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Highlights

- Hypothesizes that a flexible choice-set of varied microloan types increases take-up
- Heterogeneity in borrowers' preferences assumed to drive demand for different types
- Hypothesis tested in a laboratory microfinance setting with student subjects
- Results show that flexibility in the choice-set indeed leads to higher take-up rate
- Risk and social preferences play determining roles in the loan type chosen

Take-up of joint and individual liability loans: an analysis with laboratory experiment ☆

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Abstract

This paper reports a study on decision-making by borrowers regarding take-up of different loan types in a laboratory microfinance experiment. I show that when prospective borrowers are offered a flexible choice of different loan types (here, individual liability (IL) and joint liability (JL)), take-up increases. This is due to heterogeneous borrowers self-selecting into different loan types. Results suggest that more risk averse borrowers are less willing to take up IL loan and less selfish borrowers show signs of higher inclination to take up JL loan. The results collectively imply that microloan offers need to be customized according to the heterogeneous preferences of borrowers; also, there needs to be enough flexibility in the offered choice-set for better self-selection. This would result in a substantial increase in the take-up rate of microloans by the borrowers.

Keywords: Microfinance, Laboratory experiment, Loan take-up, Development policy

JEL: C90, D81, G21, I38, O16

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1. Introduction

Joint liability (JL) mechanism of loan repayment is central to microfinance. Through its peer-monitoring strategy, it has the potential for solving the disadvantages of asymmetric information between lenders and borrowers, and thus enforce better repayment. As a result, it has been deemed as a superior method in microloan contracts, especially when collateral cannot be used for poor borrowers.[1] However, over the years, individual liability (IL) loan contracts have also been used in parallel. In this regard, it is not unlikely that these two loan types, with their varied repayment liability schemes, could appeal to the borrowers differently. Borrowers are likely to have heterogeneous preferences; therefore, not every borrower would prefer these loan types equivalently. In this paper, I attempt to study the puzzles in decision-making of the borrowers when they select such loan types. Through a microfinance setting in the laboratory, I try to understand whether being able to self-select into different loan contracts can increase overall take-up of these microloans. And together with that, I try to uncover how heterogeneous preferences among borrowers can play a role in take-up of these two different loan types.

The motivation for this paper arises from the mediocre performance of microcredit as a poverty alleviation tool over the past years. Large-scale field experiments throughout the last decade (in India by Banerjee et al. (2015a), in Morocco by Crépon et al. (2015), in Bosnia-Herzegovina by Augsburg et al. (2015), in Mexico by Angelucci et al. (2015), in Mongolia by Attanasio et al. (2015), and in Ethiopia by Tarozzi et al. (2015)) fail to find any outstanding effect of microcredit in poverty alleviation.[2] One critical and unanticipated puzzle that all these studies stumble upon is the low take-up of microloans by those poor for whom these are specifically designed.[3] Therefore, the consistent finding of low take-up rate in these experiments calls for an investigation. In my opinion, one possible way of addressing the gap in take-up would be to enquire the issue along the channel of borrowers' preferences.[4] Although the studies mentioned above as well as many others in the related literature[5], have focused on making the loans profitable for the mass of prospective borrowers, they have not incorporated borrowers' heterogeneous preferences into the equation. Therefore, in this study,

[1] The mechanism became popular through its exceptionally successful implementation by the Grameen Bank in Bangladesh in the 80's. Later, the mechanism was commonly used by microcredit organizations in many developing countries. Theoretical models by Stiglitz (1990), Besley and Coate (1995), Armendáriz de Aghion (1999), Ghatak and Guinnane (1999) bear testimony to the endurance of the JL mechanism in influencing better repayment.

[2] An interested reader can find a comprehensive comparative study across these six field experiments in Banerjee et al. (2015b).

[3] Furthermore, the unpredicted low take-up of loans in all these studies weakened the statistical power of their corresponding samples and therefore hindered impact evaluation of microcredit.

[4] Harrison and Ng (2016) argue that programmes and policies are most often evaluated by the metric of take-up, which, unfortunately, does not automatically reflect the welfare gain of the product to the adopter. Thus, welfare evaluation requires to include risk and time preferences as well as the individual's subjective beliefs on possible losses from the policy/product in question.

[5] For example, Giné and Karlan (2014) study issues like group pressure, default rates, etc. under both individual and joint loan contracts, with villagers in the Philippines.

I focus on borrowers' heterogeneity in preferences and thereby try to understand if allowing borrowers to self-select into their desired loan type is a way of increasing take-up.[6] So, I take a simple hypothesis to the lab to test: whether take-up rate of loan increases when borrowers are offered a flexible choice-set with both JL and IL loans in contrast to when they are offered only one loan type. This set-up is distinctly different from the above mentioned field experiments; there the borrowers were unable to choose between different loan types[7]- either they took the only loan type offered or they did not. Most laboratory and framed field experiments till date, in the context of microcredit (Abbink et al., 2006; Cason et al., 2012; Kono, 2014), with their aim of studying the superior implementation of repayment through JL in comparison to IL, have also followed a similar design of allowing only one loan type at a time.

One might argue that it is not necessarily surprising in itself that offering more loan options in the choice-set would increase take-up rate. However, a growing body of literature on *cognitive load* agrees on the fact that most often an increased number of choices can impair optimal decision-making.[8] Especially in my context, when the prospective borrowers have to choose between profit-making loan options, they might end up making poor and irrational choices, possibly be even discouraged to take any loan at all. In light of this, it is actually worth checking whether my hypothesis of increased take-up rate holds for an elaborate choice-set. Besides testing the take-up rate, another emphasis of the paper lies in examining if the heterogeneous preferences of borrowers lead to varied take-up. There are certain features of JL and IL loan types that could be advantageous or disadvantageous to the borrowers according to their preferences. One of the main features of microcredit loans is the *dynamic incentive* that comes with them i.e. the promise of further loans from the lender in case of full repayment (Besley, 1995; Morduch, 1999). With dynamic incentive, JL excels over IL because the former ensures a higher probability of loan repayment through a jointly liable peer-group which in turn increases the chances of getting further loans; JL thus reduces the risk of non-repayment after every period of loan. So a borrower who is risk averse regarding repayment, would prefer JL.[9] On the other hand, as the future time horizon for availability of future loans (through dynamic

[6] In the context of take-up of economic policies, Dasgupta et al. (2015) use an artefactual field experiment to show that besides socio-economic and demographic characteristics, behavioural traits such as risk-taking and competitiveness drive take-up of training programmes intended for improving labour market outcomes. They argue that incomplete financial markets failing to smooth economic risks and institutional hurdles rendering investments to be fraught with uncertainty are common phenomena in developing countries. As a result, individuals only with a risk-taking attitude might be interested in investment activities. This implies that risk attitudes play a role in the decision to participate in such programmes/activities.

[7] The aforementioned studies in Ethiopia, India, Mexico and Morocco offered only JL, and in Bosnia-Herzegovina IL was offered; whereas in Mongolia both types were offered, but to separate groups - each being offered to one group.

[8] *Cognitive load* can affect risk-taking and patience while economic decision-making (Deck and Jahedi, 2015) and also, undermine the chooser's subsequent satisfaction and motivation (Iyenger and Lepper, 2000; Schwartz, 2004). I thank an anonymous referee for this discussion.

[9] However, the higher probability of repayment with JL is certain only when peers within a group are fully capable of monitoring each other's actions and investment outcomes. Under less strict scenarios, where information asymmetry among the JL group members is possible, several uncertainties could affect the repayment probability with JL. For example, in the field, an additional uncertainty in joint repayment could arise if a borrower runs away from her commitment when the

incentive) is unknown, it is likely that the choice might be driven by the discount factor of the borrower. The borrower, who discounts the future less and values long-run benefit of receiving further loans, would have higher willingness to take up JL loan. But for the borrower who discounts future heavily, the short-run cost of repaying on behalf of an unsuccessful partner might surpass the fruits of receiving more loans with higher chances in the long run; hence, she would prefer IL to JL.[10] Therefore, given these features, the *ex ante* optimization by the borrowers should be influenced by their risk preferences and discount factors. In addition to that, the taker of JL might also be willing to bear the cost of a partner's burden because she enjoys higher utility from the expected gains of herself and the partner's as well.[11] This possibility originates from the behavioural foundations of preferences which validate that it is not uncommon that an individual derives additional (positive/negative) utility from other's outcome i.e. *social* or *other-regarding preferences* (Rabin, 1993; Levine, 1998; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). To sum up, these possible differences in risk, discounting and other-regarding preferences incite my hypothesis that when the choice-set of loan types is constricted, there might not be as many takers as would be otherwise. Having described all the possible preferences that could play an active role in such decision-making, I, however, restrict my conclusion to the effects of risk and social preferences only and refrain from making any inference on discount factor in this study.[12]

A key feature of my design is the *outside income option*, to classify the potential non-takers of any loan type and also to understand how the takers vary from them. When analysing (non)/take-up, one needs to

partner's investment fails, since legal recourse is hardly implementable. Given this possible uncertainty, a prospective borrower could actually be deterred from selecting JL contract since she is not sure of the partner's commitment. Another degree of uncertainty could emerge under information asymmetry, and that is of freeriding. By having the insurance of a jointly liable group, a freeriding partner could actually strategically default by not repaying. Although having dynamic incentives (Giné et al., 2010) and social sanctions (Besley and Coate, 1995) can actually help minimize these additional risks and uncertainties arising with information asymmetry under JL contract, it is not always obvious in the field that JL should outperform IL. While it is true that my assumption of information symmetry among the JL partners leads to a substantial departure from the typical features of microfinance, it is also true that trying to study all these features in one setting would lead to several confounding effects and thus explain nothing.

[10] We commonly relate discount factor to an individual's *patience*, i.e. the more patient she is, the less she minds waiting for something valuable rather than having it immediately. But, another reason for using discount factor is when one is unsure of the time length for which the future will continue, i.e. if one is not sure if tomorrow will really come, she would prefer to have the valuable thing today rather than tomorrow. In this context, one can use the two interpretations interchangeably, that is, the more one believes that the future will last until period t, the more patient she is to wait for the reward until then.

[11] For example, Dreber et al. (2014) study the association between altruistic giving and cooperation in indefinitely repeated PD game. They find evidence of correlation between generosity (in terms of dictator game giving) and cooperation in infinitely repeated PD even when no cooperative equilibria exist; this implies that it is not unlikely that individuals with *other or social regarding preferences* sometimes deviate from the standard predicted way, to cooperate. However, the authors do not find similar correlation when cooperative equilibria exist. Such varied results motivate to check if there is any role of altruistic giving in this study.

