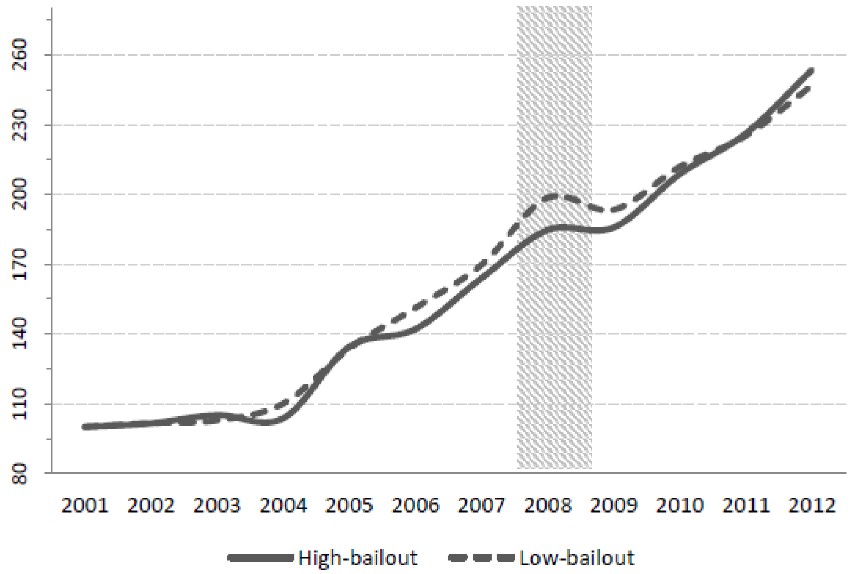
(a) Credit per capita, pre-program

(b) Program exposure, share of credit forgiven

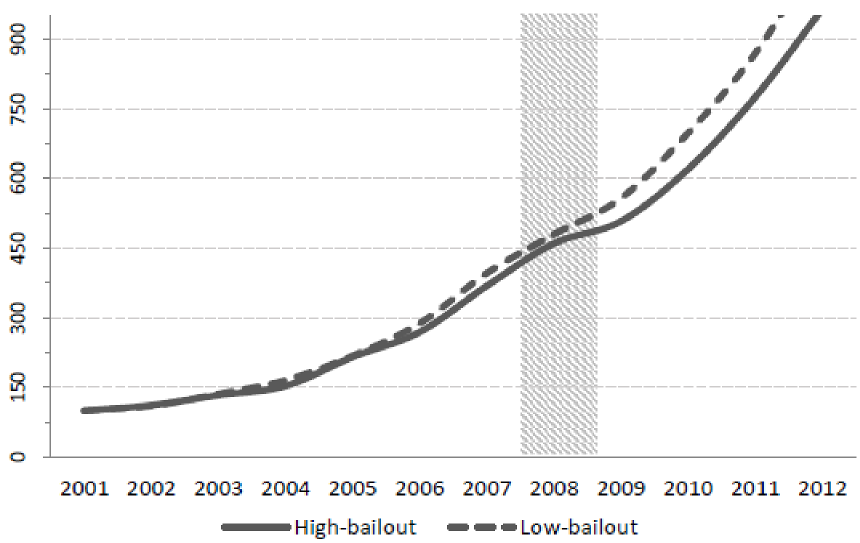
Notes: The figure plots the geographical distribution of agricultural credit per capita on March 31, 2008 (the date of the program announcement) in Panel (a) against district level exposure to the bailout, measured as the share of credit outstanding eligible for debt relief under the program in Panel (b). Both variables are plotted as quartiles, darker colors indicate higher amounts of credit per capita and bailout exposure, respectively.

