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REVUE INTERNATIONALE DES SCIENCES ET TECHNOLOGIES DE L'EDUCATION

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DOES MICROFINANCE REDUCE POVERTY ? EVIDENCE FROM CÔTE D'IVOIRE

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Abstract

In the past few decades microfinance has been considered as the best way to improve the financial inclusion of the poor, thereby alleviating poverty in developing countries. But in recent years the crisis that happened in the sector highlighted the limits and the disastrous consequences of the MFIs on clients. Using matching methods, this paper analyzes the impact of microfinance on household expenditures in Côte d'Ivoire. The results show a positive impact on household expenditure. A household with access to microfinance services increases its expenditure between 2 and 6 % more than a household without access.

JEL Classification : G210, G230, O150, O160, O170

Keywords : Average Treatment Effect, Microfinance, Poverty.

Résumé :

Au cours des dernières décennies, la microfinance a été considérée comme un instrument efficace de d'inclusion financière des pauvres, qui permet d'atténuer la pauvreté dans les pays en développement. Mais ces dernières années, la crise qui s'est produite dans le secteur a mis en évidence les limites et les conséquences désastreuses des institutions de microfinance sur les clients. À l'aide de méthodes d'appariement, cet article analyse l'impact de la microfinance sur les dépenses des ménages en Côte d'Ivoire. Nos résultats suggèrent un impact positif sur les dépenses des ménages. Un ménage ayant accès aux services de microfinance augmente ses dépenses entre 2 et 6 % de plus qu'un ménage n'ayant pas accès.

Mots clés : Microfinance, pauvreté, effet moyen du traitement.

JEL classification : G210, G230, O150, O160, O170

1. INTRODUCTION

Does microfinance really reduce poverty? Did it really reduce poverty in the world during the last two decades? That is the current debate between those who advocate microfinance as the best tool of poverty reduction over the world and those who are skeptical about its real impact in improving the well-being of its beneficiaries. Among the defenders, Mohamed Yunus, Nobel Peace Prize in 2006, an emblematic figure continues to rent the importance and ability of microfinance to alleviate poverty. Microcredit Summit Campaigns held annually to reflect this emphasis on microfinance by the Bretton Woods institutions and non-governmental organizations. Moreover, microfinance sector figures prominently in the achievement of the millennium goals in particular for fighting against poverty.

Other recent years, the real contribution of microfinance in poverty reduction is questionable. The crisis in the sector towards the end of 2011 marked by the collapse of some institutions, the suicide of indebted customers showed the setbacks and limitations of microfinance. The increasingly neoliberal orientation of the sector proved by the quotation at the stock exchange of Swayam Krishi Sangam (SKS), the largest MFI in India has attracted much questioning about the real objective of microfinance institutions. Competition and liberalization of interest rates in the industry promoted by some organizations led microfinance in the peril of neo-liberalism and the merchants of illusion (Fouillet et al, 2007).

The idea that the poor have entrepreneurial ability, then the access to microcredit would create productive activities or diversify existing activities and generate income growth remains weak in practice. The contribution of microfinance to fight against poverty remains highly mitigated. However, many studies recognize its contribution to improving family budget to deal with lean periods, unexpected events, improving the health of the household, education of children, etc. (Pitt and Khandker, 1998; Dunford, 2006; Collins *et al.*, 2009). All this shows the importance of assessing the impact of microfinance. The purpose of this paper is to assess the effects of microcredit in Côte d'Ivoire.

2. Impact of microfinance

Microfinance advocates claim that access to microcredit help to substantially reduce poverty, increase income-generating activities and possible diversification of income sources. Therefore it contributes to smooth consumption, reduces family vulnerabilities due to illness, drought and crop failures in developing countries. In addition, access to microcredit enhances women's empowerment through which education and family health are improved. It has been shown that they often use a substantial part of their income for the health and education of their children (Pitt and Khandker, 1998). Therefore, the positive results of an assessment of microfinance on reducing poverty have convinced many governments, NGOs and individuals to support MFI activities (Hermes and Lensink, 2011). But the doubt that raises this opposition suggests that assessments of the impact of microfinance are necessary to assess their effects on customers. In that case, several assessment studies on the impact of microfinance on poverty reduction have been made. But there is little solid and rigorous academic studies on this issue (Westover, 2008). This lack of highly scientific studies can be put down to the difficulty of measuring the impact of these programs and the significant costs they entail.