[12] A drawback of the experimental setting here is, it does not reflect the *patience* factor after every period of loan to such an extent that it could affect the discounting factor of the subjects.

pay careful attention to modelling the outside option of the subjects. Considering what goes on in the real field, I assume that the prospective borrowers do not have a constant source of income; therefore when they reject a loan, they merely leave themselves to chances of getting some temporary job that would lead to considerably lower earnings than what a successful investment outcome of the microloan could bring. This aspect has been incorporated in the design in the form of an uncertain outside income, i.e. an employment opportunity, when one does not take up any loan. According to the experimental design, the probability of getting employed is higher than the success probability of the investment project with the loan, but the wage is much lower than the returns from a successful investment. To my knowledge, no previous study has considered the issue of outside income option in the context of microloans.[13]

In a laboratory experiment conducted with 220 university student subjects in the decision-making lab in Turku, Finland, I find statistically significant evidence in favour of the hypothesis which claims that when offered a choice-set with the two loan types, take-up proportion is higher than an offer of just one type.[14] I find interesting effects of heterogeneous preferences, thus confirming their association with the choice-making by the borrower: willingness to take up IL loan decreases with risk aversion; but in general, willingness to take up any loan decreases with risk aversion. By testing the association between altruism and chosen loan type, I find that subjects who take JL loan tend to donate more in a one-shot dictator game (DG).

This paper makes a contribution to the literature on borrower's investment behaviour in the framework of microcredit. A closely related work by Barboni (2017) also deals with the take-up of microloans but focusses on the variability of repayment schedule by offering a menu of rigid and flexible repayment contracts. In a framed field experiment with Indian micro-entrepreneurs, she finds evidence that the high-revenue borrowers prefer flexible schedule and risk averse borrowers stick to the rigid repayment schedule, the second result providing some strength to my hypothesis that the choice of contract types could differ according to borrowers' risk preferences. Other works in this literature study demand for micro-insurance under different contract types involving joint liability (Janssens and Kramer, 2016) and risk-taking choices by borrowers under different microfinance contracts (Giné et al., 2010; Fischer, 2013). While the last two studies investigate optimal risk-taking behaviour given the contract type, my study tries to approach the issue differently - by studying which contract type is optimal for the borrower given the risk and other costs of that contract. This paper also makes a contribution to a very small literature till date, which deals with various issues of microfinance in a laboratory setting, such as loan repayment and monitoring decisions (Abbink et al., 2006; Cason et al., 2012), mechanisms for improved cooperation in social dilemmas (Baland et al., 2017), etc. In

[13] However, the influence of *outside options* has been explored in a variety of contexts within the experimental literature; for example, in the form of the forgone option by the first mover in a lost wallet game and the effect of its size and saliency on the reciprocal nature of the second mover (Cox et al., 2010), or in the form of an entrant firm's home market profits and its effects on the entrant's competitive behaviour in a contestable market (Dasgupta, 2011).

[14] Besides this study, Abbink et al. (2006) and Cason et al. (2012) have previously used samples of students from developed countries (Germany and Australia respectively) in lab experiments on microfinance.

light of the overarching focus of the paper, which is take-up of microcredit, this study contributes to the literature that concentrates on finding solutions to remove barriers to the take-up of welfare policies. In the context of various welfare products and interventions, such as insecticide-treated bednets in malaria-infested places, free/subsidized account-opening in the bank that influences financial inclusion, vocational training programmes that increase the chances of employment, etc., take-up has been found to be negatively affected by increase in percentage of cost-sharing of the welfare product by the beneficiary (Cohen and Dupas, 2010), lack of trust in the institution to which the intervention is related (Dupas et al., 2014), time and resource constraints to the utilization of the product or intervention (Bonan et al., 2017; Jain et al., 2019). In connection with these studies, my study finds that it is the lack of flexibility in choice of the product that acts as a barrier to take-up.

On a final note on the contribution, this paper reports elaborately how the sample size and power calculations for the main study were done based on pilot studies. Though this is a necessary step before conducting experiments, it is still rare that studies involving laboratory experiments report theirs. Andersson et al. (2017), who similarly calculate power from pilot studies, use *Cohen's d*, an approach frequently used in estimating sample sizes for statistical testing.[15] I, instead use *Cohen's h*, which measures the difference between two independent proportions, to indicate the necessary sample size. Therefore, this paper, along with Andersson et al. (2017), constitutes a small niche in the literature of lab experiments, that informs about statistical power and sample size calculations from pilot studies.

The remainder of the paper is structured as follows. Section 2 describes the theoretical modelling of the loan types and the borrower's decision-making conditions for taking up a given loan type. In Section 3, I describe the experimental design and the laboratory procedure. This is followed by discussion on results in Section 4. Finally, in Section 5, I conclude.

2. Theoretical framework

In this section, with the help of an economic model, I analyse a borrower's decision when offered a certain loan type.

2.1. Features of IL and JL loan types

As done in previous literature, I keep the features of the loans in their simplest forms for tractability yet without loss of generality.

I assume, the features which are common to both loan contracts are: **1.** Each borrower, if she chooses any loan type, gets loan b to invest in a one-period risky project; **2.** Repayment fee for each borrower is $(1+r).b$, where $r > 0$ is the rate of interest; **3.** Probability of success in the risky project is p, which yields

[15] A lower *Cohen's d* obtained from the difference of two independent means divided by the standard deviation, indicates the necessity of larger sample sizes, and vice versa, as can subsequently be determined together with the additional parameters of desired significance level and statistical power (Cohen, 1988).

return π. If the investment fails with probability $(1-p)$, b is lost and return is 0. In case of failure, the borrower cannot return her own loan. I assume discrete possibilities, i.e. she repays the entire loan with interest in case of success and nothing in case of failure; **4.** The contract has dynamic incentive, i.e. as long as she successfully repays her loan at the end of every period, she is eligible to get a similar loan in the next period which is to be invested in the same way. But if once her investment fails, and she is unable to repay the loan at the end of a certain period, she is no more eligible to get another loan in the latter periods; **5.** Borrowers have no other source of income when they take a loan. And, they cannot use return of one period to repay in another period; **6.** $\alpha = \frac{(1+r).b}{\pi}$ is the repayment ratio. The simple features above are similar to those used by Armendáriz de Aghion (1999).

The additional features of JL loan are: **1.** The loans (having the same features as above) are available for a group of two borrowers (assuming the simplest group formation) and each member has her own project; **2.** The project success probability remains same for each project but risk across the projects is uncorrelated; **3.** Success probability p in the individual investment is common knowledge to all group-members; **4.** If one member is successful in her own project while her partner is not, the former has to repay on behalf of the latter - here lies the joint liability. Therefore, for JL to be feasible, $(\pi - 2\alpha\pi) \geq 0 \Rightarrow \alpha < \frac{1}{2}$; **5.** As long as both the loans are repaid by one/both of them after every period, both are eligible to get another loan in the next period. If both investments fail in a certain period and none is able to repay, none of them gets a loan in the latter periods; **6.** I assume that there is symmetric information between the group-members on their respective investment outcomes; hence, no possibility of freeriding[16] by false-reporting own investment outcome. Features **1-5** have been commonly used by Besley and Coate (1995), Armendáriz de Aghion (1999) and Ghatak and Guinnane (1999), etc.

2.2. Features of the outside income option

Since the aim is to study take-up of loan types in an experimental set-up, I also need to consider the situation when the subjects do not take up the offered loan. The easiest way to model an outside option would have been to offer a sure income to the subjects. However, the literature on lab experiments provides strong evidence that the availability of safe and sure options in experimental tasks could bias the subjects towards those (Andreoni and Sprenger, 2012).[17] To avoid such certainty bias, I model the outside option to be safer than loan investment, yet with some uncertainty attached to it. To rationalize this design in the context of microcredit, one could fairly argue that when the prospective borrowers reject a loan, it is not because they have a sure fallback income option; usually they have some temporary job (or merely the prospect of getting one) which brings an income lesser than the returns from a successful microloan investment. In the

[16] In order to concentrate on the differences in features of the loan types that are already present (see Section 1), I abstract from the possibility of freeriding, as it would further complicate the set-up.

[17] The authors highlight that the intuition behind Allais Paradox can be used as a reference to understand such outcomes. When two options are far from certain, then individuals choose as expected utility maximizers, but when one of the two options is made certain, individuals do not necessarily maximize their expected utility.

same context, it is realistic to assume that the chances of obtaining such temporary employment are better than yielding a successful return from loan investment. Thus, I model the outside option as an employment opportunity for the subject if she does not choose any loan. I assume that the probability of obtaining employment is $q(> p)$ and the wage is $s < (\pi - 2\alpha\pi)$. So, if one obtains an employment in any period, then she receives the wage s. I further assume that one's chance to seek employment in the next period is independent of the outcome of her employment search in the current period.[18]

2.3. Theoretical predictions

Stating the assumptions of the three different income possibilities in the setting, I now move on to discussing the discounted expected utility of an individual under these three possible choices. In order to concentrate on the take-up decisions of borrowers, I abstract from the lender's optimization problem and instead take the set of loan types offered as given. Usually, a lender imposes different interest rates for the two loan types (lower for JL). But involving different interest rates could complicate this setting so much so that it would be difficult to single out the role of each preference category in the decision-making. Giné et al. (2010) and Kono (2014), too, follow the same strategy of focussing on the borrower's problem and abstracting from the lender's.

Let each individual i maximize her stream of expected utility by discounting the future at rate $\delta < 1$. Her preferences over payoffs in each period is represented by the utility function $U_i(\gamma; y_i)$, where $U_i'(\gamma; y_i) > 0, U_i''(\gamma; y_i) < 0, U_i(\gamma; 0) = 0$, and γ is the parameter for risk aversion. Thus she maximizes the discounted utility: $\sum_{t=1}^{\infty} \delta^{t-1} E[U_i(\gamma; y_i)]$.[19]

The expected discounted utility of individual i if she selects IL loan, is:

$$EU_{i,IL} = \frac{1}{1 - \delta p}.p.U_i(\gamma; \pi - \alpha\pi), \tag{1}$$

The expected discounted utility of individual i if she selects employment opportunity (henceforth EMPL), is:

$$EU_{i,EMPL} = \frac{1}{1 - \delta}.q.U_i(\gamma; s) \tag{2}$$

[18] As pointed out by a referee, this uncertainty in the outside option digresses from the standard assumption in traditional development economics, i.e. the poor always have a safe outside option (e.g. The *Mahatma Gandhi National Rural Employment Guarantee Act* (MGNREGA) introduced in India in 2005 allows any interested poor individual to be able to work at a fixed wage rate). However, by relaxing this assumption in my design, I can confidently eliminate any confounding effect arising from potential certainty bias in decision-making in the lab. To reconcile with the norm in development economics, I propose that it is possible to extend my experimental set-up to one where the outside option comes with zero risks. As long as the outside option is low-return and zero/low-risk, such that the expected income from it is considerably lower than that from the loan(s), I believe our interest in eliciting the effect of flexibility in the choice of loan type (and of the heterogeneity in preferences) on take-up would remain largely unaffected.

[19] She maximizes her discounted utility over an infinite-period horizon since she is unaware for how long the loans would be available in future (provided she is able to repay after every period). Giné et al. (2010) use a similar model under dynamic incentive, but they additionally consider projects with different risks.

Now, it is optimal to choose IL loan over EMPL *iff* (1) > (2)

$$\Rightarrow \frac{p(1-\delta)}{q(1-\delta p)} \geq \frac{U_i(\gamma; s)}{U_i(\gamma; \pi - \alpha\pi)} \tag{3}$$

For example, with a *Constant Relative Risk Aversion (CRRA)* utility function, (3) can be re-written as,

$$\Rightarrow \frac{p(1-\delta)}{q(1-\delta p)} \geq [\frac{s}{\pi - \alpha\pi}]^{(1-\gamma)} \tag{3'}$$

The general risk-pooling advantage of JL allows a higher probability of loan repayment and obtaining of future loans than IL. With IL loan, the probability of getting the next period of loan is p. With JL loan, the probability of getting the next period loan is $p^2 + p.(1-p) + (1-p).p$ in case of a two-person group. The first term stands for when both the group members are individually successful in their respective projects, second and third terms stand for when one is successful in her project but the other is not and the former is liable to repay on behalf of the latter (Armendáriz de Aghion, 1999; Giné et al., 2010).

