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 in general equilibrium. Using a proprietary, hand-collected district-level data set from 27 separate, for-profit 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. We also find that wages in the non-tradable sector fall more than in the tradable sector (agriculture), suggesting that one important impact of the microfinance contraction was through the effect on aggregate demand.

1. INTRODUCTION

A rich theoretical literature illustrates multiple channels through which improvements in financial intermediation can result in increased incomes and economic growth (Aghion and Bolton 1997, Greenwood and Jovanovic 1990, Evans and Jovanovic 1989, Banerjee and Newman 1993, Lloyd-Ellis et al. 2000). A more recent empirical literature documents that unexpected periods of credit tightening can have substantial impacts on the real economy. In the context of the US financial crisis, two important channels emerge linking the credit and labor markets. First, constrained firms may decrease labor demand in response to a negative shock to credit supply, leading to a fall in wages and employment (Chodorow-Reich 2014). Second, consumers may decrease demand for goods and services when hit with a shock to housing values, and a subsequent tightening of their borrowing constraints.

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Such a decrease in aggregate demand, in turn, can also lead to a fall in labor demand, putting further downward pressure on wages and employment, especially in the tradable sector (Mian and Sufi 2014).

In this paper, we seek to measure the equilibrium impacts of a large contraction in the supply of microcredit in India. While the loans are typically very small (approximately \$200), microcredit nevertheless plays an important, dual, role. It serves as a vehicle for financing investments in microenterprises for some and also allows other households to accelerate consumption, especially the purchase of durables. Thus, both types of channels diagnosed in the US financial crisis may, in principle, be at play in the context of Indian microfinance.

We explore whether a change in access to such small loans can have detectable effects on the rural economy. To do so, 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 PradesFh, India issued an emergency ordinance, bringing microfinance activities in the state to a complete halt and causing a nationwide 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 separate, for-profit 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 and where individuals did not systematically default on their outstanding loans. Thus, this natural experiment is a unique opportunity to study large labor-market level supply shocks to microfinance credit supply in a setting where there were no concurrent demand shocks.

We find that the reduced form impacts of the reduction in microcredit are large enough to affect the labor market. 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. Consequently, the reduction in credit supply causes a decrease in wage labor earnings for the average rural household. We also find that households experience statistically significant reductions in both non-durable and durable consumption.

We provide evidence that our results are consistent with both the aggregate demand channel and the microenterprise investment channel. First, we decompose the wage effect into agricultural and non-agricultural wages. While agricultural products are tradable and should not respond very intensively to changes in local demand, non-agricultural businesses mainly engage in services, construction, or petty trading, all of which are non-tradable and sensitive to changes in local demand. The aggregate demand channel should lead to larger impacts on the non-tradable sector relative to tradables. Indeed, we find that the wage response in high exposure districts is almost three times larger for non-agricultural wages than for agricultural wages.

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

We provide a battery of robustness tests in support of our identification assumptions. First, we replicate the approach of Khwaja and Mian (2008) to further support our claim

[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.

that our identification strategy captures a change in credit supply, rather than credit demand. Indeed, in districts with multiple lenders pre-crisis, we show that within a district loans from exposed lenders fell relative to credit from unexposed lenders. We also use the NSS 70th round "Debt and Investment" survey to obtain a measure of household's total credit portfolios.

Our main reduced form findings are robust to a number of 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. Our results remain virtually unchanged when we control for travel times, as measured in Allen and Atkin (2016) and are robust to sequentially dropping each state from the analysis. 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. We also can add controls for rainfall realizations as well as differential time trends by baseline economic characteristics.

Finally, 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.

The paper is directly related to active debate on the role of microfinance as a tool for business growth and poverty reduction. A recent wave of papers use RCTs to measure the partial equilibrium impacts of microcredit expansions. 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) 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. Increased access to microfinance in partial equilibrium is generally found to cause modest business creation and business expansion. While there is evidence that borrowers do purchase more household durables and business assets, there is almost no support for a large impact on business profits or on non-durable consumption one to two years post intervention.[2] In a quasi-experimental study, Kaboski and Townsend (2012) find a large short-run consumption response to an expansion of village credit in Thailand, consistent with many households using the loan proceeds for consumption.

[2]In a meta-analysis of the RCT evidence Meager (2016) confirms this general appraisal of small, positive, but undetectable effects on most key outcomes.

There is also evidence that the uses of and the returns to microcredit are very heterogeneous. Banerjee et al. (2015a) show that while any impacts are undetectable for the majority of households six years after the randomized introduction of microfinance in Hyderabad, the subset of households with a pre-existing business experience large and persistent increases in assets, revenues and inputs.

Our study differs from the RCT studies in several key ways. First, we study the removal of credit from an economy rather than its addition. Impacts may be larger from an unexpected credit contraction if households had made previous investments that were only profitable with the arrival of more credit. Second, the shock we study is large enough to plausibly affect equilibrium in the labor market, and it reduced the supply of credit for the average, rather than the marginal, borrower years after the introduction of the product. Banerjee et al. (2015d) write:

> We have only scratched the surface of identifying spillover and general equilibrium effects ... Nonborrowing wage earners could benefit from increased employment opportunities.

To our knowledge, we are among the first to empirically measure the equilibrium impacts of a microfinance supply shock. We build on the theoretical work of Buera et al. (2014) who develop an equilibrium theory of microfinance and note that, due to the wage channel, the effects of microfinance can be quite different in partial equilibrium--holding wages constant--than in general equilibrium. In a related contribution, Ahlin and Jiang (2008) also show theoretically that microfinance can affect the wage via occupational choice and can have large impacts for low-wealth households while potentially reducing incomes for high-wealth households via the wage effect.

More broadly, the paper is 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.[3] 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.

This paper is also 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)); similarly, Bustos et al. (2016) show that a positive bank

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

liquidity shock is passed on to Brazilian firms. 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, and Mobarak and Rosenzweig (2014) show that rainfall index insurance can affect the level of volatility and wages.

Our paper proceeds as follows. Section 2 discusses the setting and data. We describe our empirical strategy in Section 3. Section 4 presents our results, and Section 5 discusses the results in relation to the RCT literature and discusses the breakdown between PE and GE. Section 6 concludes.

2. Setting and Data

2.1. **Setting.**

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 stated that it was worried about widespread over-borrowing by its citizens and alleged abuses by microfinance collection agents. The crisis received media coverage in both local and international newspapers. On October 28, 2010, the Wall Street Journal ran the headline “India’s Major Crisis in Microlending: Loans Involving Tiny Amounts of Money Were a Good Idea, but the Explosion of Interest Backfires.” Other voices in the microfinance debate claimed that the government was using regulation to promote its own preferred financial inclusion initiative, bank-financed self-help groups (SHGs).[4] On November 4, 2010, the Harvard Business Review Published an article entitled “India’s Microfinance Crisis is a Battle to Monopolize the Poor.”

[4]In Section 4.1, we use two different data sources to check whether SHGs were able to offset the decrease in microcredit.

Regardless of the origins of the Ordinance (promulgated as a law in December 2010), its provisions brought the activities of MFIs in the state to a complete halt. Under the law, MFIs are not permitted to approach clients to seek repayment and are further barred from disbursing any new loans.[5] In the months following the ordinance, almost 100% of borrowers in AP defaulted on their loans.[6] Furthermore, Indian banks pulled back tremendously on their willingness to lend to any MFI across the country. The effects of the AP microfinance crisis can be seen in the aggregate country-wide patterns displayed in Figure 1. Using data from the Microfinance Information Exchange (MIX), the figure shows that total microfinance loan portfolios fell by over one billion dollars following the crisis.[7] The figure also shows that lending did begin to recover in 2013.

