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

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Abstract

Economic stimulus programs that operate through the credit market may give rise to particularly severe moral hazard problems because they change the contracting environment and distort borrower incentives. We test this proposition using a natural experiment arising from a nationwide bailout program for highly-indebted households in India. Our empirical strategy exploits local variation in cross-sectional exposure to the program, as measured by the share of qualifying loans at the time of the program announcement. We find that the program generated no measurable productivity gains, but led to significant moral hazard in loan repayment. 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. In addition, loan defaults become significantly more sensitive to the electoral cycle in the post-program period. This suggests that expectations of future credit market interventions generated by the bailout are an important channel through which moral hazard in loan repayment is intensified.

JEL: G21, G28, O16, O23
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1 Introduction

Economic stimulus programs have been used in a wide variety of economic contexts the world over in an effort to spur economic activity. The fundamental question of whether governments can improve economic outcomes through such programs, however, remains controversial and has received heightened attention in the wake of the 2008 global economic crisis. While economists in the "Keynesian" tradition advocate government interventions to increase aggregate demand and reduce the negative externalities of a prolonged economic downturn for society (Campbell et al. [2011], Mian et al. [2010]), critics argue that fiscal stimulus programs create the potential for political capture and moral hazard and can have adverse distributional consequences (Agarwal et al. [2013]).

A more nuanced version of this debate considers the comparative merits of different types of economic stimulus programs. In its simplest form, a stimulus program provides direct subsidies or income support to firms and individuals. In many cases, however, stimulus programs operate through the credit market, for example in the form of mandated debt restructuring programs or the public takeover of private liabilities.

The economic argument in favor of stimulus programs operating through the credit market rests on the premise that such policies will prevent excessive deadweight losses from foreclosure in settings where contracts are incomplete and market participants are unable to insure against macroeconomic shocks (Bolton and Rosenthal [2002]). Opponents of this view argue that political interventions in the credit market are a particularly harmful way of implementing stimulus programs because they change the contracting environment and are likely to give rise to moral hazard problems by distorting borrower expectations. Although this is ultimately an empirical question, very little evidence exists on the effect of such policies on credit supply and ex-post borrower behavior despite the fact that credit-market led stimulus programs are ubiquitous.

We shed light on these issues by analyzing one of the largest household debt relief programs in history, enacted by the government of India in 2008 against the backdrop of the global economic crisis. 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 and amounted to a volume of more than US$ 16 billion or 1.7% of GDP.

India's ADWDRS program is an especially attractive testing ground to explore the impact of a stimulus program on credit market outcomes for several reasons. First, the program is quite representative of a wide range of stimulus programs executed through policy interventions in the credit market. In the United States, several states intervened into debt contracts by passing debt moratoria intended to prevent excessive foreclosures and to protect rural constituencies from the fallout of the Great Depression (Rucker and Alston [1987]). More recent interventions in debt contracts in the United States include programs for mortgage renegotiation in response to the foreclosure crisis of 2008 (Agarwal et al. [2013], Guiso et al. [2013]). In developing countries, governments have routinely implemented debt relief and debt restructuring programs, often targeted at the economically important and politically influential rural sector. Some 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, unlike many other political interventions in the credit market, it was a one-off initiative that left the formal institutional and regulatory environment unchanged, thus allowing us to isolate the effect of the capital injection. But perhaps more importantly, unlike any of the previous debt relief initiatives in India, eligibility for the bailout program depended on the amount of land pledged at the time of loan origination, typically many years prior to the program. This rule, applied retrospectively, implies that the share of credit that could qualify for the program is a function of the land distribution in a given district. The land distribution in turn interacts with the time series of productivity shocks faced by a given district to determine the share of qualifying credit that was actually in default at the time the program was introduced.

Our empirical strategy exploits this exogenous geographical variation in program exposure at the district level to identify the causal impacts of the bailout on ex-post credit supply and borrower behavior. Using public and proprietary data on bank lending and loan performance, we construct a new dataset tracing credit allocation and loan delinquencies for 491 districts of India over the period 2001-2012. We match these data with information on the amount of debt relief as a share of total credit allocated in each district, which forms our primary measure of program exposure. The data reveal substantial variation in exposure to the debt relief program with the share of total credit waived under the program ranging from an average of less than 3% in some districts of the state of Goa to more than 50% of total credit in many districts of the states of Bihar, Jharkand, Orissa and Uttar Pradesh.

Our baseline estimates indicate that the bailout had a significant and economically large effect on post program credit allocation. These effects are larger in areas with greater exposure to the bailout, and robust to alternative specifications and construction of the variable measuring treatment intensity. A one standard deviation in the share of bailout leads to an increase in the growth rate of agricultural credit of 12.5% but no discernible increase in the number of accounts, indicating that additional credit was disbursed to existing customers in good standing. This suggests that banks acted conservatively by not expanding the customer base. Following the loan waiver program, however, defaults increased, either because borrower discipline declined (moral hazard) or because borrowers' debt burden had increased as the loan size was larger. To disentangle both explanations, we focus on defaults and disbursements around elections and find that while defaults increase before elections after the loan waiver, there is no such increase in disbursements. We thus conclude that moral hazard is the more likely explanation for the rise in defaults. Consistent with this explanation, we find no evidence of improvements in agricultural productivity following the loan waiver.

These results contribute to several strands of the literature. To the best of our knowledge, we provide the first empirical evidence on the aggregate credit market and moral hazard implications of large-scale debt relief programs. In this sense our findings contribute to a nascent literature on the market response and broader economic impact of stimulus programs. Agarwal et al. [2013] study subsidized mortgage renegotiations under the Home Affordability Modification Program in the wake of the foreclosure crisis in the United States, and Mian and Sufi [2012] study the impact of a stimulus program offering subsidies for new car purchases on auto sales and broader economic outcomes. Our analysis differs from this literature as it focuses on a stimulus initiative enacted through credit market, considering its direct effect on subsequent lending on the intensive and extensive margins, and post-program moral hazard in loan repayment.

Because the bailout affected not only borrowers but was also tied to a recapitalization of banks refinanced by the Reserve Bank of India as part of ADWDRS, our results are also related to the literature on the real effects of bank recapitalizations (Diamond and Rajan [2000], Paravisini [2008], Philippon and Schnabl [2013] and Gianetti and Simonov [2013]). There is much reason to believe that prior to the announcement of ADWDRS, Indian banks faced significant incentives for evergreening de-facto non-performing loans. Reflecting a long history of directed lending (Burgess and Pande [2005], Cole [2009a]), all banks in India are required to lend 40% of their capital to "priority sectors", which include agriculture and small scale industry. Although this mandate forced the allocation of a significant share of credit to high-risk borrowers, local branches and branch managers faced sanctions for realizing losses and consequently had a significant incentive to keep lending to defaulters. The introduction of ADWDRS removed this incentive distortion. Consistent with the evergreening hypothesis (Peek and Rosengren [2005]), we find evidence of a shift in post-program lending away from districts with a high-share of total credit waived under the bailout. Indeed, one dollar of bailout led to an increase in net lending in subsequent years of only 9 cents in a high-bailout district and almost 70 cents in a low-bailout district. This suggests that the bailout did not encourage greater risk-taking by banks and thus helps us isolate the effect of ADWDRS on bank risk-taking from its impact on borrower behavior. Despite the geographical reallocation of new lending towards districts with observably lower ex-ante risk, we find a significant negative effect of the program on loan performance, concentrated among borrowers that had previously been in good standing and who did not benefit from the bailout.

Finally, the finding that loan performance (but not loan size) is responsive to the electoral cycle and that this effect is magnified by the introduction of the ADWDRS bailout program contributes to the literature on the political economy of credit in emerging markets (Cole (2009), Dinç (2005)) and underscores the concern with stimulus programs that they may lead to an anticipation of future interventions, especially in credit markets that have a history of political intervention.