Therefore, with JL under information symmetry, individual i's discounted expected utility is:

$$EU_{i,JL} = \frac{1}{1-\delta(2p-p^2)}[p^2 U_i(\gamma; \pi - \alpha\pi) + p.(1-p)U_i(\gamma; \pi - 2\alpha\pi) + (1-p).pU_i(\gamma; 0) + (1-p)^2 U_i(\gamma; 0)] \tag{4}$$

Therefore, JL is better than IL *iff* $EU_{i,JL} \geq EU_{i,IL}$

$$\Rightarrow \frac{U_i(\gamma; \pi - 2\alpha\pi)}{U_i(\gamma; \pi - \alpha\pi)} \geq \frac{1-2\delta p}{1-\delta p} \tag{5}$$

Again by using an example of *CRRA* utility function, (5) becomes,

$$[\frac{\pi - 2\alpha\pi}{\pi - \alpha\pi}]^{(1-\gamma)} \geq \frac{1-2\delta p}{1-\delta p} \tag{5'}$$

Taken together, the above inequalities lead to the following proposition.

Proposition 1. *Combining the inequalities from* (3) *and* (5)*, we can obtain the lower threshold of risk aversion above which JL $\succeq$ IL, and the higher threshold of risk aversion until which IL $\succeq$ EMPL. For example, using a CRRA utility function (in* (3') *and* (5')*), we can see that JL $\succ$ IL $\succ$ EMPL in the range $\gamma^{**} < \gamma < \gamma^{*}$. The discount factor δ remains unchanged while this holds.*

Proof: See **Appendix A**.

Thus we see that the individuals who are comparatively more risk averse ($\gamma > \gamma^{**}$) prefer JL to IL , all other things remaining unchanged. Those who are risk averse at an even higher level ($\gamma > \gamma^{*}$) would prefer EMPL only over IL , and if exceedingly more risk averse (beyond an even higher threshold γ^{***}) then would prefer EMPL over JL too.

For the discount factor δ, we can analogously obtain thresholds across which the preference orders for IL, JL

and EMPL change. Since in a microfinance setting with dynamic incentive, the future time horizon of loan availability is unknown to the borrower, it is possible that the borrower's decision on her choice is affected by her belief on the number of periods the loan offerings would last or the number of periods she is willing to wait to enjoy her gains.

Proposition 2. *Combining the inequalities from* (3) *and* (5)*, we can obtain the lower threshold of discount factor above which JL $\succeq$ IL, and the higher threshold of discount factor until which IL $\succeq$ EMPL. For example, using the utility function in* (3') *and* (5')*, we can see that JL $\succ$ IL $\succ$ EMPL in the range $\delta^{**} < \delta < \delta^{*}$. The risk aversion parameter γ remains unchanged while this holds.*

When both risk aversion parameter γ and discount factor δ vary together, the following corollary is obtained.[20]

Corollary 1. *For a given discount factor, say δ', as the risk aversion level γ decreases below a lower threshold γ^{**},* (5') *does not hold and JL $\not\succ$ IL anymore. But for a discount factor higher than δ', the risk aversion threshold until which* (5') *holds is lower than γ^{**}.*

Corollary (1) shows how the interplay of risk preferences and discounting could affect the borrower's selection of loan type. However, one could argue here that the choice between JL and IL is not driven by risk and discounting only. An interesting line of inquiry is to also include utility from other's (here, the partner) income as a component in the utility function of the JL loan borrower. In line with models on *social preferences* (Levine, 1998; Charness and Rabin, 2002), if the borrower puts some positive weight on the partner's payoff in her own utility function, then JL is preferred more easily.

Here, I focus on the altruism aspect of social preferences only. Following the model on pure altruism by Levine (1998), let us try to understand how the utility function of individual i looks like when it includes distributed weights on two components:- one being the standard utility from her own income, and the other being the non-standard component that comprises of her utility from partner j's income. Let i put a positive weight $\beta(0 < \beta < 1)$ on her utility from j's income, and the remaining weight on the utility from her own. Formally, let the preferences of i in each period be represented by the utility function: $U_i(\gamma; y_i; \beta; y_j) = (1-\beta).U_i(\gamma; y_i) + \beta.U_i(y_j)$; and she maximizes her stream of expected utility by discounting the future at rate $\delta < 1$. Thus she maximizes the discounted utility: $\sum_{t=1}^{\infty} \delta^{t-1} E[U_i(\gamma; y_i; \beta; y_j)]$. The risk parameter only affects her utility from her own possible payoffs but has no effect on her utility from the partner's payoff. To keep the setting in its simplest form, let $\beta > 0$ when the individual chooses JL but $\beta = 0$ when she chooses IL; hence $EU_{i,IL}$ remains the same.

[20] Since my main interest lies in the comparison between JL and IL, in the next analytical part of this section I only concentrate on those two.

Now the expected discounted utility of individual i, when choosing JL, becomes,

$$EU_{i,JL,\beta} = \frac{1}{1-\delta(2p-p^2)}[(1-\beta)(p^2U_i(\gamma;\pi-\alpha\pi)+p.(1-p)U_i(\gamma;\pi-2\alpha\pi))+\beta U_i(EV_{j,JL})] \quad (6)$$

In Equation (6), the first component in the square brackets is the $(1-\beta)$-weighted expected utility of i from her own payoffs of the different outcomes in each period. And, the second component is the β-weighted utility of i from the expected value of j's income in each period, where $EV_{j,JL} = p^2(\pi-\alpha\pi)+p(1-p)(\pi-2\alpha\pi)$. Since the latter term is devoid of i's risk, the expected income of j will remain as a constant term in i's utility function in every period.

In this setting, i would prefer *JL* to *IL iff* $EU_{i,JL,\beta} \geq EU_{i,IL}$.
By assuming $\beta > 0$ and other things equal, it simplifies to the following condition in a *CRRA* setting.

$$\frac{\beta(1-\delta p)}{(1-p)(1-2\delta p)}[\frac{EV_{j,JL}}{p(\pi-\alpha\pi)^{(1-\gamma)}}-p] \geq 1-\frac{(1-\beta)(1-\delta p)}{(1-2\delta p)}[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}]^{(1-\gamma)} \quad (7)$$

Corollary 2 follows from here.

Corollary 2. *If incidence of social or other-regarding preferences is considered, and thus if the borrower of JL loan derives additional positive utility from partner's payoff, then Corollary 1 could be revised as follows: Given discount factor* δ' *and risk aversion* $\gamma'(<\gamma^{**})$*, it is still possible for the borrower to prefer JL over IL as long as the condition given by Equation* (7) *holds. And, the more weight she puts on the partner's payoff (i.e. higher* β*) the stronger is her preference for JL.*

Proof: See **Appendix A**.

Now, we move on to the experiment to empirically verify if and to what extent the discussed parameters are driving the take-up decisions of the prospective borrowers in the lab. However, in Sections 4.2.2 and 4.2.3, while estimating the effects of these preferences on take-up of loan type, I employ a reduced-form analysis and thus abstain from validating the exact threshold levels given in the propositions. In order to validate the *precise* predictions in Propositions 1 and 2 and Corollaries 1 and 2, one needs to assume a particular form of the utility function. Although I provide examples with a CRRA form in the predictions, I refrain from restricting my data analysis to this particular function. The overall implications of the theoretical predictions are that preference for JL(IL) increases with increase(decreases) in risk aversion, discount factor and other-regarding preferences; and, a reduced-form analysis is sufficient to verify these effects.

3. Experimental design & laboratory procedure

The main focus of my experiment is to examine if prospective borrowers at all decide on the basis of their heterogeneous preferences when they take or reject an offered loan type. If so, then possibly a flexible choice-set would help them pick the loan that suits their preference-profile the best; and this, as a result, would also contribute to an overall higher take-up.

3.1. Details of the treatment designs

The experimental design had three treatment variations: one group was offered a choice-set of IL, JL and EMPL, another group was offered a choice-set of IL and EMPL and the last group was offered a choice-set of JL and EMPL. I henceforth address these groups as IL-JL-EMPL, IL-EMPL and JL-EMPL respectively. Comparison of IL-JL-EMPL with the other two groups helps to evaluate whether the flexibility of being able to self-select from a bigger choice-set increases overall take-up or not.

Depending on the treatment being played in a session, the participants were offered to choose any of the income options available in the choice-set.[21] They were told that they would play for several *periods* with their respective chosen options, but that they could make the choice only in the first period. This implies that when they decided on which option to take up, they did so without knowing for how many periods the game would continue. They were only told that after every period, the next period might not occur with a 10% chance; and, should it happen, the game would end.[22] Now, a *round* of the experimental session consisted of this kind of a game.

In every session, I played ten rounds, and at the beginning of each, the participants could revise their choices from the set of income options available in the respective treatment.[23] Summarily, in each round, a participant could choose an income option from the offered menu and then she played an 'infinite'[24] horizon game. As a result, her decision-making over what to choose at the beginning of a round reflects the optimization behaviour presented in the theoretical model.

3.1.1. Design details of the three different income choices

Individual liability loan (IL). If the subject chose this income type at the beginning of a round, then she received a loan of 100 ECU (Experimental Currency Unit) in the first period. She invested that loan in a business which would yield 500 ECU with 50% chance and 0 ECU with 50% chance. At the end of the first period, she had to pay back the loan of 100 ECU plus an interest rate of 20% as a fee for the loan. If the

[21] I ran several sessions of the experiment in the lab, and I assigned one session for one treatment type only.

[22] To implement the dynamic incentive of the loans, it was necessary to have multiple periods so that if a loan is repaid successfully, another loan could be obtained. To put it into context, usually with dynamic incentive, the prospective borrowers are unaware of the exact number of periods in future until which the loans would keep coming in the case of successful repayment. So when they optimize their expected discounted utility *ex ante*, they do it over infinite periods. Replicating such a scenario in the lab would require an infinitely/indefinitely repeated game. And, a classic way of implementing it is by using a *random termination* (RT) which links the number of expected repetitions of the periods to the discount factor (Roth and Murnighan, 1978). In RT games, after every period, the game continues for another period with a known probability δ, and ends with probability $(1-\delta)$, as has been implemented in this design. Though other random termination methods are available as well, the RT method is the closest to my context and is the most commonly used method (Fréchette and Yuksel, 2017).

[23] I employed the repetition of the rounds to study the evolution of the game, i.e. the choices of the subjects, as they gained more experience over the rounds (Iyenger and Lepper, 2000; Dal Bó and Fréchette, 2018).

