Does access to microfinance affect consumption inequality? :evidence from a randomized controlled trial in Andhra Pradesh, India

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Does Access to Microfinance Affect Consumption Inequality? Evidence from a Randomized Controlled Trial in Andhra Pradesh, India

by

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Abstract

This paper examines the impact of access to microfinance on consumption inequality using panel data of 6080 households available from a randomized evaluation conducted by Banerjee et al. (2013) in 104 slums in Andhra Pradesh, India. We find that access to microcredit exacerbates consumption inequality both at the slum-level and the household-level. Further decomposition of inequality indices shows that this difference in consumption inequality is predominantly driven by expenditure on non-food items. However, once all households across treatment and control slums have equal access to microcredit in the long-run, the disparity in consumption inequality between treatment and control slums disappears. Our results also suggest that larger loan size and higher number of loan cycles completed by older microcredit borrowers do not cause any significant divergence in consumption inequality across treatment and control households. These results imply need for targeted livelihood support programmes for those who cannot participate in microcredit programmes.

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Does Access to Microfinance Affect Consumption Inequality? Evidence from a Randomized Controlled Trial in Andhra Pradesh, India

1. Introduction

Microfinance as a potential poverty alleviation tool has made tremendous progress in terms of outreach across the globe over the last few decades. By the end of 2010, there were 3,652 microfinance institutions (MFIs) serving more than 200 million microcredit clients across nations (Microcredit Summit Campaign Report 2012).[1] In 2006, Muhammad Yunus of Grameen Bank was awarded Nobel Peace Prize in recognition of his pioneering work on group based lending model of microcredit to the rural poor in Bangladesh. Microfinance has also received considerable attention of development planners and social researchers in recent years. Existing evaluation studies of microfinance programmes typically examine impact of microcredit on borrowers' consumption, income; business creation and/or profits; and other development outcomes namely health, education, and women empowerment. While success of microcredit in achieving some of its stated objectives-enabling clients to smooth consumption, alleviation of poverty, better health and education, empowering women, etc- remains a contentious topic of research, a careful review of existing literature reveals that no systematic study has been done so far to examine impact of access to microfinance on consumption inequality. This paper fills this particular void in the literature. We examine impact of access to microfinance on consumption inequality using panel data of 6080 households available from a randomized evaluation conducted by Banerjee et al. (2013) in 104 slums in Andhra Pradesh, India.

Microcredit directly alleviates credit constraint faced by poor households who virtually have no collateral to pledge for borrowing from formal financial institutions like commercial banks, cooperative banks, etc. Hence, provisioning of microcredit leads to financial deepening

[1] We use microfinance and microcredit interchangeably in this paper though strictly speaking they are not synonymous. Microfinance is a broad gamut of financial services which beside credit include insurance and savings as well.

which has implications for inequality. Existing literature on financial deepening and its impact on income inequality find mixed results (Jalilian and Kirkpatrick, 2002; Beck et al., 2004; Kai and Hamori, 2009a). Thus, it is imperative to examine impact of microfinance on inequality as well. Ahlin and Jiang (2008) theoretically argue that microcredit reduces income inequality in the long-run. Studies that empirically examine impact of microcredit on inequality also find mixed results (Copestake, 2002; Cuong et al., 2007; Mahajabeen, 2008; Kai and Hamori, 2009b). However, these studies lack empirical rigour and their results may not be robust enough because the underlying methodology fails to control for potential biases which may affect impact estimates in either direction, positive or negative. To reiterate, no study has been conducted that systematically and rigorously examines the impact of access to microfinance on consumption inequality. Therefore, this paper extends existing literature by examining impact of access to microfinance on consumption inequality using panel data on a large number of households available from a randomized controlled trial (Spandana trial henceforth) in homogeneous neighborhoods (slums henceforth) of Hyderabad city in Andhra Pradesh, India.[2] In 2005 the group-based small loan product offered by Spandana was randomized at the slum-level. The comparison slums- where Spandana was not allowed to open branches- in Spandana trial were not counterfactual in the true sense of the term as other MFIs had started lending in these slums. However, 'the probability of receiving an MFI loan was still 8.8 percentage points (48 per cent) higher in treatment areas than in comparison areas (27.1 per cent borrowers in treated areas versus 18.3 per cent borrowers in comparison areas)' after 15 to 18 months since Spandana started its branch expansion programme in the treatment slums.[3] Three years after the intervention started in 2005, the probability of borrowing from an MFI was the same in

[2] Spandana is an Andhra Pradesh based MFI.
[3] Banerjee et al. (2013) p3

treatment and control slums but 'households in treatment groups had larger loans and had been borrowing for a longer time period.'[4]

Our approach in this paper has several advantages. First, we use data on a large number of households across 104 slums in the city of Hyderabad in Andhra Pradesh, India. Hence, we are able to capture heterogeneity to a large extent both at the household-level and slum-level. Second, the advantage of using data from a randomized controlled trial (RCT) is that it allows us to control for selection bias and unobserved heterogeneity.[5] Put differently, 'RCTs allow us to estimate what would have happened without the intervention under study.'[6] Randomization ensures that, on average, treatment slums (with access to microcredit) are comparable with control slums (without access to microcredit) in terms of several observable characteristics that potentially matter for determining participation in microcredit programme and hence its outcomes. Third, this paper uses data for a large number of households over a long period of time[7]. Thus, our study can capture some of the dynamics of access to microcredit that emerge only in the medium-run or long-run.

We find that access to microcredit exacerbates consumption inequality both at the slum-level and the household-level. Further decomposition of inequality indices shows that this difference in consumption inequality is predominantly driven by expenditure on non-food items. However, once all households across all treatment and control slums have equal access to microcredit in the long-run, microfinance seems to have no statistically significant impact on consumption inequality. Our results also suggest that larger loan size and higher number of loan cycles completed by older microfinance borrowers do not cause any divergence in inequality across treatment and control households. We also find that enhanced access to

[4] Ibid. p4
[5] Microcredit borrowers are not directly comparable with non-borrowers for the fact that the former self-select to participate in microcredit program.
[6] Karlan et al. (2009), p 168
[7] The original evaluation study followed a typical household on average for 3 to 3.5 years (Banerjee et al., 2013, p 3).

microcredit impacts consumption inequality differently across treatment and control households at various percentiles of monthly per capita consumption expenditure (MPCE).

The rest of the paper is organized as follows. Section 2 discusses the theoretical motivation and briefly reviews the related literature. Section 3 describes the design of the randomized evaluation conducted by Banerjee et al. (2013). Section 4 explicates the empirical model. Section 5 provides information on the datasets used for our analysis. Section 6 presents the main results and Section 7 concludes.

2. Empirical motivation and related literature

The empirical motivation for this study comes from the existing literature on microfinance and consumption which can be broadly divided into two broad strands of literature. One strand of literature examines the extent to which microfinance helps poor households in smoothing consumption. MFIs extend microloans to poor households to help them start a small enterprise and/or in taking up an income generating self-employment activity. It is believed that if poor women are able to secure a source of income then eventually in the long-run they are in a better position to smooth consumption as well. Moreover, there is evidence which suggests that money being fungible, funds borrowed from MFIs for productive investment purposes can also partly be used for meeting consumption needs (Zeller 1999; Dichter, 2006; Johnston and Morduch, 2008, Banerjee 2013). However, existing studies that examine the extent to which microfinance has facilitated consumption smoothing using experimental data find mixed evidence (Pitt and Khandker, 1998; Karlan and Zinman, 2009; Roodman and Morduch, 2009; Crepon et al., 2011). In this context, however, we must bear in mind that in absence of any microfinance program, households in a typical slum can smooth consumption in several other ways: precautionary saving (Paxson, 1992; Dupas and Robinson, 2008); liquidation of asset held in the form of cattle (Rosenzweig and Wolpin, 1993; Fafchamps et al., 1998); use child labor (Jacoby 1994; Dehejia and Gatti, 2002); social institutions like marriage (Rosenzweig and Stark, 1989); social capital (Grootaert, 1998; Fafchamps and Lund, 2003) and keeping credit reserve in which case borrowers borrow less than their credit limit (Diagne et al., 1998). There is no reason, a priori, to believe that these means to smooth consumption are accessible in different degrees by households across treatment and control slums. The other strand of literature examines impact of microcredit on average consumption of microcredit borrowers. Since microcredit gives direct access to credit, one can hope to see a positive impact of microfinance on consumption expenditure of microfinance clients. However, based on the randomized evaluation of the microfinance program mentioned above, Banerjee et al. (2013) find 'no difference in average consumption' across treatment and control slums. This is not an unusual finding as microevaluations of similar microcredit programmes conducted elsewhere such as Crepon et al. (2011) and Attanasio et al. (2011) find no significant impact of microcredit on consumption in rural Morocco and Mongolia respectively. A few other quasi-experimental studies find positive impact of access to credit on consumption (Kaboski and Townsend, 2005; Gertler et al., 2009). But, this is an average outcome which does not tell us anything about the spread of the consumption distribution across households in treatment and control slums. Inequality itself merits in depth analysis since it has implications for economic growth (Mirrlees, 1971; Persson and Tabellini, 1994; Alesina and Perroti, 1996) and investment in human capital (Galor and Zeira, 1993). Ahlin and Jiang (2008), using a theoretical model, argue that microcredit reduces income inequality in the long-run. Studies that empirically examine impact of microcredit on inequality report mixed results (Copestake, 2002; Cuong et al., 2007; Mahajabeen, 2008; Kai and Hamori, 2009b). However, these studies lack empirical rigor and their results may not be robust enough because the underlying methodology fails to control for potential biases that may affect impact estimates in either direction, positive or negative.[8]

