

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

EMILY BREZA AND CYNTHIA KINNAN

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 nationwide shock to the liquidity of lenders, especially those 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, earnings, and employment in general equilibrium in rural labor markets. Using a proprietary district-level data set from 25 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 find a substantial consumption multiplier from credit that is likely driven by two channels—aggregate demand and business investment. We calibrate a simple two-period, two-sector model of the rural economy that incorporates both channels and show that the magnitude of our wage results is consistent with the model's predictions. *JEL* Codes: D5, E24, E51, G21, O16.

I. INTRODUCTION

What is the equilibrium impact of microfinance? Understanding this question is highly relevant: microfinance is presently one of the only sustainable and scalable vehicles for providing formal credit to poor households who lack physical collateral, and the industry has grown from 23 million to 140 million borrowers worldwide since 2000. Accordingly, understanding the impact of microfinance is important for numerous policy questions, with implications for regulation (licensing policy, interest rate caps, and determining who should regulate the sector) and subsidy

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policy.¹ Although the partial equilibrium impacts of microfinance are increasingly well understood, it has been more challenging to gain traction on the question of how these effects translate to large-scale changes in microfinance access that would bring equilibrium forces to bear.

Importantly, microfinance at scale may have multiplier effects through two distinct channels. First, to the extent that individuals consume the proceeds of their microloan, microfinance may increase aggregate demand.² Despite the policy goal of directing microfinance toward entrepreneurship, many borrowers use microfinance as a consumption loan (Kaboski and Townsend 2012; Devoto et al. 2012; Tarozzi et al. 2014; Ben-Yishay et al. 2017). Second, microfinance may stimulate business investment and labor demand. While a series of randomized controlled trials shows only moderate short-run effects of microcredit on business outcomes (Banerjee, Karlan, and Zinman 2015), recent work suggests that the initially modest impacts of microfinance persist and grow over time, especially for incumbent businesses (Banerjee et al. 2020; Beaman et al. 2020). Moreover, most studies are not designed to detect effects on nonborrowers, which could be a key driver of multiplier effects.

Through the aggregate demand and business finance channels, access to microfinance has the potential to push up local wages, affecting the local labor market more broadly. The ability of bank credit to affect labor markets has been shown in the United States (Chodorow-Reich 2014; Mian and Sufi 2014), and the theoretical potential for microfinance to have a wage effect has been highlighted by Buera, Kaboski, and Shin (2021). However, to our knowledge, our article is the first to show empirically that small loans to poor borrowers can in fact move the needle in rural labor markets.

Through expenditure, hiring, and wage impacts, microfinance may give rise to a substantial consumption multiplier. Recent evidence from Kenya documents a large multiplier from cash transfers (Egger et al. 2020), and we might expect multipliers

1. For instance, India subsidizes microfinance through its priority sector designation, which allows the sector to access cheap capital.

2. Note that loans give rise to aggregate demand impacts at the time of origination, when the proceeds are spent. However, most studies do not measure consumption impacts until a year or more after loan disbursal, a timing driven by interest in measuring business impacts. We return to this issue in Section II.D.

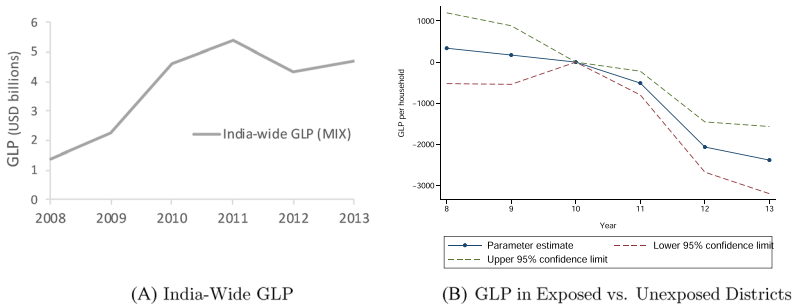


FIGURE I

Growth of Microfinance Gross Loan Portfolio: India and Analysis Sample

Panel A plots the India-wide gross loan portfolio (GLP) aggregated across microfinance institutions and states as reported in US\$ in the MIX database. The effects of the crisis are most visible in 2012 rather than 2010/11 because 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. Panel B shows the evolution of microfinance using the balance sheet data (reported in INR) from 25 microfinance institutions. Panel B uses the panel of data across all years reported to us by the participating MFIs. We restrict observations to districts with any microfinance loans outstanding in 2008.

from loans to be even larger in similar contexts. By definition, those taking loans are choosing to bring resources forward in time, so this implies that their marginal utility of funds today exceeds the marginal utility of funds in the future. Hence loan proceeds are likely to be spent/invested immediately, rather than held as low-return savings; we return to this point in [Section IV.F](#).

To quantify the impacts of microfinance in equilibrium, we study a unique natural experiment that caused a large, exogenous shock to microfinance access at the level of whole labor markets. This unique setting is important as the scale of an RCT is rarely large enough to generate quantitatively large infusions of credit across a large number of markets. In October 2010, the state government of Andhra Pradesh, India, issued an emergency ordinance, bringing microfinance activities in the state to a halt and causing a wave of defaults and a nationwide shock to lenders' liquidity. The aggregate gross loan portfolio (GLP) of Indian microlenders fell by approximately 20%, or more than \$1 billion, between fiscal year 2010 and fiscal year 2011. [Figure I](#), Panel A plots India-wide levels of microlending from 2008 to 2013. The drop in lending after 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 collected district-level administrative data from 25 for-profit microlenders detailing their loan portfolios from 2008 through 2013. We combine this information 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 of 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 Andhra Pradesh (AP) before the crisis, interacted with precrisis variation in the geographical footprint of each lender outside of AP. We show that districts that had borrowed more from lenders with portfolio exposure to AP witnessed much larger declines in lending between 2010 and 2012 than similar districts with the same amount of overall precrisis lending but whose lenders did not have balance sheet exposure to AP. Figure I, Panel B shows that, while prior to the crisis there was no differential trend in lending in exposed versus unexposed districts, after 2010, districts exposed to the crisis experienced large and significant drops in total microcredit lending relative to unexposed districts.

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 AP, which were not directly affected by the ordinance and where individuals did not default on their outstanding loans. This natural experiment is a unique opportunity to study large, exogenous, labor market-level shocks to microfinance credit supply in a setting where there were no concurrent demand shocks.

The effects of this reduction in microcredit were large enough to affect the rural 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 significant reductions in nondurable and durable consumption. The fall in the wage implies that even nonborrowing laborers may experience declines in earnings and consumption when the

local economy is hit by reduced access to credit. Our estimates imply a consumption multiplier of approximately 2.9.³

We find evidence consistent with both the aggregate-demand channel and the business liquidity channel. In support of the aggregate-demand channel, we show that the magnitude of the wage effects on agriculture, where products are tradable and should not respond very intensively to changes in local demand, are roughly half the size of the effects in the nonagricultural sector. Nonagricultural businesses mainly engage in services, construction, or petty trading, all of which are nontradable and sensitive to changes in local demand. In support of the business investment channel, we show that investment declines in response to exposure to the crisis, and moreover that households are less likely to be employers.

We directly examine district-level outcomes for the tradable sector by studying crop yields. On the one hand, firms in the tradable sector, which experiences no adverse shock to demand, will benefit from lower wage bills. On the other hand, some of these firms may be forced to scale back as liquidity constraints bind more tightly. Thus, the net impact on tradable sector output is ambiguous. We find a fairly precise zero effect for an index of yields of major crops. This suggests that in aggregate, any benefits from a fall in the wage are offset by reductions in the scale of production by constrained businesses.

To give context for the magnitude of our results on rural markets, we examine the NSS data on urban areas. The nature of the shock suggests that the effects should be much smaller in urban areas due to deeper credit markets and more labor- and product-market integration. Moreover, MFIs typically focus the bulk of their lending portfolios on rural areas. Consistent with this prediction, we show that in large cities, exposure to the AP crisis did not cause changes to consumption, wages, or labor earnings. Only small market towns and rural areas experienced the negative effects of the credit crunch. In other words, while the effects of the credit contraction were significant for rural markets, this is not the case in more urban areas. We also show that our wage results are consistent with a simple calibration of our model.

We provide a battery of robustness tests in support of our identification strategy. We replicate the approach of [Khwaja and](#)

3. [Egger et al. \(2020\)](#), in comparison, find a consumption multiplier of 2.5 in the case of cash transfers.

Mian (2008) to support the claim that our identification strategy captures a change in credit supply, rather than demand. In addition, we predict the lending footprints of MFIs not in our main sample using the correlation between MFI characteristics and footprint for in-sample firms combined with publicly available portfolio-level information for out-of-sample firms. Using this measure of predicted overall exposure generates similar estimates. Our findings are robust to a number of additional controls, such as proxies for trade, political alignment with AP, and other initial economic conditions. We also show that “placebo” shocks in other states or time periods do not replicate our results.

The article is directly related to an 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, Karlan, and Zinman (2015), Augsburg et al. (2015), Attanasio et al. (2015), Banerjee et al. (2015), Crépon et al. (2015), and Tarozzi, Desai, and Johnson (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. Although there is evidence that borrowers do purchase more household durables and business assets, there is almost no support for a large average effect on business profits or on nondurable consumption one to two years after intervention. In a meta-analysis of the RCT evidence, Meager (2019) confirms this general appraisal of small, positive, but generally statistically undetectable effects on most key consumption and business outcomes. In a quasi-experimental study, Kaboski and Townsend (2012) find a very large short-run consumption response to an expansion of village microcredit in Thailand, consistent with many households using the loan proceeds for consumption. Fink, Jack, and Masiye (2020) show that in Zambia, access to lean-season credit is associated with increased consumption and higher village-level wages, and Burke, Bergquist, and Miguel (2018) show that in Kenya, access to harvest-time credit affects local prices by helping farmers delay grain sales. Moreover, Banerjee et al. (2020) show that the entrepreneurial returns to microcredit are heterogeneous and at a longer time horizons are positive and significant for the average household.

Our study differs from RCT analyses in several ways. Most important is the magnitude and scale of the shock. The AP crisis

moved credit by a large amount, as a percentage reduction in credit and in aggregate: more than \$1 billion was wiped out of the market. Moreover, this shock played out at the level of entire districts, a large enough area to encompass whole labor markets, allowing us to estimate general rather than partial equilibrium impacts. Achieving variation at this scale via an RCT would be extremely challenging. We also study effects on average borrowers in mature markets, as opposed to studying new markets or marginal (complier) borrowers (Beaman et al. 2020). In addition, RCTs may lack power to measure profits for the right tail of the business distribution, yet that tail is likely to have disproportionate influence for aggregate outcomes (see Hsieh and Olken 2014; Meager 2020). An additional possible difference has to do with timing—RCTs often collect postintervention data 18 months or more after disbursement of credit, by which time the initial increase in consumption out of loan proceeds will have subsided. Finally, we study the removal of credit while most RCTs study increases in access to credit.⁴ The effects of addition versus removal of credit may be asymmetric due to, *inter alia*, loss of borrower–lender relationships when credit is reduced, borrowers having made plans that depended on continued access to credit, or changes in precautionary savings. Although we cannot rule out that differences such as the fact that credit was removed rather than added play a role in the difference between our results and the RCT evidence, we show that our calibrated model can replicate the observed treatment effect on wages without introducing an asymmetry between the addition and removal of credit. Our results on credit contractions also speak to the impacts of regulatory tightening, which reduces access to credit.⁵

We attempt to provide complementary evidence to the RCT literature and to fill one of the gaps in the literature highlighted by Banerjee, Karlan, and Zinman (2015, 20): “We have only scratched the surface of identifying spillover and general equilibrium effects ... Nonborrowing wage earners could benefit from increased employment opportunities.”