For Karlan and Goldberg (2007) a rigorous impact evaluation has to answer "How are the lives of the participants different relative to how they would have been had the program, product, service, or policy not been implemented?" this requires to compare two potential outcomes such as income in our case, one with treatment and the other without. The fact that we can't observe simultaneously both statuses to an individual, what can be done is to compare before and after the access of households to microcredit programs. But this method is not suitable. Other factors such as macroeconomic shocks can affect the post-treatment and it is impossible to isolate microfinance impact from time trend.

It's almost impossible to measure the impact of a program on a given individual (Duflo et al, 2006), but we can obtain the average impact of microfinance (Kono and Takahashi, 2010). That is possible if the counterfactual outcome of the treatment group is constructed from the pool of the remaining population who has a similar outcome to the treatment group in the absence of the treatment. Therefore the major challenge for impact evaluation is to create a good counterfactual through the use of appropriate techniques under a set of acceptable assumptions (Kono and Takahashi, 2010).

Non-randomized approaches are generally used to evaluate the impact of microfinance. But these approaches may be problematic due to the potential selection bias. For instance, it is known that rich agents are relatively less risky than poor agents. Therefore, if participation is voluntary, maybe agents who decided to participate are rich agents whereas poor agents do not participate. In this case, there may be a self-selection bias. Another bias can be due to non-random program placement. MFIs may decide to develop their activities in a relatively wealthy region in order to improve the success of microfinance. In this case, there is a bias between microfinance clients and the control group (Aghion and Morduch, 2005). Evidence of non-randomized studies of microfinance is mixed. The study of Pitt and Khandker (1998) is one of the most important studies, which evaluates the impact of microfinance in Bangladesh. In order to overcome bias problems, they use an eligibility criterion applied by MFIs, which is to target households with less than half an acre of land. This criterion is exogenous and permits them to identify households just above and below this threshold. They assume that these two groups are similar except for exogenously determined eligibility to microcredit. Then they use a complex econometric technique called weighted exogenous sampling maximum likelihood-limited information maximum likelihood-fixed effects to obtain the marginal impact of microfinance. Among other results, they find that access to microfinance smoothes consumption, increases consumption expenditures, especially for women. In order to conduct a robustness check of their previous study, Khandker (2005) uses panel data by combining data collected in 1991/92 and 1999. The results confirm that microfinance benefits the poorest and has a sustained impact on poverty reduction among program participants. The extremely poor benefit more to microfinance than moderately. But the results of these have received criticisms as those of Morduch (1998; 1999) and recently those of Roodman and Morduch (2009). Morduch (1999) argues that the eligibility criterion was not respected by IMFs in practice. However, in Bangladesh land markets are active and many people purchase or sell their land, all this creates a violation of eligibility criterion. Instead of the intricate method of Pitt and Khandker (1998), he uses a difference in difference (DID) approach and finds little evidence of positive impacts of microfinance except for consumption smoothing.

In response to Morduch critics, Pitt (1999)[1] noted that his methods fail to deal with program placement problems. The estimate of Morduch will be biased if Grameen Bank focuses on an area where inequality between rich and poor is greatest.

The results of these studies have been recently contested by Roodman and Morduch (2009). They revisit evidence obtained by each study. Their replication exercises and specification tests show that the impacts these studies are weak with their reconstructed data. However, they show that the methods may have failed and the results may be driven by omitted variables and or reverse causation problems (Hermes and Lensink, 2011). These examples show the difficulty inherent in determining the causal relationship between access to microfinance and the improvement recipient's

[1] Cited by Chemin (2008).

welfare with non-experimental approaches. The results of these must be taken with caution.

To overcome methodological problems due to non-randomized evaluations, recently some studies of microfinance impact have been conducted with randomized approaches. This approach used random control trials, meaning that individuals are randomly distributed in the treatment and control group. The two groups are exactly similar along all relevant dimensions, except that provided from the treatment.

3. Data and econometric model

3.1. Survey Design and Data

The data we used comes from the Households Living survey (ENV-2008), carried out by the National Institute of Statistics (INS) in 2008. Similar surveys were conducted in 1995, 1998 and 2001. These surveys were used to analyze the evolution of household living conditions in order to implement national policies to fight against poverty and reduce inequalities in the country. The survey focuses on various types of information on socio-economic characteristics of people living throughout the Ivorian territory such as education level, health, housing, food, consumption of water, electricity, fuel, employment, expenditures, income, etc. But accounts of the poverty of these surveys in issues related to household access finance institutions and the small size of the sample when some issues exist obliged us to use only the recent data.

