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The Impact of Microcredit on Farms and Rural Household: A Literature Review of Experimental Studies

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The Impact of Microcredit on Farms and Rural Household: A Literature Review of Experimental Studies *

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Résumé/Abstract

In this article, we review the literature on the best ways to identify the causal effects of microcredit, present, and discuss some empirical results of the impact of microcredit on the adoption of innovations, investments, farm incomes, and profits. The results of empirical studies converge toward a positive impact of access to microcredit on the adoption of agricultural technology and investment. In terms of the effect on the technical efficiency of farms, agricultural income and profit, and consumption, the results do not all point in the same direction. The effects of microcredit are likely to vary depending on the context of the study.

Mots clés/Keywords: Microcredit; Experimental Studies; Causal Effects; Farms; Rural Households

Codes JEL/JEL Codes: C68; C82; D58; R13; R15

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1 Introduction

Agriculture is an important economic sector, which contributes up to 30% of the GDP in less developed countries (LDCs) (FAO, WFP & IFAD, 2012). Beyond producing food, it provides multiple other services such as renewable natural resources management, landscape and biodiversity conservation, and contributes to the socio-economic viability of rural areas (Rentinga et al., 2009). More than ever, the agricultural sector faces big challenges. Some of these challenges are adaptation to climate change and food security. Agriculture has to feed hundreds of millions more people in the future while using scarce resources more efficiently and still providing environmental services. Moreover, in LDCs where the livelihoods of almost three-quarters of the population depend on agricultural activities, it has to address poverty issues as well. Recent figures tend to show that the associated growth from this sector proves to be more effective at reducing poverty than growth originating from other economic sectors (World Bank, 2013). However, without a significant improvement in the access of farmers to technology, markets, information and credits, agricultural sector will not be able to adapt production systems and cope with the challenges it is facing (FAO, 2016).

In the past fourteen years, several innovations have been introduced in agriculture to improve the productivity of different commodities. In LDCs, we can, for example, mention the use of improved seeds and organic manure, better use of mineral fertilizers, water and soil conservation techniques, etc. (Ouedraogo, 2005; Sawadogo et al., 2008; Liniger et al., 2011). Even though these innovations had proven agronomic benefits, not all farmers were able to adopt them.

What should we expect from microcredit?

In the past years, conventional banks and microfinance institutions have encouraged access to credit in order to increase farmers' adoption of innovative practices (Djato, 2001; Wampfler, 2004; Roesch, 2004; AGRA, 2014; FAO, 2016). Specifically we should expect that access to microcredit has a positive impact on investment in agricultural activities, encourages a better-input use and favors adoption of new technologies. Moreover, potentially, because of better investment and/or inputs use and/or access to new technologies, access to microcredit has a positive impact on farms technical efficiency and productivity and then improves the profitability of farms activities. There is also a potential positive effect on the non-farm revenue of the rural households through an increase of investment. Figure 1 summarizes the hypothesized impacts of the microcredit.