[8] Copestake (2002) compare existing micro credit borrowers with "pipeline clients", that is, the clients who have been selected by the MFI as eligible to borrow following the same criteria but are yet to borrow from the MFI and finds evidence of income polarization in Zambia. Cuong et al. (2007) use fixed-effect regression method to analyze the

Our objective in this paper is to fill this gap in the existing microfinance literature. We examine inequality in terms of consumption expenditure both at the slum and household level across treatment and control slums. Since slums were randomly selected, we find no evidence of divergence in inequality measures across treatment and control slums prior to the branch expansion by Spandana in treatment slums. In other words, the level of consumption inequality was the same both at the slum-level and household-level across treatment and control before the initiation of the microcredit intervention by Spandana. However, advent of microfinance program can affect consumption inequality either favorably or adversely for the following reasons and this makes analysis of consumption inequality particularly interesting. First, one of the key results of the theoretical model developed by Banerjee et al. (2013) is that households in treatment slums with access to credit are 'more likely to buy the durable, but [their] first-period total non-durable consumption and even total consumption may be higher or lower. [Their] second period non-durable consumption will be lower.' Thus, the net impact of access to microfinance on average consumption and consumption inequality remains ambiguous, and therefore examining consumption inequality across treatment and control slums can shed further light on this. Second, if more and more households with access to microcredit tend to spend approximately the same average consumption per capita then consumption inequality will tend to diminish. Third, access to microfinance can spurt creation of micro enterprises that yield high returns (McKenzie and Woodruff, 2008). These micro enterprises generate employment opportunities for other households in the same neighborhood which can actually facilitate a greater number of households in securing income and hence in smoothing consumption. Therefore, we may expect that access to microcredit can lead to reduced

impact of micro credit on measures of inequality such as Gini coefficient, Theil's indices etc in Vietnam and find supportive evidence of microfinance reducing inequality. Mahajabeen (2008) examines impact of micro credit on inequality in a computable general equilibrium framework using calibration technique in Bangladesh and finds evidence of micro credit leading to reduced inequality. Using cross-country data, Kai and Hamori (2009a) find evidence of "equalizing effect" of micro credit on income distribution.

variability in consumption per capita in treatment slums as compared to control slums. Moreover, examination of consumption inequality at the slum level among a pool of potential microcredit clients can be a good starting point for analyzing the general equilibrium implications of access to microcredit in the long run (Buera et al., 2011).

Access to microfinance can also potentially impact consumption distribution through its impact on clients' risk-bearing capacity. There is theoretical evidence which suggests that in less developed economies with imperfect capital market, ability to take up entrepreneurship increases with risk-bearing capacity wherein access to consumption credit positively affects risk-bearing capacity (Eswaran and Kotwal, 1990). Hence, the 'able but poor agents may never get to exercise their entrepreneurial skills.'[9] Morduch (1995) provides corroborative empirical evidence to this end and argues that 'the effect of risk on production (and consequent efficiency loss) can be large, especially with respect to choices made by most poor (and most vulnerable) households.'[10] Therefore, access to microcredit can have differential impact on consumption behavior for households that differ in terms of vulnerability and hence their different risk tolerance limits. In Peru, Pearlman (2007) finds that relatively more vulnerable potential entrepreneurs are less likely to participate in microfinance program than their less vulnerable counterparts even after controlling for their skills and wealth. These households choose to invest in low-yield-low-risk projects while their less vulnerable counterparts are more likely to invest in high-yield-high-risk projects by participating and borrowing from microfinance program. Thus, we may expect to see a change in the dispersion of the consumption distribution in treatment slums but the direction of change will precisely depend on the proportion of the two groups of households discussed above in the overall pool of potential borrowers. We may however expect to see not much change in the distribution of consumption in control slums.

[9] Eswaran and Kotwal (1990), p 481.
[10] Morduch (1995), p 108.

Finally, it is also possible that households say in the lower percentiles (in terms of per-capita consumption expenditure) are more credit constrained. Thus, advent of microfinance can have maximum discernible impact on the spread of the consumption distribution in the lower percentiles of treatment slums vis-à-vis control slums than in the higher percentiles even after controlling for other factors that have potential bearings on consumption. Hence, it is imperative to examine consumption inequality in treatment group vis-à-vis control group at various percentiles of the per capita consumption expenditure. We do this percentile based analysis in this paper to fill this gap in the existing literature on microfinance and consumption inequality.

3. Experimental Design[11]

In India, microfinance has proliferated rapidly over the last few decades and number of MFIs has also gone up manifold. Andhra Pradesh, a southern state, has been a major hub of the microfinance movement in India where by November 2010, MFIs were serving more than 6 million clients (Microcredit Summit Campaign Report, p5). Spandana is an MFI in India with a very strong presence in Andhra Pradesh. In 2005, Spandana identified around 120 slums[12] in Hyderabad, the capital city of the state of Andhra Pradesh (erstwhile), for opening new branches.[13] These slums were identified for two reasons. First, slums had good number of potential microcredit clients with low level of income. Second, these slums had no presence of other MFI and the baseline survey, as discussed later, conducted in these slums confirmed this. Out of these 120 originally identified slums, 16 slums were dropped from the branch expansion plan as those were deemed unfit for microcredit programme due to high concentration of migrant workers. The remaining 104 slums were ideal for branch expansion and were used by

[11] This section borrows heavily from Banerjee et al.(2013).
[12] These slums are "permanent settlements with concrete houses and some public amenities (electricity, water, etc.)" and the population in such slums ranges between 46 to 555 households (Banerjee et al., 2013, p6)
[13] A new state called Telengana was created out of Andhra Pradesh in June 2014 and Hyderabad continues to be the joint capital of both the states.

Banerjee et al. (2013) for the evaluation study. Half of the pre-identified 104 slums were randomly assigned to treatment, that is, Spandana was allowed to open branches in these slums. The remaining slums were assigned to control where Spandana did not open any branch. A baseline survey was conducted in these slums at the household-level to glean information on 'household composition, education, employment, asset ownership, expenditure, borrowing, saving', and business activities before randomly assigning slums into treatment and control.[14] The baseline information was used for initial stratification of 104 slums. Slums were grouped into pairs for randomization based on 'average per capita consumption and per-household debt.'[15] Banerjee et al. (2013) clearly show that treatment and control slums were indeed similar or homogeneous in terms of demographic, financial and entrepreneurial characteristics. This gives us enough assurance that the randomization was proper and free of any systematic bias.

The first comprehensive follow-up household survey (Endline1 henceforth) with 6850 households was conducted in all areas between August 2007 and April 2008, 15 to 18 months after the setting up of MFI branches by Spandana in treatment slums. In order to decide the sampling frame for Endline1, a comprehensive census was conducted in all the slums in early 2007. The census showed that MFI borrowing was lower than expected in treatment slums. Therefore, households who were highly likely to borrow from an MFI were selected for the Endline1 in both treatment and control slums.[16] Moreover, households who borrowed from Spandana were oversampled. We correct our results for this oversampling to make our results fairly representative of the population parameters. It must be noted that other MFIs had started their lending operations in both treatment and control slums by the time Endline1 began. However, a statistically significant difference of 8.8 percentage points in probability of

[14] Banerjee et al.(2013) p7.
[15] Ibid.
[16] Banerjee et al. (2013) identify these households based on their duration of stay in the slum (at least three years and above) and whether the households have at least one women aged between 18 and 55.

receiving an MFI loan still prevailed between treatment and control slums. In other words, at Endline1 treatment slums had higher access to microcredit than control slums.

On completion of Endline1, Spandana was allowed to open branches in control slums. Meanwhile, other MFIs also opened their branches in both treatment and control slums. The next comprehensive follow-up survey (Endline2 henceforth) was conducted two years later during 2009-2010. In 2009, Endline2 data showed that 'MFI lending overall was almost the same in the treatment and the control group' and fraction of households who had borrowed from any MFI in treatment slums (33.7 per cent) was almost equal to that in control slums (33.1 per cent).[17] Since MFI lending started later in control slums, Endline2 data also revealed the fact that households in treatment slums on average had been 'borrowing for longer than those in the control group.' Consequently the average loan cycle of the former was higher than that of the latter and the difference was statistically significant. Endline2 data also suggested that households in treatment slums had larger (by Rs. 2,344) loan size compared to the households in control slums and the difference was statistically significant. Thus, 'the key differences between treatment and control slums at [E]ndline 2' were *'length of access to microfinance'* and the consequent *'larger loans'*. Endline2 survey also possibly captured some of the effects of access to microcredit that emerge only in the long-run. To reiterate, our objective in this paper is to examine the impact of access to microfinance per se on consumption inequality and not necessarily that of microcredit offered by any particular MFI.