[24] Given the method used to implement it in a lab, the game is not really infinite per se. The term 'infinite' is used more in reference to the theoretical framework which is used to analyse the situation. Hence, the terminologies *infinite* or *indefinite* or *random termination* are used interchangeably in the jargon of lab experiments (Dal Bó and Fréchette, 2018).

business investment was successful, then the loan could be repaid. As long as 120 ECU could be repaid by the subject, she received another loan of 100 ECU in the next period, which would be invested in the same way. This process of loan availability continued for an indefinite number of periods (i.e. until some period did not occur with a 10% chance). However, if the business investment failed in any period, then the earning was 0 ECU and thus, the repayment could not be done in that period. As a result, she was not eligible to get any further loan in the subsequent periods of that round, even if the periods continued.

Joint liability loan (JL) . If the participant chose this income type at the beginning of a round, then she was matched with another participant *(Participant B)*[25] with whom she took the joint loan[26] and then made a business investment. *Participant B* was randomly selected from among all those participants who also had chosen joint loan in the same round[27]. In all periods of the same round, the 'pairs' remained constant. Once the round was over (and should the subject choose JL again), she was randomly re-matched. No communication was possible with *Participant B*.

In the first period, they received a loan of 100 ECU each. Each invested this loan in a business of her own which would yield 500 ECU with 50% chance and 0 ECU with 50% chance. The repayment amount was same for each of them, i.e. 120 ECU. If both the business investments were successful, each could repay her own loan. If one investment was successful and the other was not, the subject with the successful investment repaid both loans plus both fees, i.e. altogether 240 ECU. As long as 2*120 ECU was repaid in total by one or both of them, each could receive another loan of 100 ECU in the next period, which was to be invested in the same way. This process of loan availability continued for an indefinite number of periods (i.e. until some period did not occur with a 10% chance). However, if both business investments failed together in any period, each earned 0 ECU and thus, no repayment could be done in that period. As a result, no further loan could be obtained by either of them in the subsequent periods of that round, even if the periods continued.[28]

Employment opportunity (EMPL). If the participant chose this income type at the beginning of a round, then she had 67% chance of finding employment in each period. If employment was obtained in a given period,

[25]The language was kept as neutral possible by avoiding terms like 'partner', 'pair', 'peer', etc. I stressed on avoiding such words in the verbal and written instructions, as well as in the main experiment.

[26]I also avoided using the strong word 'liability' in the JL loan while instructing the subjects; instead, I let them figure out by themselves from the features of the loans. In the instructions and in the experiment, I termed the loans as 'Individual loan' and 'Joint loan'.

[27]If the total number of JL takers in a round was odd, then the unmatched subject was informed on the screen that she could not be matched with a participant in that round, therefore the computer would act as *Participant B* for her and select random numbers to decide *B*'s investment outcome. This could not have harmed the take-up decision, at least in the first round, because the JL taker had already made her choice before this information appeared on screen.

[28]Also, to avoid *priming*, I did not specify to the subjects the collective probability of repayment after every period (i.e. 0.75) with the JL contract. Instead, I vividly explained to them how the success probabilities of the subject and the partner would work and what the different circumstances of repayment were (See Instructions in Supplementary file for more details). However, as pointed out by a referee, this could still be a potential drawback of the design, because not telling the subjects the collective probability explicitly could actually have caused loss of control on their beliefs that led to their respective choices.

the wage was 50 ECU, otherwise 0 ECU. This process of getting an employment opportunity continued for an indefinite number of periods (i.e. until some period did not occur with a 10% chance). Unlike the case with loans, the chance of finding employment in any given period was not dependent on whether the subject had found employment in the previous period.

Figures B.1, B.2 and B.3 in **Appendix B** illustrate how a round with each of the income options proceeds. For more details, see Instructions in the Supplementary File.

3.1.2. Design details of the experimental procedure

To determine the success or failure in loan investment or employment search, the subjects were supposed to click on the screen to obtain a random number (between 0 and 1, where all values are equally likely). If the subject had chosen EMPL and if the random number was ≤ 0.33, that would imply that she was unable to obtain employment in that period; if the random number was > 0.33, it would imply she could obtain employment in that period. If the subject had chosen IL or JL and if the random number was ≤ 0.50, that would imply that her own investment was unsuccessful in that period; if the random number was > 0.50, it would imply that her own investment was successful in that period.

The presentation of the experimental task was kept neutral without linking it to microfinance; this was to follow the practice in earlier relevant literature (Cason et al., 2012) as well as to avoid any uncontrolled connotation effect (Abbink et al., 2006). I deliberately avoided the use of any effort-involving task to determine the success of investment or employment search, because that could motivate the subjects not to put true effort due to intrinsic moral hazard and freeriding incentives; and in this paper I would like to focus precisely on issues that are already prevalent even after omitting moral hazard and freeriding incentives. Also, no transaction (e.g. disbursal of loan, repayment, etc.) was done physically during the experiment. Instructions were such that each subject would have an earnings account maintained for her during the entire session; for example, if she would choose loan, then in the beginning of each period her account would be credited with 100 ECU, in the end of the period the repayment amount would be debited from the account, etc.

I maintained some further restrictions in the design in order to keep it comparable to the assumptions in the theoretical setting. **1.** The subjects were not able to report falsely about their investment outcome to the experimenter and also to *Participant B* in case of JL. This was restricted by directly showing them the outcome of their investment or employment search on their screens, and also their net incomes after each period. In case of JL, information on *Participant B*'s investment outcome also appeared on the subject's screen and necessary repayment amounts were deducted directly from their accounts. **2.** By allowing no communication between the JL group-members, I precluded the possibility of collusion against the lender (experimenter in this case).[29] The aim was to see that when no communication is allowed, what the subjects would instinctively choose in the strictest condition, that is, with no communication, with a randomly chosen

[29]Field evidence shows that often due to close ties and information-sharing among group members, group defaults become

partner, etc. **3.** The subjects were told that they would play several rounds of the game during the session, and that each round would have few periods where the next period may not occur with 10% chance. **4.** The subjects were also told that after the experiment ends, each of them would randomly draw one of the rounds played and their respective incomes of the chosen round would be converted to Euros and handed to them.

In summary, the experiment was designed in a way that subjects could only take action on their choice-making of loan(s) or employment at the beginning of each round. Once the choice was made for a round, they basically had to draw the random probabilities which decided the outcome of their respective chosen option in each period. Otherwise, everything else was hard-coded and no decision-making was needed on their part until that particular round ended with random termination probability of 0.10.

3.2. Elicitation of heterogeneous preferences

Simply by looking at their choices of loan types, it is hard to understand what actually motivates the subjects' decisions. If one chooses IL, then there could be a number of intermingled motivations - she could be simply more risk-seeking, or she could dislike taking a partner's burden. And, if one chooses JL, then it could be due to her risk aversion to non-repayment, or her generosity in sharing the costs of JL with another subject. And, it is the relative strength of these multiple underlying motivations that dictates her choice. There is a possibility of some discount factor playing a role here too, in terms of patience and/or expectation about the length of the game (i.e. discounting between the periods within a round). While making a choice, if one chooses JL, then it could be due to her expectation that there would be several periods of loan availability within the round, and if she patiently plays along, it could lead to higher final gains; thus, she would calculate her *ex ante* expected utility with a considerably high discount factor. But if one chooses IL, then probably it is due to her impatience to secure the income of the immediate period(s) without waiting for bigger expected profits after several periods (which she believes might not occur); then she would calculate her expected utility with low discount factor.

I controlled for generosity[30] of the subjects to check if that affects their choice of loan. As a measure of generosity in sharing income, I used the standard one-shot DG (Forsythe et al., 1994), where the subjects were asked to share a reward of 3 Euros with an anonymous participant.[31]

I elicited the subjects' risk preferences using the test designed by Holt and Laury (2002) (henceforth, H&L test). In spite of certain disadvantages of this test, pointed out in recent experimental literature on

rampant. When the lender is unable to perfectly oversee, members can collude and maximize the total sum of their utilities (by defaulting together or writing side-contacts with third parties). An interested reader can look into Laffont and Rey (2003), where, in a theoretic model, the authors offer solutions to problems arising from collusion.

[30] In my opinion: since there is neither any communication between the JL partners, nor they have a say in whether they would like to contribute for the partner or not, the JL takers do not necessarily have to show reciprocity to each other for future cooperation; hence, I do not measure that. Rather, I am more interested in their attitude towards pure giving.

[31] Dreber et al. (2014) suggest that DG is better than ultimatum game/ one-shot Prisoner's Dilemma/public goods games in measuring social preferences, because it is the least sensitive to priming and framing effects.

risk elicitation, I do prefer it because it helps to easily point out and drop the observations that have made inconsistent choices.[32]

I further controlled for the discount rate of the subjects by using the measure proposed by Reuben et al. (2010)[33] to examine if there is at all any association in the subjects' discounting nature in this task with their choices of loan-types in the main experiment. However, I do not stress much on the effects of this measure because, in hindsight, the design of my microfinance game was not sophisticated enough to represent the 'waiting' between the periods so well that the subjects' discounting could affect their choice-making *ex ante*.[34] Furthermore, the discount rate elicitation task was not monetarily incentivized. Therefore, later in the discussion of results, I stress more on the effects of risk and social preferences.

3.3. Laboratory procedure

3.3.1. Recruitment

The experiment was conducted in the Public Choice Research Centre (PCRC) decision-making laboratory of University of Turku, Finland. The subjects were recruited using the ORSEE software (Greiner, 2004) and the computerized decision-making task was programmed and conducted on the z-tree software (Fischbacher, 2007). The experiments were conducted during January - March, 2017. Invitations were sent out to 1716 participants registered until that time. The language of communication used in the experimental sessions was Finnish. (See more details in Supplementary file)

3.3.2. Implementation

13 sessions were allotted for the main experiment- 5 for IL-JL-EMPL, 4 for IL-EMPL and JL-EMPL each. In total 65, 63 and 94 invitees turned up to participate in IL-EMPL, JL-EMPL and IL-JL-EMPL respectively. The participants of each session were randomly allocated to the visually isolated computers in the lab. The instruction pages were then handed to them and an associate read it aloud to them. After that, the subjects were given some additional time to go through the instructions by themselves. Before

[32] Crosetto and Filippin (2016), in their study on comparison of the popular risk elicitation tests, point out that H&L test is often difficult to comprehend by subjects due to its complicated probabilities, and therefore wrong understanding can lead to multiple switching points. But then again, the authors agree that not a single test is perfect, and once the observations with inconsistent choices (multiple switching points) are removed, H&L test performs considerably well.

[33] In this test, subjects are given a set of 9 decisions- each decision consists of choosing between an amount 50 Euros today and a larger amount $(1 + r) * 50$ Euros in 1 week. For the set of decisions, the values of r used are: 0.00, 0.01, 0.03, 0.05, 0.07, 0.09, 0.10, 0.15, and 0.20. With gradual increase in r over the 9 decisions, at some value of r the subject switches from 50 Euros today to $(1 + r) * 50$ Euros in a week; the r at switching point serves as the subject's discount rate.

[34] My design did not allow the subjects to physically receive the pay-offs after each period at different time points. Instead, all of them received the payments simultaneously at the end of the session. Hence, while they decided on their expected discounted utility *ex ante*, their *patience* most likely did not affect their decision. Even if I would argue that the *continuation uncertainty* of the subsequent periods within a round dictates the discounting more relevantly in this context than *patience* (refer to Footnote 10), then also the measure by Reuben et al. is not suitable, because it elicits *time preferences* rather than *continuation uncertainty*.

