MEASURING THE EQUILIBRIUM IMPACTS OF CREDIT: EVIDENCE FROM THE INDIAN MICROFINANCE CRISIS

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Abstract. In October 2010, the state government of Andhra Pradesh, India issued an emergency ordinance, bringing microfinance activities in the state to a complete halt and causing a nation-wide shock to the liquidity of lenders, especially those lenders with loans in the affected state. We use this massive dislocation in the microfinance market to identify the causal impacts of a reduction in credit supply on consumption, entrepreneurship, and employment. Using a proprietary, hand-collected district-level data set from 27 microlenders matched with household data from the National Sample Survey, we find that district-level reductions in credit supply are associated with significant decreases in casual daily wages, household wage earnings and consumption. In contrast to many experimental studies of microfinance, our estimates capture the average impacts on households, inclusive of general equilibrium effects. Moreover, we find significant heterogeneity by household landholdings, consistent with a model in which medium-wealth households scale back their businesses and landless households are hit by a fall in the wage.

1. Introduction

Microfinance is an important tool for financial inclusion across the developing world. According to the IFC, over the past 15 years, approximately 130 million individuals have borrowed from microfinance institutions (MFIs). In 2006, Grameen Bank and its founder, Muhammed Yunus, were awarded the Nobel Peace Prize.

While microfinance was initially heralded as a silver bullet in fighting poverty, a recent wave of experimental research has brought discipline to the debate about microfinance's impacts. Angelucci et al. (2015), Augsburg et al. (2015), Attanasio et al. (2015), Banerjee et al. (2015b), Crépon et al.

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(2015), and Tarozzi et al. (2015) all find strikingly similar results in a diverse set of countries and settings. This body of short- to medium-run evidence paints a consistent picture of moderate impacts. Access to microfinance is generally found to cause modest business creation and business expansion, but the evidence on growth in revenues and profits is more mixed. There is some evidence that borrowers do purchase more household durables and business assets, but almost no evidence of an impact on business profits, or on non-durable consumption or other indicators of welfare such as health, education, or women's empowerment.

While randomized controlled trials (RCTs) provide unbiased estimates of causal effects with minimal assumptions, they are not without limitations. First, RCTs are only able to measure impacts for the group of individuals that was induced to take a loan because of the experiment. In many (though not all) research designs, these "complier" individuals are the marginal rather than the average borrowers. Further, RCTs are extremely well suited to measure partial equilibrium effects but often have a much more difficult time achieving the scale required to affect general equilibrium outcomes. Buera et al. (2014) were the first to simulate a model to highlight that when scaled economy-wide, the general equilibrium effects of microfinance may look quite different from the partial equilibrium effects measured in RCTs.

In this paper, we use variation from a natural experiment to estimate the general equilibrium impacts of a withdrawal of microfinance on the average rural household. In October 2010, the state government of Andhra Pradesh, India issued an emergency ordinance, bringing microfinance activities in the state to a complete halt and causing a nation-wide shock to the liquidity of lenders. According to data from the Microfinance Information Exchange (MIX), the aggregate gross loan portfolio of Indian microlenders fell by approximately 20% between fiscal year 2010 and fiscal year 2011. Panel A of Figure 1 plots India-wide levels of microlending from 2008 to 2013. The drop in lending post 2010 is visible in the figure.

With the help of the largest trade association of for-profit microlenders in India, the Microfinance Institutions Network (MFIN), we hand-collected proprietary district-level data from 27 microlenders detailing their loan portfolios from 2008 through 2013. We combine this data with household-level data from the National Sample Survey (NSS) rounds 64, 66, and 68 (2008, 2010, and 2012, respectively) to create a district-level panel. The NSS data gives detailed information about employment, wages, earnings, consumption, and self-employment activities.

We identify the causal impacts of microfinance by using variation in the balance sheet exposure of each lender to loans in the affected state, Andhra Pradesh, before the crisis, interacted with pre-crisis variation in the geographical footprint of each lender. We show that districts that borrowed more from lenders with portfolio exposure to Andhra Pradesh witnessed much larger declines in lending between 2010 and 2012 than similar districts with the same amount of overall pre-crisis lending whose lenders did not have balance sheet exposure to Andhra Pradesh. Panel B of Figure 1 plots the trends in district-level GLP separately for districts with high and low indirect exposure to Andhra Pradesh. Note that low exposure districts experience no absolute decrease in credit, while high exposure districts experience a large contraction following the crisis of 2010.[1] We use this massive, differential dislocation in the microfinance market as a source of quasi-exogenous variation to study the effects of district-level reductions in credit supply on consumption, entrepreneurship, wages, and employment. Our empirical strategy only considers districts outside of Andhra Pradesh, which were not directly affected by the ordinance. Thus, this natural experiment is a unique opportunity to study a large supply shock to the microfinance sector in a setting where there was no concurrent demand shock.

Given that the credit supply shocks during the crisis operate at the district level, it is important to consider the potential general equilibrium consequences in addition to the standard set of partial equilibrium outcomes. In order to develop empirical predictions for this setting, we present a simple model of households' wage employment, self-employment and credit constraints in general equilibrium. We consider an environment where households have access to a self-employment opportunity that requires capital and labor; households can also supply their labor to the casual labor market. Importantly, we assume that there exist credit market frictions that limit some households from reaching the optimal business scale. Namely, households can borrow up to a fixed fraction of their wealth to finance production. Thus, households borrow to operate a business and decide how much net labor to supply or demand from the outside market. Households vary in their

[1]Given that the crisis happened at the end of 2010, one might wonder why the effects of the crisis are most visible in 2012 rather than 2011. This is explained by the fact that most microloans have a maturity of one year. The bulk of the drop in credit came from MFIs delaying the issuance of new loans upon the maturation of existing loans. This means that we only observe changes in district microfinance levels with a 6-12 month delay.

wealth endowments so that some households choose to be net labor suppliers, while other are net demanders. The market wage is set such that net labor supply is equated to net labor demand.

We explore the comparative statics in this model when credit constraints tighten at the district level. One key prediction of the model is that district-level wages fall when credit contracts. This is a product of two forces. First, labor demand falls as households with low to intermediate levels of wealth scale back their businesses and decrease their net demand for market labor. Second, labor supply increases as the own-business labor use of net suppliers falls, and some households switch from net demanders to net suppliers of labor.

The model also predicts heterogeneous and sometimes non-monotonic effects of the crisis on households across the wealth distribution. In this model, net market labor supply is monotonically decreasing in wealth. Therefore, the impacts of the decrease in wages on labor market earnings are felt most acutely by the poorest households. In contrast, households with intermediate levels of wealth experience the largest declines in earnings from self-employment income as, for them, the credit contraction results in the largest reduction in business scale. The richest households remain unconstrained even after the reduction in credit and benefit from the decrease in the wage. These heterogeneous patterns suggest U-shaped relationships between wealth and treatment effects on both business outcomes and non-durable consumption.

We find that the reduced form impacts of the reduction in microcredit largely match the predictions of our simple general equilibrium model. First, we do indeed find a decrease in the average casual daily wage for the most exposed districts between 2010 and 2012 relative to districts with the same amount of lending, but from less-exposed MFIs. We also find that the average household experiences statistically significant reductions in both non-durable and durable consumption. At least part of this decrease in consumption can be attributed to decreases in household labor market earnings.

To test the distributional predictions of the model, we explore heterogeneity in impacts by land holdings. As predicted, the effects on labor market earnings are most pronounced for the least landed quintile of the within-district land distribution and decline as land holdings increase. Moreover, we find U-shaped patterns of effects on both non-durable and durable consumption across the land holdings distribution. Further, we examine effects on household businesses heterogeneously by the number of employees and find that the largest consumption impacts accrue to households with businesses employing fewer than six workers, while there are no detectable effects for the businesses with a larger payroll.

As a further test, we examine whether effects are stronger during times of peak labor demand, and show that they are. This suggests that in the presence of nominal wage rigidity (Kaur, 2015), the effect of reduced access to credit plays out via stagnant wages during peak periods when they would otherwise rise.

We also examine alternative data sources to verify that the effects of the credit drop are not an artifact of the 64th-68th rounds of the NSS. Using crop production data from data from the Indian Ministry of Agriculture, we show that the reduction in credit has negative impacts on agricultural output. We also use the NSS 70th round "Debt and Investment" survey to obtain a measure of household's total credit portfolios. This data source implies a first stage fall in MFI credit access due to exposure to the crisis that is strikingly similar to the estimate obtained from the MFIN balance sheet data.

Our findings are robust to alternative specifications. First, we find that our exposure measure is not simply proxying for distance to Andhra Pradesh. Results are unchanged dropping border districts or including time varying controls for distance to the affected state. We also control for time-varying effects of party affiliation, as states aligned with the same party as Andhra Pradesh could be on similar trends and have more districts exposed to the crisis; again, results are unchanged. Moreover, as a test of the parallel trends assumption we conduct placebo regressions comparing high vs. low exposure districts between 2008 and 2010, *before* the crisis. This exercise does not show evidence of (spurious) effects, offering further support for the identifying assumptions behind our research design.

Our paper is related to several distinct literatures in economics and finance. First, we contribute to the active debate on the impacts of microfinance (as summarized in Banerjee et al. (2015d)) and provide novel estimates of impacts on the average borrower in general equilibrium. These impacts are important for policy-makers when deciding how much to subsidize or regulate the microfinance sector and when designing financial inclusion strategies. The results paint a different, but complementary, picture of microfinance compared with the RCT literature. Namely, changes to the availability of credit at the district level have important distributional consequences for non-borrowers through the equilibrium wage in the rural labor market, an effect which most RCTs do not (and do not intend to) measure.

The paper is also related to the literature on financial access for the poor, especially Burgess and Pande (2005), who show evidence that bank expansions increase welfare for rural districts.[2] Our findings are broadly consistent with theirs and show that general equilibrium effects, including effects on on non-borrowers, may explain a sizable share of the poverty-alleviation effect of financial access.

Third, this paper is related to the large literature in macroeconomics and finance studying the effects of credit supply shocks and bank balance sheet effects. Many papers have shown that in diverse settings, negative shocks to bank liquidity are often passed on to borrowers through reductions in lending (Paravisini (2008), Khwaja and Mian (2008), Iyer et al. (2013), and Schnabl (2012)). More often then not, decreases in lending activity are not fully offset by the credit market. A smaller literature including Chodorow-Reich (2014), Jiménez et al. (2014), Greenstone et al. (2014), Ashcraft (2005), and Peek and Rosengren (2000) traces out effects (or lack thereof) of such credit supply shocks on real activity. In the context of India, Giné and Kanz (2015) study the real effects of a large scale borrower bailout.

Finally, our paper is related to several recent papers which examine general equilibrium effects of large-scale public programs in developing countries. Imbert and Papp (2015) find that the NREGA workfare program increases local wages. Muralidharan et al. (forthcoming) demonstrate a wage effect of biometric smartcards stemming from improved implementation of NREGA. Khanna (2015) shows that a large-scale school expansion program in India reduced skill premia by increasing supply. Related, Jayachandran (2006) shows that the impact of negative rainfall shocks in rural India is magnified by a fall in the wage caused by increased labor supply.