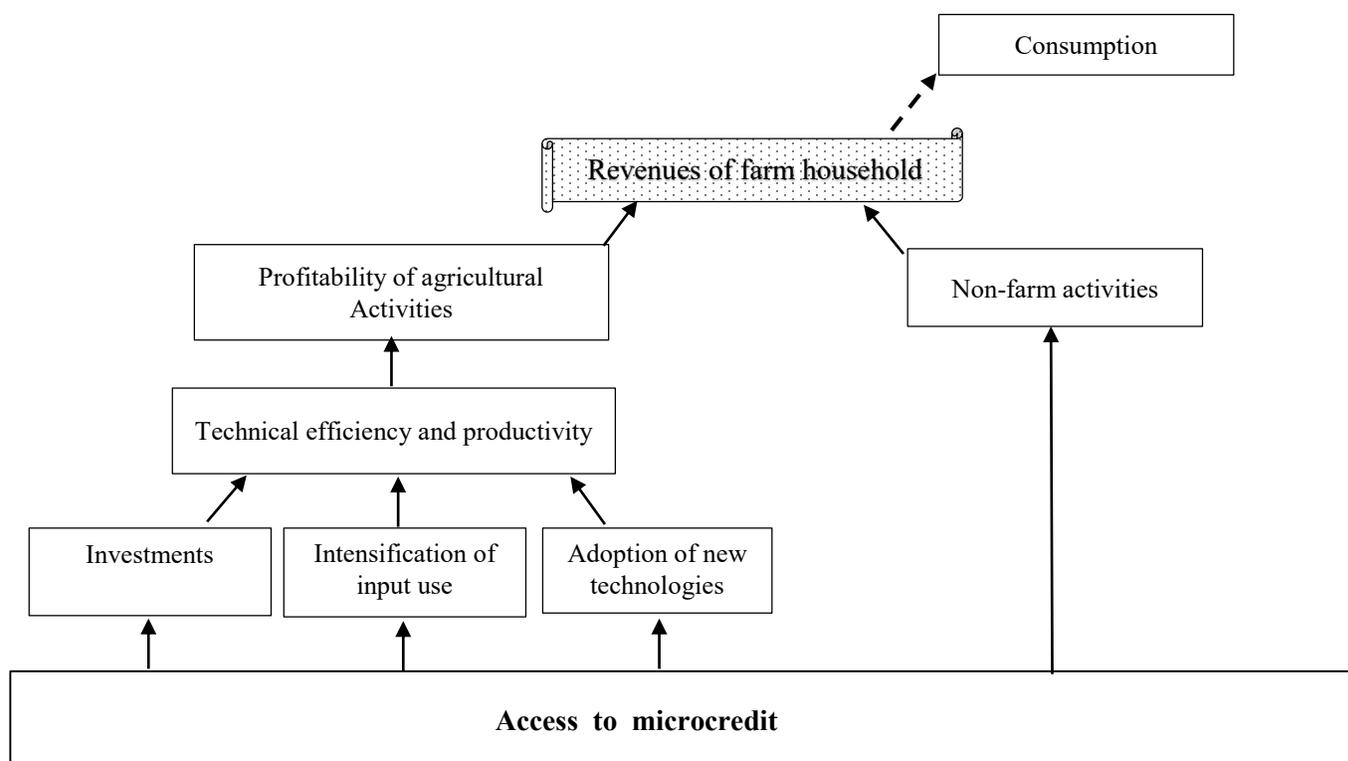


Figure 1. Impacts – postulated - of microcredit on farms and rural households.

The objective of the present literature review is to analyze the empirical results regarding the impact of microcredit on the adoption of innovations, investments and agricultural incomes and profits. Indeed, analyzing the effects of microcredit on farmers' adoption of innovative practices remains relevant as the results of various studies reported in the literature are mixed.

Assessing the impacts of access to microcredit consists of comparing the actual situation of beneficiaries of microcredit and their situation if they had not benefited from it. Obviously, the latter situation is not observable because a farmer either benefits from the microcredit or does not. There are several quantitative approaches used to identify the causal effect of microcredit.¹ These approaches are grouped into *quasi-experimental* methods and *experimental* methods. However, difficulties in estimating the real causal effects of quasi-experimental methods (See appendix A for a brief discussion) led to the development of experimental studies that allow having a group of non-beneficiaries with similar characteristics to the group of beneficiaries to serve as counterfactual. The literature on experimental methods of impact evaluations shows that one of the best methods for the construction of the counterfactual is random assignment (Duflo et al., 2007; Imbens and Wooldridge, 2009).² It allows the random assignment of individuals into two groups, the group of beneficiaries (treatment group) and the group of non-beneficiaries (control group) from a list of individuals who meet all the selection criteria established by the program. This makes it possible to have two comparable groups and therefore be able to assess the real causal effects of access to microcredit.

¹ Scriven (2008) and Bamberger and White (2007) among others suggest the use of mixed method design combining qualitative and quantitative approaches in formulation, implementation and analysis of the evaluation in developing countries.

² However, there are ongoing discussions on the limitations of the experimental methods (see e.g. Deaton, 2010; Shaffer, 2013) and the best way for strong impact evaluation using them (see e.g. Imbens, 2014; Athley and Imbens, 2016b).

The paper proceeds as follow. Section 2 identify the best approaches to measure the causal effects of microcredit when using random control trials. Section 3 presents and discusses the different empirical results regarding the impact of microcredit on farmers' investments, use of inputs and adoption of innovative practices, as well as the impacts on income and consumption. Section 4 concludes.

2 Principles and challenges of experimental methods when estimating the impact of microcredit

Banerjee et al. (2015) identify three important elements to consider when designing experimental studies on microcredit, namely low demand for credit in general, weak demand for the form of credit offered by the experiment and the presence of close substitutes that may be formal or informal (loans between producers or from a local merchant). Not considering these elements can lead to mixed conclusions on the outcome of the experiment (Banerjee, 2013).

2.1 Random assignment

Random assignment is used to avoid two types of potential selection bias on the microcredit supply and demand sides (Banerjee et al., 2015). From the demand side, it is important to prevent those who have been selected as potential borrowers (treated) from being different from those who were not selected (controls). Otherwise, the true causal effects of microcredit are not captured. The bias effect can be positive (overestimation) or negative (underestimation) (Banerjee et al., 2015). The principle is identical regarding the supply of microcredit. For example, working with well-structured farmers' organizations that are already providing (directly or indirectly) several services to their members, including financial services, could generate a selection bias.

Random assignment at the individual level

Random assignment can be done at the producer level (individually) or at the community level. Random assignment at the individual level increases the power of statistical tests because researchers have good control over the profiles of producers in the control group and those in the treatment group (Banerjee et al., 2015). However, this would mean rejecting or accepting loan applications not only based on their quality but also randomly. In several field experiments, this can create many misunderstandings. Studies such as those by Augsburg et al. (2015) and Karlan and Zinman (2008, 2011) used the method of random assignment of individuals in treatment and control groups. The advantage of this approach is that it provides significant statistical power, but the diffusion effects is difficult to control.

Random assignment at the group level

When the assignment is done at the group level, the literature suggests using additional methods to encourage credit demand if it is too low.³ For example, the advantage of an assignment at the farmers' organization (FO) level is that it captures the effects within the group in addition to the effect at the individual level. The effects of diffusion to other farmers can therefore be studied. Thus, it is possible to have a diffusion effect within FO (intra-FO diffusion effect), as in, for example, the case of farmers who adopt technological innovations because of financial support from a farmer who has more income because he/she benefited from microcredit. The same phenomenon can also exist outside the FO whose members have access to microcredit (extra-FO diffusion effect). Ideally, the experimental design should allow for the capture of the possible existence of these two types of diffusion effects. Regarding credit

³ One concern that could be introduced by this practice is that of external validity (see Deaton, 2010). Athley and Inbems (2016b) suggested some methodological approaches to tackle this issue.

access, the literature suggests that the diffusion effects are very low (Crépon et al., 2015). However, several studies show that these effects exist when it comes to studying the adoption of new technology (Tamimi, 2011; Teklewold et al, 2013). Crépon et al. (2015), in their impact assessment study of microcredit in Morocco, adopted a methodology based on random assignment of treatments at the community level. Eligible villages in the program were matched according to observable characteristics. Such an approach has the advantage of measuring the impact at the community level and internalizing the spillover effects that might occur within the communities. In addition, to be able to estimate both the direct impact of microcredit and possible externalities for non-borrowers, Crépon et al. (2015) included in their sample households that ex-ante had a high propensity to take up the credit and those with a low propensity. The assignment method of treatment at the community level (cluster) was also used by Beaman et al. (2014), Angelucci et al. (2015), Attanasio et al. (2015) and Banerjee et al. (2015). However, unlike Crépon et al. (2015), these studies randomly selected individuals or households regardless of their propensity to borrow.