The rest of the paper proceeds as follows. In Section 2, we provide an overview over the eligibility rules and timing of India's bailout program for rural households. Section 3 provides details about the data used and provides summary statistics. In Section 4 we discuss our empirical strategy and in ction 5 we present the results. Section 6 concludes.

2 India's Bailout Program for Rural Households

We study the impact of debt relief on credit supply and borrower moral hazard using a natural experiment generated by India's "Agricultural Debt Waiver and Debt Relief Program for Small and Marginal Farmers" or ADWDRS, one of the largest borrower bailout programs in history enacted by India's federal government and executed by the Reserve Bank of India beginnning in March 2008.

The goal of the program was to refinance all private, public sector, cooperative and regional rural banks through the cancellation of their non-performing rural assets accumulated due to the long history of directed lending to the rural sector. In turn, this reduction of household debt would serve as stimulus against the debt overhang and lack of access to credit among highly indebted rural households. Given that the bailout was announced a year ahead of national elections, the program also acted as a significant transfer from urban to rural voters.

The rules for program eligibility were kept deliberately simple to allow for the swift processing of claims, and to minimize corruption at local bank branches tasked with identifying eligible borrowers. In contrast to earlier debt relief initiatives, eligibility for the program at the level of an individual loan depended on the amount of land pledged as collateral at the time a loan was originated, typically several years before the program. Small borrowers who had pledged less than two hectares of land were eligible for full debt relief, while borrowers with overdue loans that had pledged more than two hectares of land qualified for 25% conditional debt relief if they were able to repay the remaining balance. Loans qualified for debt relief if they were originated between December 31, 1997 and December 31, 2007, more than 90 days overdue as of December 31, 2007 and remained in default until February 28, 2008. These rules were announced retrospectively in the Indian finance minister's budget speech on March 18, 2008 so that there was no scope for manipulation around program dates. In addition, it was the first time that eligibility was based on landholdings 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 as well as the details of the qualifying loan. Borrower lists underwent independent audits at the branch and bank level and an appeals process was put in place to allow borrowers a way to rectify errors in published beneficiary lists. 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. In contrast, 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, we use data as of December 2011, when the program was closed and all claims had been settled.

3 Data

Our dataset includes 489 districts for which we have information on debt relief amounts, as well as detailed credit outcomes and agricultural productivity data for the years 2001 to 2012. We aggregate all data to the level of an administrative district. In the base year 2001, India had 593 districts with an average population of 1,731,897 inhabitants. In that year the districts in our sample account for 94% of the Indian population and 89% of total bank credit.[1]

How did exposure to the bailout affect ex-post credit supply and borrower behavior? The key challenge we need to overcome to address this question is to form a credible control group that was not affected by the program. But because all districts were affected we define a continuous treatment variable measuring program exposure rather than classifying districts into treatment and control groups. We then form counterfactuals using the cross-sectional variation in program exposure.

To measure a district's exposure to the bailout, we collected data on the amount of debt relief granted under the program from each state's State Level Bankers' Committee, the administrative body responsible for maintaining regionally disaggregated data on publicly supported credit market interventions. Using these data we construct a variable measuring

[1]We have bank credit data for a total of 501 districts but in the analysis we drop 10 districts that correspond to the largest urban areas and 2 districts in Jammu and Kashmir that have virtually no bank penetration.

the share of outstanding credit waived under the program in each of the 491 districts in our sample. Specifically, let credit_{dt} denote the total amount of outstanding rural credit in district d at the time of the program deadline $\bar{t}$ on 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. Letting η denote the share of rural credit that was current at the program deadline and therefore unaffected by the program, district d's exposure to the program is equivalent to:

$$Bailout\_share_{d\bar{t}} = \frac{(1-\eta)\left[\text{credit}^S_{d\bar{t}} + .25\bar{\kappa}_d\text{credit}^L_{d\bar{t}}\right]}{\text{credit}^S_{d\bar{t}} + \text{credit}^L_{d\bar{t}}} \tag{1}$$

where $\bar{\kappa}_d$ 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 cutoff, our baseline estimates assume $\bar{\kappa}_d = 1$, which is equivalent to estimating the intent-to-treat (ITT) effect for households with more than two hectares of land pledged as collateral.[2] To ensure that our measure of program exposure includes all debt waiver allocations under the program,the variable is calculated with administrative data reported in December 2011, after the program had officially closed and all allocations had been made.

Table I reports summary statistics for the share of credit written off under the program and highlights significant geographical variation in program exposure. The mean (median) district in our sample saw 32.6% (28.4%) of outstanding credit waived under the program. Bailout shares at the state level are reported in Appendix B and range from 2.2% in Goa to

[2] Because debt relief was automatic for households below the cutoff, the intent-to-treat (ITT) and treatment-on-treated (TOT) estimates are identical for households with pledged landholdings below two hectares.

more than 60% in Orissa.

Data on credit are taken from the Reserve Bank of India's Basic Statistical Returns of Commercial Banks. Each year, all bank branches in India are required to report details of all loans on their books to the regulator. This annual 'census' of outstanding credit includes, for each district, details on the sectoral allocation of credit, interest rates, loan amounts and number of outstanding loans as of March 31st, the end of the Indian fiscal year. We use this geographically disaggregated dataset for the years 2001 to 2012 to construct time series of outstanding credit (amount and number of loans) for each district and for various types of credit. Figure 1 plots the growth in agricultural credit year by year. While credit outstanding continues to grow at the same rate before and after the program, the growth in the number of new accounts declines, suggesting that banks become more conservative when issuing new loans. And since disbursement is growing, average loan sizes must have increased for the relatively fewer individuals that have managed to borrow after the program.

Estimating the impact of the program on ex-post borrower behavior requires detailed information on loan performance. Because data on loan performance is not disclosed by the regulator at a regionally disaggregated level, we use a new proprietary dataset of lending and loan performance of India's five largest commercial banks. The dataset contains information on the number of loans and the volume of total/non-performing loans, disaggregated by bank and district for the years 2006 to 2012 and covers data from 27,678 bank branches, which account for approximately 62% of total agricultural credit. Figure 2 plots the year by year share of non-performing loans. Interestingly, there is a surge in NPLs from 2007 to 2008, possibly because Indian banks responded to the program by converting into default loans that were previously ever-greened. Indeed, the mandate to lend 40% of capital to "priority sectors", including agriculture, and the sanctions that local branch managers face for realizing losses creates an incentive to evergreen the loans of defaulters. Once the program is announced, these loans can be shown as non-performing in the books so that they will be covered by the waiver program. Mechanically, after the program the share of NPLs decline sharply as defaults are waived, but over time NPLs raise again.

Data on agricultural productivity come from the Indian Ministry of Agriculture's database on crop yields. We use 2001 commodity prices from Indiastat to construct the value of agricultural production per hectare in a given district and year. Appendix C provides additional details on the construction of the productivity series.

The analysis controls for exogenous changes in credit conditions using local variation in monsoon rainfall. Rainfall data were obtained from the Indian Meteorological Department and measure monthly precipitation based on rainfall gauges located in each district. The rainfall variable we use in our analysis is total monsoon rainfall between July and September for the previous year as a fraction of the district's long-run rainfall average over the same period. The analysis also controls for the district's distance to the next scheduled state election by including a full set of electoral cycle dummies. Electoral data come from the *Election Commission of India*'s database on state elections.[3] Finally, we use data on district characteristics from the Census of India and the Indian Agriculture Census. These data include the total population, urban and rural population shares, productivity and land distribution of each district. We provide additional details on the construction of variables in Appendix A. Summary statistics are reported in Table II.

