Impact of microcredit in rural areas of Morocco: Evidence from a Randomized Evaluation[1]

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Abstract

Microcredit has rapidly expanded in the past years, providing access to financial services to a large population previously excluded from the financial system. However, whether it helps the poor has been a subject of intense debate on which, until very recently, there was no rigorous evidence. This paper reports the results of a randomized experiment designed to measure the impact of microcredit in rural areas of Morocco. Within the catchment areas of new MFI branches opened in areas that had previously no access to microcredit, 81 pairs of matched villages were selected. The treatment villages, randomly selected within each pair, were offered microcredit just after Al Amana opened the branch, while the control villages were offered access only two years later. Al Amana program increased access to credit significantly. Its main effect was to expand the scale of existing self-employment activities of households, for both non-livestock agriculture and livestock activities. We find little or no effect on average consumption as well as on other outcomes such as health, education, etc. However, treatment effects are heterogeneous depending on whether the households had an existing self-employment activity at baseline. Households that had a pre-existing activity decrease their non-durable consumption and consumption overall, as they save and borrow to expand their activities. Households that had not a pre-existing activity increase food and durable expenditure and no effects on business outcomes are observed.

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1 Introduction

At the end of 2009, over 128 million people in the world were receiving microcredit (State of the Microcredit Summit Campaign – 2009). That so many have been reached shows just how adept Microfinance Institutions (MFIs) have been at expanding financial services to the large population that remains largely excluded from the conventional financial system. But the hopes placed on micro-credit go well beyond the expansion of the access to financial services. Supporters of microcredit believe that, among other effects, it alleviates poverty, creates self-employment, promotes gender equality, empowers women (usually microcredit targets women as clients), helps achieving universal primary education. Critics, on the other hand, believe that microcredit can hurt the poor by causing over-indebtedness, and is, in any case, not effective at addressing the root cause of poverty.

While microcredit has enjoyed considerable public support, MFIs have recently suffered considerable backlash in Latin America, India, and even Bangladesh. In other countries (such as Morocco), previously stellar repayment rates are now much lower. While, at least in India and Bangladesh, the this crisis in public support has its origin in politics, it remains possible that the lack of rigorous and unequivocal evidence on the impacts of microfinance, evidence that could be used to bolster the case for microfinance, may be affecting the ability of the MFI to garner public support.

Until recently, there were no randomized evaluations of the effect of microcredit. The sector relied mostly on qualitative studies, or on comparison of borrowers and non-borrowers. The most rigorous non-experimental studies (Pitt and Khandker, 1998), which relied on a structural model and the rules that Grameen Bank uses to screen its borrowers, showed positive effects. But Morduch (1998) have questioned the identification strategy used by Pitt and Khandker, who in turn have questioned the validity of the criticism by Morduch[3].

There are two randomized evaluation of microcredit programs. Banerjee et al (2010), evaluating group-lending microcredit in the city of Hyderabad, India, found that the program had positive effects on business creation and on the purchase of business durables, but no effect on consumption or on any human development outcomes. When they examine households by propensity to start a new business, they find heterogeneous effects on consumption. Households with a high propensity reduce consumption to make room for the fixed costs of starting up the new businesses, while households with a low propensity use microcredit to increase current consumption against future income. These results, the first rigorous evidence on the impacts of microcredit, helped nuance the discussion on the effect of expanded access. They suggest that while there is no radical social transformation as supporters affirm, microcredit may help a specific group of households to enter and expand entrepreneurship in the short-term. Karlan and Zinman (2010), evaluating an individual lending program for micro entrepreneurs in urban Philippines, find small or no effects on profits and business expansion.

[3] See Banerjee et al (2010) for a discussion of existing impact evaluations.

Both of these studies evaluated microfinance programs in very densely populated urban areas. In such a context, potential borrowers may have had access to various (formal and informal) sources of credit, including other MFI, moneylenders, and banks. In addition, they may also have had many more business opportunities than they would have had if they lived in a rural context. Altogether, this means that the effects of the programs may have been very different in a rural context where both credit and business opportunities are thinner. In this paper, we report the results from a randomized experiment designed to measure the impact of microcredit in rural areas in Morocco. To our knowledge, this is the first randomized evaluation of the impact of expanding access to microcredit in rural areas.

We take advantage of the expansion of Al Amana, the largest MFI in Morocco. Between 2006 and 2007, Al Amana opened around 60 new branches in rural areas. These areas were not exposed to microcredit before the program was launched and had essentially no access to microcredit except for Al Amana thereafter. For each new branch, one or more pairs of matched villages were selected, usually in the periphery of the branch intervention area. Within each pair of the 81 participating pairs, one treatment and one control village were randomly selected. The treatment villages were offered microcredit just after Al Amana opened the branch. This was done through weekly visits by the credit agents. The control villages were offered access only two years later, after the completion of an endline surveys.

The main product offered by Al Amana is a joint-liability loan to groups of three to four members, both men and women. Individual-liability loans have also been available in rural areas since March 2008. A comprehensive household survey was administered in the treatment villages before the program inception. On average 28 households per village participated (4,495 household surveys in total). The same survey was administered in control villages two years later, just before the control villages received microcredit; in total, 5,551 households participated. Households that were predicted to be more likely to borrow if microcredit were available to them—based on a propensity score regression estimated in the location where Al Amana opened the first seven branches—were selected for the survey. By the time of the endline survey, 16% of surveyed households had taken a microcredit from Al Amana.

From a theoretical point of view, the effects of microcredit can come through many channels, ranging from a relaxation of credit constraints, access to cheaper credit, changes in intra-household bargaining power (when loans are mostly given to women), to different decisions between small consumption expenses vs. large investment expenditures in environments where households do not have access to efficient saving instruments.