Round 1 started, a few 'test' questions along with multiple-choice answer options appeared on the subjects' screens; the questions were to control for their comprehension of the crucial points of the instructions. If they marked a wrong answer, a hint was provided on screen to re-read/re-think in case they have misunderstood the steps. (See more details in Supplementary file.)

In each session of the experiment, there were ten rounds of choice-making. Each round started simultaneously for every subject. At the beginning of each round, the subjects were able to select an income type - IL, JL or EMPL depending on what was available in the choice-set in that particular session. Each round continued for a few periods, with each new period having a 10% chance of not occurring.

At the end of the final round, a few more questions were asked which gave them the opportunity to add to their earnings from the main experiment. These questions were the one-shot DG and the H&L risk aversion test. Then the subjects were to randomly draw a number between 1 and 10; this number signified the round from which the experimental earnings (i.e. the sum of earnings from all the periods of that chosen round) were to be converted to the subject's actual earnings from the main experiment.[35] The subjects were then asked a few more questions - these were the test for discount rate; the 10- point-scale standardized survey questions on risk and trust as used in the lab version module on preference elicitation by Falk et al. (2016).

On way out, subjects were paid in cash a total sum of the 3 Euros as participation fee, the earnings from the main experiment and the additional earnings from the one-shot DG and the H&L test. An entire session lasted for about an hour.

4. Results

4.1. Pilot studies, sample size and statistical power

Prior to the main experiment, I also carried out pilot sessions with all the three treatment types. The three pilot sessions, each consisting of one of the three treatment types, were conducted in December, 2016 in the same lab. The subjects of the pilot were also recruited using the ORSEE software (Greiner, 2004). However, the subjects in the pilot were never invited back to the main experimental sessions. Since there is no similar previous study to get advice on effect size, I had to rely on the observed outcomes of the pilot treatments. In the pilot, the take-up proportions of the loans were 0.50, 0.70 and 0.84 with sample sizes 20, 19 and 20 in the IL-EMPL, JL-EMPL and IL-JL-EMPL treatments respectively. The difference in proportion in terms of *Cohen's h*[36] (Cohen, 1988) is 0.75 between treatments IL-EMPL and IL-JL-EMPL and 0.34 between JL-EMPL and IL-JL-EMPL. The former is a medium-to-large effect while the latter is a small-to medium effect as per *Cohen's h* standard. Even though drawing inference on effect sizes observed in

[35] i.e. they earned their total income of one of the ten games they played. See Dal Bó and Fréchette (2018) for the choice of payments.

[36] In statistics, *Cohen's h* is a popular measure of difference between two independent proportions. '*h*' is the difference in the arc-sine transformation of the two proportion values. The rule of thumb allows h=0.20 as small, h=0.50 as medium and h=0.80 as large differences.

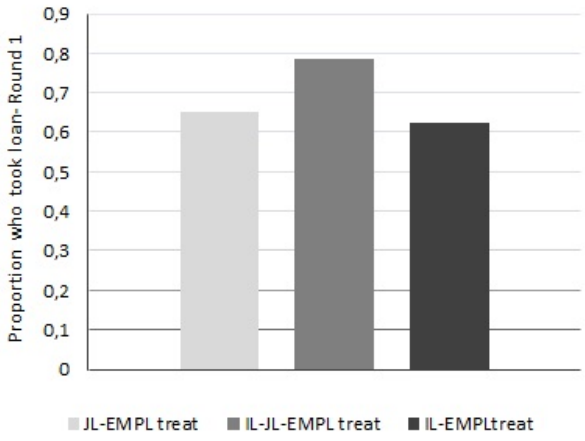
such small samples would be risky, I had to base my sample size and power calculations on that. I wanted to have enough sample size to be well-powered to detect similar effects in the main experiment. Thus, I decided on having around 100 subjects in IL-JL-EMPL group and around 80 each in IL-EMPL and JL-EMPL groups. For a medium-sized effect of h =0.44 (which is equivalent to 0.80-0.60=0.20 in terms of absolute difference in proportions) this would give a power of 83%.[37]

4.2. Main experimental findings

This section presents the experimental findings. For analysis of take-up rate and heterogeneity in preferences, I focus only on the take-up rates in Round 1 of the different treatment groups. Concentrating on the first round data will protect from potential threats against statistical independence of the choices made by each subject in the subsequent rounds. In Section 4.2.4, I investigate any learning effect among the subjects over the rounds.

4.2.1. Summary statistics

Figure 1: Take-up of loans in the three treatments- Round 1

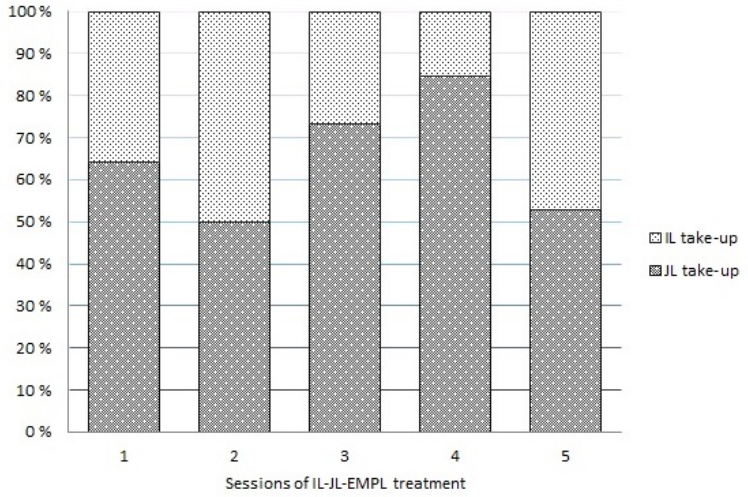


Notes: This figure describes the proportion of loan takers, irrespective of the loan type, in the first rounds of the three treatments.

Figure 1 gives an overview of the take-up proportions in Round 1 across the three different groups. The percentage of take-up was 78.5% in IL-JL-EMPL, whereas in IL-EMPL and JL-EMPL the percentages of take-up were 62.5% and 65.1% respectively. Non-parametric analysis with Chi-square test for independence between the three treatment types and take-up proportion rejects the hypothesis of independence ($p = 0.060$);

[37] If the effect sizes would be similar in the main experiment as in the pilot versions, then the decided sample sizes would give a power of 99% while testing between IL-EMPL and IL-JL-EMPL and a power of 61% in case of JL-EMPL and IL-JL-EMPL.

Figure 2: Distribution of JL and IL take-up among the loan takers in Round 1 of Treatment IL-JL-EMPL



Notes: This figure highlights the respective shares of IL and JL takers in the first rounds of the five sessions that played the treatment IL-JL-EMPL. In these rounds, subjects chose EMPL as well, with around 20% takers on average across the five sessions.

this allows to conclude that take-up behaviour significantly varies across the three groups. When I use this same test to further check separately between groups IL-EMPL & IL-JL-EMPL and then between JL-EMPL & IL-JL-EMPL, I reject independence of treatment type and take-up behaviour at 5% ($p = 0.028$) and 10% ($p = 0.064$) levels respectively.

Figure 2 gives a closer look at the distribution of JL and IL take-up among the loan takers in Round 1 in the IL-JL-EMPL treatment. The idea is to see which loan type was relatively more in demand when both types were available in the choice-set. A binomial test confirms that JL loan take up is significantly higher ($p = 0.014$). This indicates that when borrowers can make sure that partners would not be able to cheat, then JL could excel in take-up rate.

Table 1 comprises the summary statistics of the individual-specific control variables of the subjects. For clarity in understanding the summary statistics of the variables, I only include the observations where no inconsistent choice was made for the corresponding variable.[38] However, while conducting non-parametric tests on the difference of the given variables across the three treatments, I include the inconsistent ones as well; this helps to compare if the inconsistent observations also differ significantly across the three treatments. By using the Kruskal-Wallis H test[39], I find that for all the three individual-specific control variables, there

[38]It has been already mentioned before that H&L test is prone to accumulating inconsistent observations, i.e. those who switch among the lotteries more than once. It is not surprising that here too, there were 57 such observations (15 in IL-EMPL, 18 in JL-EMPL and 24 in IL-JL-EMPL).

[39]It is a rank-based non-parametric test that is used to determine if there are statistically significant differences between two or more groups on a continuous or ordinal dependent variable.

is no statistically significant difference in the mean ranks across the three treatment groups (Column (7) in Table 1).

Table 1: Summary statistics of individual-specific control variables

Variable	N[a]	Mean	Median	St.Dev.	Min.	Max.	p-value[b]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No.of safe choices in H&L test							
Treatment IL-EMPL	49	5.776	6	2.374	0	10	
Treatment JL-EMPL	45	5.533	5	2.074	0	10	0.15
Treatment IL-JL-EMPL	69	6.551	7	2.153	0	10	
Shared amount in DG							
Treatment IL-EMPL	64	0.852	1	0.776	0	3	
Treatment JL-EMPL	63	0.964	1	0.770	0	3	0.55
Treatment IL-JL-EMPL	93	0.829	1	0.609	0	2	
Discount rate							
Treatment IL-EMPL	64	0.042	0.01	0.047	0	0.2	
Treatment JL-EMPL	60	0.041	0.03	0.042	0	0.2	0.99
Treatment IL-JL-EMPL	92	0.040	0.02	0.045	0	0.2	

Notes: [a] N denotes the number of observations in the respective treatments who did not make any inconsistent choice in the corresponding test. [b] The p-values indicate the statistical significance of the Kruskal-Wallis H test.

4.2.2. Determinants of loan take-up

In addition to the non-parametric tests, I regress take-up of loan on the 3 different treatment categories in Round 1. I then add the individual-specific control variables in the regression. I use the linear probability model specification for each case:

$$Y_i = \alpha + \sum \beta_t * Trt_t + \boldsymbol{X_i'}\gamma + \epsilon_i, \tag{8}$$

where, Y_i is the binary take-up of loan by individual i. Trt_t is the dummy variable that takes value 1 if individual i was in treatment t, otherwise 0; with β_t being the treatment effect. $\boldsymbol{X_i'}$ is the vector of individual-specific control variables. As already mentioned at the end of Section 2, I only employ a reduced-form analysis here.

Table 2 summarizes the regression results of the treatment effects on take-up of any loan type in Round 1. In Column (1), we see that with respect to IL-JL-EMPL, take-up is lower by 15.99 percentage points (pp.) ($p = 0.033$) and by 13.42 pp. ($p = 0.072$) in IL-EMPL and JL-EMPL respectively. When the individual-specific control variables are included, a portion of the sample is automatically dropped, i.e. those 57 observations whose measure of risk is inconsistent. In Table 2, Column (2) reports only the treatment effect again, with this updated sample, in order to give better comparability with Column (3) estimates. Note, with the updated sample, the take-up rate in JL-EMPL treatment does not vary significantly any more from that in IL-JL-EMPL. This could be due to the reason that several observations are now dropped,

as described above. Furthermore, calculating with the given values of the parameters in the experimental task, the relative expected gain from choosing IL over EMPL is not as high as it is from choosing JL over EMPL; that is probably why we see that take-up of loan does not differ much between treatments JL-EMPL and IL-JL-EMPL, because JL clearly excels due to its relatively higher expected value. In Column (3), we see that when the individual-specific variables are controlled for, the treatment effects maintain the same direction and the model's explanatory power no doubt increases. I find a statistically significant decline in the probability to take up any loan by 4.4 pp. ($p = 0.010$), with unit increase in the level of risk aversion (i.e. the number of safe lotteries chosen in H&L test). However, no significant effect of either altruism or discount rate found.[40], [41] Finally, the results remain unchanged to robustness checks.[42]

4.2.3. Determinants of choice between loan types

4.2.3.1. Results of OLS regressions on take-up of the two loan types in Round 1. I separately study the take-up of the two different loan options in Round 1. To estimate the impact on the take-up of each of the two loan types, I again use a linear probability model. Table 3 Columns (1) and (2) respectively report the effects on take-up of JL loan and IL loan across the treatment variations.