4 Empirical Strategy

To assess the impact of the bailout on credit supply and ex-post borrower behavior we exploit variation in debt relief at the district level, due to the fact that the same program rules were applied uniformly across the country. Figure 3 plots the geographical dispersion of the share of total outstanding rural credit that was waived under the program (Panel (a)) and the growth in NPLs after the program (Panel (b)) by quartiles. In our analysis, we

[3] Available at http://eci.nic.in/eci_main1/key_highlights.aspx

treat the variation in Panel (a) as quasi-exogenous because it is determined jointly by the district's land-distribution, which affects the share of landholdings below the program cutoff that could have been eligible for full debt relief, and the time series of exogenous weather shocks in the district, which determines the share of loans actually in default at the time of the program deadline.

Table A.I correlates program exposure with district level measures of land distribution and rainfall. Column 1 reports a negative and significant correlation between share of individuals with landholdings below the two hectare eligibility threshold and the share of agricultural credit waived. Column 2 includes a non-linear term and finds a weaker relationship. However, a district's land distribution is only one factor determining program intensity, the second being the time series of exogenous income shocks which, in rural India are mostly due to variation in the onset and intensity of the southwest monsoon. This explains why program exposure varies not only across state lines, but also within areas with a relatively similar land distribution, such as parts of Rajasthan and Southern Karnataka. Column 3 correlates the numer of drought years prior to the program from 2001 to 2007 with the share of agricultural credit waived. As expected, the correlation is positive. More interestingly, columns 4 and 5 include a linear term to proxy for land distribution (column 4) or a linear and a quadratic term (column 5), the number of drought years and its interaction. The coefficient of the interaction is positive and significant suggesting that both the district's historically determined land distribution as well as the time series of exogenous weather shocks affect the district's program exposure.

We use this source of quasi-exogenous variation in exposure to the ADWDRS program in Panel (a) of Figure 3 to estimate difference-in-differences regressions that compare outcomes across districts with different levels of program exposure before and after the bailout. Thus we have 11 years times 489 districts minus 461 district-year observations for which we lack a time-varing district level control, for a total of 4,918 observations. The fixed effects model that we estimate is as follows:

$$Y_{dt}/Y_{d,t-1} = \alpha + \gamma Exposure + \delta_d + \vartheta_t + \mathbf{X}'_{dt}\zeta + \varepsilon_{dt} \tag{2}$$

where $Y_{dt}/Y_{d,t-1}$ is the year-on-year change in one of our outcomes of interest, such as the number of outstanding loans, total credit or growth in NPLs, δ_d is a district fixed effect, ϑ_t is a year dummy or district time trend and $\mathbf{X}_{dt}$ is a matrix of ime-varying district-specific controls. *Exposure* is an interaction between the share of rural credit written off under the program and a post-program dummy, which is equal to one for all years after 2008 and zero otherwise. Hence, the coefficient γ is our differences-in-differences estimate of program impact. To facilitate the interpretation of the estimate of γ, the variable "bailout share" is normalized to have mean zero and standard deviation one. We estimate the model in (2) by weighted least squares (WLS) because smaller districts may have poorer credit administrative data and might depend on a smaller number of crops becoming more vulnerable to exogenous shocks as a result. We construct the weights using the log of total outstanding credit in the base year 2001 as a proxy for the size of the district economy.[4]. Standard errors are clustered at the district level, the same unit of analysis at which exposure to the program is observed.

The validity of our identification strategy depends on the assumption that average changes in outcomes in the pre- and post-program periods are unrelated to the bailout share in a given district. This assumption would be violated if, for example, loan or credit growth were on a different time trend in districts with greater exposure to the program. While this assumption is fundamentally untestable, we report three different specifications to demonstrate that the identification assumption is unlikely to be violated. At the same time, these different specifications can be thought of as robustness checks that address other possible

[4] See Strahan and Jayaratne [1996] for a similar approach

challenges to our identification strategy.

First, the inclusion of district fixed effects control for time-invariant differences in credit growth and other outcomes of interests that differ due to unobserved factors at the district level. Some examples might include differences in local public expenditure, the distribution of land or differential exposure to price or productivity shocks. The inclusion of district fixed effects also accounts for the possibility of mean reversion in credit and productivity growth. Second, we account for the presence of regional credit cycles. To do this, we first include a set of electoral cycle dummies in all of our specifications. The electoral cycle dummies indicate the number of years until the next scheduled state election and account for the fact that both credit and loan performance have been shown to be strongly correlated with the electoral cycle.

In our second specification, we additionally account for the presence of regional cycles in credit and loan performance that are unrelated to the timing of elections. This approach reduces the likelihood that our estimate γ is biased by a correlation between program exposure and the regional business cycle. We use the Reserve Bank of India's four administrative zones and divide India into four regions accordingly. This specification includes interactions between year effects and regional dummies to allow for variation in regional cycles and uses 44 degrees of freedom.

Finally, we estimate a version of our core empirical model that allows each district to be on a separate linear time trend at a cost of 489 degrees of freedom. This third specification serves as an additional robustness check and reduces the likelihood that the estimated difference-in-differences treatment effect γ is biased by a correlation between program exposure and pre-existing time trends in our key outcomes of interest.

5 Results

Tables III and IV report the main results. The dependent variables in Table III relate to the supply of credit. In columns 1-3 we regress the year on year percentage change in the number of loans while in columns 4-6 we regress the year on year percentage change in outstanding credit on our measure of program exposure according to specification (1), including either year fixed effects (columns 1 and 4), time trends of district level covariates (columns 2 and 5) or individual district time trends (columns 3 and 6). Since the inclusion of district time trends is more conservative, we take this specification as our preferred one. In addition, all regressions control for the deviation of lagged monsoon rainfall from its normal average since agriculture production in India is mostly rainfed and the quality of the monsoon is likely to affect the default rate and subsequent credit growth. The regressions also control for the number of years since the next state election because the credit market behaves differently during election years (Cole [2009b]). All regressions include district fixed effects and are weighted by the district's total credit outstanding in 2001. Each cell in Tables III and IV reports the coefficient of interest γ which corresponds to the differences-in-differences estimate of program impact.

Panel A of Table III reports the program impact on total agricultural credit. In columns 1-3 we find that program exposure leads to no increase in the growth in the number of loans but in columns 4-6 program exposure does lead to an increase in the growth rate of agricultural credit. In particular, an increase of one standard deviation in program exposure leads to an increase in the growth rate of agricultural credit of 3.7% according to the preferred specification of column 6. These results suggest that new credit was given to existing customers, most likely those that did not participate in the loan waiver and that were therefore in good standing. In addition, it appears that banks are now lending more conservatively after the loan waiver, which may suggest that the bailout was large enough to solve the banks' debt overhang problem (Gianetti and Simonov [2013]).

Panel B of Table III runs two types of placebo tests exploiting the timing of the program. Instead of defining the post-program dummy as equal to one for all periods after 2008, we restrict the sample to before 2008 and define the post-program dummy as equal to one either after 2003 or after 2006. In neither case one cannot replicate the pattern of credit growth found in Panel A, lending credibility to the hypothesis that the increase in the growth rate of credit after the program is due to the program itself and not to peculiarities in the data.

Table IV explores the impact of debt relief on post-program loan performance. We rely on unique proprietary data of non performing agricultural loans collected from each states' State Level Banking Committee (SLBC) that are not available for non-agricultural loans. The dependent variable is a dummy that takes value one if there is an increase in non performing assets in a given disrict and year. Columns 1-3 use data for 489 districts while columns 4-6 restrict the sample to 237 districts where competition among banks defined by the number of branches in the district per capita is above the median. Using either sample we find an increase in the probability of positive growth rate of non-performing loans. Panel B reports the result of a placebo test where the post-program dummy is defined to take the value one after 2007, restricting again the sample to 2008. Using this placebo post-program dummy we find no impact on the probability of growth in defaults, suggesting again that the program did cause the increase in defaults. However, this increase in defaults could be due to the higher debt burden given that the loan sizes increased after the program or to moral hazard as borrowers inferred from the waiver that future interventions might be in store, and therefore that the consequences of default were not as severe.

