The real cost of credit constraints: Evidence from micro-finance

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Abstract

The paper studies the effect of a law that banned micro-credit lending in the state of Andhra Pradesh in India. Regions in Andhra Pradesh are matched to regions that did not face the ban. A differences-in-differences estimation of changes in matched regions is used to establish a causal impact on average household consumption in the region. The results show that the average household consumption in the ban-affected regions dropped by 15 percent immediately after the ban compared to the matched regions, and persisted for four quarters. The result is robust to cross-sectional variations in regional exposure to micro-finance prior to the ban, variation in rural and urban locations, and variations in matching strategy. The analysis points to a ban as a sub-optimal intervention to improve customer welfare.

JEL classification: D14, G21, G28

Keywords: micro-credit, consumption smoothing, household finance, regulatory ban, natural experiment

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1 Introduction

The proliferation of micro-credit, in the form of payday lending and micro-finance, and the rise in over-indebtedness of low-income households has led to questions about the extent to which micro-credit is welfare enhancing (Melzer, 2011). These concerns have been exacerbated by alleged predatory lending practices and usurious interest rates of profit-making micro-finance firms. In some cases, these concerns have led policy makers to use regulatory interventions to restrict access to such credit.

A literature has developed around using these regulatory interventions as natural experiments to analyse the impact of restricting credit (payday lending) access. It finds that restricting payday lending leads to a reduction in household welfare (Morgan and Strain, 2008; Zinman, 2010; Morse, 2011).[1] These add to the literature on the welfare impact of micro-finance by both going beyond the scope of micro-evaluation using the Randomised Control Trial (RCT) approach, and by analysing the impact of credit withdrawal rather than increased access. However, a limitation is that they do not observe household consumption directly. Instead, they rely on proxies of household welfare such as bounced cheque payments, foreclosures on mortgages, and small property crimes.

This paper analyses the impact on average household consumption of a policy intervention that forced a closure of the micro-finance industry in the Indian state of Andhra Pradesh (AP). This is an ideal natural experiment to study the effect of credit withdrawal in three respects. First, the ban was unanticipated and absolute.[2] It left households in AP un-prepared for a sudden drop in access to credit. However, borrower households had an unexpected one-time windfall in not having to repay their loans. Second, it was an intervention on a large scale: AP had a population of 84 million in 2010 which exceeded that of Germany at that time. An estimated 27 million of the population were in households that borrowed from the micro-finance industry (Srinivasan, 2012).

Lastly, the paper uses a high quality panel data with average household consumption within 200 regions across all states in India, observed for every quarter starting from 2009. The data is observed at the level of the region, and not the household. While this has its own limitations, it allows the estimation of the change in average household consumption in the regions of AP before and after the ban was implemented. It also allows the identification of control regions that are economically *matched* to these AP regions, but where the state government did not implement a similar ban on micro-finance.

[1] Morse (2011), however, does point out that while her results show that payday lenders provide a valuable service to individuals facing unexpected financial distress, they do not speak to the effect on those habitually falling to temptation.

[2] The ban is described in more detail in Section 2.2

The causal effect of the ban is estimated using a difference-in-difference regression of the relative change in average household consumption between the AP regions and their matched regions, before and after the ban.

The analysis shows that there is a significant decline of 15 percent in average household consumption in AP, compared to the control regions immediately after the ban. One explanation for the sharp decline could be the high indebtedness in the state: the number of borrower households in AP is significantly high at 85 percent compared to 37 percent of borrower households in the rest of the country. But even after adjusting the selection of the controls appropriately, the estimated impact in the AP regions is a drop of 14 percent. The average consumption in the AP regions, which was higher than the controls before the ban, became lower than the controls after the ban. There is also evidence of higher volatility of average consumption in AP compared to the controls. All this suggests greater difficulties by households in AP to smooth consumption as a consequence of the ban.

There is some cross-sectional variation in the impact of the ban. The drop in consumption is higher for rural regions compared to urban regions. This may be expected since rural regions have fewer alternative sources of credit access. But the consistency of the adverse impact on the average household consumption suggests that the episode being analysed is reminiscent of episodes of deleveraging by a subset of the economy, with economy-wide effects (Eggertsson and Krugman, 2012). The consequences of such a large-scale credit withdrawal may not be limited to direct borrowers of micro-finance institutions. Instead, there may be ramifications on the economy at large owing to the interconnections between the borrowers and the rest of the economy.

The contribution of this paper is as an addition to the literature about the effects on household consumption when access to credit is abruptly withdrawn. There is as yet little consensus on whether payday loans or micro-credit actually improve the lives of their borrowers. Revealed preference would indicate that such growth-rates are welfare improving, if customers did not benefit, they would not borrow. Research however, suggests that people are not financially savvy, often overlook the costs of such borrowing, are naive about time-inconsistent preferences and end up in financial distress (Armendáriz and Morduch, 2010; Skiba and Tobacman, 2009; Lusardi and Tufano, 2009; Thaler, 1990); problems exacerbated by the alleged predatory lending practices and usurious interest rates charged by the lenders. In the case of micro-credit there is an added tension between the goals of poverty-alleviation of the original micro-finance movement and profit maximisation of commercial micro-finance today (Arun and Hulme, 2008). These tensions are reflected in the on-going debate on how to regulate these sectors (Davis, 2009; Shankar and Asher, 2010; Skiba, 2012), driven in part by the potential welfare-improving impact of credit access, but mindful of the potential harm it may inflict. To our knowledge, this is the first rigorous analysis of the natural experiment of a state-wide ban imposed on the micro-credit industry in India. The motivation for the intervention was consumer protection. But such an extreme policy intervention adversely affected the entire state. This suggests that while regulatory interventions in the name of consumer protection can be welfare-improving, a ban is not.

The paper starts with a discussion on the role of credit access in household consumption in Section 2, followed by a description of the micro-finance industry in Section 2.1 and the ban on micro-finance in AP in Section 2.2, The research hypotheses to be tested are described in Section 2.3. Section 3 describes the data, while the research design used is presented in Section 4. Section 5 discusses the findings, and Section 6 tests the robustness of these results. Section 7 concludes.

2 The research setting

Even though the theoretical literature on how credit access affects household consumption is rich and old, there is relatively sparse empirical research on this question. A limitation is the paucity of databases on the household balance sheet (Campbell, 2006). Most empirical research, therefore, relies on proxies of household welfare. One of the branches of this literature is the research that uses regulatory restrictions on payday lending across states in the U.S. as natural experiments. For example, Morgan and Strain (2008) analyse patterns in household default data such as bounced cheques, complaints against debtors and bankruptcy filings, Zinman (2010) uses employment status and Morse (2011) uses foreclosures on mortgages and small property crimes.

Direct evidence on the response of household consumption comes from the growing literature based on randomised control trials (RCTs).[3] However, much of these are micro-evaluations that share two features: they only analyse an increase in credit access, and they only observe household consumption after the credit intervention and not before. There is an emerging literature that evaluates the general equilibrium effects of increased micro-credit access (Kaboski and Townsend, 2012). However, this literature offers no direct evidence on the effect of credit withdrawals.

[3]Some of the research in this area includes Banerjee, Duflo, Glennerster, and Kinnan (2013), Augsburg et. al. (2012), Crépon, Devoto, Duflo, and Parienté (2011), Karlan and Zinman (2011), Karlan and Zinman (2010).

A new strand of research on household consumption and problems in credit supply has risen in the aftermath of the 2008 financial crisis. For example, Mian and Sufi (2010) argue that frictions in the supply of credit made it difficult for highly leveraged households to continue to make durable consumption purchases and residential investments during the recession of 2007-2009. Similarly Mondragon (2014) estimates that supply shocks to household credit during the financial crisis reduced employment by over 4 percent. Households with high exposure to micro-credit are likely to be similar to the highly leveraged households at the time of the financial crisis.

In this paper, we use a natural experiment of an unexpected and effective withdrawal of micro-credit access at the level of a large state in India, and data with quarterly observations on average household consumption available both before and after the intervention. This overcomes the limitation of lack of data availability to a certain extent, and directly addresses the question of what effect large scale micro-credit withdrawal has on household consumption.