For JL take-up. I find that only altruism has a statistically significant effect in group IL-JL-EMPL; a unit increase in DG-sharing increases probability to take up JL by 23.60 pp. ($p = 0.012$). The interaction coefficient -.1976 gives the difference in the slopes of DG-sharing in groups JL-EMPL and IL-JL-EMPL. Though this interaction term is statistically significant (barely at 9.5%), the average marginal effect of DG-sharing on JL take-up remains positive in the treatment group JL-EMPL (i.e. main effect + interaction effect=0.04).[43] In case of risk aversion, I do not find any significant difference in slopes across the treatments.

For IL take-up. The measure of risk aversion has a statistically significant effect in group IL-JL-EMPL; with unit increase in risk aversion, the probability to take up IL loan decreases by 5.65 pp. ($p = 0.062$). The interaction coefficient indicates that the difference in the slopes of risk aversion in groups IL-EMPL

[40] The most pressing shortcoming of LPM as a binary response model is that its estimates do not restrict themselves within zero and one, and this potentially leads to bias and inconsistency. However, a *trimming rule*, proposed by Horrace and Oaxaca (2006), suggests that as long as the number of observations with predicted probabilities outside the unit range is small and as long as these observations are dropped, the estimates from LPM are consistent. Hence, I use this refinement for my estimations.

[41] Also note, I find the predicted probabilities with the LPM model to be very identical to those from logit and probit models, the correlation between the predicted probability vectors being around 0.99 *(logit and probit results not reported here)*.

[42] In my sample, ten subjects shared more than half in the dictator game, five of whom shared the entire sum of 3 Euros. There was no way to verify if these subjects had mistaken the amount to be shared for the amount that they wanted to keep for themselves. Therefore, to avoid predicting the effects based on any confusion, I conduct a robustness check by repeating the regression after dropping these ten observations. The results do not change in any meaningful way. (See the comparison of results in Table C1 in **Appendix C**).

[43] In Table C2 in Appendix C this interaction term becomes statistically insignificant, when I drop the ten observations who share more than half in the DG.

Table 2: Take-up of loan in Round 1

Variable	(1)	(2)	(3)
Treatment Category:			
IL-JL-EMPL	*(base)*	*(base)*	*(base)*
IL-EMPL	-.1599**	-.1585*	-.1995**
	(.0745)	(.0835)	(.0848)
JL-EMPL	-.1342*	-.0560	-.1092
	(.0741)	(.0802)	(.0814)
Individual specific controls:			
No. of safe lotteries chosen			-.0441***
			(.0170)
Shared amount in DG			.0628
			(.0420)
Discount rate			.9591
			(.8273)
Constant	.7849***	.8116***	1.0148***
	(.0429)	(.0475)	(.1284)
Observations	220	163	159
R^2	0.0256	0.0236	0.0767

Notes: 1. ***, **, * denote statistical significance at 1%, 5% and 10% respectively; **2.** heteroscedasticity-robust standard errors are in the parentheses; **3.** H&L risk-inconsistent observations are dropped in Columns (2) & (3); **4.** 4 observations are dropped due to Horrace & Oaxaca trimming in Column (3).

and IL-JL-EMPL is not statistically significant. Here, DG-sharing has neither significant main effect nor interaction effect.

Therefore, combining the evidence on effects of the individual-specific control variables across the treatment categories, I can conclude that altruism has a positive effect on probability to take JL loan and risk aversion has a negative effect on probability to take IL loan. And, since there is no strong, statistically significant difference of the average marginal effects across the treatment categories, we can conclude that more or less similar effects persist across the comparable treatments.

Summing up the findings: the risk parameter drives the take-up of IL more strongly than the social preferences parameter, with increased risk aversion reducing inclination for this loan type; whereas for JL loan, the social preferences parameter seems to be the stronger driving factor, with more altruistic subjects showing more willingness to choose it. Thus, relating the findings in the lab to the theoretical predictions, IL choice does relate to less risk aversion and JL choice to more other-regarding preferences.[44] As mentioned

[44]Note, the findings in Tables 2 and 3 remain unchanged when I control for additional individual-specific variables, e.g. subject's gender, if study-field is Economics, Mathematics or Statistics, etc.*(results not reported here)*

Table 3: Effect of heterogeneous preferences on take-up of JL & IL loans across the different treatment groups in Round 1

	Take-up of JL (1)	Take-up of IL (2)
Treatment Category:		
IL-JL-EMPL	*(base)*	*(base)*
JL-EMPL	.5981*	
	(.3645)	
IL-EMPL		.1991
		(.2946)
Nr. of safe lotteries chosen (in IL-JL-EMPL)	.0198	-.0565*
	(.0320)	(.0299)
Treatment category X Nr. of safe lotteries chosen	-.0448	-.0140
	(.0530)	(.0401)
Shared amount in DG (in IL-JL-EMPL)	.2360***	-.1209
	(.0928)	(.0826)
Treatment category X Shared amount in DG	-.1976*	.1428
	(.1170)	(.1107)
Discount rate (in IL-JL-EMPL)	.2805	-.5743
	(1.3441)	(1.1014)
Treatment category X Discount rate	.5186	3.176*
	(1.8663)	(1.7066)
Observations	114	115
R^2	0.1060	0.2542

Notes:

1. *** and * denote statistical significance at 1% and 10% respectively; **2.** in Column (1) only treatments JL-EMPL and IL-JL-EMPL are considered, whereas in Column (2) only treatments IL-EMPL and IL-JL-EMPL are considered; **3.** heteroscedasticity-robust standard errors are in the parentheses; **4.** only H&L risk-inconsistent observations are dropped; **5.** 2 observations are dropped due to Horrace & Oaxaca trimming in Column (1) and 6 observations are dropped due to Horrace & Oaxaca trimming in Column (2).

previously, due to limitations in the design of the discounting task, its findings are not stressed upon.

Regarding the inference on the association of individual preferences and the take-up decision of the subjects, one could point out the potential risk of reverse causality since the preference measures were elicited after the loan type choice-making. Though this proposition cannot be completely denied, I counter-argue that the chances are quite low. This is because the determination of final earnings of the microfinance experiment was done after the elicitation of social and risk preferences. So at the time the subjects were doing these additional tasks, they simply knew that they would earn according to one of the several rounds which they had just played and that round would be chosen randomly. In addition to that, while studying

effects of preferences on take-up type, I use the first round's choices only, and it is unlikely that the first round's choice-making could have particularly affected any subject's performance in the preference elicitation task which came quite later.

4.2.4. Impact of treatment on take-ups of different loan types over the rounds

I now move on to the analysis with all rounds' data. Iyenger and Lepper (2000) and Dal Bó and Fréchette (2018), in different contexts inside and outside the lab, mention that individuals learn with experience (or, repetitions of matches/supergames in the lab) and this learning substantially affects the choices they make later. In this capacity, I examine if after playing the same 'infinite' horizon game repeatedly, the subjects ended up with a favourite choice. Each session had ten rounds. In the beginning of each round, the subjects could make a new choice of the income type with which they played an indefinite-period game in that round. I separately study the effect on take-up of the two different loan types. I use the two-way interaction between *lagged take-up* (binary: 1 if the choice type in question was taken in the past round, 0 otherwise) and *lagged earnings result* (binary: 1 if earned more than zero in the past round, 0 otherwise). The lags are by one round. The individual-specific control variables and the treatment type are also present in the specification.

Using LPM with subject fixed effects, I do not find any interesting outcome as such.[45] According to the panel regression results of both JL and IL (given in Table 4 Panels A and B), there is statistically significant evidence at 7% and 5.6% that if earnings in the last round was zero, then willingness to take JL (IL) in current round by those who had taken it in the last round is 12.94 (10.60) pp. less than those who had not taken it in the last round. This implies that if earned nothing with a certain loan type in the last round, subjects are less willing to repeat take-up of the same loan type in the current round. While comparing the interaction effect (positive in case of JL and negative in case of IL), we can infer the following: in case of IL take-up, even if positive income was earned with IL in the last round, subjects are even less likely to take it again compared to those who earned zero income with it in the last round. But in case of JL, subjects are relatively less likely to turn away from re-take-up if earned a positive income in the last round than those who earned zero income with JL in the last round. However, the interaction effects are not statistically significant to confirm this interpretation. The implications remain similar when I drop the ten observations who share more than half in the DG; however, the main effects of *lagged JL(IL) takeup* on re-take-up become statistically insignificant (See results in Table C3 in Appendix C). In sum, it is safe to conclude that the subjects did not stick to only one choice type; even if they earned some positive income from their chosen type in any round, they showed less willingness to choose the same option again in the next round.

[45]Note that the individual-specific variables including the treatment type to which the subject belonged, are automatically omitted in the fixed effects estimation, since they do not vary across the rounds.

Table 4: JL & IL take-ups in 10 rounds

Panel A: JL take-up	
Lagged JL takeup	-.1294*
	(.0718)
Lagged earnings outcome	-.0120
	(.0622)
Lagged JL takeup *X* Lagged earnings outcome	.0962
	(.0711)
Subject fixed effects	Yes
Observations	1026
R^2	
overall	.0419
within	.0139
between	.6719
Panel B: IL take-up	
Lagged IL takeup	-.1060*
	(.0548)
Lagged earnings outcome	-.0213
	(.0330)
Lagged IL takeup *X* Lagged earnings outcome	-.0072
	(.0432)
Subject fixed effects	Yes
Observations	1062
R^2	
overall	.2362
within	.0122
between	.9756

Notes: 1. * denotes statistical significance at 10%; **2.** only treatments JL-EMPL and IL-JL-EMPL are considered in Panel A and only treatments IL-EMPL and IL-JL-EMPL are considered in Panel B; **3.** standard errors clustered by subjects are in the parentheses; **4.** only H&L risk-inconsistent observations are dropped; **5.** in Panel A(B), the coefficient of the interaction term reports the difference in effect of lagged take-up of JL(IL) for *Lagged earnings outcome=1* when compared with effect of lagged take-up of JL(IL) for *Lagged earnings outcome=0*.