Table VI helps disentangle these two mechanisms by interacting the post-program dummy with a variable that takes on the number of years until the next state election. Panel A reports that defaults rise closer to election years especially after the program, while in Panel B there is no difference before or after the program in the increase in the number of loans due to proximity to elections. We therefore conclude that the increase in defaults documented in Table IV is most likely due to moral hazard, since the expectation of future bailout is heightened in the run-up to elections.

An important aim of economic stimulus programs is to stabilize output, and to prevent distortions in investment and consumption decisions during exceptionally harsh economic circumstances. In the case of debt relief for rural households, it is often argued that extreme levels of indebtedness create "debt overhang" and severe disincentives for investment, so that economic stimulus programs enacted as debt relief hold the promise of improving the productivity of recipient households. The ADWDRS program offers a compelling test of this proposition. To explore the effect of debt relief on agricultural productivity, we take advantage of detailed district level panel on commodity prices and crop yields from the Indian Department of Agriculture. The dataset, which we describe in more detail in Appendix C, contains seasonal information on agricultural revenue and area cultivated so that we can construct time series of agricultural productivity over the time period 2001-2011 for 387 districts in our sample. Table VII uses this variable as the outcome of interest to investigate the impact of the stimulus program on agricultural productivity. The results show that there is no discernible effect of ADWDRS on agricultural productivity. Using our preferred specification, the estimated effect is a precise zero, suggesting that the stimulus did not create investment incentives of a magnitude sufficient to affect agricultural productivity.

Table V explores the effects of the debt relief program on the growth in agricultural credit, non-performing loans and productivity over time. As it turns out, disbursement in agricultural credit grows faster immediately after the program, in 2009 and 2010 but not 2011 (column 1) while defaults raise in all three years of available data after the program, suggesting longer-lasting deleterious effects. Productivity seems to improve in 2010 and 2011 but the point estimates, although precisely estimated, are not large. An increase of one standard deviation in program exposure in 2010 leads to an increase in productivity of 0.8%. Figure 4 complements TableV by plotting the coefficient estimates and the 90% confidence intervals of amount dibursed (Figure 4a) and number of loans (Figure 4b). WHile the point estimates on disbursement are positive and significant in 2009 and 2010, none of the coefficients on the number of accounts are significantly different from zero. This corroborates the fact that banks have improved lending practices by restricting the number of loans and raising average loans among pre-existing borrowers in good standing.

Finally Tables VIII and IX explore the heterogeneity of impacts of the program. Table VIII focuses on heterogeneity in district income. Our criterion for splitting the sample into high and low-income districts is whether districts receive support from the Backward Region Grant Fund Program (BRGF), a federal grant program targeted to the 250 poorest districts in the country. Using our preferred specification in columns 3, 6 and 9 we find that the effects of the program are concentrated in higher income districts, perhaps where the growth opportunities were larger. Table IX focuses instead on the initial level of financial development, proxied by the number of bank branches per capita in a district in the base year 2001. Consistent with the findings of Table VIII, the program had greater impact in districts with higher initial level of financial development.

6 Conclusion

Around the world, governments have routinely intervened in credit markets in an effort to stimulate economic activity. Although it is often hypothesized that such interventions have severe repercussions for credit discipline and borrower expectations, surprisingly little robust evidence exists to evaluate these claims. In this paper, we use a natural experiment surrounding one of the largest borrower bailouts in history to estimate the effect of a large economic stimulus program on productivity and loan repayment.

We find that the program generated no measurable productivity gains, but led to significant moral hazard in loan repayment. The annual post-program increase in the share of non-performing loans grows up to 5 annual percentage points faster in districts in the highest quintile of the debt relief distribution. Importantly, our findings also suggest a mechanism for the amplification of moral hazard in loan repayment arising from economic stimulus enacted through the credit market. We show that the relationship between defaults and the electoral cycle documented by earlier studies is magnified by the bailout program. This suggests that the adverse effects of the bailout become more persistent as the stimulus generates expectations of future politically motivated interventions in the credit market.

Taken together, these results provide some of the first evidence on the moral hazard generated by large government stimulus programs operating through the credit market. While such programs have received much attention in the aftermath of the recent global financial crisis, it is worth noting that programs of this kind, often more frequent and larger in scale, have been carried out in many developing countries. Understanding the moral hazard consequences of these large-scale economic stimulus programs is essential to weighing their costs and benefits, particularly in an environment where weak institutions make such programs susceptible to political capture and manipulation. The results in this paper are a first step in this broader research agenda.

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Tables and Figures

Table I: Program Exposure

The table reports summary statistics for the variable *'Bailout share'*, our main measure of program exposure at the district level. The variable measures the total amount of credit eligible for the bailout as a share of total outstanding agricultural credit at the district level, winsorized at the top percentile. Data on the total amount of debt relief granted is taken from the official figures of the respective State Level Bankers' Committees, reported after the closing of the program in December 2011. Information on the amount of credit eligible for the program was available for 491 districts, located in 22 of India's 28 states and 3 of 7 Union Territorries.The denominator of our measure of program exposure, total outstanding rural credit, is taken from the Reserve Bank of India's *Basic Statistical Returns of Commercial Banks in India*, and measures outstanding credit as of March 31, 2008. To facilitate the interpretation of our estimates, we normalize the variable *'Bailout share'* to have mean zero and standard deviation one in all subsequent tables.

	Bailout share [N=491]
Mean	.326
Median	.284
StDev	.224
Min	.002
Max	.991

Table II: Summary Statistics

This table presents summary statistics for key variables in the dataset, which covers 491 districts in 28 Indian states over the period 2001-2012. Data on all credit related variables is taken from the Reserve Bank of India's *Database on the Indian Economy*. The variable *NPL share* measures the share of non-performing loans and is based on proprietary data on lending and loan performance at India's largest four commercial banks. This data covers lending and loan performance of rural credit at 27,678 bank branches located in all districts included in the dataset. The variables *population*, *rural share*, *ag productivity* and *land share below two hectare* are taken from the *Census of India* and the *India Agriculture Census*. All monetary values are in nominal Indian Rupees. Unless otherwise indicated, all census data refers to the base year 2001. Appendix A provides additional details on the definition of variables.

Panel A: Levels	Obs	Mean	Median	StdDev	Min	Max
Credit variables						
Log(total credit)	6,952	10.66	10.76	1.79	3.71	18.52
Log(total accounts)	6,952	10.92	11.16	1.41	3.93	15.42
Log(ag credit)	6,952	9.20	9.46	1.80	1.75	14.52
Log(ag loans)	6,952	10.10	10.36	1.52	2.08	14.71
Log(personal credit)	5,948	8.90	8.90	1.36	3.73	13.19
Log(personal loans)	5,949	9.59	9.56	1.08	5.16	12.75
Log(total credit 2001)	4,012	5.34	5.72	1.88	3.52	16.27
NPL share	4,058	0.74	0.05	0.08	0	.97
District characteristics						
Log population	7,553	14.00	14.21	1.02	10.35	16.08
Log(1+rural share)	7,553	0.57	0.60	0.11	0	.69
Log (1+ag productivity 2001)	7,033	10.24	10.23	0.58	7.98	11.92
Land holdings below 2ha	6,669	0.53	0.56	0.24	0	.99
Rainfall, % of normal	6,094	97.05	88.46	50.71	8.36	1061.99
Time to election, in years	7,540	2.01	2.00	1.41	0	4

