Labor market effects of improved access to credit among the poor: evidence from Cape Verde*

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Abstract

This paper investigates the impact of microfinance loans on the labor market behavior of recipient households and, in particular, their unemployed members. In the context of a collective household choice model, we show that the effects of improved credit access on search intensity by the unemployed are heterogeneous across households and dependent on the within-household bargaining power of the unemployed. We find empirical support for the predictions of our model using a household survey conducted by the authors in Cape Verde. These findings have important implications for the optimal design of microfinance programs, in particular concerning the targeting of loans and the use of microfinance as an instrument to support improved labor market outcomes in poor countries.

JEL classification codes: D13; D14; J20; O11; O12.

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1 Introduction

Microfinance products are typically designed to finance small entrepreneurial activities, aiming to improve the livelihood of their beneficiaries and generate virtuous dynamics for the entire economy. But the small businesses financed through this mechanism are rarely sufficiently large to provide occupation to all the active members of a household. Despite having access to microfinance, several households still count some unemployed members. Our paper focuses on how access to finance at the household level influences the behaviour of the unemployed household members and, thus, on whether microfinance has an impact on the labor market.

We ask this question in the context of a model of collective household choice. We propose a model of job search and entrepreneurship that characterizes intra-household allocations, within a bargaining framework, as a Pareto efficient outcome. Each household has an unemployed member and a self-employed entrepreneur. To find a job, the unemployed worker must exert a privately costly search effort. At the same time, the household invests all its net-worth in the entrepreneurial activity. Improved access to credit makes it possible to invest in a better technology and raise the return to the entrepreneurial activity.

We show that the impact of improved access to credit on costly job search by the unemployed is affected by two competing effects. Having access to finance may raise search intensity, as it increases the return to the household's net-worth and by finding a job the unemployed worker can contribute with her wage and increase the household net-worth. But, on the other hand, unemployed individuals in households with better access to finance also enjoy an income effect, which can lower the incentive to search. We show that the overall impact on job search effort by the unemployed depends crucially on the intra-household distribution of bargaining and decision power which affects both consumption and effort choices. When the bargaining power of the unemployed member is high, the net-worth effect is relatively stronger and, hence, improved access to credit is more likely to raise search effort by the unemployed.

We test the predictions of our model using a tailored household survey conducted by the authors in 2013 in Cape Verde, an island country in the west coast of Africa, as part of a study commissioned by the United Nations Development Program (UNDP). The focus of the survey was the impact of microfinance on the livelihood of the poor and, in particular, on their labor market outcomes. We make use of unique detailed individual data on labor search behavior of unemployed individuals and we find robust empirical support for the model's predictions.

Using propensity-score techniques to address selection into microfinance, we find that the effects of improved credit access on search intensity by the unemployed are heterogeneous across households and depend on the within-household distribution of bargaining power. The effect of treatment (being part of a household with a microfinance loan) on exerted effort in job search is increasing with the bargaining power of the unemployed worker.

Our analysis is subject to two important caveats that we address. The first relates to the sample selection problem pervasive in similar studies of labor market behavior (Heckman, 1979). In our specific context, we are interested in the impact of access to finance on job search, which is only observed for unemployed individuals. But, being unemployed is itself an endogenous outcome and, hence, so is sample selection. A possible approach to solve this endogenous sample selection problem would be to use an exogenous instrument causing variation in the probability of finding work without affecting search effort. But such instrument is difficult to justify given the feedback between the choice of search effort and the probability of successful search. Instead, we address this problem by adapting the methodology proposed in Lee (2009) to estimate bounds for the treatment effects. We adapt this method by stratifying our sample in terms of a composite measure of bargaining power, so as to allow for the heterogeneous treatment effects predicted by our theory.

The second caveat refers to the measure of bargaining power we constructed. Our identification strategy is based on the assumption that the proxies that we use (e.g., gender, schooling, household size) are predetermined and, thus, not affected by the treatment. Thus, the partial correlation between our proxies and the actual bargaining power should not be affected by improved access to credit. We show some evidence to corroborate this claim, which is also supported by recent studies. For example, Banerjee, Karlan, and Zinman (2015), report how six different randomized control trials found little evidence that access to credit has any substantial effect on women's empowerment, while Banerjee, Duflo, Glennerster, and Kinnan (2015) found no significant changes in education outcomes.[1]

We relate to three main areas of the existing literature. The first focuses on the development and testing of intra-household allocation models. Targeting benefits to a particular household member (for example to women, instead of men) has been shown to have a great influence on the ultimate use of the corresponding resources (Cherchye et al., 2012). Blundell et al. (2005) label this the targeting view. Since expenditure is often observed at the household level, these tests are often inferential, aimed at determining whether expenditure shares on various goods differ based on who controls income within the household. In an early contribution, Thomas (1990) shows that male and female non-labor incomes, used as proxies for within household decision power, have different

[1]Four of the six studies examined the role of microcredit and women's empowerment and three found no effect on female decision-making power or independence. Only in one project where empowerment was emphasized as part of the loan product, women are found to have enjoyed a small but significant increase in decision-making power (Angelucci et al., 2015).

impact on children health. Browning et al. (1994) look at how intra-household sharing is affected by factors such as relative age and income by focusing on expenditure in gender-specific items like clothing. Using data from South Africa, Duflo (2000) finds that the consequences of household revenue windfalls on child nutrition strongly depend on the gender of the recipient. More recently, other studies challenged this gender approach and focused on other measures of bargaining power. Ashraf (2009), by looking at couples' financial decisions in the Philippines, suggests that having control over financial decision matters more than gender. Schaner (2013), further supports this idea: for both men and women, offering cheaper and more accessible bank accounts in Kenya have different effects depending on whether recipients are above or below the median bargaining power. One clear upshot is that to analyse how individual behavior is affected by improved access to credit, it is important to model the household as a collective of individuals rather than as a single unit. We draw on this literature and use several indicators of bargaining power both in isolation and combined together in a composite measure.

Our paper is also related to the literature studying the link between finance and labour market choices. Lentz and Tranas (2005) analyse saving and job search joint choices, and show that the latter is monotonically decreasing in wealth when the utility function is separable in consumption and search effort. This implies that the job search increases with unemployment duration, since to smooth consumption the unemployed erode their wealth. Relaxing liquidity constraints, through improved access to finance or cash-on-hand programs, can also influence job search. Card et al. (2007) study the effect on unemployment duration of lump-sum severance payments made to job losers in Austria, and find that eligibility to the program significantly decrease the probability of employment. Our theoretical model draws on the results of these papers while adapting the setup to the specificities of microfinance.

A third literature we relate to concerns the effects of financial products for the poor. Pitt and Khandker (1998) and Morduch (1998) find that the participation in microcredit programs has heterogenous effects on labor supply, depending on the borrower's gender. But different randomized studies find no significant long-run effect on borrowers' labor supply measured as the number of hours worked (Banerjee et al., 2015). One exception is Augsburg et al. (2015), who report a moderate increase in teenagers' labor supply in the household business. Recently, Callen et al. (2014) look at the effects of improved access to financial services on the incentives to provide wage work. Drawing on a natural experiment in Sri Lanka, they show that improved access to saving leads households to work more, particularly on the wage market. They propose an explanation based on a standard model of intertemporal substitution of leisure, in which an increase in the return to savings affects both the intensive (hours worked) and the extensive margin (wage work versus self-employment). The effect that an increase in the return to saving raises employment is similar to the net-worth effect we propose in our paper. We contribute to this literature by proposing and documenting a link between the job search effort and the intra-household bargaining process, in a context in which poverty makes family links particularly strong.

Our analysis provides important insights on positive and normative issues concerning the design of microfinance programs and, in particular, the targeting of microcredit. When poorly targeted, improved access to finance can lower job search by the unemployed, making the overall economic impact ambiguous. This is of particular concern to policy-makers wishing to implement active labor market policies to lower unemployment and lower poverty. To improve the impact of microfinance on labour market outcomes, the screening of beneficiaries should not be solely based on characteristics of the entrepreneurial activity and of individual borrowers, but also on the characteristics of the household.

The paper proceeds as follows. Section 2 presents a simple model in which job search decisions are analyzed as collective household choices, and derives the main proposition to be tested. Section 3 describes the survey design and the data. Section 4 outlines our estimation strategy and the identification assumptions. In Section 5, we present the empirical results, while in Section 6 we propose a solution to the endogenous sample selection problem. Lastly, Section 7 concludes.

2 Credit and Labor Search: a theoretical framework

We examine the effects of improved access to credit on labor market outcomes, in particular search intensity by the unemployed. We propose a model of job search and household collective choice, in an environment with search frictions and financial constraints. There are two periods, date 0 and date 1. A household consists of a match between an entrepreneur and a wage laborer. The latter starts date 0 unemployed. As in Card et al. (2007), there are search frictions in the labor market and the unemployed worker must choose search intensity.

There are two types of households. Those with access to credit who are able to borrow from a microfinance institution (MFI) and those without access to credit. The former are able to finance an indivisible investment of size K, that raises the return to the household's entrepreneurial activity. Instead, creditless households do not have enough net-worth to purchase the investment and, hence, enjoy a lower return on their entrepreneurial activity, set to zero without loss of generality.

2.1 Job Search with Collective Household Choice

We posit a collective model of household behavior by requiring the outcomes of household choice to be Pareto efficient.[2] This is implemented by assuming that the household's problem is represented by an objective function which is a weighted sum of the private utility function of each household member; the weights may be interpreted as the bargaining power of each individual in the household as, for example, in Anderson and Baland (2002), Blundell et al. (2005) and Cherchye et al. (2012). Both members enjoy utility from consumption and the unemployed worker dislikes searching for a job.

Let t denote the household type, with $t = 0$ for households without access to credit, and $t = 1$ for households with access to credit (the treatment group). The household type is pre-determined, known at date 0. The timing is as follows: at the start of date 0, households choose the search effort of the unemployed worker, $S(t)$, and the household's contingent consumption allocations. At the end of date 0, those unemployed workers who successfully search and become employed receive their wage W, and the households with access to credit borrow from the MFI and invest the loan and all their net-worth in the high return technology. The crucial feature of our model is the assumption that the unemployed individuals who succeed in finding a job can contribute with their wage to the investment, hence reducing the size of the loan. At date 1, households receive the returns from the entrepreneurial activity, repay their loan and enjoy consumption.

The contingent consumption allocation is defined as:

$$\begin{aligned} \mathbb{C}_e(t) &= \left(\widehat{C}_e(t), C_e(t)\right), \\ \mathbb{C}_n(t) &= \left(\widehat{C}_n(t), C_n(t)\right), \end{aligned} \tag{1}$$

where $\mathbb{C}_e(t)$ is the allocation in the event that the job search is successful while $\mathbb{C}_n(t)$ is the allocation in the event that the wage laborer stays unemployed; $\widehat{C}$ is the consumption of the entrepreneur and C that of the wage laborer.

We normalize $S(t)$ to equal the probability of finding a job by the unemployed worker and always assume an interior solution, $S(t) \in (0, 1)$. Following the work by Card et al. (2007), we adopt two key simplifying assumptions: first, we assume there is a single wage rate; and second we assume

[2]See, for example, Chiappori (1992) for a seminal contribution.

that utility is separable in consumption and search effort and is represented by the utility function:

$$\begin{aligned}\mathbf{J}(S,\mathbb{C}_e,\mathbb{C}_n;t) = \alpha v(S(t)) + S(t)\left[u\left(\widehat{C}_e(t)\right) + \alpha u\Big(C_e(t)\Big)\right] \\ + (1-S(t))\left[u\left(\widehat{C}_n(t)\right) + \alpha u\Big(C_n(t)\Big)\right],\end{aligned} \tag{2}$$

where we have normalized to one the weight placed on the entrepreneur's utility so that $\alpha > 0$ represents the relative bargaining power of the unemployed worker. The function $v(\bullet)$, is a quadratic function capturing the disutility from search, and is decreasing and concave in the domain $[0,1]$. The function $u(\bullet)$ is assumed to be increasing, concave and homothetic, and to satisfy the condition $u'''(\bullet) \geq 0$.[3]

Let $R \geq 1$ be the gross return to investment, $r \in [0,1]$ be the market interest rate and A the household's financial assets at the start of date 0. In the second period, when consumption takes place, the household total resources, $Y(t)$, are given by:

$$\begin{cases} Y_e(1) = RK - (1+r)(K-A-W) & \text{if the household has a loan} \\ & \text{and the worker finds a job;} \\ Y_n(1) = RK - (1+r)(K-A) & \text{if the household has a loan} \\ & \text{and the worker does not find a job;} \\ Y_e(0) = A + W & \text{if the household does not have a loan} \\ & \text{and the worker finds a job;} \\ Y_n(0) = A & \text{if the household does not have a loan} \\ & \text{and the worker does not find a job;} \end{cases} \tag{3}$$

The problem solved by the household is represented by the program:

$$\begin{aligned}&\max_{S,\mathbb{C}_e,\mathbb{C}_n} \mathbf{J}(S,\mathbb{C}_e,\mathbb{C}_n;t), \\ &\text{subject to } \widehat{C}_i(t) + C_i(t) \leq Y_i(t), \quad i = e,n.\end{aligned} \tag{4}$$

[3]The convex marginal utility case, i.e. $u'''(\bullet) \geq 0$, plays an important role in the theory of precautionary saving (Kimball, 1990) and is a feature of the popular CRRA class of utility functions.

The optimality condition solving problem (4) are:

$$-\alpha v'\left(S\left(t\right)\right)=\left[u\left(\widehat{C}_{e}\left(t\right)\right)+\alpha u\Big(C_{e}\left(t\right)\Big)\right]-\left[u\left(\widehat{C}_{n}\left(t\right)\right)+\alpha u\Big(C_{n}\left(t\right)\Big)\right], \tag{5}$$

$$u'\left(\widehat{C}_{e}\left(t\right)\right)=\alpha u'\Big(C_{e}\left(t\right)\Big), \tag{6}$$

$$u'\left(\widehat{C}_{n}\left(t\right)\right)=\alpha u'\Big(C_{n}\left(t\right)\Big). \tag{7}$$

Since $u\left(\bullet\right)$ is homothetic and concave, conditions (6) and (7) combined imply:

$$\frac{\widehat{C}_{e}\left(1\right)}{C_{e}\left(1\right)}=\frac{\widehat{C}_{n}\left(1\right)}{C_{n}\left(1\right)}=\frac{\widehat{C}_{e}\left(0\right)}{C_{e}\left(0\right)}=\frac{\widehat{C}_{n}\left(0\right)}{C_{n}\left(0\right)}=f\left(\alpha\right)>0, \tag{8}$$

with $f'\left(\alpha\right)<0$. It follows that the optimality condition (5) can be expressed as

$$-\alpha v'\left(S\left(t\right)\right)=\hat{u}\left(C_{e}\left(t\right)\right)-\hat{u}\left(C_{n}\left(t\right)\right), \tag{9}$$

where $\hat{u}_{i}\left(C\left(t\right)\right)=u\left(C\left(t\right)f\left(\alpha\right)\right)+\alpha u\left(C\left(t\right)\right)$. It is easy to verify that if the function $u\left(\bullet\right)$ is increasing, concave and has positive third derivative, then these properties are inherited by the function $\hat{u}_{i}\left(\bullet\right)$, for any fixed $\alpha>0$.