4. One exception is Banerjee, Duflo, and Hornbeck (2018) who find effects of reduced access to microfinance that are broadly consistent with effects of increased access.

5. Restrictive regulatory actions have also taken place in Nicaragua, Bosnia, and Morocco.

More broadly, the article is related to the literature on financial access for the poor, especially [Burgess and Pande \(2005\)](#), who show evidence that bank expansions decrease rural poverty. This article also builds on the large literature in macroeconomics and finance studying the effects of credit supply shocks and bank balance sheet effects. Many papers, such as [Khwaja and Mian \(2008\)](#), [Paravisini \(2008\)](#), and [Schnabl \(2012\)](#) have shown that in diverse settings, negative shocks to bank liquidity are often passed on to borrowers through reductions in lending. A smaller literature stemming from [Peek and Rosengren \(2000\)](#) traces out effects of such credit supply shocks on real activity.⁶ The finance literature has typically considered credit supply shocks to the traditional banking sector. In contrast, we explore lending by nonbank entities that target loans toward poor, typically rural households. Our article is also related to recent work examining general equilibrium effects of large-scale programs and economic shocks in developing countries.⁷

The article proceeds as follows. In [Section II](#), we describe the setting and our empirical predictions. [Section III](#) discusses the data and empirical strategy. [Section IV](#) presents our main results, and [Section V](#) presents a calibration of a simple two-sector model of the credit contraction to benchmark our results. [Section VI](#) concludes. All appendix material can be found in the [Online Appendix](#).

II. SETTING AND EMPIRICAL PREDICTIONS

On October 15, 2010, the AP government unexpectedly issued an emergency ordinance (The Andhra Pradesh Micro Finance Institutions Ordinance) to regulate the activities of MFIs operating in the state through a suite of new restrictions.⁸ These restrictions placed substantial limits on operations and led to

6. Other related papers include [Chodorow-Reich \(2020\)](#), [Jiménez et al. \(2020\)](#), [Greenstone, Mas, and Nguyen \(2014\)](#), [Mian, Sufi, and Verner \(2020\)](#), and [Bustos, Garber, and Ponticelli \(2020\)](#).

7. See [Imbert and Papp \(2015\)](#), [Muralidharan, Niehaus, and Sukhtankar \(2017\)](#), [Jayachandran \(2006\)](#), [Akram, Chowdhury, and Mobarak \(2017\)](#), [Cunha, De Giorgi, and Jayachandran \(2019\)](#), and [Attanasio and Pastorino \(2020\)](#).

8. Specifically, all NBFC-MFI regulated entities needed to immediately suspend operations and register with the district-level authorities before resuming any disbursements or collections in that district. The ordinance also capped interest rates at 100%, mandated that collections be made in a public place, and

mass defaults in the state of AP, with nearly 100% of borrowers in AP defaulting. Moreover, although there were no significant defaults outside of AP, the liquidity shock nonetheless led to ripple effects through the broader Indian microfinance sector.

II.A. Origins of the Ordinance

The ordinance made three claims: (i) that MFIs charged usurious interest rates; (ii) that they used coercive methods to collect their debts; and (iii) that they exploited self-help groups (SHGs) (Kinetz 2011). The first two claims generally sounded alarms about consumer protection. The third focused specifically on the effects of microfinance on SHGs, a competing government-led financial inclusion program. A debate has emerged in policy circles whether the ordinance was an honest attempt to regulate a rogue industry or a political play to support SHGs and serve clientelistic aims with indebted rural voters.

Microfinance is an inherently difficult financial product to regulate. Thousands of loan officers (typically male) travel independently to rural villages to make collections from borrowers (typically female), and stories of coercive collection practices are not uncommon (Rai 2010). In the months prior to the ordinance, there was intense media coverage of a set of farmer suicides in 2010 in AP that were allegedly linked to MFI overindebtedness (see The Hindu 2010). Moreover, in August 2010, the AP-based MFI SKS raised \$350M of equity capital in an IPO that was 13 times oversubscribed. The high market valuation of SKS raised questions about microfinance's social mission and the ethics of profiting on the backs of the poor (Sriram 2012; Cole and Saleman 2015).

Critics of this consumer protection narrative argue, however, that the government used the MFIs as a convenient scapegoat (Roodman 2010; Taylor 2011).⁹ While microfinance is only one part of household liabilities, the ordinance exclusively targeted MFIs. There was no attempt to rein in coercive practices and high interest rates of loan sharks or other types of financing

prohibited MFIs from lending to members of self-help groups (SHGs) (Cole and Saleman 2015).

9. There is a long history of politicizing farmer suicides in India, and analyses of the financial situations of the households that experienced suicides does not line up with microfinance as the main culprit (Rai 2010; Yerramilli 2012; Rao 2010; Johnson and Meka 2012).

corporations. Moreover, the 100% interest rate cap put in place by the ordinance was far higher than the rates any lenders were charging at the time (approximately 30%). [Cole and Saleman \(2015\)](#) argue that the AP government used the language of the cap to stoke popular outrage.

Critics of the ordinance claim that its primary aim was to reduce competition with the government's rival SHG program ([Yerramilli 2012](#)).¹⁰ AP was India's leader in promoting the SHG model, and the government used the program for political gain in rural communities ([Sriram 2012](#)). Moreover, debt forgiveness of the borrowers from competing MFIs is consistent with other politicized interventions in lending markets ([Cole 2009](#)). The consensus in many policy and academic circles is that politics and anticompetitive motives were the most important causes of the crisis, and AP's role as the poster child for the SHG movement was the driving factor in why the ordinance was issued there. We discuss below how these causes of the ordinance inform our identification strategy.

II.B. Impacts of the AP Ordinance across India

While the ordinance clearly hobbled lending operations in AP, there were almost immediate spillovers to the rest of the country. Specifically, Indian banks, the main source of funding for the sector, largely refused to issue new loans to MFIs across India. These effects can be seen in the aggregate country-wide patterns displayed in [Figure I](#), Panel A. Using data from the Microfinance Information Exchange (MIX), the figure shows that total microfinance loan portfolios fell by over US \$1 billion following the crisis. The figure also shows that lending began to recover in 2013. This timing coincides with the RBI taking over full regulatory authority of for-profit MFIs in mid-2012, foreclosing the possibility of similar ordinances elsewhere.

What is important for this article is that lending even in areas outside of AP 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

10. In fact, the subheading of the ordinance itself is “An Ordinance to protect the women Self Help Groups from exploitation by the Micro Finance Institutions in the State of Andhra Pradesh.”

secure additional financing from the Indian banks to maintain their desired levels of lending. Perhaps surprisingly, the defaults in AP did not spread across the country: individuals continued to make their loan repayments even though they may have anticipated that their lender would not be able to give them more credit immediately on full repayment.¹¹

II.C. Identifying the Role of the Crisis

We use exposure to the AP ordinance as an exogenous shock to the microfinance sector outside of AP, allowing us to identify the impacts of the decline in credit on market-level outcomes. The above discussion suggests some important considerations when constructing our identification strategy.

First, policy makers and MFIs were worried at the time that other states might adopt a similar regulatory stance. Places more at risk for policy contagion include states with more aggressive microfinance lending, more indebtedness, higher penetration of SHGs, and the same governing party as in AP. In our core specifications, we therefore allow for differential trends by the precrisis microcredit level and microcredit growth. We also test the robustness of our findings to inclusion of differential trends by SHG penetration and political party affiliation.

Second, the footprints of the lenders, while predetermined, are not random. As shown in [Online Appendix Table C.I](#), exposed districts are, unsurprisingly, closer to AP. This raises the concern of direct spillovers from AP onto other exposed districts, through channels other than lending. Such spillovers are a priori unlikely: borrowing firms tend to be extremely small in scale and unlikely to be selling across district borders. Tradable goods such as agricultural output are sold in local markets or to traders. Nonetheless, spatial spillovers of some kind may be possible. In our core analysis we allow for differential trends by distance to AP. Moreover, we conduct a number of robustness checks to show that such spatial spillovers are not driving our results.

11. In conversations with executives from six lenders, we learned that many MFIs went to great lengths to manage the expectations of borrowers. The loan officers played a significant role in explaining disbursement delays and answering borrowers' questions. The lenders believed that the continuous presence of loan officers in the villages gave borrowers confidence that they would eventually be given new loans.

II.D. The Impact of Credit in Equilibrium

There are two main channels—investment and aggregate demand—through which changes in microfinance supply might affect labor market equilibrium, and in turn, produce a consumption multiplier.

1. Investment Channel. A rich theoretical literature has analyzed the potential for credit directed at poor households to matter via the channel of business creation and expansion (e.g., [Banerjee and Newman 1993](#); [Buera, Kaboski, and Shin 2021](#); [Ahlin and Jiang 2008](#)). A contraction in credit supply should decrease both investment and labor demand for the firms owned by microfinance borrowers and put downward pressure on wages. Because of the fall in firm profits for borrowing entrepreneurs and wage earnings for workers, a credit contraction should also lower consumption for these groups.¹²

Although the RCT literature finds little short-run evidence of effects of microfinance on business scale or profitability for the average complier, [Banerjee et al. \(2020\)](#) find that over a longer time horizon, the average wage bill more than doubles in neighborhoods treated with access to microfinance, relative to controls, and incumbent firms earn 28% higher profits. Relatedly, [Banerjee, Duflo, and Hornbeck \(2018\)](#) find that when microfinance borrowers stop borrowing (due to a requirement to purchase health insurance), there are negative effects on incumbent businesses. This evidence suggests that a large reduction in credit may impact firm profits and hence hiring through the investment channel.

2. Aggregate-Demand Channel. Microfinance is not only used for businesses. For example, in [Banerjee et al. \(2015\)](#), only 49% of microfinance borrowers have any business one to two years postintervention. Microfinance may be particularly useful in allowing households to purchase lumpy consumption goods that are difficult to save up for (see [Devoto et al. 2012](#); [Tarozi et al. 2014](#); [Ben-Yishay et al. 2017](#)).

12. In contrast, firms in the same labor market that do not borrow from microfinance should benefit from the fall in wages ([Buera, Kaboski, and Shin 2021](#)). This should increase the labor demand of these unconstrained firms, partially offsetting the fall in equilibrium employment.

Moreover, [Kaboski and Townsend \(2012\)](#) find that short-run consumption responds nearly one-for-one with increased credit in Thai villages. It is notable that [Kaboski and Townsend's \(2012\)](#) high-frequency data allow them to measure the consumption response immediately after the increase in credit access, which is precisely when the loan proceeds are spent, and hence, to the extent that some of the spending is on locally priced goods or services, will affect local aggregate demand. Microfinance RCTs, on the other hand, tend to measure postintervention outcomes a year or more later, to coincide with the likely timing of impacts on, say, business profits.