Figure 2: Parallel Trends – Credit Supply

(a) Number of Loans [2001=100]



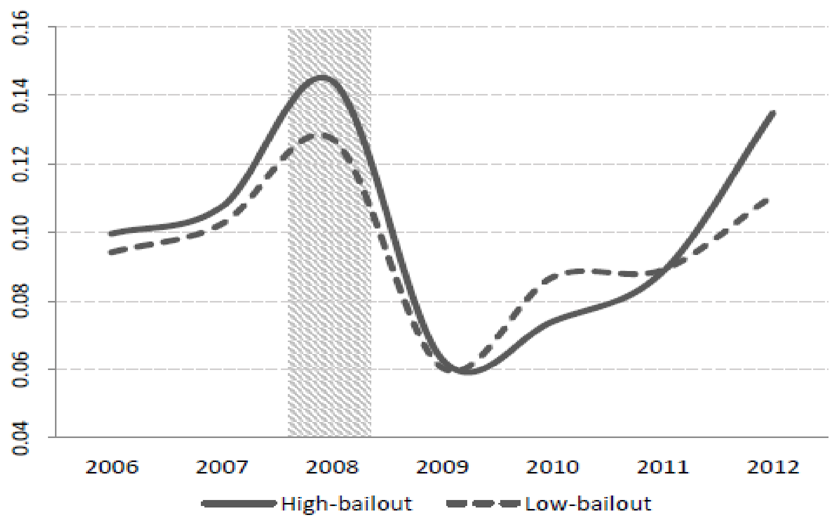
(b) Total Credit [2001=100]



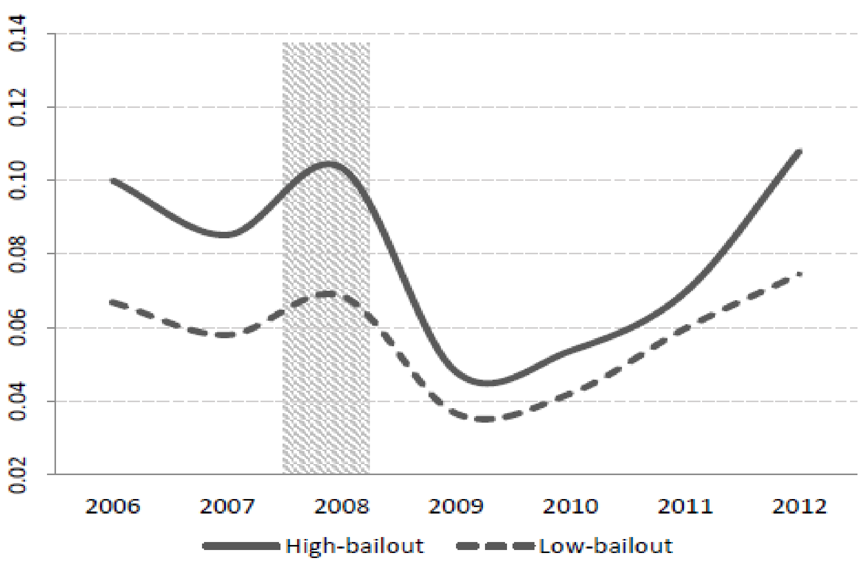
Notes: The figure plots the time series of the number of agricultural loans outstanding in panel (a) and the amount of agricultural credit outstanding in panel (b) for districts with "high" and "low" program exposure. Districts are classified into "high" and "low" program exposure relative to the median of the treatment variable. The shaded region marks the year in which the bailout program was enacted, which is omitted from the analysis.

Figure 3: Parallel Trends – Loan Performance

(a) Non-performing loans [accounts]



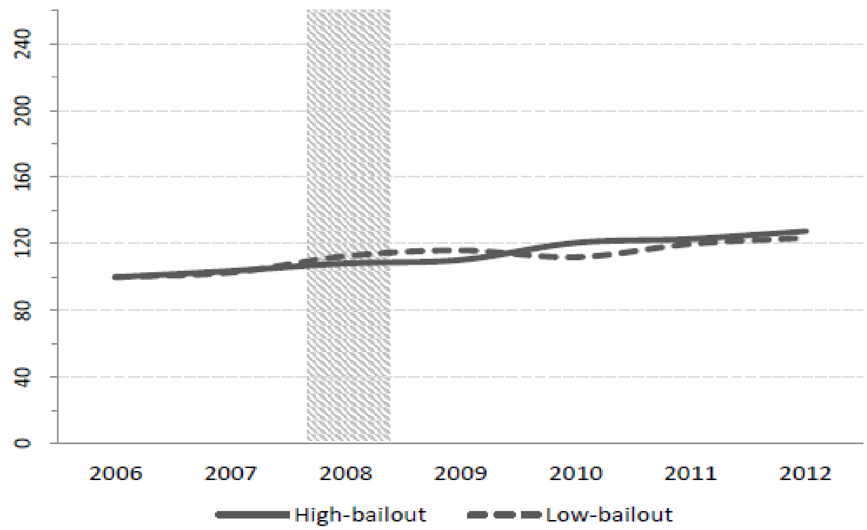
(b) Non-performing loans [share of credit]