What is important for this paper is that lending even in areas outside of Andhra Pradesh was affected by the crisis. Notably, the shock in AP was transmitted to other districts through the balance sheets of the lenders – that is, MFIs with high exposure to the defaults in AP were forced to reduce their lending in other states that were not directly affected. In general, they were not able to secure additional financing from the Indian banks to maintain their desired levels of lending.

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 have anticipated that their lender would not be able to give them more credit immediately upon full repayment. In conversations with executives from six different lenders, we learned that many MFIs did go to great lengths to actively manage the expectations of borrowers. In many cases, individuals were able to observe the delayed loan disbursements of peers. In these cases, the loan officers played a significant role in explaining the delays and answering borrower questions. The representatives from the MFIs believed that the continuous presence of the loan officers in the villages gave borrowers comfort in knowing that they would eventually be given new loans.

2.2. **Data.** We use data from several sources in our empirical analysis.

Hand-Collected MFI Data. The first requirement our proposed analysis of the AP crisis is a measurement of district-level balance sheet exposure to Andhra Pradesh pre- October 2010. Because no commonly available datasets contain such information, we partnered

[5]However, it is not illegal for borrowers to seek out their lenders to make payments.
[6]We investigate the effects of this "windfall" in a companion paper (Banerjee et al., 2014a).
[7]Note that the crisis hit the lender's loan portfolio with a lag. Given the year-long maturity of most microloans, it took twelve months for the loans to fully default. Further, many MFIs waited to write off their non-perfoming loans. Additionally, many lenders report annual data at the end of the fiscal year, which in India is often early in in the calendar year.

with MFIN, the primary trade organization of for-profit MFI-NBFCs (non-bank, financial corporations). MFIN allowed us to ask each of their 42 members for district level balance sheet snapshots from 2008 to 2012; 27 of MFIN's 42 member organizations agreed to share their data for the study.

Given that we do not have the whole universe of Indian lenders, we explore the sample composition. We are able to cross-check our sample with the aggregate data that many forms choose to report to MIX Market, an online repository of information about global microfinance.[8] Of the 27 MFIs in our sample, 22 of them report to the MIX and comprise 36% of all reporting for-profit lenders in India.[9] Our sample represents approximately 18% of the total microfinance market by loan volume. Appendix Table B.1 gives more information about the selection of reporting firms into the sample. Observe that the reporting firms are quite a bit smaller than the non-reporting firms, with more clients and more efficient operations in terms of borrowers per staff member. This is not surprising given that the largest several lenders in India chose not to participate in our study.[10] However, the loan-level details look much more similar between reporting and non-reporting institutions; the average loan sizes are very similar (between \$150 and \$160) and are not statistically different, and the default rates (measured as write-offs and 30-day portfolio at risk) are also quite similar (and low). In contrast, the non-MFIN members are even smaller and have significantly higher default rates.

Based on the final MFI data set, 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. In order to calculate the level of exposure of each district to the AP crisis, we proceed as follows. 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):

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

[8]Unfortunately, MIX only reports yearly data aggregated at the MFI level, and cannot be used to construct district-level lending patterns.

[9]Non-profit lenders represent a very small slice of total loan volume.

[10]This is likely because the larger lenders had more outside equity holders and wanted to maintain data privacy and also had the most to fear from negative press coverage. It is also likely that these lenders were large enough to have their own lobbying activities during the crisis and relied less on the activities of MFIN.

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, $GLP_{dl,Oct2010}$:

$$ExpAP_d^{Total} = \frac{\sum_l fracAP_l \times GLP_{dl,Oct2010}}{\sum_l GLP_{dl,Oct2010}}. \tag{2.1}$$

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 2.1) 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:

$$ExpAP_d = ExpAP_d^{Total} \times \frac{\sum_l GLP_{dl,Oct2010}}{RuralPop_{2010}} \tag{2.2}$$

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 2.2) 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.[11] 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

[11]As discussed below in Section 4.1, 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.

casual daily wage averages INR 200.[12] 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.

Auxiliary Data Sources. Finally, throughout our analysis we introduce several outcomes and covariates from several complementary data sources. We describe the sources of those variables when we introduce the empirical specifications and results, below.

3. 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

$$y_{idt} = \alpha + \delta_t + \delta_d + \beta \times Exposure_d \times Post_t + X'_{idt}\gamma + \varepsilon_{idt} \tag{3.1}$$

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 2.2) 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 37.29%; 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

[12] 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.

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.

4. Results

4.1. **First Stage.** Table 2 presents the first stage, estimated by equation 3.1 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 29,500,000 (295 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 95,200,000 (952 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 89 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 278 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 294, 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.[13]

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. Note that if the AP-exposed MFIs cluster in the same exogenously-determined places then that would lead to a problem of scaling the reduced form estimates, as the implied IV estimates would be too large. However, the reduced form estimates would still be measuring a causal treatment effect.

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

[13]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.

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.[14] 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.[15]

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.[16] In columns 1

[14]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.

[15]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.

[16]MFI balance sheet controls include levels and quintiles of GLP measured in 2008. RBI controls include amount of credit outstanding and number of accounts for agricultural loans, direct loans, and indirect loans.

and 3 we consider impacts on the narrow definition of microfinance, SHG-NBFC.[17] 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.

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 4.4, below, we discuss the implications of this exercise for the scaling of the reduced form results.

4.2. **Reduced Form 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 wage of INR 11.5, significant at the

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.

[17]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.

1% level, which is displayed in row 2 of column 1. This represents roughly a 8% reduction from the unexposed district mean of INR 147. We next ask if this decrease in wage affected total household labor supply and total labor earnings. Column 2 shows that there are no detectable effects on total days worked in self-employment and wage employment combined. However, column 3 shows that household days worked in casual daily wage labor did decrease. Given that wages and paid days worked both fell, this leads to an overall decline in household weekly labor market earnings of INR 101 in exposed districts relative to unexposed districts after the AP crisis, significant at the 1% level (column 4). 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), although the point estimate is positive.

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. We instead 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 116 in per capita monthly total expenditures in 2012 (significant at 1%). Column 1, row 2 indicates that those districts with an AP-exposed lender have INR 419 lower per capita monthly total 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 89 (significant at 1%), and row 2 shows that those districts with an AP-exposed lender have INR 299 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 20 (significant at 1%), and row 2 shows that those districts with an exposed lender have INR 92 lower per capita annual durable expenditure (significant at 1%).

Impacts on Agricultural Output. We next show the effect of the AP crisis-induced credit crunch on wages, separately by type in Table 7.

Column 1 shows the pooled wage result across both genders and sectors, which is identical to that of Table 5. Column 2 focuses on the pooled treatment effect across sectors, but focusing only on men. The results look very similar. Columns 3 and 4 further disaggregate the men's wage into the agricultural daily wage (tradables) and the non-agricultural daily wage (non-tradable), respectively. We find that the effects on the wage are significant in both types of industries, but the wage effects are much stronger for non-agricultural work. Recall that this is consistent with the predicted impacts of a negative shock to labor demand on the labor market. Finally, we consider changes in women's wages in columns 5 and 6. We note that the treatment effect on women's agricultural wages is very similar in magnitude to that received by men. However, the effect on non-agricultural wages is not statistically significant. Note, however, the very small sample size of women earning non-agricultural daily wages in the NSS.

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.[18] We also consider each crop separately. The results appear in Table 8. We find no effects, suggesting possible offsetting effects on net labor demanders and suppliers.

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. This also suggests that even the production of tradables was affected adversely by the microcredit contraction, which is consistent with the direct effects of agricultural

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

producers scaling back farm investments, or a failure of complete integration of Indian agricultural markets.

4.3. **Robustness checks.** We next provide evidence to rule out several key threats to identification.