These results imply that there is scope for a market-wide reduction in microcredit to decrease aggregate demand. When individuals borrowing for consumption purposes lose access to credit, their short-run consumption falls, lowering the demand for goods and services. This could lead to a reduction in labor demand, with consequences for equilibrium employment and wages. Note that reduced business spending will also have aggregate demand implications if such spending comprises locally priced assets or working capital.

In [Online Appendix A](#), we use a two-period model to isolate the impact of a decrease in credit supply on wages via the aggregate-demand channel. Our model contains two sectors: tradable goods (e.g., agriculture) can be sold to the national or global market, and nontradable goods (e.g., services or local retailers) can only be sold to the local market and so are locally priced. We show that when tradable sector workers are imperfect substitutes for nontradable sector workers, wages in both sectors fall following a credit contraction, with the nontradable sector wage falling by more.

We focus the model on the aggregate-demand channel for several reasons. One, to our knowledge this channel has received less attention in the context of microfinance. Although theories of the investment channel have long been applied to microfinance, the aggregate-demand channel has received much less attention in the microfinance literature. Second, the active debate about whether microfinance leads to significant effects on business outcomes for the average borrower means there is value in highlighting the role for the aggregate-demand channel. Empirically, the aggregate-demand channel is also much harder to study with partial-equilibrium analysis.

The two channels can interact and amplify one another. For example, if loans taken for business investment are used to purchase locally priced inputs, or if business profits are used to buy locally priced consumption goods or services, this would lead to a stronger reduction in aggregate demand.

3. Empirical Predictions. We next outline a set of empirical predictions, combining both channels. Our goal is not to tease the channels apart, especially because they may have important interactions, but to understand the equilibrium profile of microfinance impacts.

Both channels predict a fall in labor demand in response to a microcredit contraction. This reduction in demand leads to a reduction in wage employment, wage levels, and wage earnings. To the extent the aggregate-demand channel is at play, we should expect a larger decline in wages for nontradable versus tradable labor. In addition, the aggregate-demand channel suggests a fall in expenditure on locally priced goods (e.g., construction). The investment channel is expected to manifest through a reduction in employment and capital expenditure.

There are several forces that put downward pressure on consumption and that could produce a multiplier effect. First, individuals who borrow for consumption experience a direct consumption decrease when credit supply falls. Second, consumption should also fall for workers facing lower earnings. Third, credit-constrained entrepreneurs who borrow from microfinance should experience lower profits from forgoing profitable investments. Finally, if firm profits decline in the nontradable sector due to a decrease in aggregate demand, both borrowers and nonborrowers should experience a decline in profits. The only segment of the market that is insulated from both the investment and the aggregate-demand channel is nonborrowing (unconstrained) entrepreneurs in the tradable sector.

We next detail the data and empirical strategy and present event study figures in support of our identifying assumption.

III. DATA AND EMPIRICAL STRATEGY

III.A. Data

Our goal is to use the district-level, quasi-random variation in microfinance access generated by the AP crisis to obtain causal

estimates of the impact of access to finance outside of AP. First we discuss how we measure exposure to the AP crisis and the data required. We then discuss our outcome data. Finally we discuss our difference-in-differences identification strategy and present evidence in support of the parallel trends assumption. [Online Appendix G](#) provides additional details.

1. Proprietary MFI Data. Our first requirement is a measure of district-level balance sheet exposure to AP before October 2010. Because no commonly available data sets contain such information, we partnered with the Microfinance Institutions Network (MFIN), the primary trade organization of for-profit MFI-NBFCs (nonbank, financial corporations), who make up the bulk of microloan volume. MFIN allowed us to ask their 42 members for district-level balance sheet snapshots from 2008 to 2012; 25 MFIN member organizations agreed to share their data.

Given that we do not observe the universe of Indian MFIs, we explore the sample composition. We can cross-check our sample with the aggregate data that many firms choose to report to MIX Market, an online repository of information about global microfinance. We examine characteristics of MFIs in 2009, the year before the AP crisis. In total, 115 Indian MFIs provide 2009 data to MIX. Of the 25 MFIs in our sample, 21 report to MIX; these make up 36% of all reporting for-profit lenders in India. Our sample represents approximately 18% of the total microfinance market by loan volume.

[Table I](#) examines the selection of reporting firms into the sample. In Panel A, we observe that the reporting firms are smaller: they have fewer borrowers and fewer borrowers per staff member. This is not surprising given that several of the largest lenders in India, who have achieved greater economies of scale, chose not to participate in our study.¹³ However, the loan-level details look much more similar between reporting and nonreporting firms; the average loan sizes are around \$180 and are not statistically different, and the default rates (write-offs and 30-day portfolio at risk) are quite low in both samples (though the 30-day portfolio at risk is lower in the reporting sample).

13. This is likely because the larger lenders had more outside equity holders and wanted to maintain data privacy. They also had the most to fear from negative press coverage.

TABLE I
SAMPLE SELECTION

| | Average loan per borrower (1) | Number of borrowers (2) | Borrowers per staff member (3) | Write-off ratio (4) | Portfolio at risk, 30 days (5) | MFI age (6) |
|---|-------------------------------------|-------------------------------|--------------------------------------|---------------------------|--------------------------------------|-------------------|
| Panel A: Selection into the sample | | | | | | |
| MFI in the sample | -25.164 (21.324) | -34,132.611 (103,178.714) | -72.491** (35.782) | -0.002 (0.002) | -0.017*** (0.006) | |
| Control mean | 176.064 | 245,201.611 | 267.591 | 0.005 | 0.023 | |
| Control std. dev. | 198.024 | 782,389.257 | 256.144 | 0.012 | 0.047 | |
| Observations | 114 | 115 | 113 | 82 | 84 | |
| Panel B: Sampled MFIs, relation to exposure | | | | | | |
| Exposure to AP | -0.750 (8.776) | 166,780.191 (241,593.920) | 72.574 (56.126) | -0.002 (0.002) | 0.002 (0.006) | 0.132 (0.500) |
| Control mean | 152.000 | 177,160.059 | 179.176 | 0.004 | 0.007 | 1.118 |
| Control std. dev. | 29.180 | 215,349.208 | 103.882 | 0.005 | 0.007 | 0.993 |
| Observations | 21 | 21 | 21 | 18 | 18 | 21 |

Notes. Data are from the Microfinance Information Exchange (MIX). In Panel A, the right-hand side variable is a dummy indicating if the institution is in our sample. In Panel B, the right-hand side variable is a dummy indicating if the MFI was exposed to the AP crisis. Of the 25 MFIs in our sample, 21 report to MIX; these make up the sample in Panel B. Write-off ratio and Portfolio at risk are fractions of the overall portfolio. The left-hand side variable in column (6) refers to the number of years before 2010 that the MFI reports positive GLP; it is a measure of age in 2010, top-censored at two years (2008). Because it is available only for sampled MFIs, this dependent variable is used only in Panel B. Robust standard errors are in parentheses. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

In Panel B, we restrict the sample to reporting firms and examine whether the characteristics of firms exposed to the crisis (i.e., firms with loans in AP on the eve of the crisis; see below) have different characteristics than those that are not exposed. Whereas differences between reporting firms and nonreporting firms in Panel A affect the external validity of our results, any differences between exposed and unexposed lenders could pose a threat to internal validity. Reassuringly, exposed and unexposed firms look quite similar in terms of loan size, number of borrowers, borrowers per staff member, write-off ratio, and portfolio at risk. We examine an additional outcome in this sample: the MFI's age, as measured by the first year it reports positive loan volume in our data. (We cannot examine this outcome in Panel A because it is only available for reporting firms.) Exposed and unexposed firms are also similar in this dimension. In [Section IV.A](#) and [Online Appendix E](#) we further explore the robustness of our results to our only observing a subset of MFIs.

Based on the final MFI data set, [Table II](#), Panel A shows that the total 2012 gross loan portfolio of reporting lenders in districts where lenders were not exposed to the crisis is roughly INR 124 million.

2. *Measuring Exposure to the AP Crisis.* 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 AP on the eve of the AP crisis (the beginning of October 2010):

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

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

$$(1) \quad ExpAP_d = \frac{\sum_l fracAP_l \times GLP_{d,l,Oct2010}}{\sum_l GLP_{d,l,Oct2010}}.$$

$ExpAP_d$ 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

TABLE II
SUMMARY STATISTICS

| Variable | Obs. | Mean | Std. dev. |
|--|--------|----------|-----------|
| Panel A: District-level variables from balance sheet data | | | |
| Any exposed lender (all districts) | 354 | 0.35 | 0.48 |
| Exposure ratio (all districts) | 354 | 0.08 | 0.13 |
| Exposure ratio (exposed districts) | 132 | 0.23 | 0.11 |
| Gross loan portfolio in lakhs (INR 100,000, unexposed districts) | 222 | 1,236.39 | 1,819.80 |
| Panel B: Household-level variables from NSS (round 68) | | | |
| HH weekly labor earnings | 16,340 | 854.81 | 1,434.83 |
| Casual daily wage: ag | 1,176 | 140.53 | 58.53 |
| Casual daily wage: non-ag | 2,460 | 194.71 | 107.40 |
| HH weekly days worked: total | 16,340 | 10.28 | 6.74 |
| HH weekly days worked in self-employment | 16,340 | 6.55 | 7.04 |
| HH weekly days worked in non-self-employment | 16,340 | 3.73 | 5.16 |
| Any HH member invol. unemployment | 16,340 | 0.10 | 0.30 |
| Any HH member had NREGA work this week | 16,340 | 0.02 | 0.13 |
| HH size | 16,340 | 4.52 | 2.23 |
| Number of earners in HH | 16,340 | 1.45 | 0.27 |
| HH monthly consumption: total | 16,340 | 5,643.40 | 4,531.70 |
| HH monthly consumption: durables | 16,340 | 382.25 | 1,891.40 |
| Any non-ag. self-employment | 16,340 | 0.22 | 0.41 |
| Panel C: Household-level variables from NSS (round 70) | | | |
| Uncollateralized formal nonbank amt outstanding, win | 15,633 | 2,412 | 13,239 |
| Bank amt outstanding, win | 15,633 | 29,212 | 104,144 |
| Total loan amt outstanding, win | 15,633 | 52,841 | 127,636 |

Notes. Outcome variables in Panel A are from the balance sheet data collected with MFTN; see text for details. Exposed (unexposed) districts are districts with any (no) exposed lender. Outcome variables in Panel B are from NSS round 68 (2012). Outcome variables in Panel C are from NSS round 70 (2014). Household borrowing data in Panel C are winsorized at the 99th percentile.

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, while the other is based in southern India and has 40% of its portfolio in AP. Then $ExpAP_d = \frac{0.4+0}{2} = 0.20$. [Online Appendix Figure C.I](#) shows a map depicting the level of exposure for each district in our sample; summary statistics can be found in [Table II](#).