The most direct effect of microcredit is to give access to credit markets to households that were previously excluded. If these households were credit constrained, microcredit may allow them to make investments to start a new business or to expand existing business activities and/or to increase spending in household assets, education, and health. In addition to this direct effect of receiving a loan, there is the possibility that just the expectation of access to credit from an MFI to cover future needs may affect saving and insurance decisions. Households that would otherwise hold stocks or assets as a buffer against future adverse shocks may decide to decrease their holdings (Deaton 1991, Rosenzweig and

Wolpin 1993), and rely on microcredit instead. Finally, a microcredit can provide the initial sum of money to make large expenditures that would otherwise be unaffordable with the regular small amounts saved by households, such as durable assets, education, and health. Al Amana lends to women as well as men, so a shift in bargaining power away from women and towards men may not have been expected in this context.

As Banerjee et al (2010) show, the short term effects of microcredit are ambiguous. Its effects on savings and consumption depend on the how long it takes for the project returns to be realized and on how patient the household is, that is, on how much utility they obtain from current rather than future consumption. If the households borrow to consume today or to invest in projects that will generate immediate returns, then we will observe an increase in consumption. But if they borrow to invest in projects with delayed returns, be it an enterprise or education and health, then we may observe a decrease in consumption as well as an increase in short term investment.

The microcredit program evaluated here seemed to have had a clear effect on reducing credit constraint in rural Morocco. Excluding credit linked to electricity or water, only 10% of households in control villages had any credit by the endline (6% from an informal source, 2% from another MFI, and the rest from a bank). This proportion more than doubled in treatment villages (a 13 percentage point increase).

Our results suggest that, in this rural context, the main effect of access to credit was to expand the scale of existing self-employment activities of households. Activities expand both for non-livestock agriculture (henceforth, agriculture) and livestock activities. We see significant impacts on agricultural sales (26% increase compared to the control) and on profits. For livestock, we see an increase in the stock of animals held, accompanied by an increase in sales (11%) and self-consumption (11%) but no significant impacts on profits. We also find that households that were already running livestock activities diversify both the type of animals and the types of animal products they sell. We do not find any impact of microcredit on either the creation, the profit, or the expansion of non-agricultural businesses.

Moreover, there was also no effect of access to microcredit on a shift towards new activities. Households, for example, were no more likely to start a livestock business if they did not have one before.[4] It should be noted, however, that starting a new activity was not encouraged by Al Amana.

What did household do with the extra profit they earned on household-based activities? A fraction of it was saved: household in treatment villages had 1008 dirhams more (6.3%) in assets than those in comparison villages, and they had also sold fewer assets in the previous year. Interestingly, part of the extra profit was offset by a reduction in wage earnings. Wage earnings declined by 1,070 dirhams, 6.8% of the earnings in the control groups, over the previous year.

[4] This is not due to a general inertia in the area. On the contrary, entrepreneurial dynamism is quite remarkable: 44% of the households, both in treatment and control group, started at least a new new self-employment activity between the baseline and the endline surveys, and 18% stopped at least one.

Due to these offsetting effects, access to microcredit had essentially no impact on average consumption. The effect is a precisely estimated zero: the point estimate on average monthly consumption per household of having access to microcredit is a decrease in 62 dirham, and we can reject at the 5% level that the program led to an increase in monthly consumption of more than 14 dirhams (or 0.5% of the control mean!). Not surprisingly, then, there was also no effect on poverty as measured by average consumption per capita over this two year period.

As Banerjee et al (2010) found, the effects are heterogeneous, depending on whether the household had a self employment activity at baseline: we find a significant *decrease* in consumption among households who had an existing activity before Al Amana launched its program (about two thirds of the sample), while there is a positive but not significant effect on overall consumption for those without an own activity at baseline, together with a significant increase in expenditure on food, and on durable goods purchase.

Taken together, our results suggest that access to microcredit allowed some households to expand existing employment-generating activities (by buying more livestock or to expand non-livestock agricultural production) and perhaps to start activities at a larger scale than they would have been able to do otherwise. Those households saved more (some of them, who had an activity before, actually cut consumption when offered the opportunity), and stopped working for wages outside the house. All these results suggest that at least some households were clearly credit constrained before the program. They wanted to invest more in their self employment activity (and work on their own business, rather than outside), but were not able to do so in the absence of access to credit. In the short run, this did not translate into an increase in their standard of living as measured by consumption (or education, health, etc). But to the extent these activities are indeed profitable, these may be realized in the longer run, when household feel they have reached an optimal scale and stop saving to expand further.

It is worth noting that a fairly low take-up (16% after two years), similar to what was found in other studies, suggest that the effect of the program on poverty reduction and welfare is necessarily going to be relatively limited, even in the longer run. This is not necessarily a failure of this program in particular, or micro-credit in general. It may well be a very effective tool precisely for the minority of households who wants to expand their activity.

2 The program and the experimental design

2.1 Al Amana program

With about 401,000 active clients and a 2,736 million Moroccan Dirhams (MAD) portfolio (338 million USD) as of December 2009, Al Amana is the largest microcredit institution in Morocco. Since the start of its activities in 2000, Al Amana expanded from urban areas, into peri-urban areas and then to rural areas. Between 2006 and 2007, Al Amana opened around 60 new branches in non-densely populated area.

The main product Al Amana offers in rural areas is a group-liability loan. Groups are formed by three to four members who agree to mutually guarantee the reimbursement of their loans. Loan amount ranges from 1,000 MAD to 15,000 MAD (124 to 1,855 USD) per member. It can take 3 to 18 months to reimburse loans, through payments made weekly, two times a month or monthly. For livestock activities, a two-month grace period is granted. Interest rate of rural products was between 12.5% and 14.5% between 2006 and 2009.

Since March 2008, individual loans for housing and non-agriculture businesses were also introduced in rural areas. These loans (up to 48,000 MAD, or about 6,000 USD) are targeted at clients that can provide some sort of collateral. Investments such as acquisition, improvement or construction of housing as well as water or electricity connections may be funded through individual housing loans. Reimbursements can be made over a period of 6 months to 7 years, through repayments made every two weeks or monthly. Individual loans for enterprises target entrepreneurs that are looking to invest in equipment or promote their existing activities. These loans can also take between 6 months and 5 years to repay, with repayments also every two weeks or monthly. As we will see below, in the areas of the study, households mostly took group-liability loans.

