

# Evaluating Casual Impact of Microfinance: Challenges and Results

Dmitry Shapiro<sup>(1)</sup>

In this paper, I survey the literature that studied the impacts of microfinance. The survey consists of four parts. First, I describe the methodological challenges associated with the impact evaluation of microfinance and how the randomized control trial (RCT) approach can address those *challenges*. I then present the results of six highly influential RCT studies published in the Special Issue of the *American Economic Journal: Applied Economics* in 2015. All six studies reached a similar conclusion that the overall impact of microfinance was modest and not transformative. Second, I describe the reasons suggested in the literature explaining why the impact is limited. Third, I survey two recent RCT studies that found a positive impact of microfinance and then discuss why these two studies differ from those in the Special Issue. Finally, I survey evidence of the negative impacts of microfinance.

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**JEL Classification Codes:** O12, O16

## 1. Introduction

Microfinance, more accurately known as “microcredit,” refers to the provision of loans to the poor and the poorest, where the latter are defined as households earning under US\$1 day per person (Daley-Harris, 2009).<sup>(2)</sup> At the end of the last century,

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(1) Department of Economics and Institute of Economic Research, College of Social Sciences, Seoul National University, 1 Gwanak-ro, Gwanak-gu, Seoul 08826, South Korea. Phone: +82-2-880-2287, Email: dmitry.shapiro@snu.ac.kr. Financial support from the Creative-Pioneering Researchers Program through Seoul National University is gratefully acknowledged.

(2) “Microfinance” is the most commonly used term today, which is what I use in this paper as

microfinance institutions (MFIs) turned providing credit to the poor from a near-impossible task into a well-established booming industry throughout the developing world. As of December 2010, a total of 3,652 MFIs reached over 200 million people, most of whom were among the poorest when they took their first loan (Maes and Reed, 2012). The success is particularly remarkable given the plethora of obstacles that, for a long time, have discouraged formal credit institutions from financing the poor. Adverse selection, moral hazard, a lack of collateralizable assets, the absence of enforcement mechanisms and high costs have made microfinance almost non-existent, or at least subsidized.<sup>(3)</sup> The impossibility of providing loans to the poor, combined with the apparent eventual success of MFIs, has been acknowledged by the Norwegian Nobel Committee. In its announcement of the 2006 Nobel Peace Prize award to Muhammad Yunus—the man who successfully pioneered the concepts of microfinance and microcredit—it specifically stated that, until recently, “...*loans to poor people without any financial security had appeared to be an impossible idea*” (Nobel Foundation, 2006).

The success of microfinance has been widely attributed to several innovative features that have led to a large reduction in interest rates. The first most important innovation is group lending, a distinctive feature of early microlenders, such as Grameen Bank, BRAC or BancoSol. Group lending offers several benefits to lenders and helps reduce the likelihood of defaults. It provides insurance against idiosyncratic negative income shocks faced by individual group members. It also reduces the problem of moral hazard, as it is easier for group members to monitor other borrowers' actions than it is for lenders. Another important feature of microfinance contracts is the provision of dynamic incentives combined with progressive lending;

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well. However, it should be noted that whenever I refer to “microfinance,” I specifically refer to “microcredit” or “microlending.” Microfinance, accurately speaking, is a broader term that includes other financial services, such as microsavings, microinsurance, and so on.

- (3) During pre-microfinance times, in Bangladesh, loans targeted for the poor households by traditional banks had repayments rates of as low as 51.6% in 1980, further decreasing to 18.8% by 1988-89, and were heavily subsidized by the government (Khalily and Meyer, 1993).

for example, repayment in the current period is a pre-requisite for any future loan disbursement. By a standard repeated-game argument, borrowers have incentives to repay current loans, as it provides them access to future, usually larger, loans. Finally, there are other notable features of microfinance contracts, such as conducting regular group meetings with the loan officer, or creating a repayment culture by making repayments frequent and small. All of these innovations had a large impact on reducing lending costs, which then translated into an even larger effect on reducing interest rates (Banerjee, 2013).

Once it became clear that providing credit to the poor can be sustainable—thanks to Grameen Bank and other early MFIs—microfinance was embraced by policymakers and donors as an effective policy tool to fight poverty.<sup>(4)</sup> The following quote by the Prime Minister of Bangladesh at the 1997 Microcredit Summit shows the high expectations that politicians placed on the potential of microfinance: *“In our careful assessment, meeting the credit needs of the poor is one of the most effective ways to fight exploitation and poverty. I believe that this campaign will become one of the great humanitarian movements of history. This campaign will allow the world’s poorest people to free themselves from the bondage of poverty and deprivation to bloom to their fullest potentials to the benefit of all — rich and poor.”* (Banerjee et al., 2015b). In the same 1997 Microcredit Summit, a group of high-profile policymakers, charities, and practitioners started a fund drive to raise US\$20 billion dollars for microfinance start-ups in the next ten years. That included attracting new resources *and* reallocating existing resources from traditional poverty alleviation programs toward microfinance (Morduch, 1999).

Since the 1997 Microcredit Summit, microfinance has become a multibillion-dollar

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(4) The average operational self-sufficiency of MFIs is estimated to be 111% (Table 11.1 in Armendáriz and Morduch, 2010), where the value of 100% indicates full operational self-sufficiency. However, there are some caveats. First, there is a substantial variation, with some MFIs having operational self-sufficiency of as low as 77%. Second, operational self-sufficiency does not mean self-sufficiency at “market” prices, as MFIs often get “soft” loan terms from donors and investors, which can be viewed as indirect subsidies. Third, there is also a concern of whether financial reports provided by MFIs are reliable.