3. NSS Data. Our primary outcome measures come from the Indian National Sample Survey (NSS). In our core empirical specification, we use household data from waves 64, 66, and 68 of the NSS, which correspond to 2007–2008, 2009–2010, and 2011–2012, respectively.¹⁴ The NSS surveys are designed to be nationally representative, and we use sampling weights in all of our regressions. We focus on the schedules containing household composition, consumption, and employment. Key variables are summarized in [Table II](#). (We summarize the 2012 values in low-exposure districts for ease of comparison to the reduced-form results below.) An average household works 10.28 person-days a week. Those days are split roughly $\frac{2}{3}$ self-employment (typically agricultural) and $\frac{1}{3}$ non-self-employment. Non-self-employment activities are largely casual (nonpermanent) wage jobs. Household total weekly wage earnings average INR 855. The agricultural casual daily wage averages INR 140, and the nonagricultural casual daily wage averages INR 195.¹⁵

The NSS waves 64, 66, and 68 do not contain detailed data on household indebtedness. However, as discussed below, we can use the NSS 70th wave that contains a “Debt and Investment” survey, collected in 2012 and 2013. We use information on household borrowing as of June 30, 2012. The average household in a low-exposure district had INR 2,412 in outstanding microloans ([Table II](#), Panel C). Note that these measures average across borrowers and nonborrowers.

14. We also use data from waves 60, 61, and 62 in our pretrend analysis ([Section III.C](#)).

15. 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. Households only report working in public works in 2% of weeks.

4. *Auxiliary Data Sources.* Finally, throughout our analysis we introduce several outcomes and covariates from several complementary data sources. These cover variables such as rainfall, interdistrict travel times, political party affiliation, and crop yields. We describe the sources of those variables when we introduce the empirical specifications and results below; more detail is available in [Online Appendix G](#).

III.B. Empirical Strategy

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

$$(2) \quad y_{idt} = \alpha + \delta_t + \delta_d + \beta \times Exposure_d \times Post_t + X'_{idt}\gamma + \varepsilon_{idt},$$

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 above); and β is the coefficient of interest. The vector X'_{idt} includes controls for the calendar month of the survey and household size. We also include a number of controls for precrisis district-level outcomes interacted with time dummies; this allows for unrestricted differential time effects that are functions of these precrisis characteristics. These outcomes are: the 2010 rural population of the district at t and its square, dummies for quintiles of 2008 and 2010 gross loan portfolio, linear distance from the district centroid to AP, the district average level of per capita consumption in 2010, and the district average casual daily wage in 2010. Note that we do not observe a household panel but repeated cross sections, which form a district-level panel. Standard errors are clustered at the district level.

We use two measures of exposure to the AP crisis. Our preferred measure is the exposure ratio $ExpAP_d$ (defined by [equation \(1\)](#)). Although this is the most natural measure, emerging directly from the effect of the crisis on lenders, for ease of interpretation we also present a binary indicator for the presence of any lender that had exposure to the AP crisis, $ExpAP_d > 0$. The proportion of districts with positive exposure is 37.3%; the proportion of household-level observations located in these districts is very similar, at 36.9%.

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 controls we include, our identifying assumption is that households in districts with the same rural population, the same levels of total MFI lending in 2008 and 2010, the same distance to AP, and the same 2010 levels of consumption and wages, are on similar trends regardless of whether the MFIs lending in the district were highly exposed to the AP crisis.

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. Given this standardization, the identifying assumption is *a priori* reasonable. Moreover, we present a number of robustness and placebo checks below that lend direct support to this assumption.

As a way to shed further light on our identification strategy, [Online Appendix Table C.I](#) compares baseline characteristics of exposed versus unexposed districts. (Recall that since we use a difference-in-differences strategy, level differences across exposed versus unexposed districts do not in and of themselves pose a concern, but trend differences would be a concern; we discuss this further in the next subsection.) Columns (1) and (2), respectively, examine whether exposed districts are closer to AP or more likely to border AP. Unsurprisingly, they are: MFIs that operated in AP also operated in nearby districts. Accordingly, all of our empirical specifications control for distance to AP. Columns (3)–(7) show that exposed and unexposed districts do not differ in their baseline levels of agricultural or nonagricultural daily wages, weekly labor earnings, or nondurable or durable consumption. In [Section IV.E](#), we will show a variety of checks to rule out that differential trends by distance are driving our results. The results in [Section IV.A](#), which show that all of the fall in credit in exposed districts is driven by exposed MFIs, not unexposed MFIs, provides further support for the identification strategy.

III.C. Pretrend Analysis

As a further test of our identification strategy, [Figure II](#) shows event study figures for our key outcomes, presenting

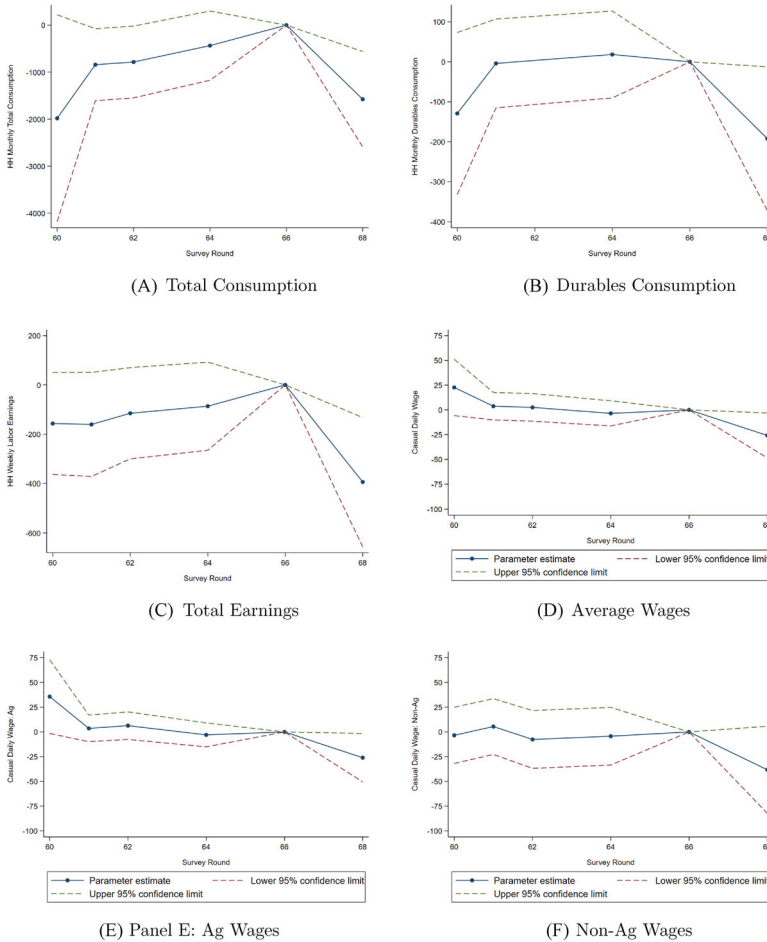


FIGURE II
Pretrends Analysis

Pretrend plots using data from NSS rounds 60, 61, 62, 64, 66, and 68. The plotted estimates with solid connecting lines are year-specific coefficients from regressions of the outcome variable on the continuous exposure measure. Dashed lines represent the 95% confidence intervals. Round 66 is the omitted category. Controls include round and district fixed effects, Round 66 average district consumption, average casual wage and linear distance to AP interacted with round, quintiles of household size, number of rural households and its squared term, GLP quintiles in 2008, and 2010 dummies along with round and survey month fixed effects. Wages and earnings are winsorized at the 99th percentile. Standard errors are clustered at the district level. Observations are weighted by NSS sampling weights. Durable consumption was not asked in NSS round 62.

estimates from the equation

$$(3) \quad y_{idt} = \alpha + \delta_t + \delta_d + \sum_{\tau=60, \tau \neq 66}^{\tau=68} \beta_{\tau} \times Exposure_d \\ \times \mathbf{I}[t = \tau] + X'_{idt} \gamma + \varepsilon_{idt},$$

where $Exposure_d$ is the exposure ratio $ExpAP_d$ defined by equation (1). We use data from NSS rounds 60, 61, 62, 64, 66, and 68. NSS round 66, on the eve of the AP crisis, is the base time period.

If the AP crisis led to an exogenous shock to credit availability in exposed districts, we should see no systematic difference in more- versus less-exposed districts prior to the crisis, that is, before round 66. Changes in outcomes in more- versus less-exposed districts after the AP crisis are then the causal effects of indirect exposure to the crisis. Panel A shows the evolution of consumption. In NSS rounds 60, 62, and 64, collected prior to the AP crisis, there is no systematic difference in the evolution of consumption in more- versus less-exposed districts. Panel B shows that consumption of durables shows the same pattern. Likewise, in Panel C, we examine total earnings, which are not evolving differently in more- versus less-exposed districts prior to the crisis. In Panels D, E, and F, we examine, respectively, overall casual wages, agricultural wages, and nonagricultural wages. For these outcomes, too, there is no precrisis difference between more- versus less-exposed districts.

These figures also visually demonstrate our main results—we observe decreases in total and durable consumption, earnings, and wages in round 68, postcrisis.

IV. RESULTS

IV.A. First Stage

The event study figures already qualitatively demonstrate the key results. However, to improve precision and obtain quantitative estimates of the effect of the crisis, we present estimates of the difference-in-differences specification (equation (2)). For ease of interpretation, we normalize the exposure ratio $ExpAP_d$ to have a unit standard deviation so that the coefficient is the effect associated with a one std. dev. change in exposure to the

crisis, as measured by the precrisis portfolio-weighted exposure of the district's lenders.

Table III, column (1) presents the first stage, estimated by equation (2) with district-level gross loan portfolio per rural household as the dependent variable. The first row shows that those districts with an AP-exposed lender have INR 324 less credit outstanding from the reporting MFIs per rural household in 2012 (significant at 1%).¹⁶ The second row indicates that a one std. dev. increase in exposure to the crisis is associated with INR 171 less credit outstanding from the reporting MFIs per rural household in 2012 (significant at 1%), compared with other similar districts whose lenders were not exposed to the crisis. The regression results match the patterns shown in Figure I, Panel B.

These effects imply that AP-exposed lenders cut back significantly on lending and this shortfall was not made up by other, nonexposed 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 staff. Second, even nonexposed MFIs report having trouble obtaining credit from the Indian banking sector, which traditionally provided most of the funding to the MFIs, due to uncertainty in the aftermath of the AP crisis. 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 have been smaller.