2.2 Finance and Search Intensity

We first show that the impact that having access to micro-loans has on search intensity by the unemployed is ambiguous, as there are two competing effects. Having access to finance may raise search intensity, as it raises the return to the household's net-worth. But at the same time, the households with access to finance experience a positive income effect that lowers the incentive to search. The overall effect depends on the concavity of the utility function.

For a given bargaining power parameter α, it follows from condition (8) and the household budget constraint that

$$\begin{aligned} C_{n}\left(t\right)&=\left(1+f\left(\alpha\right)\right)^{-1}Y_{n}\left(t\right) \quad \text{and} \\ C_{e}\left(t\right)&=\left(1+f\left(\alpha\right)\right)^{-1}Y_{e}\left(t\right). \end{aligned} \tag{10}$$

Define the function

$$\Delta\left(C_{e},C_{n};t\right)=\hat{u}\left(C_{e}\left(t\right)\right)-\hat{u}\left(C_{n}\left(t\right)\right), \tag{11}$$

such that the optimality condition for the choice of search intensity (9), can be expressed as

$$-\alpha v'(S(t)) = \Delta(C_e, C_n; t). \tag{12}$$

To identify the two competing effects of finance on job search intensity, take the first-order Taylor expansion of $\Delta(C_e, C_n; t)$ around

$$\{C_e^{\bullet}(t), C_n^{\bullet}(t)\} = \left\{(1+f(\alpha))^{-1} Y_n(t), (1+f(\alpha))^{-1} Y_n(t)\right\} \tag{13}$$

and impose the budget constraint (10). This yields

$$\widetilde{\Delta}(\alpha; t) = \begin{cases} \hat{u}'\left((1+f(\alpha))^{-1} Y_n(1)\right)\left[(1+f(\alpha))^{-1}(1+r)W\right], & t=1 \\ \\ \hat{u}'\left((1+f(\alpha))^{-1} Y_n(0)\right)\left[(1+f(\alpha))^{-1} W\right], & t=0 \end{cases} \tag{14}$$

with $\widetilde{\Delta}(\alpha; t)$ that denotes the Taylor expansion of $\Delta(C_e, C_n; t)$ around (13).[4]

Ignoring higher order terms,[5] the optimality condition for the choice of search intensity can be expressed as

$$-\alpha v'(S(t)) = \widetilde{\Delta}(\alpha; t). \tag{12'}$$

Thus, the effect of treatment on search intensity is given by

$$\frac{dS(t)}{dt} = -\frac{d\widetilde{\Delta}(\alpha; t)/dt}{\alpha v''(S(t))}, \tag{15}$$

which is ambiguously signed because of $\left(d\widetilde{\Delta}(\alpha; t)/dt\right)$. On the one hand,

$$(1+f(\alpha))^{-1}(1+r)W > (1+f(\alpha))^{-1} W, \tag{16}$$

which raises $\widetilde{\Delta}(\alpha; 1)$ relative to $\widetilde{\Delta}(\alpha; 0)$, representing the net-worth effect. But, on the other hand, because $u''(\bullet) < 0$ and $Y_n(1) > Y_n(0)$, we have that

$$\hat{u}'\left((1+f(\alpha))^{-1} Y_n(1)\right) < \hat{u}'\left((1+f(\alpha))^{-1} Y_n(0)\right), \tag{17}$$

which lowers $\widetilde{\Delta}(\alpha; 1)$ relative to $\widetilde{\Delta}(\alpha; 0)$, representing the income effect; Since $v''(\bullet) < 0$, if the

[4]See Appendix A for details.

[5]This is without loss of generality, since we want to show that the change in $S(t)$ conditional on treatment is ambiguously signed. For that, it suffices to show that the first order change in $\Delta(C_e, C_n, t)$ is ambiguous.

net-worth effect dominates we have that $S(1) > S(0)$, while the opposite is true if the income effect dominates.

Whereas the impact of improved finance on search intensity is ambiguous, the model delivers a clear prediction for the relationship between the unemployed worker's bargaining power α, and the relative strengths of the net-worth and income effects.

To see this first notice that, because $v''(\bullet)$ is a constant ($v(\bullet)$ is a quadratic function), we have from (12′) that

$$\frac{S(1)}{S(0)} = \frac{\widetilde{\Delta}(\alpha;1)}{\widetilde{\Delta}(\alpha;0)} = \frac{\hat{u}'\left((1+f(\alpha))^{-1} Y_n(1)\right)(1+r)}{\hat{u}'\left((1+f(\alpha))^{-1} Y_n(0)\right)}. \tag{18}$$

We are interested in the sign of the derivative

$$\begin{aligned} \frac{\partial\left({}^{S(1)}/_{S(0)}\right)}{\partial\alpha} &= \frac{\partial}{\partial\alpha}\left[\frac{\hat{u}'\left((1+f(\alpha))^{-1} Y_n(1)\right)(1+r)}{\hat{u}'\left((1+f(\alpha))^{-1} Y_n(0)\right)}\right] \\ &= \frac{-f'(\alpha)(1+r)}{(1+f(\alpha))^2}\left[\frac{\hat{u}''(C_n(1))\,\hat{u}'(C_n(0))\,Y_n(1) - \hat{u}''(C_n(0))\,\hat{u}'(C_n(1))\,Y_n(0)}{\hat{u}'(C_n(0))^2}\right] \\ &> 0. \end{aligned} \tag{19}$$

The derivative is positive since $f'(\alpha) < 0$ and $C_n(0) < C_n(1)$, which implies that $u'(C_n(0)) \geq u'(C_n(1))$ and $u''(C_n(0)) \leq u''(C_n(1))$.[6]

Intuitively, the effect of improved access to credit on search is more positive (or less negative) for individuals with higher bargaining power because the strength of the income effect is weaker if consumption is high. In turn, when the bargaining power of the unemployed worker is high his/her consumption will be relatively high, since he/she receives a higher fraction of the household resources. Thus, the income effect is weaker and the net-worth effect dominates.

We, therefore, establish the following proposition:

Proposition 1. *The effects of improved credit access on search intensity by the unemployed are heterogeneous across households and dependent on the within-household bargaining power of the unemployed. In particular:*

1. *Being part of a household with access to a loan exerts two competing effects: the loan raises the return to job search, since finding a job raises the household's net-worth; but, receiving a loan implies a positive income effect which discourages job search. The overall effect on search*

[6] This follows from $\hat{u}''(\bullet) < 0$ and $\hat{u}'''(\bullet) \geq 0$.

intensity of an unemployed individual is ambiguous.

2. *All else equal, the search intensity of an unemployed individual who is in a household receiving a loan, relative to the search intensity of the same individual if her household did not receive the loan, is increasing in the bargaining power of the unemployed worker:*

$$\frac{\partial \left({}^{S\,(1)}/_{S\,(0)} \right)}{\partial \alpha} > 0. \tag{20}$$

The proposition suggests two observations. First, that the effect of treatment on job search is ambiguous and hence, invites further empirical investigation. Second, the model provides a clear testable prediction: as the bargaining power of the unemployed member increases, the effect of improved access to finance on search intensity should become more positive (or less negative). In the sequel, we test this prediction using the survey data that we have collected in Cape Verde.

3 Survey Design and Data

We use data from a household survey undertaken by the authors in the Isle of Santiago, Cape Verde, in 2013, as part of a broader project evaluating the impact of microfinance in the country. We begin by describing the survey design and sampling methods.

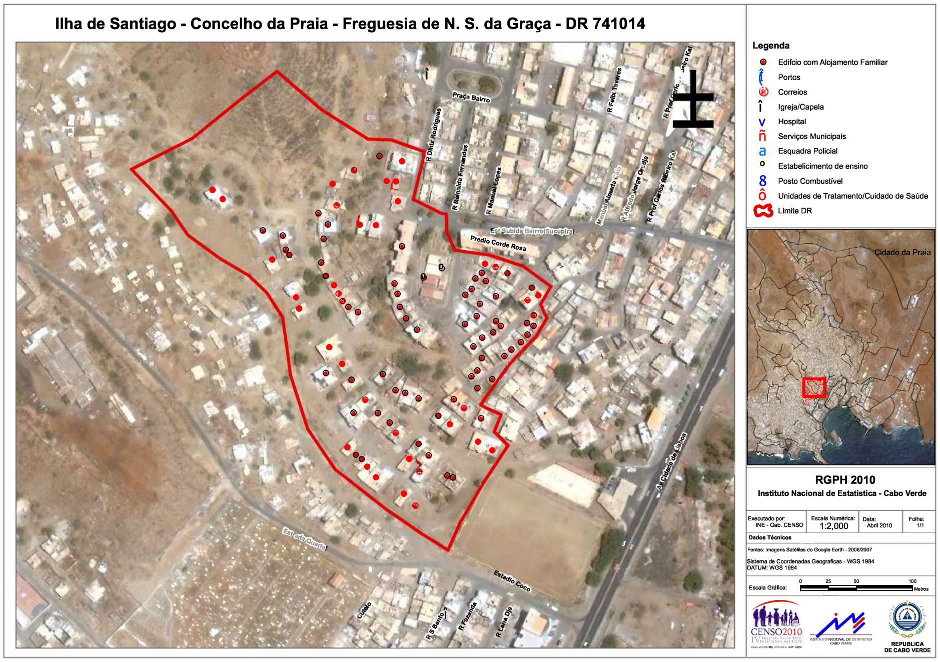
3.1 Survey Design and Sample

The original sample consists of 600 households and is obtained using a stratified random sampling technique. Since we are interested in labor market outcomes and job and business opportunities differ considerably between urban and rural settings, the dimension of stratification is whether households live in an urban or rural area. Thanks to detailed interviews to the main microfinance institutions of the country, we identified the areas where microfinance clients are more likely to reside. In the capital city, Cidade da Praia, we chose 10 neighborhoods based on their relevance for microfinance.[7] We excluded the wealthier neighborhoods and those where the employment rate is well above the national average as reported in the 2010 National Census.

Our primary sampling unit in Cidade da Praia (urban stratum) are 20 randomly selected census

[7] The neighborhoods are Fazenda, Achadinha, Várzea, Terra Branca, Safende, Achada Grande de Frente, Achada Grande de Trás, Ponta d'Água, São Felipe and Achada de Santo António.

Figure 1: Satellite picture of a census district (CD)



districts (CD) that overlap the selected neighborhoods.[8] Each CD contains 180 dwellings (and so approximately 180 households). Concerning the stratum of rural households, we choose three areas characterized by the highest population density and a large number of MFI clients, and randomly selected 10 CD in each of these areas.[9] Finally, from each CD we randomly select 20 households using maps provided by the National Institute of Statistics, like the one shown in Figure 1.[10] Given the CD design, this procedure guarantees that each household has approximately the same probability of being interviewed.

The survey elicits detailed information on demographic characteristics, income, consumption, assets and a complete recording of the financial services used by the household. In particular, we have information on all kinds of loans received by households (either from conventional commercial banks or from an MFI). Thus, we can distinguish four types of household in terms of access to credit: households without loans, households that borrowed from an MFI, households that borrowed from

[8] Census Districts are precisely delimited geographical areas, drawn for the 2010 National Census and covering the entire national territory.

[9] The selected areas are Assomada, Calheta de São Miguel and Pedra Badejo.

[10] The maps are satellite pictures that give a clear image of the border of the CD, the streets and the location of dwellings. Each dwelling is marked by a dot. The images are of high quality, but they do not allow assessing the quality, age and status of the buildings. The enumerators were asked to abide by the following protocol: *'Interview only the households in the randomly selected dwellings marked on the maps; if a dwelling turns out to be abandoned, go to the nearest one. If the dwelling hosts more than one household, select the first door to the right'*. Reassuringly, we found very few abandoned dwellings, probably because of the rapid increase of the population in the isle of Santiago. Also, very few households refused to be interviewed. This, is mainly due to the fact that many households still have a traditional structure and are formed by different cohabiting generations (often, grandparents, parents and sons), so that dwellings are rarely empty and there is always someone able to speak to visitors. Besides, it testifies to the friendliness of Cabo Verdeans.

Table 1: Characteristics of households with unemployed members

	Household access to lending							
	1: no loan		2: MFI loan		3: bank loan		4: full sample	
# of households	218		56		43		317	
Rural household (%)	30	(0.03)	29	(0.06)	26	(0.07)	29	(0.03)
Household size	5.29	(0.17)	6.14**	(0.35)	6.42***	(0.43)	5.59	(0.15)
# of children 15 or younger	1.55	(0.10)	2.04**	(0.21)	1.51	(0.22)	1.63	(0.08)
Head is woman (%)	51	(0.03)	59	(0.07)	33**	(0.07)	50	(0.03)
Age of head	49.33	(1.09)	48.46	(1.77)	52.58	(2.16)	49.62	(0.86)
Head's schooling (years)	4.68	(0.27)	4.14	(0.51)	5.58	(0.63)	4.71	(0.22)
Spouse's schooling (years)	4.78	(0.46)	5.15	(0.64)	4.79	(0.67)	4.84	(0.33)
Head is unemployed (%)	35	(0.03)	29	(0.06)	16***	(0.06)	31	(0.03)
Spouse is unemployed (%)	27	(0.03)	18	(0.05)	47**	(0.08)	28	(0.03)
members self-employed ≥ 1 (%)	26	(0.003)	48***	(0.07)	21	(0.06)	29	(0.03)
member working in hh enterprise ≥ 1 (%)	5	(0.01)	9	(0.04)	7	(0.04)	6	(0.01)
# of members unemployed	1.64	(0.06)	1.60	(0.10)	2**	(0.18)	1.68	(0.05)
# of income sources	1.69	(0.08)	1.80	(0.15)	2.21***	(0.21)	1.77	(0.07)
Total annual income p.c. (CVE)	86,700	(8,637)	74,304	(12,182)	129,169*	(22,146)	90,271	(7,032)
Poverty headcount ratio (%)	51	(0.03)	57	(0.07)	28***	(0.07)	49	(0.03)

Standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.

banks and households that borrowed from both banks and an MFI. In addition, we have detailed information on the labor market participation of each member and, in particular, on the job search effort by the unemployed and on the length of their unemployment spell.