To be eligible to a group-liability loan, the applicant must be between 18 and 70 years, hold a national ID card, have a residency certificate, and have been running an economic activity other than non-livestock agriculture for at least 12 months. Al Amana does not lend exclusively to women, but usually requires to credit agents to have at least 35% of women among their clients. But as we explain below, this requirement was removed among the branches participating in the study.

Eligibility for individual loans has additional requirements. For individual enterprise loans, the borrower needs to have been running the activity in the same physical space for at least 12 months, to own and manage the activity, to have non-itinerant activity, and to give proof of solvency of a guarantor. For housing loans, the borrower need to have property title, construction cost projections, income attestation, and a solvent guarantor.

2.2 Experimental design

Alamana opened around 60 new branches between 2006 and 2007 in sparsely populated rural areas. Each branch has a well-defined catchment area[5] served by permanent credit agents. The areas in which new branches were opened were selected by Al Amana based on an assessment of potential clients. Before each branch was opened, data was collected from at least six villages located on the periphery of the intervention areas, villages that, in principle, could be included or excluded in branch's catchment area. Villages or groups of nearby villages were matched in pairs based on observable characteristics, such as the number of households, accessibility to the center of the community, existing infrastructure, type of activities carried out by the households, type of agriculture activities, number of livestock ,and

[5] A map is established and approved by Al Amana headquarters before the branch is opened, specifiying the exact area, and therefore, villages, that are eligible to be served by the branch. An intervention area can consist of 1 to 6 rural communities, and several villages belong to a community.

the existence of other MFIs[6]. Villages that were close to a rural center or that had to be crossed to access other areas served by the branch were not selected as this would have disrupted Al Amana development (as well as the evaluation) if they were randomly assigned to the control. On average, two pairs per branch with the minimal distance on the characteristics mentioned above were kept for the evaluation. Selected villages had on average 120 households. In each pair, one village was randomly chosen and assigned to treatment, while the other was assigned to control.[7]

The baseline survey followed the timeline of Al Amana branch openings. Between 2006 and 2007, there were 6 waves of openings ('*promotions'*, in French), grouped in four waves for the purposes of the evaluation. Before credit distribution started among the villages of a given wave, a comprehensive baseline survey was conducted. Modules included socio-economic characteristics, household assets, housing, households' production on agriculture, livestock and other non-agricultural activities, member's outside work, consumption, credits and woman's role in the household.

In choosing the sample to be surveyed, we followed a novel approach to maximize the power of the intervention in the presence of a limited budget. In the first wave every household in the village was surveyed[8]. Data from this survey plus credit-take up in treatment villages over the first six months (reported weekly by credit agents) were used to estimate a model to predict the likeliness to borrow for each household. For the following waves, a short survey was first administered to 100 households per village.[9] In this survey, we collected the variables necessary to estimate the probability of borrowing for any given household, based on the model estimated in the first wave. We then selected the 25% of households predicted to be the most likely to borrow on the basis of the model, plus five households randomly chosen, to be able to distinguish direct effects and spillovers. In total, 4,495 households (a median of 29 households per village) were surveyed at baseline.

After the baseline survey, one treatment and control village was randomly chosen within each pair and communicated to credit agents. While control areas would only be served two years after, in treatment areas credit agents had to promote microcredit regularly, starting immediately. They visited villages once a week and performed various promotional activities: door to door campaigns, meetings with potential clients (including the participation of clients to share their experiences), contact with village associations, cooperatives and women centers, etc. As take up in the village increased, intensity of visits decreased.

Initially, 80 pairs of villages were chosen among the four waves of branch openings. However, as of the end of 2006, i.e. after the baseline survey had been administered to waves 1 and 2, 5 pairs of villages had to be dropped. These villages were dropped because Al Amana had already disbursed credit (though pre-existing branches) by the time of the baseline survey (2 pairs), baseline data quality was poor (2 pairs) and the existence of illegal activities in one pair made microcredit (and surveys!) unappealing to

[6] Before Al Amana launched its program, only 2% of households had a microcredit from a MFI. The very few villages that were already served by another MFI were excluded from the study.

[7] Pairwise stratification has been shown to be an efficient way to construct a treatment and control group (Barrios et al, 2010).

[8] If the village had more than 100 households a random sample was taken. Therefore, the household survey was administered to either the whole village or to a representative sample consisting of a maximum of 100 households.

[9] Same as for the first wave, if the village was larger, 100 households were randomly selected to administer the short survey.

villagers and Al Amana stopped their activity in certain villages. Dropping pairs due to these reasons does not undermine evaluation integrity since it is not base on treatment. It was then agreed with Al Amana to select additional pairs in order to replace the lost pairs. By the time of the endline survey, 81 pairs belonging to 47 branches were included in the survey.

An endline household survey, based on the same instrument, was administered two years after Al Amana intervention started in each wave. Power calculations based on take up showed the usefulness of increasing the number of observations at endline. Therefore, using the prediction model and the short survey conducted at baseline, new households, those most likely to borrow among those not yet surveyed, were included in the endline survey. In total 5,551, out of which around 1,400 were new households (i.e. only surveyed at endline), were surveyed. All data was collected by an independent team led by J-PAL and a local survey firm, Team Maroc. Survey was presented (and to our knowledge perceived) to be completely independent of Al Amana.

2.3 Experiment integrity

Table 1 shows summary statistics for the households in the sample. Both the treatment and control groups seem to be well balanced, with no significant difference in most of the observable characteristics. Attrition represents 8% of the total sample, 7% of the control villages, and 9% of the treatment villages. While this difference is small, it is statistically significant due to our large sample size. [10] However, regressions on observable characteristics of these households show no significant difference between treatment and control groups in the characteristics of the attritors.

Table 1 also shows that at baseline the average household had 6 members, was led by a male aged 50 years (89% of households). Households run mainly livestock (68% of households) and non-live stock agricultural (65% of households) activities. Only 14% of households have a non-agricultural business (small commerce, handicraft, services, etc). On average, households run two activities. Men are usually responsible for running these activities: 74% of activities are declared to be managed by a male household member. The average household consumes monthly 3000 MAD in total (at 2007 prices), while 16% of households have a per capita consumption below the poverty line.