Placebo regression. To provide support for the identifying assumption, Table 9 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. Moreover, the point estimates are all much smaller in magnitude than those of the main regressions and can be statistically distinguished from the main treatment effects. This suggests that pre-existing differential trends cannot explain our main results.

Geographical Distance to AP. Recall that exposure to the AP crisis is correlated with distance to Andhra Pradesh. Thus, if places closer to AP systematically had different (worse) economic trends post 2010, then our identification strategy would be compromised. Moreover, it is also conceivable that the direct fallout of the AP crisis could have "spilled over" onto nearby districts through channels other than the MFI balance sheet effect we measure (economic uncertainty, decreased trade, etc.). We propose and run several tests to check that our results are not simply capturing such a spurious effect.

In Table 10, we conduct three robustness exercises using distance measures to Andhra Pradesh. First, in Panel A, we rerun our main specification, but drop districts with a geographical border with Andhra Pradesh. Second, in Panel B, we instead include the geographical distance of each district from AP, interacted with survey round. While the district fixed effects control for time invariant correlates with distance, this specification allows for differential trends by distance. In both panels, the results look very similar to those in our main specification.

The raw geographical distance may not adequately capture some types of relationships between districts, such as trade costs. As an alternate distance measure, we add the inter-district travel times measured in Allen and Atkin (2016) interacted with survey round to the main regression. Panel C of Table 10 displays the results. Again, we find that adding this alternate measure of distance interacted with time does not change our results in any substantive way.

Finally, we also conduct Altonji-type tests, systematically dropping each state from the analysis. Appendix Table B.3 presents the results. Here we find that no single state is driving the results, even those bordering AP. This again provides support of our identification strategy.

Political Affinity with AP. While distance and trading relationships represent the most serious threats to identification, we also investigate whether the direct shock to AP may have spread to other places with similar political ideologies. Table B.4 tests for the possibility that states with greater exposure to the crisis may have been more "aligned" with Andhra Pradesh through 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.

Rainfall and Other Economic Conditions. Finally, we also check that our results are not coming from time-varying differences in other economic characteristics. First, we check that the differences between high and low exposure districts after 2010 are not coming from differences in rainfall. While rainfall is exogenous, we could have gotten unlucky with the correlation between realized rainfall post-2010 and the patterns of exposure to the AP crisis. We construct an index for abnormal rainfall using the methodology of Jayachandran (2006). If anything, we find that highly exposed regions actually had slightly better rainfall realizations after the crisis. Such a positive correlation cannot explain the negative effect on wages and consumption that we find in our main specification. In Appendix Table B.5, we again run our main specification, including time varying rainfall realizations and find no major differences with our main results.

Finally, in Appendix Table B.7 we allow for differential trends by baseline economic conditions. For example, we allow districts with different levels of baseline poverty, casual wage, or self-employment to evolve differently. Such differential trends again cannot explain our findings.

4.4. **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 **??** 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.

Heterogeneity: Peak labor demand. If agricultural 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.6 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.

5. Discussion

5.1. **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[19]–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

[19]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.

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 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. 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 larger in the non-agricultural (tradable sector) labor market than in the agricultural (tradable) market.

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, and earnings. Indeed this episode shows that microfinance, despite the small loan sizes, can have meaningful impacts on rural economies.

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

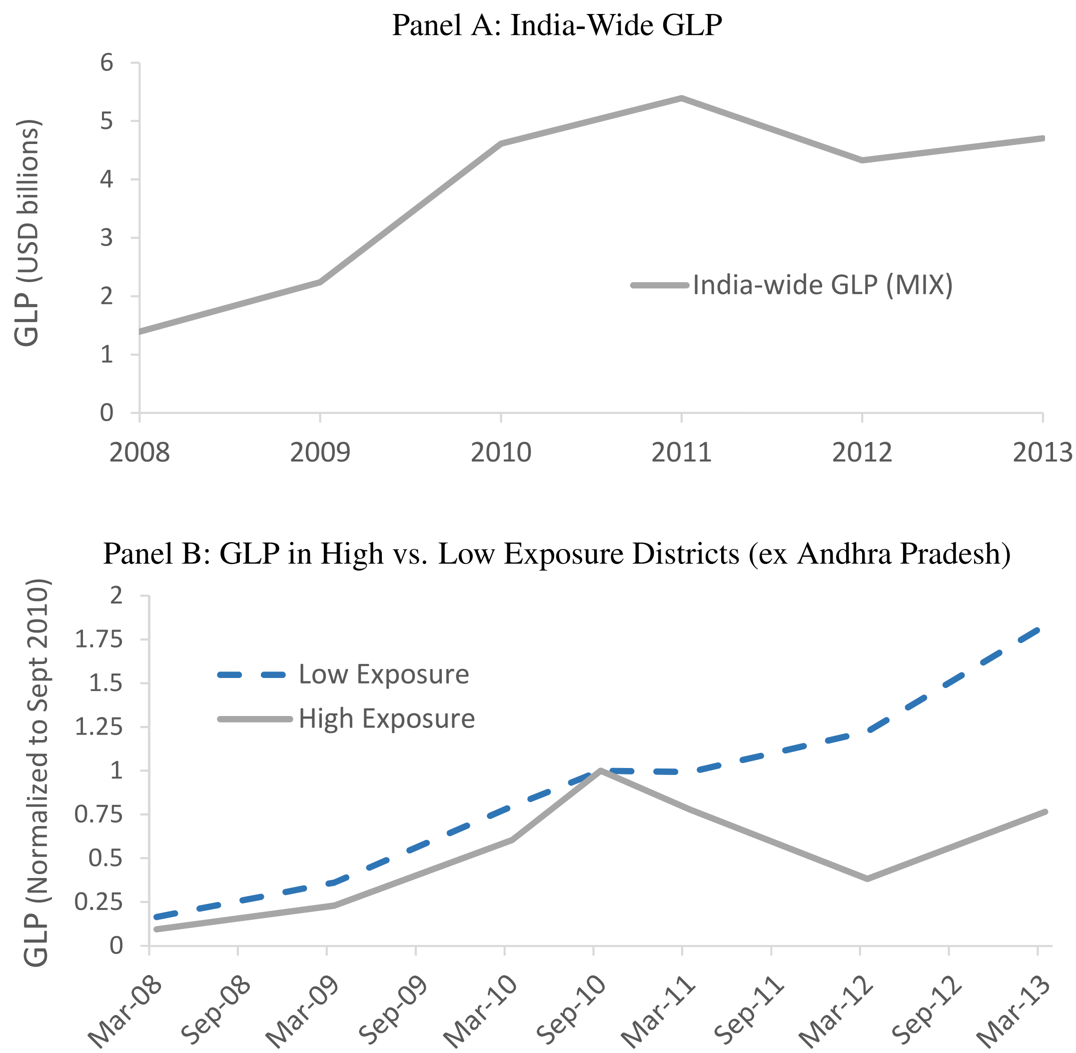
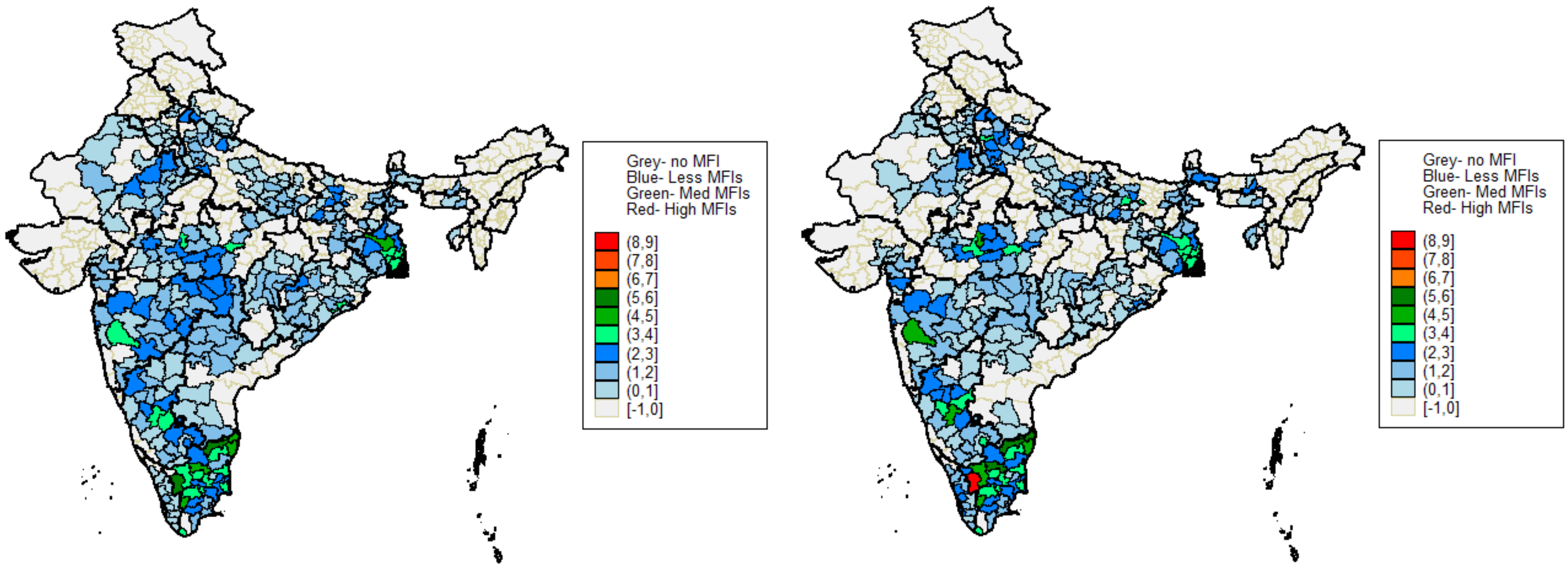