(usually non-profit) industry enjoying the strong support of the wealthy and the powerful. Currently, it affects hundreds of millions of the poorest, most vulnerable individuals, both directly (by providing credits and other financial services) and indirectly (by influencing allocation of funds among poverty alleviation programs). Thus, a natural and vital question arises: What is the impact of microfinance? Specifically, has the impact of microfinance been transformative for the poor, and has it delivered on its promise to “*allow the world’s poorest people to free themselves from the bondage of poverty*”?

This paper provides a survey of studies that aimed to answer these questions and evaluate the impacts of microfinance. The survey is centered on the randomized control trial (RCT) approach to the impact evaluation. The reason is two-fold. First, as I will discuss in the paper, a methodologically rigorous evaluation of the causal effect of microfinance is challenging because of the numerous selection biases that are common for any program with voluntary participation. By introducing exogenous randomization, the RCT approach has the advantage of being able to correctly evaluate treatment (access to microfinance) effects. The second is because both the proponents of the RCT approach and its opponents (e.g. Bédécarrats et al., 2020) agree that, currently, this approach is widely regarded as the benchmark in the field of impact evaluation (Bédécarrats et al., 2020) and “... *entirely dominate[s] development economics*” (Nobel Memorial Prize in Economic Sciences, 2019, p. 1).

I begin the survey by describing the RCT approach to evaluating the impact of microfinance and then present a very influential group of RCT studies that examined the impacts of seven different MFIs in six different countries. These studies were published in the Special Issue of the *American Economic Journal: Applied Economics* (hereafter the Special Issue) in 2015. The Special Issue has been widely celebrated for its seminal contribution to the field of impact evaluation and, as some argued, has provided a decisive contribution to the debate on the impact of microfinance (Ogden, 2016).

While each study in the Special Issue was designed and implemented independently, all six studies were coordinated in that they all used comparable outcomes and

estimation strategies. Moreover, all six studies reached a roughly similar conclusion: *“Summarizing and interpreting results across [the six] studies, we note a consistent pattern of modestly positive, but not transformative, effects.”* (Banerjee et al., 2015b, p. 1). The result itself was not new in that there were earlier impact studies, including non-RCT ones, that reached a similar conclusion (e.g., Coneman, 1999). Nonetheless, the sheer scale of the evidence amassed by the Special Issue—data from six different countries and different types of MFI (urban vs. rural) employing varying methodologies (group vs. individual liability)—and the similarity of results shifted the debate from what the impact of microfinance is to why it is so limited.

The second part of the survey reviews three strands in the literature that offer various reasons why the availability of microfinance has had such modest effects. The first strand focuses on the overall methodological limitations of the RCT approach, including the problems in implementing the RCT protocol and issues related to the internal and external validity of the RCT studies in the Special Issue. The second one argues that the results of the RCT studies are mis-interpreted. The argument goes that RCT studies measure the effect on marginal borrowers rather than on infra-marginal borrowers, which means that the RCT studies understate the average impact of microfinance. The third strand of literature makes the argument that, perhaps, microcredit is not something that the poor need to begin with.

The third part of the survey looks at more recent RCT studies that find the positive impact of microfinance, including one paper that used the exact same methodology and estimation strategy as the papers from the Special Issue. These recent papers are interesting not only because they find positive results but also because they specifically address why their results are different from those of the Special Issue studies.

The last part of the survey is the only one that does not look at RCT studies. Instead, it focuses on the arguments of opponents of microfinance and the studies that reported the negative impacts of microfinance on both the individual level and on the macrolevel. This part was included to demonstrate that the debate on microfinance is not limited to whether its impact is transformative or modest; the impact (potentially)

can be negative. In fact, the Special Issue itself begins with the acknowledgment that “*Various theories—of poverty traps, behavioral decision making, general equilibrium effects, and/or credit market competition—suggest that the impacts of expanding access to credit on poor people need not be positive, and could even be negative.* (Banerjee et al., 2015b, p. 1). I conclude the survey with my personal opinions regarding the impact of microfinance and the current state of the literature.

## 2. Impact Evaluation and RCT approach

### 2.1. Methodological Challenges

With all the focus placed by impact evaluation studies on methodology and minor details of estimation procedures, the fact that microfinance does come with many inspiring stories of pulling poor households out of poverty can be easily overlooked. For example, consider the story of a family of seven who used to live in a poor neighborhood in Monterrey, Mexico, in a home with cardboard walls and dirt floors. Upon taking out their first loan of US\$150 from ADMIC, a local microlender, the household was able to purchase enough supplies to launch a successful business of selling hand-sewn decorations. After ten loans, the household earned enough to install a toilet and an outdoor shower and was preparing to build the second floor (Armendáriz and Morduch, 2010, p. 267).

However, these stories—as inspiring as they are—are not a substitute for rigorous impact evaluation. Methodologically speaking, the impact evaluation of microfinance is a challenging task due to statistical biases that commonly arise when participation and the use of the program is non-random (Banerjee et al., 2015b). First, there is a demand-side selection bias. As taking microloans is voluntary, microborrowers are likely to self-select based on their entrepreneurial drive, productivity, and so on. This bias can go in either direction. On the one hand, people may borrow because of a negative shock that will produce downward bias: impact estimates will be lower than the true causal effect. On the other hand, people may borrow when they expect a

positive shock or because they are more capable, which will produce upward bias.

Second, there could be a supply-side selection bias due to non-randomness in lenders' decision to lend. While some characteristics used by lenders in their decision making can be observable and measurable (e.g., age, gender, education, etc.), others might not be. Third, another supply-side selection bias arises from the fact that MFIs do not locate their offices randomly. Again, the bias can go in either direction. If an MFI establishes its offices in locations with good infrastructure, this will produce upward bias in estimates. In comparison, if an MFI locates its offices to serve stagnant and poor areas, this situation will produce downward bias. Finally, there is an issue of reverse causality. If households that take microloans are richer than their counterparts, then is it because easy access to microfinance made them richer, or is it because it is easier for richer households to get a microloan?