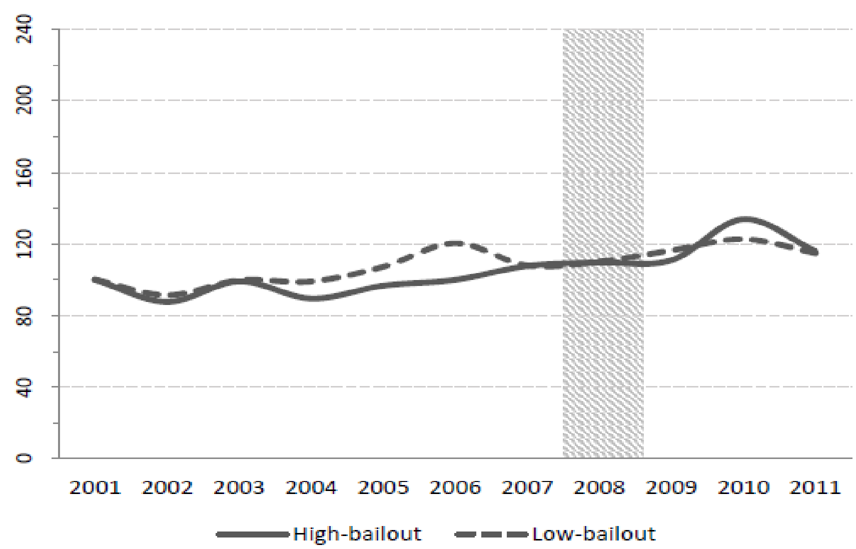
Notes: The figure shows the time series of loan performance, measured as the share of non-performing agricultural loans in panel (a) and the share of non-performing agricultural credit in panel (b) for districts with "high" and "low" program exposure, respectively. Districts are classified into "high" and "low" program exposure relative to the median of the treatment variable. The shaded region marks the year in which the bailout program was enacted, which is omitted from the analysis.

Figure 4: Parallel Trends – Real Effects

(a) Rural wages [2006=100]



(b) Productivity [2001=100]



Notes: The figure plots the time series of rural unskilled wages (a) and agricultural productivity, measured as revenue per hectare at constant 2001 commodity prices (b) for districts with "high" and "low" program exposure. Districts are classified into "high" and "low" exposure relative to the median of the treatment variable. The shaded region marks the year in which the bailout program was enacted, which is omitted from the analysis.

Supplemental Appendix

'The Economic Effects of a Borrower Bailout: Evidence from an Emerging Market'

Xavier Giné[†] **Martin Kanz**[‡]

A Post-Program Credit Supply: How much Reallocation?

In this appendix, we report an alternative measure of the credit reallocation from "high-bailout" to "low-bailout" districts in response to the bailout. To do this, we estimate a difference-in-differences model similar equation 4.1, but replace our measure of treatment intensity by the total amount of credit eligible for the bailout in a given district. We then estimate the effect of this variable on the net amount of post-program credit for districts above and below the median program expenditure, so that our coefficient estimates can be interpreted as the *amount of new lending per dollar of bailout received*. The estimates in Table A.I indicate that 1 dollar of bailout is associated with 2.4 - 4.1 dollars of new lending in low-bailout districts, but only 0.23 - 0.34 cents of new lending in high-bailout districts. This provides evidence that banks actively reallocated credit away from districts with poorer loan performance in the pre-program period. The results also suggest that this credit reallocation was asymmetric, with high-bailout districts suffering a disproportionate decline in new lending after the program.