FIGURE 1. Growth of Microfinance Gross Loan Portfolio (GLP) in India and in Analysis Sample

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.

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
Gross loan portfolio in lakhs (100,000 INR)	354	838.84	1462.60
Gross loan portfolio, per rural household	354	269.64	478.12
Household-level variables from NSS (round 68)			
HH Weekly Labor Earnings	22603	807.14	1403.28
Casual Daily Wage: Ag	1664	135.03	55.07
Casual Daily Wage: Non-Ag	3312	185.68	98.36
HH Weekly Days Worked: Total	22603	10.36	6.91
Household weekly days worked in self-employment	22603	6.69	7.22
Household weekly days worked in non-self-employment	22603	3.68	5.27
Any HH Member Invol. Unemployment	22603	0.09	0.29
HH size	22603	4.60	2.24
Number HH earners	22603	1.46	0.31
HH Monthly Consumption: Total	22603	5576.21	4368.65
HH Monthly Consumption: Durables	22603	365.30	1699.10
Any non-Ag. Self Employment	22603	0.27	0.45

Note: Outcomes 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(Exposure Ratio) × Post 2010	-294.745***	-89.084***
	(38.336)	(10.795)
Any exposed lender × Post 2010	-952.055***	-277.891***
	(150.519)	(40.767)
Control mean	1207.5	293.6
Control SD	1763.9	480.5
Observations	1048	1048

Note: Outcomes data from MFI balance sheets. 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. 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 round and district fixed effects, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010 dummies*round. Standard errors are clustered at the district level.

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

	(1) Total No. accounts ('000)	(2) Total Amt outstanding ('000 Rs)	(3) Log Total Amt outstanding ('000 Rs)	(4) Total Amt outstanding ('000 Rs)
Log(HH Exposure Ratio) × Post 2010	-0.007**	-0.803	0.015	-0.911
	(0.003)	(0.690)	(0.016)	(0.580)
Any Exposed Lender × Post 2010	-0.017	-2.706	0.085	-2.962
	(0.012)	(2.318)	(0.061)	(2.047)
Control mean	0.285	25.559	2.883	25.222
Control SD	0.240	33.708	0.902	23.837
Observations	1048	1048	1048	1048

Note: Outcomes data from RBI "District-Wise Classifiication 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round . In column (4), the dependent variable is winsorized at the 99th percentile. 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**	-1319.222***	-300.405***	-1319.384***	-1642.650	-3247.449
	(109.581)	(400.632)	(100.115)	(393.074)	(2189.598)	(3404.538)
Control mean	398.467	2175.315	347.200	2103.205	26777.250	68472.690
Control SD	7397.667	17090.174	4099.224	14004.112	98994.735	141525.190
Observations	38492	38492	38492	38492	38492	38492

Note: Otcomes 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 coefficients from separate regressions using the continuos 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. In all columns, we included 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, daily wage, and percent of weekly earnings from self-employment. Standard errors are clustered at the district level.

TABLE 5. Reduced Form: Labor Outcomes

	(1) Casual Daily Wage	(2) HH Weekly Total Days Worked	(3) HH Weekly Casual Days Worked	(4) HH Weekly Labor Earnings	(5) Any HH Member Invol. Unemployed	(6) Any non-Ag Self Employment
Log(Exposure Ratio) × Post 2010	-3.132***	-0.006	-0.092*	-26.780***	0.003	-0.003
	(0.734)	(0.048)	(0.048)	(7.432)	(0.003)	(0.003)
Any exposed lender × Post 2010	-11.474***	0.008	-0.421**	-100.920***	0.009	-0.012
	(3.143)	(0.195)	(0.187)	(29.215)	(0.012)	(0.013)
Control mean	147.388	10.364	3.450	807.137	0.089	0.272
Control SD	79.720	6.910	5.250	1403.284	0.285	0.445
Observations	40584	119668	119668	119668	119668	119668

Note: Outcomes data from NSS rounds 64, 66, 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round . Standard errors are clustered at the district level.

TABLE 6. Reduced Form: Consumption Outcomes

	(1) HH Monthly Consumption: Total	(2) HH Monthly Consumption: Nondurables	(3) HH Monthly Consumption: Durables
Log(Exposure Ratio) X Post 2010	-115.779***	-89.231***	-20.367***
	(27.269)	(23.595)	(5.912)
Any exposed lender X Post 2010	-418.923***	-299.326***	-92.143***
	(119.974)	(105.171)	(24.108)
Control mean	5576.211	5210.909	365.302
Control SD	4368.648	3613.775	1699.098
Observations	119668	111692	111692

Note: Outcomes data from NSS rounds 64, 66, 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round. Standard errors are clustered at the district level.

TABLE 7. Reduced Form: Casual Daily Wages

	(1) Casual Daily Wage: Pooled	(2) Casual Daily Wage: Men	(3) Casual Daily Wage: Men, Ag	(4) Casual Daily Wage: Men, Non-ag	(5) Casual Daily Wage: Women, Ag	(6) Casual Daily Wage: Women, Non-ag
Log(Exposure Ratio) × Post 2010	-3.132***	-3.381***	-1.731**	-5.786***	-1.842**	-0.750
	(0.734)	(0.859)	(0.775)	(1.396)	(0.715)	(1.723)
Any exposed lender × Post 2010	-11.474***	-12.333***	-6.562**	-19.559***	-5.978*	-3.290
	(3.143)	(3.533)	(3.268)	(5.693)	(3.302)	(7.528)
Control mean	147.388	159.188	135.032	185.676	101.777	115.510
Control SD	79.720	82.700	55.067	98.358	43.687	59.465
Observations	40584	29493	14554	14939	8890	2201

Note: Outcomes data from NSS rounds 64, 66, 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round. Standard errors are clustered at the district level.