Most closely related is Buera et al. (2014), who investigate theoretically the distributional effects of microfinance access, and show that there may potentially be large equilibrium wage effects. Our paper confirms empirically the existence of large wage effects. However, in their model, general equilibrium effects on output and consumption are smaller than in partial equilibrium because

[2] Other papers investigating the effects of financial development on growth include Dehejia and Lleras-Muney (2007), Fulford (2009) and Young (2015).

low-TFP potential entrepreneurs are drawn into operation by having access to microfinance. Our findings, in comparison to partial equilibrium RCT evidence, suggest the opposite: that GE effects are *larger* than PE, because the largest direct impacts of credit are on high TFP entrepreneurs.[3] In a related, earlier paper, Ahlin and Jiang (2008) also show theoretically that microfinance can affect the wage and can have large impacts for low-wealth households while potentially reducing incomes for high-wealth households via the wage effect; in their framework the long-term effects on output per capita are ambiguous as microfinance discourages low-TFP subsistence but also reduces hiring by high-TFP net labor demanders. Given these theoretically ambiguous predictions, empirical evidence is needed.

Our paper proceeds, in section 2, with a model exploring the effects of a credit shock on the investment of SMEs and the effects on labor demand and supply. Section 3 discusses the setting and data. We describe our empirical strategy in Section 4. Section 5 presents our results, and Section 6 discusses the results in relation to the RCT literature and discusses the breakdown between PE and GE. Section 7 concludes.

2. Model

Before turning to the empirical strategy and results, we first present a simple static general equilibrium model of the rural economy. We model the AP crisis as a tightening of credit constraints faced by households and generate empirical predictions by exploring the comparative statics resulting from the solution of each household's problem and the equilibration of labor demand and labor supply. Our model, which considers occupational choice (here captured by whether a household is a net supplier or demander of labor) in the presence of credit constraints, relates to a number of papers, particularly Banerjee and Newman (1993) and Buera et al. (2011). Our focus is to examine the implications of changes to credit constraints for factor prices, namely the wage.[4]

2.1. **Model Environment.** Our goal is to capture the equilibrium effects of changes to aggregate credit supply on rural household outcomes including wages, labor hours, total labor market earnings, and business profits. We start by assuming that households each have access to a decreasing returns

[3]Using an RCT design, Banerjee et al. (2015a) directly confirm that it is high-productivity businesses who respond most to increased credit access by expanding their scale, including increasing employment.

[4]As noted above, the spirit of our empirical exercise is closely related to the simulations inBuera et al. (2014), though the data and methods are quite different.

production technology $y_i = AK_i^{\alpha}L_i^{\beta}$, where$\alpha + \beta < 1$. Output ($y$) is the numeraire good, and the two factors of production, capital (K_i) and labor (L_i), can be purchased for unit prices r and w, respectively. Households may use labor from both their households L_i^H and from the outside labor market L_i^D for their businesses, such that $L_i = L_i^H + L_i^D$.

Households are endowed with a time endowment $\bar{T}$ that can be used toward outside labor supply L_i^S, home business labor supply L_i^H, or leisure l_i. In the basic version of the model, we assume that all agents supply their total labor inelastically, $L_i^S + L_i^H = \bar{T}$.[5]

Households are heterogeneous in their land endowments, D_i. In what follows, we assume $D_i \sim U\left[0, \bar{D}\right]$. We assume that land is an illiquid asset that cannot be used directly as a factor of production. However, land can be converted into capital through the financial markets. By posting land as collateral, households can borrow $b_i \leq \lambda D_i$. We assume that the market for loans is a nationwide market, thus households are price-takers in the interest rate r. The borrowing constraint λ is determined by the supply of funds to the microfinance market. We also assume that households must borrow to finance both capital and labor for production.

This form of borrowing constraint captures several of the salient features of the Indian microfinance market in an extremely simple way. First, low-wealth individuals are typically screened out from access to microfinance by MFIs.[6] Microlenders also tend to screen out potential borrowers who are "too rich."[7] Our model gives rise to some households being unconstrained, that is their optimal choice of investments are below λD_i, which is consistent with microfinance serving clientele with intermediate levels of wealth.

In equilibrium, the labor market must clear. The land endowments D_i will determine each household's total demand for labor. Wealthier households will thus be net demanders of labor, and poorer households will be net suppliers of labor to the market.

[5]The results are similar if we allow labor supply to be endogenously determined.

[6]The fact that individuals can be "too poor" for microfinance gives rise to the types of ultrapoor programs tested in Banerjee et al. (2015c). These programs aim to increase a household's wealth, captured by D_i in our model, so that they can become eligible for microfinance.

[7]The idea expressed by MFIs in conversations is that wealthy people have low value of future credit (and more disutility from weekly meetings) and are more prone to strategic default.

2.2. **Household Maximization.** Holding factor prices w and r fixed, households choose total labor, capital and borrowing to maximize business profits:

$$\max_{L_i,K_i} AK_i^{\alpha}L_i^{\beta} - wL_i - rK_i$$

s.t.

$$rK_i + wL_i \leq \lambda D_i$$

Turning to labor supply, if $L_i > T$, then $L_i^D = L_i - T$, $L_i^H = T$, and $L_i^S = 0$. If $L_i \leq T$, then $L_i^D = 0$, $L_i^H = L_i$, and $L_i^S = T - L_i$.

Let $\left(\tilde{L}(w,r), \tilde{K}(w,r)\right)$ be the labor and capital demand under perfect capital markets (i.e., $\lambda = \infty$), for fixed w, r. To make the problem interesting, and consistent with our application, we assume the parameters are such that $\tilde{L}(w,r) > T$ for reasonable values of (w,r), so that unconstrained households are net labor demanders and the market-clearing wage is positive.

Proposition 1. *Households will fall into one of three types, depending on their land holdings, D_i: a) Households with sufficiently high landholdings will be unconstrained (i.e., able to invest $\tilde{L}$), net demanders of labor; b) households with intermediate landholdings will be constrained, net demanders of labor; and c) households with low landholdings will be constrained, net suppliers of labor.*

2.3. **Equilibrium.** Given that the labor market clears at the local level, equilibrium labor supply must equal labor demand.

$$\int L_i^S dF_i = \int L_i^D dF_i$$

This equilibrium condition will pin down the wage.

2.4. **Comparative Statics and Empirical Predictions.** We now explore what happens to the labor market equilibrium when credit supply is contracted, that is when λ decreases.

Proposition 2. *The equilibrium wage $w(\lambda)$ is strictly increasing in credit supply,* $\frac{\partial w(\lambda)}{\partial \lambda} > 0$.

We can now interpret how a decrease in credit supply should affect each type of household. To facilitate this discussion, we solve the model under two different borrowing regimes. Figure 3 plots household earnings against land endowments in the case of $\lambda = 1.2$ and $\lambda = 1$. The bottom panel shows the change in earnings from a decrease in credit supply for individuals of varying levels of land.

The unconstrained, net labor demanders face two different effects. First, the decline in the equilibrium wage increases business profits, holding labor and capital fixed. Thus, households with high wealth that remain unconstrained after the policy change benefit from the decline in credit supply. Note that for the parameters used in Figure 3, this increase in earnings is very small.[8] Second, some households that were previously unconstrained, can no longer borrow enough after the credit contraction to reach the optimal scale of their business. This negative effect more than offsets the benefits from the lower wage for a substantial set of households in Figure 3.

The constrained, net labor demanders are hit hardest by the decrease in credit supply. These households become more constrained and are forced to operate their businesses at a smaller scale. For those households that continue to be net demanders of labor, the loss is partially offset by the decrease in wage. Moreover, some households switch from net demanders to net suppliers of labor. These households are made even worse off by the decrease in wages earned on the labor market.[9]

Finally, the constrained net labor suppliers also experience a negative effect of the credit contraction. However, the negative effect is smaller for individuals with extremely low levels of wealth. This pattern is clear in Figure 3. Individuals with the lowest levels of land experience a moderate decrease in earnings, which is mostly attributed to a decrease in labor market earnings. However, as wages increase, the reduction in earnings from the reduction in credit supply increases. This increase is due to the reduction in credit that limits the scale of the households business. However, these negative effects start to eventually decrease with wealth.

Therefore the model predicts monotonically decreasing treatment effects with wealth for labor market earnings and U-shaped treatment effects on business profits, total household earnings, business investment, and both durable and non-durable consumption.

3. Setting and Data

3.1. **Setting.**

[8]The model is solved for a uniform distribution of wealth on $[0, 30]$. We truncate the wealth levels shown in Figure 3. Note that due to the decreasing returns assumption, all households with high levels of wealth make the same production and labor supply decisions.

[9]This scenario is similar to (Jayachandran, 2006).

The Andhra Pradesh Ordinance of 2010. On October 15, 2010, the AP government unexpectedly issued an emergency ordinance (The Andhra Pradesh Micro Finance Institutions Ordinance, 2010) to regulate the activities of MFIs operating in the state. The government was worried about widespread over-borrowing by its citizens and alleged abuses by microfinance collection agents. The provisions of the Ordinance (promulgated as a law in December 2010) brought the activities of MFIs in the state to a complete halt. Under the law (which still stands as of this writing), MFIs are not permitted to approach clients to seek repayment and are further barred from disbursing any new loans.[10] In the months following the ordinance, almost 100% of borrowers in AP defaulted on their loans.[11] Furthermore, Indian banks pulled back tremendously on their willingness to lend to any MFI across the country.

What is important for this paper is that even MFIs even outside of Andhra Pradesh were affected. Perhaps surprisingly, the defaults in Andhra Pradesh did not spread across the country: individuals continued to make their regular loan repayments even though they may may have anticipated that their lender would not be able to give them more credit immediately upon full repayment. But, due to balance sheet effects, lenders were forced to contract their lending activities in healthy districts in other states.

3.2. **Data.** We use data from several sources in our empirical analysis. First, we hand collected proprietary administrative data from 27 microfinance institutions. This data is essential for constructing each district's pre-crisis balance sheet exposure to Andhra Pradesh. Based on this data, Table 1 shows that the total 2012 gross loan portfolio in districts where lenders were not exposed to the crisis is 1024 lakhs (roughly INR 102 million). Scaled by the number of rural households, this translates to INR 320 per household (averaging across borrowers and non-borrowers) in the average non-exposed district.

Measuring exposure to the AP Crisis. First, for each lender l, we calculate the share of the MFI's overall portfolio that was invested in Andhra Pradesh on the eve of the AP Crisis (the beginning of October, 2010):

[10]However, it is not illegal for borrowers to seek out their lenders to make payments.
[11]We investigate the effects of this "windfall" in a companion paper (Banerjee et al., 2014a).

$$fracAP_l = \frac{GLP_{l,AP,Oct2010}}{GLP_{l,Total,Oct2010}}.$$

Then, for each district d, we construct an aggregate exposure measure by taking the weighted average of $fracAP_l$ over all lenders who had outstanding loans in the district on the eve of the crisis, where the weights are that lender's total loan portfolio in the state, $GLP_{dl,Oct2010}$:

$$ExpAP_d^{Total} = \frac{\sum_l fracAP_l \times GLP_{dl,Oct2010}}{\sum_l GLP_{dl,Oct2010}}. \tag{3.1}$$

Thus, $ExpAP_d$ is a measure of the extent to which the district's loan portfolio on the eve of the crisis was exposed to the crisis. For instance, consider a district served by two lenders, each of whom makes 50% of the loans in the district. One lender operates solely in Northern India and has 0% of its portfolio in AP. The other is based in Southern India and has 40% of its portfolio in AP. Then $ExpAP_d^{Total} = \frac{.4+0}{2} = 0.20$.