Table A.I Effect of the Bailout on Credit Supply – Reallocation

	(1)	(2)	(3)	(4)
	Amount credit			
	High-bailout district		Low-bailout district	
Eligible_amount*post	0.232*	0.336	2.437***	4.078***
	[0.136]	[0.212]	[0.268]	[0.439]
observations	2,276	2,276	2.277	2,277
R-squared	0.736	0.844	0.787	0.870
District FE	Yes	Yes	Yes	Yes
District trends	No	Yes	No	Yes
Clustered SE	district	district	district	district

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B Pre-program Outcomes and District Characteristics

In this appendix, we present additional tests to validate our instrumental variables identification strategy.

First, one might be concerned the exclusion restriction is violated. The exclusion restriction requires that the instrument –the interaction between the program cutoff and the number of pre-program drought years in a given district– affect outcomes only through its effect on program exposure. This condition may be violated if, for example, changes in economic outcomes are significantly different in districts that are more prone to drought shocks or have a larger share of households below the two hectare collateral threshold.

While this cannot be tested directly, we address this possibility by estimating the conditional correlation between the instrument and outcomes of interest in the pre-program period. To perform this test, we restrict the sample to the period prior to the bailout and estimate panel regressions estimating the correlation between the instrument (lagged for each time period) and pre-program outcomes. The results, reported in Table B.II, indicate that there is no correlation between the first stage and any of the main outcomes of interest in the period prior to the bailout.

Table B.II Test – Effects on Pre-program Outcomes

	(1)	(2)
	Coefficient	*p-value*
Dependent variable	*Drought_years*cutoff*	(H_0: *No conditional correlation*)
Log(# loans)	-0.012 [0.036]	0.739
Log(amt credit)	0.002 [0.038]	0.957
Non-performing loans	-0.069*[0.040]	0.086
Log(productivity)	-0.002 [0.004]	0.610
Log(rural wage)	0.018 [0.102]	0.860
Log(consumption)	-0.088 [0.086]	0.310

The second potential challenge to our identification strategy is the possibility that cross-sectional variation in the instrument correlates with district characteristics at the time of the bailout, in a way that puts districts on a different growth path in the post-program period for reasons unrelated to the program. We address this possibility by testing for a direct relationship between our first stage and a set of observable district characteristics at the time of the program announcement. The results are presented in Table B.III. We find no evidence that our predictor of program exposure is systematically related to observable district characteristics, such as the share of a district's rural population or productivity that may have put districts on a different growth path in the post-program period for reasons other than bailout exposure.

Table B.III Test – Effects on District Characteristics in 2008

	(1)	(2)
	Coefficient	*p-value*
Dependent variable	*Drought_years*cutoff*	(H_0: *No conditional correlation*)
Log(Population)	-0.129 [0.132]	0.329
Share rural population	0.006 [0.017]	0.713
Log(Total ag credit)	-0.064 [0.134]	0.633
Log(Productivity)	-0.012 [0.011]	0.282

C Derivation of Bank-District Level Regression Equation

This appendix presents a simple model of the credit market, from which we derive the estimating equation for credit market outcomes at the bank-district level. We model the bailout as an equity shock to the bank and relax the assumption that banks must lend in the same district, as intended by the government. This allows us to derive an estimating equation in which we can separate the impact of the equity shock on the bank lending channel from the reallocation of post-program lending across districts where the bank has a presence.

Consider an economy in which $j \in 1...J$ banks provide credit to borrowers in $i \in 1...N$ districts, so that the lending of bank j in district i at time t can be written as C_{ijt}. We assume banks can raise deposits at the marginal cost $\theta_D D_{ijt}$, and that loans are financed by either deposits or equity, so that each bank's lending must be consistent with the identity

$$C_{jt} \equiv D_{jt} + E_{jt}. \tag{C.1}$$

In each period, banks may experience supply shocks $\tilde{e}_{jt} = e_t + e_{jt}$ that determine the level of equity available to each bank. For the average district, the effect of this supply shock can be written as $E_{ijt} = (e_t + e_{jt})/N$, where N denotes the number of districts.