Given these challenges, the overall lack of rigorous impact evaluation studies is not surprising. As recently as 2010, Armendáriz and Morduch (2010, p. 267) wrote "*There's much interest in microfinance and many anecdotes about its benefits. But, so far, there are surprisingly few rigorous empirical studies of net impacts.*" To put this into perspective, this was written five years after 2005, the year declared by the United Nations as the "Year of Microfinance," and four years after Muhammad Yunus received the Nobel Peace Prize. In 2011, a systematic review of the existing impact evaluation studies ordered by the UK government found that almost all studies were biased, incomplete or poorly designed (Duvendack et al., 2011). The few studies that were valid typically concluded that microfinance had a limited and heterogenous impact. Finally, Banerjee et al. (2015b) noted that most of the empirical evidence invoked by microcredit's proponents has been largely based on anecdotes, descriptive statistics, and impact studies that, overall, failed to disentangle causation from correlation.

## **2.2. RCT Method**

The methodological breakthrough came with studies using the RCT method, which, when done properly, can overcome many of the statistical biases mentioned above. "*The beauty of randomized evaluations is that the results are what they are: we compare*

*the outcome in the treatment with the outcome in the control group, see whether they are different, and if so by how much*" (Banerjee, 2007, pp. 115-16). Setting aside implementation issues, the way the RCT approach works is that it introduces an exogenous source of variation. For example, if a researcher can randomly choose a group (treatment) that gains access to microfinance services and then randomly choose another group (control) that is denied access, then, as long as the randomization is done properly, the difference between the average outcomes of the treated and control groups is the average causal impact of intervention.

To see how RCTs are implemented in practice, consider the study of Karlan and Zinman (2011). The authors worked with a Philippine bank that gave three-month loans to poor individuals based on a credit-scoring model. A random lottery was used to determine whether loan applicants, who were marginally eligible for the loan could obtain a loan. Comparing the outcomes of the control group (those who did not get the loan) against the treatment group (those who did get the loan), the authors showed that for the population of marginally eligible borrowers, obtaining a loan had no effect on consumption, had a *negative* effect on business creation, and led to a higher stress level among male borrowers.

While randomized evaluations have been embraced as the gold standard for evaluations, the RCT approach does have its limitations. Bauchet and Morduch (2010) summarized them as follows. First, it provides only an estimate of the average impact; it cannot say much about the median impact or the overall distribution. While the unbiased estimate of the average impact is an important parameter that is worth knowing, consider the situation in which half of the treated population gains by 100% and the other half loses by 100%. Then, the average impact is zero, but the heterogeneity of the impact remains hidden.<sup>(5)</sup> Second, RCT studies might have high internal validity but low external validity. Consider, for example, the Karlan and Zinman (2011) study mentioned above. The study provides an unbiased estimate of the impact of providing the loan to individuals who are a) interested in obtaining the

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(5) To be clear, these are limitations and not insurmountable obstacles. It is possible to learn about the distribution of impacts, for example, by building in stratification.

loan and b) marginally eligible. However, the extent to which these results would be applicable to another group of borrowers remains unclear. Third, for RCT to provide an unbiased estimate, it is essential that the initial random assignment is maintained throughout the study. This, however, can be difficult to implement in the field. For example, it is common for some participants to quit in the middle of the study; if the group of “quitters” is non-random, then this can lead to biased estimates. Another example is contamination, which occurs when the MFI being evaluated—or another MFI—starts working with people in the control group/area thereby contaminating the results.

### 2.3. The Special Issue

Despite its limitations, the RCT method has been widely viewed as a promising approach for studying the microfinance impact. In 2015, *American Economic Journal: Applied Economics* published a special issue dedicated to RCT studies on the impact of microfinance. The Special Issue had six articles that examined seven microfinance lenders in six different countries—Bosnia and Herzegovina, Ethiopia, India, Mexico, Mongolia, and Morocco—during various periods from 2003 to 2012. Table 1 provides detailed characteristics of the microfinance programs studied in the Special Issue. While there were natural variations among the six studies, and each study was conducted independently, the authors exerted substantial effort to ensure that the impact of microcredit was estimated using common estimation strategies for a set of common outcomes.

⟨Table 1⟩ Main Characteristics of the Six RCTs in the Special Issue

	Bosnia & Herzegovina	Ethiopia	India	Mexico	Mongolia	Morocco
Interest rate (APR)	22%	12%	24%	110%	27%	14%
Liability	Individual	Group	Group	Group	Both	Group
Average loan/household income	9%	118%	22%	6%	43%	21%
Sex of potential clients	Both	Both	Female	Female	Female	Both
Area coverage (urban/rural)	Both	Rural	Urban	Both	Rural	Rural
Area coverage (regions/cities)	14 (nationwide)	2 (Western)	1 (City)	4 (NC Sonora)	5 (North)	11 (nationwide)
Unit of randomization	Individual	Association	Neighbourhood	Neighbourhood & village	Village	Village
Sample size (endline)	995	6,263	6,862	16,560	964	5,551

Source: Table 1 in Bédécarrats et al. (2020)

Despite the variations in the program characteristics, the findings of the six studies proved to be quite similar and rather disheartening, as the main conclusion of all studies was that access to microcredit was not transformative for borrowers' businesses or for their consumption. For example, Banerjee et al. (2015a) studied Spandana, one of the largest Indian MFIs at the time, and its microlending program in the city of Hyderabad. The authors showed that access to microcredit had no impact on nondurable and food consumption and found no evidence of increased human capital investment. The impact on consumption of durable goods, as well as on business creation and business assets, was positive in the initial endline survey. In later surveys, however, increases in profit and consumption of durable goods became insignificant.