TABLE 8. Reduced Form: Crop Yields

	(1) Crop Yield Index	(2) Rice Yield	(3) Wheat Yield	(4) Jowar Yield	(5) Sugarcane Yield	(6) Groudnut Yield
Log(HH Exposure Ratio) × Post 2010	-0.009	-0.024	-0.021	-0.004	-2.954**	0.022
	(0.017)	(0.038)	(0.031)	(0.026)	(1.251)	(0.023)
Any Exposed Lender × Post 2010	-0.024	-0.092	-0.154	0.007	-6.644	0.072
	(0.081)	(0.135)	(0.112)	(0.090)	(5.953)	(0.097)
Control mean	1.044	1.429	1.202	0.402	30.599	0.669
Control SD	0.286	1.495	1.642	0.587	39.970	1.007
Observations	802	802	802	802	802	802

Note: Outcomes data from the Ministry of Agriculture, Directorate of Economics and Statistics. Index in column 1 is a weighted average of log yield for the five major crops: rice, wheat, sugarcane, jowar (sorghum), and gorundnuts. Yields in columns2-6 are in tonnes per hectare. In all columns, controls include month, round, and district fixed effects, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round. Standard errors are clustered at the district level.

TABLE 9. Robustness: Placebo

	(1) HH Monthly Cnsumption: Total	(2) HH Monthly Consumption: Durables	(3) HH Weekly Labor Earnings	(4) HH Weekly Casual Days Worked	(5) Casual Daily Wage
Log(HH Exposure Ratio) × Post 2008	-5.954	1.468	-1.727	-0.007	-0.358
	(22.074)	(3.471)	(5.853)	(0.050)	(0.369)
Any Exposed Lender × Post 2008	-92.096	15.608	-2.606	0.009	-1.455
	(89.665)	(16.186)	(22.959)	(0.197)	(1.630)
Observations	83826	75850	83826	83826	30506

Note: Outcomes data from NSS rounds 64 and 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, and GLP quintiles in 2008 dummies*round.

TABLE 10. Robustness: Distance to Andhra Pradesh

	(1) HH Monthly Cnsumption: Total	(2) HH Monthly Consumption: Durables	(3) HH Weekly Labor Earnings	(4) HH Weekly Casual Days Worked	(5) Casual Daily Wage
Panel A: Drop border districts					
Log(HH Exposure Ratio) × Post 2010	-103.888***	-20.118***	-23.681***	-0.072	-2.973***
	(30.498)	(6.371)	(7.998)	(0.046)	(0.812)
Any Exposed Lender × Post 2010	-351.815***	-89.401***	-85.591***	-0.343*	-10.672***
	(128.444)	(25.539)	(30.311)	(0.182)	(3.361)
Observations	113346	105801	113346	113346	37774
Panel B: Control for distance to AP × round					
Log(HH Exposure Ratio) × Post 2010	-113.522***	-21.405***	-27.419***	-0.050	-3.346***
	(28.332)	(6.095)	(7.433)	(0.047)	(0.745)
Any Exposed Lender × Post 2010	-407.914***	-96.050***	-102.820***	-0.268	-12.145***
	(123.215)	(25.015)	(29.252)	(0.182)	(3.215)
Observations	119668	111692	119668	119668	40584
Panel C: Control for travel time to Hyderabad × round					
Log(HH Exposure Ratio) × Post 2010	-128.343***	-25.680***	-30.047***	-0.062	-2.957***
	(39.382)	(8.843)	(10.382)	(0.062)	(0.908)
Any Exposed Lender × Post 2010	-476.147***	-110.225***	-111.185***	-0.334	-10.150***
	(164.349)	(36.067)	(38.914)	(0.241)	(3.880)
Observations	67939	63071	67939	67939	22868

Note: Outcomes data from NSS rounds 64, 66, 68. In each panel, 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round . In the first panel, the sample is restricted to districts that share no border with Andhra Pradesh. In the second panel, the linear distance to Andhra Pradesh interacted with the round is also included as control. In the third panel, we use instead the road travel time between provinces as calculated by Allen and Atkin (2016). Standard errors are clustered at the district level.

Online Appendix

Appendix A. Framework Overview: Investment-Driven Equilibrium Effects

Here, we present a simple static general equilibrium model of the rural economy. In this model, microfinance only affects investment, and not consumer demand for goods and services. This exercise allows us to investigate one of the two key channels in isolation. 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.[20]

A.1. **Model Environment.** For parsimony, we consider a static environment in which households each have access to a decreasing returns production technology $y_i = AK_i^{\alpha}L_i^{\beta}$, with$\alpha + \beta < 1$.[21] 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, $L_i^S + L_i^H = \bar{T}$. Note that in this simple environment with inelastic labor supply, utility maximization is equivalent to total income maximization.[22] Our static model also abstracts from consumption smoothing motives, as our low frequency, annual data do not allow us to measure consumption variance as an outcome. Moreover, the typial contract structure of microfinance, with inflexible regular payments and annual maturities, is not well suited for consumption smoothing.[23]

[20]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.

[21]This is a standard assumption in the literature (e.g., Banerjee and Newman (1993), Buera et al. (2014)). Moreover, the maximal scale of rural agricultural businesses is often constrained by land holdings.

[22]If we allow labor supply to be endogenously determined with households maximizing $u(c_i, l_i)$, subject to the budget constraint $c_i \leq \pi_i + wL_i^S$, where π_i are business profits, our results are qualitatively similar for reasonable Frisch elasticities.

[23]See Field et al. (2013) for a discussion of the rigidity of microfinance contracts.

The main modeling choice is how to introduce both household heterogeneity and credit constraints. We assume that households are heterogeneous in wealth endowments. Given that in the data, we are not able to observe total household wealth, we instead capture wealth heterogeneity with land, D_i. In what follows, for simplicity, 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.

We note that an alternate type of credit constraint might entail screening by lenders on total factor productivity (TFP). However, the wealth-based borrowing constraint that we use better captures several of the salient features of the Indian microfinance market in a parsimonious manner.. First, in our setting, rural households have limited access to any source of formal credit, as banks typically only offer loans collateralized by land holdings. Second, MFIs in our setting do not actively screen on TFP, but instead screen on wealth. Low-wealth individuals are typically screened out from access to microfinance.[24] MFIs also tend to screen out potential borrowers who are "too rich."[25] 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. Finally, consistent with lenders not screening on productivity, Banerjee et al. (2015a) finds substantial heterogeneity in the returns to microfinance.[26]

To close the model, the labor market must clear in equilibrium. 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.

A.2. **Household Maximization.** Holding factor prices w and r fixed, households choose total labor, capital and borrowing to maximize business profits:

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

[24]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.

[25]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.

[26]Kaboski and Townsend (2011) also find results consistent with substantial heterogeneity in the returns to a rural credit expansion in Thailand.

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.*

See Online Appendix B for details.

A.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.

A.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.[27] 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.

[27]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.