3.1 Spandana's microcredit programme: an overview

Spandana offers microcredit in groups which consists of six to ten women. Women in the age group of 18 to 59 with valid identification and residential proof and having own house are considered to be eligible to join a microcredit group. Group members are jointly liable for the repayment of the loans given to the group. These groups are formed by women on their own.

[17] 38.5% households borrowed from an MFI in treatment slums and 33.1% of households did the same in control slums (see Table 3 Panel B of Banerjee et al., 2013).

The size of the first loan is Rs. 10,000 which is equivalent to \$ 1000 at 2007 purchasing power parity (PPP)-adjusted exchange rates (World Bank, 2007 as cited in Banerjee et al., 2013, p.6). The repayment tenure of such loan is 50 weeks and the rate of interest charged by Spandana is 24 per cent per annum. On repaying the loan received in first cycle, the groups are eligible to borrow upto Rs. 20,000 in the next cycle. 'Spandana does not determine loan eligibility by the expected productivity of the investment' (Banerjee et al., 2013: 6) and hence money being fungible, women clients can potentially use part of the loan amount to meet consumption needs. This enables microcredit clients to smooth consumption, and hence examination of consumption inequality becomes pertinent and particularly interesting.

3.2 What else changed between Endline1 and Endline2?

As mentioned previously, Endline1 and Endline2 were conducted during 2007-2008 and 2009-2010 respectively. Thus, the time gap between the revisit of the same household for the two waves of survey was approximately two years. In these two years no significant or catastrophic event took place unevenly across treatment and control slums that could potentially change the lending and borrowing patterns of households in these slums. Thus, the treatment and control slums remain comparable both in Endline1 and Endline2. At this juncture, we would like to mention that the infamous 'AP microfinance crisis' erupted in October 2010 (CGAP, 2010) after the completion of Endline2, and hence we can safely assume no significant impact of the crisis on Endline2 survey results.

We next turn to the discussion on changes in some economic indicators. Banerjee et al. (2013) find that average household consumption in original control slums increased from Rs .7,662 in Endline1 (2008) to Rs 11,497 in Endline2 (2010) even after adjustment for price movements between Endline1 and Endline2. The authors also find similar growth in business activities (from 34 per cent in Endline1 to 42 per cent in Endline2), savings bank account holding (from 82 per cent in Endline1 to 85 per cent in Endline2) and health insurance uptake (from 12 per cent in Endline1 to 76 per cent in Endline2) in control slums.[18]

4. Empirical model

In this paper we analyze consumption inequality at two levels: slum-level and household-level. It must be noted here that in this paper we are interested in examining consumption inequality among a pool of households who are potential participants of microcredit programmes. Households in treatment slums and those in control slums were homogeneous in terms of several observable characteristics. The only difference that remained between them was in terms of their access to microcredit. First, we estimate kernel densities of MPCE (median adjusted) of both treatment and control groups at the slum-level and household-level to examine the difference in the spread of the distribution graphically. However, we cannot comment much on inequality by looking at the kernel densities, and hence slum-level and household-level analyses are formally done as follows.

4.1 Slum-level analysis

We measure consumption inequality at the slum-level in terms of three indicators: coefficient of variation (CV), Gini coefficient and Theil index.[19] These indices are computed based on MPCE. These measures belong to the Generalized Entropy (GE) class proposed by Theil (1967).[20] Also, use of these measures has several advantages. First, these measures are scale independent and hence we do not adjust MPCE for price movements between Endline1 (2007) and Endline2 (2009). Second, these measures meet some of the desirable axioms of a 'good' measure of inequality.[21] Third, these measures are decomposable into factor components. Gini coefficient varies between zero (perfect equality) and unity (perfect inequality) and Theil index can vary between zero (perfect equality) and *logn* (perfect inequality), where 'n' is the size of

[18] This rapid rate of growth of health insurance penetration was perhaps due to the expansion of government funded Rashtriya Swasthya Bima Yojna (RSBY) health insurance program in the study areas.
[19] For computational formulas for these inequality indices, see Appendix-A.
[20] For e.g. GE(α =1) = Theil's measure, where α is the weight given to the distances between MPCE at different parts of the MPCE distribution.
[21] For e.g. population independence and Pigou-Dalton condition as discussed in Fields (2001, pp 17-18).

the population. For all the three measures, higher the value of the index higher is the level of inequality. To measure the impact of access to microcredit on consumption inequality at the slum-level we estimate the following regression equation:

$$y_i = \beta_0 + \beta_1 \times Treatdummy_i + X_i^{'} \phi + \varepsilon_i \qquad (1)$$

where, y_i is the slum-level measure of consumption inequality, *Treatdummy* is an indicator for treatment slums (*treatdummy* =1 for treatment slums and = 0 for control slums). X is a vector of slum-level control variables, calculated at baseline, including MPCE, fraction of household heads who are literate, fraction of households who purchased items under Public Distribution System (PDS) from ration shops in last 30 days, fraction of households who experienced a health shock, and fraction of households with insurance (life, health, and accidents).[22] The coefficient of interest is β_1. We estimate Eq.(1) separately for baseline, Endline1 and Endline2. Finally, we test robustness of our findings by using nonparametric bootstrap technique (Efron and Tibshirani, 1993).[23] Nonparametric bootstrap is used because inferences drawn based on such method are 'asymptotically efficient' (Efron and Tibshirani, 1993: 395). We conduct 1000 bootstrap replications, each of original sample size and compute ratio of mean inequality in control slums to mean inequality in treatment slums. Inequality is measured in terms of the inequality indices previously mentioned, and thus we obtain three ratios: CV-ratio, Gini-ratio and Theil-ratio. Therefore, a typical ratio exceeds (is less than) unity if consumption inequality in control slums is higher (lower) than that in treatment slums. Finally, we compute 90 per cent fully empirical confidence intervals (CI) of the ratios to test statistical significance.

4.2 Household-level analysis

[22] A household suffered a health shock if it had spent Rs 500 or more for treating a sick or injured household member.
[23] For a brief description of bootstrap method see Appendix-B.

We analyze household-level inequality across all households in treatment slums vis-à-vis households in control slums in terms of two measures: Gini coefficient and Theil index.[24] We use nonparametric bootstrap technique[25] to compute 90 percent fully empirical confidence intervals of the ratio of Gini coefficient (Gini-ratio)of treatment households to Gini coefficient of control households using 1000 bootstrap replications, each of original sample size. Similarly, we compute the ratio for the Theil index (Theil-ratio) between treatment and control households. A typical ratio exceeds (is less than) unity if inequality among control households is higher (lower) than those among treatment households. We perform the household-level analysis separately for baseline, Endline1 and Endline2.

After comparing levels of inequality across treatment and control households, it is imperative to decompose inequality in two ways.[26] First, we can ask: what proportion of total MPCE inequality is attributable to various constituent expenditure categories? This is a "levels question" and hence we term it as levels decomposition. Second, we may ask: how much of the difference in inequality between treatment and control is accounted for as a proportion by different constituent expenditure categories? This is a "differences question" and hence we term it as difference decomposition which is helpful in determining relative importance of different expenditure categories in determining inequality differences between treatment and control households. For the purpose of decomposition, we divide MPCE into three components: monthly per-capita expenditure on cereals, per-capita expenditure on non-cereal food items and per-capita non-food expenditure.[27] We carry out the source-decomposition

[24] Gini coefficient and Theil index are "strongly-Lorenz-consistent" (Fields, 2001: p. 31)

[25] Use of bootstrap technique for analyzing inequality is not new in the related literature. For example, Mills and Zandvakili (1997) use bootstrap technique to examine inquality trends in the United States. Athanasopoulos and Vahid (2003) use bootstrap method to analyze changes in income inquality in Australia between 1986 and 1999. For a more technical discussion on use of bootstrap in inequality measurement, see Biewen (2002).

[26] See Fields (2003) for a detailed discussion on levels and difference decomposition.

[27] Cereals consist of rice, maize, wheat, bajra etc. Non-cereal food items are pulses and pulse based products, milk and milk products, edible oil, vegetables, fruits and nuts, egg, fish and meat, other food items such as sugar, salt, spices, tea, coffee, processed foods, etc. excluding food items consumed outside home, meals and snacks consumed outside home, drinking water etc. Non-food items are intoxicants (pan/tobacco etc.), fuels, monthly expenditure on

analysis following the method suggested by Shorrocks (1982). Using Shorrocks (1982) decomposition framework, we argue that "natural decomposition rule"-which presupposes that the index of MPCE inequality is a weighted sum of individual MPCE's- fails to determine factor inequality weights uniquely. Shorrocks (1982) resolves this problem of non-uniqueness of factor inequality weights by imposing a few additional restrictions on the properties of the inequality index in question namely, normalization for equal factor contributions and two-factor symmetry.[28] These two restrictions are sufficient to ensure uniqueness of factor inequality weights regardless of the measure of inequality used. In other words "unique decomposition" rule as discussed in appendix C is independent of the choice of inequality measure.