On the demand side, we assume that in any given period, a district may experience a demand shock $\tilde{\delta}_{it} = \delta_t + \delta_{it}$ with an economy-wide and a district-specific component. Hence, the marginal return of a loan L_{ijt} is given by $\delta_t + \delta_{it} - \theta_C C_{ijt} - \theta_B B_{ijt}$, where B_{ijt} is the amount of non-performing assets of bank j in district i. We assume that θ_C and $\theta_B \in [0, 1)$, so that the marginal return of an additional unit of credit to district i is declining in the amount of credit outstanding as well as the district's stock of non-performing loans.

Equating marginal cost and marginal return, we obtain the condition $\theta_D D_{ijt} = \delta_t + \delta_{it} - \theta_C C_{ijt} - \theta_B B_{ijt}$. Combining this expression with the identity in equation (C.1), we can solve for the credit market equilibrium

$$C_{ijt} = \frac{\delta_t + \frac{1}{N}\theta_D e_t}{\theta_D + \theta_L} + \frac{\frac{1}{N}\theta_D}{\theta_D + \theta_L} e_{jt} - \frac{\theta_B}{\theta_D + \theta_L} B_{ijt} + \frac{1}{\theta_D + \theta_L}\delta_{it} \tag{C.2}$$

The first term in this equation is an economy-wide time-varying constant that includes demand and supply shocks. The last term is a district-specific time-varying constant. Both can be combined into a time-varying district fixed effect ξ_{it}. The second and third terms contain the main coefficients of interest. We can then rewrite equation (C.2) as follows

$$\ln(C_{ijt}) = \xi_{it} + \gamma_E e_{jt} + \gamma_B B_{ijt} + \chi_{ijt} \tag{C.3}$$

This equation can be estimated directly, using credit data at the bank-district level, and allows us to isolate the impact of the bailout on credit supply by controlling for demand shocks using a simple fixed effects strategy. The model underlying our estimating equation predicts that $\gamma_E > 0$ and $\gamma_B < 0$. That is, banks experiencing a larger equity shock (greater bailout exposure) should increase lending. At the same time, banks should redistribute lending away from districts with greater program exposure, as the program allows them to clean troubled assets from their books and thus reduces incentives that may have existed to "evergreen" loans close to default prior to the program (Peek and Rosengren [2005]). This can be tested against the alternative hypothesis that the program encouraged banks to lend in a given district an amount proportional to the bailout funds received. If this were the case, then one would expect $\gamma_E > 0$ and $\gamma_B > 0$.

D Mechanisms: Moral Hazard

The results in section 4.2 document that the Adwdrs bailout had a strong negative effect on loan performance, which does not appear to be driven by greater bank risk-taking. In this section, we present additional results to shed light on the mechanism behind this result. Specifically, we exploit the exogenous timing of Indian state elections to isolate changes in loan performance that occur for economic reasons from defaults that correlate with the electoral cycle and can be regarded as pure moral hazard.

Our is based on the following argument. There are two potential channels that can lead to a decline in loan performance ahead of election years. First, electoral cycles in default can arise from changes in credit market conditions, such as increased credit supply, reduced screening and monitoring standards and a change in loan sizes around elections.[21] Second, electoral cycles in default can arise from moral hazard among borrowers who expect credit enforcement to be more lenient in election years. If the bailout indeed induced moral hazard by heightening expectations of future credit market interventions, we would expect the magnitude of political cycles in default to increase after the bailout, even when we condition on credit market conditions and the local business cycle.

To test for this channel explicitly, we exploit the exogenous timing of Indian state elections. Each of India's 28 states holds state assembly elections on a 5-year cycle. These elections are staggered in time, with an average of 4–6 states holding elections in a given

[21] See Cole [2009b] and Cole [2009a]for evidence on politically motivated lending in India and political cycles in agricultural lending.

calendar year. We use this exogenous source of variation in Table D.IV, where we regress the share of non-performing loans on an interaction between a post-program dummy and an indicator equal to one in the two years preceding a state election. Because state governments have the power to call early elections, we define this variable with reference to the time remaining to the next *scheduled* state election.