Few restrictions are imposed on the original sample of surveyed households. First, since we are interested in the effects of improved access to credit on the search behavior of the unemployed, we drop households that have no members aged between 16 and 65 years old who are unemployed. Second, we exclude households that borrowed from both banks and an MFI.[11] We are left with a sample of 317 households. The 317 households correspond to 1,100 individuals. Among the 1,100 individuals, 620 are employed and 480 unemployed.[12] Individuals are defined as unemployed if they are between 16 and 65 and claim to be unemployed, either looking for a job or not actively engaged in search (rest unemployment).[13]

3.2 Preliminary Data Description

Table 1 describes the main characteristics of each household type, together with the results from a difference in means test between the households with no loan and those with access to finance, either through an MFI or through a bank. The frequency of types is the same in urban and rural areas,

[11] Only 12 households received loans from both an MFI and a bank.

[12] Specifically, as shown in Table 2, there are 315 unemployed individuals in households without loan, 86 in households with MFI loans and 79 in households with bank loans. Unemployed individuals, living in households that borrow from conventional banks are excluded when we estimate the treatment effect of microfinance.

[13] This is the appropriate definition of unemployment as we are interested in the effects of improved access to credit on job search effort.

Table 2: Individual level characteristics of unemployed

	Household type					
	1: no loan		2: MFI loan		3: full sample	
# of individuals	315		86		401	
Female (%)	64	(0.03)	61	(0.05)	63	(0.02)
Age	33	(0.73)	31	(1.40)	33	(0.65)
Schooling (years)	6.67	(0.24)	6.90	(0.46)	6.71	(0.21)
Owns mobile phone (%)	63	(0.03)	57	(0.05)	61	(0.03)
Owns bank account (%)	30	(0.03)	33	(0.05)	31	(0.03)
Is looking for a job (dummy) (%)	51	(0.03)	45	(0.05)	50	(0.03)
Job search intensity	0.86	(0.06)	0.71	(0.09)	0.83	(0.05)
# of initiatives to search for job	0.55	(0.04)	0.58	(0.08)	0.56	(0.04)
Unemployment duration: 1 — 6 months (%)	18	(0.02)	13	(0.04)	17	(0.02)
Unemployment duration: 7 — 12 months (%)	12	(0.02)	4**	(0.02)	10	(0.02)
Unemployment duration: 1 to 4 y (%)	32	(0.03)	33	(0.05)	32	(0.03)
Unemployment duration: more than 4 y (%)	26	(0.03)	40**	(0.05)	29	(0.02)

Standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.

indicating that there are no ex-post differences in credit access across the two strata. Looking at household size, we find that the households borrowing from either an MFI or a bank are on average of larger size than the households with no loan. Among MFI clients, the difference in size is reflected in the number of children below working age which is significantly larger.

An important indicator to understand the targeting of loans is the fraction of households in which the head is a woman. MFIs are often portrayed as targeting women and, hence, we may expect households headed by a woman to be more frequent among the MFI clients. We find that 59% of the MFI households are headed by a woman while this happens in 51% of the households without loans, but the difference is not statistically significant. However, looking at the households that borrowed from a conventional bank, we find that only 33% of them have a woman as head. Thus, for households headed by a woman, MFIs offer significantly more viable access to lending than the conventional banks. This finding confirms to some extent the widely spread notion of the MFIs targeting women.

In terms of schooling achievement, households are similar across types, with an average schooling achievement around 5 years. Another variable of interest is self-employment. MFIs in both urban and rural areas typically give out loans to finance some form of business, either formal or informal. One way to measure entrepreneurship is to look at the fraction of households with at least one member self-employed. We find that 48% of the households borrowing from an MFI have at least one member self-employed. This is substantially more than among the households borrowing from banks and those not borrowing (at, respectively, 21% and 26%). However, turning to the occurrence of work in the family business, the number of households with at least one additional member working in the family enterprise is small. Only 9% of the households with microfinance have multiple members working in the family business, not significantly different from the other groups.

The average number of unemployed members per household is 1.68, with no significant differences among microfinance and no-loan groups. But, the average number of unemployed individuals is significantly higher for households borrowing from banks, as is the number of income sources.[14] Households without loans have on average 1.69 sources of income, while the value is 1.80 for those with micro-loans. The standard errors are small, indicating very little dispersion. Thus, it is fair to say that the stylized representation of the household in Section 2, as a match between an entrepreneur and an unemployed worker, is not far from the typical household in our sample. On average, households have one or two members unemployed and one or two sources of income.

The incidence of poverty is pervasive in our sample, in particular among the households with no access to finance and with micro-loans, our core sample. This is confirmed by the poverty head-count, showing 28% of households with bank loans below the poverty line, with the share rising to 57% among MFI borrowers and 51% among households with no loans.[15]

Table 2 reports individual characteristics of interest of the unemployed individuals aged between 16 and 65 that are members of households in the treatment and in the control group (hence, after the exclusion of households borrowing from banks). It is interesting to notice that unemployed individuals from households with no loans are very similar to unemployed individuals from MFI households, especially for job search measures.[16] Another interesting observation is that there is a significantly higher share of long-term unemployment (unemployment spells longuer than 4 years) among members of households borrowing from an MFI, but lower medium term unemployment.

[14] Families with bank loans have higher income than families borrowing from the MFI. The higher unemployment rate among this group suggests that leisure is a normal good. This is consistent with our definition of unemployment, that includes rest unemployment.

[15] Since no recent information in this respect is available, we updated the 2007 national poverty line (World Bank, 2007) by taking into account the inflation over the period 2007-2013. We attain an income value of 55,319 CVE per capita per year which is roughly equivalent to 2 US$ per capita per day in PPP. Households are considered poor if their income per capita per day is lower than 2 US$.

[16] The job search measures constructed are the following three: *Looking for a job*; *Labor search intensity*; and *Number of initiatives to search for work*. The first, *Is looking for a job*, is a discrete variable taking value 1 if the unemployed individual has taken any initiative to find a job in the previous four weeks and 0 otherwise; The second, *Labor search intensity* is an ordinal variable capturing the intensity of labor search. It takes value 0 if the individual did not take any initiatives to find job; 1 if the individual searched a job on the internet, asked help from friends, family or worker union, or registered in a job center; and 2 if she asked an employer for work, took part in a job selection process or responded to a job offer. The rational of this variable is to rank the amount of effort needed for the different types of initiatives in the context of Cape Verde. *Number of initiatives to search for work* is a cardinal variable taking the values 0, 1, 2 or 3 depending on the number of different initiatives taken to find a job.

Table 3: Loan characteristics of treated households

	Descriptives
# of households	56
Number of loans per hh	1,7
Female clients (%)	83
Main use of the loan: business related (%)	82.5
Loan size (average) (CVE)	62,200
Loan size (median) (CVE)	50,000
Loan duration (months)	8
Default rate (%)	8
Difficulty to repay (%)	6

Finally, some characteristics of the micro loans taken by the treated households are displayed in Table 3. First, it can be seen that women represent a high proportion of clients, at 83%. A microfinance loan was taken for business related purposes in 82.5% of the cases (to start a business, expand it, buy goods for petty trade or buy work equipment), which is consistent with the intrinsic objective of microfinance to promote entrepreneurship. Looking at the loan size, we see that the distribution is right skewed, the average amount lent being higher than the median.[17] Looking at loan performance, only 8% of clients defaulted and 6% had difficulties in repaying the loan.[18]

4 Estimation Strategy and Identification Assumptions

In this Section we introduce the econometric model used to assess the effects of improved access to credit on job search. The main purpose of the analysis is to test Proposition 1 and, in particular, the prediction in Equation (20). We estimate the impact of borrowing from an MFI on the job search effort by the unemployed household members: the Average Treatment Effect for the Treated (ATT).

Formally, let $S(t)$ be the outcome, job search effort, and t be the indicator of treatment, with $t = 1$ if the household has received a microfinance loan and $t = 0$ if it has not. We wish to evaluate the difference between the outcome of individuals in treated households and the counterfactual outcome of the same group of individuals had they not received the loan, given a vector X of observable characteristics

$$\text{ATT} = E\left[S(1) \mid X, t = 1\right] - E\left[S(0) \mid X, t = 1\right]. \tag{21}$$

[17] The median loan size is equal 588 $US. 1 US dollar is roughly equal to 85 Cape Verdean Escudos (CVE), the national currency.

[18] A loan is considered in default if it is still ongoing 3 months after the due date.

The evaluation problem lies in the fact that, for each individual, only one of either $S(1)$ or $S(0)$ is observed. In particular, for individuals in households with $t = 1$, only $S(1)$ is observed. We need a counterfactual that is based on the observable outcome of the non-treated households, constructed in such a way to be as close as possible to the potential outcome for treated households in the absence of treatment. The obvious candidate is the outcome of individuals in the non-treated households, $E[S(0) \mid X, t = 0]$. However, an evaluation based on differences in means is subject to various sources of bias when treatment is not randomly assigned and is, instead, determined by household characteristics such as schooling, entrepreneurial spirit and ability.

In their seminal paper, Rosenbaum and Rubin (1985) propose a method that corrects for potential selection and omitted variable bias in estimating ATT. They show that, under the conditional independence assumption (CIA), adjusting for differences between treated and control units in the propensity score removes all biases associated with differences in the observed covariates in the treated and control groups. The propensity score is defined as the conditional probability of receiving an MFI loan:

$$p(X_j) = \text{Prob}\ (t_j = 1 \mid X_j)\,, \tag{22}$$

where X_j are observed covariates, which are assumed to be pre-determined.[19] For each household j, the estimated propensity score $\widehat{p}(X_j)$ is estimated based on a range of observable pre-program household characteristics, collected in the vector X_j. Access to credit is modeled at the household level as we want to distinguish between the households who borrow from the MFI and those that do not.

Hirano et al. (2003) extend Rosenbaum and Rubin (1985)'s result and show that, under the conditional independence assumption, weighting observations by $\widehat{p}(X_j) / (1 - \widehat{p}(X_j))$ for the control units, where $\widehat{p}(X_j)$ is a consistent estimator of $p(X_j)$, and by unity for the treated units, leads to an efficient estimator of the ATT. The intuition is that the control households with observables very similar to the treated households are assigned higher weights, while those relatively more dissimilar are assigned lower weights. The weighs function is given by

$$\omega(t_j, X_j) = t_j + (1 - t_j)\frac{\widehat{p}(X_j)}{1 - \widehat{p}(X_j)}. \tag{23}$$

The construction of the weights ensures that, under the CIA, the treatment and the potential outcomes are independent conditional on the probability of receiving treatment and, hence, the

[19]The CIA or unconfoundedness property requires that, conditional on the covariates X, receiving treatment is independent of the potential outcome with and without the treatment, $S(1)$ and $S(0)$. This implies not only that participation in the program is based entirely on observed characteristics, but also that average differences in outcomes between treated and control units with the same observed characteristics are attributable to the treatment, so that $t \perp S(1), S(0) \mid X$.

weighted estimator is consistent.

We follow Imbens (2004)'s suggestion of combining weighting methods with added covariates. This is particularly useful to evaluate the impact of the treatment but also of other covariates and their interactions. In particular, we estimate the following equation:

$$S_{ij} = Z_{ij}\beta_0 + t_j\beta_1 + t_j\alpha_i\beta_2 + \varepsilon_{ij}, \tag{24}$$

where S_{ij} refers to the labor search behavior of individual i in household j, t_j denotes the treatment, defined as the household j having received at least one microfinance loan since 2010, and α_i is a proxy for individual i's within-household bargaining power. Individual and household level controls are collected in Z_{ij}.[20]

It is worth to notice that, although the theoretical model assumes that all unemployed workers earn the same wage if employed and generally enjoy the same labor market opportunities, in reality individuals will differ in their productivity and human capital levels. These individual characteristics are also likely to be related to bargaining power. Thus, one might worry about consistent estimation of β_1 and β_2 if the covariates affecting bargaining power α_{ij} also affect the labor market opportunities and, hence, incentives to search. However, these concerns are addressed directly, by including in the vector of controls Z_{ij} the covariates related to the individual's bargaining power such as education and gender that also affect labor market opportunities.

Testing Proposition 1 and, in particular, Equation (20) boils down to testing the null hypothesis that β_2 is positive. A positive and significant β_2 would confirm the presence of heterogeneous treatment effects, increasing in the bargaining power of the unemployed. In turn, as implied by the first part of Proposition 1, we have no theoretical prediction concerning the sign of β_1. Nonetheless, estimating β_1 precisely will reveal how the targeting of credit to poor households contributes to improving labor market outcomes.

5 Microfinance and Job Search: Empirical Findings

We now turn to the empirical analysis to compare the theoretical predictions of our model with the data. The first step, described in Section 5.1 is to model the probability of receiving a microfinance loan at the household level and estimate the propensity scores. Then, in Section 5.2 and onwards,

[20] This estimator is said to be "double robust" since weighting by the propensity scores and the inclusion of the covariates Z_{ij} both contribute to guaranteeing that t_j and ε_{ij} are independent (Imbens, 2004). See bottom of Table 6 for a complete list of the control variables.

Table 4: Multinomial probit model at the household level (first stage)

	MFI Loan (1)		Bank Loan (2)	
# of hh members	0.103	(0.081)	0.267***	(0.087)
# of children 15 or younger	0.080	(0.131)	-0.227	(0.142)
Hh owns house	−0.613**	(0.307)	0.636	(0.421)
Hh has family abroad	0.718**	(0.317)	0.329	(0.309)
# of times per week reads journal	0.215	(0.291)	0.233	(0.274)
Head - primary school	0.081	(0.672)	0.657	(0.630)
Head - high-school	−0.155	(0.818)	0.694	(0.746)
Head - college	0.353	(1.068)	2.485***	(0.868)
Parent of head was self-employed	0.416	(0.325)	0.656*	(0.362)
Head has a partner	−0.073	(0.310)	0.862**	(0.353)
Head is separated	0.273	(0.795)	−10.715***	(0.704)
Head is widower	−0.150	(0.503)	0.960	(0.626)
Head can read or write	−1.154*	(0.603)	−0.753	(0.499)
Head is from Santiago	0.349	(0.332)	0.577	(0.395)
Head is foreigner	0.660	(0.906)	−11.086***	(0.958)
Head is woman	−0.285	(0.316)	−0.562*	(0.334)
Age of head	0.041	(0.056)	0.035	(0.059)
Age of head squared	−0.001	(0.001)	−0.001	(0.001)
Constant	−2.947**	(1.501)	−4.870***	(1.621)
Log pseudo likelihood				−211.706
Wald Chi2				5485.790
Prob > Chi2				0.000
Neighborhood fixed effects			yes	
Observations			317	

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.

we use the propensity scores as weights in the individual level regressions, with weights as in Equation (23).