Before Al Amana started operating in these areas, microcredit lending was almost inexistent. Only 2% of households in control villages reported that they had an outstanding microcredit at the time of the endline survey or had completed its reimbursement over the 12 months preceding the survey and 26% of households had access to some sort of credit. More of half of the loans, however, were used to fund an electricity or water connection (16% of the total sample). As for the rest, 2% of the sample borrows from Banks, 2% from MFIs other than Al Amana, 2% from other formal sources, and 6% from informal lenders (grocery store, neighbors, friends, etc.). Thus access to financial credit is extremely low in this sample (for comparison, Banerjee et al (2010) find that over two third of the households in their sample have a loan).

[10] A regression of attrition on treatment gives a coefficient for treatment of 0.0166 with a standard deviation of 0.0073.

These families live in villages that are located 10km far from the center of the rural community (distance to the closest market), where Al Amana branch is also usually placed. The typology of villages is quite heterogeneous, from densely to non-densely populated villages. The average household has to walk 4 minutes to reach the closest neighbor, while 5% of households live in villages in which the next house is located at a 10-minute walk distance.

2.4 Analysis

We first estimate the average effect of microcredit access on households living in villages where it becomes available. To do that, we compare outcome averages of households in treatment and control groups; in other words, we look at intention to treat (ITT) estimates. The equations reported in Table 2 through Table 12 are specified as follows:

$$y_{p,i,j} = \propto + \beta * T_{\mathrm{p,i}} + \delta * y^{\mathrm{b}}_{\mathrm{p,i,j}} + \partial * \mathrm{M_j} + \theta * \mathrm{C_j} + \sum_{m=1}^{p} \propto_{\mathrm{m}} 1(\mathrm{p} = \mathrm{m}) + v_{\mathrm{i}} + \omega_{\mathrm{j}}$$

Where T is an indicator of living in an area where microcredit was offered by Al Amana, β is the estimator of the ITT effect, p is an index of the pair, i an index for the village and j an index for the household. We also include indicators (α_m) for the pairs of villages matched before randomization took place (m or p pairs in total), as well as baseline outcome levels (y^b). An additional indicator is introduced (M) for outcomes of households for which we only collected endline data, as well as for outliers that have been corrected at cleaning (C). Standard errors are clustered at the village level.

We also estimate the effect of microcredit offer in specific groups of our sample. We consider a sub-population identified through taking the value 1 of a variable S ∈ {0, 1}. We examine the impact of microcredit on this sub-population compared to the impact on a comparison sub-population (for which S equals 0). The equation is specified as follows:

$$y_{p,i,j} = \propto + \beta * T_{\mathrm{p,i}} + \beta_{\Delta} * \mathrm{S_{p,i}} * T_{\mathrm{p,i}} + \mu * \mathrm{S_{p,i}} + \delta * y^{\mathrm{b}}_{\mathrm{p,i,j}} + \partial * \mathrm{M_j} + \theta * \mathrm{C_j} + \sum_{m=1}^{p} \propto_{\mathrm{m}} 1(\mathrm{p} = \mathrm{m}) + v_{\mathrm{i}} + \omega_{\mathrm{j}}$$

Where β measures the ITT effect on the comparison sub-population (S=0), while β_Δ*S measures the difference in the effect between the two sub-populations, that is, the differential effect on the sub-population with S=1. Following Banerjee et al (2010), the two subgroups we consider in this version of the paper are households how had an activity at baseline, and households that did not.

2.5 Program take-up

Since we are interested in measuring the effect of microcredit, we first need to verify that there is a difference in microcredit take-up between households living in treatment and control villages. Column 3 of Table 2 shows that there is no significant difference on loans taken by households from a MFI different from Al Amana: in both groups, 2.4% of households declared to have borrowed from an MFI. This implies that any difference we find in outcomes between treatment and control groups will not be due to microcredit distributed by other MFIs.

To look at Al Amana take-up we use two sources of data: (i) administrative data on disbursed credit: credit agents reported weekly disbursed credit to people living in treatment areas. This list was regularly matched to the baseline survey in order to follow take-up evolution. Moreover, a final check was carried out on the field between credit agents and evaluation team members just after the endline survey took place. Globally, 16% of households living in treatment villages (and also surveyed) had taken a loan from Al Amana since the branch was launched, and none had in control areas (table 2, column 1); ii) endline household survey data: data on loans as well as their characteristics were collected through a borrowing module. As presented in column 2 of Table 2, households in treatment villages are 10 percentage points more likely to report borrowing from Al Amana than those in control villages (12% of treatment households vs. 2% of control households[11]). These results suggest that survey respondents might have under-declared borrowing from Al Amana. Independently from the source we choose, however, the differential in the likelihood to borrow in treatment vs. control areas is highly significant, and reasonably large (10 percentage point or 16 percentage point).

Al Amana mostly distributed group-liability loans among households living in treatment villages and surveyed (three fourths of households that borrowed from Alamana). Moreover, Al Amana distributed credit mostly among men. It is not surprising to find that woman participation is below the quota requirement since groups have been exempted from this requirement since mid-2006. This measure was taken to boost credit take-up in treatment villages. Irrespective of whether they borrowed or not, treatment households had borrowed on average a total of 940 MAD from Al Amana at the time of the endline survey (777 MAD more than control households)[12]. Given a take-up rate of 12% among treatment households, the average amount of loans disbursed among treatment clients is around 7,800 MAD, which corresponds well to the range of loan amount offered by Al Amana (column 1 of Table 3).

3 Results

As we previously saw, rural non-densely populated areas in Morocco are characterized by a very low use of both formal and informal financial services, which may well reflect low access. In control villages, 4% of households had a loan from a bank or microcredit and 6% from informal lenders. In this context,

[11] Evaluation protocol allowed to disburse Al Amana loans to groups in which there was at maximum one member from a control village.