Despite the relative's low questions about access to financial institutions, it contains a large sample compared to previous surveys. The database contains 12,600 households. To determine the households of the survey, two types of random draws were carried out. The first random selection was a selection of 630 clusters within 19 administrative regions determined by the general census of population and housing (RGPH) realized in 1998. Selection is done in the proportion of households in each region and according to the environment (urban or rural). The second selection was a systematic sampling of 20 households in each of the previously selected clusters. These households were then counted in proportion to the size of the household. The selection of households was done by the method of "drawing steps" and the survey was conducted at the household home. The respondent was the head of the household. When the head was absent, the interviewer chooses another member of the family who is able to give all information on other members.

We use the Household Survey (ENV) of 2008 for the analyses. This database was used for the implementation of programs against poverty. It has been used as a compass of eligibility of the country on a program of Heavily Indebted Poor Countries (HIPC) and for the implementation of the national development program (PND). However, the existence of biases due to the exceptional period of crises in which the survey was conducted requires us to take into account this aspect of the interpretation of results coming from this database.

Questions about access to credit and microfinance, in particular, are available in section F of the database. Also, this section covers the bulk of the household's expenditure. Only four questions about the demand for credit are available and concerning access or not to credit, getting or not credit and credit institutions from which they seek and obtain loans. Using the answers to these questions we are able to build access to credit into four funding sources by grouping items. Therefore, we distinguish individuals who access to formal banks, MFIs, informal loans and other forms of credit undefined by the survey.

3.2. Propensity score model

We study the effect of microfinance on poverty reduction. Thereby the major purpose of this study is to estimate the mean impact of microcredit on households that have access to microfinance programs. To highlight this impact we define two kinds of an individual: the ones who have access to MFIs and therefore receive credit, and the ones who do not have access. We denote $Y_{1i}$the outcome of interest of the household i (where $i = 1, \dots, N$) which access microfinance program, and Y_{0i}, the outcome of the same household without access. D is a treatment indicator equal to 1 if the household has access to credit in a microfinance program and 0 otherwise. The treatment effect can be written :

$$\Delta_i = Y_{1i} - Y_{0i} \tag{1}$$

This represents the difference of interest between a household with access to microcredit and a household without access. The fundamental difficulty is that we can observe only one outcome of the same household. It means that it is impossible to observe simultaneously the outcome of a household with access to microcredit and its non-access $E(y_{0i}|D_i = 1)$. This estimated treatment effect is a problem of missing data. One idea can be to use the mean outcome of a household without access to the same area as a counterfactual. But using non-access household as the counterfactual is not advisable because both households differed even in the absence of the treatment. This is known as the selection bias problem. To overcome this problem, the matching approach is used in literature. The basic idea is to create a counterfactual from the pool of non-participant households that are similar to participants in a set of pretreatments characteristics X defined by the evaluator. In this way, the outcome difference between the access household the households with no access can be attributed to the neighborhood effect.

To obtain an unbiased estimate of the average treatment effect, two important assumptions called the "strong ignorability" are necessary: the unconfoundedness assumption and the overlap assumption. The unconfoundedness assumption also called the conditional independence assumption Lechner (1999) assumes that given a set of covariates X similar for both treatment and control group, potential outcomes are attributable to treatment. This implies that selection is based on observables characteristics and all variables that affect treatment assignment, as well as potential outcomes, are observed by researchers (Caliendo and Kopeinig, 2008).