Overall, the main takeaways from the six RCTs are as follows. First, there is a low take-up rate among prospective borrowers (except for the Bosnian study). In the treatment group, the take-up rate varies from 17% in the Morocco study to 57% in the Mongolian study (Table 10 in Cai et al., 2020). The low take-up rate poses the problem of low statistical power for randomized identification strategies. Furthermore, it raises the question of whether relaxing credit constraints is an effective tool for lifting poor people out of poverty. Second, all six studies reported a lack of evidence regarding the transformative effects of microcredit on the average borrower. Third, the lack of a transformative effect does not come from borrowers' lack of attempts. There is evidence of borrowers' increased investment in their business growth. Fourth, while there is some evidence that microfinance has a heterogeneous impact on borrowers (some groups benefitted, while others did not), the overall low statistical power made statistical analysis of this conjecture impossible. Finally, microcredit does not have any catastrophic effect either. In other words, neither claims made by the proponents of microfinance *nor* claims made by its critics, such as those made by Bateman and Chang (2012), received support from the studies.

While the study by Banerjee et al. (2015b), which is the introductory paper in the Special Issue, frames the results somewhat positively (i.e., the positive effect is not transformative but is, at least, modest), it is worth emphasizing just how few of

〈Table 2〉 Main Results of the Six RCTs in the Special Issue<sup>(6)</sup>

	Bosnia and Herzegovina	Ethiopia	India	Mexico	Mongolia	Morocco
<b>Business ownership</b>	Positive	n.s.	n.s.	n.s.	Positive	n.s.
<b>Business revenue</b>	n.s.	n.s.	n.s.	Positive	n.s.	Positive
<b>Business assets</b>	Positive	-	Positive	-	Positive	Positive
<b>Business investment</b>	n.s.	n.s.	Positive	Positive	-	Positive
<b>Business profits</b>	-	-	-	-	-	Positive
<b>Household income</b>	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
<b>Household consumption</b>	n.s.	Negative	-	Negative	Positive	-
<b>Social well-being</b>	n.s.	n.s.	n.s.	Positive	-	n.s.
<b>Women's empowerment</b>	-	n.s.	-	Positive	-	-

Source: Table 2 of Bédécarrats et al. (2020).

studied impacts proved to be significant despite having such a large, geographically diverse, and coordinated group of studies. Table 2 summarizes the significance and signs of some of the outcomes studied by the Special Issue papers. All in all, among the 298 impacts analyzed in the Special Issue, only 10 were significant at 1%. The Bosnian, Ethiopian, and Mongolian studies had zero significant impacts out of 47, 37, and 41 estimated impacts, respectively. The Indian study had one significant impact out of 99 (Bédécarrats et al., 2020). Using 10% significance instead of 1% still means that as many as 81% of the studied effects are insignificant. Table 2 also uses the 10% threshold and shows 17 significant impacts. If a more stringent threshold of 1% was used, then none of the impacts would be significant.

### 3. Reasons for the Limited Impact

Based on the results mentioned above, a natural question arises: “Why do these studies show such limited impact?” In this section, I discuss three groups of explanations suggested in the literature: improper implementation of the RCT protocol by (at least some of) the six studies as well as concerns over their internal and external validity; incorrect interpretation of the results; and, more broadly, an argument that microcredit is not something that the poor need to begin with.

(6) ‘Positive’ (‘Negative’) means the impact is positively (negatively) significant at 10%; ‘n.s.’ means not significant at 10%; ‘-’ means no data.

### 3.1. Methodological Issues Related to RCTs

Since its publication, the Special Issue featuring six RCT studies has become a highly respected source of knowledge on microfinance to date. One can see it in the number of citations received by the papers from the Special Issue, in how reverently it is treated in more recent works (see, e.g., Meager, 2019), and in how widely its results have been disseminated among microfinance practitioners. Not everyone, however, agrees that the six RCT studies deserve such a prominent position in the field. Bédécarrats, Guérin and Roubaud (2020), hereafter BGR, provided a thorough and critical analysis of the six Special Issue studies, highlighting such issues as low statistical power, inconsistencies with sampling, data collection, and questionable interpretations.

The most detailed criticism in BGR was directed toward the Moroccan study (Crépon et al., 2015), specifically, regarding its internal validity.<sup>(7)</sup> First, BGR argued that randomization, a key element of the RCT methodology, was not done properly in Crépon et al. (2015). A very low take-up rate, much lower than initially expected, made the research team and loan officers perform some mid-study corrections to push more villagers toward taking a microloan and to increase the overall number of loan-takers. That included a variety of measures, such as introducing one-off bonuses for loan agents, withdrawing the minimum quota for women, pushing back village borders, and dropping villages with zero take-up rates. Notably, these mid-study corrections created not only internal but also external validity issues. Tweaking the product for the sake of the RCT meant that the RCT's outcome was different from how the actual product functioned in reality. Second, there were issues with the data trimming of the baseline and endline surveys. According to BGR, changing the endline trimming threshold by 0.2% produced radically different results in terms of sales, expenses, investment, and profits. Finally, BGR raised concerns regarding the

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(7) The debate seems to be ongoing. Authors of Crépon et al. (2015) have responded to the critique raised in BGR in Crépon et al. (2019). In turn, BGR, together with the fourth co-author, produced a working paper where they “rebutted the rebuttal” (Bédécarrats et al., 2019).

data collection process. In particular, local interviewers in the Moroccan study did not speak Berber, a language spoken in the targeted area, which meant that many improvisation and impromptu translators were used during the data collection, which likely resulted in numerous errors in the data.