Finally, we go one step further by examining the same ratios for different percentiles of MPCE estimated at Endline1. We compute 90 percent empirical confidence intervals of Gini-ratio and Theil-ratio for different percentiles of Endline1 MPCE using 1000 bootstrap replications, each of original sample size. Since we are interested in relative inequality across treatment and control households, we compute 90 per cent empirical confidence intervals of the same ratios based on estimated MPCE at Endline2 for the same set of households who were in different percentiles based on MPCE at Endline1. This allows us to track the same set of households between Endline1 and Endline2, and hence we can capture the dynamic nature of consumption inequality between treatment and control households at different percentiles of MPCE (based on Endline1 estimates).

5. Data

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miscellaneous good and services like telephone and electricity, cinema, theatre, school and tuition fees, school books and other educational expenses (annual), newspapers, magazines, medical expenses (non-institutional), medical expenses (annual institutional) toilet items, clothing (annual), footwear (annual), durable goods (annual) regular conveyance costs, house rent and other payments, informal fees.

[28] Shorrocks (1982) imposes four other conditions: continuity, factor symmetricity and population symmetricity, independence of the level of disaggregation, and consistency.

In this paper we primarily use Endline1 and Endline2 data available from Spandana trial conducted by Banerjee et al. (2013) to create a panel dataset. Additionally, we use the baseline data *only* to examine the situation in terms of consumption inequality prior to the intervention both at the slum-level and household-level across treatment and control groups. The baseline survey was administered in all sample slums in 2005. In 100 slums out of 120 slums, 20 households were surveyed per slum; in the remaining 20 slums, 40 households were surveyed per slum, and thus in total 2800 households were surveyed in baseline. In each slum households were randomly selected conditional on having a woman aged between 18 and 55 years.

In 2007, Endline1 was conducted in both treatment and control slums 15 to 18 months after Spandana started opening branches in treatment slums back in 2005. In each slum on average 65 households randomly selected from the sampling frame discussed previously in Section 3 were surveyed using a structured questionnaire and in total 6850 households were enumerated across 104 slums. The same set of households was surveyed again in 2009-2010 during Endline2 using a structured questionnaire very similar to the one administered in Endline1. Availability and getting consent of households for participation in surveys during repeated visits become a challenge in the creation of a panel dataset. Enumeration of households that are available for participation in repeated surveys brings in the issue of 'attrition bias' as known in the parlance of evaluation literature (Duflo et al., 2006). Fortunately in Spandana trial 'the re-contact rate at Endline2 for household initially interviewed at Endline1 was very high, at 89.9 per cent in the treatment group and 90.2 per cent in the control group' (Banerjee et al. 2013: 8). Banerjee et al. (2013) further show that average characteristics along most dimensions, of households who could not be enumerated in Endline2 did not differ statistically significantly from those of enumerated households in both treatment and control slums[29]. However, for

[29] For more information on attrition rates, see Banerjee et al. (2013) pp 8-9.

slum-level analysis, we dropped the slums in which less than 30 households were enumerated across both treatment and control slums. Thus, we have 87 slums in total across both waves of the survey. By combining Endline1 and Endline2 datasets we created a balanced panel dataset of 5,287 households across these 87 slums for our analysis. Out of these 5,287 households, 2,718 and 2,559 households were in treatment and control slums respectively. We also use household-level data for the same 87 slums available from the baseline survey discussed previously.

In both Endline1 and Endline2 surveys, fairly detailed data on household composition, education, household assets, consumption expenditure, savings, borrowing, health and other shocks, and business activities were collected.[30] Since we use consumption expenditure data extensively in this paper, we describe it in detail next as follows. Data on consumption expenditure was collected at the household level by interviewing the household member who was most informed about household expenses. The respondent was asked to report expenses incurred by the household in the last month on food items (such as cereals, pulses, oil, spices, etc.), fuel, and on other miscellaneous goods after excluding business expenses, if any, on such items. Annual data on educational expenditure, medical expenses, expenditure on festival and seasonal clothing, footwear expenses, and gifts were also collected. We compute monthly consumption expenditure by adding all monthly expenses wherever reported and one twelfth of the total expenditure on goods and services purchased annually.

6. Results

Figure 1.1 and Figure 1.2 show the kernel densities of MPCE (median adjusted) for treatment and control slums for Endline1 and Endline2 respectively. By looking at the densities we can conclude that distribution of MPCE across treatment and control slums has changed between Endline1 and Endline2. Graphically, it appears that distribution of MPCE of treatment slums has higher spread in comparison with that of control slums. Figure 2.1 and Figure 2.2

[30] To know more about definitions of variables such as business see appendix of Banerjee et al. (2013).

show the kernel densities of MPCE (median adjusted) for treatment and control households for Endline1 and Endline2 respectively. The kernel densities of treatment and control households by and large overlap in both Endline1 and Endline2. However, we cannot comment much on inequality based on these densities because kernel densities have drawbacks especially when applied to long-tailed distributions.

[INSERT FIG 1.1 and 1.2 HERE]

[INSERT FIG 2.1 and 2.2 HERE]

6.1 Slum-level results

The summary statistics of some of the slum-level variables of interest from the baseline survey are reported in Panel A of Table 1. Average MPCE of treatment slums and controls slums in baseline were Rs.1,012 and Rs.978 respectively. The standard test of difference in means (results not reported in the table) reveals that these differences are not statistically significant. Treatment and controls slums are otherwise balanced in terms of most of the observed characteristics namely, average fraction of household heads who are literate, average family size, average fraction of households borrowing for consumption, average fraction of households availing PDS, average fraction of households having insurance, average fraction of households who experienced a health shock, average land ownership, average size of the livestock, and average number of businesses per household.

[INSERT TABLE 1 HERE]

Estimation results of Eq.(1) are reported in Table 2. The baseline results are reported in Panel A. Panel B and Panel C show results for Endline1 and Endline2 respectively. In Panel B, the point estimate of the coefficient of the treatment dummy is positive and statistically significant at 5 and 10 per cent level of significance for Gini Coefficient and Theil index respectively; implies that in terms of Gini Coefficient and Theil Index, treatment slums on average had higher inequality by 0.02 points and 0.04 points respectively. Thus, enhanced microcredit access seems to cause statistically significant rise in consumption inequality in treatment slums. At this juncture, it is worth looking at baseline results in Panel A which shows that the coefficient of the treatment dummy is statistically insignificant for all the three measures of inequality. Hence, consumption inequality at the slum-level was the same prior to the microcredit intervention by Spandana, thanks to randomization. Next, we turn to Endline2 results. In Panel B of Table 2, we find that none of the coefficients of the treatment dummy is statistically significant. Thus, we do not find any statistically significant difference in consumption inequality measured in terms CV, Gini Coefficient, and Theil Index across treatment and control slums. Hence, our results suggest that once all slums had equal access to microcredit, consumption inequality became equal across treatment and control slums. Put differently, larger loan size and higher loan cycles enjoyed by households who had been borrowing from MFIs in treatment slums in Endline2 did not matter for consumption inequality.

[INSERT TABLE 2 HERE]

We test robustness of our findings using bootstrap technique as elaborated in Section 4.1 and the results are presented in Table 3. The baseline results are reported in Panel A. Panel B and Panel C show results for Endline1 and Endline2 respectively. Panel A shows that during baseline, mean values of all the three ratios were close to unity and the 90 per cent fully empirical confidence intervals include unity for all the three ratios. Thus, we do not find any evidence of divergence in terms of consumption inequality across treatment and control slums during baseline survey. In Panel B, the bootstrap means of CV-ratio, Gini-ratio and Theil-ratio are all less than unity. More importantly, 90 per cent fully empirical confidence intervals of CV-ratio, Gini-ratio and Theil-ratio do not include unity and the upper limit of all the three ratios are less than unity; implies that treatment slums had higher consumption inequality than control slums in Endline1. Coming to Endline2 results in Panel C, we find that all the 90 per cent fully empirical confidence intervals include unity and the bootstrap estimates of means of all the three ratios are close to unity. Hence, we do not find any statistically significant difference in consumption inequality across treatment and control slums in Endline2.

[INSERT TABLE 3 HERE]

6.2 Household-level results

Average characteristics of treatment households and control households across all the slums during baseline are presented in Panel B of Table 1. In baseline, average household-level MPCE was Rs. 1,044 and Rs. 985 for treatment and control households respectively but the difference was not statistically significant (mean difference results are not reported in the table). Treatment and control households on average are approximately equal in terms of other observed characteristics namely, literacy status of the household head, family size, whether borrowing for consumption, availing PDS, having insurance, whether experienced any health shock, land ownership, size of the livestock, and number of businesses per household.