5.1 First Stage: Estimation of the Propensity Scores

Our focus is on borrowing by the poor (the households targeted by the MFI). Hence, we exclude the households that borrow from conventional banks when we estimate Equation (24). However, to have a complete model of access to credit it is important in the first stage to include the households borrowing from conventional banks and to distinguish them from households who borrow from the MFI.[21]

Specifically, we estimate a multinomial probit model allowing for three possible household statuses: receiving a loan from an MFI, receiving a loan from a bank and not receiving any loan.[22] The

[21] This avoids obtaining a biased estimation of the propensity score due to model miss-specification. For example, some households may be without a loan but able to borrow from conventional banks if they need to. These households have very different characteristics from those of poor households unable to borrow. It is, therefore, important to include borrowing from a conventional banks as a possible household status when modeling access to credit.

[22] While being computationally heavier, the multinomial probit model is based on weaker assumptions than the

Table 5: Balancing of covariates after propensity score weighting

Variable	Treated	Control	$p >\mid t \mid$
Household level characteristics (first stage covariates)			
# of hh members	6.074	6.176	0.847
# of children 15 or younger	1.963	2.198	0.448
Hh owns house	0.611	0.591	0.830
Hh has family abroad	0.685	0.627	0.526
# of times per week reads journal	0.148	0.168	0.836
Head - primary school	0.352	0.362	0.917
Head - primary school	0.222	0.213	0.907
Head - high-school	0.056	0.060	0.914
Head - college	0.019	0.022	0.887
Parent of head was self-employed	0.204	0.177	0.725
Head has a partner	0.389	0.308	0.381
Head is separated	0.037	0.039	0.953
Head is widower	0.074	0.064	0.831
Head can read or write	0.630	0.675	0.623
Head is from Santiago	0.833	0.843	0.898
Head is foreigner	0.037	0.045	0.832
Head is woman	0.574	0.590	0.869
Age of head	48.481	46.825	0.517
Individual level characteristics			
Female (dummy)	0.560	0.672	0.254
Schooling (years)	7.320	6.073	0.136
Age	31.200	33.168	0.425
Father School (dummy)	0.578	0.567	0.923
Head (dummy)	0.120	0.188	0.351
Unemployment duration			
Unemp. Duration: 7–12 m	0.020	0.108	0.073
Unemp. Duration: 1 to 4 y	0.400	0.368	0.745
Unemp. Duration: > 4 y	0.340	0.308	0.736

identification of the causal impact of the treatment is based on the assumption that allocation of the treatment is purely random among households with the same estimated propensity score, conditional on the pre-treatment characteristics. Therefore, the participation equation includes variables that influence participation but are not affected by the treatment. The estimated model is shown in Table 4.

multinomial logit. In particular, it does not rely on the independence of irrelevant alternatives assumption which allows for the correlation of household access to each available category. See Imbens (2000), Lechner (2001) and Caliendo and Kopeinig (2008) for a discussion of modeling propensity scores in multiple discrete dependent variables settings.

Figure 2: Propensity score distribution

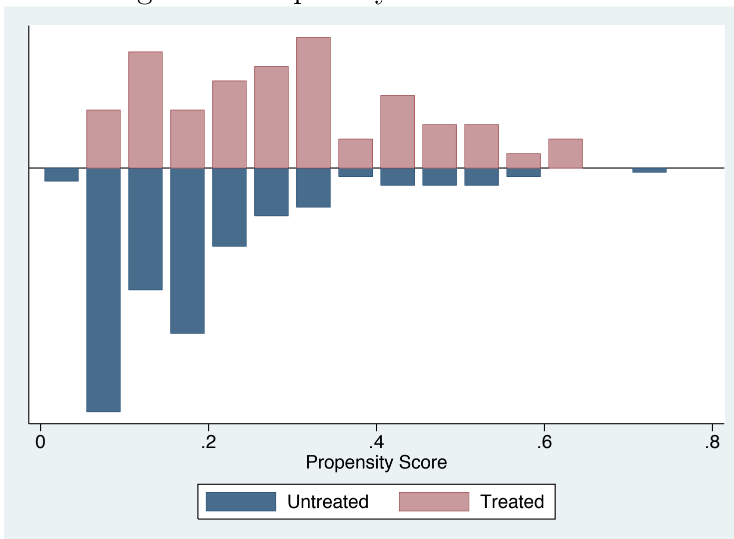


Figure 2 gives the kernel density of the estimated propensity scores for treated and non-treated households. There is substantial overlap in the distribution of the propensity scores of both treated and non-treated households. However, to better enforce the common support condition, in what follows we exclude individuals in treated households whose probability of treatment exceeds the maximum probability among the untreated households, and individuals in the untreated households whose probability of participating is below the minimum probability of participation of treated households. The upshot is that we keep observations with propensity scores such that $0.038 \leq \widehat{p}(x) \leq 0.704$.

Finally, Table 5 looks at the balancing of covariates after the propensity score weighting, including $p-$values for t tests of differences in mean. The first panel (Household level characteristics) includes the covariates that are part of the first stage model. The second panel of the table (individual level characteristics) includes individual level covariates that are not used to obtain the propensity scores. Notice that, even if this covariates were unbalanced across the treatment and control groups, this would not pose a threat to identification because the individual level covariates are included as control variables in the second stage. Lastly, the third panel looks at the average unemployment duration for the treatment and control groups, which is also used as a control variable in the second stage model. Based on the $p-$values, we confirm that balancing is successful for the vast majority of covariates.

5.2 Second Stage: Labor Search Model

We now turn to the estimation of the main regression equation (24). The survey asks households if they have ever received a microfinance loan, how many times and when. The treated group, $t_j = 1$, are those households that received at least one microfinance loan since 2010. We exclude from the sample the households who received the last loan earlier because they may no longer have access to microfinance.[23] In turn, households who never had any loan (either commercial loan or from an MFI) are used to form the control group, for which $t_j = 0$. The resulting sample in our main regression analysis includes 262 unemployed individuals, leaving in 191 distinct households.

The dependent variable, S_{ij}, is the job search effort by the unemployed. We use three alternative measures of labor search effort.[24] Firstly, we use a discrete variable taking value 1 if the unemployed individual has taken any initiative to find a job in the previous four weeks and 0 otherwise. We call the regression equation with this dependent variable Model 1. Secondly, we use an ordered discrete variable capturing the intensity of labor search, denoted Model 2. Finally, we use an ordered discrete variable corresponding to the number of initiatives taken to search for a job, denoted Model 3.

We want to test Proposition 1 and, in particular, Equation (20) predicting that the search intensity of an unemployed member of a household with an MFI loan compared to that of an unemployed in a household with no loan increases in the individual's bargaining power α_i. But bargaining power is unobservable and, hence, must be proxied by some observable variables. The literature has proposed several measures of bargaining including income, employment, asset ownership and assets brought to marriage.[25] In our context, we must also make sure that our measure of bargaining power is exogenous and, in particular, unaffected by the treatment.[26] But, as long as the partial correlation between our predictors of bargaining power and the actual bargaining power is not affected by improved access to credit we can assume that our measure of bargaining power is structural, in the sense that it is not affected by the treatment.

We use the following variables: *Household size*, assumed negatively correlated with bargaining power since, all else equal, the larger the household, the lower the share of resources received by each member; *Gender*, which should capture the disadvantages often faced by women documented

[23] In the model, unemployed workers may decide to search more intensively to raise the household's net-worth and lower the size of the loan needed to be able to invest. So the treated households are those that are going to borrow from an MFI. Instead, given the nature of our survey we must identify the treated households as those who have already borrowed from an MFI. We think this is justified because the typical use of the MFI loans is to finance working capital (see Table 3), and households have repeated interaction with the MFI institution. Thus, households who have borrowed in the past are likely to borrow again so that the net-worth channel described in the model is relevant. However, if the household did not borrow from an MFI for a long time, this assumption is no longer appropriate. This is why we exclude households that did not borrow from an MFI since before 2010.

[24] See footnote 16.

[25] Several of these proxys are discussed in, for example, Lundberg and Pollak (1996), Quisumbing and de la Brière (2000), Friedberg and Webb (2006) and Doss (2013).

[26] Something which we do not explore in this paper is how bargaining power is determined. However, the bargaining power of each household member may not be invariant to the set of investment opportunities available to each member. This point is made by Tassel (2004). However, Banerjee et al. (2015) reporting on the results from six randomized control trials (RCT) on microcredit find little evidence that accessing microcredit has any substantial effect on womens empowerment, for instance.

Table 6: Access to microcredit and labor search (Model 1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	0.026	−0.932**	0.830	−0.279	−2.097***	−2.868***	−0.905**	−1.024**
	(0.310)	(0.446)	(0.806)	(0.486)	(0.730)	(0.613)	(0.438)	(0.416)
MFI × Household Size			−0.268**					
			(0.106)					
MFI × Female (dummy)				−1.002*				
				(0.583)				
MFI × Schooling (years)					0.179**			
					(0.070)			
MFI × Father School (dummy)						2.906***		
						(0.668)		
MFI × Head (dummy)							2.559***	
							(0.959)	
MFI × Bargaining Power PC								0.981***
								(0.355)
Household size	0.155**	0.183**	0.319***	0.170*	0.167**	0.165*	0.146*	0.133
	(0.066)	(0.090)	(0.109)	(0.089)	(0.084)	(0.085)	(0.084)	(0.087)
Female (dummy)	−0.706***	−0.480	−0.298	−0.106	−0.566*	−0.513	−0.445	−0.476
	(0.216)	(0.336)	(0.358)	(0.355)	(0.334)	(0.321)	(0.332)	(0.328)
Schooling (years)	−0.002	0.054	0.046	0.064	−0.035	0.063	0.035	0.007
	(0.031)	(0.046)	(0.046)	(0.045)	(0.051)	(0.045)	(0.047)	(0.048)
Father School (dummy)	−0.035	−0.293	−0.470	−0.320	−0.250	−1.621***	−0.307	−0.586*
	(0.218)	(0.300)	(0.302)	(0.302)	(0.301)	(0.394)	(0.300)	(0.313)
Head (dummy)	−0.055	0.935**	1.162***	0.824*	0.722	0.642	0.526	0.755*
	(0.314)	(0.452)	(0.442)	(0.443)	(0.483)	(0.441)	(0.518)	(0.450)
Unemp. duration: 7 - 12 m	0.868***	0.814	0.670	0.837*	1.070**	0.986*	0.784	1.128**
	(0.327)	(0.550)	(0.516)	(0.505)	(0.481)	(0.508)	(0.515)	(0.502)
Unemp. duration: 1 to 4 y	0.612***	0.516	0.415	0.562*	0.470	0.691**	0.438	0.619*
	(0.227)	(0.314)	(0.294)	(0.290)	(0.304)	(0.335)	(0.319)	(0.329)
Unemp. duration: > 4 y	−0.645**	−0.317	−0.388	−0.330	−0.398	−0.324	−0.471	−0.290
	(0.262)	(0.355)	(0.339)	(0.349)	(0.359)	(0.351)	(0.359)	(0.369)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R-squared	0.306	0.584	0.598	0.591	0.602	0.642	0.598	0.607

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Country or Island of Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

in numerous studies of intra-household resource allocation; *Schooling*, assumed positively correlated with the bargaining power, since human capital affects individual outside options; *Father's schooling*, assumed positively correlated with the bargaining power of their offspring, for instance, in the marriage market; *Role of the individual in the household*, since we expect the head of the household to have a higher bargaining power. The fact that these covariates are balanced across the treatment and control groups after the propensity score weighting (panel 2 of Table 5) supports the assumption that these are structural measures of bargaining power.

5.3 Access to Credit and Job Search: Baseline Findings (Model 1)

We now turn to the paper's main empirical findings. Table 6 shows the results of Model 1. For all regressions reported below we account for the dependence between observations by computing robust standard errors.[27] We first estimate the model without re-weighting and without allowing for heterogeneity in the treatment effects, reported in Column 1. This amounts to estimating a standard Probit model. We compare the estimated coefficient β_1 with the one obtained using the inverse probability weighting (IPW), reported in Column 2.

While the unweighted β_1 coefficient is positive (but not statistically significant), the IPW estimate is negative and statistically significant and, hence, there is evidence of a positive selection bias. This is what we would expect if, for example, the MFI are able to select households where the unemployed are more diligent in searching for work. The sign of the IPW coefficient implies that the average treatment effect of MFI lending on the incentives for job search by the unemployed is negative. However, given Proposition 1, we are especially interested in the interaction between household access to credit and individual bargaining power, which is what we look at next.

We allow for heterogeneous treatment effects by interacting the treatment variable with our proxies for bargaining power (Columns 3 to 7). There is robust evidence of heterogeneous treatment effects consistent with Proposition 1. All interaction coefficients are significant and of the expected sign. Being a woman, as well as being a member of a larger household, which are both associated with smaller bargaining power, lower the treatment effect. Instead the effects of own schooling and father's schooling, and of being the household head, each associated with a larger bargaining power, is positive and precisely estimated. For all except one specification (in Column 3), the coefficient β_1 is estimated to be negative.

The negative coefficient estimate for β_1 in Column 2 indicates that, given the distribution of the bargaining power in our sample of unemployed workers and the size of the β_1 and β_2 coefficients, there is an implied negative average treatment effect of improved access to credit on search effort by the unemployed. This finding suggests that there is scope for improving labor market outcomes by better targeting microfinance programs. In particular, improving the access to credit by households whose unemployed members are more likely to exhibit positive treatment effects on job search, which are those with stronger bargaining power, can potentially improve aggregate labor market outcomes in frictional markets. This is a hitherto unexplored channel through which microfinance may have aggregate benefits.

[27] The findings are unaltered if, instead, we consider clustering the standard errors at the neighborhood level, as reported in Appendix B.

5.4 A Composite Measure of Intra-household Bargaining Power

Each of our proxies for bargaining power captures a different underlying feature of the intra-household distribution of resources and is, therefore, a partial measure. In order to construct a more comprehensive measure of bargaining power, we use a Principal Component Analysis (PCA) that aggregates the information scattered in the different proxies.[28] Since, for the PCA method to be valid, the included variables should have a multivariate normal distribution (or at least be continuous), and since we want to include a combination of dichotomous and continuous variables (gender, own and father's schooling, age, and household size), we perform a polychoric correlation analysis (Kolenikov and Angeles, 2004).

This is implemented as follows. The pairwise correlations between variables are estimated based on the nature of the variable: Pearson moment correlation if the two variables are continuous, Polychoric correlation if the two variables are ordinal and Polyserial correlation if one variable is ordinal and the other continuous. This allows us to run a principal component analysis on the resulting correlation matrix and interpret the first principal component as an index of bargaining power. For Model 1, results are presented in Column (8) of Table 6 and confirm the theoretical predictions: the treatment effect on job search intensity is increasing with the intra-household bargaining power of the unemployed worker.