The question of internal validity can also be raised for other studies. For example, as in the Moroccan study, randomization issues can be found in the Mongolian and Bosnian studies. In the Mongolian study, in each selected village, loans were given to the first 30 women (rather than 30 randomly selected women) among those who expressed interest in obtaining a loan. In the study conducted in Bosnia and Herzegovina, instead of using random selection, loan officers were simply asked to select potential clients who were not deemed eligible by the current MFI's standards. Another example of an internal validity problem that affected the studies in the Special Issue was how pre-treatment baseline surveys and post-treatment endline surveys were conducted. In the Ethiopian study, different individuals were surveyed in the baseline and endline surveys, whereas in the Mexican study, 73% of the baseline households were not surveyed at the endline, and 89% of the endline households were not surveyed at the baseline.

In addition to internal validity concerns raised in BGR, there is another concern—acknowledged in the Special Issue itself—regarding low statistical power. McKenzie (2012) was the first to point out that the RCT studies in the Special Issue were heavily underpowered, demonstrating that, to detect a 10% increase in profits with 90% power in the Indian study (Banerjee et al., 2015a), the required sample size had to be over 15 million (McKenzie, 2012, p. 218).

More recently, Dahal and Fiala (2020) reviewed the six Special Issue studies together with the Philippine study by Karlan and Zinman (2011) and the Uganda study by Fiala (2018). They also showed that due to the low take-up rate, every study was considerably underpowered to detect the effect sizes. Pooling the data across the eight studies considerably improved the situation, though not perfectly. With the pooled sample, they found a significantly positive (at 1%) impact of microfinance on business profit and a significant increase of household assets. The impact on total

consumption remained insignificant.<sup>(8)</sup> Dahal and Fiala's (2020) overall conclusion, however, was very cautious:

“Unlike the perception among many critics of microfinance, the studies reviewed here do not discredit the role of microcredit in poverty alleviation and improving [the] livelihoods of poor households. Nor does combining the samples together definitively show impacts. What these results do suggest is that the impact of the microfinance programs studied in these experiments is not well known.” (p. 4, emphasis is mine).

### 3.2. Interpretation of the RCT Results

Dahal and Fiala (2020) chose a very measured interpretation of their results, despite being able to obtain some positive significance from the pooled sample. As they have acknowledged, the reason is that theirs is a push-button replication. This means that their approach took data at their face value and did not address issues related to possible data contamination, such as those found in the Moroccan study discussed in the previous subsection.

Another reason why one should be cautious when interpreting RCT study results is that, as argued in the literature, they do not actually answer the “big” questions regarding the impact of microfinance. These so-called “big” questions that should be addressed by impact studies are as follows: Have regional poverty rates fallen thanks to microcredit? or Have Grameen Bank and others like it made an appreciable difference in the economic and social lives of customers? (Morduch, 2020). These are, in fact, not the questions that these studies answered. Instead, the RCT studies of the Special Issue evaluated the impact of microfinance on marginal microfinance clients who adopted microfinance later than typical MFI clients. In other words, even though RCT studies provide unbiased impact estimates, the impact estimates they provide are on the external margin, which means that they understate the average impact of microfinance (Wydick, 2016).

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(8) Meager (2019) used pooled data from seven RCT studies—the six studies from the Special Issue and the Philippines study by Karlan and Zinman (2011)—and reached similar conclusions.

Indeed, four out of the six Special Issue studies relied on randomization of the order in which a given microcredit lender entered new locations. However, the new locations entered by MFIs in the mid-2000s were likely to be marginal areas that had been previously neglected, entirely or partially, by microfinance industry. They were unlikely to be representative of “regular” areas. Furthermore, the control areas did not remain void of microcredit during the study duration. For example, a study of a large Indian MFI, Spandana, entering the city of Hyderabad (Banerjee et al., 2015a) used randomization at the level of the neighborhoods where Spandana provided its loans. However, other lenders moved into the neighborhoods left vacant by Spandana; hence, 18.3% of the control group (vs. 26.7% in the treatment group) had microcredit loans 12-18 months after the baseline survey. In other words, the interpretation of Banerjee et al. (2015a)’s results is actually very narrow, as it identifies the impact of bringing additional microcredit into parts of Hyderabad (Morduch, 2020).

In comparison, the Bosnia and Herzegovina study (Ausburg et al., 2015) did not use entry as the basis for its randomization strategy. Instead, it used an MFI that operated in an area that was already heavily credit-saturated and over-indebtedness was a recognized problem: 41% of the treatment group had microcredit at baseline. The randomization strategy was applied to randomly select borrowers, whose credit scores were too low get a loan. Clearly, these are not typical borrowers.<sup>(9)</sup> From a policy perspective, it might be worthwhile to see what happens if those borrowers are given access to microcredit, and this is what the results of Ausburg et al. (2015) tell us. However, it remains unclear whether we can infer anything from this study regarding the impact on typical borrowers (Morduch, 2020).

The discussion in this subsection, so far, has explained why the RCT studies in the Special Issue may have understated the microfinance impact. The impact they

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(9) Providing credit to those borrowers also raises some ethical issues. Giving loans to individuals with low scores and history of repayment difficulties violates the “do not harm” principle (Bédécarrats et al., 2020). In fact, those marginal borrowers who received the loan during the study had repayment difficulties and risked over-indebtedness. As the authors of the Bosnian study concluded themselves, “*All this suggests that the loan officers had good reason to classify our target population as marginal*” (Ausburg et al., 2015, p. 201).

identified was on marginal areas and/or marginal borrowers rather than on typical average borrowers, who were likely to benefit more from access to microcredit. However, RCT studies can also overstate the impact of microfinance because, by design, they cannot evaluate the negative consequences of over-indebtedness.