Takahashi et al. (2014) experimented on the effect of the design of loan contracts on the credit demand of poor households in Bangladesh using a two-stage double stratification sampling strategy. The random assignment was done at two levels: at the village level and at the household level. The challenge of such an approach is to control the effects of contamination due to informal transfers that could arise among treated individuals and controls because of their proximity. Table 1 summarizes the assignment approaches used in recent studies.

Table 1. Synthesis of experimental design

Studies	Countries	Randomization unit	Randomization process	Targeted Population	Sample size	Type of credit
Takahashi et al (2014)	Bengladesh	1600 households	Stratified selection at village and household level	Random selection of households stratified by poverty status (very poor, averagely poor and not poor)	1600	Group credit
Crépon et al (2015)	Morocco	162 villages	Random selection of treatment clusters after baseline	(1) Households with a high propensity to contract credit; (2) random selection of households	5551 with addition of 1433 new household at the end line	Group credit
Angelucci et al (2015)	Mexico	238 clusters (villages and town neighborhood)	Random selection of treatment clusters	Woman aged 18-60 years with IGA or will create one if she has enough money or take credit from an MFI	16560 of which 1,823 in panel	Group credit
Attanasio et al (2015)	Mongolia	40 villages	Random selection of treatment clusters after baseline	Woman having fulfilled the eligibility criteria and willing to take credit from an MFI	1148	Individual and Group credit
Augsburg et al (2015)	Bosnia-Herzegovina	1196 credit applicant individuals	Random selection of individual in treatment group after baseline	credit applicant considered too risky to be given credit as a normal borrower	1196	Individual credit
Banerjee et al (2015)	India	104 neighborhood (clusters)	Random selection of treatment clusters after baseline	Household with at least one woman aged 18-55 years resident at the same address for at least 3 years	6863	Group credit

Note: IGA=Income Generating Activity; MFI= Microfinance Institution

Table 1. Synthesis of experimental design (Cont'd)

Studies	Countries	Randomization unit	Randomization process	Targeted Population	Sample size	Type of credit
Beaman et al (2014)	Mali	198 villages	Random selection of treatment clusters after baseline	Woman who joined a savings and loan association created on the occasion of program	6807	Group credit
Karlan & Zinman (2011)	Philippine	1601 credit applicant individuals	Random selection of individuals in the treatment group based on their credit score	random approving credit applications based on the credit scores of candidates	1601	Individual credit
Karlan & Zinman (2008)	South Africa	58168 clients	Random selection of individual in treatment group	Individuals who have already taken a loan in the past 24 months but has been unpaid for at least 30 days	58168	Individual credit

Note: IGA=Income Generating Activity; MFI= Microfinance Institution

2.2 Methodological challenges

The current major challenges for experimental studies on microcredit impact evaluation are mainly related to statistical power and selection bias. Banerjee et al. (2015b) indicate that the low take-up rate of microcredit by potential borrowers in randomized experiments poses serious problems regarding statistical power. The main determinant of statistical power in microcredit studies is the difference between the uptake rate of credit in the treatment and control groups. A small difference in the credit uptake rates between the two groups is partly due to the low credit interest in the treatment group and, on the other hand, the credit uptake rate in the control group is due to the entry of competitors (Crépon et al., 2015).

Controlling selection bias is a challenge (Banerjee et al., 2015b). On the demand side, selection bias may stem from the likelihood that individuals who choose to borrow are different from those who choose to not. Banerjee et al. (2015b) argue that when these differences are unobservable (therefore not fully controlled by the researcher) and correlated with the outcome, the results of the impact estimates will be biased because they do not capture the true causal effect. Similarly, the selection bias on the supply side may come from the likelihood that the lender makes strategic decisions that are beyond the control of the researcher.

2.3 Causal effect identification

In the literature, three main estimators are used to measure the impact of programs in randomized experiments: the intention to treat (ITT) estimator, the average treatment effect on the treated (ATT), and the local average treatment effect (LATE).

2.3.1 The intention to treat (ITT) estimator: effect on the entire population

Individuals in the treatment group (treated or not) are compared to individuals in the control group to evaluate the effect of the microcredit program. The estimates take into account all the sample observations. Then the intention to treat estimator measures the average treatment effect of microfinance programs on the entire population (Karlan and Zinman, 2011; Beaman et al., 2014; Takahashi et al., 2014; Angelucci et al., 2015; Attanasio et al., 2015; Banerjee et al., 2015; Basu and Wong, 2015; Crépon et al., 2015; Tarozzi et al., 2015).

When the dependent variable is continuous, the ordinary least squares model is used to estimate the impact or a Tobit to correct for censorship. A Probit or Logit model is used when the dependent variable is binary.