We scale the exposure ratio (defined by equation 3.1) by the amount of credit outstanding per rural household. We calculate the rural population using the 2010 round of the NSS (discussed below). This scaling captures the idea that the same amount of outstanding credit will have a greater per-household impact in a less populous district vs a more populous one:

$$ExpAP_d = ExpAP_d^{Total} \times \frac{\sum_l GLP_{dl,Oct2010}}{RuralPop_{2010}} \tag{3.2}$$

Finally, we construct two measures of exposure to the AP crisis, both based on $ExpAP_d$. First is the log of the exposure ratio (defined by equation 3.2) plus one. Second is a dummy for a positive exposure ratio, that is, for the presence of a lender that had any exposure to the AP crisis. The proportion of districts with a positive exposure ratio is 37.3% (Table 1); the proportion of household-level observations located in these districts is very similar, at 36.9% (not reported in table).

NSS Data. Our primary outcome measures come from the Indian National Sample Survey (NSS). We use household data from waves 64, 66, and 68 of the NSS, which correspond to years 2007-2008, 2009-2010, and 2011-2012, respectively.[12] We focus on the schedules containing household composition, consumption and employment. Key variables are summarized in Table 1. (We summarize the 2012 values in low exposure districts for ease of comparison to the reduced form results, below.) Household total weekly earnings average INR 1015. The agricultural casual daily wage averages INR 142, and the non-agricultural casual daily wage averages INR 200.[13] Members of the average household work approximately 11 person-days per week, of which 7.8 are in self-employment and 2.9 in non-self-employment. Household size is 4.7, and the average household has 1.55 income earners. Nondurable household consumption is INR 6807 per month. Durable consumption per household is reported on an annual basis: it is INR 7902 per year. Just over one third (36%) of households report any non-agricultural self-employment.

4. Empirical Strategy

We estimate ITT impacts of reduced access to microfinance on a range of outcomes. The main estimating equation takes the difference-in-difference form

$$y_{idt} = \alpha + \delta_t + \delta_d + \beta \times Exposure_d \times Post_t + X'_{idt}\gamma + \varepsilon_{idt} \tag{4.1}$$

where y_{idt} are outcome variables for individual i in district d at time t; δ_t and δ_d are fixed effects for survey round (time) and district, respectively; $Exposure_d$ is a measure of the exposure of district d to the AP crisis (discussed below); and β is the coefficient of interest. X'_{idt} includes controls for the calendar month when the survey was conducted; household size; the rural population of the district at t and its square; a dummy for the presence of microfinance in the district in 2008 interacted with round; and dummies for quartiles of 2008 gross loan portfolio, interacted with round. Note that we do not observe a panel of households, but rather repeated cross-sections. Standard errors are clustered at the district level.

We use two measures of exposure to the AP crisis, both based on $ExpAP_d$. First is the log of the exposure ratio (defined by equation 3.2) plus one. Second is a dummy for a positive exposure

[12] As discussed below in Section 5.1, we also use the credit module of the 70th wave of the NSS to provide an alternate measure of the credit response to the crisis.

[13] We exclude work performed as part of public works programs such as NREGA from the wage calculations since NREGA wages are set administratively, not via market clearing. See Imbert and Papp (2015) for a discussion of how NREGA affects market wages.

ratio, that is, for the presence of a lender that had any exposure to the AP crisis. The proportion of districts with a positive exposure ratio is 37.29%; the proportion of household-level observations located in these districts is very similar, at 36.94%.

Our identification comes from the differential change in outcomes of household cohorts in otherwise-similar districts with differing degrees of exposure to the crisis. Given the time-varying controls we include, our identifying assumption is that households in districts with the same rural population and the same level of total MFI lending in 2008 are on similar trends regardless of whether the MFIs lending in the district were highly exposed to the AP crisis or not.

One piece of evidence supporting this assumption is the fact that microlenders before the crisis tended to offer a very homogeneous product. Most lenders used all of the following features: interest rates of approximately 25-30% APR, weekly or monthly meetings, meetings held in groups, similar loan sizes, and similar dynamic incentives. Moreover, most MFIs had borrowers recite a joint oath at the beginning of each repayment meeting. Given this standardization, this assumption appears *a priori* reasonable. Moreover, we present robustness and placebo checks below that lend direct support to this assumption.

5. Results

5.1. **First Stage.** Table 2 presents the first stage, estimated by equation 4.1 with a measure of credit outstanding in 2012 on the left-hand side. We show results for the district-level total gross loan portfolio (column 1) and the gross loan portfolio per rural household (column 2). Row 1 of column 1 shows that a 1 log point increase in exposure to the crisis (as measured by the pre-crisis portfolio weighted exposure of the district's lenders to the AP crisis) is associated with roughly INR 21,900,000 (219 lakhs) less credit outstanding in the district in 2012 (significant at 1%). The second row of column 1 indicates that those districts with an AP-exposed lender have INR 75,200,000 (752 lakhs) less credit outstanding in 2012 (also significant at 1%), compared to other similar districts whose lenders were not exposed to the crisis. Row 1 of column 2 shows that a 1 log point increase in exposure to the crisis is associated with INR 67 less credit outstanding per rural household in 2012 (significant at 1%). The second row of column 2 indicates that those districts with an AP-exposed lender have INR 228 less credit outstanding per rural household in 2012 (significant at

1%), compared to other similar districts whose lenders were not exposed to the crisis. The average of the household-level dependent variable in 2012 for households in non-exposed districts is INR 324, so this is a large effect, implying that AP-exposed lenders cut back significantly on lending and this shortfall was not fully made up by other, non-exposed microlenders.

It is not surprising that other microlenders were unable to target the borrowers of exposed MFIs. First, expanding to new villages requires fixed investments in branch infrastructure and in staff. Second, even non-exposed MFIs report having trouble obtaining credit from the Indian banking sector, which traditionally provided most of the funding to the MFIs. Third, borrowers often were allowed to take larger loans only after establishing a successful repayment record with their lenders. Given that there was no microfinance credit registry, even if households were able to secure new loans from new lenders, those loans would likely have been smaller in size.

Did banks fill the gap? To understand the effects of the crisis on total access to credit, it is important to understand whether other sources, such as commercial bank lending, filled some or all of the gap left by the reduction in access to microcredit. To examine this, we use information from the Reserve Bank of India (RBI) "District-Wise Classification of Outstanding Credit of Scheduled Commercial Banks." These were merged at the district-year level to examine whether more-exposed districts saw a differential change in commercial bank lending. We focus on the category of agricultural loan accounts as this category includes most forms of lending to households, including "artisans," i.e. non-agricultural microenterprises. Table 3 reports the results. There is no effect of exposure to the crisis on the number of agricultural loan accounts, nor the amount outstanding. When we distinguish direct accounts (largely made to individuals) from indirect counts (largely made to other entities, including MFIs, for on-lending) we again see no effect for direct accounts or amounts, and a fall in indirect accounts, likely reflecting reduced lending to MFIs in response to regulatory uncertainty surrounding the MFI sector. In sum, there is no evidence that commercial bank lending filled the gap.[14]

[14]Neither the NSS or RBI data allows us to examine the effect of the crisis on informal/interpersonal lending; however, the results in Table 4, discussed below, show that the effect on total lending is negative and large, albeit imprecisely estimated, so there is no evidence that informal lending filled the gap. This is intuitive since the credit shock was aggregate to districts, so the social networks of affected households were themselves affected.

Alternative Credit Data. Our hand-collected credit data is not without limitations. In particular, it represents approximately 18% of the Indian market: a large share of the market was comprised of MFIs who declined to share their data with us. If the responding firms are a random sample of all firms, this will only add noise to our measure of exposure, attenuating our measures of the effect of exposure to the crisis toward zero. However, one may worry that the subset of firms who responded is non-random in some way.

As a check, we draw on an alternative source of data, based on survey reports of household indebtedness, rather than MFI reports of their loan portfolios. The source we use is the NSS 70th round "Debt and Investment" survey, collected in 2012 and 2013. Its questions are asked to allow a researcher to reconstruct a household's total credit outstanding on June 30, 2012.

This is an entirely different data source than that used in Table 2. It is reported by households, not MFIs, and covers a nationally representative sample of Indian households. Thus, to the extent that we observe similar patterns in this data and in the data we collected with MFIN, it confirms that the patterns of exposure we observe are not artefacts of MFI reporting decisions. However, the "Debt and Investment" data is not without its own drawbacks: most significantly, we only have this data for 2012, so we are unable to use our preferred differences-in-differences empirical strategy. We must instead rely on cross-sectional comparisons.[15] This is viewed as complementary with our analysis above.

Another challenge with the "Debt and Investment" data relates to the classification of MFI loans. The credit survey asks households to enumerate each loan outstanding and aims to capture detailed data on the type of lender and terms of the loan. There are 17 different lender types. The NSS handbook (NSSO, 2014) states that for-profit microfinance should be grouped as SHG-NBFC (self-help group - non-banking financial company); however, non-profit microfinance and bank-linked SHGs are grouped under SHG-BL (self-help group - bank-linked). Further, there are three other categories that describe non-bank formal loans from financial institutions, which can be collateralized or uncollateralized. In sum, there is significant uncertainty about how respondents and surveyors would choose to treat a MFI loan.[16]

[15]The NSS did collect a small household indebtedness survey as a part of Round 66. However, this module was given only to landless agricultural households, so is unlikely to adequately capture district-level microfinance.

[16]Our experience in the field suggests that these differences in legal structure of loans—e.g., whether an MFI lender is for-profit or non-profit—are not always salient to respondents.

To address this ambiguity, we construct two measures intended to capture MFI borrowing. First, we present a measure based on the narrow NSS definition, those classified as SHG-NBFC. We also present a measure that captures all uncollateralized non-bank credit from formal institutions. We include in this definition all non-collateralized SHG loans, some of which may be linked to a bank. As well as addressing mis-classification, our broader definition allows us to capture impacts on microcredit that are *net* of any offsetting SHG supply response.

Table 4 presents OLS regressions of household credit on our pre-crisis AP exposure variables. Because we cannot use our differences-in-differences strategy, we instead control for numerous pre-crisis, district-level covariates from our three data sources.[17] In columns 1 and 3 we consider impacts on the narrow definition of microfinance, SHG-NBFC.[18] Remarkably, we find impact estimates that are strikingly close to those in Table 2. Districts that are exposed to AP pre-crisis experience a decrease in per capital microcredit outstanding of Rs. 273. This effect size is large relative to the control mean of Rs. 508.6, implying a drop in (narrowly defined) MFI credit of over 50%.