Panel B: Changes	Obs	Mean	Median	StdDev	Min	Max
Credit variables						
Total credit, amount	5,397	1.23	1.20	0.26	0.63	2.66
Total credit, accounts	5,397	1.06	1.06	0.14	0.66	1.56
Agricultural credit, amount	5,397	1.25	1.22	0.27	0.62	2.48
Agricultural credit, accounts	5,397	1.08	1.08	0.19	0.58	1.77
Personal credit, amount	5,384	1.27	1.23	0.30	0.59	2.66
Personal credit, accounts	5,386	1.09	1.07	0.23	0.58	2.14
NPL share	3,476	0.00	0.00	0.07	-0.93	0.64
NPL share, 1 if $\Delta > 0$	3,476	0.51	1.00	0.50	0.00	1.00

Table III: Credit Supply – Intensive and Extensive Margin Effects

This table reports estimates of the impact of debt relief on post-program credit supply. Within a panel, each column represents results from a separate regression. The dependent variable in columns [1] to [3] is the year-on-year percentage change in the number of agricultural loans outstanding. The dependent variable in columns [4] to [6] is the percentage change in the amount of agricultural credit outstanding. Panel A reports reduced form difference-in-difference effects. Panel B restricts the sample to the pre-program years 2001 to 2008 and estimates placebo treatment effects for the years 2003 and 2006. All regressions are weighted by total district level credit outstanding in the base year 2001. 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 and a full set of electoral cycle dummies. Robust standard errors, in brackets, are clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of interest						
	%Δ *Accounts*			%Δ *Amount*		
Bailout_share*post	0.008	0.004	0.009	0.022***	0.031***	0.037**
	[0.005]	[0.005]	[0.012]	[0.007]	[0.007]	[0.019]
Observations	4,918	4,918	4,918	4,918	4,918	4,918
# clusters (districts)	489	489	489	489	489	489
R-squared	0.182	0.285	0.107	0.107	0.141	0.081
Panel B: Placebo, program timing						
Bailout_share*placebo$_{2003}$	0.001	-0.004	0.015	-0.006	-0.008	-0.02
	[0.007]	[0.007]	[0.017]	[0.005]	[0.005]	[0.013]
Bailout_share*placebo$_{2006}$	-0.009	-0.01	-0.012	0.007	0.011	0.016
	[0.011]	[0.011]	[0.018]	[0.006]	[0.007]	[0.011]
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	–	–	Yes	–	–
Year*region fixed effects	–	Yes	–	–	Yes	–
District time trends	–	–	Yes	–	–	Yes

Table IV: Moral Hazard: Impact on Loan Performance

This table explores the impact of debt relief on post-program loan performance. The dependent variable in all regression is a dummy equal to one for all years in which there an increase in the share of non-performing loans was recorded in a given district. Columns [1] to [3] report difference-in-difference estimates for all districts in the sample. Columns [4] to [6] report coefficient estimates for districts with greater than average bank competition, defined as districts in which the number of bank branches per capita in the base year 2001 is greater than the sample mean. Data on loan performance are taken from a proprietary dataset on loans and loan performance at India's four largest public sector banks. The dataset covers loan performance at approximately 27,678 bank branches in 491 districts across 24 Indian states over the years 2006-2012. All regressions control for the deviation of lagged monsoon rainfall from its long run average and years until the next state election. Robust standard errors, in brackets, are clustered by district. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of interest			**1** if Δ NPA share > 0			
	All districts			*High bank competition sample*		
Bailout_share*post	0.074***	0.088***	0.080*	0.092***	0.075**	0.240***
	[0.021]	[0.022]	[0.048]	[0.033]	[0.033]	[0.072]
Observations	2,676	2,676	2,676	1,402	1,402	1,402
# clusters (districts)	489	489	489	237	237	237
R-squared	0.243	0.276	0.297	0.214	0.253	0.305
Panel B: Placebo						
Bailout_share*placebo$_{2007}$	-0.024	-0.027	-0.024	0.024	0.034	0.024
	[0.036]	[0.038]	[0.036]	[0.055]	[0.060]	[0.055]
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	–	–	Yes	–	–
Year*region fixed effects	–	Yes	–	–	Yes	–
District time trends	–	–	Yes	–	–	Yes

Table V: Credit Supply and Loan Performance: Effects over Time

This table explores the impact of debt relief over time. The dependent variable in columns [1] and [2] are the change in agricultural credit and the change in the number of agricultural loans outstanding. The dependent variable in colun [3] is the share of non-performing l loans, as previously defined. The dependent variable in column [4] is agricultural productivity, measured as the log revenue per hectare of all agricultural output in a district-year. All regressions control for the deviation of lagged monsoon rainfall from its long run average and a full set of electoral cycle dummies. Robust standard errors, in brackets, are clustered by district. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)
Dependent variable	%Δ Ag credit		%Δ NPL	Productivity
	Amount	Loans	1 if >0	
$\hat{\gamma}_{t+1}$	0.061***	-0.007	0.087***	0.001
	[0.02]	[0.01]	[0.03]	[0.00]
$\hat{\gamma}_{t+2}$	0.031**	-0.003	0.062**	0.008*
	[0.01]	[0.01]	[0.03]	[0.00]
$\hat{\gamma}_{t+3}$	0.002	0.021**	0.104***	0.005*
	[0.01]	[0.01]	[0.03]	[0.00]
Observations	4,918	4,926	2,682	4,241
#clusters (districts)	489	489	237	439
R-squared	0.142	0.287	0.28	0.181
Year fixed effects	Yes	Yes	Yes	Yes
Year*region fixed effects	Yes	Yes	Yes	Yes
District time trends	–	–	–	–

Table VI: Moral Hazard: Expectation of Future Bailouts

This table explores the interaction between post-program loan performance and the electoral cycle. The dependent variable in all regressions is a dummy variable equal to one for district-year observations in which an increase in the share of non-performing loans was recorded. Difference-in-difference estimates are obtained from a regression of this loan performance variable on the interaction between the number of years to the next state election and a dummy variable equal to one for all post-program observations and a set of time-varying controls. Columns [1] to [3] report estimates for the entire sample, columns [4] to [6] report estimates for a 'high bank competition' subsample, which includes only districts with a number of bank branches per capita greater than the sample mean in the base year 2001. Data on loan performance are taken from a proprietary dataset on loans and loan performance at India's four largest banks. The dataset covers loan performance at approximately 27,678 bank branches in 491 districts for the years 2006-2012. In addition to the fixed effects listed in the table, regressions control for the deviation of lagged monsoon rainfall from its long run average and the number of years until the next state election. Robust standard errors, in brackets, are clustered by district. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Loan performance	$\mathbf{1}$ if Δ *NPA share* > 0					
	All districts			*High bank competition sample*		
Years to election*post	-0.011	-0.035**	-0.154***	-0.023	-0.051**	-0.120***
	[0.014]	[0.017]	[0.012]	[0.021]	[0.026]	[0.017]
Observations	2,913	2,913	2,913	1,506	1,506	1,506
# clusters (districts)	508	508	508	237	237	237
R-squared	0.234	0.273	0.344	0.208	0.257	0.324
Panel B: Loan size	*Log loan size (district mean)*					
Years to election*post	-0.002	-0.002	-0.006	0.001	0.006	-0.006
	[0.006]	[0.008]	[0.005]	[0.008]	[0.011]	[0.007]
Observations	2,406	2,406	2,406	1,244	1,244	1,244
# clusters (districts)	503	503	503	236	236	236
R-squared	0.444	0.471	0.736	0.485	0.526	0.752
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	–	–	Yes	–	–
Year*region fixed effects	–	Yes	–	–	Yes	–
District time trends	–	–	Yes	–	–	Yes