[12] This is the sum of active loans plus reimbursed loans in the 12 months preceding the survey.

households might face significant credit constraints that can affect them in several ways. New viable projects may not be funded, on-going profitable projects may not be expanded, households may be unable to use credit as an insurance mechanism and to smooth up consumption in case of shocks. In addition to this, constraints may divert households from undertaking large expenditures that are unaffordable with the regular small amounts saved. The relaxation of the credit constraint might thus affect many dimensions of the lives of poor households. Moreover, initial conditions of households may differ: they may not all have the same investment opportunities, face the same credit constraint, etc. This heterogeneity might drive the demand and the effect of microfinance in different directions.

We start by presenting the average differences on treatment-control outcomes for the entire sample, and then examine, in the next section, the results for specific sample groups.

3.1 Average effects

3.1.1 Demand for microfinance

The introduction of microfinance in treatment villages significantly improves access to credit. It corresponds to an increase in the fraction of households having an outstanding loan of 13 percentage point between treatment and controls. For those taking a loan, it amounts to an increase of their total debt of around 8,900 MAD, $1100 dollars (taking the LATE estimate) a four-fold increase over their previous financial borrowing (excluding an average of roughly 500 MAD in loan from the water or the electricity company). This is a very different result from Banerjee et al (2010), who found no significant increase on the total amounts borrowed (in an environment where household were borrowing a lot from various sources). This suggest that, in this context, the primary direct effect of microcredit may be to relax a potential credit constrain, rather than to make cheaper credit available to people.

Although the effect on credit access is positive, it affects only a small fraction of the population, the 12%-16% that are borrowing from Al Amana. This may suggest that, while financial constraints do affect some households, they may not be binding for the larger part of households. . Only a small fraction of the population have profitable investments (above the interest rates charged by Al Amana, i.e. 12.5%-14.5%) that were not undertaken in the absence of microcredit. However, it may also mean that the specificities of the product offered by Al Amana (group-liability, small loans, regular repayments, etc…) may not solve the inter-temporal maximization problem of some households, even if they are constrained. For instance regular repayment schedule could discourage investments opportunities that are generating returns only at longer terms (Field et al, 2011). Regardless of the interpretation, microfinance seems successful in reducing financial constraints of a specific group of households.

3.1.2 Starting new businesses or activities?

The development of new businesses and economic activities is at the heart of strategies seeking to improve the access to credit. Interestingly, however, we see that the Al Amana program had no effect on the probability that households start new activities.

As shown in table 4, many households started a new activity between the baseline and the endline survey (we define "starting a new activity" as starting a livestock business, a non-livestock activity, or a non agricultural activity for a household that did not have one before). In the control group, on average, 0.44 new activities were started, and 0.18 activities were stopped. This indicates a high degree of churning and dynamism. One might have expected that microcredit access would have helped this process, but apparently it did not: the number of new activities started was almost the same in treatment and control villages.

Looking at it in more details within each sector, we see that households that were already raising livestock, diversify within this activity (both the types of animals and the kind of animal products produced, such as milk, cheese, etc.). No similar effect is found for agriculture or non-agricultural activities.

One possible reason for the lack of investment in new sectors of activities is that Al Amana program, as it was implemented in rural areas, did not particularly encourage the start of new activity. In rural areas, a decision was made to finance *existing* activities, which had a track record. This was to make sure that repayment rates would be high. This close attention paid to repayment rates, which may lead to a certain conservatism by credit officers, is not at all unique to Al Amana, and may reduce the extent to which microcredit indeed leads to starting new, profitable activities (on this, see also Field et al, 2011). Nevertheless, since money is fungible and credit is only loosely monitored, one might have expected that the loans would help those who were desiring to start something new.

Therefore, if credit does not lead more households to venture into new sector, it may be that finance is not the main constraint household face to start a new activity: rather, it may important for them to see a new opportunity.

3.1.3 Expanding current activities

Where financial constraints do seem to play a key role, however, it is in the scale of the self-employment activities. Table 6 display the effect of microcredit on the scale of existing activities. Table 6 displays the results for all activities together. Overall, we find a substantial effect on sales (3,305 MAD), expenses (2,297 MAD, or 14%), in-kind savings (11%) and self consumption (11.8%). Household in the treatment villages also hire significantly more employees from outside the households (19 days per year on average, or 52%).[13]

This overall result is however driven by different impacts on the three types of activities present in our sample: non-live stock agriculture, livestock and (non-agricultural) businesses.

[13] These results are also robust when we apply quantile regressions, not shown in the paper, that are not sensitive to potential outliers.

First of all, microfinance has no effect on any outcomes of the non-agricultural businesses. Access to microcredit does not increase profits and employment of small businesses. Only a minority of households were deriving their income from small businesses before the introduction of microfinance, however. As such, our estimates for this category are relatively imprecise.

In contrast, we find that agricultural activity expands, with a growth in sales, expenses, profits and employment outside the household. There is no effect on savings (in the form of stocks) and on self-consumption. The effects (Table 7) are large, the ITT estimates show an increase in profits far superior than the interest rates charged by Al Amana.

In the case of livestock activities, most of its expansion comes from an increase in savings (due to an increase in the stock of livestock), with some growth of sales and self-consumption but not profits (Table 8). Savings in the form of livestock may be easy to constitute and are relatively liquid. It is difficult to say whether this reflects the fact that livestock raising is less profitable than agriculture. While we do not see an increase in realized profit among these households, it is possible that they have some un-realized profit (in the form of a large stock of animals). Alternatively, they may simply be buying livestock because it is a convenient way to save.

3.1.4. Income

Treated households increase the proceeds derived from their agricultural or livestock activities. What do household do with these extra proceeds? Part of it is, as we saw, immediately saved in the form of livestock.

Very interestingly, however, we also observe that households in treatment villages reduce their labor supply outside the village. The decline is sufficient to offset most of the increase in income from self employment: wage earnings decline by 1070 MAD, or 6% (if this effect could only be attributed to households to actually got a credit, this would correspond to a decline of 44% in wage earnings by those households). The most obvious possibility is that household members spend more time on their own activity. However, note that we had not seen any increase in labor supply by household members on their activities (instead, they were hiring new labor). To the extent this latter result was not to be attributable to measurement error, this suggests that treated households may be consuming more leisure.