2.1 Microfinance in India

Micro-finance institutions are part of a large but fragmented credit industry that provides micro-loans. The industry consists of three kinds of providers:

- Bank linked Self-Help Group (SHG) programs. These are programs run by commercial banks to lend to groups of 10 to 20 women.
- Micro-Finance Institutions (MFIs). These are private sector entities in the business of extending credit to small groups similar to that of the SHGs. These include not-for profit non-governmental organisations (NGOs), and for-profit non-banking finance companies (NBFCs). The model of lending is the joint-liability group (JLG) model where non-repayment by a member of the group has repercussions on credit availability for the entire group.
- Traditional money lenders.

While loans outstanding of MFIs were much smaller compared to that of the SHGs, the growth rate of the MFIs had been larger than that of SHGs at the time of the crisis (Srinivasan, 2009).

The customers of these products, especially the SHGs and MFIs, are women, whose families typically have high volatility in incomes, do not have adequate resources to deal with emergencies, lack collateral, and are denied credit by the mainstream banking sector.

Lending practices of MFIs vary, but typically, loans are made to informal groups consisting of 5 to 10 women. These groups work through "social collateral", in the form of group co-guarantees i.e. defaults on repayments by one member affect the credit availability for all others. In this way, members regulate each other for repayment, and credit is obtained without collateral. Repayments are also split over lower frequencies, sometimes even daily or weekly, making it easier to match cash-flows.

Groups that are new to the MFI get smaller loans, and the size of the loan increases with every repayment cycle, as the group builds a credit history with the MFI. At the time of the Ordinance discussed in the paper, interest rates varied, with an average of about 30 percent.[4]

The MFIs had come to be regarded as an important development in the financial sector, because they had been perceived as coming closest to taking over the role of the traditional informal-sector money lender by providing lending services in areas where the formal financial sector did not reach. This perception had earned them the accolade of being responsible for improving financial access for the disadvantaged parts of the population (Thorat, 2007; Rangarajan, 2008) while the other banking sector alternatives (with the exception of the SHGs) had failed to do so.

2.2 A natural experiment in credit withdrawal

The state of Andhra Pradesh (AP) was the locus of growth of the micro-finance industry in India. It was at the forefront of promoting the Self-Help Group (SHG) program, which was the state-promoted version of a joint liability loan to groups of women through the largely publicly owned banking sector described earlier. It was also the state where the largest micro-finance institutions (MFIs) were head-quartered. As a consequence, AP also had the largest share of micro-credit borrowers and loans outstanding by value in India. At the same time, there were also many instances when MFIs were accused of mis-selling and using coercive collection practices.[5]

The State first threatened action against the micro-finance industry in 2005, even though

[4]Recent regulation has specified the maximum lending rate as the lower of cost of funds plus 10 percent for large MFIs (size of Rs.100 crores (US$ 15 million) and above) and cost of funds plus 12 percent for small MFIs (size less than Rs.100 crore (US$ 15 million)) or average base rate (as advised by the Reserve Bank of India) of the five largest commercial banks by assets multiplied by 2.75 (IFMR Investments, 2014).

[5]There are several sources detailing the role played by AP in the growth of the micro-finance industry in India, and the subsequent problems that bedeviled the industry in this state. These include Datta and Mahajan (2003), Srinivasan (2010), Shylendra (2006), Sa-dhan (2007), Arunachalam (2010), Sane and Thomas (2013).

only one MFI was closed and warnings issued to the rest at the time. On the claim that MFIs continued to engage in irresponsible lending practices,[6] the state government proposed an ordinance in October 2010 that imposed operational constraints on the MFIs (State government of Andhra Pradesh, 2010). The Ordinance was enacted as law in December 2010. The law aimed to "protect the women of Self Help Groups from exploitation by the Micro Finance Institutions in the State of Andhra Pradesh", and severely restricted the collection of loan repayments or the origination of new loans. Any minor annoyance to customers could now justify court proceedings. While some sort of action by the government was anticipated, the content of the Ordinance, and its impact on bringing the entire microfinance machinery to an abrupt halt had the micro-finance industry and other observers in a state of shock (Sriram, 2012). In this sense, the ban was unanticipated, and absolute.

As a consequence, more than Rs.70 billion (USD 1.2 billion) worth of loans in AP effectively went into default, with recovery rates dropping from 95 percent, on average, to about 10 percent (Srinivasan, 2012). Loan disbursements between September 2010 and March 2011 were 1.7 percent of loans disbursed in the previous six months. Outside AP, assets of the micro-finance industry rose by 25 percent during the same period (MFIN, 2012).

The state government is said to have facilitated a larger disbursal of SHG loans that year, as a counter-measure. However, provisional data indicates that the number of SHGs declined by 0.18 million and disbursements declined by Rs.3.5 billion (USD 60 million) compared to the previous year. The estimated credit shortfall was estimated to be about Rs.30 billion (USD 500 million) (Srinivasan, 2012).

This shortfall in credit is also reflected through other studies. For example, Ghiyazuddin and Gupta (2012) found that majority of the clients found it more difficult to raise credit after the crisis. Household consumption, education, and health experienced change in ability to raise finance for, with 85 percent, 81 percent, and 83 percent of clients respectively indicating that financing for these needs has become more difficult. Approximately a third of clients reported large fall in spending across all needs.

[6] Such practices included lending without due diligence on the ability of the group to repay, lending for consumption and consumer durables, lack of transparency in operations, charging usurious interest rates, and employing coercive recovery practices (Yerramilli, 2013).

2.3 Research questions

We use this episode as a natural experiment to answer the following questions about how micro-credit withdrawal affects consumption:

1. Does average household consumption fall when access to micro-credit is reduced, even when micro-credit appears to be a small component of a larger credit industry?
2. Is there cross-section variation in the impact of the ban – are regions with higher micro-credit exposure more affected?
3. Is there an effect on average saving?
4. Does the volatility of average consumption change?

Empirical studies on credit withdrawals from the payday lending industry in the developed economies offer a mixed evidence on the impact on welfare (Morgan and Strain, 2008; Zinman, 2010; Morse, 2011).

A different perspective may be brought to bear in this natural experiment. The ban on the operations of the MFIs meant that neither could they offer fresh credit nor could they collect on existing outstanding loans. Thus, in the case of the AP ban, borrowers received a windfall from non-repayment to MFIs. This in turn leads us to hypothesise that the ban may not lead to a drop in average household consumption.

However, the value of the windfall is the value of the repayment due at that time, and this is likely to be small depending on where the household was in its credit cycle. The windfall is also short-term in nature. Zeldes (1997) suggested that even if the current liquidity constraints are not binding, the possibility that such constraints will bind in the future will lower the current consumption of a risk-averse individual. Further, higher uncertainty leads households to hold higher buffer-stocks, and even lower consumption expenditure. This may lead to higher savings rather than higher consumption as a consequence of the ban.

3 Data characteristics

The database used in the analysis is the "Consumer Pyramids" (CP).[7] The data consists of quarterly averages calculated for the households within a defined geographical region

[7] The CP database is created by the Centre for Monitoring Indian Economy (CMIE). http://www.consumer-pyramids.com. It is based on a national panel survey covering 150,000 households, which has been administered every quarter from June 2009 onwards.

called a *Homogeneous Region* or HR.[8] The database covers a total of 200 HRs in India and 14 HRs in the state of AP.[9] Among the HRs in AP, there are seven in a rural region and seven in an urban region, which provides variation in levels of credit constraints as measured by credit access. Typically, rural regions have a significantly lower level of formal financial presence compared with urban regions.

The CP database is unique by the standards of other databases that are used to answer the research questions presented in Section 2.3 because it has been administered over a large number of households with the geographical coverage described above, for several periods *before and after* the intervention of the ban. The advantages of this database are described in detail below.