2.3.2 *Average treatment effect on the treated (ATT): effect on the beneficiaries of microcredit*

The impact of microcredit on respondents in the treatment group who actually take the credit is measured by the average treatment effect on the treated estimator. Attanasio et al. (2015) suggest that the average treatment effect on the treated can be estimated by dividing the ITT by the likelihood of receiving the treatment. This probability is equal to the proportion of individuals participating in the program if they are offered the program i.e. the *compliers* (Gertler et al., 2011). This population is the group of those who change their behavior due to the intervention and is of great interest in terms of public policies. However, in the case of microcredit programs, the outcome of the ATT estimator cannot be generalized to the entire population because those who receive the treatment may be systematically different from those who do not receive it (Attanasio et al., 2015). Beaman et al.'s (2014) study on Mali also highlighted the issues of estimating the average treatment effect on the treated in the microcredit context, given the heterogeneity of treatment effects with respect to the likelihood of taking the loan. They argue that there is a problem of self-selection in credit programs and that those who choose not to borrow have a return on capital that is significantly lower than those who choose to borrow.

Heckman and Urzua (2009) show that in the presence of self-selection, there is not a single treatment effect but a variety of effects depending on the conditional variables. Therefore, the estimate of the average treatment effect on the treated in this context would be inappropriate. The characteristics of the treated individuals may not be representative of the population because of selection based on unobservable factors. Thus, under these conditions, extrapolation of the ATT to the entire population would imply that all individuals would behave the same way if they had entered the microcredit program, which would exclude the self-selection.

2.3.3 *Local Average Treatment Effect (LATE): effect on the beneficiaries if they are offered the program*

The LATE estimator is an alternative approach to measure the treatment effect on the *compliers*. This estimator is based on a model with an instrumental variable (Imbens and Angrist, 1994). The instrument is correlated with treatment status but not with the outcome variable. The LATE estimator takes into account the fact that individuals respond differently to treatment, contrary to the ATT, which makes the assumption of a homogeneous treatment effect. Imbens and Angrist (1994) distinguish four types of individuals according to their reaction to the instrument. Table 2 shows the characteristics of the four types of individuals in the case of treatment (T_i) and a binary instrument (Z_i) with a monotonicity hypothesis (absence of *defiers*). For Imbens and Angrist (1994), the instrument that satisfies the assumptions of independence to the outcome and correlation to the treatment is an indicator variable for the treatment assignment. In the case of a binary instrument, Z_i takes the value "one" if the individual has been assigned to the treatment and "zero" otherwise, irrespective of whether they were actually treated or not.

Table 2. Type of individual per treatment and instrument status

Treatment (W_i)	Instrument (Z_i)	
	0	1
0	Defiers /Never takers	Never takers
1	Always takers	Compliers /Always takers

The LATE estimator can be estimated by parametric or nonparametric methods. Unlike the ATT estimator, the LATE can be extrapolated to the entire population of *compliers* (Imbens and Angrist, 1994;

Imbens, 2009). More specifically, Imbens (2009) argues that the LATE is the best-unbiased estimate of the average treatment effect on a specific sub-population represented by the *compliers*.

Desai et al. (2013) and Tarozzi et al. (2015) in Ethiopia are examples of microcredit studies that use the LATE estimator for the estimation of the effect of microcredit programs on a specific sub-population represented by the *compliers*.

2.3.4 *Heterogeneous treatment effects*

Individuals may vary not only according to their socioeconomic characteristics but also according to their response to treatment. Thus, the average treatment effect may be zero but positive or negative for a particular group of beneficiaries. In the impact evaluation literature, quintile treatment effect methods have been developed to analyze heterogeneous treatment effects. The most recent examples are from Angelucci et al. (2015) and Crépon et al. (2015) who used the quintile treatment effect to measure the heterogeneity of effects on microcredit beneficiaries. The method consists of a quintile regression of the outcome variable on the treatment variable by adding relevant control variables.

Setting using instrumental variables with heterogeneous treatment effects could also be used to avoid the issue of external validity (e.g. Angrist and Rokkanen, 2015). The instrumental variables estimator is interpreted as an estimator of the local average treatment effect. To identify individual causal effects, the literature also suggests multiple hypothesis testing (List et al., 2016) and subgroup analysis (Athley and Imbens, 2016a).

Table 3 summarizes the different approaches used by the studies found in the literature.

Table 3. Summary of main estimators of the impact of microcredit programs

Impact estimators	Measured impact	Studies example
Intention to Treat (ITT)	Impact of the microcredit program on the entire population	Karlan & Zinman (2011, Phillipine); Takahashi et al. (2014, Bangladesh); Beaman et al. (2014, Mali); Angelucci et al. (2015, Mexico); Attanasio et al. (2015, Mongolia); Augsburg et al. (2015, Bosnia-Herzegovina); Banerjee et al. (2015, India); Basu and Wong (2015, Indonesia); Crépon et al. (2015, Morocco)
Average Treatment Effect on the Treated (ATT)	Impact of the program on the beneficiaries of microcredit	Attanasio et al. (2015, Mongolia); Crépon et al. (2015, Morocco)
Local Average Treatment Effect (LATE)	Impact of the program on the beneficiaries if they are offered the program	Desai et al. (2013, Ethiopia); Tarozzi et al. (2015, Ethiopia)

3 Results of the evaluations of the effects of microcredit

3.1 Adoption of new agricultural technologies and intensification of the use of agricultural inputs

The results of empirical studies converge to a positive impact of access to microcredit on adoption of agricultural technology. Indeed, in Ethiopia, Abate et al. (2015) used the propensity score matching method with household data to examine the impact of microfinance institutions and cooperatives on the adoption of agricultural technologies. Their findings suggest that access to microcredit has a positive and significant impact on both the adoption of new agricultural technologies (e.g., improved seeds) and the intensification of the use of fertilizers and pesticides. Islam et al. (2012) showed that access to microcredit had a positive and significant impact on the adoption of a new rice variety with high yield in Bangladesh. Households that had access to formal microcredit have an adoption rate of inputs that is relatively higher than households who do not have access to microcredit (Tadesse, 2014). When studying the constraints related to the adoption of agricultural inputs in Ethiopia, Croppenstedt et al. (2003) find that credit is the

major supply-side constraint to adoption, suggesting that household financial resources are generally insufficient to cover fertilizer purchases.