The proprietary balance sheet data is not without limitations. In particular, it represents approximately 18% of the Indian market by loan volume: a number of MFIs declined to share their data with us. This presents a potential difficulty when comparing the reduced-form estimates based on a representative sample of Indian households with the first-stage estimates which use only the responding subsample of MFIs. The impact of the sample selection depends on the correlation between the lending footprints of reporting and nonreporting lenders. For example, if exposed lenders tend to have correlated geographical footprints, then our first stage is likely to be too small, while the reduced form is relatively unaffected, leading to an overestimate of the treatment on the treated (TOT) effect. To see this, imagine

16. This corresponds to a reduction in microlending of roughly US \$2 million per exposed district.

TABLE III
EXPOSURE TO THE AP CRISIS AND TOTAL MFI LENDING

| | District gross loan portfolio per household (1) | MFI amt outstanding (2) | Bank amt outstanding (3) | Total loan amt outstanding (4) | MFI amt outstanding log (5) | Bank amt outstanding log (6) | Total loan amt outstanding log (7) |
|--------------------------------|--|-------------------------------|--------------------------------|---|--------------------------------------|---------------------------------------|--|
| Any exposed lender × post 2010 | -324.631*** (50.480) | -1,296.836*** (389.146) | -815.937 (1,898.591) | -3,286.771 (3,004.950) | -0.634*** (0.159) | 0.123 (0.244) | -0.773** (0.374) |
| Exposure ratio × post 2010 | -170.985*** (23.703) | -626.543*** (185.490) | 465.688 (901.626) | -1,069.412 (1,398.391) | -0.331*** (0.067) | 0.063 (0.115) | -0.355* (0.195) |
| Control mean | 423.496 | 2,394.640 | 29,531.260 | 69,353.672 | -5.360 | -2.641 | 5.476 |
| Control std. dev. | 546.901 | 13,200.690 | 104,467.426 | 142,601.618 | 4.836 | 7.641 | 8.028 |
| Observations | 1,048 | 33,559 | 33,559 | 33,559 | 33,559 | 33,559 | 33,559 |

Notes. Outcome data in column (1) is from MFI balance sheets. Outcome data in columns (2)–(7) are from the NSS 70th round “Debt and Investment” survey reflecting outstanding credit on June 30, 2012. Outcome variables are winsorized at the 99th percentile. Each cell provides coefficients from separate OLS regression specifications. The first row reports coefficients from regressions using the binary indicator for high exposure to the AP crisis. The second row reports coefficients from separate regressions using the continuous exposure measure. The outcome of interest in columns (1) and (4) is total formal, nonbank, noncollateralized credit (i.e., microfinance credit). Total bank credit is shown in columns (2) and (5), and total credit in columns (3) and (6). The dependent variables in columns (1)–(4) are in levels, and outcomes are transformed using a $\log(X + 0.001)$ transformation for columns (5)–(7). All columns include precrisis district-level controls. Balance sheet controls include levels and quintiles dummies for 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 clustered at the district level are reported in parentheses. Observations in columns (2)–(7) are weighted by NSS sampling weights. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

that the footprints of the exposed MFIs we do observe are perfectly predictive of the exposed MFIs we do not observe, and similarly for the unexposed MFIs. Then our exposure instrument, $Exposure_d$ is measured without error—recall that the exposure measure is in percent of total precrisis GLP, not in rupees. In this case the reduced-form regressions will yield unbiased estimates of the true effect. However, the measured first stage will be too small by a factor p , which represents the share of total GLP represented by the MFIs whose balance sheets we observe. If the footprints of exposed and unexposed MFIs are positively but imperfectly correlated, similar reasoning applies.

On the other hand, if there is no correlation between the footprints of responding and nonresponding MFIs, then this will introduce classical measurement error into our measure of exposure which will then “wash out” of implied TOT calculations, which scale the reduced form by the first stage. In this case, the estimate in Table III, column (1) represents an estimate of the true market-wide first stage (modulo measurement error as discussed above). At the other extreme of full correlation, the estimate in column (1) is too small by a factor of $p = 0.18$, implying a market-wide contraction of credit of approximately INR 949 per rural household in response to a one standard deviation increase in exposure.

Consistent with the idea that the footprints of responding and nonresponding MFIs are likely to be correlated, we observe that exposed districts are closer to AP in Online Appendix Table C.I.¹⁷ This suggests that the “true” first stage should be closer to the full correlation scenario.

1. Representative Credit Data. Given that the MFI balance sheet data does not capture the entire market, we instead construct our preferred measure of household microfinance borrowing using the NSS 70th round “Debt and Investment” survey, which measures a household’s total credit outstanding on June 30, 2012. This is an entirely different data source than that used in Table III, column (1). It is reported by households, not MFIs, and covers a nationally representative sample of Indian households. However, the “Debt and Investment” data is not without drawbacks: most significantly, we only have this

17. One common expansion strategy for MFIs is to open new branches close to existing operations, which we document in Online Appendix Table E.XXII.

data for one point in time, 2012, so we are unable to use a difference-in-differences empirical strategy. We must instead rely on cross-sectional comparisons, controlling for district-level precrisis characteristics calculated from other data sources.¹⁸

The “Debt and Investment” survey asks households to enumerate each loan outstanding and aims to capture the lender’s regulatory classification (from 17 options). The NSS handbook (NSSO 2014) states that for-profit microfinance should be grouped as SHG-NBFC; however, nonprofit microfinance and bank-linked SHGs are grouped under SHG-BL (self-help group—bank-linked). Furthermore, there are three other categories that describe non-bank formal loans from financial institutions, which can be collateralized or uncollateralized. In sum, there is uncertainty about how respondents and surveyors would choose to treat a MFI loan. Because microloans are almost always uncollateralized, we capture microcredit with a measure that captures all uncollateralized nonbank credit from formal institutions, including noncollateralized SHG loans, some of which may be linked to a bank. Importantly, this definition allows us to capture impacts on microcredit that are net of any offsetting SHG supply response.

Table III, columns (2)–(7) present OLS regressions of household credit on our precrisis AP exposure variables. Because we cannot use our difference-in-differences strategy, we instead control for numerous precrisis, district-level covariates.¹⁹ In columns (2) and (5), we examine the impacts of exposure to the crisis on the level of microcredit. The binary measure of precrisis exposure is associated with a reduction of outstanding credit in 2012 of INR 1,297, significant at the 1% level. This represents a fall of roughly 50% of the control mean of INR 2,395. Column (5), using the log of MFI credit outstanding, shows a similar result, a

18. To allay concerns about the cross-sectional regression, Online Appendix Table C.II, column (1) presents a first stage based on the balance sheet data, but using only cross-sectional postcrisis variation. Reassuringly, this gives an estimate quite similar to that using both cross-sectional and panel variation (column (2)), suggesting that, given our rich set of controls, the ability to absorb district fixed effects is not crucial.

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

drop of 63 log points in exposed districts. Note that the first stage using this representative borrowing data does indeed provide an estimate that is larger in magnitude than what we obtain using the data constructed from the sample MFIs, though smaller than the full correlation scenario.²⁰

In the remaining columns, we present bank credit (columns (3) and (6)) and total credit (columns (4) and (7)) as outcomes. While the coefficients are estimated somewhat imprecisely, we find, in columns (3) and (6), no evidence that bank credit increased and thereby offset the fall in microcredit. (This finding is consistent with [Online Appendix Table C.III](#), which uses a different source of data, namely, RBI data on banks' balance sheets.) In column (4), we observe a large negative but imprecisely measured coefficient on total credit outstanding, suggesting that, as expected given the aggregate nature of the shock, other sources such as informal lending could not compensate for the loss of microcredit. In column (7), where we use the log of total credit outstanding, we observe a 77 log point decline in total credit for exposed versus unexposed districts, significant at the 5% level.²¹

We conduct an additional exercise to probe the issue of partial MFI reporting. Specifically, we construct an alternative measure of our instrument, including all MFIN member MFIs, using predicted lending footprints for those MFIs that are not in our main data set. See [Online Appendix E](#).

Importantly, the fact that this was a microfinance shock matters for aggregate outcomes, over and above its impact on total district-level credit. The propagation of a credit supply shock will depend critically on the uses to which the credit would have been put, and microcredit serves specific needs, namely, accelerating lumpy consumption and financing business investment, that are not well met by other sources. Moreover, the significant negative effects seen in columns (5) and (7) using the log transformation, which gives greater weight to the left tail of

20. These results, which use a measure of microcredit that includes SHG loans, also demonstrate that SHGs did not in fact fill the void left by reduced access to microcredit loans.

21. In [Online Appendix Table C.V](#) we present a measure based on the narrow NSS definition, those classified as SHG-NBFC. As expected, the estimated effects are smaller, consistent with widespread misclassification of MFI loans into other categories in the survey. Nonetheless, the results are negative and significant at the 10% level in both level and log specifications, using both the binary and continuous measures of exposure.

the distribution, indicates that credit particularly fell for poor households, who were most reliant on microcredit and likely least able to replace it with bank credit or other financing strategies such as drawing down assets.

2. *Khwaja and Mian (2008) exercise.* As an additional check on the first stage, we conduct an exercise exploiting within-district variation, modeled after Khwaja and Mian (2008). We focus on districts with exposed and unexposed MFIs and show that the fall in credit is driven by exposed MFIs. This also serves as an additional test of the identifying assumption that exposed and unexposed districts would have had similar counterfactual outcomes in the absence of the crisis: if exposed districts differed in some unobservable way, or suffered a demand shock due to being exposed to the AP crisis via other channels, we would expect unexposed lenders' portfolios to fall in those districts as well.

We examine this relationship via the following regression:

$$(4) \quad \Delta y_{ld12-10} = \alpha + \delta_d + \beta \times Exposure_l + X'_{ld}\gamma + \varepsilon_{ldt},$$

where $\Delta y_{ld12-10}$ is the change in per rural household GLP lent by MFI l to district d between September 2010 and March 2012. The δ_d are district fixed effects, and $Exposure_l$ is the exposure of lender l to the AP crisis, measured by either the share of its portfolio in AP as of September 2010, or a dummy equal to 1 if the MFI operated in AP in September 2010.

Online Appendix Table C.IV shows the results. Column (1) shows that moving from 0% to 100% exposure of the MFI, captured by the share of its portfolio in AP as of September 2010, is associated with a INR 281 fall in GLP per rural household over 2010 to 2012. The constant reflects any excess change in GLP for an unexposed MFI in a district where some MFIs were exposed; it is very small and not significantly different from zero, demonstrating that unexposed MFIs in exposed districts do not show a change in lending compared with similar MFIs in unexposed districts. Column (2) uses an exposure dummy equal to 1 if the MFI operated in AP on the eve of the crisis. Exposed MFIs saw an average decline in GLP per rural household of INR 153; again, the constant shows that unexposed MFIs saw no excess change. This supports the idea that exposure is identifying the effect of reduced credit supply from affected lenders, not an unobservable correlated with exposure which would affect unexposed lenders as well.

Columns (3) and (4) show that controlling for MFI size (so that size is not proxying for exposure) does not change the results.²²

IV.B. *Reduced-Form Results*

1. *Labor Outcomes.* We begin by examining how the reduction in district-level credit access observed in Table III affects the local labor market. Table IV 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 the casual daily wage. Exposed districts experienced a fall in the daily wage of INR 6.4, significant at the 5% level, which is displayed in column (1). This represents roughly a 4% reduction from the unexposed district mean of INR 153. 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 (i.e., in self-employment and wage employment combined). However, column (3) shows that household days worked in casual daily wage labor did decrease by almost half a day on a base of 3.5 person-days, demonstrating a substitution away from paid casual labor and toward self-employment.

Given that wages and paid days worked fell, there is an overall decline in household weekly labor market earnings of INR 86 in exposed relative to unexposed districts after the AP crisis, significant at the 1% level (column (4)). The significant effects on wages and labor earnings echo the predictions of Buera, Kaboski, and Shin (2021) and highlight the importance of incorporating general equilibrium effects into the analysis of the effects of credit access. Consistent with the null effect on total employment, 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)).