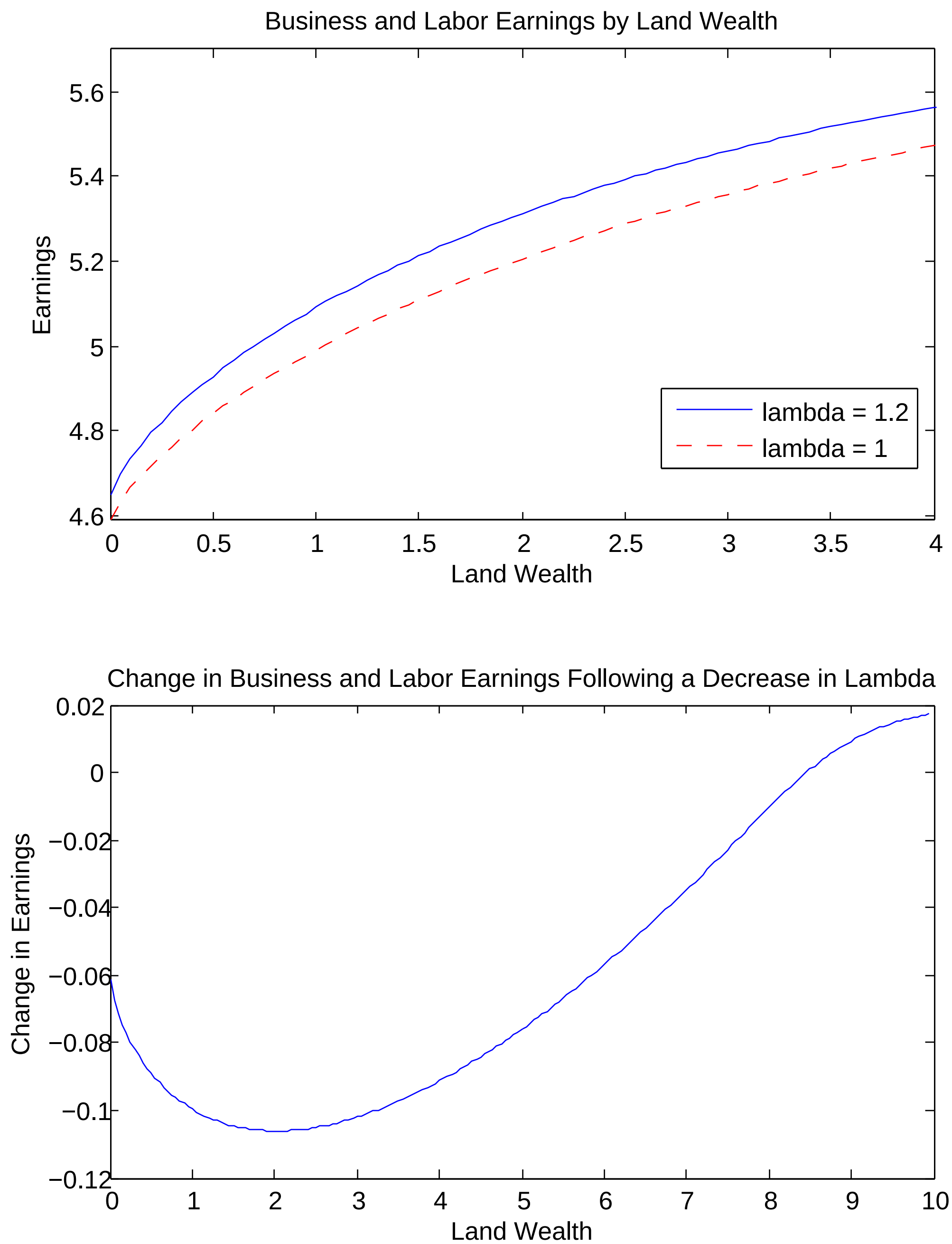


FIGURE 3. Modeled Earnings under High $(\lambda = 1.2)$ and Low $(\lambda = 1)$ Credit Supply Regimes

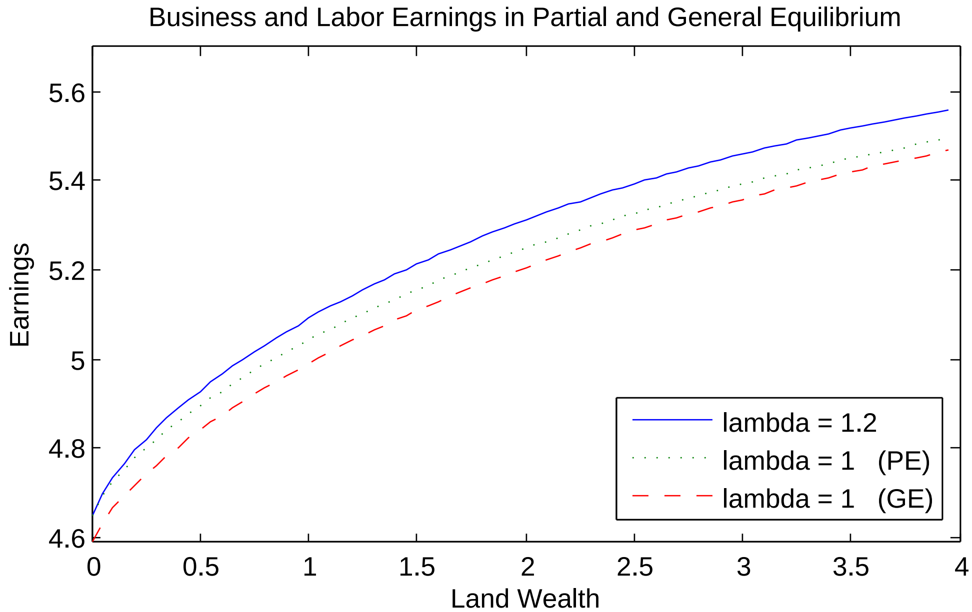
The constrained, net labor demanders are hit hardest by the decrease in credit supply. These households become more constrained and are forced to operate their businesses at a smaller scale. For those households that continue to be net demanders of labor, the loss is partially offset by the decrease 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.[28]

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 modeling the business investment channel predicts monotonically decreasing treatment effects with wealth for labor market earnings and U-shaped treatment effects on credit, business profits, total household earnings, business investment, and both durable and non-durable consumption. Given microfinance's dual role as financing both investment and consumer credit, this is one piece of the measured treatment effect.

A.5. **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.

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



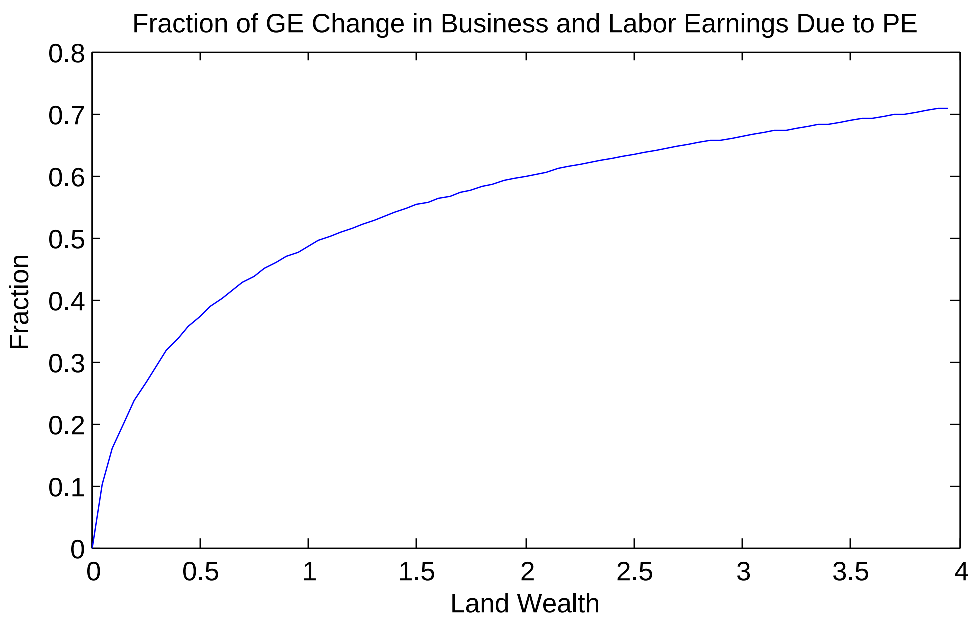


FIGURE 4. Earnings in Partial vs. General Equilibrium

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.[29] Intermediate-wealth

[29]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.

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 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.