We find that electoral cycles in loan defaults are magnified in the post-program period. On average, defaults in pre-election years increase by an additional 1–2 percentage points (over the post-program sample mean of 7.25%) after the bailout. In unreported results, we verify that the greater loan delinquencies around election times are not driven by concurrent changes in loan sizes that might affect the average debt burden and repayment capacity of borrowers. We find no evidence of a systematic change in average loan sizes around elections prior to the bailout or as a result of ADWDRS.

These results are consistent with the concern that the cost of politically motivated credit market interventions arises, to a significant extent, from their effect on borrower expectations. While our data do not allow us to test this hypothesis explicitly, one might expect this adverse effect of credit market-led stimulus programs on borrower behavior to be particularly pronounced in credit markets with significant state ownership of banks and a history of politically motivated credit market interventions, such as the one we study.

Table D.IV Mechanism – Defaults and the Electoral Cycle

	(1)	(2)	(3)	(4)
Dependent variable	**NPL (Loans)**		**NPL (Amount)**	
	OLS	OLS	OLS	OLS
Close_to_election*post	0.064*	0.081	0.132***	0.131*
	[0.039]	[0.073]	[0.048]	[0.076]
observations	3,096	3,096	3,096	3,096
R-squared	0.270	0.461	0.27	0.462
Region*Year FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
District trends	No	Yes	No	Yes
Clustered SE	district	district	district	district

E Measuring Real Economic Activity

This section describes the construction of outcome variables used to analyze the effect of the bailout on real economic activity in Table VII.

Productivity: To construct a measure of agricultural productivity at the district level, we use data on crop yields from the Indian Department of Agriculture database on Agricultural

Prices in India. The dataset is an unbalanced panel which reports the (i) area cultivated and (ii) total harvest in tons for each the 25 most common crops in India for all districts of India over the time period 2000/01–2010/11. We first aggregate these data to the district as defined in the 2001 census, our unit of analysis throughout the paper. For each year and crop we aggregate total area planted and crop output for the two crop seasons in India, Kharif and Rabi. To measure productivity in monetary terms, we collect commodity prices for the base year 2001 from the Indiastat dataset on *"District-wise farm harvest prices of principal agricultural crops in India"* (available at http://www.indiastat.com/agriculture) for all crops in the dataset as of January 2001. Letting a^c_{it} denote the area planted with crop $c \in \{1...C\}$ in district i and year t, letting r^c_{it} denote the total production of crop c in district i and year t, and letting $p^c_{\cdot 01}$ denote the price of commodity c in the base year 2001, we calculate productivity per hectare in district i and year t as $Productivity_{it} = \frac{\sum_{c=1}^{C}\{r^c_{it} \cdot p^c_{\cdot 01}\}}{\sum_{c=1}^{C} a^c_{it}}$. We use constant prices for the base year 2001 to construct a productivity measure that is unaffected by commodity price shocks and allows us to isolate the component of productivity that can be affected by bailout beneficiaries.

Wages: Rural wages are calculated from the Indian Department of Agriculture's Agricultural Wages in India (AWI) dataset. The dataset contains wages for several unskilled professions by district and month. We measure the wage for unskilled labor as the average wage of agricultural field labor. Data are available for an unbalanced panel of 264 districts between the years 2006–2012.

Consumption: We measure household consumption using round of the Indian National Sample Survey (NSS). We use the 2004–2005, 2006–2007, 2009–2010 and 2011–2012 rounds of the NSS and focus on monthly household consumption expenditure, measured annually in schedule 1 of the NSS. To ensure that our measure of household consumption is consistent across different NSS rounds, we use monthly per capita expenditure (Mpce). This key indicator of household consumption is available for all survey rounds and widely used in the calculation of poverty measures at the national and sub-national level. We calculate the arithmetic mean of repeated cross-sectional survey rounds at the district-level, and obtain a panel of monthly per capita consumption for 238 districts.