3.1 Frequency and span

The CP database has been administered over the sample every quarter from 2009 onwards. The first quarter covered in the dataset is June 2009, and the last quarter considered in our paper is September 2012. This gives us observations for 14 quarters, spanning nearly four years from 2009 to 2012. The micro-finance ordinance was announced in September 2010 and implemented in December 2010. This allows us to measure the *short-term* impact of the ban as the change in household behaviour over four quarters, and *longer-term* impact over seven quarters after the ban.

CP provides aggregate estimates for the underlying households surveyed. It uses projected growth rates in households from Census data to arrive at the weighted aggregate (and average) estimates. Growth rates observed during the decade 1991-2001 were first used to make projections for all the years and quarters. Once the 2011 Census weights were available, estimates for all quarters in 2011 were recalibrated using growth rates in the 2001-2011 period.

We use two sets of information within this span of data to understand the household response to the ban on micro-credit: (1) Average household income and consumption

The standard data-set considered by the literature for consumption expenditure by Indian households is the Consumption Expenditure Survey (CES), carried out by the National Sample Survey Organisation (NSSO) at a regular but far lower frequency than the CPsurvey. Thus, while it may have been possible to get one estimate of the impact of the ban using the CES data as well, the wide spacing between the CES surveys would have implied missing out on the short-term impact of the ban on household consumption. Details on the NSSO data and further reasons for not using it in the analysis are presented in the Appendix.

[8]Each HR is a group of two or more districts within a state.

[9]Each region will cover different number of households. For example, the 14 HRs in AP covers around 7 percent of the survey households.

expenditure, reported each quarter. (2) Financial participation measured as fraction of households in an HR with borrowings from specific sources including banks and micro-finance (MFI/SHG).

3.2 Income categories

Table B.1 in the Appendix reports the various income categories reported in the CP database and the fraction of the sample that fall into these income categories. For instance, the largest fraction of the sample fall in the "I-8" category (nearly 20 percent), where the annual income of the household is between Rs.60,000 (USD 990) and Rs.96,000 (USD 1568). There is limited representation of categories with either very high incomes or very low incomes in the sample surveyed in the database.

3.3 Consumption

Table B.2 in the Appendix reports the various items that the average household in the sample surveyed in the database consumes. The largest component is food at almost 50 percent of expenditure. This along with expenditure on power items (including electricity, cooking fuel and petrol), cosmetics (which includes soaps and other toiletries), and education add to around three-quarters of the consumption expenditure of the average household in the sample. Average estimates of consumption at the level of a HR and income group are calculated using Census data, which are used to assign weights in the computation. The estimates presented in this paper are based on Census 2011 weights.

3.4 Household debt

Household debt is presented as a binary variable in the CP database: have households borrowed from a particular source or not, and is calculated for all sources of borrowing. Table B.3 presents the fraction of households in the sample which borrow, separately for different income categories as well as from different sources of credit, both informal (friends and family, money lenders) and formal (banks and SHG/MFIs). We extrapolate from the database to estimate that there are 28 million customers of micro-credit (including both SHG and MFI) in India. (Srinivasan, 2012) separately estimated this as 27 million, validating the CP database as being consistent with other research in this area.

Table B.3 shows that all income groups in India borrow from all sources. But the extent to which a given lender is the dominant source of credit for a particular group varies across groups. For instance, a larger fraction of the high income group take loans from banks, while a larger fraction of the lower income group use the micro-finance channels. The table also shows that the incidence of borrowing in AP is higher than that of the overall sample for India.

Table B.4 presents the purpose for borrowing across income categories. This shows that consumption as the most important reason for borrowing, for all but one income category (I-3). A significant fraction of the higher income categories (I-3 to I-6) borrow to make investments and to purchase consumer durables, in contrast to the lower income groups.

4 Research design

The data described in the previous section allows us to analyse changes in average consumption before and after the ban for the homogeneous regions (or HRs) in AP. It also allows us to identify HRs in the rest of India that are matched to the AP HRs, but where the ban was not implemented. This enables causal inference using a DID estimation with the AP observations as the treatment group, with their counterfactuals being HRs in states that did not ban micro-finance, but which match the treatment HRs on some observables and become the control group.

4.1 Matching procedure

The matching exercise identifies a control HR for each AP HR which is the treatment HR in the causal analysis. Both these are matched to have consistently comparable consumption in the period before the ban. The matches are identified by applying a distance measure on a set of selected match covariates.

The covariates used to match treatment and control HRs on consumption, include a mixture of characteristics that measure prosperity and access to finance such as:

1. Average household income.
2. Number of households.
3. Working population, the fraction of the HR between 20 and 60 years.
4. Proportion of households who have graduated the 10^{th} grade.
5. Proportion that is financially excluded, measured as the fraction of households without a bank account, credit card, life insurance policy, or other formal financial products.

6. Proportion of farmers in the region.[10]

We do not use the presence of micro-finance institutions as a match variable because it is directly effected by the treatment. We do not use consumption variables in the matching procedure as it introduces difficulties in estimation (Stuart, 2010).

Once the match covariates are selected, matching is implemented using the Mahalonobis distance measure for nearest neighbour matching, This is calculated as:

$$D_{ij} = \left[(X_i - X_j)'\Sigma^{-1}(X_i - X_j)\right]^{\frac{1}{2}}$$

where D_{ij} is the distance between unit i and j and X_i and X_j are the characteristics of the control and treatment units. A "1:1 nearest neighbor matching" method selects the control unit with the smallest distance from any given treated unit i. This measure was used because the policy change was exogenous. Alternative measures are tested as part of the sensitivity analysis in Section 6.2.

We exclude HRs from the neighbouring states of south India[11] from the set of possible control HRs because they may have suffered from an indirect treatment effect, either through shocks in inter-state trade or because of the similarity of political actions by state governments to the AP government.

4.2 The difference-in-difference (DID) estimator

The following DID model estimates the causal impact of the ban:

$$C_{i,t} = \beta_0 + \beta_1 \text{AP}_{i,t} + \beta_2 \text{POST-CRISIS}_{i,t} + \beta_3(\text{AP}_{i,t} \times \text{POST-CRISIS}_{i,t}) + \epsilon_{i,t}$$

where $C_{i,t}$ is the average household consumption, AP is a dummy which takes value "1" if i is a region in AP (the treatment HR) and "0" otherwise (the control HR). POST-CRISIS captures whether the observation is from the period before the ban (post-crisis = "0") or after (post-crisis = "1").

$\hat{\beta}_3$ will be negative and statistically significant if there is a greater fall of average consumption in the treatment HRs after the ban compared to the matched HRs. The matching DID estimator considerably improves on standard matching estimators (Blundell and Dias, 2000) by eliminating unobserved, time-invariant differences between the treatment

[10] The household participation in the agricultural sector of a region has implications for fluctuations in income and therefore, the need to access finance for consumption smoothing (Rosenzweig, 2001).

[11] These include the states of Tamil Nadu, Kerala and Karnataka.

Table 1 Match balance using t-stat and standardised difference

This table presents the match balance statistics between the treatment and control group. The p values (p-val) are generated from the t-test, and SDIFF reflects the standardised difference.
percent balance improvement refers to the improvement in balance after matching for all the covariates.

	Means Treated	Means Control	Mean Diff	t-stat	p-val	SDIFF	% Bal. Impr.
Average HH income	10.25	10.33	-0.09	-0.80	0.43	-24.78	58.30
No.of HH	7.08	6.71	0.37	1.58	0.13	40.66	52.99
Working	3.65	3.47	0.17	0.29	0.77	7.16	49.08
Graduated 10^{th} grade	2.59	2.51	0.08	-0.52	0.60	-12.21	84.65
Financial excluded	4.35	4.27	0.08	1.24	0.23	28.49	91.71
Farmer	0.36	0.24	0.12	-0.84	0.41	-19.69	80.01

and control groups (Smith and Todd, 2005). It is also an improvement on a simple DID where the treatment and control units may not have match balance.

4.3 Evaluating the design

We now evaluate the design from three points of view. Is there match balance? How does the design fare when presented with a placebo (a non-event)? How much power does the design have?