Appendix B. Framework Proofs: Investment-Driven Equilibrium Effects

B.1. **Proof of Proposition 1.** Households optimize:

$$\max_{L,K} AK^{\alpha}L^{\beta} - rK - wL$$

s.t.

$$rK + wL \leq \lambda D_i$$

Denote by μ the Lagrange multiplier associated to the borrowing constraint. Hence, the first order conditions are given by:

$$\begin{aligned} L_i: \quad \beta A L_i^{\beta-1} K_i^{\alpha} &= (1+\mu)w \\ K_i: \quad \alpha A L_i^{\beta} K_i^{\alpha-1} &= (1+\mu)r \end{aligned}$$

which implies the following labor to capital ratio:

$$\frac{L}{K} = \frac{\beta r}{\alpha r}$$

There are two cases to consider:

(1) If the household has sufficent wealth ($D > \hat{D}$), then she is not constrained and demands:

$$K^* = \left(\frac{A\alpha}{r}\right)^{\frac{1-\beta}{1-\alpha-\beta}} \left(\frac{A\beta}{w}\right)^{\frac{\beta}{1-\alpha-\beta}}$$

$$L^* = \left(\frac{A\alpha}{r}\right)^{\frac{\alpha}{1-\alpha-\beta}} \left(\frac{A\beta}{w}\right)^{\frac{1-\alpha}{1-\alpha-\beta}}$$

(2) If the household has lower wealth such that the solution above would violate her borrowing constraint, then factor demands are given by:

$$K = \frac{\alpha}{\alpha+\beta}\frac{\lambda D}{r}$$

$$L = \frac{\beta}{\alpha+\beta}\frac{\lambda D}{w}$$

Notice that a household is constrained as long as

$$D < \hat{D} = (\alpha+\beta)\frac{A}{\lambda}\left(\frac{\beta r}{\alpha r}\right)^{\frac{\beta}{1-\alpha-\beta}}$$

Assuming that households inelastically supply $T < L^*$ units of labor, then unconstrained households are net demanders of labor. This does not need to be the case for constrained households, which supply for the market $L^{out} = max\{T - L, 0\} = max\{T - \frac{\alpha}{\alpha+\beta}\frac{\lambda D}{w}, 0\}$. Therefore, a household is a net supplier of labor if $D < D^* = \frac{(\alpha+\beta)wT}{\alpha\lambda}$.

B.2. **Proof of Proposition 2.** The equilibrium wage is characterized by market clearing in the labor market:

$$L_d(w,\lambda)\int_0^{\hat{D}} = \frac{\alpha}{\alpha+\beta}\frac{\lambda D}{w}dG(D) + \left[1 - G(\hat{D})\right]\left(\frac{A\alpha}{r}\right)^{\frac{\alpha}{1-\alpha-\beta}}\left(\frac{A\beta}{w}\right)^{\frac{1-\alpha}{1-\alpha-\beta}} = T = L_s$$

Since the left-hand side is increasing in λ and decreasing in w while the right-hand side is not affected by neither of these variables, then the relationship between λ and w is positive. Formally, since $\frac{\partial L_d(w,\lambda)}{\partial w} < 0$ and $\frac{\partial L_d(w,\lambda)}{\partial \lambda} > 0$, then by the Implicit Function Theorem $w'(\lambda) = -\frac{\frac{\partial L_d(w,\lambda)}{\partial \lambda}}{\frac{\partial L_d(w,\lambda)}{\partial w}} > 0$.

Appendix C. Supplementary Tables

TABLE B.1. Selection into the MFI Data Sample

	(1) MFIs in the Analysis Sample	(2) Non-Reporting MFIN Members	(3) For-Profit, MFIN Non-Members	P-value (1) vs. (2)	P-value (1) vs. (3)
Average Loan Size per Borrower ($)	153.30	163.29	163.95	0.37	0.72
	(30.05)	(31.37)	(135.73)		
Number of Borrowers	201,517	1,276,501	58,282	0.02	0.02
	(275357)	(1714844)	(77347)		
Borrowers per Staff Member	193.17	404.57	185.76	0.01	0.83
	(115.38)	(311.67)	(107.76)		
Indicator for Above-Median Age	0.30	0.50	0.52	0.24	0.13
	(0.47)	(0.52)	(0.51)		
Write-Off Ratio (%)	0.38	0.45	1.62	0.91	0.26
	(0.52)	(0.46)	(4.18)		
30-Day Portfolio at Risk (%)	0.73	1.21	14.83	0.93	0.12
	(0.76)	(2.72)	(33.9)		
N	23	14	22		

Note: Information taken from MIX Market for 2010. Standard errors are clustered at the MFI level.

TABLE B.2. Estimation of the Bank Lending Channel: GLP per rural household

Panel A: Levels	(1)	(2)	(3)	(4)
Fraction of Exposed GLP	-290.047*		-293.440*	
	(159.028)		(158.118)	
Exposed MFI Dummy		-155.250*		-157.207*
		(81.009)		(80.211)
Logarithm of MFI Size			-9.218***	-9.268***
			(2.924)	(2.907)
Observations	422	422	422	422

Panel B: Logs	(1)	(2)	(3)	(4)
Fraction of Exposed GLP	-2.872***		-2.886***	
	(0.626)		(0.636)	
Exposed MFI Dummy		-1.202***		-1.210***
		(0.377)		(0.378)
Logarithm of MFI Size			-0.037*	-0.037*
			(0.022)	(0.023)
Observations	422	422	422	422

Note: Outcomes data from MFI balance sheets. In each column, the dependent variable is the difference of per rural household GLP by the MFI to the district between September 2010 and March 2012. In columns (1) and (3), exposure of the MFI is captured by the share of its portfolio in Andhra Pradesh as of September 2010. In columns (2) and (4), we use a exposure dummy equal to one if the MFI operates in Andhra Pradesh. All specifications include district dummies. In columns (3) and (4), the logarithm of the total portfolio of the MFI is also used as control. The sample is restricted only to MFI-district pairs with positive GLP. Standard errors are clustered at the district level.

TABLE B.3. Robustness: Sequentially Exclusion of States

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Excluding:	AS	BR	CG	GJ	HR	HP	JH	KA	KL
Total consumption:									
Log(HH Exposure Ratio) × Post 2010	-117.7***	-119.9***	-116.2***	-110.9***	-110.6***	-115.8***	-116.7***	-109.5***	-95.6***
	(27.2)	(27.9)	(27.5)	(27.3)	(27.2)	(27.3)	(27.7)	(30.0)	(26.4)
Any Exposed Lender × Post 2010	-426.5***	-443.3***	-418.7***	-386.2***	-398.2***	-418.9***	-421.3***	-375.9***	-316.8***
	(119.9)	(124.1)	(120.8)	(120.0)	(119.6)	(120.0)	(121.6)	(126.6)	(115.8)
Labor earnings:									
Log(HH Exposure Ratio) × Post 2010	-27.0***	-24.2***	-25.2***	-26.8***	-26.2***	-26.8***	-24.7***	-31.7***	-24.3***
	(7.4)	(7.6)	(7.5)	(7.5)	(7.5)	(7.4)	(7.3)	(7.5)	(7.6)
Any Exposed Lender × Post 2010	-101.7***	-90.4***	-95.3***	-100.0***	-98.9***	-100.9***	-93.3***	-110.3***	-88.3***
	(29.3)	(30.2)	(29.4)	(29.7)	(29.4)	(29.2)	(28.8)	(29.3)	(29.7)
Observations	119412	115856	118756	117471	118229	119668	118612	114937	114675