In the literature, there are two ways of estimating treatment effect: average treatment effect of population ATE which determines the difference between the expected outcomes after participation and non-participation. The second parameter is the average treatment effect on treated ATT. It concerns the effects on participants who actually receive the treatment. Our study focuses on the effects of those who access microcredit programs or participants who really access to programs. Therefore evaluation based on the treatment effect on the treated is for us the most prominent parameter. The ATT is expressed as :

$$\begin{aligned} \Delta^{\mathrm{ATT}} &= \mathrm{E}(\mathrm{Y}_1 - \mathrm{Y}_0)|\mathrm{D} = 1) \\ &= \mathrm{E}(\mathrm{Y}_1|\mathrm{D} = 1) - (\mathrm{Y}_0|\mathrm{D} = 1) \end{aligned} \quad (2)$$

We need only to evaluate the average effect of the treated, in that case, the weaken unconfoundedness assumption can be used because of the moments of the distribution of Y_1 are directly estimable (Heckman et al., 1997; Caliendo and Kopeinig, 2008). Given that Y_0 are outcomes of those who have no access to microcredit and X denotes all household characteristics. The unconfoundedness for controls is :

$$Y_0 \coprod D | X \quad (3)$$

Where $\coprod$ denotes independence, i.e. given a set of observable covariates X which are not affected by treatment, potential outcomes are independent of treatment assignment. This implies that all variables that influence treatment assignment and potential outcomes simultaneously have to be observed by the researcher (Caliendo and Kopeinig, 2008).

Therefore given a set of observable covariates X, potential outcomes Y_0 are independent on the treatments assignment, D. Similarly the weaker overlap assumption can be written:

$$P(D = 1|X) < 1 \quad (4)$$

This means that given a set of observable covariates X the probability of a household having access to microcredit is less than 1.

One of the practical shortcomings of this method is that if X is high-dimensional, and the number of characteristics in the match increases, it is quite difficult to find a good comparison group similar or sufficiently close to the treatment group in all dimension of X. For instance, in the case, X contains s covariates which are all dichotomous we will have for matches possibilities 2^s. To overcome the problem of dimension, Rubin and Rosenbaum (1983) show that matching on a single index that captures the propensity to participate conditionally on X gives consistent estimates of the treatment in the same manner as matching on all elements of X. In other words, it means that if potential outcomes are independent of treatment conditional on covariates X, it is also the same when using a balancing score $b(X)$. The propensity score is $P(D = 1|X) = P(X)$, i.e. the probability of a household to have access to the treatment given his observed covariates X is one possible balancing score (Caliendo and Kopeinig, 2008).

In practice, two hypotheses are needed to derive one dimensional variable $P(X)$. First, given the propensity score $P(X)$, Balancing of pretreatment variables can be written :

$$D \perp X|p(X) \tag{5}$$

Corollary assignment to the treatment is unconfounded given the propensity score :

$$Y_1,\ Y_0 \sqcup D|P(X)$$

Given the previous assumptions i.e. unconfoundedness holds and there is overlap between the group, the propensity score estimator matching strategy for ATT can be written as follow:

$$\Delta_{ATT}^{PSM} = E_{P(X)|D=1}\{E[Y_1|D = 1, P(X)] - E[Y_0|D = 0, P(X)]\} \tag{6}$$

It defines the mean difference in outcomes over the common support, appropriately weighted score distribution of participants (Caliendo and Kopeinig, 2008).

When the balancing properties are satisfied any standard probability can be used to estimate the propensity score. The first step uses analysis of probit model and propensity score method developed by Becker and Ichino (2002).

Their methods combine estimation of propensity score and test of balancing hypothesis following in several steps[2]

In the first steps, the probit model fitted is estimated as follow :

$$\Pr(D_i = 1|X_i) = \Phi\{h(X_i)\} \quad (7)$$

Where Φ denote the normal cumulative distribution function and $h(X_i)$ is the starting specification that includes all the covariates as linear terms without interactions or higher-order terms. The second step consists in splitting into k equally spaced intervals of the propensity score. Then, within each interval, a test is done to see if the average propensity score of the treated and the control group does not differ. If the test fails in one interval, this interval is split in half and tested again. This exercise is repeated in all intervals until the average propensity score of the treated and control unit does not differ. Therefore, the balancing hypothesis is satisfied. Because the quality of the matches used to estimate ATT is improved in the common support, we imposed the common support condition. But for Lechner and Pfeiffer (2001) this restriction is not necessarily better because high-quality matches may be lost at the boundaries of common support and it may considerably reduce the sample. The balancing property is determined only on the observations of whose propensity score belongs to the intersection.

4. Implementation, results, and discussion

4.1. Model specification

To implement the previous models we use different variables as the good specification of access to credit in general and to microcredit in particular.

[2] See Becker and Ichino (2002) for more details.

Variables are used according to the literature on access finance sources and also depend on availability variables in the database.