Table VII: Impact on Productivity

This table explores the impact of debt relief on agricultural productivity. The dependent variable is the log per hectare revenue from the sale of agricultural commodities at the district level. Columns [1] to [3] present unweighted difference-in-difference estimates. Estimates in columns [4] to [6] are weighted by each district's rural population share in the base year 2001. Data on per hectare crop yields are taken from the Indian Department of Agriculture's database on crop tields and revenue is calculated using average commodity prices for the year 2001. Additional details on the calculation of the productivity series are reported in Appendix C. Robust standard errors, in brackets, are clustered by district. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log revenue per hectare					
Bailout_share*post	0.004	0.004	0.001	0.003	0.004*	0.001
	[0.002]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
Observations	4,241	4,241	4,241	4,182	4,182	4,182
#clusters (districts)	488	488	488	488	488	488
R-squared	0.098	0.18	0.411	0.105	0.187	0.396
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	–	–	Yes	–	–
Year*region fixed effects	–	Yes	–	–	Yes	–
District time trends	–	–	Yes	–	–	Yes
Weighted	No	No	No	Yes	Yes	Yes

Table VIII: Heterogeneous Effects: District Income in 2001

The table reports heterogeneous effects by district level income. Districts are classified as 'low-income' if the received financial support from the Backward Regions Grant Fund Program (BRGF), a central government program that provides support for development projects in the poorest 250 districts of India. Panel A presents estimates for low-income districts that receive BRGF funding, panel B presents estimates for high-income districts. Within a panel, each column represents results from a separate regression. The dependent variable in columns [1] to [3] is the year-on-year change in total credit, and the dependent variable in columns [4] to [6] is the change in outstanding loans. Estimates in columns [1] to [6] are based on specifications identical to those in Table V. The dependent variable in columns [7] to [9] is the year-on-year change in non-performing loans and estimates are based on specifications identical to those in Table VI. All regressions are weighted by the district's total credit outstanding in 2001 and control for the deviation of lagged monsoon rainfall from its long run average, and years until the next state election. Robust standard errors, in brackets, are clustered at the district level. * $p<0.10$ ** $p<0.05$ *** $p<0.01$.

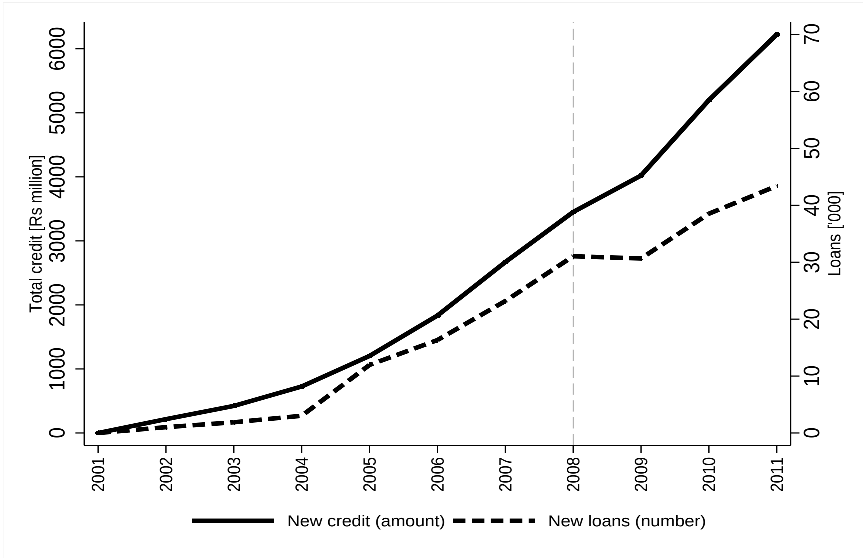
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$\%\Delta$ *Loans*					$\%\Delta$ *Amount*		$\%\Delta$ *NPL*	
Panel A: Low-Income Districts									
Bailout_share*post	0.016**	0.017**	0.042**	0.047***	0.052***	-0.024	0.133***	0.144***	-0.039
	[0.008]	[0.008]	[0.017]	[0.011]	[0.011]	[0.029]	[0.037]	[0.036]	[0.078]
Observations	2,162	2,162	2,162	2,162	2,162	2,162	1,140	1,140	1,140
R-squared	0.231	0.333	0.112	0.202	0.25	0.083	0.324	0.363	0.332
Panel B: High-Income Districts									
Bailout_share*post	0.006	-0.002	0.037**	0.020**	0.022**	0.070***	0.051*	0.045	0.139**
	[0.006]	[0.007]	[0.017]	[0.008]	[0.010]	[0.026]	[0.027]	[0.030]	[0.064]
Observations	2,756	2,756	2,756	2,756	2,756	2,756	1,536	1,536	1,536
R-squared	0.161	0.279	0.115	0.081	0.111	0.087	0.213	0.24	0.282
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	–	–	Yes	–	–	Yes	–	–
Year*region effects	–	Yes	–	–	Yes	–	–	Yes	–
District time trends	–	–	Yes	–	–	Yes	–	–	Yes

Table IX: Heterogeneous Effects: Initial Financial Development [Bank Branches per Capita in 2001]

The table reports heterogeneous effects by a district's initial level of financial development. We proxy financial development by the number of bank branches per capita in the base year 2001 and split the sample into districts with high and low levels of initial financial development based on whether they are above or below the median number of bank branches per capita in the base year. Panel A reports results for the subsample of districts with low levels of initial financial development, Panel B presents estimates for the subsample of districts with high levels of financial development. The dependent variable in columns [1] to [3] is the year-on-year change in total credit, and the dependent variable in columns [4] to [6] is the change in outstanding loans. Estimates in columns [1] to [6] are based on specifications identical to those in Table V. The dependent variable in columns [7] to [9] is the year-on-year change in non-performing loans and estimates are based on specifications identical to those in Table VI. All regressions are weighted by the district's total credit outstanding in 2001 and control for the deviation of lagged monsoon rainfall from its long run average, and years until the next state election. Robust standard errors, in brackets, are clustered at the district level. * p<0.10 ** p<0.05 *** p<0.01.

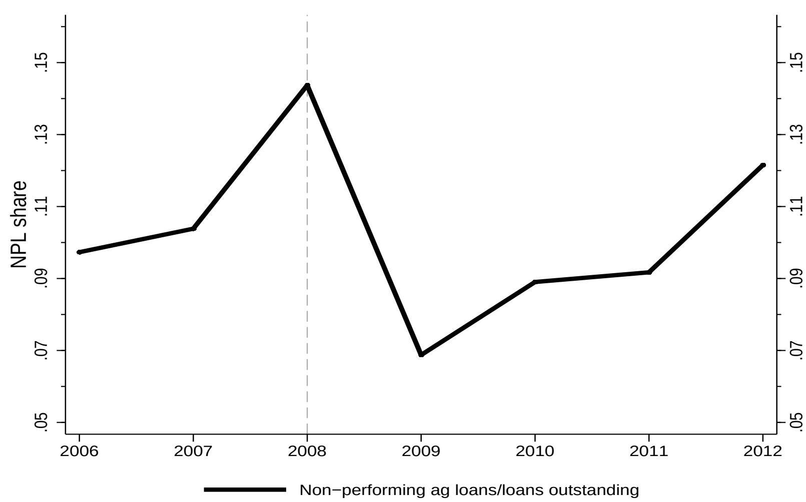
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	%Δ *Total credit* [all categories]			%Δ *Loans* [all categories]			%Δ *NPL*		
Panel A: Low Financial Development									
Bailout_share*post	0.01	0.009	-0.027**	0.033***	0.040***	-0.003	0.090***	0.099***	-0.041
	[0.006]	[0.006]	[0.011]	[0.009]	[0.009]	[0.021]	[0.029]	[0.029]	[0.057]
Observations	2,398	2,398	2,398	2,398	2,398	2,398	1,274	1,274	1,274
R-squared	0.206	0.316	0.109	0.168	0.211	0.09	0.303	0.338	0.3
Panel B: High Financial Development									
Bailout_share*post	0.008	0.002	0.059**	0.021**	0.025**	0.094***	0.092***	0.075**	0.240***
	[0.008]	[0.008]	[0.024]	[0.010]	[0.011]	[0.031]	[0.033]	[0.033]	[0.072]
Observations	2,520	2,520	2,520	2,520	2,520	2,520	1,402	1,402	1,402
R-squared	0.184	0.298	0.118	0.09	0.131	0.084	0.214	0.253	0.305
District fixed effects	Yes	–	–	Yes	–	–	Yes	–	–
Group time trends	–	Yes	–	–	Yes	–	–	Yes	–
District time trends	–	–	Yes	–	–	Yes	–	–	Yes

Figure 1: Pre and Post-Program Credit Growth – Raw Data



Notes: The figure shows the growth of (i) total agricultural credit outstanding and (ii) the number of agricultural loans outstanding (averages) over the sample period, relative to the base year 2001, for all districts in the sample.