Next, in columns 2 and 4, we examine the impacts of high exposure on the broader measure of non-collateralized formal credit. Here, we find that pre-crisis exposure reduces outstanding credit in 2012 by Rs. 1,319. as with the narrower measure, this represents a fall of just over 50% compared to the control mean. This suggests that SHGs did not in fact fill the void. It also suggests that it is indeed likely that some for-profit microfinance loans were mis-classified in the NSS surveys as SHG-BL rather than SHG-NBFC loans.

In columns 5 and 6, we present bank credit and total credit as outcomes. While the coefficients are estimated imprecisely, we again find, in column 5, no evidence that bank credit was able to offset the fall in microcredit. (A finding which is consistent with Table 3, which uses a different source of data, namely RBI data on banks' balance sheets.) Finally, in column 6 we observe a negative, but imprecisely measured, coefficient on total credit outstanding.

[17]MFI balance sheet controls include levels and quintiles of GLP measured in both 2008 and 2010. RBI controls include amount of credit outstanding and number of accounts for agricultural loans, direct loans, and indirect loans. NSS 66 controls include average monthly household expenditures, annual durables expenditures, weekly earnings from and days worked in self-employment and non-self employment, daily wage, and percent of weekly earnings from self-employment.

[18]Columns 1 and 2 use non-winsorized values, while columns 3-6 use data winsorized at the 99th percentile of non-zero observations. We find very similar results whether we used winsorized data or not.

The results from the "Debt and Investment" survey data are reassuring in that they find very similar patterns as those seen in our main data source, the balance sheet collected data with MFIN. Thus, the first-stage effects of exposure to the crisis are not an artefact of differential reporting to MFIN or of geographical clustering across MFIs. In Section 5.4, below, we discuss the implications of this exercise for the scaling of the reduced form results.

5.2. **Reduced Form: Main Results.**

Labor Outcomes. We begin by measuring district level impacts of the reduction in credit observed in Table 2 on the labor market. Table 5 reports treatment effects on casual daily wages, household total labor supply, total labor earnings, involuntary unemployment and entrepreneurship. We begin by noting that the reduction in credit did have economically and statistically significant effects on both the agricultural and the non-agricultural daily wage. Exposed districts experienced a fall in the daily agricultural wage of INR 5.3, significant at the 10% level, which is displayed in row 2 of column 1. This represents roughly a 4% reduction from the unexposed district mean of INR 142. The effect on the daily non-agricultural wage is even larger, INR 16, significant at the 1% level (row 2, column 2); this is roughly an 8% reduction from the unexposed district mean of INR 200. We next ask if this decrease in wage affected total household labor supply and total labor earnings. Column 3 shows that there are no detectable effects on total days worked. Given that wages fell, but labor supply did not, this leads to an overall decline in household weekly labor market earnings of INR 78 in exposed districts relative to unexposed districts after the AP crisis, significant at the 1% level (column 4), a 7.7% fall relative to the unexposed district mean of INR 1015. We also observe that households do not change their assessment of whether they are involuntarily unemployed differentially in high versus low exposure districts after the crisis (column 5). Thus we do not find evidence the the crisis resulted in rationing in the market for casual labor, suggesting that the market equilibrates via the wage.

Column 6 examines effects on the likelihood that a household has any non-agricultural self employment. There is no evidence of a significant average effect; however, we will show below that there is evidence for an effect for households with intermediate landholdings. Thus the principal direct margin of adjustment seems to have been the scale of business operations, rather than the extensive margin of entrepreneurship or of household labor supply. Consistent with the assumptions of the model, we find a large indirect effect on households through the equilibrium wage.

Our strong wage and labor earnings results correspond with the predictions of Buera et al. (2014) and highlight the importance of incorporating general equilibrium effects into the analysis of the effects of credit access. Looking at effects on downstream outcomes, and comparing them with results from partial equilibrium studies, can shed light on the question of whether high- or low-TFP firms are most responsive to the credit shock. We next turn to examining these downstream outcomes.

Consumption. Table 6 reports the effects of reduced credit access on total expenditure and its components: nondurables and durables, measured on a monthly basis. Column 1, row 1 shows that a 1 log point increase in exposure to the crisis is associated with a reduction of INR 86 in per capita monthly nondurable expenditures in 2012 (significant at 1%). Column 1, row 2 indicates that those districts with an AP-exposed lender have INR 345 lower per capita monthly nondurable expenditure (significant at 1%), compared to other similar districts whose lenders were not exposed to the crisis. Column 2 examines per capita monthly nondurable expenditures. Row 1 shows that a 1 log point increase in exposure to the crisis is associated with a reduction of INR 67 (significant at 1%), and row 2 shows that those districts with an AP-exposed lender have INR 246 lower per capita annual durable expenditure (significant at 5%). Column 3 repeats the analysis for per capita monthly durable expenditures. Row 1 shows that a 1 log point increase in exposure to the crisis is associated with a reduction of INR 16 (significant at 1%), and row 2 shows that those districts with an exposed lender have INR 82 lower per capita annual durable expenditure (significant at 1%).

Impacts on Agricultural Output. We next examine whether the effects seen on household level outcomes are also apparent in other indicators of economic activity. Given the importance of agriculture to rural Indian economies, we examine crop yields. We use data from the Ministry of Agriculture, Directorate of Economics and Statistics, which collects information on crop production. Following Jayachandran (2006), we consider a weighted average of log yield (production in tonnes/area cropped in hectares) for the five major crops: rice, wheat, sugar, jowar (sorghum), and groundnuts.[19] We also consider each crop separately. The results appear in Table 7. Column 1 shows that, for each log point of exposure to the crisis, the yield index falls by 1.73 units, or roughly 3.6% compared to the control mean of 48.34. This effect is significant at the 5% level. Column 2 examines the effect on rice yield and finds essentially no effect; this is as expected since rice production is concentrated in the north of India, which had relatively little MFI exposure. Columns 3-6 show significant effects on the yields (in Tonnes/Hect.) of wheat, jowar (sorghum), sugarcane and groundnut. The effect of a one log point increase in exposure (row 1) range from 4.5% of the control mean (groundnut) to 8.3% of the control mean (sugarcane); all four effects are significant at the 10% level or better. The effect of any AP exposure, relative to no exposure (row 2), is roughly a 25% reduction in yields for wheat and sugarcane (significant at the 1% level); for jowar and groundnut the effects of the binary measure of exposure are imprecisely estimated.

Thus, a data source completely independent from the NSS data suggests that agricultural enterprises are scaling back in response to the loss in credit access, and further, that the consequence is statistically significant and economically meaningful effects on agricultural output.

5.3. **Robustness checks.** Our results are robust to a variety of possible confounds. Table B.2 reports key outcomes for two alternate specifications that test the idea that exposure to the AP crisis may be proxying for distance to AP, and hence may be picking up effects that do not work through firms' balance sheets, but through other "spillover" effects of the crisis (economic uncertainty, etc.). The top panel drops districts which border AP. The second panel controls for distance from the district capital to Hyderabad (AP's capital), interacted with round. In both cases the effects on expenditure, earnings, labor supply and wages remain significant and quantitatively similar.

Table B.3 tests for the concern that more-exposed areas were systematically surveyed by the NSS at times of the year when outcomes looked worse. The table adds controls for state dummies interacted with month of survey, and our conclusions remain robust.

Table B.4 tests for the possibility that states with greater exposure to the crisis may have been more "aligned" with Andhra Pradesh for other reasons, such as having similar political parties in power. We add as controls indicator variables for the political party of the state's chief minister in 2010, at the time of the crisis, interacted with round. This allows all states with a certain party in power to be on a differential trend. Again, our results remain robust.

[19]As in Jayachandran (2006), the weights are the district-average revenue share of the crop.

Placebo regression. Finally, as a check of the identifying assumption, Table B.5 conducts a placebo test, dropping the round 68 data and assigning the round 66 observations the status of Post. If districts that were more exposed to AP were on differential trends prior to the crisis, we should see significant effects in round 66. Reassuringly, for none of the main outcomes is the placebo treatment significant at standard levels.

5.4. **Scaling the Reduced Form Treatment Effects.** Due to the concerns with both our pre-crisis measure of exposure (where we capture a fairly small share of the market and where, as discussed below, the timing of our data may miss the worst of the crisis) and with our ex post measure of the drop in credit (where there is likely to be mis-classification of MFI loans), one needs to use caution when thinking about scaling the reduced form effects into treatment on the treated (TOT) effects measuring the effect of a given amount of credit.

One issue with our MFI balance sheet data is a timing mis-match. The post-crisis data reflects balance sheets as of March 2011 and March 2012. Credit likely bottomed out around the end of 2011, by which time all of the loans outstanding at the time of the crisis would have rolled over; this is consistent with Figure 1. Thus, our data likely misses the bottoming-out of the market and hence the full magnitude of the credit contraction. Our NSS “Debt and Investment” data measures credit at an even later point of time, June 2012. Thus, from a timing perspective, the measured drop in credit associated with exposure to the crisis—that is, the first stage—is likely too small. The NSS round 68 outcomes data, on the other hand, were measured for most households at the end of 2011, likely reflecting the full brunt of the credit contraction. Thus, scaling the reduced form impacts by the measured first stage may imply TOT effects that are too large.

Another issue, discussed above, is that the first stage based on the balance sheet data, as used in Table 2, only measures lending from the subsample of MFIs who provided their data. This will attenuate the first stage relationship. A similar issue is present in the narrow definition of MFI borrowing from the “Debt and Investment” data—to the extent that some MFI lending is missed, the implied effects of the crisis will appear too small. Consistent with this concern, the first stage based on the balance sheet data in Table 2 and the narrow measure of MFI borrowing in Table 4 are strikingly similar, while the broader measure of microfinance in Table 4 implies that the first stage is larger.

In sum, any scaling of reduced form effects by first stage estimates should be done with caution. If a first stage number is desired for back-of-the-envelope purposes, the broader measure of microfinance in Table 4 (roughly Rs. -1300) is arguably the most appropriate.

5.5. **Heterogeneous Effects.** So far we have reported average effects, but another question of interest–both from a policy perspective and in terms of testing the implications of our model–is how the effects of the credit contraction are felt among those who are differentially affected by both the direct (lending) effect and the general equilibrium wage effect. Recall that the model predicts monotonically decreasing treatment effects with wealth for labor market earnings and U-shaped treatment effects on business profits, total household earnings, business investment, and both durable and non-durable consumption.

While we do not have panel data at the household level and so cannot follow households over time, we can examine effects separately for different parts of the distribution, defined by contemporaneous but "sticky" measures of household wealth. One such measure is land holdings; another is the size (measured as employment) of the household's business.[20] For these analyses we focus on the binary measure of exposure to the crisis.

Heterogeneity: Landholdings. Table 8 reports effects on key outcomes separately for each quintile of the within-district land distribution. Column 1 shows the effects on household weekly labor earnings associated with a high exposure to the crisis. As predicted, there is a fall for the earnings of households in quintile 1 (landless and near-landless) of INR 25 (significant at 5%). For higher land/wealth households, the effects are insignificant, with a pattern of point estimates that are generally shrinking in magnitude as land holdings increase. Thus, the low wealth households who are the largest suppliers of outside labor see the largest effect via the labor earnings channel.