Before using Gini coefficient and Theil index for inequality comparison between treatment and control groups, we examine the Lorenz curve of MPCE of these two groups. From Figure 3.1 it is evident that the Lorenz curve of the control group lies somewhere above and never goes below the Lorenz curve of the treatment group in Endline1. Thus, in Endline1 the MPCE distribution of the control group "Lorenz-dominate" the MPCE distribution of the treatment group (Fields, 2001: p.20). In Figure 3.2, we find evidence of "Lorenz-coincidence" in Endline2 as the Lorenz curve of control group and that of treatment group more or less overlap. Results from our subsequent analysis of inequality using Gini coefficient and Theil index accord with these graphical findings. The bootstrap estimation results at the household-level for Gini-ratio and Theil-ratio are presented in Table 4. Panel A shows estimation results for the baseline. Panel B and Panel C show estimation results for Endline1 and Endline2 respectively. In Panel A, the bootstrap mean of Gini-ratio and Theil-ratio are respectively 0.92 and 0.78. The 90 per cent fully empirical confidence intervals of both Gini-ratio and Theil-ratio include unity; implies no statistically significant divergence between treatment and control households in terms of consumption inequality in baseline. In Panel B, the bootstrap mean of Gini-ratio and Theil-ratio are respectively 0.93 and 0.81. The 90 per cent fully empirical confidence intervals of both Gini-ratio and Theil-ratio exclude unity and the upper limits of the ratios are less than unity but very close to unity. Thus, we find some weak evidence of increased consumption inequality at the household-level across all treatment slums. In other words, increased access to microcredit seems to be associated with higher consumption inequality at the household-level. We now turn to Endline2 estimation results in Panel C of Table 4. Although bootstrap estimates of Gini-ratio (1.04) and Theil-ratio (1.08) exceed unity, the 90 per cent empirical confidence intervals of both the ratios include unity. Thus, we do not find any evidence of statistically significant difference in consumption inequality across treatment and control households in Endline2 during which all households had equal access to microcredit. In other words, at the household-level also, we find that larger loan size and higher loan cycles enjoyed by households who have been borrowing from MFIs in treatment slums for longer time do not matter for consumption inequality.

[INSERT TABLE 4 HERE]

6.3 Inequality Decomposition by Factor Components

We report results of levels decomposition of inequality in Table 5. Consumption inequality among households across treatment and control slums is predominantly driven by non-food expenditure and more so in case of treatment group. Share of non-food expenditure in total consumption inequality is as high as 87 percent and 81 percent for treatment and control households respectively in Endline1.

[INSERT TABLE 5 HERE]

Turning to Endline2, we find that non-food expenditure continues to be the predominant contributor to consumption inequality. The factor weight of non-food expenditure dwindles to 76 percent from 87 percent for treatment households and the same dwindles to 79 percent from 81 percent for control households. After non-food expenditure, non-cereal food expenditure is the second important contributor to consumption inequality. Its factor share is 10.5 percent and 14 percent for treatment and control households respectively in Endline1. Factor share of non-food expenditure remains the second highest even in Endline2. Non-food expenditure's shares in total consumption inequality are 19 percent and 16 percent for treatment and control households respectively in Endline2. Coming to share of expenditure on cereals, we find that its factor weight never exceeds 5 percent for treatment and control households in both Endline1 and Endline2.

We report results of difference decomposition analysis in Table 6 for Endline1 only. Since we do not find any statistically significant difference in consumption inequality across treatment and control households, difference decomposition analysis for Endline2 is not warranted. Factor shares in difference decomposition analysis are dependent on the measure of inequality used (see appendix C). Hence, we carry out difference decomposition analysis for both difference in consumption inequality measured in terms of Gini Coefficient and Theil Index across treatment and control households and report the results separately in Table 6. Interestingly in Endline1, we find that factor shares of expenditure on cereals and non-cereal food items are both negative. Thus, expenditures on cereals and non-cereal food items play an equalizing role in mitigating consumption inequality differences across treatment and control households. However, expenditure on non-food items predominantly drives the observed consumption inequality differences between treatment and control households. Factor shares of non-food expenditure in difference decomposition in terms of Gini Coefficient and Theil index are 170 percent and 112 percent respectively. Thus, the inequality augmenting effect of non-food expenditure far outweighs the equalizing effect emanating from expenditure on cereal and non-cereal food items.

[INSERT TABLE 6 HERE]

6.4 Percentile Analysis

In this paper we also examine disparity in consumption inequality across treatment and control households at different percentiles of MPCE. 90 per cent empirical confidence intervals of Gini-ratio and Theil-ratio computed based on bootstrap replications at various percentiles of MPCE at Endline1 are reported in Panel A and Panel B of Table 7 respectively. In Panel A, we find that 90 per cent empirical confidence intervals of Gini-ratio for Endline1 (column 1 and 2) do not include unity (column 3) for 9 percentiles (namely, 20^{th}, 35^{th}, 40^{th}, 45^{th}, 65^{th}, 70^{th}, 75^{th}, 80^{th}, and 90^{th}) out of the 20 percentiles at which such confidence intervals are estimated. Moreover, out of these 9 percentiles, the upper limit of the confidence interval was less than unity for 6 percentiles (namely, 20^{th}, 35^{th}, 45^{th}, 70^{th}, 75^{th} and 90^{th}) which implies that consumption inequality was more pronounced among treatment households compared to control households at these percentiles in Endline1. Similar results are found in case of Theil-ratio as well. Figure 4.1 and 5.1 show Gini-ratio and Theil-ratio and their 90 per cent empirical confidence intervals for Endline1 graphically.

[INSERT TABLE 7 HERE]

Next we turn to Endline2 results for Gini-ratio in Panel A of Table 7. We find that 90 per cent empirical confidence intervals of Gini-ratio for Endline2 (column 4 and 5) include unity (column 6) for 19 percentiles (except 75^{th}) out of the 20 percentiles at which such confidence intervals are estimated. Qualitatively similar results are obtained in case of Theil-ratio in Endline2 as well (Col 6 of Panel B). Thus, our results suggest no statistically significant divergence in consumption inequality across treatment and control slums in Endline2 at various percentiles of Endline1 MPCE. Figure 4.2 and 5.2 show Gini-ratio and Theil-ratio and their 90 per cent empirical confidence intervals for Endline2 graphically. Two broad conclusions can be drawn from this percentile based analysis. First, enhanced access to credit impact consumption inequality differently across various percentiles of MPCE and for most percentiles it impacts consumption inequality adversely in case of treatment households. Second, once all households across treatment and control slums have equal access to microcredit, bigger loan size and higher loan cycles have little impact on consumption inequality at most of the percentiles of MPCE.

[INSERT FIG 4.1 and 4.2 HERE]

[INSERT FIG 5.1 and 5.2 HERE]

7. Conclusion

Evaluation studies of microcredit programmes done so far typically evaluate impact of microcredit on consumption and other indicators of well being such as, health, education and women empowerment, etc. Since microfinance is a credit market intervention which directly removes credit constraint faced by poor households in an imperfect credit market, it is imperative to examine its impact on consumption inequality among a group of potential microfinance participants as well. However, there is dearth of evidence on impact of access to microcredit on consumption inequality in the existing literature. This study fills this gap in the existing literature.

In this paper we rigorously analyse impact of access to credit on consumption inequality using a large panel dataset of more than 5000 households available from Spandana trial conducted by Banerjee et al. (2013). We examine consumption inequality at the slum-level and household-level using various broadly accepted measures of inequality: CV, Gini Coefficient and Theil Index. There are two main findings of this study. First, we find evidence of enhanced access to credit exacerbating inequality both at the household-level and slum-level. This finding is similar to what Copestake (2002) finds in terms of income polarization in Zambia. Further decomposition of inequality indices shows that the difference in consumption inequality is predominantly driven by expenditure on non-food items. Second, once all households have equal access to microcredit in terms of probability of getting a microloan, microcredit has no statistically significant impact on consumption inequality. Even larger loan size and higher loan cycles do not matter for consumption inequality both at household-level and slum-level. The second finding can be perceived as the impact of microcredit on consumption inequality in the long-run and hence it is in sharp contrast with the theoretical postulate of Ahlin and Jiang (2008). However, it must be noted that we examine inequality in terms of MPCE but Ahlin and Jiang (2008) postulate is about income inequality in the long-run.

We go one step further to examine impact of access to microcredit on consumption inequality at various percentiles of MPCE. Our results suggest that improved access to microcredit does impact consumption inequality differently across treatment and control households at different percentiles of MPCE. However, we find no statistically significant impact of larger loan size and higher loan cycles completed by older microfinance borrowers on consumption inequality at different percentiles of MPCE.

The findings of this paper are based on the panel data available from Spandana trial conducted by Banerjee et al. (2013) in Hyderabad slums of Andhra Pradesh in India. It is imperative to test external validity of our results using data from some other similar randomized controlled trial conducted elsewhere in the world. This could be a potential topic of future research.

An immediate policy implication of our results is this: financial inclusion through microcredit may not be enough for mitigating consumption inequality. To achieve this, we need targeted livelihood support programmes (e.g. National Rural Livelihood Mission (NRLM) launched in 2005 by Government of India, non-government initiatives such as Targeting Hard-Core Poor programme of Bandhan in West Bangal as narrated in Banerjee et al. (2010), etc.) for those who are unable to participate in microcredit programmes. Moreover, United Progressive Alliance (UPA) government at the centre in India tried its best to enact Food Security Bill in India. Hence, it will be interesting to examine impact of access to microcredit on consumption inequality in a similar randomized controlled setting but after the enactment of Food Security Act in India. Microfinance as a potential poverty alleviation tool deserves special attention and its general equilibrium impacts on income or consumption inequality should be carefully examined for effective policy making.