Figure 2: Pre and Post-Program Loan Performance – Raw Data

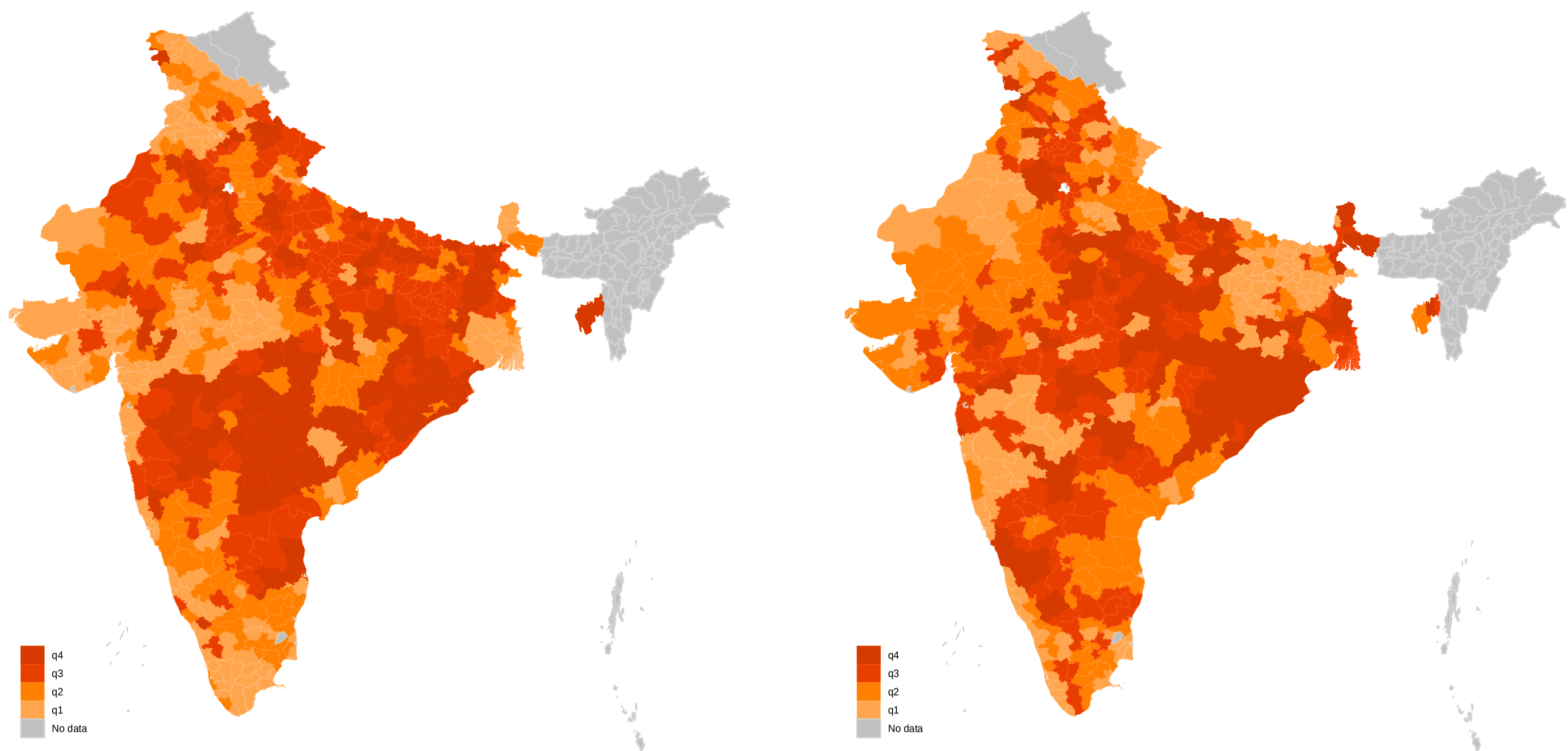


Notes: The figure shows the share of non-performing agricultural loans. Data are taken from the loan performance dataset, describes in section 3 and covers the performance of agricultural loans at all branches of India's four larges commercial banks.

Figure 3: Program Exposure and Ex-Post Loan Performance

(a) Debt relief [as share of outstanding credit]

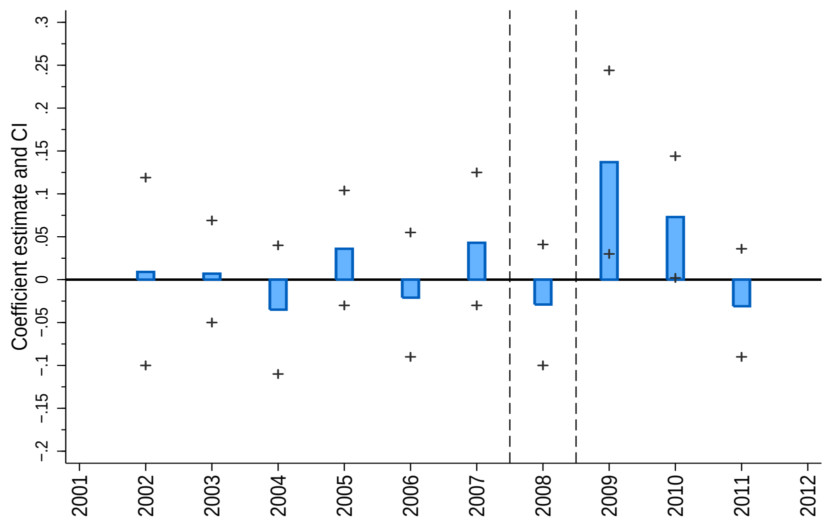
(b) Δ NPL share 2009-2012



Notes: The figure plots the share of rural credit written off under the debt relief program in Panel (a) against increases in non-performing loans over the post-program period 2009-2011 in Panel (b). The share of credit written off under the program is identical to the treatment variable described in Table 1. Increases in non-performing loans is the arithmetic mean of the annual percentage change in the share of non-performing loans $\Delta y_t = (y_t - y_{t-1})/y_{t-1}$ for the years 2009-2011. Loan performance is calculated from a proprietary panel data on rural credit and loan performane of india's four largest commercial banks covering 27,678 branches in all districts in the dataset. To make the two panels comparable, Panel (b) excludes 42 districts for which data on debt relief amounts were not available.