5.5 Alternative Measures of Search Effort (Model 2 and 3)

Next, we replicate the empirical analysis with our alternative measures of job search effort: the labor search intensity (Model 2) and the number of labor search initiatives (Model 3).

The estimation results from Model 2 are shown in Table 7. In this case, the dependent variable is an ordered discrete indicator of the intensity of job search, hence we run an ordered Probit model. In turn, the specification of the regression equation and the computation of the regression weights are as in the baseline model. Looking at the weighted regression estimates of the coefficient β_2 for each measure of bargaining power, the estimated coefficient have the same sign and are precisely estimated.

Finally, in Model 3, we use as a dependent variable the discrete indicator of the number of job search initiatives undertaken by the unemployed. The results are shown in Table 8. The precision of the estimates for this specification is lower. Nonetheless, the evidence of heterogeneous treatment

[28] See Filmer and Pritchett (2001) for an early and influential paper in development economics and population studies constructing socio-economic indices using PCA.

Table 7: Access to microcredit and labor search (Model 2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	−0.060	−0.780*	1.528*	−0.139	−2.167***	−2.787***	−0.750*	−1.028***
	(0.265)	(0.400)	(0.837)	(0.434)	(0.667)	(0.575)	(0.383)	(0.395)
MFI × Household Size			−0.349***					
			(0.116)					
MFI × Female (dummy)				−1.140**				
				(0.569)				
MFI × Schooling (years)					0.199***			
					(0.065)			
MFI × Father School (dummy)						3.048***		
						(0.640)		
MFI × Head (dummy)							1.397*	
							(0.836)	
MFI × Bargaining Power PC								1.201***
								(0.376)
Household size	0.012	−0.117	0.016	−0.109	−0.104	−0.094	−0.128	−0.124
	(0.055)	(0.089)	(0.098)	(0.089)	(0.083)	(0.0818)	(0.0865)	(0.0845)
Female (dummy)	−0.727***	−0.547*	−0.268	−0.192	−0.631**	−0.380	−0.506*	−0.507*
	(0.195)	(0.296)	(0.314)	(0.336)	(0.299)	(0.290)	(0.304)	(0.286)
Schooling (years)	0.024	0.084**	0.070*	0.094**	−0.004	0.080**	0.066	0.032
	(0.027)	(0.041)	(0.040)	(0.040)	(0.046)	(0.039)	(0.041)	(0.041)
Father school (dummy)	0.113	0.089	−0.322	−0.032	0.087	−1.245***	0.138	−0.351
	(0.191)	(0.298)	(0.307)	(0.303)	(0.291)	(0.360)	(0.297)	(0.309)
Head (dummy)	−0.032	0.729*	1.200***	0.591	0.261	0.609	0.239	0.530
	(0.290)	(0.409)	(0.436)	(0.393)	(0.437)	(0.377)	(0.536)	(0.388)
Unemp. duration: $7-12$ m	0.601**	−0.053	−0.025	−0.081	0.521	0.099	0.043	0.387
	(0.276)	(0.410)	(0.416)	(0.391)	(0.421)	(0.388)	(0.385)	(0.412)
Unemp. duration: 1 to 4 y	0.414**	0.590*	0.676**	0.652**	0.591*	0.861**	0.540	0.743**
	(0.210)	(0.336)	(0.339)	(0.321)	(0.334)	(0.342)	(0.341)	(0.343)
Unemp. duration: >4 y	−0.562**	−0.188	−0.035	−0.193	−0.236	−0.185	−0.329	−0.135
	(0.253)	(0.371)	(0.394)	(0.370)	(0.378)	(0.359)	(0.373)	(0.375)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R−squared	0.206	0.419	0.441	0.429	0.443	0.482	0.426	0.453

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Country or Island of Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

effects remains consistent with Proposition 1, in particular for the schooling of the father, the dummy indicating that the individual is the head of the household and the composite measure of bargaining power. We conclude that the evidence of heterogeneous treatment effects consistent with the theoretical prediction are robust to changes in the measure of search effort.

Table 8: Access to microcredit and labor search (Model 3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	0.171	−0.379	0.500	0.134	−0.976	−2.042***	−0.332	−0.597
	(0.273)	(0.360)	(0.769)	(0.440)	(0.761)	(0.513)	(0.342)	(0.376)
MFI × Household Size			−0.130					
			(0.097)					
MFI × Female (dummy)				−0.897				
				(0.613)				
MFI × Schooling (years)					0.081			
					(0.078)			
MFI × Father School (dummy)						2.408***		
						(0.604)		
MFI × Head (dummy)							1.698**	
							(0.698)	
MFI × Bargaining Power PC								0.697**
								(0.353)
Household size	0.053	−0.033	0.022	−0.0223	−0.031	−0.016	−0.058	−0.039
	(0.055)	(0.068)	(0.0816)	(0.067)	(0.067)	(0.065)	(0.069)	(0.066)
Female (dummy)	−0.598***	−0.297	−0.175	0.009	−0.348	−0.096	−0.238	−0.279
	(0.195)	(0.300)	(0.292)	(0.308)	(0.310)	(0.299)	(0.308)	(0.305)
Schooling (years)	0.023	0.053	0.047	0.061	0.016	0.051	0.036	0.021
	(0.025	(0.041)	(0.042)	(0.039)	(0.046)	(0.040)	(0.041)	(0.043)
Father School (dummy)	0.006	0.054	−0.082	−0.082	0.053	−1.041***	0.053	−0.206
	(0.178)	(0.234)	(0.234)	(0.259)	(0.235)	(0.300)	(0.229)	(0.255)
Head (dummy)	0.025	0.986***	1.147***	0.902**	0.801*	0.844**	0.444	0.856**
	(0.283)	(0.381)	(0.404)	(0.365)	(0.432)	(0.344)	(0.475)	(0.366)
Unemp. duration: 7 − 12 m	0.506*	0.241	0.221	0.322	0.477	0.447	0.315	0.569
	(0.273)	(0.466)	(0.477)	(0.468)	(0.522)	(0.504)	(0.464)	(0.511)
Unemp. duration: 1 to 4 y	0.471**	0.203	0.200	0.298	0.204	0.417	0.157	0.307
	(0.200)	(0.370)	(0.381)	(0.372)	(0.377)	(0.396)	(0.378)	(0.394)
Unemp. duration: > 4 y	−0.494**	−0.038	−0.022	−0.040	−0.047	−0.036	−0.201	0.017
	(0.244)	(0.408)	(0.417)	(0.412)	(0.412)	(0.409)	(0.414)	(0.429)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R−squared	0.207	0.381	0.386	0.389	0.386	0.434	0.393	0.396

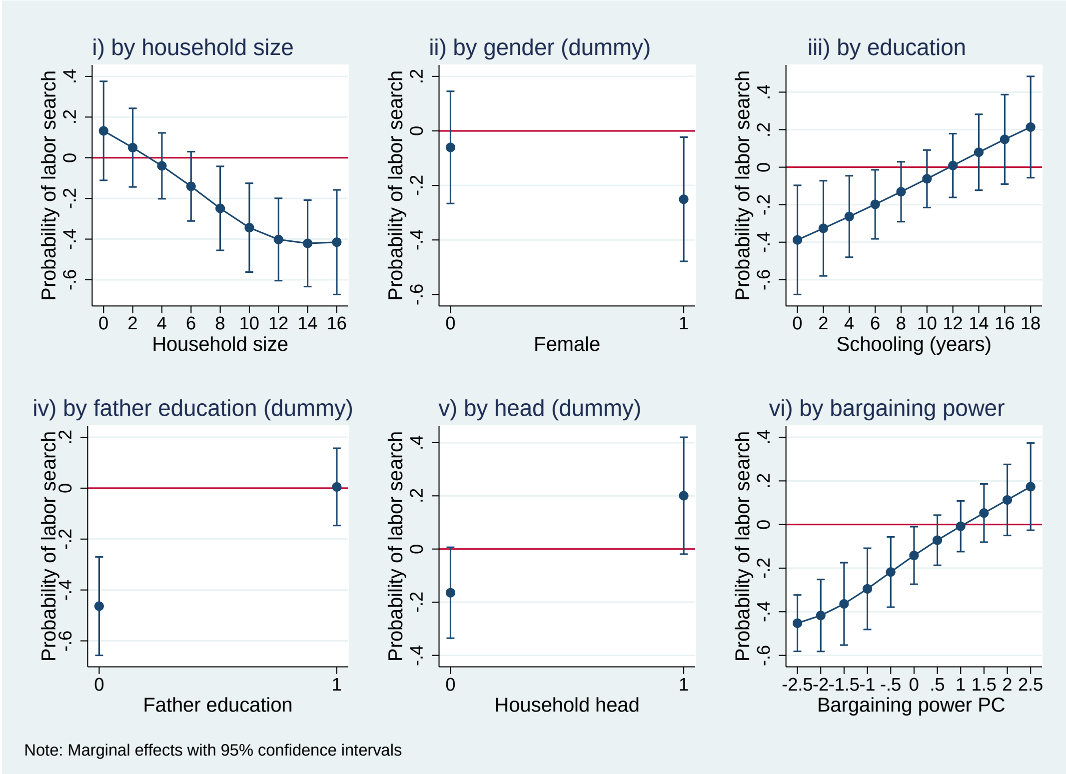
Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy).Additional individual level controls: Age, Age2, Country or Island of Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

5.6 Marginal Effects

To gain a better sense of the magnitude of the heterogeneous treatment effects estimated under the baseline specification (Model 1), we compute the marginal effect of each interaction term. These are shown in Figure 3, together with their confidence intervals.

Each panel shows the treatment effect on job search as the bargaining power, measured by each proxy, changes. Take first the upper left panel of Figure 3. It reports the marginal effect on job

Figure 3: Average marginal effects across bargaining power



search as the individual's household size increases. The maintained assumption is that, all else equal, an unemployed individual in a large household receives a low share of resources and, hence, has a lower decision weight α_i. The figure shows that receiving an MFI loan lowers the probability of unemployed workers searching for jobs if the household size is greater than 3. The estimated negative marginal effect is statistically significant for unemployed individuals living in household of 8 members or more, with individuals 40% less likely to be searching for work.

In turn, Table 9 shows the distribution of each bargaining power proxy among treated households. This information allows measuring the fraction of unemployed leaving in treated households for which the treatment is predicted to raise job search. For instance, only around 5% of the households with an MFI loan have fewer than 4 members. Thus, in most treated households, the bargaining power of the unemployed individuals is judged low based on household size. The upshot is that for those individuals, the treatment effect on job search effort is negative.

The implications of this result for the optimal design of microfinance programs are interesting. The size of the household and, in particular, the number of children may be positively correlated with poverty. Hence, targeting large families may be desirable for the MFI with social objectives.[29]

[29]On the other hand, there is some evidence that larger families are not necessarily poorer. For instance, Lanjouw and Ravallion (1995) show that taking into account scale economies can substantially lower poverty estimates among large families. Instead, Alkire and Santos (2014) shows that poor households are indeed likely to have more children.

Table 9: Bargaining proxys distribution among individuals of treated households

	Percentile				
	5%	25%	50%	75%	95%
Household size	3	5	6	7	12
Gender	man	man	woman	woman	woman
Years of schooling	0	4	9	12	12
Father went to school?	no	no	yes	yes	yes
Unemployed head of household?	no	no	no	no	yes

Selected percentiles for the sample of treated households (those receiving a microfinance loan). The number of unemployed individuals in treated households is 45, roughly 17% of the sample.

However, our results suggest that by targeting loans to smaller households, the contribution of microfinance to improving aggregate labor market outcomes would be raised. This is because the relative bargaining power of each individual is higher in smaller sized families.

The same analysis can be conducted for each measure of bargaining power. For example, considering gender, the second panel of Figure 3 shows that if the unemployed worker is a woman, receiving an MFI loan lowers the probability of job search by 20 percentage points. If, instead, the man is unemployed, the effect of receiving a loan on the job search is negligible. This is, once again, exactly what is predicted by Proposition 1 if we assume that woman have on average a lower intra-household bargaining power. But, Table 9 shows that more than 50% of the treated households have unemployed workers who are women. For those households, receiving a loan lowers the job search by the unemployed members in the household. Once again, there is scope to improve targeting to support better aggregate labor market outcomes: targeting lending to households in which women are (self-) employed and men are unemployed. This would support the entrepreneurial activity of the household and at the same time raise the incentive for job search by the husband.

The heterogeneity in treatment effects is striking when we compare unemployed workers who are the head of the household to those who are not. If the household head is unemployed, receiving an MFI loan is associated with a 20 percentage points increase in the probability of job search. If, instead, the unemployed is not the head of household, the treatment lowers the probability of job search by 20 percentage points.This finding is interesting, as the household role is a clean measure of bargaining power. Looking at Table 9, the targeting seems, once again, suboptimal. Only about 5% of unemployed individuals in treated households are the head of household.

Looking at the individual's schooling and father's schooling measures, the findings are as predicted by Proposition1 and the conclusions concerning targeting are very similar.

In Appendix B we perform several robustness checks, including estimating a linear probability model, clustering the standard errors at the neighborhood level, changing the specification of the first stage model, excluding the households that have defaulted on their MFI loan and running a placebo test, where the treatment is defined as having knowledge of microfinance.

6 Addressing Endogenous Sample Selection

A potential caveat in interpreting our findings is that we are interested in the impact of treatment on job search, which is only observed for the individuals currently unemployed. However, employment status itself is affected by job search and, hence, sample selection is endogenous. In particular, there may be individuals who were unemployed at the time of the loan but that have since found work. In expectation these are the individuals who increase their search effort by a greater amount following the treatment. More precisely, consider the following set up adapted from Heckman (1979)

$$S^{\star}_{ij} = W_{ij}\beta_0 + t_j\beta_1 + t_j\alpha_i\beta_2 + e_{ij}, \tag{25}$$

$$U^{\star}_{ij} = W_{ij}\gamma_0 + t\gamma_1 + t_j\alpha_i\gamma_2 + v_{ij}, \tag{26}$$

with W_{ij} a vector of control variables, t_j and α_{ij} defined as before, and where $S^{\star}_{ij}$ is the search intensity of individuals who are unemployed at the time of treatment. However, $S^{\star}_{ij}$ is not observed for those individuals who found employment between the treatment period and the time we conduct the survey. We represent the selection model with equation (26), where $U^{\star}_{ij}$ is a latent variable such that if $U^{\star}_{ij} \geq 0$ the individual is unemployed at the time of the survey (hence, is sampled) and, otherwise, is not sampled. Since the likelihood of finding employment is affected by search intensity, e_{ij} and v_{ij} are unlikely to be independent and there is endogenous sample selection. Moreover, the vector of control variables W_{ij} must guarantee the CIA property, so that $t_j \perp (e_{ij}, v_{ij})$.[30]

The sample selection bias shows as the last term in the following conditional expectation

$$E\Big[S^{\star}_{ij}|W_{ij}, t_j, \alpha_i, U^{\star}_{ij} \geq 0\Big] = W_{ij}\beta_0 + t_j\beta_1 + t_j\alpha_i\beta_2 + E\Big[e_{ij}|v_{ij} \geq -W_{ij}\gamma_0 - t\gamma_1 - t_j\alpha_i\gamma_2\Big]. \tag{27}$$

A possible approach to solve this endogenous selection problem would be to use an exogenous instrument that would cause variation in the probability of finding work but that would not affect

[30]To implement this we include in the vector of control variables W_{ij} the estimated propensity scores $\widehat{p}(X_j)$. In practice, this is done by stratifying the sample in terms of this covariate, as explained next.

search effort. However, such an exclusion restriction is obviously difficult to uphold given that the incentives to search will naturally vary with the probability of success. Instead, we implement a bound estimator that addresses non-random sample selection and in particular the method proposed by Lee (2009).[31] One key assumption for the Lee (2009) bounds to be valid is that treatment has to affect sample selection in one direction. However, our main claim is, exactly, that the effects of treatment on search effort vary with the bargaining power of the unemployed.