Several other empirical studies in different settings confirm the positive relationship between access to microcredit and the adoption of new agricultural technology. Recent examples are Zeller et al. (1998) for Malawi; Isham (2002) for Tanzania; Lapar and Ehui (2004) for the Philippines; Abdulai and Huffman (2005) for Tanzania; He et al. (2007) for China; Dercon and Christensen (2011) for Ethiopia; Girabi and Mwakaje (2013) for Tanzania; Odozi and Omonona (2013) for Nigeria; Lambrecht et al. (2014) for the Republic Democratic of Congo; Tigist (2015) for Ethiopia; and Hazarika et al. (2016) for India.

However, the convergence of the literature on the positive impact of access to microcredit on the adoption of new agricultural technologies could hide serious methodological problems. As shown by Beaman et al. (2014), Attanasio et al. (2015) and Crépon et al. (2015), very often, farmers self-select into participation in microcredit programs. The failure to account for this potential selection bias may result in inconsistent estimates of the impact of access to microcredit on the adoption of new technologies. Very few studies in the literature have made an explicit attempt in this direction, implying that the above-mentioned results could suffer from sample selection bias. The Double Hurdle approach used by Croppenstedt et al. (2003); Hazarika et al. (2016); the Heckman selection probit models in Lambrecht et al. (2014); or the instrumental variable regressions in Tadesse (2014) have the advantage of accommodating for selectivity bias. However, such techniques do not mitigate biases stemming from observed variables that could explain the differences in adoption between beneficiaries of microcredit and non-beneficiaries.

3.2 Agricultural Investments

The clear majority of empirical studies converge on the fact that access to microcredit has a positive effect on smallholder farmers' investment, regardless of the form of credit. Some examples are those of Kaboski and Townsend (2012) for Thai farmers, Banerjee et al. (2015) for India and Crépon et al. (2015) for Morocco. However, the form of the microcredit contract plays a role in the intensity of the impact. Banerjee (2013) indicates that microcredit programs often impose a rigid repayment plan that makes it less useful for farmers whose income tends to be seasonal and unpredictable, especially in the absence of a reliable savings system. Thus, repayment periods must reflect the agricultural enterprise's cash flow (Beaman et al., 2014) and offer grace periods allowing risk-taking so that the farmer has time to adjust for errors (Field et al., 2011).

3.3 Technical efficiency and farm productivity

Some empirical studies have examined the link between access to microcredit and the technical efficiency of farms. The results do not all point in the same direction. Awotide et al. (2015) used a stochastic production frontier model to analyze the impact of credit availability on the technical efficiency of cocoa farmers in Nigeria. The authors found that access to credit is associated with a higher technical efficiency, as farmers who have access to formal credit adopt more efficient production techniques and use their production inputs more effectively to produce closer to their production frontier. Binam et al. (2003) also found that access to credit is one of the major factors explaining the differences in the technical efficiency of farmers in Cameroon. Other studies in various socioeconomic contexts such as Liu and Zhuang (2000) for China; Abdulai and Eberlin (2001) for Nicaragua; Bozoglu and Ceyhan (2007) for Turkey; Girabi and Mwakaje (2013) for Tanzania; and Zhao and Barry (2014) for China confirm the positive relationship between access to formal credit and technical efficiency.

In contrast, Mghenyi (2015) concluded that access to credit had no effect on the technical efficiency of maize farmers in Kenya. The author argued that the credit was assigned to inputs whose use was already efficient. A null effect of microcredit on technical efficiency was obtained by Taylor et al. (1986) in their study in Brazil. Rezitis et al. (2003) indicate that although the credit allows farmers to use modern production inputs more intensely, other types of inputs such as better use of resources, access to information and better management of the farm are needed to improve technical efficiency. This means that access to credit alone is not enough to enhance smallholder farmers' technical efficiency. Table 4 summarizes the results of studies that analyzed the impact of microcredit on technical efficiency and farm productivity.

Tableau 4. Impact of microcredit on farms technical efficiency and productivity

Studies	Countries	Results
Liu & Zguang (2000)	China	Positive impact of microcredit on technical efficiency
Abdulai & Eberlin (2001)	Nicaragua	Positive impact of microcredit on technical efficiency
Diagne & Zeller (2001)	Malawi	Access to credit helps poor households engage in productive agricultural and non-agricultural Activities
Binam et al. (2004)	Cameroun	Positive impact of microcredit on technical efficiency
Bozoglu & Ceyhan (2007)	Turcky	Positive impact of microcredit on technical efficiency
Girabi & Mwakaje (2013)	Tanzania	Credit recipients had a relatively higher agricultural productivity than non-beneficiaries
Zhao & Barry (2014)	China	Positive impact of microcredit on technical efficiency
Awotide et al. (2015)	Nigeria	Access to credit is associated with a higher score of technical efficiency
Mghenyi (2015)	Kenya	Access to credit has no effect on the technical efficiency of maize producers
Taylor et al. (1986)	Brazil	The microcredit program has no effect on the technical efficiency of producers