Column (6) examines effects on the likelihood that a household has a business that employs others. The point estimate on the extensive margin of being an employer is negative, but not significant at conventional levels ($p = .199$) for the binary indicator; for the continuous measure of exposures, we observed a fall of 0.3 percentage points, significant at the 10% level. This

22. Note that the constant no longer has the same interpretation because it reflects the average positive growth rate of an MFI that was very small in 2010.

TABLE IV
LABOR OUTCOMES

| | Casual daily wage (1) | HH weekly total days worked (2) | HH weekly casual days worked (3) | HH weekly labor earnings (4) | Any HH member invol. unemployed (5) | HH is an employer (6) | HH operates business (7) |
|--------------------------------|--------------------------------|--|---|---------------------------------------|--|--------------------------------|-----------------------------------|
| Any exposed lender × post 2010 | - 6.432** (2.954) | 0.057 (0.234) | - 0.446** (0.196) | - 86.227*** (30.333) | 0.012 (0.011) | - 0.005 (0.004) | 0.041** (0.018) |
| Exposure ratio × post 2010 | - 3.439** (1.335) | - 0.063 (0.111) | - 0.154* (0.089) | - 44.836*** (14.181) | 0.002 (0.005) | - 0.003* (0.002) | 0.013* (0.008) |
| Control mean | 153.361 | 10.275 | 3.455 | 836.465 | 0.098 | 0.035 | 0.584 |
| Control std. dev. | 87.097 | 6.738 | 5.134 | 1,266.456 | 0.297 | 0.183 | 0.493 |
| Observations | 40,584 | 119,668 | 119,668 | 119,668 | 119,668 | 119,668 | 119,668 |

Notes. Outcome data are from NSS rounds 64, 66, and 68. Each cell provides coefficients from separate difference-in-differences regressions. The first row reports coefficients from separate regressions using the binary indicator for high exposure to AP. The second row reports specifications that use the standardized continuous exposure measure. In all columns, controls include Round 66 average district consumption, average casual wage and linear distance to AP interacted with round, quintiles of household size, number of rural households and its squared term, GLP quintiles in 2008, and 2010 dummies along with round and survey month fixed effects. Wage and earnings are winsorized at the 99th percentile. Standard errors clustered at the district level are reported in parentheses. Observations are weighted by NSS sampling weights. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

represents a fall of roughly 9% of the mean in unexposed districts. Of course, the businesses we capture in this measure are likely only a small fraction of total labor demand, as many businesses will not be owned by rural households; the data also do not allow us to examine the intensive margin of labor demand. However, these businesses are likely more representative of those operated by microfinance borrowers.

Finally, we consider how household self-employment activities respond to the labor market shock stemming from the fall in credit. In column 7, we show that households are more likely to report having a household enterprise in districts that are more exposed to the crisis. A one std. dev. increase in exposure (second row) corresponds to a 1.3 percentage point increase in the likelihood a household operates a business, significant at the 10% level. An increase in self-employment days is also consistent with the fact that we observe total worker days (column (1)) falling by less than days worked in casual wage labor (column (3)). This is consistent with the finding that self-employment can function as an employer of last resort and potentially represent masked underemployment (Karaivanov and Yindok 2017; Adhvaryu, Kala, and Nyshadham 2019; Breza, Kaur, and Shamdasani 2021). The opposite signs of impacts in columns (6) and (7) are also consistent with other work documenting high levels of heterogeneity in firm profitability in developing-country settings (Meager 2019; Hussam, Rigol, and Roth 2021). Although some firms face binding credit constraints and fit the narrative of the investment channel, other firms have limited growth potential and function more as an insurance policy against negative shocks to earnings.

2. Effects on Tradable and Nontradable Sectors. We next consider the effect of the AP crisis-induced credit crunch on wages, separately for agriculture (tradable) and nonagriculture (nontradable). Recall that while the tradable sector should not be affected by changes to local demand, responding only to effects on firm liquidity, the nontradable sector will be hit by both the local aggregate demand channel and the firm liquidity channel and, hence, may exhibit larger falls in wages.

Table V presents the results, estimated via a version of equation (2) that separately estimates the effect of exposure for the agricultural and nonagricultural sectors. The top panel of the table uses the binary measure of exposure, and the bottom panel uses the continuous measure. Column (1) uses observations of

TABLE V
CASUAL DAILY WAGES BY SECTOR

| | Casual daily wage: pooled (1) | Casual daily wage: men (2) | Casual daily wage: winsorized (3) | Casual daily wage: men, win. (4) |
|--|--|-------------------------------------|--|---|
| (Any exposed lender \times post 2010) \times agriculture | -5.081 (3.340) | -4.231 (3.732) | -5.555* (3.173) | -4.887 (3.478) |
| (Any exposed lender \times post 2010) \times non-agriculture | -9.436** (4.380) | -9.194* (4.810) | -7.949* (4.084) | -7.819* (4.455) |
| <i>p</i> -value: ag = non-ag | .304 | .276 | .551 | .497 |
| (Exposure ratio \times post 2010) \times agriculture | -2.342 (1.469) | -1.737 (1.665) | -2.802** (1.386) | -2.365 (1.550) |
| (Exposure ratio \times post 2010) \times non-agriculture | -5.315** (2.209) | -5.072** (2.487) | -4.803** (2.045) | -4.680** (2.279) |
| <i>p</i> -value: ag = non-ag | .155 | .150 | .311 | .290 |
| Ag mean | 128.581 | 140.534 | 128.211 | 140.068 |
| Non-ag mean | 184.242 | 194.709 | 178.099 | 187.703 |
| Observations | 40,584 | 29,493 | 40,584 | 29,493 |

Notes. Outcome data are from NSS rounds 64, 66, and 68. Each cell provides coefficients from separate difference-in-differences regressions. The first row reports coefficients from separate regressions using the binary indicator for high exposure to AP. The second row reports specifications that use the standardized continuous exposure measure. In all columns, controls include Round 66 average district consumption, average casual wage and linear distance to AP interacted with round, quintiles of household size, number of rural households and its squared term, GLP quintiles in 2008, and 2010 dummies along with round and survey month fixed effects. Outcomes in columns (3) and (4) are winsorized at the 99th percentile. Standard errors clustered at the district level are reported in parentheses. Observations are weighted by NSS sampling weights. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

wages earned by both men and women. We find negative effects on the wage in both the tradable and nontradable sectors, but the wage effects are larger for nonagricultural work. In the top panel, the wage drop associated with the binary measure of exposure is INR 5.08 ($p = .128$) for the agricultural sector, and INR 9.44 (significant at 5%) for the nonagricultural sector. (The p -value of the test of the hypothesis that the effects are the same is .304.) While the average level of agricultural wages is lower, the effect on the nonagricultural wage is also larger in percentage terms: it falls by 5.12% of the control mean, compared with 3.95% for the agricultural wage. In the bottom panel we find similar results using the continuous measure of exposure, with a one-unit increase in exposure associated with an INR 2.34 fall in the agricultural wage (not significant at conventional levels) and a fall of INR 5.32 in the nonagricultural wage (significant at 5%); the p -value of the test of the hypothesis that the effects are the same is .155.

Column (2) shows effects on men's wages, which may be less affected by the margin of whether to work for a wage. As in the results pooling genders, the wage effects are stronger for nonagricultural work. The fall in the wage associated with a one-unit increase in exposure is roughly three times larger for the nonagricultural sector than the agricultural sector: INR 5.072 versus INR 1.737. The p -value of the test of the hypothesis that the effects are the same is .150. Columns (3) and (4) repeat the analysis, winsorizing the wage data at the 99th percentile; the results are quite similar.

This pattern of results is consistent with the predicted impacts of a negative shock to labor demand through the business investment channel, affecting both tradables and nontradables, combined with a reduction in aggregate demand putting additional downward pressure on employment and wages in the nontradable sector.

3. Consumption. Reduced access to credit may also affect household consumption, through the falls in wages and earnings and through a reduced ability to bring consumption forward in time via borrowing.²³ Table VI reports effects on total expenditure

23. Recall that the NSS data is collected contemporaneously with the credit shock and thus can pick up the direct effect on consumption resulting from reduced ability to move consumption over time. This is in contrast to many studies of microfinance where consumption and other outcomes are measured 12 or more

TABLE VI
CONSUMPTION OUTCOMES

| | HH monthly consumption: total (1) | HH monthly consumption: nondurables (2) | HH monthly consumption: durables (3) | Below poverty line (4) |
|---------------------------------------|--|--|---|---------------------------------|
| Any exposed lender \times post 2010 | -138.218 (118.719) | -89.202 (106.911) | -41.714** (16.737) | 0.000 (0.021) |
| Exposure ratio \times post 2010 | -151.222*** (51.919) | -127.775*** (46.950) | -17.130** (7.502) | 0.010 (0.010) |
| Control mean | 5,502.140 | 5,183.746 | 284.541 | 0.254 |
| Control std. dev. | 3,433.515 | 2,977.998 | 665.044 | 0.435 |
| Observations | 111,692 | 119,668 | 111,692 | 111,692 |

Notes. Outcome data are from NSS rounds 64, 66, and 68. Each cell provides coefficients from separate difference-in-differences regressions. Notice that due to missing values the sample size for durable consumption is lower than for nondurable consumption. Total consumption and poverty are then considered missing when durable consumption is missing. The first row reports coefficients from separate regressions using the binary indicator for high exposure to AP. The second row reports specifications that use the standardized continuous exposure measure. In all columns, controls include Round 66 average district consumption, average casual wage and linear distance to AP interacted with round, quintiles of household size, number of rural households and its squared term, GLP quintiles in 2008, and 2010 dummies along with round and survey month fixed effects. Outcomes in columns (1) to (3) are winsorized at the 99th percentile. Standard errors clustered at the district level are reported in parentheses. Observations are weighted by NSS sampling weights. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

and its components: nondurables and durables, measured on a monthly basis. Column (1), second row shows that a one std. dev. increase in exposure to the crisis is associated with a reduction of INR 151 in per household monthly total expenditures in 2012 (significant at 1%). Column (2) examines per household monthly nondurable expenditures. The second row shows that a one std. dev. increase in exposure to the crisis is associated with a reduction of INR 128 (significant at 1%). Column (3) repeats the analysis for per household monthly durable expenditures. The second row shows that a one std. dev. increase in exposure to the crisis is associated with a reduction of INR 17 (significant at 5%). In sum, reduced credit access resulted in reduced total consumption, stemming from falls in both nondurables and durables. In column (4), we also examine whether exposure to the crisis has any effect on whether households are below the poverty line. We find no significant effect on poverty, suggesting that the reduction in consumption is concentrated higher up in the distribution.²⁴

4. Investment. Unfortunately, the NSS consumption module does not include any information on whether purchases were used for investment purposes. However, the NSS Round 70 survey contains data on household and business investment during the six-month period beginning on July 1, 2012. Given that we only have cross-sectional information on investment, we follow the same empirical strategy as in [Table III](#), columns (2)–(7), and control for a range of baseline district-level borrowing, consumption, and earnings measures. [Table VII](#) presents the results. We find significant declines in total investment (column (1)), with the largest component coming from home construction and home improvements (column (2)). The home improvement effects are consistent with an aggregate-demand channel, as most construction inputs are nontradable. A large impact of home repair is also documented in [Kaboski and Townsend \(2012\)](#). Consistent

months after the change in credit access, by which time the direct effect on consumption is likely to have passed. Thus the consumption multiplier we estimate (see [Section IV.F](#)) is inherently short term.