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Appendix A

Coefficient of variation (CV) $= \frac{SD}{Mean} \times 100$

Gini Coefficient = $\frac{1}{2n^2\overline{y}} \sum_{i=1}^{n} \sum_{j=1}^{n} \left| y_i - y_j \right|$, where $\overline{y}$ is the mean

Theil index $=\frac{1}{n}\sum_{i=1}^{n}\frac{y_i}{\bar{y}}\ln\left(\frac{y_i}{\bar{y}}\right)$, where $\bar{y}$ is the mean

Appendix B: A short note on bootstrap method[31]

A single inequality index say, Gini coefficient or Theil Index tells us the extent of inequality at a given point of time. The ratio between the inequality indices in question computed for two different sample groups indicates whether extent of inequality differs across the two groups. However, a single point estimate of the ratio does not tell us whether the observed difference in inequality across groups, if any, is statistically significant or not. Therefore, it is imperative to test the statistical significance asymptotically and thus bootstrap method comes handy. The advantage of using bootstrap method is that it does not impose any functional form for the underlying distribution of MPCE and therefore the method is applicable whenever samples are drawn independently from the population.

Suppose, a random sample of size *n* is drawn from a completely unspecified probability distribution *F*. Let $\hat{I}$ denotes the point estimate of the inequality measure, *I*. We generate the empirical distribution $\hat{F}$ of the inequality measure by drawing random samples of size *n* with replacement from the original sample such that each sample observation, y_i for *i = 1(1)n*, has *1/n* probability of being included in the drawn sample. We repeat this procedure *B* times and thus we get *B* bootstrap samples of size *n*.

$$\hat{F} \rightarrow \{y_1^{*b}, y_2^{*b}, \ldots\ldots, y_n^{*b}\} \ for\ b = 1, 2, \ldots.., B$$

We calculate the inequality measure of interest (*I*), I^{*b} for each sample. Thus, we generate a series of values for the inequality measure,

$$\{I^{*1}, I^{*2}, \ldots\ldots, I^{*B}\}$$

The estimated standard error of I^{*b} calculated on the basis of *B* sample observations mentioned above is a consistent estimator of $\hat{I}$, i.e.,

$$s\hat{e}_F(\hat{I}) = s\hat{e}_{\hat{F}}(I^{*b}) = [\sum_{b=1}^{B}(I^{*b} - \overline{I^*})^2/(B-1)]^{1/2}$$

The α^{th} lower and upper percentiles of the bootstrap distribution of I^{*b} are denoted by I_l^{*b} and I_h^{*b} respectively. Then, the lower and upper percentile values can be used as the boundary values of *100-2α* percent empirical confidence interval for *I*.[32]

Suppose, we have two samples of size n_1 and n_2 drawn from two unspecified probability distributions F_1 and F_2 respectively. Let, $\hat{R}(=\hat{I}_1/\hat{I}_2)$ denotes the ratio between the estimated inequality measures for the two samples[33]. Using bootstrap procedure as described above, we generate a series of values of *R* as,

$$\{R^{*b} = I_1^{*b}/I_2^{*b}\} \ for\ b = 1, 2, \ldots.., B$$

[31] This description is heavily based on the description of the bootstrap method in Athanasopoulos and Vahid (2003)
[32] α = 0.05, 0.025, 0.005, for conventional levels of significance namely, 10%, 5% and 1% respectively.
[33] For the purpose of our analysis, sample 1 and 2 can be considered as control and treatment samples respectively.

Thus, the *100-2α* percent empirical confidence interval for R is given by

$$\Pr(2\hat{R} - R_h^{*b} \le R \le 2\hat{R} - R_l^{*b}) = \frac{100 - 2\alpha}{100}$$

where, R_l^{*b} and R_h^{*b} denote α^{th} lower and upper percentiles of the bootstrap distribution of R^{*b} respectively. If this empirical confidence interval does not include unity then we conclude that the estimated ratio between the indices of inequality across two groups is statistically significant.

The above description holds good when sample units have no weight attached to it. However, in our case Spandana borrowers were oversampled and hence different sample units had different weights, $\{y_i^{*b}, w_i^{*b}\}$ *for b =1, 2, ...,B and i =1(1)n*. We adjusted our inequality measure computation and subsequent confidence interval computation following the method suggested by Biewen (2002).

Appendix C: Decomposition of Inequality into Factor Components

A. Levels decomposition

Suppose there are n households and $\mathbf{y}$ represents the vector of MPCE of the n households.
Thus, $\mathbf{y} = \{y_1\ y_2\ y_3\ .....y_n\}$ where y_i denote MPCE of the ith household.

Let there be K expenditure categories which we call factor components such that MPCE of the ith household (y_i) can be decomposed into K components: $y_{i1}, y_{i2}, \ldots\ldots, y_{iK}$. In our case we have three MPCE factor components namely, monthly per-capita expenditure on cereals, per-capita expenditure on non-cereal food items and per-capita non-food expenditure and hence K = 3. Thus we get,

$$y_i = \sum_{k=1}^{k=K} y_{ik} \qquad (1)$$

We define the following:
s_k = relative factor inequality weight which shows proportion of total consumption inequality that is contributed by the k^{th} expenditure category.

Therefore, $\sum_{k=1}^{k=K} s_k = 1$

Let $I(\mathbf{y})$ denotes the measure of inequality such that

$$\begin{aligned} I(y) &= \sum_{i=1}^{i=n} a_i(y) y_i \\ &= \sum_{i=1}^{i=n} a_i(y) \sum_{k=1}^{k=K} y_{ik} \quad \text{[replacing } y_i \text{ from 1]} \\ &= \sum_k \sum_i a_i(y) y_{ik} \\ &= \sum_k S_k \end{aligned}$$

where, $S_k = \sum_i a_i(y) y_{ik}$

Therefore by natural decomposition rule,

$$s_k = \frac{S_k}{I(y)} \text{ and it is obvious that } \sum_{k=1}^{k=K} s_k = 1$$

However, these factor components are not unique due to non-uniqueness of the factor weights i.e., a_i's. In fact, Shorrocks (1982: p. 202) shows that "the contribution of any factor expressed as a proportion of total inequality can be made to give *any* value between plus and minus infinity!" To circumvent this problem of non-uniqueness or "multiplicity" of potential decomposition rules, Shorrocks (1982) imposes the following conditions to the properties of $I(\mathbf{y})$:[34]

Normalisation for equal factor distributions: This condition implies that if all households spend the same amount on the k'th factor, then the share of inequality accounted for by that factor should be zero.

Two factor symmetry: If there are two factor components such that *K=2* and distribution of one of the factors is simply a permutation of the other factor, then both factors should get the same factor weight in the decomposition.
By imposing the above mentioned conditions on the properties of $I(\mathbf{y})$, Shorrocks (1982) shows that the unique decomposition of inequality index $I(\mathbf{y})$ is given by[35]

$$s_k^* = \frac{\operatorname{cov}(y_k, y)}{\sigma^2(y)} = \frac{\rho_k \sigma(y_k)}{\sigma(y)} \quad \textit{for all } k = 1,2,\ldots\ldots K \textit{ and for all } y \neq \mu e$$

where,
$\boldsymbol{y_k}$ denote the distribution ($y_{1k}, y_{2k}, \ldots\ldots\ldots, y_{nk}$) of expenditure on k^{th} item across n households.
μ is the mean of y and e=(1 1 1 1….1)

$\sigma^2(y)$ and ρ_k represent variance of y and correlation coefficient between $\mathbf{y_k}$ and $\mathbf{y}$ respectively.

Evidently, s_k^* is "independent of the choice of inequality measure" and $\sum_{k=1}^{k=K} s_k^* = 1$

B. Difference decomposition

We are comparing difference in inequality between two groups: treatment (T) and control (C). $I(\cdot)_j$ denote the measure of inequality for group-j where j = T, C and we denote difference in inequality between treatment and control by ΔI such that,

$$\Delta I = \sum_{k=1}^{k=K} [s_{k,T} * I(\cdot)_T - s_{k,C} * I(\cdot)_C]$$

Let us denote the contribution of factor-k to ΔI *by* $\Pi_k[I(\cdot)]$ such that,

$$\Pi_k[I(\cdot)] = \frac{s_{k,T}^* * I(\cdot)_T - s_{k,C}^* * I(\cdot)_C}{\Delta I} \quad \textit{for all } k = 1,2,\ldots..K\ldots\ldots\ldots..(2)$$

and $\sum_k \Pi_k = 1$

It is evident from (2) above that $\Pi_k[I(\cdot)]$ depends on the choice of inequality measure, $I(\cdot)$.

[34] Shorrocks (1982) calls these conditions assumptions.
[35] Shorrocks (1982) imposes four other conditions: continuity, factor symmetricity and population symmetricity, independence of the level of disaggregation, and consistency.