5. Conclusion

The motivation for this study comes from the gradual decline in faith in microcredit loans as a global poverty alleviation tool. From large-scale field experiments implemented in several developing countries, the short-run and long-run welfare effects from microcredit are found to be miles behind what had been projected two decades ago. And, one of the key instruments for this bad performance could be the low

demand or take-up of these loans by those poor who are the very target group. This outcome of low take-up is common to many field experiments conducted over the last decade. Such unanticipated findings from big studies give rise to a new interest in the post-analysis of what could have gone wrong. This study makes a modest attempt to explore certain behavioural channels that could provide some answer. I start with a very controlled set-up in the lab where I simply try to dissect the advantageous and disadvantageous sides of IL and JL loan types and understand if individuals with different preferences are inspired by the loan features differently. I argue that if it is so, then borrowers should be able to better self-select into their desired loan type from a flexible choice-set of different types; this, in turn, would be able to lead to the final goal of increasing overall take-up. Therefore, the aim of this paper is to show that the heterogeneity in preferences of prospective borrowers drives take-up. In a lab experiment with student subjects, I study a group who are offered a broader choice-set of both loan types *vis-à-vis* two other groups who are offered only one type each. I do find statistically significant evidence that take-up rate is higher in the former group. More importantly, I find statistically significant evidence that risk averse subjects totally stay away from any loan type and takers of JL type have more than 'self-regarding' preferences.

A limitation of this lab setting is that the JL loan type only allows partnering up with someone anonymous, which is unlike the case in real setting [46]. In defence of that limitation, if already JL is well in demand in a framework which allows only random anonymous partners, then its demand would certainly be more pronounced in the real situation where the borrowers are willing to partner up with friends and acquaintances.[47] If we think of the real situation, JL loan is actually a safer choice if and when borrowers are fully assured that their partners cannot cheat on them and will help them in case of a genuine investment failure. If such an assurance is there within a group - by means of information symmetry on investment outcomes, then it is justified that the borrowers would be convinced to opt for it. However, it would be interesting to check how the interplay of preferences evolves when information asymmetry on investment outcomes is allowed within the group, because that will unleash uncertainties in several aspects. For example, it would be interesting to see if the fear of getting freeridden by the partner demotivates a borrower from choosing JL or if the freeriding incentive actually drives the borrower to choose JL?

Moving on to further conclusions from this study, though we find that JL loan has a higher demand in a flexible choice-set, we cannot, however, ignore the need for IL loan. According to the evidence here, demand for JL is higher among those who show more generosity in sharing income; therefore, those who are not so generous and yet have less risk averse preferences could be more interested in IL loan. As a result, being a lender, it would be useful to elicit the preferences of the prospective borrower and then offer a suitable loan type; or, if preference elicitation is not feasible, it is better to offer a bigger choice-set of loan types so

[46] although exogenous group formation has been used to some extent in the context of urban slums (Armendáriz de Aghion and Morduch, 2007)

[47] Nevertheless, Wydick (1999) finds evidence from group lending in Guatemala that social ties per se have little or no impact on borrowing group behaviour.

that the borrower has room for self-selection. In the context of lending flexible choice-sets, Ghatak (2000) is the first to explore the feasibility of a menu of JL contracts (where IL type is also present as a special case of JL with zero liability). Ghatak shows that the lender as a *planner* can, under certain conditions, optimally offer different contracts in a separating equilibrium to groups differing in risk types. So far, I have abstracted from discussing optimal lending conditions, but if the lender aims to maximize aggregate surplus as a planner, it would not be impossible to find out conditions in my set-up either, under which she can be better-off by offering optimal separating JL and IL contracts to borrowers who vary in risk and social preferences.[48]

Despite such theoretical feasibility for the lender to offer an optimal menu of various contracts from which borrowers of different types can self-select, in practice, we have seen most microfinance institutions (MFIs) across the world tied to one contract type only. Though some eventually introduced different contracts in parallel, they usually offered different loan types according to the risky nature or business size of the prospective borrower, as opposed to giving the borrower the possibility to self-select from a menu.[49] This way of offering loan type according to the need of a particular borrower is indeed conducive to better take-up, however, it is still limited to situations where the lender can elicit information on risk and size of investments (e.g. a borrower with a successful repayment history on previous loans from the MFI, can be easily offered an individual loan since she has proven her reliability). But, it is not always possible to elicit this information on the nature of the investment and borrower (e.g. first-time borrowers). This is where my experiment tries to provide some solution. It tests whether under the circumstances, where the lender does not have much information, offering a menu of contract types for self-selection increases take-up.[50]

With a final reflection on the outcomes of the experiment, though the magnitude of effects found here may not be the same in a framed-field/field experiment, the directions of the effects provide significant evidence that loan offers personalized according to heterogeneous preferences of the borrowers would lead to a better take-up rate.

[48] Ghatak further shows that a separating equilibrium can exist in a competitive credit market too, where lending firms compete by offering loans of varying amounts of joint liability (including IL with zero liability). Although Ghatak's lending structure differs from my set-up (in terms of dynamic incentive and adverse selection), in essence, a competitive equilibrium can be feasible in my setting too, e.g. where *lender A* offers JL and its competitor *B* lends IL and the borrowers can self-select the lender type (e.g., Ghatak points out that in the 90s Bolivia, Banco Sol offered JL loans and its competitor Caja Los Andes offered IL, and thus catered to different types of borrowers).

[49] For example, both Banco Sol and Grameen Bank after pioneering in group-lending, introduced IL contracts in parallel for the clients who were successful and/or who preferred not being obligated to others by being in a group. Other examples are two Canadian MFIs which offered both IL and JL contracts in the early 2000s, but the borrowers who received IL loans usually had higher income, larger and reliable businesses (Armendáriz de Aghion and Morduch, 2007).

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Appendix A.

Proof of Proposition 1. By assumption, $s < (\pi - \alpha\pi)$; therefore, the *RHS* in (3') increases with γ. This implies that when γ increases beyond a certain threshold, say γ^*, the *RHS* exceeds the *LHS* and it is not optimal to choose IL instead of EMPL anymore.

Again, as $(\pi - 2\alpha\pi) < (\pi - \alpha\pi)$, with decrease in the value of γ the *LHS* becomes smaller. As γ declines and crosses a certain threshold, say γ^{**}, the inequality in (5') does not hold anymore. (Note, in order to have EMPL preferred to JL, given that JL has a higher expected discounted utility than IL, a borrower has to have an even higher threshold, say γ^{***} ($\gamma^{***} > \gamma^*$).)

Thus, combining the two inequalities in (3') and in (5') together, the thresholds of preferences in case of a *CRRA* utility function can be obtained. This can be further extended for the general functional form, as described by Proposition 1.

Proof of Corollary 2. $EU_{i,JL,\beta} \geq EU_{i,IL}$ implies

$$\frac{(1-\beta)p}{1-\delta(2p-p^2)}[pU_i(\gamma;\pi-\alpha\pi)+(1-p)U_i(\gamma;\pi-2\alpha\pi)]+\frac{\beta}{1-\delta(2p-p^2)}U_i(EV_{j,JL}) \geq \frac{1}{1-\delta p}.p.U_i(\gamma;\pi-\alpha\pi) \quad \text{(A.1)}$$

By simplifying (A.1) and using a *CRRA* utility function, *JL* is preferred to *IL* as long as the following holds:

$$\frac{\beta(1-\delta p)EV_{j,JL}}{p(1-p)(1-2\delta p)(\pi-\alpha\pi)^{(1-\gamma)}}-\frac{\beta p(1-\delta p)}{(1-p)(1-2\delta p)} \geq 1-\frac{(1-\beta)(1-\delta p)}{(1-2\delta p)}[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}]^{(1-\gamma)} \quad \text{(A.2)}$$

Of course, when $\beta = 0$ even for the JL utility function, (A.2) simplifies to (5'). Other things equal, when γ is lower than the threshold γ^{**}, (5') does not hold anymore. Therefore, in a $\beta = 0$ model, an individual with risk aversion $\gamma'(< \gamma^{**})$ would optimally always choose IL over JL.

But, by assuming $\beta > 0$ and $\gamma = \gamma'$, while other things remain equal, JL $\succ$ IL as long as,

$$\frac{\beta(1-\delta p)}{(1-p)(1-2\delta p)}[\frac{EV_{j,JL}}{p(\pi-\alpha\pi)^{(1-\gamma')}}-p] \geq 1-\frac{(1-\beta)(1-\delta p)}{(1-2\delta p)}[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}]^{(1-\gamma')} \quad \text{(A.3)}$$

(Note that, it is also possible to find similar conditions by simultaneously varying the discount factor δ.)

To simplify the implications of the mathematical findings above, let's look at an example where IL $\succ$ JL when we consider a β-free model, but it is not necessarily true when β is positive.

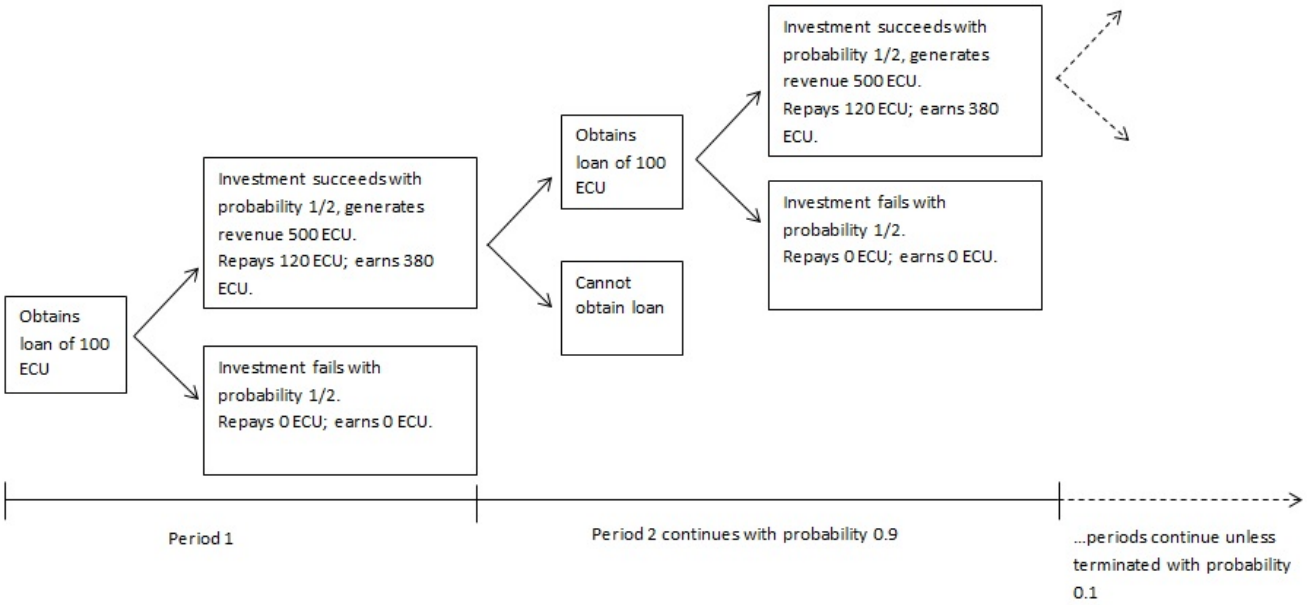
The simplest case is with $\gamma = 0$ and $\delta = 0$ *(a risk neutral individual who discounts future totally)*, where (A.3) boils down to the following condition so that JL can yield higher expected utility than IL,

$$\frac{\beta}{(1-p)}[\frac{EV_{j,JL}}{p(\pi-\alpha\pi)}-p] \geq 1-(1-\beta)[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}] \quad \text{(A.4)}$$

However, when $\beta = 0$, JL $\not\succ$ IL as the above inequality never holds ($1-\frac{\pi-2\alpha\pi}{\pi-\alpha\pi} \not\leq 0$).