3.1.5 Poverty and consumption

Household in treated villages expand their activities and have higher proceeds. However, part of this money is saved, and part is offset by a reduction in wage income. Thus, perhaps not surprisingly, we see no impact on per capita consumption in the short run (two years). The results are presented in table 11. We can reject even very small increases in consumption due to the program (0.5%). Correspondingly, there is also no effect on "poverty" as it is defined in Morocco (as in most countries), consumption

below a certain threshold. The probability of being poor[14] is around 16% and is the same in both treatment and control villages. This is very similar to the finding in Banerjee et al (2010). [15]

As pointed out by Banerjee et al (2010), consumption levels are, however, perhaps not a very good measure of permanent income when households are credit constrained and have the opportunity to undertake lumpy investments: in this case, we may see them consume less in the short term in order to be able to undertake the investment and be richer in the longer term. Moreover, the reduction in wage income (and hence, perhaps, the increase in utility) should in principle be factored in to value the total increase in welfare.

3.1.6 Education, health and women empowerment

The impact of microfinance is supposed to go beyond the expansion of business activity and consumption levels. Indirect effects such as women empowerment, improvement in the health status and education levels of children are often considered as potential impacts of microfinance.

When we look how consumption shifts, we observe only some limited changes in its composition: treatment households are more likely to spend on health and less likely to spend on social events (marriages, religious celebrations, etc.) than control households (this was also found in Banerjee et al, and is consistent with the idea that household save more).

The majority of borrowers of our sample being men, the expected effect on women empowerment is limited. We use several proxies for women empowerment. The first one is the number of activities managed by a female member in the household. In remote rural areas, the activities are usually managed by male members (1.5 activities in average compared to 0.38 for women). We find no effect on this share with the introduction of microcredit. We also use a series of qualitative indicators to describe women empowerment such as the capacity of women to take some decisions and their mobility inside and outside the villages. We construct a summary index of these qualitative variables as they are part of the same "family" of outcomes. We also find no evidence of the effect of microfinance on this index.

These results are in line with the fact that only a small proportion of women borrow in remote rural areas and that additional borrowing for men is unlikely to change the bargaining power of women within the household. They are also consistent, however, with Banerjee et al (2010), in a context where only men borrowed.

Concerning education, results (table 12) show some evidence that microcredit improves children schooling, but the magnitude of the effect is quite small. The proportion of children going to school is increasing from 0.73 in control villages to 0.76 in treatment villages. While the effect is small, the direction is reassuring: it shows that microfinance did not lead to an *increase* in child labor.

[14] Based on the poverty line defined by the *Haut Comissariat au Plan* of Morocco.

[15] Quantile regressions (not shown in this paper) on aggregate consumption confirm the results of OLS regressions for the most of the quantiles except for the top of the distribution (quantile of order 95%) where the effect is negative.

Microfinance could also have adverse effects for children when the activities are expanding with a reallocation of their time from school to household activities. We do not find any evidence of such mechanisms despite significant expansion of agricultural and business activities (table 12).

3.2 Heterogeneity

As seen above, average effects show that the main impact of microcredit access is to expand the scale of self-employment activities of households. Since having an existing activity at baseline is independent from microcredit (i.e. from treatment), we now examine the impact of microcredit access on the sub-samples of households that had and had not an own activity by the time the program was launched. As Banerjee et al (2010) found, the effects for these two groups are heterogeneous.

Treatment households with a pre-existing activity (about two thirds of the sample) borrowed in total 1,046 MAD from Alamana, almost double than households without an own activity (the average loan for this group totals 553 MAD). For both groups, households in the treatment group are not more likely to start a new activity. Regarding business expansion, as expected, we find positive effects only among treatment households with a pre-existing activity. For this group, there are significant positive effects on sales and self-consumption (15%), business expenses (12%) and savings (7%), while no effect on these outcomes is observed among those households without a pre-existing activity (table 13).

When we look at overall consumption, we find a significant decrease (3%) for households that were running an own activity at baseline. This effect is essentially led by a decrease in social event expenditures (marriages, religious celebrations, etc), while no decrease in food, health or education expenditure is observed. These results suggest that both microcredit and savings from consumption were used to expand existing activities.

For households that did not have a self-employment activity at baseline, there is a positive but not significant effect on overall consumption. When we look at consumption composition, we find a significant increase in expenditure on food, and on durable goods purchase among treatment households of this group.

4 Conclusions

This paper presents the results of an impact evaluation of a microcredit program in rural areas using a randomized experiment. Our experiment is based on the assignation of large set of villages to treatment villages, where microcredit is offered, and control villages, where it is not. Our results show that microcredit increases substantially access to the financial markets, but only a fraction of households benefit from it. After two years, 16% of the households had become client of the IMF. Yet microfinance initiated sizeable changes in household's activity as measured for the second year after the beginning of the experiment. These changes are not related to new business starting. We didn't see a major diversification in household's activity. The only effect on new business starting consisted in the diversification among livestock activities. The main effect of microfinance is related to the development

of already existing activities. This is especially true for agricultural activities for which we see a large increase in sales as well as an increase in expenditure and employment associated with a substantial increase in profits. Microfinance had also an effect on livestock activities. We see that sales significantly increase as well as savings, which is the livestock's value and self consumption.

The components of income of household in treatment villages were also affected. Although net income does not increase we see an increase in agricultural income and a reduction in wages for external work and sales of assets. Households seem to focus on their own agricultural activities and save their income under the form of livestock.

However we do not see any impact on household consumption associated with these changes. We observe a small reduction not significant in global consumption. We observe minor statistically significant increases in education and health expenditure but no changes in women empowerment.

One interesting finding consists in the heterogeneity of the response of households. We examined the results separately for households with and without a self-employment activity at baseline. Our results echoes these obtained by Banerjee and al. (2010) for people likely to start a business. Households with an existing activity had large increases in their activities through increases in sales, expenditure, and savings and associated with a reduction in consumption, especially in social consumption. Households without an activity at baseline, although they also had an increase in the amount of microcredit, they had no significant increase in their activities and had an increase in their consumption (in food and durable expenses).