Figures and Tables

Figure 1.1: Kernel Density Plots (Endline1)

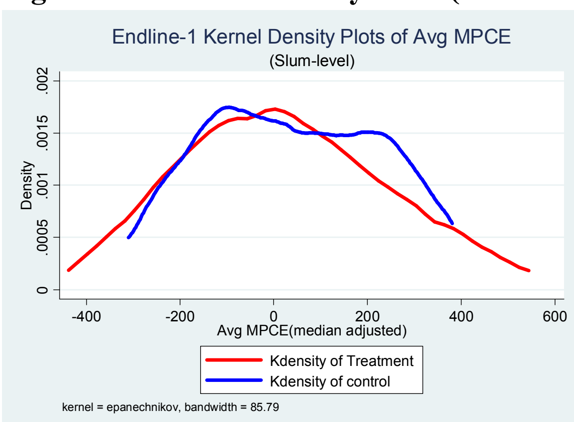
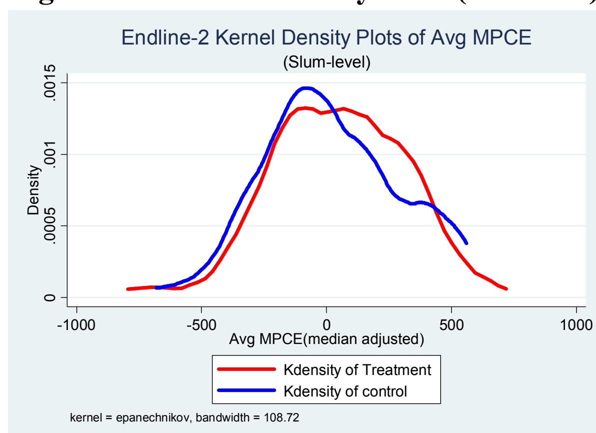


Figure 1.2: Kernel Density Plots (Endline2)



Source: author's calculation

Figure 2.1: Kernel Density Plots (Endline1)

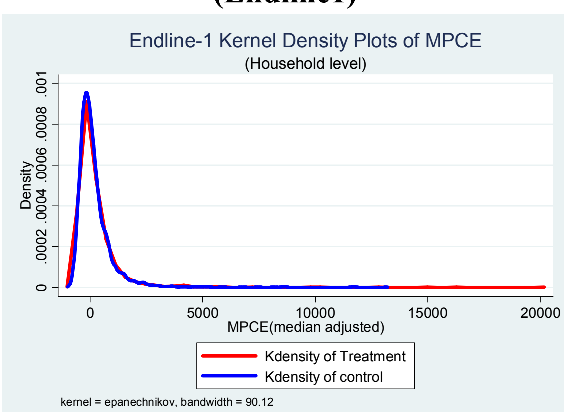
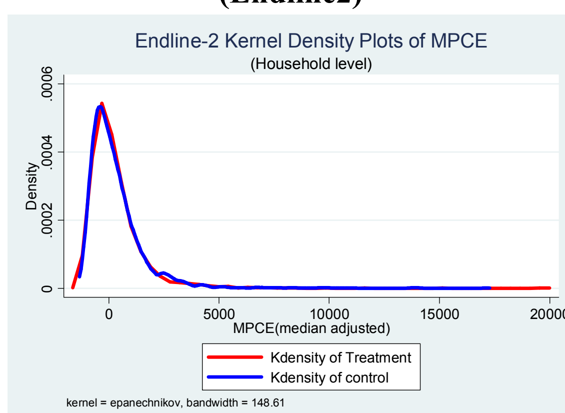
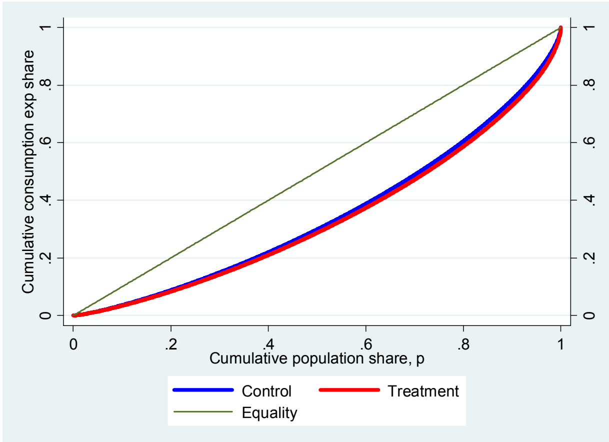


Figure 2.2: Kernel Density Plots (Endline2)



Source: author's calculation

Figure 3.1: Lorenz curve of Endline1 (Household-level)	Figure 3.2: Lorenz curve of Endline2 (Household-level)



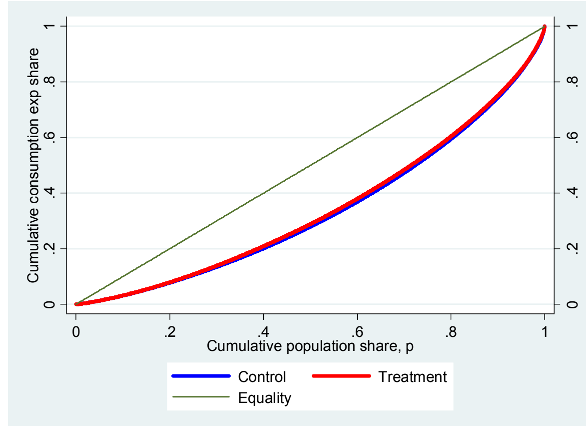


Fig 4.1: Endline1 graph (90% CI)

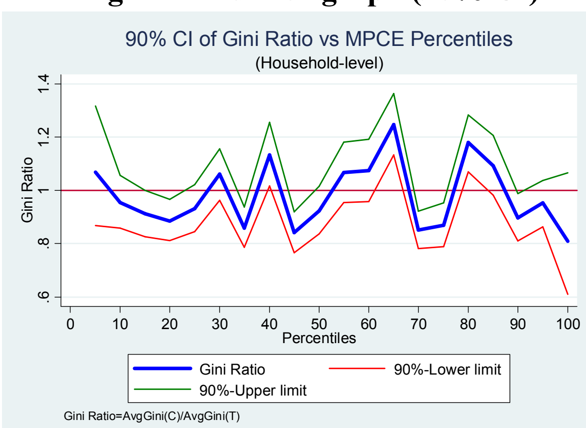
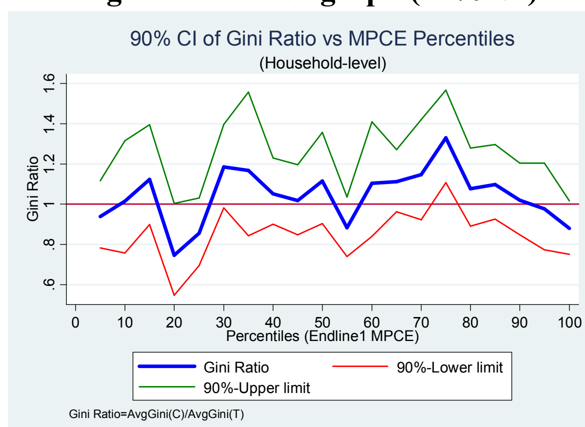


Fig 4.2: Endline2 graph (90% CI)



Source: author's calculation

Fig 5.1: Endline1 graph (90% CI)

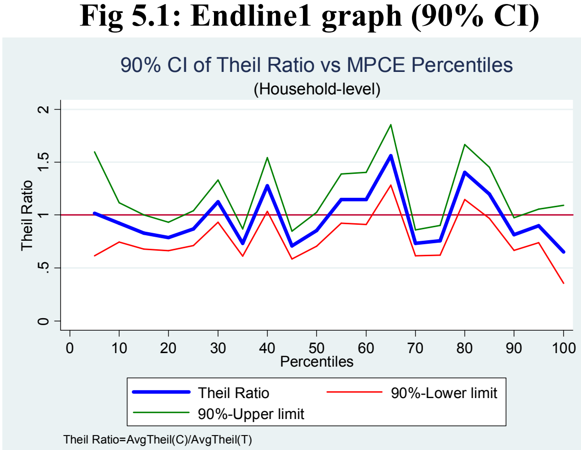
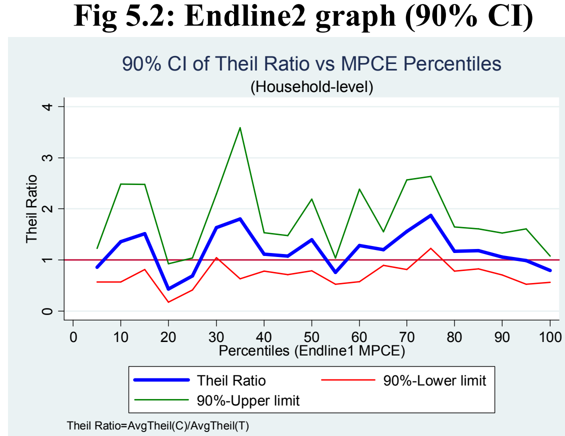


Fig 5.2: Endline2 graph (90% CI)