Altogether, there is no clear evidence of the impact of access to microcredit on farmers' technical efficiency. Moreover, the results of some of the studies above must be interpreted with caution because they may suffer from selection bias or simultaneity bias. In Liu and Zhuang (2000), the liquidity constraint was used as a proxy for access to credit. The liquidity variable is the amount of money the farmer has in his bank account at the beginning of the season plus formal and informal loans standardized per hectare of land to control for farm size effect. Such a measure may suffer from simultaneity bias as acknowledged by the author. Households make resource allocations at the beginning of the season, which affects their productivity, which in turn has an impact on the liquidity available at the beginning of the following season. The authors have certainly tried to address this issue using the predicted value of the liquidity in their econometric model, but this is not sufficient to estimate the marginal effect of credit access. It is difficult to separate households with liquidity constraints from those who do not have such constraints.

In addition, some of the empirical studies above focus on small samples that do not provide sufficient statistical power for the results. For example, the sample size in Abdulai and Eberlin's (2001) study was only 120 households. Furthermore, none of the studies that found positive impacts of access to credit on smallholder farmers' technical efficiency made an explicit attempt to correct for self-selection bias. When there is self-selection, as is the case for microcredit programs, the estimated stochastic production frontier parameter and associated technical efficiency scores are likely to be biased (Greene, 2010). Hence, we speculate that the positive impact found could be because farmers who choose to take up the credit are those who are the most productive.

3.4 Agricultural incomes and profits

Some authors have studied the impact of access to microcredit on farm income or profits and some others on the two variables at the same time.

3.4.1 *Farm income*

The impact of microcredit on farm income is not clear enough. Using individual level randomization, Kaboski and Townsend (2012) found that access to credit significantly increases the incomes of Thai farmers. However, under the method of randomization used, the measured effect could be biased because individual level randomization does not internalize spillover or general equilibrium effects (see Banerjee et al., 2015a).

Furthermore, Giné and Mansuri (2011) and Desai et al. (2013) also found a null effect of microcredit on farm income. Studies from Crépon et al (2015) confirm that credit allows beneficiaries to increase their investment but found that it has no effect on overall income. Giné and Mansuri (2011) and Augsburg et al. (2015) do not explicitly correct for selectivity bias in their experimental design. In the latter, the null effect obtained could suffer from a lack of precision if it was converted into ATT.

3.4.2 *Profits of agricultural enterprises*

Mghenyi (2015) found that corn farmers' access to credit significantly increased agricultural profit through increased use of fertilizers and hired labor. Banerjee et al. (2015) also found identical effects of microcredit. The authors implemented a randomized experiment targeting women aged 18-59 years organized in groups. However, the magnitude of the impact was limited. Banerjee (2013) argues that this is because micro-entrepreneurs have no credit constraint at the interest rate offered by microfinance institutions, and therefore, the impact of microcredit on their profits is limited. In Sudan, Ibrahim and Bauer (2013) analyzed the impact of microcredit on farm profits. They showed that the limited effect of

credit on farm profits is due to the small amount of credit offered, which is not sufficient to induce a real change in agricultural production.

Angelucci et al. (2015), through their randomized experiment in Mexico, showed that the impact of microcredit on income and profit is heterogeneous. They argue that microcredit has no effect on the income and profit of individuals in the first 95 percentiles, while it does on those in the last 5 percentiles. The null effect of microcredit on farm income found by Giné and Mansuri (2011), Deseai et al. (2013), Augsburg et al. (2015) and Crépon et al. (2015) could be because these authors did not consider the heterogeneity of the impact of microcredit. Table 5 summarizes the results of studies that analyzed the impact of microcredit on profit and farm incomes.

From all of the above, we see that in the literature, the impact of microcredit on investment and farm income is limited and controversial. The results of experimental and non-experimental studies also indicate that the effects of microcredit are likely to vary from one place to another and depend partly on the parameters and program design (Islam, 2015). Controlling for sample selection bias is a great challenge to empirical studies.

Tableau 5. Impact of microcredit on profit and farm incomes

Studies	Countries	Results
<i>Panel A: Farm income</i>		
Giné & Mansuri (2011)	Pakistan	No significant effect on farm income
Kaboski & Townsend (2012)	Thailand	Significant increase in revenue
Desai et al. (2013)	Ethiopia	No significant effect on farm income
Angelucci et al. (2015)	Mexico	On average, no significant effect on household income but the effect is positive and significant on the income of households that are in the last 5 percentiles
Attanasio et al. (2015)	Mongolia	No significant effect on household income
Augsburg et al. (2015)	Bosnia-herzegovina	No significant effect on household income
Crépon et al. (2015)	Morocco	No significant effect on household income
<i>Panel B : Profit</i>		
McKernan (2002)	Bengladesh	Microcredit has a positive impact on profit of non-farm business
Karlan & Zinman (2009)	Philippine	Access to microcredit significantly increases the benefit of businesses run by men hat has no effect on the female's
Ibrahim & Bauer (2013)	Sudan	Limited effect of credit on farm profit due to the fact that the smallest of amount of credit is not sufficient to induce a real change in agricultural production.
Crépon et al. (2015)	Morocco	Increased profits of households that ex-ante have high propensity to borrow because of a significant increase in investment in assets used in income generating activities.
Banerjee et al. (2015)	India	Significant increase in agricultural profit
Mghenyi (2015)	Kenya	Significant increase in agricultural profit through intensification of the use of fertilizer and hired labor

The specific case of non-farm business profit

Most empirical studies tend to find a positive impact of microcredit on non-farm business profit. However, the chain of causality is not yet well understood. Indeed, Karlan and Zinman (2009) conducted a randomized experiment in the Philippines and found that access to microcredit significantly increased the benefits to businesses run by men but had no effect on women's. McKernan (2002) used household data on participants and non-participants in the microcredit programs of Grameen Bank in Bangladesh to assess the impact of microcredit and non-credit-related services provided by microcredit programs. The author found that microcredit and non-credit-related services had a positive impact on profits of non-farm businesses. Crépon et al. (2015) showed that among households that ex-ante have a high propensity to borrow, access to microcredit led to an increase in profits due to a significant increase in investment in assets used in income-generating activities.