To evaluate the impact of microfinance, RCT studies have to look at areas or populations that, at the baseline, are ideally free of microcredit. Even if the baselines of the Special Issue studies were not perfectly microcredit-free, it would be fair to say that the studied areas were underserved by MFIs.<sup>(10)</sup> Therefore, almost by design, the issues of over-indebtedness and debt trap, which often arise when microcredit becomes available, do not appear. However, we know that these issues do appear in “regular” areas. Such over-indebtedness has been documented in four out of six studied countries: Bosnia, Mexico, India, and Mongolia (BGR and references therein, footnote 30). Thus, because of their focus on marginal areas, the RCT impact studies underestimated both the positive *and* negative outcomes of microcredit.

### 3.3. Mismatch between Microcredit and Borrowers’ Needs

In addition to the aforementioned methodological issues faced by RCT studies, the literature has suggested another explanation for the limited impact of microfinance: it does not consider “*the lifestyles, income levels and cash flows of the poor*” (Collins et al., 2009, p. 26). Similarly, Verrest (2013, p. 58) argues that business-development programs aimed at owners of small and micro-enterprises “*are relevant to only a minority of entrepreneurs*” due to variations in household vulnerability or a lack of business ambition. In fact, most microfinance recipients do not view microfinance loans as a tool for business development, but rather as a valuable diversification tool for dealing with irregularities in income sources (Krishna, 2004); as a way to reduce

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(10) For example, the Mexican study (Angelucci et al., 2015) followed a microfinance lender who was expanding in Sonora state after a long period of violence. At the baseline, 10% of the treatment group had microcredit loans, and 29% had formal bank loans. Thus, while the market was not microcredit-free, it was not credit-saturated either. The Bosnian study is an obvious exception, as the study itself acknowledged the credit-saturation of the market and focused on marginal borrowers instead.

the household's vulnerability to negative shocks, such as job loss or illness (Ellis, 2000); or as a strategy for consumption and income smoothing (Bateman and Chang, 2012; Banerjee and Duflo, 2011).

Furthermore, Banerjee (2013, p. 512) argues that many microfinance clients are neither interested nor “...*particularly good at growing [their] businesses.*” He based this argument on the following observations. First, in a survey conducted in India, 80% of parents hoped their children would obtain government jobs, while 0% hoped their children would build successful businesses. In other words, the fact that the poor happened to be self-employed entrepreneurs is a reflection of their lack of other options rather than a reflection of their business ambitions. Second, because of imperfect labor markets, poor households tend to have unused labor endowment, and while access to microcredit allows households to make full use of such labor, it also means that we should not expect micro-enterprises to grow beyond the size dictated by the amount of that unused labor. Finally, many MFIs exclude men and target only female borrowers. However, as studies have shown, there is no evidence that women are better entrepreneurs than men. For example, a study in Sri-Lanka randomly assigned either US\$250 or US\$500 loans to micro-enterprises and found that, after controlling for human capital, the profits of female entrepreneurs did not go up at all, while the profits of their male counterparts went up significantly (de Mel et al., 2009). Similar results have been reported by Fafchamps et al. (2014). While there are some suggestions as to why female entrepreneurs performed worse—Fafchamps et al. (2014), for example, suggest that women are less able to turn grants into investments, because of other claims on their resources, such as family tax—the very fact that they do perform worse, combined with MFIs' focus on female entrepreneurs, can contribute to a lack of significant impact of microfinance on business-related parameters, such as profits.

Another way in which access to microcredit does not fully suit borrowers' needs is that borrowers might not be credit-constrained. A standard tool for evaluating whether borrowers are credit-constrained is to conduct capital-drop RCT studies. In this approach, microentrepreneurs are randomly assigned to different amounts of either in-

cash or in-kind grants, allowing researchers to estimate the rate of return generated by the provision of extra cash to microentrepreneurs.

The evidence is somewhat mixed. On the one hand, several capital-drop RCT studies have found very high capital return rates, indicating that borrowers *are* credit-constrained. McKenzie and Woodruff (2008) report that the marginal product of capital is 20%-33% per month, while de Mel et al. (2008) report it to be around 5% per month. Similarly, Fafchamps et al. (2014) report that both in-cash and in-kind grants generate very high returns on investment. On the other hand, Karlan et al. (2012) and Berge et al. (2015) gave a randomly selected group of micro-entrepreneurs in Ghana and Tanzania, respectively, a capital drop and found that this had no effect on investment, profits, or revenues.

#### **4. RCT impact studies with significantly positive results**

Recently, two RCT studies have demonstrated the significantly positive impacts of microfinance: Fiala (2018) evaluated the effect of providing loans to existing business owners in Uganda, and Cai et al. (2020) evaluated the impact of village credit programs in China. In this section, I will discuss both papers and explain how they differ from earlier RCT studies.

Fiala (2018) collected a sample of 1550 existing business owners who twice expressed interest in taking business loans and business training. These 1550 individuals were randomly assigned to one of five treatments: control, loan treatment, loan treatment combined with business training, grant treatment, and grant treatment combined with business training. The grants came in the form of cash drops that did not need to be returned. In comparison, the loans had to be returned. However, unknown to the study participants, they were subsidized to induce the microcredit organization to take on clients they would not normally work with, such as men, those without a credit history, and those without sufficient collateral. Thus, the study provided a test of the impact of microloans on a sample that was interested in loans

but, for whatever reason, could not obtain them prior to the study.

The main result is as follows. Consistent with the earlier literature, including the papers surveyed above, there was no effect on business profits from any of the five treatments in either the pooled sample or in a sub-sample of female business owners. On the other hand, male business owners in the loan treatment group reported up to 54% greater profit in the last month compared to the control group, and the overall result is statistically significant at 1%. The effect increases with business training, lower risk preferences, and no prior history of loans. There is no effect on business profit in grant treatments for male business owners.