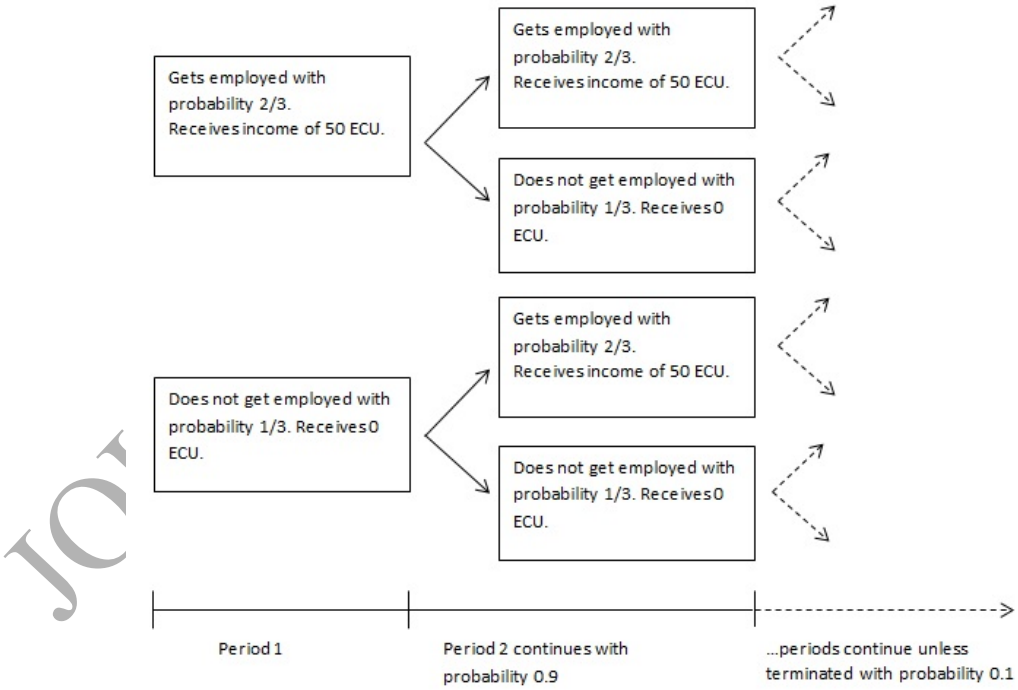
Appendix B.

Figure B.1: A Round with IL Loan



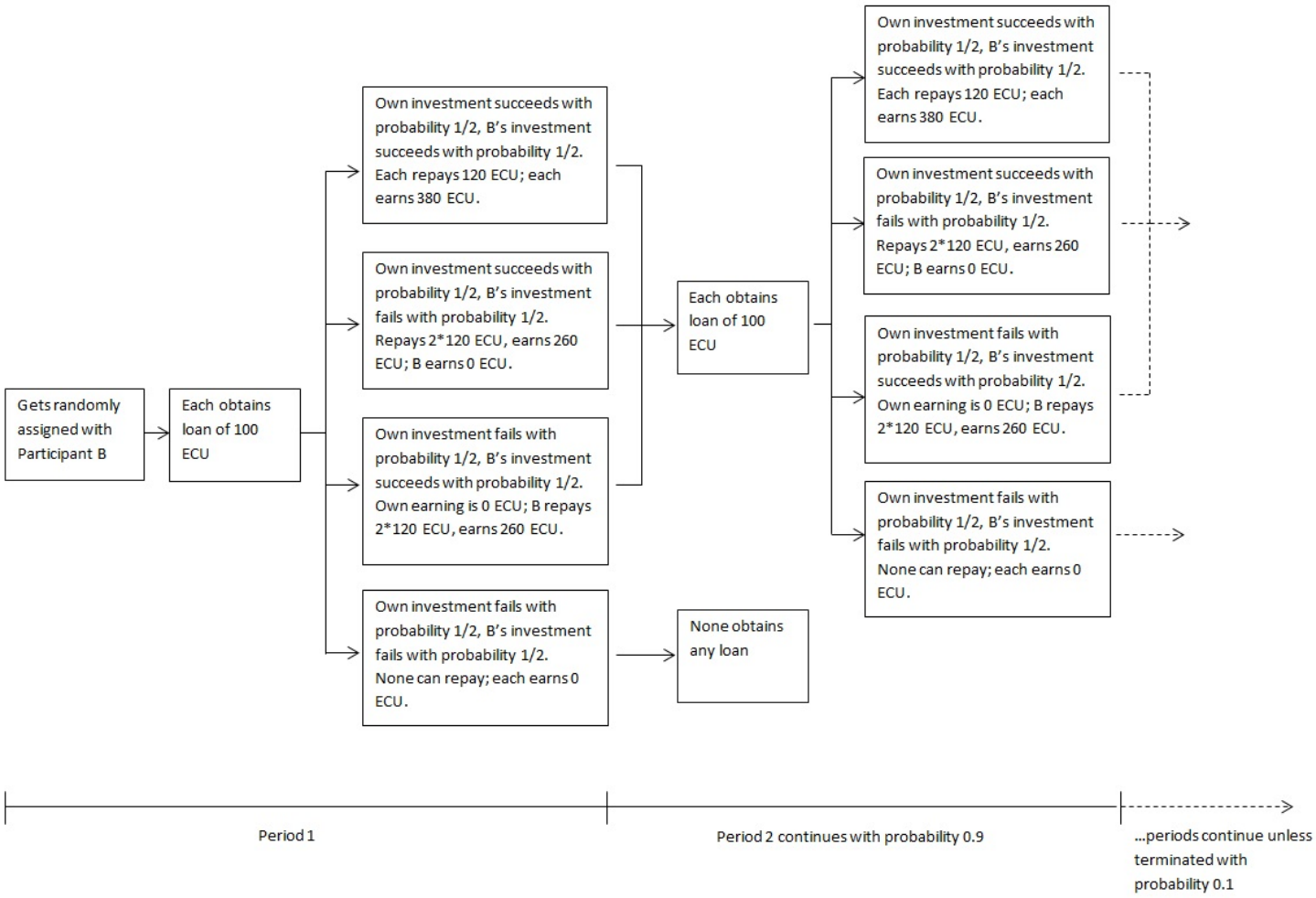
Notes: This figure describes how the steps of a Round proceed if IL loan is chosen from the set of options (depending on what treatment type is being offered). The sum of the subject's earnings over the respective periods constitutes her income from this particular round.

Figure B.2: A Round with EMPL



Notes: This figure describes how the steps of a Round proceed if EMPL loan is chosen from the set of options (depending on what treatment type is being offered). The sum of the subject's earnings over the respective periods constitutes her income from this particular round.

Figure B.3: A Round with JL loan



Notes: This figure describes how the steps of a Round proceed if JL loan is chosen from the set of options (depending on what treatment type is being offered). The sum of the subject's earnings over the respective periods constitutes her income from this particular round.

Appendix C. Online

Table C1: Take-up of loan in Round 1

Variable	(1)	(2)
Treatment Category		
IL-JL-EMPL	*(base)*	*(base)*
IL-EMPL	-.1995**	-.1922**
	(.0848)	(.0893)
JL-EMPL	-.1092	-.0958
	(.0814)	(.0850)
Individual specific controls		
No. of safe lotteries chosen	-.0441***	-.0390**
	(.0170)	(.0180)
Shared amount in DG	.0628	.0530
	(.0420)	(.0565)
Discount rate	.9591	.6125
	(.8273)	(.7891)
Constant	1.0148***	.9998***
	(.1284)	(.1423)
Observations	159	151
R^2	0.0767	0.0631

Notes: **1.** Column (1) represents the estimates when the observations which share more than half amount in DG are included, whereas in Column (2) they are dropped; **2.** *** and ** denote statistical significance at 1% and 5% respectively; **3.** heteroscedasticity-robust standard errors are in the parentheses; **4.** H&L risk-inconsistent observations are dropped for both columns; **5.** 4 and 2 observations are dropped due to Horrace & Oaxaca trimming in Column (1) and Column (2) respectively.

Table C2: Effect of heterogeneous preferences on take-up of JL & IL loans across the different treatment groups in Round 1 *(dropping observations who share more than half in DG)*

	Take-up of JL (1)	Take-up of IL (2)
Treatment Category:		
IL-JL-EMPL	*(base)*	*(base)*
JL-EMPL	.5266	
	(.3795)	
IL-EMPL		.2391
		(.3086)
Nr. of safe lotteries chosen (in IL-JL-EMPL)	.0210	-.0577*
	(.0324)	(.0303)
Treatment category X Nr. of safe lotteries chosen	-.0268	-.0126
	(.0565)	(.0417)
Shared amount in DG (in IL-JL-EMPL)	.2262**	-.1107
	(.0977)	(.0864)
Treatment category X Shared amount in DG	-.2064	.1109
	(.1433)	(.1505)
Discount rate (in IL-JL-EMPL)	.3019	-.5979
	(1.3533)	(1.1111)
Treatment category X Discount rate	.1871	2.4202
	(1.8769)	(1.8308)
Observations	109	111
R^2	0.0986	0.2417

Notes: 1. ** and * denote statistical significance at 5% and 10% respectively; **2.** in Column (1) only treatments JL-EMPL and IL-JL-EMPL are considered, whereas in Column (2) only treatments IL-EMPL and IL-JL-EMPL are considered; **3.** heteroscedasticity-robust standard errors are in the parentheses; **4.** H&L risk-inconsistent observations and the observations which share more than half amount in DG are dropped; **5.** 0 observation is dropped due to Horrace & Oaxaca trimming in Column (1) and 2 observations are dropped due to Horrace & Oaxaca trimming in Column (2).

Table C3: JL & IL take-ups in 10 rounds *(dropping observations who share more than half in DG)*

Panel A: JL take-up	
Lagged JL takeup	-.1065
	(.0697)
Lagged earnings outcome	.0057
	(.0610)
Lagged JL takeup X Lagged earnings outcome	.0837
	(.0710)
Subject fixed effects	Yes
Observations	981
R^2	
overall	.0244
within	.0132
between	.5650
Panel B: IL take-up	
Lagged IL takeup	-.0874
	(.0552)
Lagged earnings outcome	-.0111
	(.0330)
Lagged IL takeup X Lagged earnings outcome	-.0280
	(.0441)
Subject fixed effects	Yes
Observations	1008
R^2	
overall	.2289
within	.0114
between	.9747

Notes: 1. only treatments JL-EMPL and IL-JL-EMPL are considered in Panel A and only treatments IL-EMPL and IL-JL-EMPL are considered in Panel B; **2.** standard errors clustered by subjects are in the parentheses; **3.** H&L risk-inconsistent observations and the observations which share more than half amount in DG are dropped; **4.** in Panel A(B), the coefficient of the interaction term reports the difference in effect of lagged take-up of JL(IL) for *Lagged earnings outcome=1* when compared with effect of lagged take-up of JL(IL) for *Lagged earnings outcome=0*.