Therefore, we use socio-economic characteristics which included the age of the household head, the household size, the marital status of the household members who receive microcredit, the level of education (primary, secondary and higher level). In addition, we use other variables such as religion, whether household lives in urban or in a rural area, the employment status performed by the beneficiary of microcredit in the household (public sector, private sector, informal sector).

In our initial analysis, we consider only the household members who have access to microcredit and those who do not have access to any form of credit whether formal or informal. In this case, we use a probit model under common support to estimate propensity score with the *pscore Stata command* method describe by Becker and Ichino (2002)

As balancing property satisfied all estimation of the propensity score, we can now estimate the average treatment of the treated (ATT) of poverty effect for the subsample of those who access to microcredit compared to non-access households to any finance sources. We appreciate the effect on the poverty of households on access to microcredit programs. Nearest Neighbor estimator (NN) is currently the most used in the studies. This method consists in matching each treated unit and searching for the control unit that is closest in terms of the propensity score. Becker and Ichino (2002) define this estimator. Let Y_i^T and Y_j^c be respectively the observed outcomes of treated and control units. Where T is a set of treated units and C a set of control units. The set of control units matched to the treated unit i with propensity score p_i is denoted $C(i)$ call the nearest-neighbor matching sets given by :

$$C(i) = \min_j \parallel p_i - p_j \parallel \tag{8}$$

That represents a singleton set unless there are multiple nearest neighbors. Denote the number of controls matched with observation $i \in T$ by N_i^C and define the weights $w_{ij} = \frac{1}{N_i^C}$ if $j \in C(i)$ and $w_{ij} = 0$ otherwise. The matching estimator can be formulated as follows :

$$\tau^M = \frac{1}{N^T} \sum_{i \in T} Y_i^T - \frac{1}{N^T} \sum_{j \in C} w_j Y_j^C$$

Where M stands for either nearest-neighbor matching or radius matching, and the number of units in the treated group is denoted by N^T, and the weights w_j are defined by $w_j = \sum_i w_{ij}$.

In addition to the nearest neighbor matching algorithm, other matching methods exist. We use two other methods: Kernel matching and stratification method. Kernel matching is a nonparametric matching estimator that uses weighted averages of all individuals in the control group to construct the counterfactual outcome depending on the choice of the Kernel function (Caliendo and Kopeinig, 2008). Kernel matching estimator is given by Becker and Ichino (2002) :

$$\tau^K = \frac{1}{N^T} \sum_{i \in T} \{ Y_i^T - \frac{\sum_{j \in C} Y_j^C G(\frac{p_j - p_i}{h_n})}{\sum_{k \in C} G(\frac{p_k - p_i}{h_n})}$$

Where $G(\cdot)$ is the Kernel function and h_n is a bandwidth parameter. A smaller bandwidth imposes the assumption of Common Support while Kernel matching converges to the nearest neighbor with decreasing bandwidths

(Chemin, 2008). Therefore, as Chemin (2008) we used bandwidths 0.05, 0.02, and 0.02.

One advantage of this method is that the variance is lower because more information is used to construct the counterfactual outcome. What seems important in this method is the choice of bandwidth parameter. This choice is a compromise between small variance and unbiased estimate of the true density function. We used Kernel Gaussian the default method in Becker and Ichino (2002) program or the Epanechnikov Kernel.

For the stratification matching method, we used the same stratification procedure used in the propensity score method, and then the common support is to split into sets of intervals. Therefore, the impact is calculated within each interval based on the mean difference in outcomes between treated and untreated observations. In a program created by Becker and Ichino (2002), the covariates are balanced and the assignment to treatment is considered random in each block defined by propensity score. Hence, denote q be the index block define by propensity score, matching estimator within each block is written :

$$\tau_q^S = \frac{\sum_{i \in I(q)} Y_i^T}{N_q^T} - \frac{\sum_{j \in I(q)} Y_j^C}{N_q^C}$$

Where $I(q)$ is the set of units in block q and N_q^T and N_q^C are the numbers of treated and control units in block q.