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Appendix A. Tables

Table 1. Baseline treatment-control comparison

	(1) Number of Members	(2) % M Head	(3) Age HH	(4) Distance Souk	(5) Loans Total	(6) Savings	(7) Number of own activities	(8) % Agriculture	(9) % Livestock	(10) % Non-agricult. business	(11) Consumption	(12) Poverty rate
Treatment	0.03	-0.01	1.06***	1.61	-176.03	-0.05	0.00	0.01	0.04*	-0.04***	31.11	0.00
	(0.078)	(0.010)	(0.390)	(1.962)	(356.184)	(0.032)	(0.042)	(0.018)	(0.019)	(0.014)	(32.135)	(0.013)
Observations	4,495	4,495	4,445	3,987	4,495	4,495	4,495	4,495	4,495	4,495	4,451	4,099
Control mean	6.099	0.884	50.436	11.128	2148.385	1.392	1.886	0.650	0.686	0.143	2993.807	0.158
Sample mean	6.115	0.890	50.740	10.789	2712.067	1.417	1.891	0.655	0.691	0.134	2972.528	0.158
Median	6.000	1.000	50.000	20.000	0.000	2.000	2.000	1.000	1.000	0.000	2588.648	0.000

Notes: Household averages collected at the baseline survey.

Table 2. Effect on credit access

	(1) Al Amana - Admin data	(2) Al Amana- Survey data	(3) Other IMF	(4) Other Bank	(5) Other Formal	(6) Branching	(7) Informal	(8) Total
Treatment	0.16***	0.10***	-0.00	0.00	0.00	0.02	0.00	0.13***
	(0.012)	(0.012)	(0.005)	(0.003)	(0.003)	(0.017)	(0.006)	(0.021)
Observations	5,551	5,551	5,551	5,551	5,551	5,551	5,551	5,551
Control mean	0.000	0.021	0.024	0.017	0.018	0.160	0.064	0.263
Sample mean	0.078	0.072	0.022	0.021	0.021	0.170	0.065	0.327
Median	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Clustered standard errors in parentheses.

Table 3. The loans

	(1) Al Amana	(2) Other MFI	(3) Banks	(4) Other formal	(5) Branching	(6) Informal	(7) Total	(8) Loan reimburs.
Treatment	777.17***	-31.20	253.21	243.66	524.73	-359.16*	1,154.43***	34.05**
	(102.150)	(33.847)	(180.786)	(180.258)	(326.428)	(200.626)	(416.527)	(13.569)
Observations	5,551	5,551	5,551	5,551	5,551	5,551	5,551	5,524
Control mean	162.599	142.014	682.456	689.573	573.982	722.231	2148.385	41.031
Sample mean	545.708	125.394	781.039	784.642	830.923	550.794	2712.067	57.930
Median	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Clustered standard errors in parentheses.

Table 4. Effect on starting new activities

	(1) Increases	(2) Decreases	(3) Stable	(4) Number of activitites
Treatment	-0.00	-0.02*	0.02	0.03
	(0.014)	(0.009)	(0.014)	(0.040)
Observations	5,551	5,551	5,551	5,551
Control mean	0.441	0.183	0.376	1.886
Sample mean	0.442	0.175	0.383	1.891
Median	0.000	0.000	0.000	2.000

Notes: Clustered standard errors in parentheses.

Table 5. Diversification whithin activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Agriculture				Livestock		Non-agricultural businesses	Total
	Cereals	Tree	Vegetables	Total	Animals	Products	Total		
Treatment	0.08	-0.00	0.02	0.09	0.15***	0.16***	0.31***	-0.02*	0.39**
	(0.051)	(0.029)	(0.028)	(0.073)	(0.059)	(0.046)	(0.101)	(0.008)	(0.152)
Observations	5,551	5,551	5,551	5,551	5,551	5,551	5,551	5,551	5,551
Control mean	1.322	0.715	0.251	2.288	2.258	1.384	3.642	0.151	6.081
Sample mean	1.364	0.711	0.264	2.340	2.327	1.462	3.790	0.142	6.271
Median	1.000	0.000	0.000	2.000	3.000	1.000	4.000	0.000	6.000

Notes: Clustered standard errors in parentheses.

Table 6. Did activities expand?

	(1) Output	(2) Expenses	(3) Savings	(4) Investment	(5) Self-empl.	(6) Employment	(7) Self-consump.
Treatment	3,304.74**	2,296.90**	1,008.19*	1,153.93	1.53	19.06*	669.24**
	(1,606.410)	(937.151)	(521.599)	(1,115.475)	(14.802)	(10.378)	(331.336)
Observations	5,505	5,505	5,507	5,551	5,551	5,551	5,518
Control mean	22902.418	18226.840	16026.866	9879.272	365.509	36.398	5625.553
Sample mean	24307.498	19157.672	16702.994	10046.916	365.771	46.584	5977.128
Median	5720.000	5192.000	9037.500	0.000	281.000	0.000	2500.000

Notes: Clustered standard errors in parentheses.

Table 7. Expansion of farm activities

	(1) Output	(2) Expenses	(3) Investment	(4) Savings	(5) Profit	(6) Employment	(7) Self-empl.	(8) Self-consump.
Treatment	1,670.39***	503.72	-198.20	13.02	976.07**	20.03*	2.02	451.35
	(611.003)	(313.891)	(125.708)	(168.144)	(433.339)	(10.319)	(4.610)	(299.395)
Observations	5,535	5,535	5,551	5,535	5,520	5,551	5,551	5,534
Control mean	6405.893	5295.953	344.498	1984.290	1163.531	26.141	101.188	3708.529
Sample mean	7291.800	5605.015	247.529	1998.433	1678.063	36.810	102.926	3948.700
Median	1200.000	1200.000	0.000	0.000	0.000	0.000	50.000	785.000

Notes: Clustered standard errors in parentheses.