We solve this problem by restricting our sample to individuals with low bargaining power, since these are individuals for whom we are confident that the treatment effect is negative. This is because the baseline treatment effect we have estimated is negative (column (2) in Table 6) and low bargaining power individuals are predicted to have the lowest treatment effect (hence, more likely to have a negative treatment effect).[32]

Based on this subsample of individuals, we construct Lee (2009) bounds that will determine an interval for the treatment effect, taking into account the selection bias. We assume that for the subsample of individuals with low bargaining power (small α_i) receiving a loan lowers search effort and, hence, raises the probability of sample selection. That is, we make the following assumption

$$\begin{aligned} &\phi_i = \beta_1 + \alpha_i \beta_2 \leq 0, \qquad \text{and} \\ &\mu_i = \gamma_1 + \alpha_i \gamma_2 \geq 0, \end{aligned} \tag{28}$$

for all individuals i that are part of the subsample of individuals with sufficiently low bargaining power.[33] Sharp lower and upper bound for the treatment effect are given by

$$\tau^l = E\Big[S_{ij}|t=1, U^\star_{ij} \geq 0, S_{ij} \leq S_{1-p}\Big] - E\Big[S_{ij}|t=0, U^\star_{ij} \geq 0\Big], \tag{29}$$

$$\tau^u = E\Big[S_{ij}|t=1, U^\star_{ij} \geq 0, S_{ij} \geq S_{p}\Big] - E\Big[S_{ij}|t=0, U^\star_{ij} \geq 0\Big], \tag{30}$$

[31]This method is an alternative to the seminal method proposed by Heckman (1979) which is a parametric approach that relies on strong assumptions and that requires exclusion restrictions. Bounds estimators require only very few assumptions and do not rely on valid exclusion restrictions. Rather than correcting estimates for potential bias, bound estimators determine an interval for the true treatment effect.

[32]This assumption is further supported by the bottom-right panel of Table 5, showing the average frequency of short-term and long-term unemployment among treated and control. The frequencies are weighted averages based on the propensity scores, so that we are controlling for differences in the probability of receiving a loan. Among the treated group long-term unemployment (duration longer than one year) is more predominant than short-term unemployment (duration less than one year). This suggests that the likelihood of unemployment (and, hence, sample selection) is greater among the treated households.

[33]Notice that the construction of the bounds does not rely on a constant treatment effect (Lee, 2009). In other words ϕ_i and μ_i can vary across i, as long as the treatment is monotonic, which is what condition (28) imposes.

Table 10: Lee (2009) bounds: (probability of job search)

bargaining power	τ^l	τ^u	trimming %	95% C.I.	N
$\alpha_{ij} \leq \mathcal{Q}\,(25\%)$	-0.531 (0.167)	-0.349 (0.110)	26.9%	$[-0.811, -0.164]$	89
$\mathcal{Q}\,(25\%) < \alpha_{ij} \leq \mathcal{Q}\,(50\%)$	-0.016 (0.177)	0.284 (0.104)	47.1%	$[-0.308, 0.454]$	62

Note: the bounds have been tightened using the estimated propensity scores $\widehat{p}\,(X_j)$.
Standard errors reported below the coefficient, in brackets.

where S_{1-p} and S_p are, respectively, the $(1-p)^{\text{th}}$ and p^{th} percentiles of the outcome variable S, with p given by

$$p = \frac{\text{Prob}\,\left(U^{\star}_{ij} \geq 0|t=1\right) - Prob\,\left(U^{\star}_{ij} \geq 0|t=0\right)}{\text{Prob}\,\left(U^{\star}_{ij} \geq 0|t=1\right)}, \tag{31}$$

which has a sample analog and, hence, can be computed from the data. Intuitively, the bounds are based on the construction of two "worst-case" scenarios, one that assumes that the infra-marginal individuals (those who find employment and, hence, are not part of the sample) are those who searched the most intensively or, instead, those that searched the least.

Finally, because we are dealing with observational data and assignment is not random, we use the estimated propensity scores as covariates to sharpen the bounds. In practice, this is done by stratifying the sample in terms of the estimated propensity scores $\widehat{p}\,(X_j)$, splitting the sample in two: $\widehat{p}\,(X_j) \in [0, 0.50]$ and $\widehat{p}\,(X_j) \in [0.50, 0.70]$.[34]

As said earlier, we also focus only on those individuals with low bargaining power. In particular, we look at the subsample of individuals for whom the composite measure of bargaining power constructed in Section 5.4 is below the first quartile of the distribution and another subsample for which the measure of bargaining power is between the first quartile and the median. Thus, we discard all the individuals who have bargaining power above the median as, given Proposition 1, the monotonicity condition (28) is less credible the larger the bargaining power.

The results are shown in Table 10. The outcome of interest is the discrete variable taking value 1 if the unemployed individual has taken any initiative to find a job in the previous four weeks and 0 otherwise (Model 1). The table shows lower and upper bounds for the average treatment effects.[35]

[34] The stratification is done making sure that there is overlap between the treatment and control groups.

[35] The magnitude of these bounds should be compared to the marginal effects computed in Section 5.6 and, in particular, the bottom-right panel of Figure 3. It is also useful to use as a benchmark the estimated treatment effect estimated corresponding to the linear model estimated using weighted least squares to control for non-random assignment, reported in Table A1 of Appendix B. In particular, the baseline ATT effect estimated under the linear probability model is -0.194 (column 2 of Table A1).

For individuals with bargaining power bellow the first quartile, $\alpha_{ij} \leq \mathcal{Q}(25\%)$, the treatment effect estimated set is given by $[-0.531, -0.349]$, with 95% confidence interval given by $[-0.811, -0.164]$, indicating a negative treatment effect.[36] Instead, for individuals with bargaining power above the first quartile, the treatment effect estimated set is given by $[-0.016, 0.284]$, with 95% confidence interval given by $[-0.308, 0.454]$, covering both the negative and the positive region.[37]

Thus, this exercise through which we address concerns about sample selection supports Proposition 1 concerning heterogeneous treatment effects and suggests that we can be confident about our findings and their interpretation. Unemployed workers with low bargaining power reduce their search effort when access to credit is obtained, indicating the predominance of the income effect. Instead, the treatment effects turns more positive as the bargaining power is increased, consistent with the net-worth effect becoming more predominant.[38]

7 Conclusion

We propose a simple collective household choice model in which individual job search in frictional labor markets depends on intra-household bargaining and on access to finance. We show that the impact of access to finance on job search intensity is ambiguous and depends on the interaction between the net-worth and income effects. The search intensity of the unemployed workers in households with access to finance relative to those without access is increasing in the individual's bargaining power within the household.

Using several measures of individual intra-household bargaining power, we test the predictions of our model using data collected by the authors in Cape Verde. The data was collected to evaluate the impact of microfinance programs and, therefore, the population of interest are poor households. Our assumptions are general, but we believe that our findings are mostly relevant for poor households, characterized by lack of capital and whose livelihood, in the absence of public welfare schemes, is strongly influenced and supported by family ties. This is the typical target of microfinance programs.

Linking access to finance to collective household decision making is important when evaluating the impact of development programs since family links affect the livelihood and the decision making

[36]The standard errors to construct the confidence intervals must reflect the variance of the sample analogs of the trimmed distribution, the trimming thresholds, S_p and S_{1-p}, and the trimming quantile, p.

[37]It is also interesting to note that both confidence intervals cover the average treatment effect corresponding to the linear model estimated via weighted least squares, which is equal to -0.194 (reported in column (2) of Table A1).

[38]The trimming percentage for the first and second quartiles are 26.9% and 47.1%, respectively. This is consistent with relatively higher bargaining power individuals having better job market opportunities and, therefore, stronger incentives to search and find work, leaving the unemployment pool.

process of the poor very strongly (Platteau, 1991, Fafchamps and Susan, 2003, Collins et al., 2009). Besides contributing to the literature on collective decision making, our results also provide some new and important policy implications for the design of microfinance programs. Microfinance was primarily designed to promote self-employment in those areas of the world where the poor have little or no opportunity to find a job. Our results highlight the importance of rethinking the targeting of microfinance, since relaxing credit constraints to poor households may have complementarities with improved labor market outcomes but may also discourage the supply of wage labor. Which effect prevails, depends on the targeting of loans.

We show that the behavior of unemployed household members is affected by the access to credit in a non-trivial way, potentially undermining the positive effects of microfinance. When poorly targeted, access to finance can lower the incentives to search for work, making the overall impact on welfare ambiguous. Our findings suggest that, to improve the impact of microfinance on labor market outcomes, the screening should not be solely based on the characteristics of the entrepreneurial activity and individual borrower, but also on characteristics of the household she/he belongs to and, in particular, the within-household distribution of decision power. We explore simple indicators of bargaining power that are easy to measure and scrutinize. If they are used to improve targeting, they can improve the impact of access to finance, generating positive externalities in terms of labor market outcomes. A corollary is that to assess the impact of microfinance programs, it is important to focus not only on the direct impact on borrowers, but also on the indirect effects on other family members.

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A Taylor expansion of $\Delta(C_e, C_n; t)$

In the main text, we use the first order Taylor expansion of $\Delta(C_e, C_n; t)$ around the point

$$\left\{C_e^{\bullet}(t), C_n^{\bullet}(t)\right\} = \left\{(1+f(\alpha))^{-1} Y_n(t), (1+f(\alpha))^{-1} Y_n(t)\right\}.$$

This is given by

$$\begin{aligned}\Delta(C_e, C_n; t) \approx\ & \hat{u}(C_e^{\bullet}(t)) - \hat{u}(C_n^{\bullet}(t)) + \\ & \hat{u}'(C_e^{\bullet}(t))\left[C_e(t) - C_e^{\bullet}(t)\right] - \hat{u}'(C_n^{\bullet}(t))\left[C_n(t) - C_n^{\bullet}(t)\right],\end{aligned} \tag{A.1}$$

where

$$C_e^{\bullet}(t) = C_n^{\bullet}(t) = (1+f(\alpha))^{-1} Y_n(t). \tag{A.2}$$

Using the budget constraint (10) we have that

$$C_n(t) = (1+f(\alpha))^{-1} Y_n(t) = C_n^{\bullet}(t), \tag{A.3}$$

$$\begin{aligned}C_e(t) &= (1+f(\alpha))^{-1} Y_e(t) \\ &= \begin{cases} C_e^{\bullet}(1) + (1+f(\alpha))^{-1}(1+r)W, & t=1 \\ C_e^{\bullet}(0) + (1+f(\alpha))^{-1} W, & t=0 \end{cases}\end{aligned} \tag{A.4}$$

Using (A.2), (A.3) and (A.4) to substitute in (A.1) yields

$$\Delta(C_e, C_n; t) \approx$$

$$\widetilde{\Delta}(\alpha; t) = \begin{cases} \hat{u}'\left((1+f(\alpha))^{-1} Y_n(1)\right)\left[(1+f(\alpha))^{-1}(1+r)W\right], & t=1 \\ \\ \hat{u}'\left((1+f(\alpha))^{-1} Y_n(0)\right)\left[(1+f(\alpha))^{-1} W\right], & t=0 \end{cases} \tag{A.5}$$

which corresponds to equation (14) in the main text.

B Robustness Tests

In this Appendix we investigate the robustness of our findings to: alternative estimation model; alternative construction of the standard errors, changes in model specification; changes in the sample. In particular, we estimate a linear probability model, we try clustering the standard errors at the neighborhood level, we change the specification of the first stage model, and we change the sample by excluding the households that have defaulted on their MFI loan. Finally, we perform a placebo test, where the treatment is defined as having knowledge of microfinance.[39]

B.1 Linear Probability Model

As a first robustness check, we repeated the analysis using a linear probability model (LPM), estimated by Ordinary Least Squares (OLS). If the true conditional expectation function is unknown, it is helpful to contrast the LPM to the Probit model (Wooldridge, 2010). The estimation results are reported in Table A1 and the corresponding marginal effects in Figure A1. Comparing marginal effects, we can see that for all measures of bargaining power considered, the significance and general trends are very similar for both models.

B.2 Clustering of Standard Errors at the Neighborhood Level

Second, in Table A2 and Figure A2 we take into account the fact that the standard errors may be correlated within neighborhoods. Clustering the standard errors at the neighborhood level does not alter the results in any significant way.[40]

B.3 Alternative First Stage Model

Third, since ATT estimated with propensity score methods can be sensitive to specifications of the matching model (see Smith and Todd, 2005, and Heckman et al., 1998), we estimate the model using alternative sets of covariates in the first stage.[41] In particular, we include higher order variables (the

[39] Although not reported, we also check the sensitivity of our results to deviations from the CIA assumption by simulating a potential confounder in order to assess the robustness of the estimated treatment effects following the methodology proposed by Ichino et al. (2008). The estimated ATT with various specifications for the confounding factors change by less than 5% from the baseline findings.

[40] In our main specification (Model 1, Model 2 and Model 3), we chose not to cluster standard errors and only use neighborhood fixed effects since clustering can be misleading in the case of few clusters (Angrist and Pischke, 2008). Moreover, the fixed effects and control variables should absorb most of the systematic within-cluster correlation (Cameron and Miller, 2015).

[41] Dehejia and Wahba (2002) advocate this type of checks in the absence of an experimental benchmark estimate.

square of household size and number of children) and remove the neighborhood fixed effects. The results of this alternative specification are displayed in Table A3 and Figure A3 and are qualitatively and quantitatively very similar to our baseline specification.