Based on the stratification method the estimator ATT is computed using formula below :

$$\tau^S = \sum_{q=1}^{Q} \tau_q^S \frac{\sum_{i \in I(q)} D_i}{\sum_{\forall i} D_i}$$

Where Q the number of blocks and the weight of each block is given by the fraction of corresponded treated units. One might wonder about the choice of several matching estimator methods. What is the best method to estimate average treatment on treated? Or how should one select a particular matching estimator? How to justify the relevance of this choice compared to other methods? Our choice is guided by the idea that the performance of different matching estimators varies case-by-case and depends largely on the data structure at hand (Zhao, 2003). Therefore, it is sensible to dry different approaches in order to reveal the source of disparity when the results differ. In practice, it seems clear that the choice of the matching method makes little difference[3], the choice is important in the case of samples[4]. It is worth noting that there is no "winner for all" matching method and the choice of the estimator crucially depends on the situation at hand (Zhao, 2003).

4.2. Results and discussion

Propensity score results

The results from the propensity score are given in table 2. I use three specifications. First, I consider variables on the survey which determined access to MFIs and satisfy balancing property in columns Total. The exclusion of households on MFI services depends on many factors, but mostly on the level of poverty. I derive according to poverty index two groups, Non-poor and Poor. This specification is based on the poverty line given by INS[5]. In order to have a comparable result between these two groups, I choose independent variables that similarly satisfied balancing property in two

[3] See Bryson et al. (2002) for other reasons.
[4] See for example see Smith (2000)
[5] Households with an annual income lower than FCA 202, 250 is considered poor.

groups. A household with an older household head is more likely to be a client, but the coefficient is not significant for all specifications. Also, the negative coefficient of age square suggests a nonlinear effect for both specifications.

A household with a female as the head is less likely to be a client for all the specifications. This relation is positive and not significant for the poor household but negatively significant for non-poor households. We are tempted to argue that women who access the MFIs are generally poor women. But non-significance of coefficient makes us less confident.

Household size is positively and significantly correlated with access to MFIs in all cases. It means that access to MFIs increases with the level of household size.

Marital status is not significantly correlated with access to MFIs. But when the married status is positively correlated and separated or widowed is negatively correlated in reference to those who are single.

According to the professional status of households who have access to MFIs credit, correlation is positive with unemployment and those who work in private in all cases. We expected a highly significant coefficient of those who work in both informal sectors, but the coefficient is positive and not significant for the total case. In other cases, access to microcredit is negatively correlated with both the informal sectors. Even worse, this correlation is significant with the non-agricultural informal sector.

However the informal sector is considered to be financed by MFIs, this seems not the case of Côte d'Ivoire according to our results.

On the other hand, the private sector seems to receive credit from MFIs but only for the poor. This correlation is negative and not significant for non-poor. It's possible that non-poor prefer conventional banks than MFIs to realize their activities.

The level of education is significantly correlated with access to MFIs in the first case in reference to those households in which head never schooled. When the head has a secondary level the household is able to access microcredit for non-poor and poor households.

Religious practice is positively and significantly correlated with access to microcredit in the first case. But this not significant when we compare non-poor and poor and their access to microcredit.

Lastly, access to microcredit is negatively correlated with the area for all, poor and non-poor. In this case, the area seems not to be discriminant criteria for accessing microcredit. However, we expected a positive correlation in the urban area and a negative correlation in the rural area because MFI institutions are more established in the urban area than rural.

Table 1: Probit estimates whether a household has access to an MFI

Independent Variables	Case 1: Total		Case 2: Poor		Case 3: Non-poor	
	Coef.	**Z value**	**Coef.**	**Z value**	**Coef.**	**Z value**
Age	0.0206	1.31	0.0279	0.99	0.0145	0.79
Age scare	-0.000171	-1.04	-0.000318	-1.06	-0.000132	-0.68
Female	-0.0832	-0.72	0.170	0.99	-0.316**	-2.00
Household size	0.284***	4.37	0.407***	3.82	0.248***	3.15
Married	0.00648	0.04				
Separated/widowed	-0.0112	-0.09				
Single			0.0544	0.28	-0.0505	-0.32
Without employment	0.775**	2.19				
Work in private sector	0.711**	1.96	0.162	0.83	-0.0210	-0.14
Work in informal sector	0.528	1.51	-0.130	-0.80	-0.297***	-2.68
Work in NA informal sector	0.328	0.93	-0.441**	-2.47	-0.449***	-3.77
Primary	0.418***	5.17				
Secondary	0.447***	5.16	0.326***	2.70	0.193**	2.08
Post graduate	0.510***	3.32				