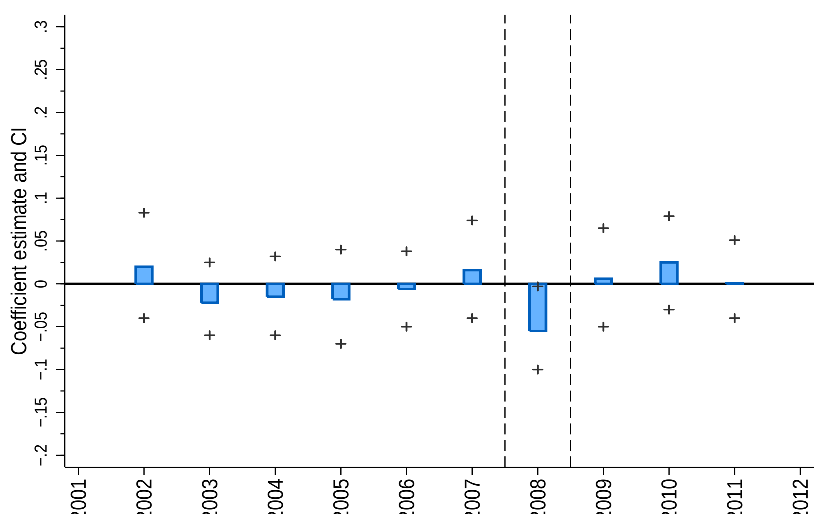
Figure 4: Intensive and Extensive Margin Dynamics

(a) Intensive Margin DD Coefficients



Notes: The figure shows coefficient estimates (and 90% confidence intervals) measuring the effect of *Bailout_share* on the amount of agricultural credit outstanding. Each coefficient estimate is obtained from a regression of the year-on-year change in total agricultural credit on a set of year dummies and their interactions with *Bailout_share*. Additional controls include district fixed effects, lagged rainfall as a percentage of the district's long term, a full set of state fixed effects and their interaction with year dummies. Confidence intervals are calculated based on standard errors clustered at the district level. The omitted category is the base year 2001. Dashed vertical lines indicate the timing of the ADWDRS program.

(b) Extensive Margin DD Coefficients



Notes: The figure shows coefficient estimates (and 90% confidence intervals) for the effect of *Bailout_share* on the total number of agricultural loans outstanding. Each coefficient estimate is obtained from a regression of the year-on-year change in the number of agricultural loans outstanding on a set of year dummies and their interactions with *Bailout_share*. Additional controls include district fixed effects, lagged rainfall as a percentage of the district's long term, state fixed effects and their interaction with year dummies. Confidence intervals are based on standard errors clustered at the district level. The year 2001 is the omitted category.

Appendix

Table A.I: Predicting Program Exposure

This table reports estimates from cross-sectional regressions of program exposure on measures of the land distribution and the time series of weather shocks at the distric level. The dependent variable in all columns is the amount of debt relief as a share of total outstanding agricultural credit at the time of the program. All regressions control for state fixed effects. Standard errors are given in brackets and calculated using the Huber-White correction for heteroskedasticity. * p<0.10 ** p<0.05 *** p<0.01.

	(1)	(2)	(3)	(4)	(5)
			Bailout share		
Share landholdings $\leq$ 2 ha	-0.235***	-0.366		-0.366***	-0.571**
	[0.086]	[0.235]		[0.099]	[0.249]
Share landholdings $>$ 2 ha		0.138			0.213
		[0.227]			[0.228]
Drought years$_{2001-2007}$			0.029***	-0.044**	-0.045**
			[0.008]	[0.020]	[0.020]
Drought years*Landholdings $\leq$ 2 ha				0.131***	0.133***
				[0.032]	[0.032]
Observations	4,607	4,607	5,056	4,607	4,607
Districts	491	491	491	491	491
R-squared	0.499	0.499	0.487	0.502	0.502

A Data Appendix

Table A.II: Description of Variables

Variable	*Description*	*Source*
Total credit	The natural logarithm (plus 1) of outstanding credit in units of Rs 100,000 for each district of India. Includes all loans given by private, public, cooperative and regional rural banks. Adjusted to account for loans below Rs 200,000 using district level loan size distributions reported in *India Agricultural Census Input Survey* 2001.	Reserve Bank of India, *Basic Statistical Returns of Scheduled Commercial Banks in India*, (2001 -2012)
Total credit, agriculture	The natural logarithm (plus 1) of total agricultural credit in units of Rs 100,000 for each district of India. This includes direct agricultural finance and financing for activities related to agriculture and covers all loans given by private, public, cooperative and regional rural banks. Adjusted to account for loans below Rs 200,000 using district level loan size distributions reported in *India Agricultural Census Input Survey* 2001.	Reserve Bank of India, *Basic Statistical Returns of Scheduled Commercial Banks in India*, (2001-2012)
Total personal credit	The natural logarithm (plus 1) of total personal credit. This includes all credit given by private, public, cooperative and regional rural banks. It excludes loans given for the purchase of durables and is adjusted for loans below Rs 200,000 using district level loan size distributions reported in *India Agriculture Census, Input Survey* 2001.	Reserve Bank of India, *Basic Statistical Returns of Scheduled Commercial Banks in India*, (2001-2012)
Non-performing loans	This information is not publicly available. We have calculated it from proprietary data obtained from four of India's largest five banks. The data consists of annual information on the amount of outstanding rural credit and the amount of rural NPAs (both denominated in units of Rs 100,000). 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).	Proprietary bank data.
Rainfall	Log deviation of monsoon rainfall (in mm) from its 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]
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.
Total population, 2001 Rural population, 2001	Total adult population and rural population of each district between the ages of 15-65 years in 2001.	Census of India, *District Tables* (2001)
Total agricultural productivity	Per hectar value of output of 32 standard crops in units of Rs 1,000 at the district level.	India Agriculture Census, *District Tables* (2001)
Land distribution	Share of landholdings in the district less than two hectares (approximately 5 acres) in size.	India Agriculture Census, *District Tables* (2001)
Drought Affected District	Dummy variable equal to one if a district was classified as 'drought affected' at the time that the debt relief program came into effect. The program had a higher minimum disbursement for qualifying households in these districts.	Reserve Bank of India
Low income district	Dummy variable equal to one if a district receives subsidies from the *Backward Regions Grant Fund* (BRGF) program.	India Ministry of Statistics and Program Implementation [MOSPI]

B Program Exposure by State

This section reports the share of total credit waived at the state level ordered by program exposure. *Bailout share* denotes the share of total agricultural credit outstanding waived under the program and includes both unconditional debt relief for households below the 2 hectare program threshold and conditional debt relief for households above the program cutoff. The reported state level figures are the average figure for all districts in each state included in our dataset.

Rank	State	Bailout share	Rank	State	Bailout share
1.	Orissa	60.2%	12.	Sikkim	23.0%
2.	Maharashtra	50.0%	13.	Himachal Pradesh	22.7%
3.	Jharkhand	41.3%	14.	Chhattisgarh	22.0%
4.	Tripura	39.4%	15.	Rajasthan	21.4%
5.	Andhra Pradesh	38.5%	16.	Karnataka	16.8%
6.	Bihar	37.7%	17.	West Bengal	16.4%
7.	Uttar Pradesh	31.8%	18.	Jammu & Kashmir	13.8%
8.	Uttaranchal	31.7%	19.	Tamil Nadu	12.2%
9.	Haryana	31.1%	20.	Gujarat	11.1%
10.	Madhya Pradesh	25.9%	21.	Punjab	9.2%
11.	Kerala	23.0%	22.	Goa	2.2%

C District-Level Productivity

This section describes the construction of the productivity time series for each district, used in Table VII. To construct this variable, we use data on crop yields from the *Indian Department of Agriculture* (available at `http://apy.dacnet.nic.in/`). 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. The for each year and crop we aggregate total area planted and crop output for both crop seasons (Kharif and Rabi). To measure productivity in monetary terms, we obtained commodity prices for the base year 2001 from *Indiastat* (*District-wise Farm Harvest Prices of Principal Crops in India* available at `http://www.indiastat.com/agriculture/`) for all crops in the dataset as of January 2001. Letting a^c_{dy} denote the area planted with crop $c \in \{1...C\}$ in district d and year y, letting r^c_{dy} denote the total production of crop c in district d and year y, and letting p^c_{01} denote the price of commodity c in the base year 2001, we calculate productivity per hectare in district d and year y as $Productivity_{dy} = \frac{\sum_{c=1}^{C}\{r^c_{dy} \cdot p^c_{01}\}}{\sum_{c=1}^{C} a^c_{dy}}$.