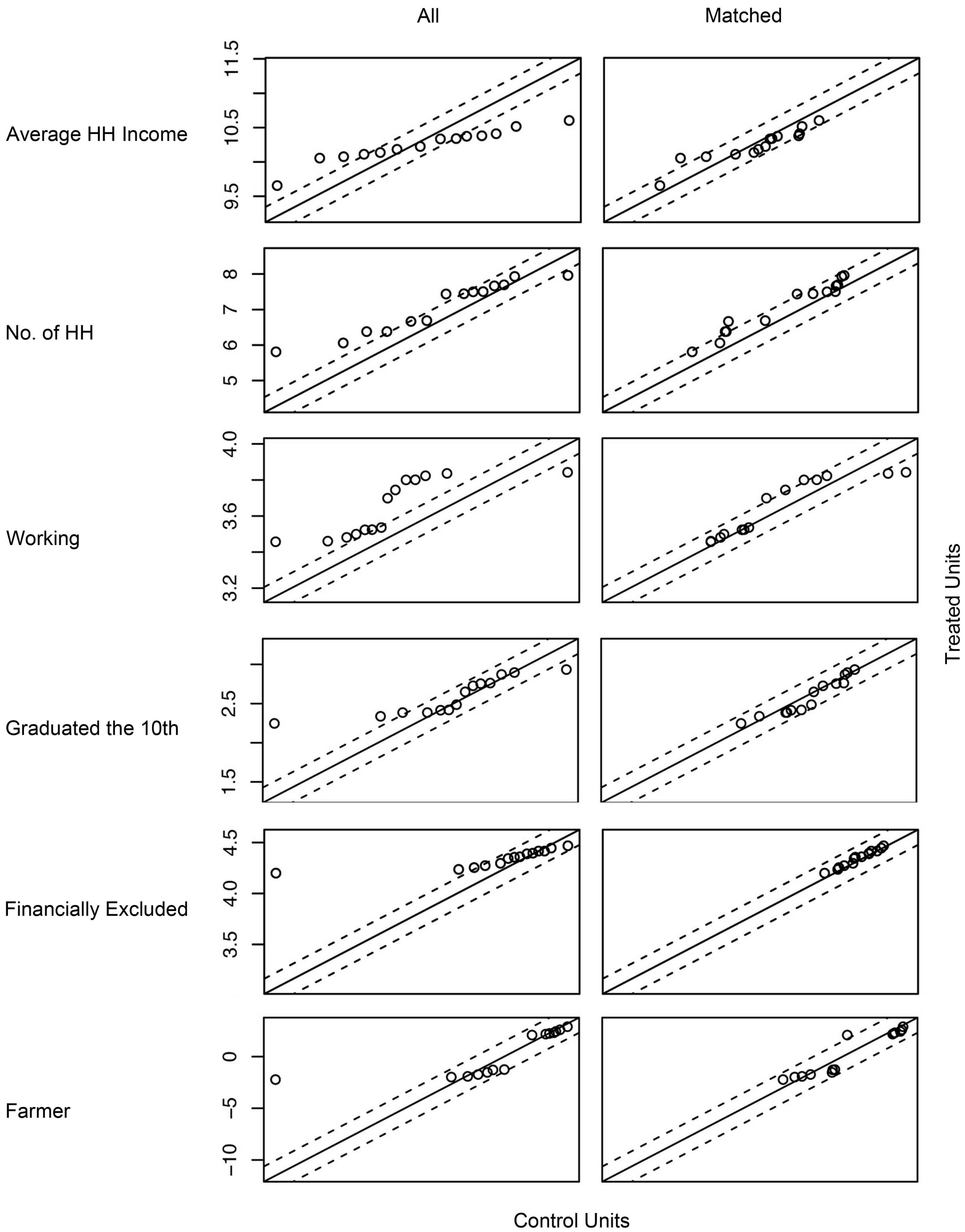
4.3.1 Is there match balance?

In a sound design, achieving match balance implies that the distribution of the match covariates for the treatment and control HRs are equal in the pre-treatment period. Without adequate match balance, the estimation of the treatment effects relies on extrapolation, which is fraught with difficulties (Rosenbaum and Rubin, 1983).

Parametric tests such as the t-test do not accurately capture match balance as they compare only the averages rather than the entire distribution (Stuart, 2010). An alternative test is a visual inspection of the quantile-quantile (QQ) plots for each covariate pair used in the matching exercise. If there is match balance, the points would fall on the 45° line. Figure 1 shows that this is not the case with the full dataset, but is substantially the case with the matched data. There is also the Hotelling's T-square test which considers the joint significance of the differences between the co-variates. The sample χ-statistic is 10.44 with a p-value of 0.12 and the F-statistic is 1.35 with a p-value of 0.27. Both indicate that the differences between the covariates are not jointly significant. The four tests together confirm that match balance is obtained.

Figure 1 QQ plots of the match covariates

The figure plots the QQ-plots of the covariates used in matching, before and after the matching exercise. The y-axis in each box reflects the treated units and the x-axis the control units. Deviations from the 45° line indicate differences in the empirical distribution and a low match balance. In all cases, there is good match balance after the matching is done.



4.3.2 Analysing a placebo

One test of the soundness of the estimation strategy is how it behaves in a Monte-Carlo simulation with a placebo, where the null of no change is true. In the placebo simulation but the AP HRs are excluded. Instead, 14 HRs are selected at random as the treatment group for which we replicate the matching exercise to find control HRs and test the difference between the treatment and control HRs using a DID estimation.

We find that the null of no change is rejected 0 percent of the time in a simulation which is repeated 10,000 times. While the rejection rate is not the same as the size of the test, the results act as a check that the research strategy does not falsely reject the null when presented with a placebo.

4.3.3 What is the size of the impact that can be resolved by the design?

Since there are only 14 out of 200 HRs where the ban applies, our ability to reject the null, when the null is not true, may be limited owing to small sample. Hence, we undertake a power study in order to assess the magnitude of deviations from the null that can be detected using our research design. This is implemented using simulations where 14 HRs are randomly chosen to be a treatment group, and a shock is artificially induced into this set. The matching exercise is repeated and a DID estimator is used to test whether there is a statistically significant difference between the treatment and the control at 95 percent level of significance.

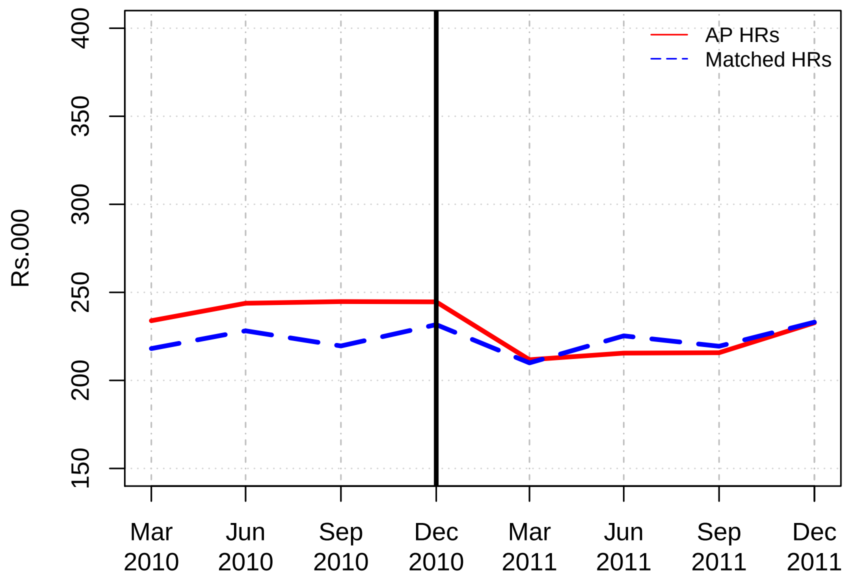
We find that the size in the change in consumption that can be discerned with a 70 percent probability is Rs.6000 (USD 99). This implies that a change in total consumption of Rs.6000 per quarter is discerned by a 95 percent test with 70 percent probability. This shows that the research design has fairly high power.

5 Results

We analyse for the causal impact of the ban on the AP micro-finance industry on household consumption. We first present graphical evidence on average consumption around the date of implementation of the ban, and then present the results of a DID regression.