24. Poverty headcounts in India have fallen substantially since the banking reform studied by [Burgess and Pande \(2005\)](#). During the timeframe of their study, 48% of rural households were classified as below the poverty line. In our data from 2010, the poverty rate is only half as large, at 25%. See [Online Appendix G](#) for details on the construction of the below-poverty line variable.

TABLE VII
EXPOSURE TO THE AP CRISIS AND INVESTMENT: NSS ROUND 70 DATA

| | Total investment (1) | Home improvements (2) | Ag. business investment (3) | Non-ag business investment (4) |
|---------------------------------------|----------------------------|-----------------------------|-----------------------------------|--------------------------------------|
| Any exposed lender \times post 2010 | -1,134.137* (629.364) | -889.759* (474.631) | -31.508 (148.675) | -39.155 (25.297) |
| Exposure ratio \times post 2010 | -719.334** (286.876) | -412.223* (222.258) | -51.892 (69.236) | -36.517*** (11.716) |
| Control mean | 6,072.643 | 3,759.068 | 928.797 | 187.458 |
| Control std. dev. | 25,836.638 | 19,110.354 | 4,522.611 | 977.247 |
| Observations | 33,559 | 33,559 | 33,559 | 33,559 |

Notes. Outcome data are from the NSS 70th round “Debt and Investment” survey reflecting investments made in home improvements and businesses between July 1, 2012, and December 31, 2012. Each cell provides coefficients from separate OLS regression specifications. The first row reports coefficients from separate regressions using the binary indicator for high exposure to AP. The second row reports coefficients from separate regressions using the standardized continuous exposure measure. The dependent variables are levels and at the top 1 percentile. All columns include precrisis 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. Other controls are Round 66 average district consumption, average casual wage, and linear distance to AP interacted with round for survey months before July. Standard errors clustered at the district level are reported in parentheses. Observations are weighted by NSS sampling weights. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

with a business investment channel, we find negative impacts on agricultural and nonagricultural business expenditures (columns (3) and (4)). Although the effects on agricultural business expenditures are not statistically significant, a one std. dev. increase in district exposure to AP corresponds to a INR 39 fall in nonfarm business investment, a drop of approximately 20% relative to the unexposed district mean. These effects are consistent with our interpretation of the findings in [Table IV](#); overall business investment and hiring falls, despite an increase in self-employment.

Note that unlike our main measures of consumption, the investment outcomes can be matched to microfinance borrowing for the same set of households. In [Online Appendix D](#), we investigate the extent to which the observed declines in investment are attributable to the same households that experienced a decline in microfinance credit supply. We use a random forest model to classify households into likely borrowers and nonborrowers, based on household demographics. We then show that the overall investment results are substantially larger for likely borrowers, largely due to home improvement expenditures. We find significant effects on nonagricultural business investment for nonborrowers and borrowers alike, which is consistent with broader equilibrium impacts.

5. Impacts on Agricultural Output. We next examine district-level impacts of the credit supply shock on output in the tradable (agricultural) sector. While tradables experience no shock to product demand, the effect on total output is unsigned. Although agricultural firms will benefit from lower wage bills, some may be forced to scale back as liquidity constraints bind more tightly. We examine district-level crop yields to shed light on this question. Following [Jayachandran \(2006\)](#), we consider a weighted average of log yield (production per area cropped) for the five major crops: rice, wheat, sugar, jowar (sorghum), and groundnuts. We also consider each crop separately. The results appear in [Online Appendix Table C.VI](#). We find a fairly precise zero effect for the index and for all crops but sugarcane, where the effect is less precise but still insignificant. This suggests that for the agricultural sector, the benefit from the wage reduction cancels out the cost from tighter liquidity constraints.

IV.C. Quantile Treatment Effects

The microfinance RCT literature has demonstrated that the effects of access to microloans need not be constant across the

distribution (Meager 2020; Banerjee et al. 2020). To examine whether such heterogeneity is also apparent in equilibrium, in Table VIII we perform quantile regressions for the 25th, 50th, and 75th percentiles of the distribution on key outcomes (total and durable consumption, labor earnings, casual days worked, and the casual daily wage).²⁵ For total and durable consumption, days worked, and the wage, significant negative effects are seen at the median and 75th percentiles (with the effect at the 75th percentile appearing larger in magnitude, but somewhat noisier), whereas there is no significant impact at the 25th percentile for these outcomes. In contrast, for weekly labor earnings, negative effects of a similar magnitude are apparent at all three points in the distribution.

These patterns, namely, muted effects at the bottom, moderate effects in the middle, and larger effects higher in the distribution echo a similar finding for the RCT literature (Meager 2020). This similarity is consistent with the idea that there need not necessarily be an asymmetry in the effects of expansions and contractions of credit (modulo the sign), inasmuch as the pattern of effects through the distribution are similar. In addition, these effects demonstrate that the effect of microfinance is apparent throughout the top half of the distribution and is not concentrated among just a small fraction of households or firms.

IV.D. *Scaling the Reduced-Form Treatment Effects*

Due to the limitations of our precrisis measure of exposure and our ex post measure of the drop in credit, one needs to use caution when thinking about scaling the reduced-form, intent-to-treat (ITT) effects into TOT effects. One issue, discussed above, is that the first stage based on the balance sheet data, as used in Table III, column (1) only measures lending from the subsample of MFIs who provided their data, while the reduced-form regressions using the NSS are based on a representative sample of households. For this reason we consider the effect on MFI

25. For computational reasons, the quantile regressions are estimated on unweighted data, meaning that we do not reweight to account for the sampling design of the NSS, which oversamples less populous areas. For comparison, Online Appendix Table C.XV presents unweighted OLS regressions. The resulting estimates are somewhat larger in magnitude than the weighted estimates, suggesting that the effects are larger in less populous places, likely due to thinner markets and/or less market integration.

TABLE VIII
QUANTILE REGRESSIONS

| | HH monthly consumption: total (1) | HH monthly consumption: durables (2) | HH weekly labor earnings (3) | HH weekly casual days worked (4) | Casual daily wage (5) |
|-------------------------------------|--|---|---------------------------------------|---|--------------------------------|
| Panel A: 25th percentile regression | | | | | |
| Exposure ratio \times post 2010 | -29.177 (37.351) | 5.328 (3.474) | -21.353*** (7.610) | -0.041 (0.033) | -1.337 (1.418) |
| Control mean | 6,945.676 | 658.482 | 1,086.034 | 2.634 | 176.244 |
| Control std. dev. | 6,518.771 | 3,045.746 | 1,996.492 | 4.622 | 107.029 |
| Observations | 111,692 | 111,692 | 119,668 | 119,668 | 40,584 |
| Panel B: Median regression | | | | | |
| Exposure ratio \times post 2010 | -137.117** (57.158) | -9.775** (4.815) | -21.600** (8.992) | -0.056 (0.034) | -3.380** (1.592) |
| Control mean | 6,945.676 | 658.482 | 1,086.034 | 2.634 | 153.361 |
| Control std. dev. | 6,518.771 | 3,045.746 | 1,996.492 | 4.622 | 87.097 |
| Observations | 111,692 | 111,692 | 119,668 | 119,668 | 40,584 |
| Panel C: 75th percentile regression | | | | | |
| Exposure ratio \times post 2010 | -301.318*** (85.134) | -52.596*** (16.284) | -22.572 (31.810) | -0.221** (0.100) | -5.180* (2.840) |
| Control mean | 6,945.676 | 658.482 | 1,086.034 | 2.634 | 176.244 |
| Control std. dev. | 6,518.771 | 3,045.746 | 1,996.492 | 4.622 | 107.029 |
| Observations | 111,692 | 111,692 | 119,668 | 119,668 | 40,584 |

Notes: Outcome data are from NSS rounds 64, 66, and 68. In each panel, each column provides coefficients from separate difference-in-differences quantile regressions using the standardized continuous exposure measure. In all columns, controls include Round 66 average district consumption, average casual wage and linear distance to AP interacted with round, quantiles of household size, number of rural households and its squared term, GLP quantiles in 2008, and 2010 dummies along with round and survey month fixed effects. Standard errors cluster bootstrapped (250 repetitions) at the district level are reported in parentheses. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

borrowing from the “Debt and Investment” data (Table III, columns (2) and (5)) to be the most appropriate measure of the total impact of the crisis on households’ access to credit. Thus, if a first-stage number is desired for scaling purposes, our preferred specification is from Table III, column (2), second row (INR –625.7). This implies a back-of-the-envelope multiplier on total consumption of 2.9.²⁶ We can also calculate a lower-bound estimate of the multiplier by using the implied first stage under perfect correlation of MFI lending locations, as discussed in Section IV.A; the resulting estimate is 1.9. In Section IV.F, we discuss the multiplier in the context of other comparable estimates from the literature.

IV.E. Robustness

1. Urban Markets. To give context for the magnitude of our results on rural markets, we examine the NSS data on urban areas. (Recall that all of the other analysis in this article focuses on areas classified as rural by the NSS.) The microfinance shock should be much smaller in urban areas, due to deeper credit markets, less microfinance per capita, and more labor- and product-market integration.

One challenge with analyzing the “urban” subsample is that this classification can encompass areas ranging from small market towns to large metropolises.²⁷ Thus, from the perspective of market integration and depth of credit access, some “urban” areas will actually be rural. Therefore, we split urban areas into those located in districts that are less than 100 km from the state capital, typically the largest city in the state, versus those in districts that are more than 100 km from the state capital.

26. This is obtained by annualizing the monthly consumption impact in Table VI, $12 * 151 = 1,812$, and dividing it by the change in credit (–626.5), yielding a fall in consumption of INR 2.9 per rupee of lost credit. Recall that our exposure measure is also only based on a subset of MFIs. So this means that our reduced-form and preferred first-stage results are likely attenuated, but in the same way. Thus, the “back-of-the-envelope IV” is not subject to attenuation bias from measurement error.

27. The NSS definition of an urban area typically requires that an area have (i) a minimum population of 5,000, (ii) at least 75% of the male working population are nonagriculturists, and (iii) a density of population of at least 400 per sq. km., but even areas that do not meet all three criteria can be classified as urban (NSSO 2001). Moreover, urban observations are only tagged to the district, with no indicator for town/city name or population.

These more-distant urban areas are likely to be less integrated, and are more often small market towns. For each of these urban subsamples, we examine the effect of exposure to the AP crisis on consumption, labor earnings, and wages (total and nonagricultural). The results are shown in [Online Appendix Table C.XIX](#).

Consistent with this prediction, we show that in urban areas close to state capital cities, exposure to the AP crisis did not cause consumption, wages, or labor earnings to fall ([Online Appendix Table C.XIX, Panel A](#)). As shown in Panel B, among the “urban” sample, only small market towns experienced the negative effects of the credit crunch; in these areas the effects are similar to those in the rural sample. In other words, while the effects of the credit contraction were significant for rural markets, this is not the case in more urban areas. Thus it may be especially important to consider equilibrium effects in less-integrated markets, as found for price effects in Mexican villages by [Cunha, De Giorgi, and Jayachandran \(2019\)](#).

We next provide evidence to rule out several key threats to identification.