Quran Schools	0.314	1.12				
Muslim	0.214*	1.72	-0.0457	-0.36	0.137	1.19
Christian	0.211*	1.67	0.0229	0.17	0.103	0.88
Other religion	0.251*	1.71				
Urban area	-0.0916	-1.15	-0.0752	-0.56	-0.00790	-0.08
Constant	-3.611***	-6.86	-2.713***	-4.32	-2.349***	-5.51
Observations	10397		3747		6650	

*** p<0.01$, ** p<0.05, * p<0.1.; *NA : Non-Agricultural*

Matching Results

Matching results with the corrected propensity score is presented in table 2. We expected the impact on the probability to have a positive impact on the expenditure of households who have access to microcredit compared to those who don't have access. The average treatment effect allows us to determine the impact expenditure of those who really benefit from microcredit.

Values in the table below represent the difference between outcome values of access and no access to microcredit. We can say that access to microcredit increases expenditures in first the case corresponding in Total and in the second case (poor) because the logarithm expression of per household expenditure is positive in each method used for this first two cases. This means that a household that accesses to microcredit with NNM method would be able to spend 5.8% more than a comparable household without access to microcredit. This percentage decreases with the stratification method at 2.1%

and at 1.5% for Kernel Method. Results seem to be similar even with different Bandwidths used with Kernel method. Thus, it means also out of a 100 FCFA loaned, a household may spend 5.8% or 2.1 % and 1.5% depending on the matching method used. This amount decreases when the household is poor at 2.8% for NNM and increased for the stratification method at 3% and 3.6% for Kernel method.

For the last case, the non-poor, average treatment effect on household expenditure is negative with all method estimation method used. With the NNM method we obtain a decrease in household expenditures equal to 7%, 5% for stratification method and a percentage between 1.9 and 2.5% for Kernel method. It means that access to microcredit decreases non-poor household expenditure. For 100 FCFA borrowed a non-poor household reduce his consumption expenditure between 1.9% and 7% depending on the estimated method used. How can we explain this decline? One possible explanation is that, when a non-poor household receives a credit, it generally reduces its consumption of non-durable goods to deal with loan repayment on time in order to acquire more loans in the future. That is not the case for the poor household where microcredit is used either to finance an activity or pay household basic needs and unexpected situations (illness, death, etc.).

If we consider only Kernel method because it seems to be the better techniques as said by Chemin (2008) or because of the similarities of results for the three bandwidths used in all cases, we can say that the effect of access to microcredit on household expenditure is relatively low. While poor households increased their expenditure, those of non-poor households decreased when they each accessed to microcredit.

However, it should be noted that the results of our estimates are not significant and this can be attributed to the proportion of households with access to MFIs (less than 2%) compared to those without access.

5. Conclusion

Drawing upon a cross-sectional household survey set in Côte d'Ivoire in 2008, the present study analyses the impact of microfinance institutions on households poverty as defined by per household consumption measured by the log of household expenditure in the year. The propensity score with matching techniques allows us to estimate the average treatment effect of access to MFI between poor and non-poor households. We found that microfinance enhances household expenditure. When we consider poor and non-poor separately, some interesting observations emerge. For poor households, we saw that access to microfinance increased poor household expenditure and decreased those of non-poor households.

Table 2 : Descriptive statistics

Variables	Mean	Std. Dev.	Min	Max
Age	43,772	13,527	15	99
Sex	0,158	0,365	0	1
Household size	0,264	0,441	0	1
Married	0,126	0,332	0	1
Separated/widowed	0,749	0,434	0	1
Single	0,125	0,331	0	1
Without employment	0,108	0,310	0	1
Work in private sector	0,050	0,217	0	1
Work in informal sector	0,531	0,499	0	1
Work in NA informal sector	0,290	0,454	0	1
Public sector	0,021	0,145	0	1
Never schooled	0,598	0,490	0	1
Primary	0,184	0,388	0	1
Secondary	0,178	0,383	0	1
Post graduate	0,028	0,165	0	1
Quran school	0,011	0,105	0	1
Muslim	0,437	0,496	0	1

Christian	0,369	0,483	0	1
Other religion	0,101	0,302	0	1
Without religion	0,092	0,290	0	1
Urban	0,427	0,495	0	1
IMF	0,019	0,138	0	1
ldepcons	12,699	0,802	9,449	16,423

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