Column 2 shows effects on monthly nondurable consumption. The largest magnitude effects are seen in the fourth quintile of the distribution, where monthly nondurable consumption falls by INR 141 (significant at 1%). Households in the 1st (poorest) quintile see a fall of INR 55 (significant at 10%); those in the third quintile see a fall of INR 76 (also significant at 10%). The effect for

[20] We verify that these measures are not themselves correlated with exposure to the crisis: more-exposed districts do not have a differential number of large businesses and, mechanically, the share of households in each within-district land holdings quintile is not correlated with exposure to the crisis. The results are presented in Appendix table B.6.

the largest landholders is insignificant. Thus the effects are broadly consistent with the U-shaped pattern of results predicted by the model.

Column 3 examines annual durable consumption, and finds a similar pattern: large and highly significant effects for the fourth quintile of the distribution, where annual durable consumption falls by INR 358 (significant at 1%). The effects at both lower and higher quintiles are smaller in magnitude, again suggesting a U-shaped pattern.

Finally, column 4 shows effects for the likelihood that a household has any non-agricultural self employment. Again, effects are seen for the fourth quintile of the distribution, where the likelihood of any non-agricultural self employment falls by 0.9 percentage points (significant at 5%). The effects at other quintiles are close to zero.

This pattern suggests that medium landholders, who may be most likely to borrow directly from microfinance, respond to reduced credit access by reducing consumption and investment in household businesses (proxied by durable spending) as well as the extensive margin of business ownership. The landless and near-landless experience falls in earnings, due to a combination of a reduced daily wage arising from reduced labor demand from local businesses; their consumption also falls. Finally, the largest landholders, whose businesses may be able to reach the optimal scale even after the credit contraction, appear relatively unaffected by the reduction in credit access.

Heterogeneity: Business scale. Table 9 reports effects on key outcomes separately for owners of "small" and "large" businesses: those with fewer than 6 employees and 6 or more, respectively.[21] Consistent with the model's predictions, the effects are entirely experienced by owners of small businesses, those whose scale is most likely to fall in response to the credit contraction. For these households, the effect of a 1 log point increase in exposure to the crisis is a fall in household weekly labor earnings of INR 17. Monthly nondurable consumption falls by INR 84, and annual durable consumption falls by INR 214 (all significant at 5% or better). There are no significant effects for owners of larger businesses. Using the binary measure of exposure to the crisis, household weekly labor earnings fall by INR 77, monthly nondurable consumption falls by INR 309, and

[21]We show in Appendix Table B.6 that owning a large business is not differentially more common in high exposure versus low exposure districts following the AP crisis.

annual durable consumption falls by INR 1101 (all significant at 5% or better); again there are no significant effects for owners of larger businesses.

Heterogeneity: Peak labor demand. If wages display downward rigidity (Kaur, 2015), a crucial determinant of wages may be whether they adjust upward when demand is at its peak. To address this possibility, we examine whether the effects of (lack of) access to microcredit differ in times of peak labor demand. Namely, the effects of the reduction in credit access may be most pronounced during peak labor demand periods, when wages would have counterfactually have risen but instead remain unchanged.

To investigate this hypothesis, we use variation in the timing when different households are administered the NSS survey and the fact that certain times of the year (planting, harvest) will be characterized by high labor demand. Due to differences in crop choices, weather patterns, etc., these peak demand periods differ across districts. We split the calendar year into two-week bins of time and, for a given district, calculate the percentage of survey respondents who report that they are employed in an agricultural activity (sowing, weeding, harvesting, etc.), counting both self- and non-self-employment. We identify peak demand periods in a given district as the 6 two-week bins (i.e., 12 weeks total) with the highest agricultural employment.

Appendix Table B.1 presents the effects of exposure to the crisis for the subsample of households surveyed in peak demand periods and those surveyed in non-peak periods. Column 1 shows that the effect on the agricultural wage is almost three times larger during peak periods than non-peak periods. Column 2 shows that the effect on the non-agricultural wage shows no similar pattern—in fact the effect in non-peak periods is slightly larger. This is as expected since we have focused on peak periods of *agricultural* labor demand, and suggests that agricultural and non-agricultural labor markets are somewhat segmented. As with the effect on the agricultural wage, the effects on total consumption are larger in peak vs. non-peak periods. The difference is not significant in levels (column 3), but is in logs (column 4), possibly suggesting that lack of upward wage adjustment is particularly painful for liquidity constrained households with low consumption.

6. Discussion

6.1. **Relation to Microfinance Evaluations.** Why do we find significant differences with the RCT evidence (e.g. Angelucci et al. (2015), Augsburg et al. (2015), Attanasio et al. (2015), Banerjee et al. (2015b), Crépon et al. (2015), and Tarozzi et al. (2015))? These studies paint a consistent picture of modest impacts on both business and household outcomes, while our findings of significant negative impacts of *loss of* access to microcredit suggest that the effects of microcredit were sizable and positive. Studies based on randomized designs offer gold standard internal validity; however, they are typically not designed to, and do not intend to, measure GE effects. Nonetheless, some of the findings from RCTs shed light on the possible direction of spillover/GE effects.

In their study in rural Morocco, Crépon et al. (2015) document labor supply effects–a reduction in supply of labor to the outside market stemming from access to microcredit[22]–and note that any wage effects are likely to be biggest for those who do not have high propensity to borrow. Studies that sample likely borrowers–a common strategy to increase statistical power–will likely miss these individuals/households. Thus we may fail to conclude that microfinance is a low-cost way to encourage economic activity (potentially less distorting and expensive than, e.g., workfare programs such as NREGA) if positive wage effects are not taken into account. Further direct evidence of wage effects comes from Kaboski and Townsend (2012) who, using a natural experiment, find that increased credit access due to the Million Baht Program in Thailand increased wages.

One contributor to the coexistence of modest average effects on borrowers combined with significant GE effects may be firm heterogeneity. Wage effects may result if a small fraction of firms experience significant treatment effects of microfinance, and these firms generate significant employment. Yet, at the same time, the estimated average treatment effects may be modest if a large fraction of households/firms exhibit small or no treatment effects. This is exactly the pattern documented by Banerjee et al. (2015a), who find significant and persistent treatment effects for the roughly 30% of households who selected in to entrepreneurship prior to the entry of microfinance. Other households, despite borrowing at similar rates, and starting new businesses, experience close to a zero treatment effect; as a result, the overall average effect on many key outcomes is small

[22]Interestingly, they find this reduction in hours worked outside even among low-probability households, perhaps because the option to borrow in the future reduces the need for income diversification.

and imprecisely estimated. The effects stemming from increased employment in the productive businesses may be hard to see in partial equilibrium.

Another suggestion of spillover impacts comes from Banerjee et al. (2014b), who examine the effects of dis-enrollment in microfinance, using an experiment in which microloans were bundled with compulsory health insurance. The requirement to pay the insurance premium caused people to reduce loan takeup by 22 percentage points (31 percent), although the premium was relatively modest (Rs. 525). The authors find large measured effects on businesses' sales, profits and amounts spent on assets and workers, providing intriguing evidence of spillovers on workers and of the possibility that aggregate welfare benefits of microfinance may be significant despite low revealed valuation by the borrowers.

6.2. **Decomposing Partial and General Equilibrium.** Finally, we use a parametrization of our model to examine the breakdown of the total effect of reduced credit access into partial vs. general equilibrium channels. To provide a visual illustration of how the the PE and GE effects play out across the wealth distribution, in Figure 4, we take the plots of the model presented earlier in Figure 3, and consider a third scenario. We take the wage from the pre-crisis period and assume that it does not change—shutting down the GE channel—and consider the *direct* impact of a fall in credit across the wealth distribution. The top panel shows the earnings effects in each scenario (pre-crisis; post-crisis with pre-crisis wage (PE); and post-crisis with the new equilibrium wage (GE). The bottom panel shows the fraction of the total change in earnings which is due to PE.

Landless households are not able to borrow, so they are only affected through the GE (wage) channel: the fraction of their total effect due to PE is 0.[23] Intermediate-wealth households experience both PE and GE effects. Under the specific parameters chosen, for the households that have the largest treatment effects, 60% is PE, 40% GE. These qualitative patterns are robust to alternate parameter values, though the exact magnitudes will of course differ.

This breakdown, while purely illustrative and not intended as a calibration exercise, highlights that the poorest households will disproportionately experience GE rather than PE effects of credit

[23]The wealthiest households are not affected directly (in PE) either, because they can reach the optimal business scale even after λ falls and so they benefit only through GE; however, to produce a readable figure we do not show the portion of the land distribution corresponding to these households.

access, or lack thereof. Thus, to the extent that RCT sample populations are drawn from low-wealth/vulnerable populations, the control group, if located in the same labor market(s) as treated households, will experience these same GE effects. Thus, the GE effects will be netted out and RCT estimates will understate the true effects of access to credit.

7. Conclusion

We use the Andhra Pradesh microfinance ordinance as a natural experiment to measure the real impacts of the loss of microfinance on rural households. Given the scale and maturity of the microfinance sector in India before the ordinance, the crisis presents a unique opportunity to study the impacts of microfinance on the average borrower in general equilibrium, in contrast to experimental work which often measures impacts for marginal borrowers in partial equilibrium. We find that districts outside Andhra Pradesh, that were nonetheless exposed to the crisis through the balance sheets of their lenders, experience decreases in lending, consumption, earnings, and wages. Further, these impacts are borne heterogeneously across the wealth distribution within each district. The effects on the poorest households are largely mediated through the fall in equilibrium wage, while households with intermediate levels of wealth experience the largest declines in both durable and nondurable consumption. No impacts are detectable for the richest households.

Our results show that the actions of politicians in Andhra Pradesh had large negative externalities on microcredit supply to the rest of the country. Microfinance institutions were no longer able to finance creditworthy borrowers in other states, which in turn led to decreased wages, consumption, earnings, and even agricultural yields.

In summary, our findings complement the RCT literature. Randomized evidence has documented that, on average, intent-to-treat effects on households offered access to microcredit appear to be modest. Using an unique large-scale natural experiment, we show that, nonetheless, the increase in the scale of economic activity generated by microcredit access increases wages in general equilibrium and therefore has positive effects on welfare even for households who do not themselves borrow.