B.4 Excluding Defaulting Households

As a fourth robustness check, we exclude from the sample the few households which defaulted on their microfinance loans, which reduces our sample to 259 observations. Our motivation in doing this is to fully align the empirical analysis with the assumptions of the theoretical model. In fact, in Section 2, we assume that households know they have access to finance when deciding on the search intensity. In our baseline specification, we took this into account by restricting the sample to loans given since 2010. Now, we further refine the sample excluding the 'bad' clients. Since MFI usually have repeated interactions with their clients, it is likely that recent clients and clients that did not default have much easier access to microfinance.[42] Reassuringly, the results are again consistent with our baseline results and our theoretical prediction (see Table A4 and Figure A4).

B.5 Placebo Test

Finally, we perform a placebo test to evaluate whether the relationship we have estimated is induced by some other mechanism underlying the characteristics of our covariates and not by the mechanisms we have outlined. In particular, we replace the dummy treatment variable *MFI* capturing the fact that households had access to microfinance, by the dummy variable *Heard about microfinance*, capturing whether households know what microfinance is. We expected this placebo treatment not to have predictive power on the dependent variable, the probability of Job Search. Table A5 and Figure A5 show that, as expected, the placebo treatment, interacted with measures of bargaining power, has no significant impact on the job search of the unemployed.

[42] Our definition of default (loan not repaid 3 months after the due date), is in general stricter than the one used by Cape Verdean MFI in their operations. When deciding whether a clients is eligible for more credit, soft information available to credit officers plays a crucial role, so loans can be given out also to clients who repaid with significant, but "justified", delays.

Table A1: Access to microcredit and labor search: OLS regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	0.034	−0.194*	0.091	−0.004	−0.469***	−0.591***	−0.185*	−0.245**
	(0.094)	(0.116)	(0.194)	(0.111)	(0.209)	(0.165)	(0.109)	(0.115)
MFI × Household Size			−0.042*					
			(0.023)					
MFI × Female (dummy)				−0.322**				
				(0.160)				
MFI × Schooling (years)					0.038**			
					(0.019)			
MFI × Father School (dummy)						0.612***		
						(0.174)		
MFI × Head (dummy)							0.351	
							(0.225)	
MFI × Bargaining Power PC								0.216**
								(0.087)
Household size	0.040**	0.0203	0.040*	0.023	0.020	0.025	0.015	0.017
	(0.018)	(0.022)	(0.023)	(0.021)	(0.020)	(0.019)	(0.022)	(0.020)
Female	-0.194***	-0.116	-0.085	-0.005	-0.137*	-0.0508	-0.102	-0.104
	(0.065)	(0.072)	(0.075)	(0.080)	(0.076)	(0.0711)	(0.0714)	(0.0736)
Schooling (years)	0.004	0.011	0.010	0.012	-0.007	0.007	0.009	-0.002
	(0.009)	(0.011)	(0.011)	(0.010)	(0.013)	(0.011)	(0.011)	(0.012)
Father School (dummy)	-0.005	-0.011	-0.050	-0.063	-0.018	-0.304***	-0.013	-0.101
	(0.064)	(0.078)	(0.075)	(0.085)	(0.077)	(0.091)	(0.076)	(0.079)
Head (dummy)	-0.007	0.292**	0.334***	0.260**	0.194	0.223**	0.168	0.243**
	(0.097)	(0.113)	(0.115)	(0.108)	(0.130)	(0.0981)	(0.171)	(0.107)
Unemp. duration: 7 - 12 m	0.259***	0.025	0.013	0.060	0.154	0.086	0.035	0.148
	(0.095)	(0.113)	(0.112)	(0.102)	(0.117)	(0.114)	(0.111)	(0.112)
Unemp. duration: 1 to 4 y	0.168**	0.095	0.092	0.132	0.095	0.137	0.081	0.126
	(0.068)	(0.085)	(0.082)	(0.083)	(0.087)	(0.093)	(0.085)	(0.093)
Unemp. duration: > 4 y	-0.181**	-0.102	-0.099	-0.094	-0.101	-0.108	-0.138	-0.081
	(0.081)	(0.111)	(0.108)	(0.111)	(0.112)	(0.106)	(0.110)	(0.116)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R-squared	0.346	0.623	0.630	0.634	0.638	0.668	0.630	0.644

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

Table A2: Access to credit and labor search: Clustering at neighborhood level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	0.026	-0.932**	0.830	-0.279	-2.097***	-2.868***	-0.905*	-1.024**
	(0.284)	(0.455)	(0.752)	(0.463)	(0.768)	(0.549)	(0.534)	(0.400)
MFI × Household Size			-0.268**					
			(0.108)					
MFI × Female (dummy)				-1.002**				
				(0.504)				
MFI × Schooling (years)					0.179**			
					(0.076)			
MFI × Father School (dummy)						2.906***		
						(0.563)		
MFI × Head (dummy)							2.559***	
							(0.957)	
MFI × Bargaining Power PC								0.981***
								(0.305)
Household size	0.155**	0.183**	0.319***	0.170**	0.167**	0.165*	0.146*	0.133*
	(0.062)	(0.082)	(0.085)	(0.084)	(0.075)	(0.089)	(0.077)	(0.078)
Female	-0.706***	-0.480	-0.298	-0.106	-0.566	-0.513	-0.445	-0.476
	(0.155)	(0.333)	(0.380)	(0.307)	(0.361)	(0.399)	(0.286)	(0.360)
Schooling (years)	-0.002	0.054	0.046	0.064	-0.035	0.063	0.035	0.007
	(0.025)	(0.047)	(0.043)	(0.040)	(0.054)	(0.045)	(0.046)	(0.050)
Father School (dummy)	-0.035	-0.293	-0.470	-0.320	-0.250	-1.621***	-0.307	-0.586*
	(0.290)	(0.317)	(0.360)	(0.340)	(0.294)	(0.332)	(0.329)	(0.304)
Head (dummy)	-0.055	0.935**	1.162***	0.824**	0.722*	0.642	0.526	0.755*
	(0.255)	(0.392)	(0.412)	(0.361)	(0.387)	(0.424)	(0.446)	(0.398)
Unemp. duration: 7 - 12 m	0.868**	0.814	0.670	0.837*	1.070***	0.986**	0.784*	1.128***
	(0.353)	(0.527)	(0.572)	(0.469)	(0.406)	(0.427)	(0.449)	(0.412)
Unemp. duration: 1 to 4 y	0.612*	0.516	0.415	0.562	0.470	0.691**	0.438	0.619*
	(0.339)	(0.392)	(0.430)	(0.346)	(0.405)	(0.338)	(0.414)	(0.347)
Unemp. duration: > 4 y	-0.645*	-0.317	-0.388	-0.330	-0.398	-0.324	-0.471	-0.290
	(0.359)	(0.570)	(0.556)	(0.543)	(0.567)	(0.516)	(0.576)	(0.536)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R-squared	0.306	0.584	0.598	0.591	0.602	0.642	0.598	0.607

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

Table A3: Alternative specification of the first stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	0.009	-0.660	0.884	-0.146	-1.625**	-1.870***	-0.573	-0.776*
	(0.306)	(0.431)	(0.789)	(0.512)	(0.713)	(0.596)	(0.433)	(0.422)
MFI × Household Size			-0.246**					
			(0.100)					
MFI × Female (dummy)				-0.844				
				(0.622)				
MFI × Schooling (years)					0.136**			
					(0.068)			
MFI × Father School (dummy)						1.814***		
						(0.611)		
MFI × Head (dummy)							2.848**	
							(1.129)	
MFI × Bargaining Power PC								0.660*
								(0.352)
Household size	0.132**	0.263***	0.396***	0.276***	0.276***	0.299***	0.225**	0.263***
	(0.057)	(0.081)	(0.106)	(0.095)	(0.091)	(0.094)	(0.090)	(0.0942)
Female	-0.703***	-0.499	-0.202	-0.0991	-0.429	-0.417	-0.404	-0.383
	(0.206)	(0.312)	(0.365)	(0.382)	(0.342)	(0.335)	(0.345)	(0.340)
Schooling (years)	0.017	0.069	0.049	0.069	-0.006	0.052	0.037	0.027
	(0.029)	(0.044)	(0.048)	(0.047)	(0.050)	(0.047)	(0.049)	(0.048)
Father School (dummy)	-0.0585	-0.365	-0.383	-0.225	-0.219	-0.860**	-0.175	-0.395
	(0.205)	(0.268)	(0.280)	(0.281)	(0.279)	(0.343)	(0.279)	(0.298)
Head (dummy)	-0.091	0.739	0.961**	0.697	0.585	0.636	0.239	0.694
	(0.292)	(0.450)	(0.450)	(0.447)	(0.495)	(0.433)	(0.547)	(0.456)
Unemp. duration: 7 - 12 m	0.882***	0.919**	0.658	0.835**	0.887**	0.951**	0.764*	0.968**
	(0.321)	(0.418)	(0.424)	(0.408)	(0.427)	(0.434)	(0.424)	(0.439)
Unemp. duration: 1 to 4 y	0.553***	1.026***	0.919***	1.043***	0.983***	1.142***	1.004***	1.067***
	(0.214)	(0.313)	(0.305)	(0.315)	(0.319)	(0.341)	(0.322)	(0.335)
Unemp. duration: > 4 y	-0.655***	-0.296	-0.375	-0.398	-0.424	-0.252	-0.497	-0.327
	(0.247)	(0.330)	(0.325)	(0.330)	(0.335)	(0.348)	(0.339)	(0.348)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R-squared	0.323	0.638	0.660	0.654	0.659	0.672	0.662	0.659

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

Table A4: Microfinance and job search excluding households who defaulted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MFI	-0.040	-0.804**	0.908	-0.187	-1.497**	-2.261***	-0.861**	-0.871**
	(0.315)	(0.405)	(0.806)	(0.447)	(0.715)	(0.607)	(0.400)	(0.412)
MFI × Household Size			-0.252**					
			(0.099)					
MFI × Female (dummy)				-0.928				
				(0.567)				
MFI × Schooling (years)					0.101			
					(0.071)			
MFI × Father School (dummy)						2.025***		
						(0.639)		
MFI × Head (dummy)							1.827**	
							(0.761)	
MFI × Bargaining Power PC								0.396
								(0.350)
Household size	0.164**	0.150*	0.265***	0.140*	0.139*	0.151*	0.137*	0.129
	(0.067)	(0.086)	(0.097)	(0.084)	(0.083)	(0.085)	(0.083)	(0.0863)
Female	-0.711***	-0.411	-0.276	-0.063	-0.470	-0.484	-0.388	-0.422
	(0.216)	(0.316)	(0.320)	(0.313)	(0.316)	(0.306)	(0.310)	(0.313)
Schooling (years)	-0.004	0.019	0.018	0.029	-0.025	0.043	0.012	0.004
	(0.031)	(0.047)	(0.046)	(0.046)	(0.050)	(0.046)	(0.047)	(0.047)
Father School (dummy)	-0.059	-0.813***	-0.908***	-0.814***	-0.750**	-1.511***	-0.828***	-0.867***
	(0.219)	(0.305)	(0.298)	(0.294)	(0.310)	(0.383)	(0.304)	(0.306)
Head (dummy)	-0.057	0.618	0.764*	0.528	0.550	0.562	0.357	0.587
	(0.314)	(0.459)	(0.441)	(0.454)	(0.466)	(0.449)	(0.482)	(0.462)
Unemp. duration: 7 - 12 m	0.849***	0.786	0.633	0.800*	0.941*	0.917*	0.793	0.911*
	(0.328)	(0.512)	(0.499)	(0.483)	(0.493)	(0.494)	(0.491)	(0.504)
Unemp. duration: 1 to 4 y	0.583**	0.286	0.203	0.323	0.272	0.511	0.217	0.356
	(0.228)	(0.341)	(0.324)	(0.324)	(0.336)	(0.345)	(0.333)	(0.351)
Unemp. duration: > 4 y	-0.667**	-0.765**	-0.840**	-0.788**	-0.752**	-0.556	-0.850**	-0.688*
	(0.262)	(0.349)	(0.351)	(0.344)	(0.352)	(0.357)	(0.354)	(0.365)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	259	259	259	259	259	259	259	259
Pseudo R-squared	0.303	0.494	0.513	0.501	0.501	0.518	0.505	0.498

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

Table A5: Placebo test: Impact of Having heard about Microfinance on labor search

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Heard about MF	0.142	0.098	-0.305	-0.202	0.884	0.580	0.104	0.072
	(0.226)	(0.333)	(0.728)	(0.513)	(0.700)	(0.683)	(0.352)	(0.340)
Heard about MF × Household Size			0.060					
			(0.099)					
Heard about MF × Female				0.448				
				(0.600)				
Heard about MF × Education					-0.131			
					(0.089)			
Heard about MF × Father School (dummy)						-0.732		
						(0.803)		
Heard of MF × Head (dummy)							-0.065	
							(0.653)	
Heard of MF × Bargaining Power PC								-0.404
								(0.332)
Household size	0.152**	0.180**	0.133	0.187**	0.195**	0.165*	0.180**	0.204**
	(0.066)	(0.092)	(0.118)	(0.093)	(0.097)	(0.094)	(0.092)	(0.098)
Female	-0.717***	-0.287	-0.281	-0.634	-0.275	-0.336	-0.286	-0.434
	(0.214)	(0.343)	(0.346)	(0.496)	(0.343)	(0.357)	(0.346)	(0.377)
Schooling (years)	-0.002	0.041	0.042	0.040	0.154*	0.047	0.041	0.088
	(0.031)	(0.046)	(0.046)	(0.046)	(0.082)	(0.047)	(0.046)	(0.057)
Father school (dummy)	-0.052	-0.213	-0.179	-0.203	-0.222	0.313	-0.213	0.022
	(0.216)	(0.302)	(0.311)	(0.298)	(0.300)	(0.675)	(0.303)	(0.374)
Head (dummy)	-0.053	1.160***	1.162***	1.152***	1.185***	1.129**	1.221**	0.802
	(0.312)	(0.448)	(0.446)	(0.446)	(0.449)	(0.451)	(0.574)	(0.517)
Unemp. duration: 7 - 12 m	0.885***	0.798	0.779	0.785	0.841	0.729	0.799	0.782
	(0.323)	(0.543)	(0.555)	(0.553)	(0.556)	(0.539)	(0.542)	(0.547)
Unemp. duration: 1 to 4 y	0.603***	0.433	0.443	0.465	0.447	0.361	0.433	0.408
	(0.230)	(0.324)	(0.329)	(0.326)	(0.330)	(0.327)	(0.324)	(0.320)
Unemp. duration: > 4 y	-0.637**	-0.551	-0.574	-0.524	-0.522	-0.613*	-0.551	-0.546
	(0.253)	(0.357)	(0.368)	(0.355)	(0.359)	(0.354)	(0.357)	(0.355)
Other hh. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Other ind. level controls	yes	yes	yes	yes	yes	yes	yes	yes
Neighborhood FE	yes	yes	yes	yes	yes	yes	yes	yes
Inv probability weighting	no	yes	yes	yes	yes	yes	yes	yes
Observations	262	262	262	262	262	262	262	262
Pseudo R-squared	0.307	0.569	0.570	0.570	0.572	0.571	0.569	0.571

Robust standard errors in parentheses, *p<0.10, ** p<0.05, *** p<0.01.
Additional household level controls: House ownership (dummy), Number of household members<15, Contacts with family abroad (dummy) and Number of times per week reads journal (dummy). Additional individual level controls: Age, Age2, Origin, Parent Self-Employed (dummy), Number of children, Ownership of Bank Account (dummy) and an indicator for whether the member received the loan (dummy).