2. *Geographical Distance to AP.* Recall from [Online Appendix Table C.I](#) that exposure to the AP crisis is correlated with distance to AP. While all of our regression specifications control for distance to AP interacted with time, 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 (e.g., economic uncertainty, decreased trade). We perform several tests to check that our results are not simply capturing such an effect.

In [Online Appendix Table C.XVIII](#), we conduct two additional robustness exercises using alternate measures of distance to AP. First, in Panel A, we rerun our main specification for key consumption and labor supply outcomes, but drop districts with a geographical border with AP. Second, because raw geographical distance may not adequately capture some types of relationships between districts, such as trade costs, due to variation in the quality of infrastructure, in Panel B we add the travel time between a given district and Hyderabad, as measured in [Allen and Atkin \(2016\)](#), interacted with time. Across these iterations, the results look very similar to those in our main specification.

Finally, we also conduct Altonji-type tests in [Online Appendix Table C.VIII](#), systematically dropping each state from the analysis. We find that no single state is driving the results, including those bordering AP.

3. Additional Robustness. Our results are robust to a battery of robustness checks. We assign placebo shocks to alternate time periods; we examine placebo shocks (using randomization inference to reassign exposure to the crisis across districts and examining placebo exposure to states other than AP); we include time-varying controls for SHG penetration and political affiliation to proxy for risks of policy contagion; we include additional time-varying controls for initial economic conditions and rainfall; and we explore the robustness of our findings to interdistrict migration. Additionally we perform several checks to ensure that our results are not sensitive to our treatment of the tails of the data, nor to concerns of multiple inference. Details are in [Online Appendix B](#).

IV.F. GE Effects in the Development Literature

To benchmark our results, [Online Appendix Table C.XX](#) presents the findings of five GE studies in the development economics literature. The most relevant point of comparison is [Egger et al. \(2020\)](#), which analyzes a large RCT that randomized cash transfers to rural communities in Kenya. Notably, these transfers were large—worth 17% of local GDP—and the randomization design permits analysis of equilibrium effects at a local market level (at a radius of two kilometers around each village). The authors calculate a consumption multiplier of 2.5.

In our setting, we obtain a similar estimate: our back-of-the-envelope, short-run consumption multiplier estimate is 2.9. It is perhaps unsurprising that these estimates are somewhat larger than those from the developed-country literature (where, e.g., [Serrato and Wingender 2016](#) and [Huber 2018](#) calculate multipliers of 1.6 to 1.8). Agents in developing countries are more likely to face binding liquidity constraints, and, as pointed out by [Chodorow-Reich \(2019\)](#), binding liquidity constraints break Ricardian equivalence and hence increase short-run multipliers.

In the short run, we may also expect multipliers from loans to be larger than multipliers from grants. By definition, those selecting into loans are choosing to bring resources forward

in time, implying that their marginal utility of funds today exceeds the marginal utility of funds in the future. This would lead to larger multiplier impacts from loans versus (relatively) untargeted cash transfers, *ceteris paribus*, at the time of loan disbursement. However, in the longer run, the need to repay a loan may lead to an offsetting effect. Moreover, relative to [Egger et al. \(2020\)](#), we study variation at the much larger, district level, which likely allows us to capture more of the possible spillover effects from the intervention. Moreover we study a negative shock, while [Egger et al. \(2020\)](#) focus on a positive windfall. More work is needed to understand the circumstances under which fiscal or monetary policy is most impactful.

[Burgess and Pande \(2005\)](#) and [Kaboski and Townsend \(2012\)](#) measure the change in the wage in response to an increase in access to finance. [Burgess and Pande \(2005\)](#) study the coordinated expansion of rural bank branches in India and find a 7% agricultural wage increase from one additional branch per 100,000 people. [Kaboski and Townsend \(2012\)](#) find a comparable agricultural wage response to an increase in short-term credit stemming from an increase in rural credit supply.²⁸ The table also highlights two studies of the effects of a change in labor demand ([Imbert and Papp 2015](#); [Muralidharan, Niehaus, and Sukhtankar 2017](#); [Akram, Chowdhury, and Mobarak 2017](#)). These results suggest that modest changes to labor demand can lead to substantial wage effects.

V. MODEL CALIBRATION

We calibrate a simple equilibrium model of microfinance borrowing that incorporates both aggregate-demand and business investment impacts. We aim to understand whether the magnitudes of our empirical results are similar to those that emerge from such a model. Moreover, we can recalibrate the effects on wages in the model by changing the propensity for microfinance borrowers to invest in profitable businesses. We provide details of the environment and calibrated parameters in [Online Appendix F](#).

28. Consistent with our results, [Kaboski and Townsend \(2012\)](#) find a much larger wage impact for jobs in construction, a nontradable sector. They also find large short-run consumption effects that are consistent with an aggregate-demand story.

We assume a two-period, two-sector economy with tradable and nontradable goods. Households are endowed with labor in each sector, and labor markets are segmented.²⁹ Some households are further endowed with the skill to become an entrepreneur in one of the sectors. Households cannot save and are endowed with nonlabor income in each period; this nonlabor income is larger in the second period, giving households a motive to borrow. When available, microcredit can be used by eligible households who wish to bring consumption forward. Equilibrium occurs in the labor and the product markets. In the tradable sector, prices are determined by global factors, outside of the economy. However, in the nontradable sector, consumer demand and producer supply must be equalized. Moreover, the wage in each sector equates labor supply with demand.

Of course, any exercise of this type requires us to make a number of assumptions. We view this calibration largely as a sanity check, showing that plausible parameter values can deliver wage effects in the range of our estimated magnitudes.

V.A. Calibrated Parameters of the Model

We calibrate the parameters describing households' skilled and unskilled labor endowments and tradable and nontradable consumption shares using NSS data. Production function parameters (TFP and returns to scale) are chosen to match those for India in [Buera, Kaboski, and Shin \(2021\)](#). The credit market parameters are chosen to match the real interest rate charged by Indian microlenders, the size of a loan as a share of annual labor earnings, and the share of households who borrow from microfinance.

To generate calibrated treatment effects, we compare a simplified economy with status quo access to microfinance with an economy experiencing a credit supply contraction. We assume that credit supply contracts by 54%, as we observe in our first stage in [Table III](#).

V.B. Calibration Results

We present the calibrated treatment effects on wages in the tradable and nontradable sectors in [Figure III](#) and examine how they change with the share of microcredit borrowers who borrow

29. This could be driven, for example, by skill requirements, especially in the nontradable sector.

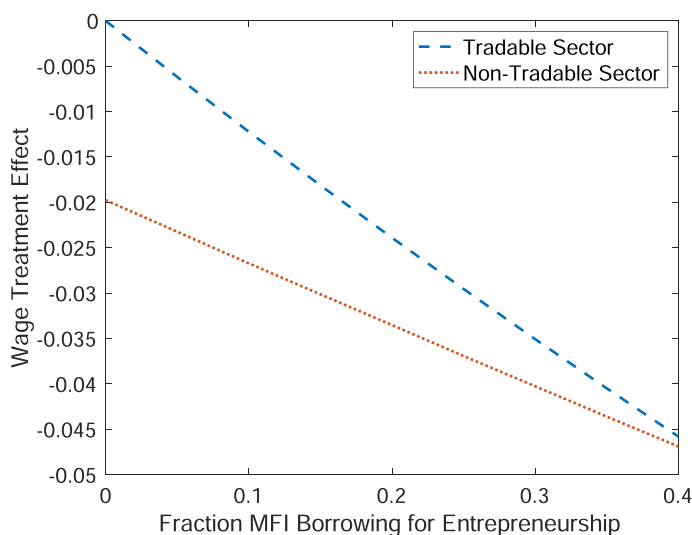


FIGURE III

Calibrated Wage Effects by Share Borrowing for Entrepreneurship

The figure shows the percentage drops in the sector-specific wages resulting from a 54% decrease in access to credit in the calibrated model. The *x*-axis is the fraction of borrowers who borrow to invest in a business. For details, see [Online Appendix F](#).

for business investment. When approximately one-third of microfinance is used for business investment, and the remainder is used for consumption, the wage effects implied by the calibrated model are in the range of our estimated impacts (3%–4% for tradable and 4%–5% for nontradable). The finding that roughly one-third of microfinance loans are used for businesses in the nonagricultural sector is consistent with [Banerjee et al. \(2020\)](#), who show that the top tercile of urban households experiences persistent and positive effects on business profitability and scale using variation from a microfinance RCT.

We note that there are unmodeled channels that could also contribute to the size of the empirical effects. A precautionary savings motive would amplify the aggregate-demand effect as households save to rebuild buffer stocks ([Kaboski and Townsend 2011](#)). And a labor supply response would magnify the wage fall,

as households respond to consumption drops by increasing labor supply (Jayachandran 2006).

VI. CONCLUSION

We use the AP microfinance ordinance as a natural experiment to measure the real effects of the loss of microfinance on rural households. Given the scale and maturity of the microfinance sector in India before the ordinance and the extent of the crisis, which wiped roughly a billion dollars off lenders' balance sheets, this episode presents a unique opportunity to study the impacts of microfinance in general equilibrium.

We present evidence that our effects are likely driven by a combination of two distinct channels: business investment and aggregate demand. Because we only have one instrument, we cannot empirically disentangle the separate contribution of each channel. Moreover, the two channels may interact in important ways. However, our results resonate with recent microevidence, suggesting heterogeneity in both the uses of and returns to microfinance (Kaboski and Townsend 2011; Banerjee et al. 2019, 2020; Meager 2019). The following narrative emerges: a sizable minority of households grow their businesses when microfinance expands, while the remaining borrowers consume the loan (or invest in home improvements or household durables). The former group's business spending and hiring drives the investment channel, and the latter group's consumption borrowing fuels an aggregate-demand channel.

Our results also show that the actions of politicians in AP 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, earnings, and consumption. This episode shows that microfinance, despite its small loan sizes, can have meaningful effects on rural economies.

Insofar as credit not borrowed today does not need to be repaid in the future, one might think that over time, the net effect of the crisis will "wash out." However, for poor households, utility is likely quite concave and so the volatility in the credit market, which translates into volatility in wages, earnings, and consumption, may be quite costly. The loss in total welfare resulting from a pronounced short-term consumption fall will not be fully offset by a future stream of slightly higher consumption. Moreover,

some effects of the credit crunch could persist, due to, for instance, durable investment or adjustment costs.

Ultimately, we believe our findings complement the RCT literature. Randomized evidence has documented that microfinance has modest medium-term effects on borrowers relative to nonborrowers in the same area. By studying quasi-exogenous variation in credit supply at a higher level of aggregation, we find evidence of important equilibrium effects through changes to labor demand, effects that affect both borrowers and nonborrowers in affected markets. These results together can paint a more complete picture than either taken alone.

HARVARD UNIVERSITY AND NATIONAL BUREAU OF ECONOMIC RESEARCH, UNITED STATES

TUFTS UNIVERSITY AND NATIONAL BUREAU OF ECONOMIC RESEARCH, UNITED STATES

SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

DATA AVAILABILITY

Data and code replicating the tables and figures in this article can be found in Breza and Kinnan (2021) in the Harvard Dataverse, <https://doi.org/10.7910/DVN/ABJE4K>.

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