Figure 2 Average household consumption in AP vs. matched, 2010-2011

The graph shows the average quarterly household consumption in the treatment HRs (in AP) and control HRs (matched, outside AP) between the March 2010 to December 2011 quarters.
The vertical bar marks the quarter in which the state government passed the micro-finance ban in Dec 2010. If we compare the four quarters from March 2010 up to the ban, and the four quarters after, we see that while consumption in AP dropped sharply relative to consumption in the matched HRs.



5.1 Did consumption fall with reduced access to micro-credit?

Figure 2 plots the average household consumption in the 14 AP HRs as the solid line in the graph, from June 2009 up to September 2012. The average household consumption for the control HRs is plotted as the dashed line.

The graph in the figure shows that over the period of four quarters after the ban, the average consumption of AP households *dropped* by 9 percent. At the same time, average household consumption dropped by only 1 percent for the matched, non-AP households. The drop in AP in the quarter immediately after the ban was almost 13 percent, while that in non-AP control regions was lower at 9 percent.

Further average household consumption in AP was higher than that in the matched HRs before the ban. But after the ban, the AP levels have remained lower than the average household consumption in the matched, non-AP regions reaching parity only by December 2011.

Table 2 DID estimates for average household consumption across treatment and control HRs

The table presents $\hat{\beta}_3$ from the following DID estimation:

$$C_{i,t} = \beta_0 + \beta_1 \text{AP}_{i,t} + \beta_2 \text{POST-CRISIS}_{i,t} + \beta_3(\text{AP}_{i,t} \times \text{POST-CRISIS}_{i,t}) + \epsilon_{i,t}$$

The table also reports heteroskedasticity-consistent standard errors (std.err.), p-values (p.val), the adjusted p-values (adj.p) and Q_{power} which is the effect size for different consumption components that could be detected with 70 percent probability.

$\hat{\beta}_3$	std.err.	p.val	Q_{power}
-2677.1	1204.6	0.03**	6000

** indicates significance at 5%
The total number of observations is 220

The DID regression estimates in Table 2 shows $\hat{\beta}_3 = -2677.1$. On a base of average HR quarterly consumption of Rs.17,471.5[12], this implies a drop of 15 percent, which is significant at 95 percent level of confidence.

This implies that the ban of the micro-finance industry by the state government of AP had a large and adverse effect on the average household consumption in the state. For such a magnitude of impact to hold on average, it would require that all households suffered a negative drop in consumption, regardless of whether they were high-income or low-income category households.

Before we draw any conclusive inferences, we first address some possible criticisms about the matching approach we have used.

Were there consumption differences prior to the ban?

One concern of the matching exercise is that the consumption between AP and the matched HRs was different even before the micro-finance ban. Figure 2 shows that there is a parallel trend showing that the AP consumption was consistently above consumption in the control regions for all quarters prior to the ban. After the ban, consumption in control regions was consistently *above* that of the AP regions, reaching parity only by December 2011. This implies that the impact of the ban was negative *and* persistent.

We test this statistically. We simulate another placebo where the treatment and control HRs are identified as in Section 4.1, but for the periods *before* the micro-finance ban. This includes the period from 2009 up to the last quarter of 2010. If there is no significant

[12]This is calculated as average household consumption in AP and non-AP in Figure 2 divided by the number of HRs

difference in the average consumption between the AP and matched HRs in the periods prior to the ban, we can infer that the results are not driven by differences in prior consumption.

We find that $\hat{\beta}_3 = -1906.95$, with s.e. $= 1258.88$ and t-test $= -1.51$. The difference in average consumption in AP and control HRs in the pre-ban period, between 2009 and 2010, is not statistically significant. Thus, the results are robust to differences in prior consumption.

Does the extent of indebtedness in AP matter?

One of the main motivations for the ban used by the AP State Government was that MFI lending was leading to a buildup of household debt beyond their ability to repay, which would eventually cause households to suffer financial distress. Indeed, Section 3.4 shows the average household in AP had a higher level of loans compared to the average level in India. But in the matching exercise, we explicitly ignored all aspects of household characteristics that were related to lending.

This raises concerns of whether the match process identifies controls that have very different levels of borrowing compared to the treatment. If control HRs had as high a level of borrowings, would a DID estimation reveal that the AP HRs are better off after the micro-credit ban? In order to test this, we search for controls that are also better matched for the indebtedness of the AP HRs, using two approaches:

1. Include as covariates the fraction of household with borrowings, and the fraction of borrower households who had debt outstanding with a SHG/MFI before the crisis.

   With this, we identify a fresh set of control HRs.[13] If the high indebtedness of AP HRs does lead to a reduction of household welfare, then the drop in consumption seen in AP after the ban will be much less than a drop seen in the other HRs that are similarly indebted.

2. Add the states of South India as candidates for the control group.[14]

   Section 4.1 argued that HRs from the states of South India were removed from the control pool in order to avoid spillover effects. However, these states have similar levels of indebtedness as AP. To the extent that a certain partial treatment effect was probably found all over South India, the use of HRs from South India in the control pool would

[13]We continue to achieve match balance on the initial variables. The match balance on the proportion of indebtedness households was mixed: the standardised difference was 72.5, and the t-stat was statistically significant at 5%. However, the KS-stat was at 0.15 indicating that the distributions were relatively close even if the mean was not. We did not achieve balance on the SHG/MFI variable.

[14]We achieve match balance on all variables.

Table 3 DID estimates adjusting for the high indebtedness in AP

The table presents $\hat{\beta}_3$ from the following DID estimation:

$$C_{i,t} = \beta_0 + \beta_1 \textsc{ap}_{i,t} + \beta_2 \textsc{post-crisis}_{i,t} + \beta_3(\textsc{ap}_{i,t} \times \textsc{post-crisis}_{i,t}) + \epsilon_{i,t}$$

The table also reports heteroskedasticity-consistent standard errors (std.err.), p-values (p.val), the adjusted p-values (adj.p) and Q_{power} which is the effect size for different consumption components that could be detected with 70 percent probability.

Adjustments to matching	$\hat{\beta}_3$	std.err.	p.val
Include fraction of borrower households	-3060.9	1261.4	0.02**
Include fraction of SHG/MFI borrower households	-3096	1341	0.02**
Include South Indian states	-2451.7	1148.8	0.03**

*, ** indicate significance at 10% and 5% respectively
The total number of observations is 220

generate a downward bias in the measured treatment effect. We re-run the matching exercise including the southern states to identify a fresh set of control HRs.

We re-run fresh DID estimations using the new control regions as described above, and present the results in Table 3 which finds that $\hat{\beta}_3$ is around -3000 in all cases. This corresponds to the declines in the main results.

What does the fall in consumption say about welfare?

While consumption is regarded as a good proxy for material well-being (Meyer and Sullivan, 2003), it could be argued that consumption levels were irrationally high before the ban, and any fall in consumption cannot be interpreted as welfare reducing.

AP saw large drops in food expenditure after the ban (See Figure 3). International research has documented the welfare consequences of fall in food consumption owing to increases in global food prices.[15] This makes it hard to argue that the ban has no negative consequences for households in the AP region.

5.2 Is there cross-sectional variation in the impact?

Since household level data is not available, it is difficult to establish cross-sectional variation in how households with different liquidity constraints respond to the micro-credit ban. However, the CP database records which HRs are rural and urban. We expect that rural regions will suffer a higher impact because these regions are more financially

[15] For example, see (Ivanic, Martin, and Zaman, 2012; Ferreira et. al., 2013).

Figure 3 Average food consumption in AP vs. matched, 2010-2011

The graph shows the average quarterly household consumption on food treatment HRs (in AP) and control HRs (matched, outside AP) between 2010 and 2011.
The vertical bar marks the quarter in which the state government passed the micro-finance ban in Dec 2010.