Given that earlier RCT studies, including those in the Special Issue, found that access to microfinance has an insignificant impact on business profits, we are prompted to ask why the results reported by Fiala (2018) are different. Fiala (2018) himself points at two factors. First, his study is less statistically underpowered. Given the sample design-business owners who expressed an interest in loans twice-the take-up rate was relatively high: 41% for loans and 71% for grants. As we discussed earlier, the Special Issue RCT findings were severely underpowered due to the low take-up rates (Dahal & Fiala, 2020). Additionally, the use of multiple data collections employed in the paper (two separate baselines and endline surveys) has been shown to further increase the statistical power. Second, Fiala (2018) paid particular attention to a group that tends to be underserved by microfinance (i.e., the group of male borrowers). Out of the six Special Issue RCTs, three (India, Mexico, and Mongolia) examined MFIs that did not provide loans to men, and one study (Morocco) had the minimum quota for female participants, which was later withdrawn due to a low take-up (Bédécarrats et al.,2020). Similarly, a well-known non-Special-Issue RCT impact study done by Karlan and Zinman (2011) in the Philippines was also primarily female, with 85% of the sample being women. However, as follows from Fiala's (2018) estimation, there was no significant impact of loans on business profit in the pooled sample and in women-only sub-sample. The significantly positive impact was present exclusively in the men-only sub-sample.

Cai et al. (2020) conducted another RCT study that reported the positive impact

of microfinance on household incomes and poverty reduction. The study evaluated a large-scale village banking program in China that aimed to increase credit access to households in poor villages. A total of 1500 households in 50 villages, comprising 10 counties, were surveyed at baseline, and then 1351 households (90%) were re-interviewed at the endline survey conducted two years later. The study found that the credit program increased household income by 46%, with the intent-to-treat effect being significant at the 1% level. The program also reduced poverty by 17% and increased durable consumption by 30%.<sup>(11)</sup>

Notably, Cai et al. (2020) followed the exact same empirical specification as the Special Issue studies and, therefore, a large part of their paper is dedicated to the investigation as to why their results differ. They argue that the difference can be attributed to several factors: lower access to formal credit before the program, lower interest rate, less frequent repayment schedule and, what is perhaps unique to China, a higher capital return of credit-constrained households on off-farm employment.<sup>(12)</sup>

We discussed earlier how, given the overall high penetration of MFI, it can be difficult to find areas where potential borrowers had no prior access to microfinance. Even if one can find those areas/populations, they are unlikely to be representative. In China, microfinance NGOs are heavily regulated and have a lower presence than in other studied countries. At baseline, only 13% of households had previously borrowed from formal institutions. Therefore, program participants are likely to be more representative of typical rural, poor households than those studied in the Special

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(11) The study did not look at gender differences in the impacts so it cannot be compared to Fiala (2018). The credit program did not exclude men, although it says it gave priority to poor households and female members. According to Table 1 in Cai et al. (2020), 94% of heads of households, which was the unit of analysis, consisted of men.

(12) Jiang et al. (2020) is another impact study of the credit village program in China. The study examined the impact of the credit program in Yunan County, a poor rural county in Guangdong Province. Based on the sample of 214 households, Jiang et al. (2020) found that the program loosened the credit restrictions; however, there was no impact on educational expenditures, medical expenditures, or long-term assets. The paper did not use the RCT methodology and conducted just one survey instead of baseline and endline surveys, as in Cai et al. (2020), thus explaining the difference in results.

Issue. Another difference is that the banking program studied in Cai et al. (2020) had much lower interest rates (9.4%) and less frequent repayment schedule (annual) than other RCT studies. In contrast, all but one Special Issue studies had either monthly or weekly repayment frequency and interest rates varying from 14.5% in Morocco to 118% in Ethiopia. Finally, China might be unique in its wide availability of high-paying migrant-wage jobs located in distant cities that are too costly to reach. Credit-constrained households, who had no choice but to forgo the migrant opportunities prior to the program, were able to undertake them upon enrolling in the credit program. Indeed, Cai et al. (2020) demonstrated that the increase in wage income was driven by a 24-day increase in work as out-migrants to other provinces and that the total wage labor supply increased by 26 days per year as compared to the baseline of 116 wage labor days.

## 5. Negative Impact of Microfinance

This review would not be complete without mentioning how microfinance can negatively impact the poor and their communities. It might be surprising that providing credit to the poor can have negative consequences, but, as it turns out, there are many examples—both on a local and on a global scale—of microfinance going awry.

On a local scale, there is an abundance of anecdotal stories of extremely abusive and coercive microfinance officers who enforce repayments with, as one paper put it, “*alarming zeal*”. For example, Rutherford (2006) mentioned an African MFI that locked groups of borrowers into a meeting hall until they settled their outstanding loan repayments for that week. Even more disturbingly, in India, the prosecution of defaulters can involve insults, physical threats, property confiscation, or even tying up a defaulter on a public square under direct sunlight (Arunachalam, 2011; Bédécarrats et al., 2020).

On a global scale, microfinance penetration can lead to over-indebtedness and full-

blown sub-prime-style crises. The first country-level microfinance crisis occurred in Bolivia in 1999. Initially, in the early 1990s, Bolivian MFIs were highly successful in providing credit to the poor in a sustainable manner. Standard microfinance practices, such as frequent group meetings, joined liability, immediate follow ups on delinquency, and small initial loan amounts, helped minimize lenders' risks and ensured high repayment rates. Even though these practices sacrificed borrowers' convenience, they were tolerated by borrowers due to a lack of alternatives. In the late 1990s, the commercialization of the industry, including entry by large consumer lenders, forced the MFIs to relax their practices to become more attractive to clients. With easily obtainable loans from multiple lenders, be it commercial lenders or standard MFIs, the crisis of *sobreendeudamiento* (over-indebtedness) arrived. The debtors revolted, and "by mid-1999 the consumer lending movement crashed" (Rhyne, 2002, p. 13).