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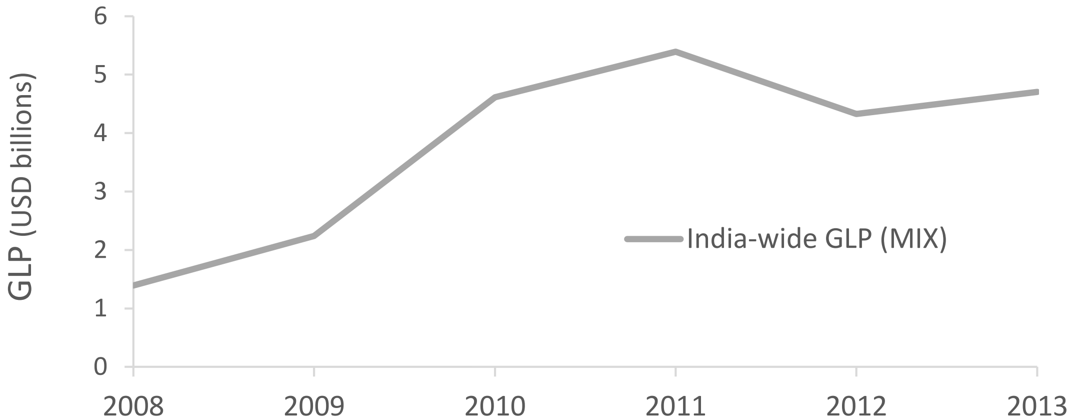
Figures

Panel A: India-Wide GLP



Panel B: GLP in High vs. Low Exposure Districts (ex Andhra Pradesh)

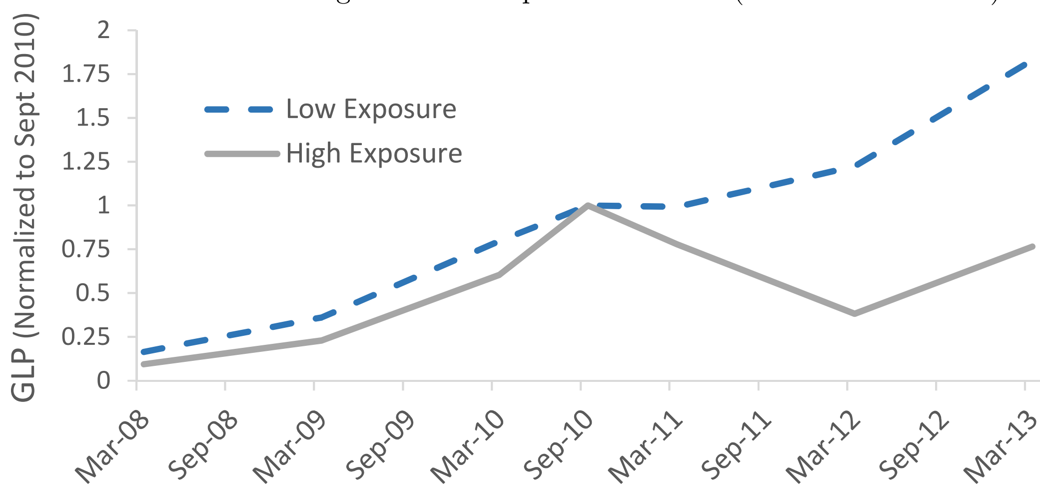
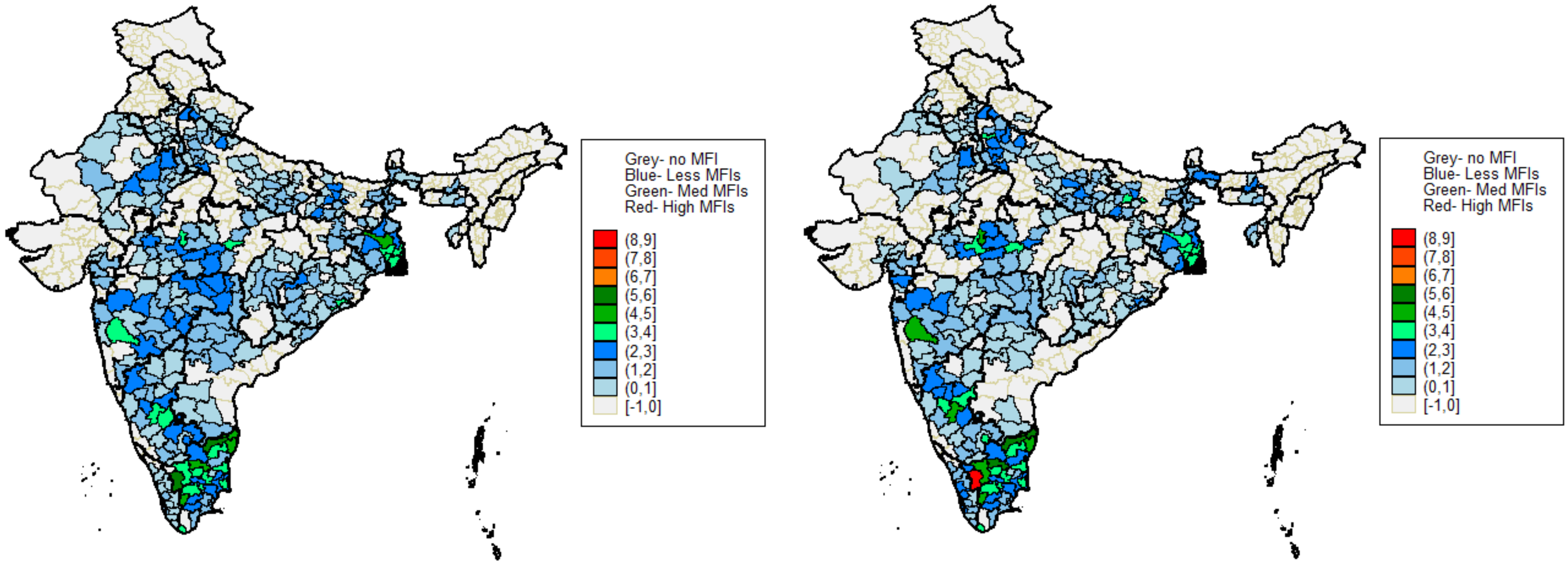


Figure 1. Growth of Microfinance Gross Loan Portfolio (GLP) in India and in Analysis Sample

Note: Panel A plots the India-wide gross loan portfolio (GLP) from 2008 to 2013 aggregated across microfinance institutions and states as reported in USD in the MIX database. Reporting to the MIX is voluntary, and thus the reporting dates may vary by lender. Panel B shows the evolution of microfinance using the hand-collected data (reported in Indian rupees) from 27 microfinance institutions. The figure in Panel B splits the districts between low and high AP exposure. A district is defined to have low exposure if it did not have any loans from an MFI that did have outstanding loans in Andhra Pradesh in September 2010. GLP in each year is scaled by the pre-crisis district level of microcredit on September 30, 2010.



(A) September 2010

(B) March 2012

Figure 2. Number of MFIs by District

Note: These maps present visualizations of the hand-collected data from 27 microfinance institutions. The first subfigure plots the number of lenders per district in our dataset in September 2010, on the eve of the AP crisis. Subfigure 2 plots the number of lenders per district after the contraction in lending was underway in March 2012. Districts without coloration indicate that none of the 27 lenders in our sample were lending in those districts at the time.

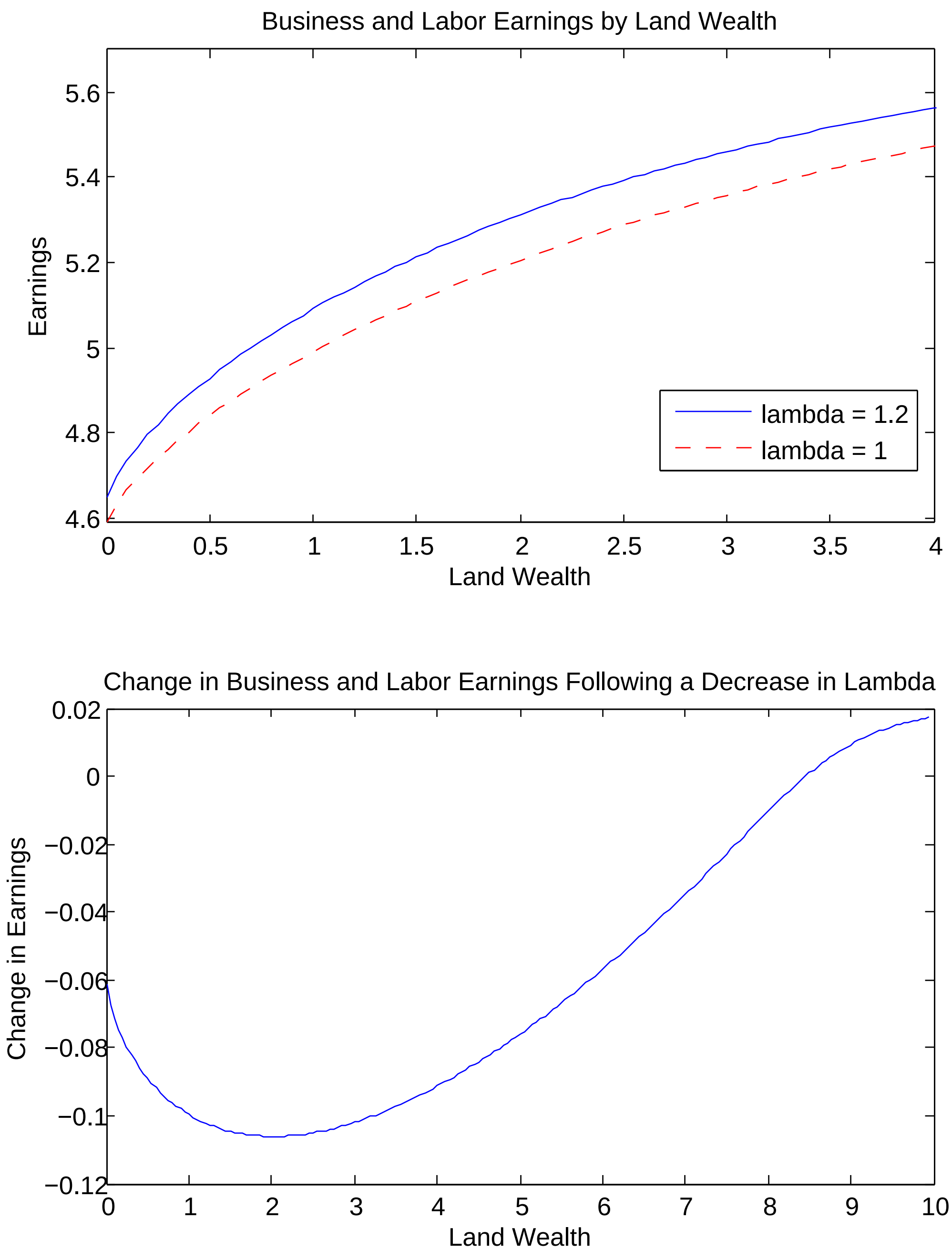


FIGURE 3. Modeled Earnings under High ($\lambda = 1.2$) and Low ($\lambda = 1$) Credit Supply Regimes