Source: author's calculation

Table 1: Summary statistics of Some Variables of Interest

Panel A: Slum-level

	Baseline		
	Control	Treatment	Total
Household head literate (%)	60.7	62.1	61.41
	(15.61)	(12.25)	(13.94)
Average family size	5.08	5.16	5.12
	(0.47)	(0.46)	(0.46)
Avg MPCE	977.71	1012.46	995.28
	(233.69)	(250.26)	(241.43)
Household borrowing for consumption (%)	10.64	12.9	11.78
	(9.65)	(13.31)	(11.63)
Household purchased items from PDS shops (%)	60.58	58.01	59.28
	(19.61)	(19.69)	(19.58)
Household having insurance (%)	23.55	22.78	23.16
	(13.12)	(11.43)	(12.23)
Household spent more than Rs 500 due to health shock (%)	35.23	41.87	38.6
	(21.84)	(14.31)	(18.61)
Average land ownership (in acres)	0.46	0.7	0.5
	(0.49)	(0.76)	(0.64)
Average size of livestock(birds+large animals)	0.31	0.35	0.33
	(0.67)	(0.89)	(0.78)
Average number of business	1.24	1.15	1.19
	(0.38)	(0.23)	(0.32)
N	43	44	87

Panel B: Household-level

	Baseline		
	Control	Treatment	Total
Household head literate (Yes/No)	0.63	0.64	0.63
Family size	5.04	5.1	5.08
	(1.67)	(1.78)	(1.73)
MPCE	985.29	1044.27	1014
	(831.3)	(1074.3)	(960.73)
Household borrowing for consumption (Yes/No)	0.14	0.17	0.16
Household purchased items from PDS shops (Yes/No)	0.57	0.58	0.57
Household having insurance (Yes/No)	0.24	0.23	0.24
Household spent more than Rs 500 due to health shock (Yes/No)	0.37	0.42	0.40
Land ownership (in acres)	2.22	2.72	2.50
	(3.79)	(6.90)	(5.67)
Size of livestock(birds+large animals)	0.18	0.20	0.19
	(1.19)	(1.20)	(1.19)
Number of business	1.17	1.13	1.15
	(0.45)	(0.40)	(0.42)
N	1220	1220	2440

Note: In Panel A Standard deviations are in parentheses; In Panel B Standard deviations are in parentheses for non-binary variables

Table 2: Slum-level Inequality

	Dependent variable		
	CV	Gini	Theil
	(1)	(2)	(3)
Panel A: Baseline			
Treatment	0.92	-0.0004	-0.002
	(5.70)	(0.014)	(0.024)
Mean in control	64.76	0.28	0.17
	(35.56)	(0.096)	(0.15)
N	87	87	87
R^2	0.56	0.62	0.59
Panel B: Endline1			
Treatment	8.47	0.02**	0.04*
	(5.79)	(0.01)	(0.02)
Mean in control	63.17	0.28	0.15
	(22.12)	(0.043)	(0.068)
N	87	87	87
R^2	0.21	0.32	0.23
Panel C: Endline2			
Treatment	-0.058	-0.004	-0.0002
	(3.28)	(0.006)	(0.011)
Mean in control	63.33	0.30	0.16
	(14.89)	(0.034)	(0.049)
N	87	87	87
R^2	0.25	0.37	0.31

Notes:
1. The table presents the coefficient of the "treatment" dummy in a regression of each dependent variable on the treatment dummy (with control variables mentioned in the text).
2. Robust standard errors are in parentheses
3. Results are weighted to account for oversampling of Spandana borrowers
4. * significant at 10% level, ** at the 5% level, *** at the 1% level

Table 3: Bootstrap results (slum-level)

Panel A: Baseline

	Mean	90% CI Lower limit	90% CI Upper limit
CV-ratio	0.94	0.76	1.14
	(0.11)		
Gini-ratio	0.97	0.85	1.09
	(0.07)		
Theil-ratio	0.93	0.66	1.26
	(0.18)		

Panel B: Endline1

	Mean	90% CI Lower limit	90% CI Upper limit
CV-ratio	0.85	0.72	0.98
	(0.075)		
Gini-ratio	0.91	0.85	0.97
	(0.035)		
Theil-ratio	0.77	0.62	0.93
	(0.092)		

Panel C: Endline2

	Mean	90% CI Lower limit	90% CI Upper limit
CV-ratio	0.99	0.90	1.09
	(0.058)		
Gini-ratio	1.00	0.96	1.05
	(0.027)		
Theil-ratio	0.98	0.87	1.12
	(0.070)		

Notes:
1.Bootstrap standard errors are in parentheses
2. Number of replications = 1000
3. Each ratio = average of control/average of treatment

Table 4: Household-level Inequality

		Panel A: Baseline	
		90% CI	
	Mean	Lower limit	Upper limit
Gini-ratio	0.92	0.82	1.02
	(0.059)		
Theil-ratio	0.78	0.58	1.02
	(0.136)		
		Panel B: Endline1	
		90% CI	
	Mean	Lower limit	Upper limit
Gini-ratio	0.93	0.87	0.99
	(0.037)		
Theil-ratio	0.81	0.65	0.99
	(0.105)		
		Panel C: Endline2	
		90% CI	
	Mean	Lower limit	Upper limit
Gini-ratio	1.04	0.99	1.09
	(0.029)		
Theil-ratio	1.08	0.95	1.23
	(0.085)		

Notes:
1.Bootstrap standard errors are in parentheses
2. Number of replications = 1000
3. Each ratio = average of control/average of treatment

Table 5: Inequality Decomposition of Levels

Expenditure Categories	Factor shares (in %)			
	Endline1		**Endline2**	
	Treatment	Control	Treatment	Control
Cereals	1.85	4.34	4.86	4.87
Non-cereal food	10.56	14.09	19.4	16.11
Non-food	87.59	81.57	75.74	79.02
Total	100	100	100	100

Notes:

Table 6: Inequality Decomposition of Differences

Expenditure Categories	Factor shares (in %)	
	Endline1	
	ΔGini	ΔTheil
Cereals	-32.33	-8.41
Non-cereal food	-37.90	-3.98
Non-food	170.23	112.39
Total	100	100

Table 7: Inequality at different percentiles of MPCE

Panel A: Gini-ratio

	Endline1: 90% CI			Endline2: 90% CI		
	(1)	(2)	(3)	(4)	(5)	(6)
MPCE Percentile (Endline-1)	Lower limit	Upper limit	CI Includes unity	Lower limit	Upper limit	CI Includes unity
5	0.87	1.32	Yes	0.78	1.12	Yes
10	0.86	1.06	Yes	0.76	1.32	Yes
15	0.83	1.00	Yes	0.90	1.40	Yes
20	0.81	0.97	No	0.55	1.00	Yes
25	0.84	1.02	Yes	0.70	1.03	Yes
30	0.96	1.16	Yes	0.98	1.40	Yes
35	0.79	0.94	No	0.84	1.56	Yes
40	1.02	1.26	No	0.90	1.23	Yes
45	0.77	0.92	No	0.85	1.20	Yes
50	0.84	1.02	Yes	0.90	1.36	Yes
55	0.95	1.18	Yes	0.74	1.04	Yes
60	0.96	1.19	Yes	0.84	1.41	Yes
65	1.13	1.36	No	0.96	1.27	Yes
70	0.78	0.92	No	0.92	1.42	Yes
75	0.79	0.95	No	1.11	1.57	No
80	1.07	1.28	No	0.89	1.28	Yes
85	0.98	1.21	Yes	0.93	1.30	Yes
90	0.81	0.99	No	0.85	1.20	Yes
95	0.86	1.04	Yes	0.77	1.20	Yes
100	0.61	1.07	Yes	0.75	1.02	Yes

Panel B: Theil-ratio

	Endline1: 90% CI			Endline2: 90% CI		
MPCE Percentile (Endline-1)	Lower limit	Upper limit	CI Includes unity	Lower limit	Upper limit	CI Includes unity
5	0.62	1.60	Yes	0.57	1.23	Yes
10	0.75	1.12	Yes	0.57	2.49	Yes
15	0.68	1.00	Yes	0.82	2.48	Yes
20	0.66	0.94	No	0.18	0.93	No
25	0.71	1.04	Yes	0.41	1.04	Yes
30	0.93	1.33	Yes	1.04	2.29	No
35	0.61	0.87	No	0.63	3.58	Yes
40	1.03	1.54	No	0.78	1.53	Yes
45	0.59	0.85	No	0.71	1.48	Yes
50	0.70	1.03	Yes	0.79	2.19	Yes
55	0.92	1.39	Yes	0.52	1.04	Yes
60	0.91	1.40	Yes	0.57	2.39	Yes
65	1.28	1.86	No	0.89	1.55	Yes
70	0.62	0.86	No	0.82	2.56	Yes
75	0.62	0.91	No	1.22	2.63	No
80	1.15	1.67	No	0.78	1.65	Yes
85	0.97	1.45	Yes	0.83	1.61	Yes
90	0.66	0.97	No	0.71	1.53	Yes
95	0.74	1.06	Yes	0.53	1.61	Yes
100	0.35	1.09	Yes	0.56	1.08	Yes

Notes: Total number of treatment households = 2718; total number of control households = 2559; CI: Confidence interval ;
Each ratio = average of control/average of treatment