3.5 Some other dimensions

3.5.1 Microcredit and consumption

Household consumption (or consumption expenditure) is widely used as a proxy of living standards and is of important interest for poverty alleviation policy. The results from empirical studies on the link between microcredit and consumption are mixed at best (see Table 6). For example, Banerjee (2013) showed that microcredit promoting sustained access to credit has a positive effect on household consumption through its effect on savings. Several other studies have found a positive impact of access to microcredit on food consumption (Rahman, 2010; Attanasio et al., 2015). In contrast, Islam (2015) showed that the effect of microcredit on consumption is heterogeneous and that the poorest households are those who benefit the most. The author also found that the effects are lower for households that are

at the margin of the participation decision. Finally, the effects are generally stronger for female than for male.

Other studies have led to conclusions contrary to those of the studies above. For example, Crépon et al. (2015) and Banerjee et al. (2015) showed that access to microcredit had no statistically significant effect on household consumption. Giné and Mansuri (2011), in their experience in Pakistan, also concluded that microcredit had no significant effect on consumption. Banerjee et al. (2015b) reported the results of several empirical studies that corroborated the lack of effect of access to microcredit on consumption. Banerjee et al. (2015a) argue that the composition of the consumption bundle provides more insight on the impact. The authors reviewed six experimental studies and found that with regard to food consumption, four found null effects; one found evidence of a modest increase, and one other found evidence of a substantial decrease, meaning that there was an increase in food insecurity. With regard to non-food consumption, Banerjee et al. (2015a) found that microcredit decreased discretionary spending such as temptation goods, recreation, entertainment, and celebrations. These results are summarized in Table 6.

Tableau 6. Impact of microcredit on consumption

Studies	Countries	Results
Giné & Mansuri (2011)	Pakistan	No significant effect on consumption
Kaboski & Townsend (2012)	Thaïland	Significant increase in consumption
Attanasio et al (2015)	Mongolia	Group of credit has a positive impact on business creation and household food consumption but individual credit has no effect
Attanasio et al (2015)	Mongolia	Group of credit has an impact positive effect on entrepreneurship among women and household food consumption
Augsburg et al (2015)	Bosnia- herzegovinia	Reduces consumption and savings
Banerjee et al (2015)	India	No significant effect on consumption
Crépon et al (2015)	Morocco	No significant effect on consumption

3.5.2 *Microcredit and labor supply*

In the literature, there is no clear evidence of the impact of microcredit on the labor supply. Angelucci et al. (2015) conducted a randomized experiment in Mexico focused on women aged 18 to 60 who had a business/self-employment activity or intended to create one. The authors found that access to microcredit had no statistically significant effect on labor supply. In contrast, for Augsburg et al. (2015), access to microcredit had a positive effect on youth household labor supply and reduced the supply of paid labor among the poor. Karlan and Zinman (2009) also showed that access to credit had a negative effect on family labor supply through the labor substitution effect with the education it had generated. They explain that the credit helped to increase the profit of businesses and that households used this increase in profit to send children to school. Crépon et al. (2015), in their study in Morocco, also reached similar conclusions. These results are summarized in Table 7. The limited analysis of heterogeneous treatment effects in the literature does not provide evidence on what would be the effect on each segment of

borrowers. The wealthiest farmers may not face the same labor constraint as the poorest in order to observe the substitution effect between education and youth labor reported by Karlan and Zinman (2009) and Crépon et al. (2015).

Tableau 7. Impact of microcredit on labor supply

Studies	Countries	Results
Karlan & Zinman (2009)	Philippine	Reduction of youth labor supply via substitution effect of work to education
Angelucci et al (2015)	Mexico	No significant effect on labor supply
Attanasio et al (2015)	Mongolia	No significant effect on labor supply
Augsburg et al (2015)	Bosnia- Herzegovina	Boost the work of young (16-19 years) in the household business and reduces the supply of wage labor of poor's
Crépon et al (2015)	Morocco	Reduced supply of labor especially among young people (16-20 years) and elderly (51-65 years)

4 Summary and conclusions

Several studies have been conducted in the context of developing economies to measure the impact of microcredit on many variables of interest to rural households: investment, technology adoption, income, technical efficiency, etc. The quantitative approaches to identify the causal effects of microcredit are grouped into quasi-experimental methods and experimental methods. However, empirically, quasi-experimental methods do not always clearly identify the causal effects of microcredit. The alternative is to retrospectively have a group of non-beneficiary farmers with similar characteristics to the group of beneficiary farmers, to serve as a counterfactual in the development of experimental studies. One of the

best methods for constructing the counterfactual is random assignment, which can be at the farmer level (individually) or at the group level. In the literature, three main estimators are used to measure the impact of programs in randomized experiments. These are the "intention to treat" estimator, the average treatment effect on the treated and the local average treatment effect.

The effects of microcredit have been the subject of several studies over the past fifteen years. However, the debate on this issue remains relevant as the results of various studies reported in the literature are mixed.