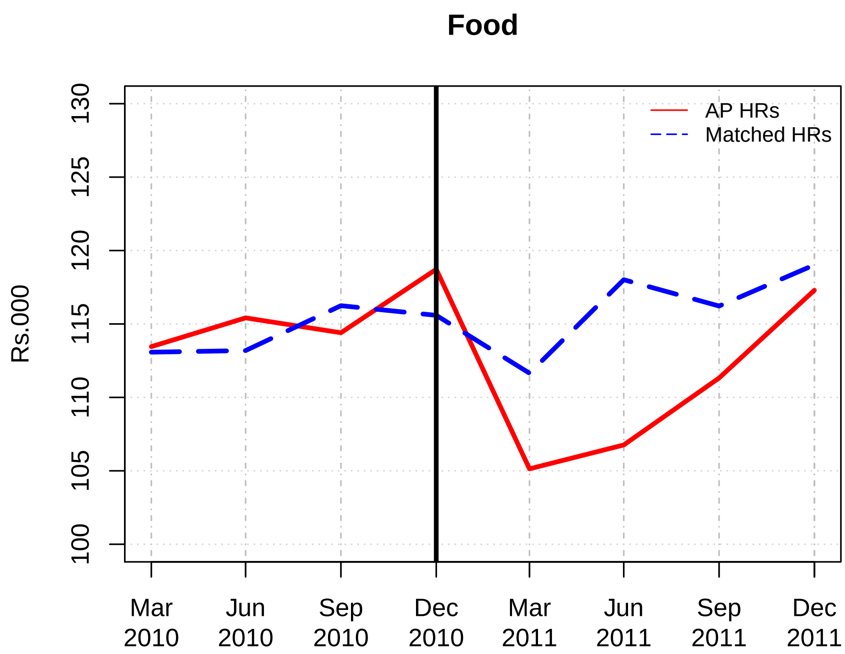


Table 4 DID estimates between rural and urban HRs

This table presents the estimation results for:

$$\begin{aligned} C_{i,t} &= \beta_0 + \beta_1 \textsc{ap}_{i,t} + \beta_2 \textsc{post-crisis}_{i,t} + \beta_3(\textsc{ap}_{i,t} \times \textsc{post-crisis}_{i,t}) \\ &\quad + \beta_4(\textsc{ap}_{i,t} \times \textsc{post-crisis}_{i,t} \times \textsc{rural}_{i,t}) + \epsilon_{i,t} \end{aligned}$$

where $\textsc{rural}_{i,t}$ takes the value 1 if the i^{th} HR is rural and 0 if it is urban. β_4 is the impact of the ban on rural AP HRs compared to urban HRs. The results include the heteroskedasticity consistent standard errors and the adjusted p-values.

$\hat{\beta}_4$	std.error	p.val
-5059.2	555.9	0.00***

*** indicates significance at 1%
The total number of observations is 220

excluded and have a greater dependence of micro-finance than the former. In order to test for cross-sectional variation, we estimate the following DID model:

$$\begin{aligned} C_{i,t} &= \beta_0 + \beta_1 \textsc{ap}_{i,t} + \beta_2 \textsc{post-crisis}_{i,t} + \beta_3(\textsc{ap}_{i,t} \times \textsc{post-crisis}_{i,t}) \\ &\quad + \beta_4(\textsc{ap}_{i,t} \times \textsc{post-crisis}_{i,t} \times \textsc{rural}_{i,t}) + \epsilon_{i,t} \end{aligned}$$

where $\textsc{rural}_{i,t}$ takes the value 1 if the i^{th} HR is rural and 0 if it is urban. Then β_4 captures the effect of the ban on a rural HR in the treatment units compared with a rural HR in the control unit. The estimated results are presented in Table 4, and show that rural AP saw a decline in average consumption. This suggests that regions lacking wider access to finance suffered more as a consequence of the ban.

The large, negative and significant estimate of β_4 suggest that rural households, that can access fewer credit sources, are more vulnerable to shocks in their consumption, compared to urban households with more sources of credit access.

5.3 Did volatility of consumption rise with lower access to micro-finance?

Table 5 presents the standard deviation of the percentage change in average household expenditure in the period before the ban as $\sigma_{\text{pre-ban}}$ and after the ban as $\sigma_{\text{post-ban}}$. We calculate the percentage change in the average expenditure for the 14 HRs in AP and the 14 control HRs as seen in Figure 2.

The results suggests that there has been an increase in the volatility of household expenditure between the treatment and control HRs. Since access to finance is important

Table 5 Volatility of consumption before and after the ban

The table presents the volatility of average household consumption for the AP and the control HRs, in the pre-ban and the post-ban quarters, where * shows that $\sigma_{\text{post-ban}}$ is higher at 94% level of significance.

	Treatment HRs		Control HRs	
	$\sigma_{\text{pre-ban}}$	$\sigma_{\text{post-ban}}$	$\sigma_{\text{pre-ban}}$	$\sigma_{\text{post-ban}}$
Average household consumption	5.87	7.87	6.33	4.72

for consumption smoothing, the withdrawal of such access impacts not just the level but also the volatility of consumption. The results are consistent with this expectation, with higher volatility in the consumption of treatment (AP) HRs relative to the control HRs.

5.4 Is there an effect on average saving?

Table 6 compares the average household savings and the saving rate for treatment and control HRs, and finds that the saving rate is higher for the treatment HRs. These results suggest that the households in the treatment HRs are building up buffer stocks to hedge against future uncertainty, and a future lack of credit access.

Table 6 DID on average household savings and savings rate

The tables shows the DID estimates of average savings and savings rates in the treatment and control groups.

	coeff	std.error.HC	p.val
Average hh. savings	-449.59	1709.69	0.79
Saving rate	7.03	3.55	0.05*

* indicates significance at 10%
The total number of observations is 220

6 Threats to validity

An assumption in the current research design is that outcomes from the treatment and control group are likely to be driven by a set of common factors, and that the only differing factor is the micro-finance ban. In that case, the fall in average consumption observed in the AP HRs can be solely attributed to the ban. In this section we address alternative explanations that might explain the fall in the average consumption. Two alternatives can be offered:

1. Some other event in AP caused the same result.

2. The results are sensitive to the matching strategy.

6.1 Did another event in AP cause the results?

A possible criticism is that some event in AP, other than the micro-credit ban, is what caused the drop in consumption observed in Section 5. For instance, if there was a drought or a flood in AP but not the other states, this would adversely effect the average consumption of the AP HRs but not the controls.[16]

One way to address this concern is to analyse the drop in consumption in a given HR by the exposure of the HR to micro-finance. The exposure to micro-finance is the fraction of borrower households in an HR that have loans from SHG/MFI (Table B.3). If a treated HR with a higher micro-finance exposure sees a larger drop in consumption after the ban, the drop can be more confidently attributed to the ban.

Figure 4 presents the change in average household expenditure for the 14 AP HRs by the micro-finance exposure of the HR. The top panel shows the drop between the first quarter after the ban, and the last quarter before the ban. The bottom panel shows the drop between the second quarter after the ban, and the last quarter of the ban.

Consumption was actually slightly higher in HRs with higher micro-finance exposure just immediately after the ban - this perhaps is a result of the windfall discussed earlier. Consumption dropped for 13 out of 14 HRs in the bottom panel. The decline in consumption is larger on average with higher exposure to micro-finance.

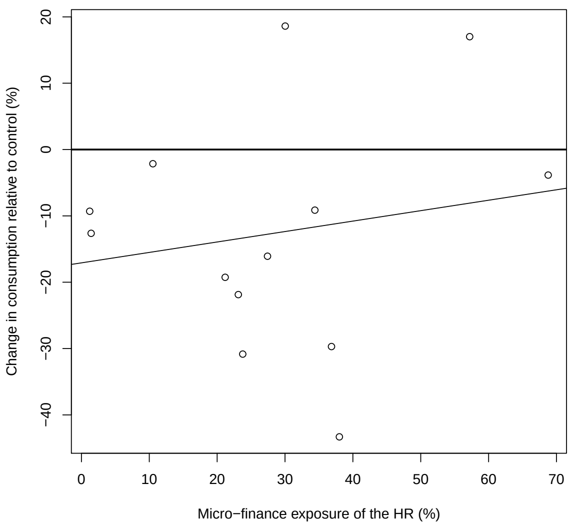
Another method of testing if other changes were driving the fall in consumption (such as the drought) is to examine changes in average income of HRs. If we do not find a significant fall in the average income of HRs, then we can more confidently claim that the consumption decline was a result of the ban.