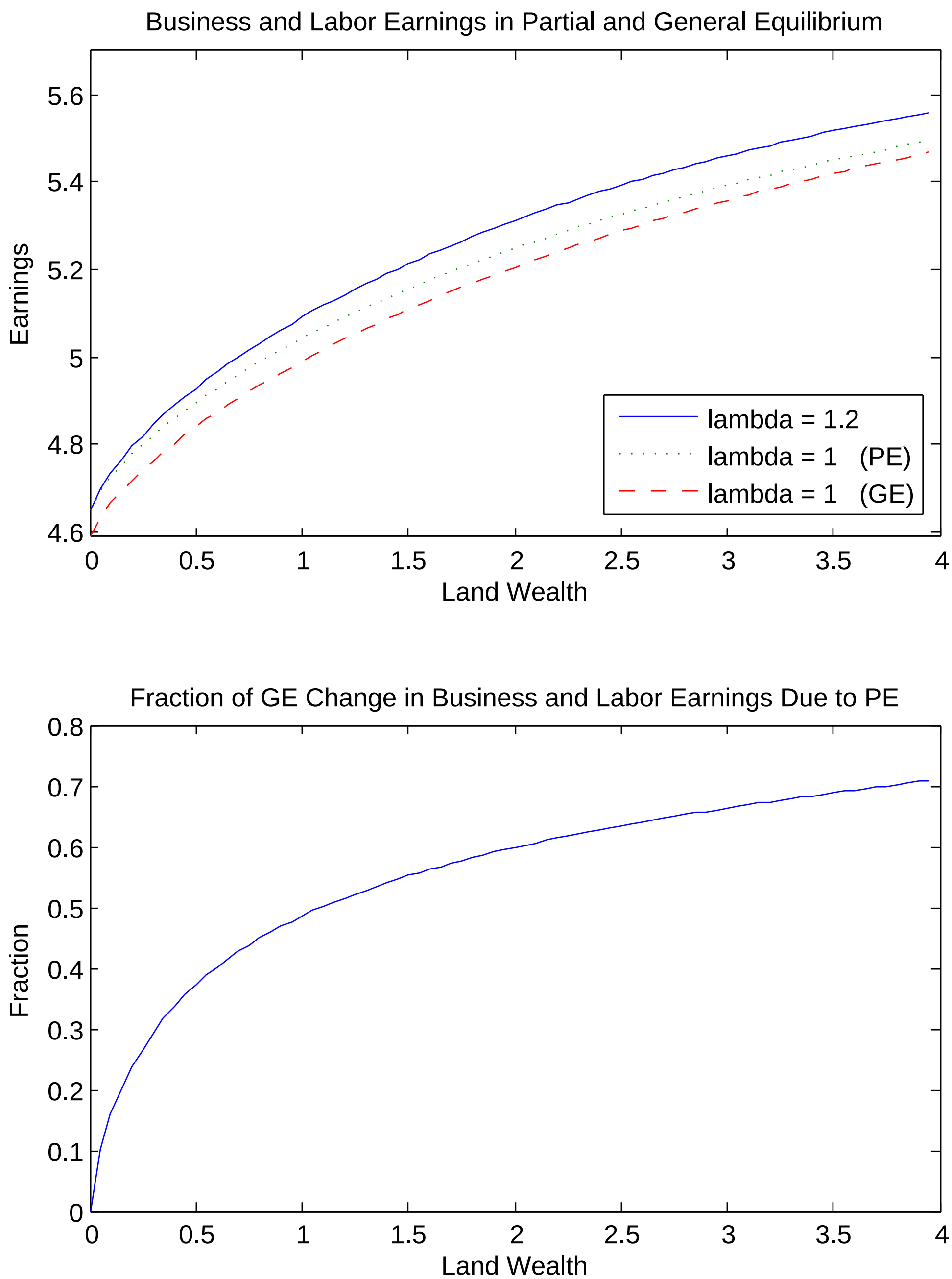


FIGURE 4. Earnings in Partial vs. General Equilibrium

Tables

Table 1. Summary Statistics, 2012 NSS

Variable	Obs	Mean	Std. Dev.
District-level variables from balance sheet data			
Any exposed lender, 2010	354	0.37	0.48
GLP per district (lakhs), 2012	222	1024	1705
GLP per rural household, 2012	222	320.3	559.4
Household-level variables from NSS (round 68)			
HH weekly labor earnings	22,603	1015	1903
Casual daily wage: Ag	1,664	141.8	67.6
Casual daily wage: Non-ag	3,312	200.2	109.3
HH weekly days worked	22,603	10.72	7.13
HH weekly days worked: self-emp	22,603	7.82	7.45
HH weekly days worked: non-self-emp	22,603	2.90	4.82
Any HH member invol. unemployed	22,603	0.09	0.29
HH size	22,603	4.76	2.37
Number of income earners	22,603	1.45	0.32
Monthly consumption: Total	22,603	6807	6215
Annual durables consumption	22,603	7314	33745
Any non-ag self-employment	22,603	0.36	0.48

Note: Outcome variables in first panel are from the balance sheet data collected with MFIN; see text for details. Sample is restricted to only low exposure districts (control group) except for the "Any exposed lender" measure, which is computed based on the full sample. Outcome variables in second panel are from NSS round 68 (2012). Sample is restricted to only low exposure districts (control group).

Table 2. Exposure to the AP Crisis and total MFI lending: Balance sheet data

	(1) District total gross loan portfolio in lakhs (100,000 INR)	(2) District gross loan portfolio per household (INR)
Log(HH Exposure Ratio) X Post 2010	-218.961***	-66.772***
	(26.993)	(6.623)
Any exposed lender x Post 2010	-752.176***	-227.658***
	(120.657)	(28.837)
Control mean	1249	324.4
Control SD	1746	496.2
Observations	119,670	119,670

Note: Outcomes data from MFI balance sheets. Each row provides coefficients from separate differences - in - differences regression specifications. The first row reports specifications that use the continuous exposure measure. The second row reports coefficients from separate regressions using the binary indicator for high exposure to AP. The outcome of interest in column 1 is total district-level credit outstanding (GLP) in lakhs (100,000 INR), while column 2 scales this value by the number of rural households. In all columns, controls include month, round, and district fixed effects,, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table 3. Exposure to the AP Crisis and commercial bank lending: RBI data

	(1) No. accounts (agriculture) ('000)	(2) Amt outstanding (agriculture) ('000 Rs.)	(3) No. accounts (direct) ('000)	(4) Amt outstanding (direct) ('000 Rs.)	(5) No. accounts (indirect) ('000)	(6) Amt outstanding (indirect) ('000 Rs.)
Log(HH Exposure Ratio) X Post 2010	-0.003	-0.086	0.001	-0.277	-0.004***	0.044
	(0.002)	(0.423)	(0.002)	(0.244)	(0.001)	(0.180)
Any exposed lender x Post 2010	-0.006	-0.583	0.005	-1.123	-0.011***	0.045
	(0.007)	(1.636)	(0.006)	(1.057)	(0.003)	(0.667)
Control mean	0.305	28.13	0.283	24.38	0.0222	3.804
Control SD	0.232	24.95	0.195	20.86	0.0467	6.244
Observations	117,828	117,828	117,828	117,828	117,828	117,828

Note: Outcomes data from RBI "District-Wise Classication of Outstanding Credit of Scheduled Commercial Banks." Each row provides coefficients from separate differences - in - differences regressions. The first row reports specifications that use the continuous exposure measure. The second row reports coefficients from separate regressions using the binary indicator for high exposure to AP. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table 4. Exposure to the AP Crisis and total MFI lending: NSS round 70 data

	(1) MFI amt outstanding	(2) Uncollateralized formal non-bank amt outstanding	(3) MFI amt outstanding, win.	(4) Uncollateralized formal non-bank amt outstanding, win.	(5) Bank amt outstanding, win.	(6) Total loan amt outstanding, win.
Log(HH Exposure Ratio)	-80.078***	-369.078***	-83.842***	-373.248***	-89.469	-806.296
	(28.601)	(113.510)	(26.450)	(111.714)	(572.251)	(882.927)
Any exposed lender	-272.925**	-1,319.222***	-300.405***	-1,319.384***	-1,642.650	-3,247.449
	(109.581)	(400.632)	(100.115)	(393.074)	(2,189.598)	(3,404.538)
Control mean	508.6	2557	441.0	2286	37259	68473
Control SD	9068	26447	4494	14794	114385	141525
Observations	38,492	38,492	38,492	38,492	38,492	38,492

Note: Outcomes data from the NSS 70th round "Debt and Investment" survey reflecting outstanding credit on June 30, 2012. Each row provides coefficients from separate OLS regression specification. The first row reports specifications that use the continuous exposure measure. The second row reports coefficients from separate regressions using the binary indicator for high exposure to AP. The outcome of interest in columns 1 and 3 is total SHG-NBFC credit outstanding. Columns 2 and 4 consider total formal, non-bank, non-collateralized credit, with individual-liability bank credit in column 5, and total credit in column 6. All outcomes in columns 3-6 are winsorized at the 99th percentile of non-zero observations. In all columns, we include pre-crisis district-level controls. Balance sheet controls include levels and quintiles of GLP measured in both 2008 and 2010. RBI controls include amount of credit outstanding and number of accounts for agricultural loans, direct loans, and indirect loans. NSS 66 controls include average monthly household expenditures, annual durables expenditures, weekly earnings from and days worked in self-employment and non-self employment, daily wage, and percent of weekly earnings from self-employment. Standard errors and clustered at the district level.

Table 5. Reduced Form: Labor Outcomes

	(1) Casual Daily Wage: Ag	(2) Casual Daily Wage: Non-Ag	(3) HH Weekly Days Worked	(4) HH Weekly Labor Earnings	(5) Any HH Member Invol. Unemployed	(6) Any non-Ag Self Employment
Log(HH Exposure Ratio) X Post 2010	-1.247*	-4.568***	-0.044	-18.042**	0.003	-0.004
	(0.731)	(1.192)	(0.043)	(7.155)	(0.003)	(0.003)
Any exposed lender x Post 2010	-5.288*	-16.353***	-0.119	-77.759***	0.010	-0.015
	(3.149)	(5.195)	(0.182)	(29.693)	(0.011)	(0.012)
Control mean	141.8	200.2	10.72	1015	0.0914	0.359
Control SD	67.61	109.3	7.129	1903	0.288	0.480
Observations	14,554	14,939	119,668	119,668	119,668	119,668

Note: Outcomes data from NSS rounds 64, 66, 68. Each row provides coefficients from separate regression specifications. The first row reports specifications that use the continuous exposure measure. The second row reports coefficients from separate regressions using the binary indicator for high exposure to AP. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round.