The results of empirical studies converge to a positive impact of access to microcredit on adoption of agricultural technology and investment. In terms of the effects on farms' technical efficiency, the results do not all point in the same direction. Some authors found that access to credit is associated with a higher score of technical efficiency, as the farmers who have access to formal credit adopt more efficient production techniques and use their production inputs more efficiently. Other authors found that access to credit had no effect on technical efficiency because credit does not affect productive inputs that allow technical efficiency gains. Furthermore, none of the studies that found a positive impact of access to credit on smallholder farmers' technical efficiency made an explicit attempt to correct for self-selection bias. The impact of microcredit on income and agricultural profit is also not clear enough. The results are mixed. One reason lies in the fact that access to credit is not the main constraint faced by farmers. Finally, we note that the results of empirical studies on the link between microcredit and consumption are heterogeneous. The positive impact on consumption is mostly observed in the poorest and with female household borrowers.

From all the above, we note that the results of experimental and non-experimental studies indicate that the effects of microcredit are likely to vary from one place to another and depend partly on the parameters

and program design and methodological approaches used. We conclude that microcredit programs have to be tailored to reflect particular conditions of individual locales. Future research should make an explicit attempt to control for selectivity bias in both microcredit program design and evaluation techniques.

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Appendix A – Quasi-experimental methods

The quasi-experimental methods include the *before-after* method, which compares the outcome of treated individuals after treatment to it before the treatment. It is assumed that only the program is likely to change the outcome. This assumption is source of bias because many factors can affect the outcome of the beneficiaries even in the absence of access to microcredit. Thus, the causal effect cannot be established by a simple before-after comparison.

The *difference-in-differences* method or *double difference* (See for example Deininger et al., 2011; Hounghbedji, 2015) measures the outcome before and after the program to calculate the differences between individuals in the treatment group and those in the control group. The method consists in the double difference between the outcome of the treatment group and the control group before and after the program being evaluated. However, the double difference method is based on the assumption that in the absence of access to microcredit, producers in the treatment group and the control group could have the same evolution over time. This is a strong assumption and if violated, double difference estimators would be biased.

With the *instrumental variables* method (e.g. Goldstein and Udry, 2008; Ali et al., 2011; De Brauw and Mueller, 2012; Bellemare, 2013; Jin and Jayne, 2013) program participation is predicted by a random factor or instrumental variable that is not correlated with the outcome variable while predicting participation (which affects the outcome variable). The control group is composed of individuals who, because of this random factor are predicted to not participate and (possibly as a result) do not participate in the program. The advantage of this method is that it allows controlling for selection bias on unobservables (omitted variable bias, simultaneity bias and bias due to measurement errors). However, instrumental variables method leads to biased estimators for small samples (Greene, 2003) or when the

instrument is not exogenous (Greene, 2003; Cameron and Trivedi, 2005). Finding exogenous instruments is difficult when analyzing access to microcredit and its impact. Table A1 presents some of instruments reported in the literature.

Table A1. Example of instruments reported in the literature

Authors	Country	Instruments
Akotey and Adjasi (2016)	Ghana	Professional identity card and the National Health Insurance Scheme's identity card
Cuong (2008)	Vietnam	The commune poverty rate of commune authorities; Distance from a village where households are located to the nearest bank
Godquin (2004)	Bangladesh	Value of the previous loan (instrument for the size of the loan)
Khandker and Faruqee (2003)	Pakistan	Education and landholding
Mazumder (2015)	Bangladesh	Household income, wage value, household net worth, non-land assets, savings amount, moderate poverty and extreme poverty
Pitt and Khandker (1998)	Bangladesh	Landholding
Quayes (2015)	87 countries	Cost of loan per borrower
Shoji (2010)	Bangladesh	Distance from member's residence to the place holding MFI member meetings and its quadratic term

The *regression discontinuity* method (RD) is used when there is a threshold (cut-off) for participation in a program. Individuals are ranked in a scale closely linked to the likelihood of being treated. The basic assumption is that after controlling for the eligibility criteria, the remaining differences between individuals directly below and above the cut-off score are not statistically significant and will not bias the results. The average treatment effect is measured at the point of discontinuity. However, with RD, there is a problem of external validity since it uses only individuals who are close to the cut-off score.

The average treatment effect may be different for individuals who are far from the cut-off. Thus, the results cannot be generalized to the entire population.

The *matching* method uses statistical methods to identify non-participants who have the same observable characteristics with participants to serve as counterfactual (e.g. Petracco and Pender, 2009; Valente, 2009; Gerezihar and Tilahun, 2014; Ghebru and holden, 2015; Melesse and Bulte, 2015; Garcia et al., 2015). In other words, program participants are matched with non-participants who are a priori similar. The difference between the two groups is interpreted as the impact of the program. The propensity score matching method corrects for the selection bias from observable characteristics. However, bias from unobservable characteristics may persist. Moreover, some authors use a combination of difference-in-differences and propensity score matching method (See Bezabih et al., 2011 on Ethiopia; Moura and Bueno, 2014 on Brazil; Peralta, 2015 on Nicaragua; and Mandola and Simtowe, 2015 on Malawi). The combination of the two methods help neutralizing any residual bias due to unobserved variables that are constant over time between treatment and control groups which are not controlled by the propensity score matching.

The alternative to the difficulties mentioned above is to have a group of non-beneficiaries with similar characteristics to the group of beneficiaries to serve as counterfactual. This alternative has led to the development of experimental studies (Duflo et al., 2007). However as mentioned by Shaffer (2011), the choice of approach to impact assessment should be driven by the research question at hand.