Table 7 reports the DID regression of the average income in the treatment and control HRs. The estimated coefficient is negative but not significant. This suggests that there was a likely negative shock on income in the treatment HRs after the ban, and implies that there may have been some factor in AP that may have contributed to the drop in consumption other than the ban. However, it is unlikely to have caused the full magnitude of the negative drop in consumption, particularly given that the drop in consumption is strongly correlated with the micro-finance exposure of the region.

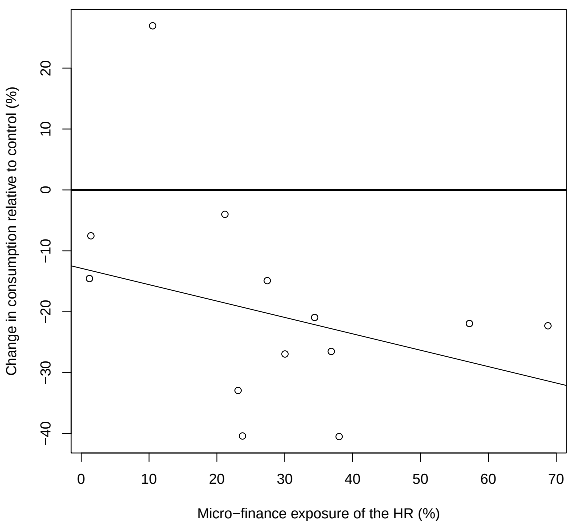
[16]There was a drought in AP in the period of July to September 2011. However, this falls after the period analysed in this paper.

Figure 4 Impact of the ban by micro-finance exposure

This figure plots the impact of the ban on household consumption in the AP HR by the micro-finance exposure of that HR. Here, impact is measured as the change in consumption expenditure in the AP HR before and after the ban relative to the consumption expenditure in the control HR before and after the ban. Micro-finance exposure of the HR is measured as the fraction of borrower households in AP that have borrowed from the SHG/MFI category. The graph shows that there is a sharper fall in consumption with higher exposure to micro-finance.



(a) Consumption change in the first quarter after the ban



(b) Consumption change in the second quarter after the ban

Table 7 DID estimates of changes in average income

This table presents the $\hat{\beta}_3$ DID estimators from:

$$AI_{i,t} = \beta_0 + \beta_1 \text{AP}_{i,t} + \beta_2 \text{POST-CRISIS}_{i,t} + \beta_3(\text{AP}_{i,t} \times \text{POST-CRISIS}_{i,t}) + \epsilon_{i,t}$$

where AI is the average income of the HR. The results include the heteroskedasticity consistent standard errors and the adjusted p-values.

	$\hat{\beta}_3$	std.err	p.val
Average income	-3502	2248	0.12
The total number of observations is 220			

6.2 Are the results sensitive to the matching strategy?

One concern is how sensitive the results are to either the set of matching covariates or the specific matching algorithm used for the results in Section 5. We re-estimate the DID using other matching approaches including:

1. Create a new set of matching covariates by dropping one covariate used in the original matching procedure, re-estimating the DID and testing for the change in results with the new matched HRs.

2. Using a *genetic matching* algorithm over the same covariates to obtain a set of control HRs.

   This is a method of multivariate matching, that uses an evolutionary search algorithm to determine the weight each covariate is given (Diamond and Sekhon, 2012).

3. Using the "proportion of women in an HR" as a match variable as a proxy for exposure to micro-credit, since micro-finance institutions (either SHG or MFI) typically lend to women.

4. Use the neighbouring HRs of AP instead of HRs that come out of a matching estimator.[17]

Table 8 shows estimates from these DID estimations. All the estimated coefficients are negative and statistically significant. While different approaches yield coefficients that vary in magnitude, the direction and significance in each case is consistent with those of the estimates presented in Table 2. Thus, we infer that our original results are not sensitive to the matching strategy used.

[17] These include the following HRs: Baleshwar-Gajapati, Malkangiri-Nayagarh and Dhamtari-Dantewada in Orissa, Hingoli-Gadchiroli and Jalna-Osmanabad in Maharashtra, Bidar-Bellary, Chitradurga-Mysore and Bangalore-Kolar in Karnataka, Chennai-Vellore and Coimbatore-Dharmapuri in Tamil Nadu.

Table 8 DID estimates across varying matching strategies

	$\hat{\beta}_3$	std.err	p.val
Baseline result			
	-3375.1	1450.5	0.02**
1. Dropping one covariate at a time			
Average household income	-2934.3	1181.4	0.01***
Number of households	-3183.1	1092.2	0.00***
Working age population	-2431.0	1211.1	0.05**
Graduated 10^{th} grade	-2379.5	1185.7	0.05**
Financially excluded	-3141.9	1292.9	0.02**
Farmer	-3389.7	1237.2	0.01***
2. Using a genetic matching algorithm			
	-2952.4	1228.3	0.02**
3. Adding proportion of women			
	-3183.1	1092.2	0.00***
4. Neighbouring HRs of AP			
	-1182.6	443.8	0.01***

*** indicates 1%, ** 5% and * 10% level of significance
The number of observations for all regressions except neighbouring HRs of AP is 220
The number of observations for neighbouring HRs of AP regressions is 272

7 Conclusion

The rapid growth in the micro-credit business by financial firms in recent decades have been dogged by concerns about consumer protection. Policy makers worldwide have implemented a variety of interventions, ranging from relatively subtle rules on consumer protection to outright restrictions. The optimal public policy response to new kinds of micro-credit firms is constantly evolving, drawing from a small literature analysing the impact of these interventions.

The contribution of this paper lies in utilising a large natural experiment – a complete ban on micro-credit in the Indian state of Andhra Pradesh (AP) that has the population of Germany – and in having high quality measurement of average consumption from a large panel of households observed every quarter all over the country. Since the ban was imposed in only one state, controls from other locations without the ban are used to draw causal inference about the impact.

The results suggest a fairly large negative impact of the ban on micro-finance. By the most conservative estimates, consumption dropped by between 14-15 percent in the short-term after the ban. The drop was even higher in rural regions where the choices to access formal sources of finance is lower. The volatility of consumption also increased, and there is some evidence that households in AP increased savings in order to hedge against the higher uncertainty of future credit access. The effect is not just short-term. Even seven quarters after the ban, average household consumption in AP remained lower than the controls. This is in marked contrast to their relative position before the ban was implemented.

Thus, while the ban on the micro-credit industry was initiated by policy makers in AP under the claim that this would help poor people, in reality it has imposed real costs on everyone in the economy.

There are intriguing analogies between the experiment in AP – where credit access was cut off abruptly for a subset of society borrowing from micro-finance firms – and the macroeconomics and finance literature on deleveraging (Eggertsson and Krugman, 2012). This literature also focuses on what happens when some borrowers in a country are highly indebted and face an abrupt shock to credit access. Since micro-credit is typically a small component of the larger credit outstanding by value, disruptions to the micro-credit industry are assumed to impact only those who have borrowed from these firms. However, a large scale withdrawal of micro-credit may have larger consequences where access to capital is constrained, in general.

A drawback of the analysis is that we only observe household aggregates at the level of geographical areas and income classes, rather than individual households. Record level data might reveal that welfare is improved without micro-finance, for *certain* households. Since we lack household level records, we cannot distinguish between a bigger impact on the households that directly used micro finance, and indirect effects on their peers in the same income class. When such data is eventually released, these effects could be measured.

The findings in this paper suggest that the overall average treatment effect associated with banning micro-finance in Andhra Pradesh was negative. One lesson is that a blunt policy instrument, such as a complete ban of micro-finance, is inadvisable. In the global debate about the welfare consequences of for-profit micro finance, this would suggest that extreme government restrictions are ill-advised. Thus, even though this analysis does not rule out the potential presence of market failure in the form of weak decision making by some poor people, the optimal response to market failures involves a more subtle approach of consumer protection, rather than the blunt instrument of a ban. These questions are important avenues for future research.