Unfortunately, the collapse of consumer lending in Bolivia in 1999 is not unique. Morocco, Nicaragua, and Pakistan in 2008; Bosnia in 2009; and the Indian state Andhra Pradesh in 2010 all had their own MFI crises that featured the same scenarios: easily obtainable loans, over-indebtedness, rapidly growing client defaults, and then the losses and/or bankruptcy of the MFIs (Bateman and Chang, 2012).

The introductory article for the Special Issue (Banerjee et al., 2015b) acknowledged the rising concerns about over-indebtedness and microcredit becoming a debt trap. However, it also argued that the six Special Issue RCTs found no evidence that expanded access from one MFI led borrowers to take on additional debt from other sources (p. 11). In other words, the six studies found no evidence of borrowers exhibiting the debt-trap behavior. Recall, however, that the same RCT studies examined the margins (areas or populations) that were underserved by MFIs. As the very first step was to find a group that, at baseline, had little access to the microcredit, the Special Issue studies were ill-equipped to evaluate the negative aspects associated with over-indebtedness.

In addition to the well-recognized potential for over-indebtedness, other negative impacts of microfinance suggested in the literature include worsened or, at best,

unchanged poverty levels, increased income inequality, increased workloads and child labor, decreased school enrollment, and the creation of dependencies and barriers to sustainable local economic and social development (van Rooyen et al., 2012). However, convincingly showing the negative impacts of microfinance runs into the exact same methodological problems as any other microfinance impact study, such as selection issues, low statistical power, and so on. For example, there is little debate that over-indebtedness is a problem, but establishing a causal link between microfinance and over-indebtedness would be a much harder task.

Nonetheless, there are studies that are able to rigorously demonstrate that microfinance can lead to negative outcomes—not necessarily on average but, at least, for certain groups of borrowers. For example, Coleman (1999) showed that microfinance had a negative impact on men’s healthcare expenditures and trapped some female borrowers in a debt trap. Kondo et al. (2008) showed that microfinance helped well-to-do households by increasing their earnings and expenditures, but it had a negative effect on the impoverished households. Gantle et al. (2015) studied female entrepreneurs in Ghana and showed that some of the borrowers became worse off due to their inability to repay the loans. Finally, Seng (2018) provided a rigorous analysis of the impact of microfinance on household welfare in Cambodia, a country in the top five in terms of MFI penetration rates. The selection bias was addressed using two different econometric techniques: the endogenous switching model and the bivariate discrete choice model with endogenous treatment effects. Using a sample of 7801 households, Seng (2018) showed that the effect of microcredit on household welfare was negative and that the difference was statistically significant at the 1% level.

## 6. Conclusion

I conclude the survey by presenting my personal take on the impact evaluation literature and its current state. First, in my opinion, the use of RCT methodology, which occupies a central part of this paper—is a correct move in the right direction

but one that came too late. With an exception of, perhaps, China, where microcredit programs are heavily regulated and are not widespread, microfinance penetration is too high. It is a difficult, if not impossible, task to find a population that, at baseline, has no access to microfinance. While other surveys tend to summarize the literature results cautiously as “*we still know very little about the impact of microfinance*” (Dahal and Fiala, 2020, p. 1), I personally think differently: as far as the big questions go (e.g., Have regional poverty rates fallen thanks to microcredit? and Do MFIs have a transformative impact in the lives of their customers?) we will never find out.

Second, there is an undeniable disconnect between how much the microfinance industry is praised for its transformative role in alleviating poverty and the lack of rigorous evidence supporting those claims. It creates a fundamental puzzle: If microcredit is not creating large, positive impacts, why are poor people continuing to borrow, especially when they are paying relatively high interest rates year after year? (Morduch, 2020). A positive answer is that borrowers clearly need microloans, and researchers are the ones asking the wrong questions and looking at the wrong places. I personally tend to think negatively. Setting aside the issue of some borrowers falling into a debt trap, there is evidence of constant pressure from microfinance officers that borrowers use microfinance services. Rutherford (2006) reports multiple in-depth interviews with Grameen Bank clients, and the word “pressure” is very common in those interviews: “*Now they [a household that was a microfinance borrower] ... are resisting pressure from the centre manager to take a loan*” (p. 15); “*but as is usually the case she came under strong pressure from the staff to borrow as well as save.*” (p. 38); “*As pressure piles on to centre managers to recruit as fast as possible*” (p. 43), etc. The more borrowers MFIs serve, the more success they can claim, the more donations they can receive and, we should not forget that, the higher the increase in the salaries of executives.<sup>(13)</sup> In my opinion, it is this pressure to take loans that microlenders transfer onto the borrowers, and not a genuine need for microloans, that drives so many people to continue to borrow from these MFIs.

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(13) In a few instances when MFIs went public via IPO, the gains that senior managers were able to garner were at the level of tens of millions of dollars (Bateman and Chang, 2012).

Third, there is a clear consensus that the impact of microfinance is heterogeneous. In particular, there are groups of borrowers who seem to benefit from microfinance. However, given the statistical power issues, it is difficult to determine an effect that is statistically significant, because looking at sub-groups necessarily reduces the sample size. While more work is needed, this seems highly promising, as it allows for a more targeted application of microfinance instead of pressuring all potential borrowers into taking loans.

Finally, when something becomes as huge as the microfinance industry has become, and when this something deals with the most vulnerable population, it is prudent to err on the side of skepticism and cautiousness. However, credit must be given where credit is due, no pun intended. Microfinance has accomplished something that, for a long time, has been considered impossible: providing credit and other financial services to the poor. Microfinance has also accomplished this in a very smart way by combining innovative methodologies that worked. Thus, in my mind, there is no question that the existence of sustainable services providing financial services to the poor is a miracle, not a mirage.

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