Figure A1: Average marginal effects in the linear probability model

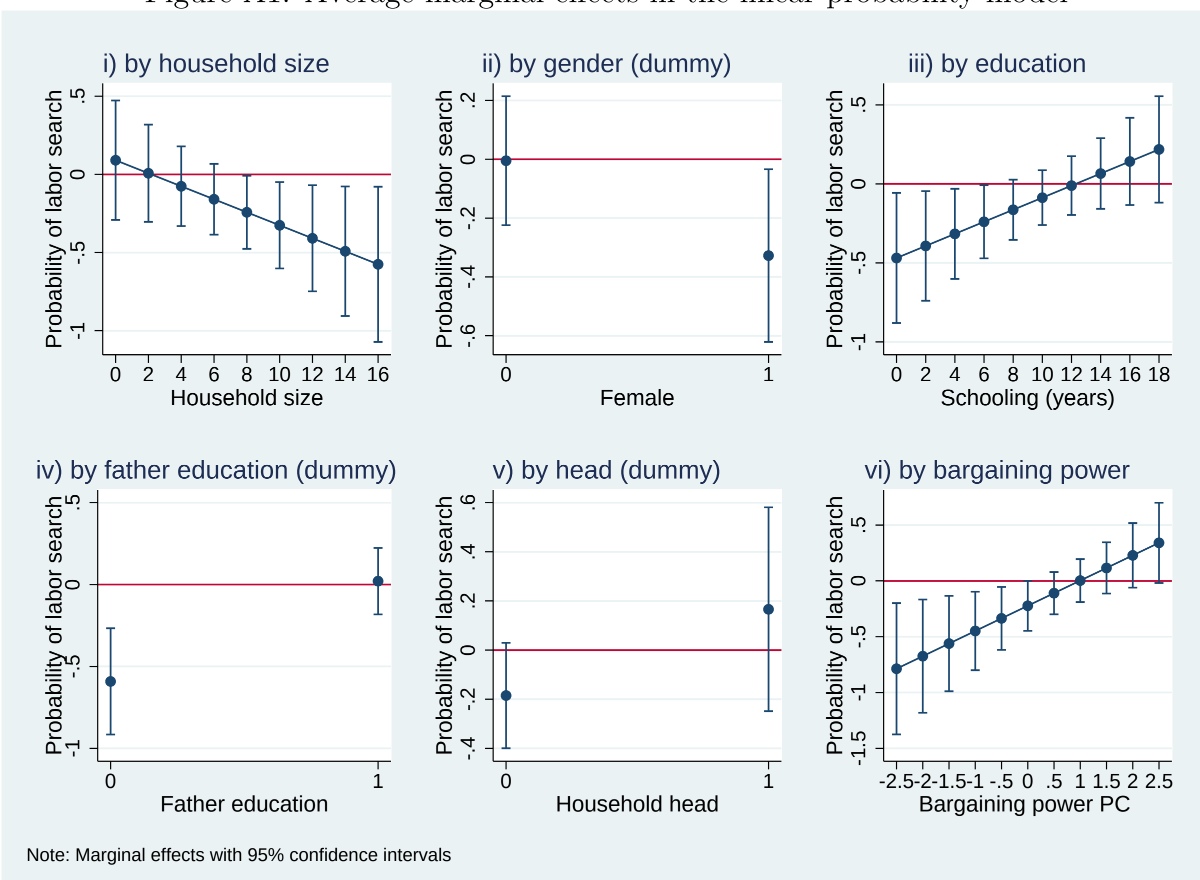

Figure A2: Average marginal effects with clustering at neighborhood level

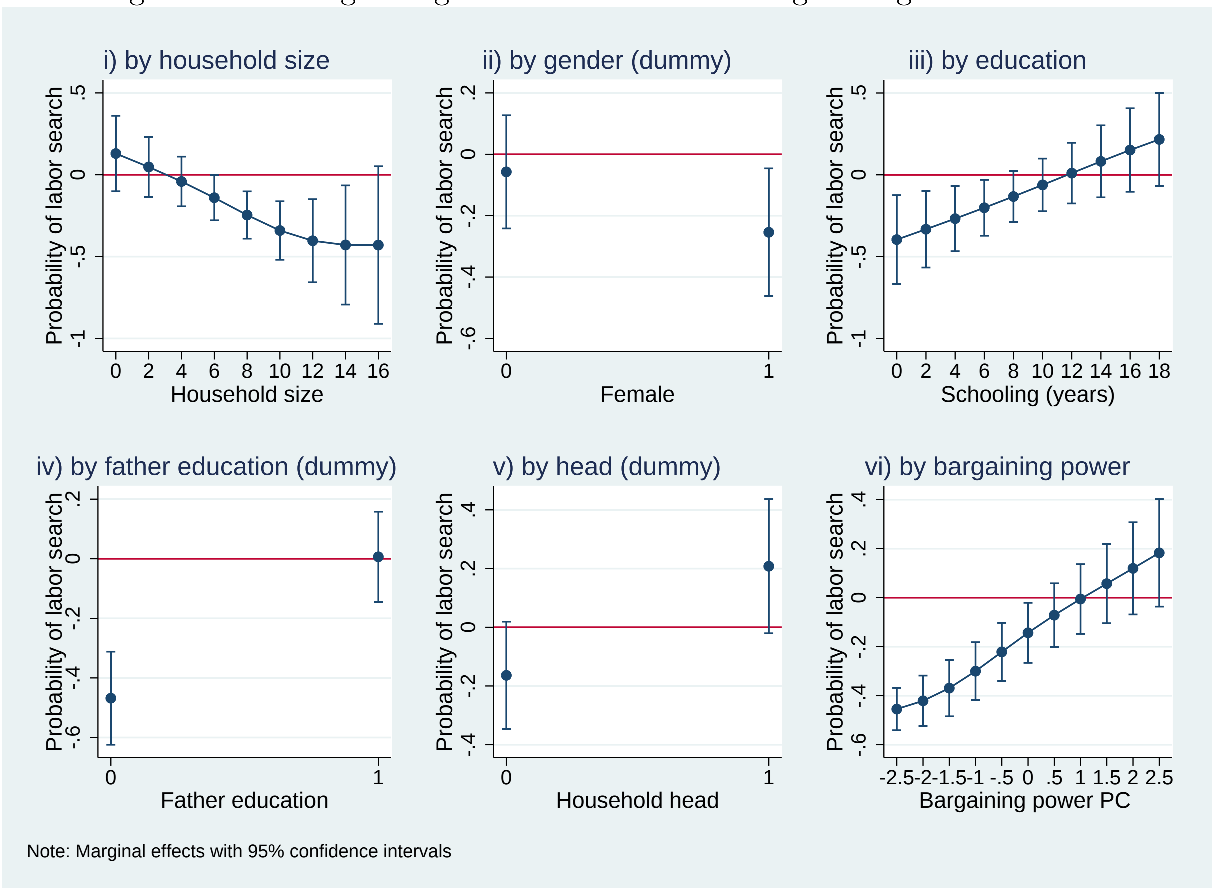

Figure A3: Alternative specification of the first stage

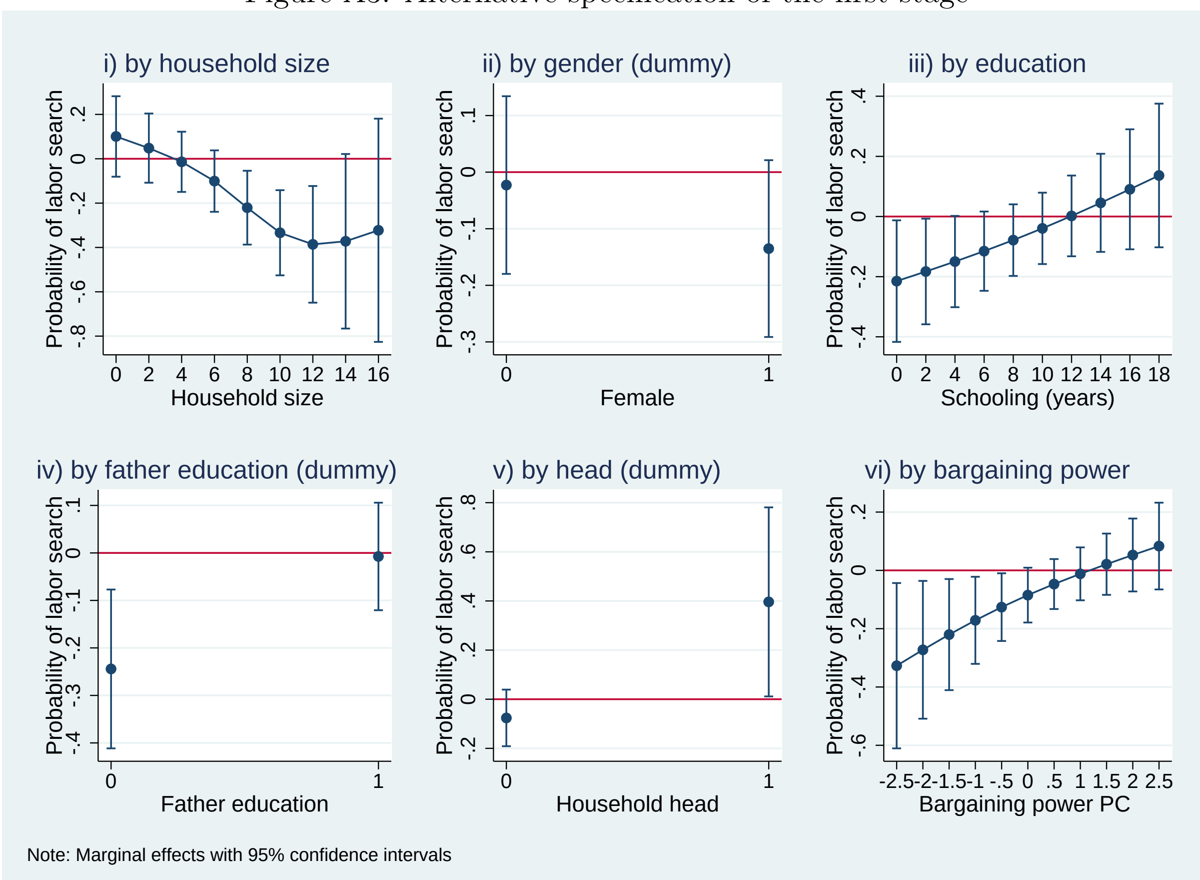

Figure A4: Average marginal effects excluding households who defaulted

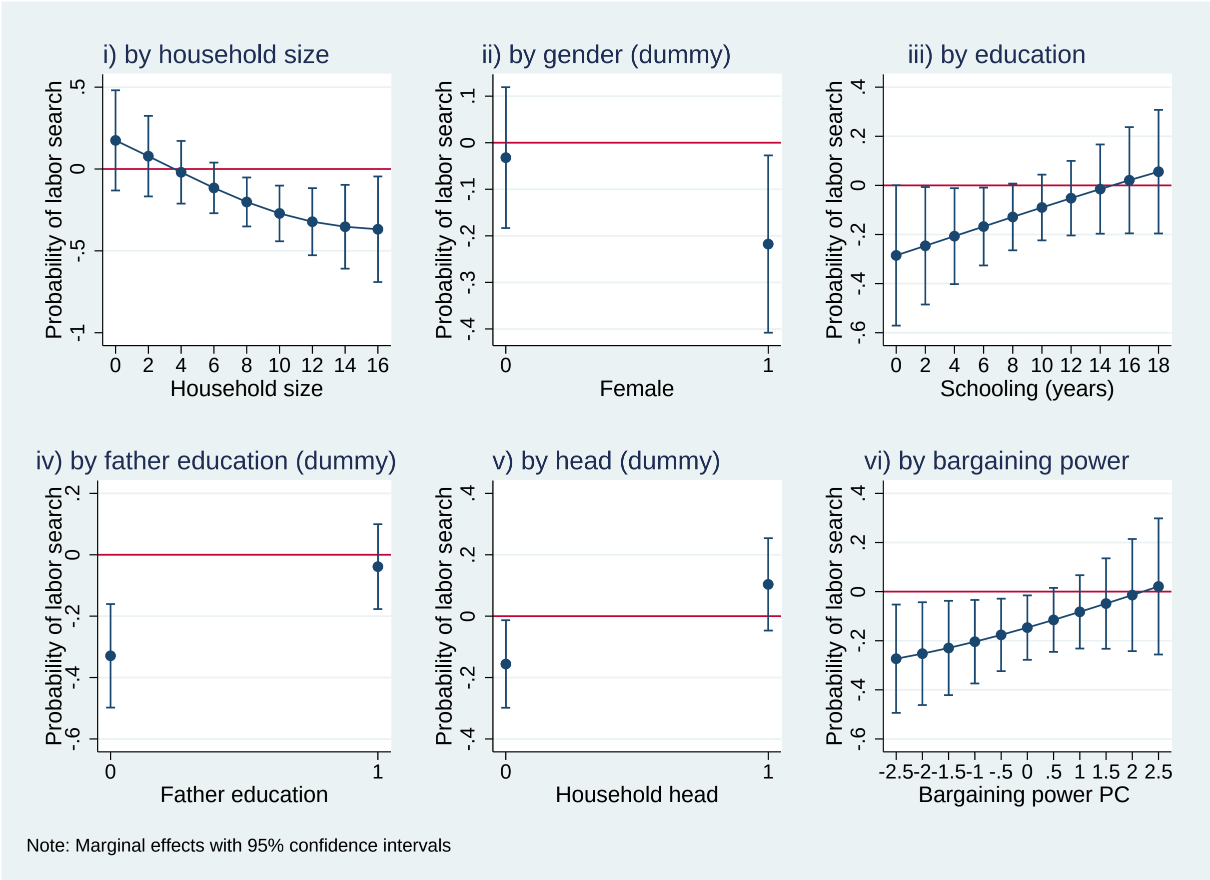

Figure A5: Average marginal effects of Having heard about MF