Table 6. Reduced Form: Consumption

	(1) HH Monthly Consumption: Total	(2) HH Monthly Consumption: Nondurables	(3) HH Monthly Consumption: Durables
Log(HH Exposure Ratio) X Post 2010	-86.233***	-66.932***	-16.444***
	(25.792)	(22.985)	(6.342)
Any exposed lender x Post 2010	-345.071***	-245.761**	-81.861***
	(117.914)	(105.414)	(24.312)
Control mean	6807	6197	609.5
Control SD	6215	4910	2812
Observations	119,668	111,692	111,692

Note: Outcomes data from NSS rounds 64, 66, 68. Each row provides coefficients from separate regression specifications. The first row reports specifications that use the continuous exposure measure. The second row reports coefficients from separate regressions using the binary indicator for high exposure to AP. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table 7. Reduced Form: Crop Yields

	(1) Crop yield index	(2) Rice Yield	(3) Wheat Yield	(4) Jowar Yield	(5) Sugarcane Yield	(6) Groundnut Yield
Log(HH Exposure Ratio) X Post 2010	-1.731**	-0.052	-0.085***	-0.032*	-3.925***	-0.039*
	(0.784)	(0.036)	(0.033)	(0.019)	(0.950)	(0.021)
Any exposed lender x Post 2010	-4.103	-0.170	-0.414***	-0.087	-12.870***	-0.120
	(3.806)	(0.150)	(0.136)	(0.066)	(4.570)	(0.093)
Observations	93,971	106,925	106,925	106,925	106,925	106,925
Control mean	51.65	1.829	1.351	0.468	38.76	0.839
Control SD	39.06	1.495	1.725	0.637	43.33	1.107

Note: Outcomes data from the Ministry of Agriculture, Directorate of Economics and Statistics. Index in column 1 is a weighted average of log yield (production in tonnes/area cropped in hectares) for the five major crops: rice, wheat, sugar, jowar (sorghum), and groundnuts, weighted by the district-average revenue share of the crop. Yields in columns 2-6 are in Tonnes/Hect. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table 8. Heterogeneous effects: Land

	(1) HH Weekly Labor Earnings	(2) HH Monthly Consumption: Total	(3) HH Annual Consumption: Durables	(4) Any non-Ag Self Employment	Obs.
Log(HH Exposure Ratio) x Post 2010					
1st Quintile District Land Dist.	-25.137**	-55.345*	-103.591	0.001	30,238
	(10.242)	(31.176)	(69.520)	(0.005)	
2nd Quintile District Land Dist.	-18.569	-23.526	-154.630*	0.001	21,906
	(17.350)	(39.688)	(83.459)	(0.006)	
3rd Quintile District Land Dist.	1.876	-76.283*	-120.615	-0.007	21,527
	(19.479)	(43.363)	(74.708)	(0.006)	
4th Quintile District Land Dist.	-16.341	-141.434***	-358.184***	-0.009**	21,981
	(10.530)	(35.735)	(119.248)	(0.004)	
5th Quintile District Land Dist.	-2.985	-71.092	-122.266	-0.006	19,917
	(14.495)	(56.831)	(342.928)	(0.006)	

Note: Each cell contains estimates from a different regression. The rows report the sample restriction by quintile of the district wealth distribution. The columns reflect different outcomes. All specifications use the continuous exposure measure. Outcomes data from NSS rounds 64, 66, 68. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table 9. Heterogeneous effects: Employees

	(1) HH Weekly Labor Earnings	(2) HH Monthly Consumption: Total	(3) HH Annual Consumption: Durables	Obs.
Log(HH Exposure Ratio) x Post 2010				
<6 Employees	-17.063**	-84.217***	-213.754***	69,970
	(7.540)	(27.922)	(79.137)	
>=6 Employees	2.869	204.611	-109.069	1,327
	(56.211)	(244.715)	(795.341)	
Any exposed lender x Post 2010				
<6 Employees	-77.235**	-308.565**	-1,101.087***	69,970
	(31.534)	(124.251)	(320.394)	
>=6 Employees	43.348	671.329	-812.644	1,327
	(215.790)	(1,008.614)	(3,089.220)	

Note: Each cell contains estimates from a different regression. The rows report the sample restriction by number of employees in the household business. The top two row report results using the continuous exposure measure, while the bottom two rows use the binary measure. The columns reflect different outcomes. Outcomes data from NSS rounds 64, 66, 68. In all columns, controls includemonth, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Online Appendix

Appendix A. Supplementary Tables

Table B.1. Heterogeneity: Peak labor demand periods

	(1) Casual Daily Wage: Ag	(2) Casual Daily Wage: Non-Ag	(3) HH Monthly Consumption	(4) Log HH Monthly Consumption
PEAK LABOR DEMAND PERIODS				
Log(HH Exposure Ratio) X Post 2010	-2.002	-4.776**	-86.469**	-0.016*
	(1.216)	(2.104)	(42.061)	(0.009)
High HH Exposure x Post 2010	-10.023*	-17.226*	-364.243**	-0.043
	(5.346)	(9.156)	(180.382)	(0.038)
Observations	2,515	3,482	25,625	25,625
NON-PEAK LABOR DEMAND PERIODS				
Log(HH Exposure Ratio) X Post 2010	-0.749	-5.546***	-64.240**	-0.005
	(1.018)	(1.496)	(31.671)	(0.006)
High HH Exposure x Post 2010	-3.421	-19.939***	-230.745	0.003
	(4.003)	(6.127)	(145.577)	(0.026)
Observations	12,039	11,457	94,043	94,043

Note: Outcomes data from NSS rounds 64, 66, 68. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table B.2. Robustness: Distance to Andhra Pradesh

VARIABLES	(1) Household average total monthly expenditure	(2) Household expenditure on durables, last 365 days	(3) Household weekly labor earnings	(4) Household weekly days worked	(5) Household weekly days worked in self-employment	(6) Household weekly days worked in non-self-employment	(7) Household daily wage from casual labor
Drop border districts							
Log(HH exposure ratio) x Post 2010	-73.203**	-187.797**	-14.848*	-0.020	0.061	-0.081**	-1.524**
	(28.571)	(83.726)	(7.770)	(0.045)	(0.051)	(0.041)	(0.732)
High HH exposure x Post 2010	-278.412**	-935.026***	-62.877**	-0.018	0.356*	-0.374**	-6.746**
	(125.186)	(310.193)	(30.850)	(0.187)	(0.211)	(0.170)	(3.267)
Observations	113,346	105,801	113,346	113,346	113,346	113,346	38,800
Control for distance to Andhra Pradesh X round							
Log(HH exposure ratio) x Post 2010	-87.748***	-220.348***	-20.123***	-0.035	0.010	-0.045	-1.584**
	(26.944)	(79.336)	(7.096)	(0.045)	(0.055)	(0.042)	(0.744)
High HH exposure x Post 2010	-347.189***	-1,067.981***	-84.904***	-0.080	0.181	-0.261	-6.736**
	(120.966)	(307.325)	(29.350)	(0.189)	(0.220)	(0.170)	(3.358)
Control mean, round 68	5334.8	3870.1	768.4	10.6	6.8	3.8	139.0
Observations	119,668	111,692	119,668	119,668	119,668	119,668	41,264

Note: Data from NSS rounds 64, 66, 68. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table B.3. Robustness: State-by- calendar month controls

VARIABLES	(1) Household average total monthly expenditure	(2) Household expenditure on durables, last 365 days	(3) Household weekly labor earnings	(4) Household weekly days worked	(5) Household weekly days worked in self-employment	(6) Household weekly days worked in non-self-employment	(7) Household daily wage from casual labor
Control for state X month							
Log(HH exposure ratio) x Post 2010	-89.081***	-214.914***	-18.209**	-0.045	0.048	-0.093**	-1.607**
	(24.884)	(72.745)	(7.175)	(0.043)	(0.053)	(0.042)	(0.682)
High HH exposure x Post 2010	-355.4***	-1,052.8***	-77.518***	-0.120	0.317	-0.437**	-6.875**
	(115.335)	(283.883)	(29.363)	(0.183)	(0.213)	(0.173)	(3.108)
Observations	119,668	111,692	119,668	119,668	119,668	119,668	41,264

Note: Data from NSS rounds 64, 66, 68. In all columns, controls include state-specific calendar month fixed effects, round fixed effects, district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table B.4. Robustness: Political party controls

VARIABLES	(1) Household average total monthly expenditure	(2) Household expenditure on durables, last 365 days	(3) Household weekly labor earnings	(4) Any non-Ag Self Employment	(5) Household weekly days worked	(6) Household daily wage from casual labor (ag)	(7) Household daily wage from casual labor (non-ag)
Log(HH exposure ratio) x Post 2010	-76.275**	-123.091	-13.085*	-0.000	-0.001	-1.004	-3.446***
	(30.345)	(98.978)	(7.445)	(0.003)	(0.051)	(0.794)	(1.229)
High HH exposure x Post 2010	-270.133**	-703.298**	-61.064**	-0.001	0.066	-4.249	-12.188**
	(129.735)	(355.533)	(29.609)	(0.014)	(0.214)	(3.260)	(5.293)
Observations	119,668	111,692	119,668	119,668	119,668	14,554	14,939

Note: Data from NSS rounds 64, 66, 68. In all columns, controls include state-specific calendar month fixed effects, round and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round and contols for the party affilion of the state's chief minister in 2010*round. Standard errors are clustered at the district level.

Table B.5. Placebo Test: "Treatment" in Round 66

	(1) HH Weekly Labor Earnings	(2) Casual Daily Wage: Ag	(3) Casual Daily Wage: Non-Ag	(4) HH Weekly Days Worked	(5) HH Monthly Consumption: Total	(6) HH Annual Consumption: Durables
Log(HH Exposure Ratio) x Post 2008	-1.872	-0.272	-0.930	-0.014	-7.352	16.798
	(5.769)	(0.422)	(0.952)	(0.056)	(22.916)	(41.793)
Any exposed lender x Post 2008	-2.124	-0.859	-4.081	-0.046	-91.584	188.152
	(22.777)	(1.870)	(3.999)	(0.226)	(91.533)	(194.295)
Observations	83,826	11,791	9,953	83,826	83,826	75,850

Note: Data from NSS rounds 64, 66, 68. In all columns, controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table B.6. Robustness: Heterogeneous Covariates as Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Business with >6	Land Quintiles				
	Employees	1	2	3	4	5
Log(HH Exposure Ratio) x Post 2010	0.001	-0.003	0.005	-0.001	0.002	0.000
	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Any exposed lender x Post 2010	0.003	-0.008	0.015	-0.001	0.011	0.002
	(0.002)	(0.013)	(0.014)	(0.012)	(0.010)	(0.007)
Observations	71,297	119,668	119,668	119,668	119,668	119,668

Note: Each cell corresponds to a different regression. The top row uses the continuous exposure indicator, while the second row uses the binary exposure indicator. Data from NSS rounds 64, 66, 68. Controls include month, round, and district fixed effects, HH size, number rural HH * round, num rural HH^2 * round, presence of MF in 2008 dummy * round, GLP quintiles in 2008 dummies * round. Standard errors are clustered at the district level.

Table B.7. Correlates of Crisis Exposure

Variable	Low-exposure mean	High-exposure mean	Difference	Obs
Log of average monthly expenditures	8.252	8.130	-0.079**	35,455
Log of value of expenditures on durables last 365 days	6.428	6.414	0.066	35,455
Household weekly earnings from working	699.8	623.0	-40.006	35,455
Household weekly earnings from self-employment	377.8	353.4	-32.794*	35,455
Household weekly earnings from non-self-employment	322.0	269.6	-7.212	35,455
Household weekly days worked	10.84	11.73	0.940***	35,455
Household weekly days worked in self-employment	7.660	8.196	0.207	35,455
Household weekly days worked in non-self-employment	3.178	3.532	0.733***	35,455
Household daily wage from casual labor	112.3	85.34	-19.750***	11,388
Percent of household weekly earnings from self-employment	0.317	0.307	-0.041***	16,909
2008 GLP without SKS scaled by number of households (rural)	38.30	40.02	1.444	35,455
2010 GLP without SKS scaled by number of households (rural)	261.9	498.1	191.031***	35,455

Note: Each row reports the variable mean for low-exposure and high-exposure districts separately, and reports the raw difference. Data from NSS round 66. Standard errors are clustered at the district level.