Table 8. Expansion of livestock activities

	(1) Output	(2) Expense	(3) Investment	(4) Savings	(5) Profit	(6) Employment	(7) Self-empl.	(8) Self-consump.
Treatment	709.59**	452.08	84.92	932.68*	308.91	0.31	0.48	217.09**
	(326.651)	(409.569)	(178.135)	(473.441)	(296.470)	(1.018)	(11.588)	(99.656)
Observations	5,535	5,535	5,551	5,535	5,519	5,551	5,551	5,535
Control mean	6120.813	6230.827	851.123	13701.760	-120.170	6.376	222.804	1965.918
Sample mean	6524.220	6537.070	902.044	14379.193	24.380	6.576	222.503	2076.459
Median	1496.250	1200.000	0.000	7357.500	0.000	0.000	154.286	1000.000

Notes: Clustered standard errors in parentheses.

Table 9. Effect on non-agriculltural businesses

	(1) Assets	(2) Inputs	(3) Sales	(4) Savings	(5) Profit	(6) Employment	(7) Self-empl.
Treatment	-48.15	1,370.97	1,484.16	8.22	236.39	-1.53	-1.66
	(51.151)	(1,137.993)	(1,328.282)	(50.341)	(685.189)	(1.733)	(3.592)
Observations	5,551	5,551	5,537	5,537	5,521	4,827	5,551
Control mean	93.719	8589.932	10458.148	529.334	1917.148	4.495	41.517
Sample mean	70.162	8827.181	10790.900	527.508	2031.718	3.678	40.343
Median	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Clustered standard errors in parentheses.

Table 10. Effect on income

	(1)	(2)	(3)	(4)	(5)
	From self-employment activities:			Dependent	Asset sales
	Agriculture	Livestock	Business		
Treatment	976.07**	308.91	236.39	-1,070.40**	-612.86***
	(433.339)	(296.470)	(685.189)	(452.831)	(214.634)
Observations	5,520	5,519	5,521	5,549	5,551
Control mean	1163.531	-120.170	1917.148	15623.851	669.591
Sample mean	1678.063	24.380	2031.718	15093.142	381.600
Median	0.000	0.000	0.000	10000.000	0.000

Notes: Clustered standard errors in parentheses.

Table 11. Effect on consumption

	(1) Food	(2) Schooling	(3) Health	(4) Temptation	(5) Social	(6) Total non-durables	(7) Total durables	(8) Total consumption	(9) Poverty Rate
Treatment	6.87	-0.29	6.45**	-3.28	-25.07**	-52.51	-6.96	-62.74	0.00
	(23.528)	(0.866)	(2.811)	(5.804)	(10.429)	(39.273)	(6.373)	(43.049)	(0.008)
Observations	5,540	5,524	5,524	5,524	5,524	5,524	5,524	5,527	5,476
Control mean	1780.669	23.192	40.325	293.918	420.846	2797.347	52.003	2993.807	0.158
Sample mean	1787.276	23.066	43.419	292.558	410.928	2782.130	48.102	2972.528	0.158
Median	1603.305	8.333	0.000	234.630	296.667	2442.038	0.000	2588.648	0.000

Notes: Clustered standard errors in parentheses.

Table 12. Social effects

	(1) Members in school	(2) Childwork hours	(3) Members left	(4) Membres arrived	(5) Women index	(6) % household self-activities managed by women	(7) Number women activities	(8) Women work total hours	(9) Women work hours in business
Treatment	0.03***	0.13	0.00	0.00	0.14	-0.01	-0.01	-0.29	-0.18**
	(0.010)	(0.171)	(0.003)	(0.010)	(0.197)	(0.008)	(0.013)	(0.296)	(0.075)
Observations	3,469	3,469	5,545	5,551	5,551	5,551	5,551	4,433	5,422
Control mean	0.728	4.877	0.047	0.109	-0.036	0.244	0.382	32.871	0.399
Sample mean	0.743	5.008	0.048	0.110	0.000	0.240	0.376	32.676	0.309
Median	1.000	2.667	0.000	0.000	-1.910	0.000	0.000	30.800	0.000

Notes: Clustered standard errors in parentheses.

Table 13. Heterogenous effects: conditional on activity at baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Consumption					Output	Investment	Expenses	Savings	Alamana	Income	Self-
	Total	Social	Tempt.	Dur.	Food	Total	Total	Total	Total	amount	dep. work	empl.
	Activity in livestock or agriculture or "business" present at baseline											
Treatment	-90.52*	-31.66**	-6.74	-14.03	-9.95	3,960.94**	1,582.82	2,635.12**	1,330.27**	869.20***	-1,100.87**	-1.65
	(48.871)	(12.221)	(7.023)	(8.598)	(27.055)	(1,848.416)	(1,323.372)	(1,037.563)	(617.172)	(119.155)	(475.732)	(17.482)
Observations	4,304	4,302	4,302	4,302	4,313	4,282	4,320	4,280	4,279	4,320	4,318	4,320
Control mean	3150.840	443.489	303.211	60.525	1876.273	26423.696	11351.712	21376.722	18854.380	176.628	14697.014	423.342
Sample mean	3116.605	428.204	301.288	53.926	1878.093	28390.428	11806.467	22570.917	19789.225	602.311	13905.717	422.190
Median	2717.328	301.667	238.975	0.000	1694.550	8383.125	0.000	7400.000	12755.000	0.000	7000.000	338.571
	Activity in livestock or agriculture or "business" not present at baseline											
Treatment	55.29	18.23	-4.84	14.98**	69.90*	1,710.61	1,272.70	2,190.03	795.56	440.52***	-985.59	21.16
	(69.032)	(16.176)	(11.798)	(6.246)	(40.151)	(2,235.963)	(1,568.543)	(1,703.771)	(880.412)	(110.292)	(1,148.357)	(20.024)
Observations	1,223	1,222	1,222	1,222	1,227	1,223	1,231	1,225	1,228	1,231	1,231	1,231
Control mean	2429.882	339.435	260.503	21.363	1435.519	10303.297	4579.935	6912.130	5940.520	112.111	18958.038	157.368
Sample mean	2465.491	350.110	261.824	27.600	1468.047	10012.234	3872.051	7232.212	5948.936	347.067	19258.294	167.778
Median	2232.780	270.000	208.560	0.000	1342.605	181.250	0.000	0.000	252.500	0.000	16000.000	42.857

Notes: Clustered standard errors in parentheses.