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Appendix

A The CES data

The standard data-set on consumption expenditure in India is the quinquennial survey, the Consumption Expenditure Survey (CES), carried out by the National Sample Survey Organisation. The last two surveys were carried out in 2009-2010 (known as the 66th round) and 2011-12 (known as the 68th round).[18] These years make the CES suitable for the study of the impact of the Andhra Pradesh ban on consumption expenditure. There are however three reasons that the NSS data was not used for this analysis. These are as follows:

- The CES is not a panel-survey. This makes it difficult to compare consumption in households across the two time-periods.
- The 66th and 68th rounds of the CES are two years apart. This makes it difficult to study consumption expenditures immediately before and after the ban.
- While the NSS has several sub-periods within a year, the sample in a state gets divided across the sub-periods, leading to very small sample sizes for each sub-period, even for aggregates. For example, the sample in Andhra Pradesh in CP every quarter is about 7500 households. The total Andhra Pradesh sample in NSSO is 6899.[19]
- The CES uses a moving reference period i.e. it asks households to report expenditure over the past 7 (or 30) days preceding the date of the survey. This makes it difficult to compare outcomes across households. CP on the other hand, uses a fixed reference period, that of the last three months.

B Tables

[18] The 68th round was carried out by a decision of the National Statistical Commission, even though it was only two years after the 66th round.

[19] Table 2.2, Key Indicators of Household Consumer Expenditure in India, 2011-2012

Table B.1 Income categories for all India

Income groups are formed at various percentiles, with the corresponding income values rounded to the nearest thousand rupees to reflect how respondents report their incomes.

			(in September 2010)
	Annual household income (Rs.) Lower limit	Upper limit	% share in total sample
I-1	1,000,000	Infinity	1.0
I-2	720,000	1,000,000	1.4
I-3	360,000	720,000	8.8
I-4	240,000	360,000	11.7
I-5	180,000	240,000	10.9
I-6	120,000	180,000	16.6
I-7	96,000	120,000	9.9
I-8	60,000	96,000	19.3
I-9	36,000	60,000	15.2
I-10	24,000	36,000	3.5
I-11	0	24,000	1.7

Table B.2 Components of consumption

The table presents components of average household consumption reported in the CP database for AP in September 2010 which is two quarters before the ban, and the percentage share of each component. This shows that the largest component of household consumption is food.

Consumption On	Description	% share in Total
Food		48.7
Power and Fuel	Cooking fuel, petrol, diesel, electricity	9.6
Cosmetics	Includes toiletries	7.1
Education	Books and various fees	5.5
Miscellaneous	Includes tourism, social obligations	4.8
Communication	Telephone, newspaper, TV, internet	4.6
Clothing	Garments, footwear and accessories	4.5
Transport	Bus/train/autorickshaw	3.8
Intoxicants	Cigarettes and alcohol	2.7
Rent	House rent and other charges	2.2
Monthly repayments (EMIs)	Installments on cars, durable goods, home	1.9
Restaurants		1.8
Health	Medicines, doctor fees, hospitalisations	1.6
Recreation	CDs, movies, toys	1.0

Table B.3 Who borrows and from where: India and AP in 2010-11

The table reports the fraction of the sample that borrow from various sources in the four quarters of fiscal year 2010-11, for India and for AP. The last column in the table shows the fraction of households in each income group in the region who have some borrowing. This varies from a fifth of the richest group to a bit less than half of the poorest in India. This changes dramatically in the case of AP where more than half the sample borrows.
The table also shows the sources from which the sample borrows. Of the five sources, the SHG/MFI are the micro-finance lenders. The role of banks is the highest for the **I4** income group and steadily drops away for lower income groups. Micro-finance plays an important role for households with average annual income of less than Rs.100,000 (USD 1652). These are around 8 percent of the all India sample and around 40 percent of the AP sample.

		Sources of Borrowing					
	No. of HH	Friends	Money lenders	SHG/MFI	Banks	Others	Any
India:							
I1 (Rich)	438	0.44	0.12	0.12	17.69	1.29	19.81
I2	887	2.92	2.86	0.28	15.29	3.24	19.14
I3	8222	7.88	6.56	0.70	18.77	6.85	27.08
I4	13200	9.87	7.71	1.65	19.17	7.30	31.53
I5	14314	11.60	7.83	2.09	14.59	7.23	32.42
I6	24434	16.24	10.23	4.05	13.09	8.69	36.52
I7	15189	20.60	13.01	6.12	10.41	10.66	41.97
I8	33389	21.65	14.47	6.99	7.31	11.28	40.27
I9	28796	24.69	14.26	7.64	5.20	11.67	42.55
I10	7786	29.46	14.06	7.06	4.60	10.11	46.07
I11 (Poor)	2658	30.00	13.33	6.65	3.13	8.71	44.91
Total	149313	20.42	12.45	5.76	9.18	10.10	36.69
AP:							
I1 (Rich)	2						
I2	6		67.76				67.76
I3	131	33.90	23.57	3.80	25.61	8.83	47.06
I4	471	30.72	21.45	20.58	26.10	8.51	55.67
I5	775	44.56	30.07	16.26	20.16	13.33	63.05
I6	2014	52.25	41.23	26.65	20.84	16.48	74.14
I7	1531	61.14	50.84	32.65	16.92	22.57	82.56
I8	3598	58.67	54.40	35.51	14.47	25.82	85.43
I9	2179	62.00	54.91	39.04	12.56	31.54	86.69
I10	200	60.00	58.28	34.06	9.14	29.32	88.02
I11 (Poor)	44	51.70	60.28	41.21	6.87	22.75	87.53
Total	10951	57.46	50.31	33.37	15.64	24.49	82.67

Table B.4 Purpose of household borrowing in AP, March 2010

This table presents the proportion of borrowers in AP that have borrowed for various purposes. Among the columns in the table, *Number* is the number of borrower households in each income group; *Consumption* includes reasons of general consumption, but excludes reasons of health, marriage and education; *Investment* includes borrowing for business purposes as well as investments in other instruments.

	Number of households	Housing	Consumption	Durables	Investment	Debt repayment
I-3	46	45.2	46.8	10.8	55.7	27.3
I-4	192	29.8	51.7	7.6	53.1	22.0
I-5	389	36.1	60.3	15.5	56.8	27.2
I-6	1277	33.4	65.9	18.4	44.6	28.6
I-7	1127	33.4	72.2	20.8	36.8	28.9
I-8	2930	34.3	77.8	21.2	27.3	28.2
I-9	1907	32.6	80.6	21.1	20.0	30.1
I-10	175	28.7	77.9	13.2	21.4	27.5
I-11	37	32.2	70.3	16.2	24.8	39.2

Table B.5 List of districts that make up the control

The table lists the names of all the districts that are covered in the HRs that make up the control in the paper.

Districts in Bihar:	**Districts in Odisha:**
Paschim Champaran	Bargarh
Purba Champaran	Jharsuguda
Sheohar	Sambalpur
Sitamarhi	Debagarh
Madhubani	Sundargarh
Saharsa	Anugul
Patna	Kandhmal
Bhojpur	Sonapur
Buxar	**Districts in Madhya Pradesh:**
Kaimur (Bhabua)	Jhabua
Rohtas	West Nimar
Jehanabad	Barwani
Aurangabad	East Nimar
Districts in Gujarat:	Umaria
Banas Kantha	Jabalpur
Patan	Narsimhapur
Mahesana	Dindori
Sabar Kantha	Mandla
Panch Mahals	Chhindwara
Dohad	Seoni
Vadodara	Balaghat
Narmada	**Districts in Maharashtra:**
Bharuch	Washim
Surat	Bhandara
The Dangs	Gondiya
Navsari	Gadchiroli
Valsad	Chandrapur
Kachchh	Yavatmal
Jamnagar	Hingoli
Sundernagar	**Districts in West Bengal:**
Rajkot	Darjiling
Porbandar	Jalpaiguri
Junagadh	Koch Bihar
Amreli	
Bhavnagar	