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Excluding:	MP	MH	OD	PB	RJ	TN	UP	UK	WB
Total consumption:									
Log(HH Exposure Ratio) × Post 2010	-106.4***	-135.1***	-109.8***	-112.3***	-124.6***	-119.9***	-126.5***	-115.9***	-121.6***
	(28.0)	(30.2)	(27.9)	(27.2)	(27.0)	(26.8)	(28.0)	(27.3)	(28.2)
Any Exposed Lender × Post 2010	-397.6***	-508.0***	-373.3***	-403.1***	-449.0***	-438.7***	-468.8***	-421.2***	-458.3***
	(122.8)	(125.8)	(124.2)	(119.6)	(120.8)	(119.9)	(126.4)	(120.6)	(127.5)
Labor earnings:									
Log(HH Exposure Ratio) × Post 2010	-23.4***	-29.8***	-24.9***	-26.3***	-26.6***	-26.6***	-29.9***	-26.7***	-29.9***
	(7.7)	(7.6)	(7.5)	(7.4)	(7.6)	(7.8)	(7.6)	(7.4)	(7.5)
Any Exposed Lender × Post 2010	-88.7***	-109.5***	-91.9***	-98.8***	-100.6***	-104.9***	-122.1***	-100.8***	-115.8***
	(29.7)	(29.9)	(30.1)	(29.3)	(29.9)	(29.8)	(30.0)	(29.2)	(30.3)
Observations	114651	109971	114983	118844	113512	117188	107536	119138	112646

Note: Outcomes data from NSS rounds 64, 66, 68. In each panel, 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round. In the first panel, the dependent variable is monthly total expenditures. In the second panel, the dependent variable is weekly labor earnings. In each column, observations in the state indicated at the top of the column are excluded from the sample; all other 17 states are included.

TABLE B.4. Robustness: Political Party

	(1) HH Monthly Cnsumption: Total	(2) HH Monthly Consumption: Durables	(3) HH Weekly Labor Earnings	(4) HH Weekly Casual Days Worked	(5) Casual Daily Wage
Log(HH Exposure Ratio) × Post 2010	-104.082***	-13.378*	-20.406**	-0.032	-2.604***
	(32.190)	(7.044)	(8.398)	(0.054)	(0.801)
Any Exposed Lender × Post 2010	-330.905**	-66.938**	-79.679***	-0.205	-9.516***
	(132.962)	(26.864)	(30.621)	(0.213)	(3.273)
Observations	119668	111692	119668	119668	40584

Note: Outcomes data from NSS rounds 64, 66, 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round, and the party affiliation of the state prime-minister in 2010*round.

TABLE B.5. Robustness: Rainfall

	(1) HH Monthly Cnsumption: Total	(2) HH Monthly Consumption: Durables	(3) HH Weekly Labor Earnings	(4) HH Weekly Casual Days Worked	(5) Casual Daily Wage
Log(HH Exposure Ratio) × Post 2010	-104.646***	-17.092***	-26.126***	-0.095**	-2.861***
	(26.548)	(5.426)	(7.668)	(0.048)	(0.733)
Any Exposed Lender × Post 2010	-375.462***	-80.105***	-98.090***	-0.431**	-10.361***
	(117.755)	(22.396)	(30.144)	(0.188)	(3.104)
Observations	119668	111692	119668	119668	40584

Note: Outcomes data from NSS rounds 64, 66, 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round, GLP in 2010*round , and rainfall shocks. Rainfall data is from the Global Precipitation Climatology Centre (GPCC) and rainfall shocks are calculated as in Jayachandran (2006): if rainfall in a year is above the 80-th percentile of the rainfall distribution from 1950-2014, then the rainfall shock equals 1; if it is below the 20-th percentile, the rainfall shock equals -1; otherwise, its value is zero.

TABLE B.6. Heterogeneity: Peak agricultural labor demand periods

	(1) HH Monthly Cnsumption: Total	(2) HH Weekly Labor Earnings	(3) HH Weekly Casual Days Worked	(4) Casual Daily Wage: Ag	(5) Casual Daily Wage: Non-Ag
Peak labor demand periods					
Log(HH Exposure Ratio) × Post 2010	-141.764***	-48.835***	-0.234***	-2.715**	-6.507***
	(45.202)	(11.727)	(0.080)	(1.251)	(2.457)
Any Exposed Lender × Post 2010	-518.807***	-150.748***	-0.800***	-11.663**	-21.155**
	(185.121)	(49.556)	(0.304)	(5.346)	(10.083)
Observations	25625	25625	25625	2515	3482
Non-peak labor demand periods					
Log(HH Exposure Ratio) × Post 2010	-105.388***	-27.491***	-0.119*	-1.789*	-6.924***
	(35.596)	(10.216)	(0.061)	(1.024)	(1.736)
Any Exposed Lender × Post 2010	-359.322**	-103.227***	-0.544**	-6.561	-23.682***
	(149.525)	(38.156)	(0.230)	(4.030)	(6.717)
Observations	94043	94043	94043	12039	11457

Note: Outcomes data from NSS rounds 64, 66, 68. 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, quintiles of HH size, number rural HH*round, num rural HH squared*round, presence of MF in 2008 dummy*round, and GLP quintiles in 2008 dummies*round, GLP in 2010*round .

TABLE B.7. Robustness: Economic Conditions

	(1) HH Monthly Consumption: Total	(2) HH Monthly Consumption: Durables	(3) HH Weekly Labor Earnings	(4) HH Weekly Casual Days Worked	(5) Casual Daily Wage
Panel A: consumption in round 66 × round					
Log(HH Exposure Ratio) × Post 2010	-73.564***	-12.702**	-19.809**	-0.075	-3.106***
	(27.625)	(5.529)	(7.736)	(0.050)	(0.737)
Any Exposed Lender × Post 2010	-231.575*	-59.640***	-70.256**	-0.352*	-11.544***
	(121.120)	(21.997)	(30.753)	(0.201)	(3.151)
Observations	119668	111692	119668	119668	40584
Panel B: Poverty head count × round					
Log(HH Exposure Ratio) × Post 2010	-108.953***	-19.803***	-25.304***	-0.086*	-3.120***
	(27.138)	(5.883)	(7.508)	(0.050)	(0.736)
Any Exposed Lender × Post 2010	-385.576***	-90.193***	-93.930***	-0.392*	-11.479***
	(119.525)	(24.092)	(29.765)	(0.201)	(3.136)
Observations	119668	111692	119668	119668	40584
Panel C: Casual wage in round 66 × round					
Log(HH Exposure Ratio) × Post 2010	-125.428***	-21.910***	-28.373***	-0.084	-3.245***
	(27.888)	(6.171)	(7.421)	(0.048)	(0.745)
Any Exposed Lender × Post 2010	-458.614***	-98.706***	-107.514***	-0.385**	-11.902***
	(124.520)	(25.207)	(29.160)	(0.188)	(3.173)
Observations	119668	111692	119668	119668	40584
Panel D: Share of self-employment × round					
Log(HH Exposure Ratio) × Post 2010	-102.093***	-20.983***	-25.926***	-0.088*	-3.108***
	(27.582)	(5.904)	(7.420)	(0.047)	(0.728)
Any Exposed Lender × Post 2010	-365.078***	-94.658***	-97.424***	-0.405**	-11.442***
	(122.081)	(24.445)	(29.020)	(0.184)	(3.129)
Observations	119668	111692	119668	119668	40584

Note: Outcomes data from NSS rounds 64, 66, 68. In each panel, 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 esposure to AP. In all columns, controls include month, round, and district fixed effects, quintiles of HH size, number rural HH*round, num rurallHH squared*round, presence of MF in 2008 dummy*round, GLP quintiles in 2008 dummies*round. In each panel, additional controls are included: average district consumption in round 66*round (first panel), district proverty head count in round 66*round (second panel), average casual wage in agriculture in round 66*round (third panel), and district share in self-employment*round (fourth panel) . Standard errors are clustered at the district level.