Report on "Why Not Borrow, Invest, and Escape Poverty"

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In this paper, the authors develop a model of investment under uncertainty with the aim of shedding light on the reason for the low take-up of micro-finance products. The main interpretation given by the authors is that investments made by households may be either intended to increase returns for a given risk, or to improve the chances of success of existing projects for a given return. The specific shape of the expected utility in such a case gives rise to either zero investment or "full" investment, to the difference of more traditional investment decisions. They first illustrate this idea using theoretical models, and then conduct online experiments with a poor population in the Czech Republic designed to provide empirical support for the results set out in the theoretical part.

In the theoretical part, the authors consider a model with a CARA utility function and contrast two situations. In the first, projects have either low or high returns with a certain probability. Investments are designed to increase the chances of high returns. They show that utility expectation is U-shaped in the first case. This implies that the optimal solution may be no investment or maximum investment. The authors show that the more risk-averse households are, the more they will opt for no investment. In the other model, investment is traditionally aimed at increasing the profitability of existing equipment, at a given risk. In this situation, utility expectation is inverted-U-shaped, implying that there is an optimal investment which is not necessarily either the minimum or the maximum investment.

The authors then conduct an online experiment in which they first measure risk aversion. They then had participants, randomly divided into two groups, play "investment games" in which investments were either linked to a change in risk or to a change in return given risk. They compare the profiles of the participants' investment decisions with the proposals of the theoretical part.

General comment

There is an interesting idea in this paper. Nevertheless, the paper suffers from several limitations. The theoretical part of the paper contains many gaps and imprecisions that make for tedious reading. The empirical part is inconclusive. The authors transcribe the propositions of the theoretical part into testable propositions, but not into the tests carried out. With this imperfect prism, there does not seem to be strong support for the testable propositions. The authors do not follow through on the implications of the empirical results they produce. They could also have a more ambitious empirical approach, since they are in an environment in which they control many parameters. Also, to be fully convincing, it would in any case be necessary to show that, in practice, the opportunities offered to potential household clients of micro-finance institutions are more a question of probabilities of success than of returns with a given probability.

There are also strong policy conclusion that seems a bit ambitious given the evidence produced.

Comment: theoretical part

The theoretical part contains several propositions whose proofs are incomplete.

The first proposition is key. In the corresponding model, household incomes are either low or high (with probability p). Investment does not change the levels of income, but increases the probability of success. The proposition argues that the profile of utility expectation is U-shaped as a function of the probability of success p of the project. The proof shows that the second derivative is positive, but it does not show that the derivative in zero is negative and the derivative at the upper bound of the interval of variation of probabilities is positive. In any case it seems impossible to me that this U-shaped curve is true for all values of $\overline{p}$, it can only be true for values of $\overline{p}$ large enough.

The second proposition is about the link between risk aversion and the investment decision. The consequence of Proposition 1 is that the household decision is either no investment ($p = 0$) or maximum investment ($p = \overline{p}$). The proposition states that if risk aversion is above a threshold $\lambda > \lambda_0$ then households opt for the maximum investment ($p = \overline{p}$) and that otherwise they opt for no investment ($p = 0$). The proof of the proposition does not define the value λ_0, and it seems to me that the proposition is wrong anyway. Noting $U(p, \lambda) = pu(H - \alpha p, \lambda) + (1-p)u(L - \alpha p, \lambda)$, we immediately observe that $U(1, \lambda- > U(0, \lambda)\ \forall\ \lambda$ (this requires that $H - L > \alpha$, which is a hypothesis). All households would opt for the $p = 1$ solution if they could. They don't, because the interval of possible probabilities is truncated at $[0, \overline{p}]$. Households will opt for $p = \overline{p}$ on the simple condition that $U(\overline{p}, \lambda) \geq U(0, \lambda)$. The authors do not provide a study of this inequality, nor do they establish that it resumes at $\lambda \geq \lambda_0$. It is clear, however, that the solution to this inequality, if it is of this nature (which is not proved - it is only proved that if λ tends to infinity, the inequality is satisfied), can only be of the form $\lambda \geq \lambda_0(\overline{p})$. And this can only be the case for sufficiently large values of $\overline{p}$. ($U(1, \lambda- > U(0, \lambda)\ \forall\ \lambda$ is a simple remark but it highlights that the intuition behind the proposition is not straightforward).

Remark: it seems to me that it is possible to show that

1. $pe^{\lambda(\alpha.p+(L-H))} + (1-p)e^{\lambda\alpha p} < 1$ for λ close to zero, which implies that there is a solution to the equation $pe^{\lambda(\alpha.p+(L-H))} + (1-p)e^{\lambda\alpha p} = 1$.
2. $pe^{\lambda(\alpha.p+(L-H))} + (1-p)e^{\lambda\alpha p}$ is increasing in λ for $pe^{\lambda(\alpha.p+(L-H))} + (1-p)e^{\lambda\alpha p} > 1$, which might be of some help for the proof

The statement of the proposition is therefore imprecise and the proof incomplete.

Proposition 3 poses no problem, but the reward model deviates significantly from the probability model. Actually the basics of the two models are never clearly exposed. This is especially the case for the reward model for which there is for example a quantity B appearing in the appendix, the only place some features of the model are exposed. More importantly, it can be the case that the reward model produces investment profiles close to those of the probability model: even if the distribution is continuous, there may be masses of investments in zero and in the maximum investment.

The authors also consider a hybrid model in which households could choose between investing in return at a given risk or investing in chances of success. In fact, this part is unclear to me, as the authors do not consider two alternative forms of investment, but a single

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investment that would simultaneously affect both dimensions of chance of success and return at a given risk. The authors also restrict the situation they consider to cases in which the expected return is the same between the different forms of investment. They show that in this situation risk-averse agents will always prefer investments designed to reduce risk (which in the constrained model means adopting the maximum investment). I'm not sure I understand the insight of this result in such a constrained model.

Comment: empirical part
The empirical analysis also contains numerous inaccuracies.
The authors begin their empirical validation process by measuring risk aversion. They then randomly divide the population of survey respondents into two groups, one playing a game in which investments affect the probability of success at a given return, the so-called "probability game", and another in which investments affect the return at a given probability of success (the "reward game"). In this way, they aim to provide empirical investigation of the propositions established in the first part.
They begin by examining the decisions taken in the two games as a function of risk aversion. The game's prediction for varying the probability game is that the sample of participants should split in two: those investing the amount $\overline{p}$ (the least risk-averse) and those investing nothing. We therefore expect a bimodal distribution. On the other hand, we don't expect much from the investment game in terms of return for a given risk. There is an $i^*(\lambda)$, but nothing in the results establishes any particular shape for the $i^*(\lambda)$ distribution. Figure 2 suggests a partial validation of Proposition 1 and 2: authors look at the distribution of investments in each of the two games. We see as expected a bimodal distribution for the probability game but not in the reward game, although there are large proportion of participants for whom the investment decision is zero or the maximum. (I am surprised by the scale of figure 2 - I expected to see the proportion of participants investing each amount, obviously this is not the case.)
The authors then conduct an equality of variances test which is rejected. It seems to me that this is a one-sided test, but the authors give few details. (The paper does not give the values of the estimated standard deviations, just the test result.) Nevertheless, although it seems to make sense, here too I cannot see what justification there is for this test with regard to the theoretical part.

The authors then search for the relationship between risk aversion and investment decision, which is in line with proposition 2. The authors perform a series of probit regressions to document the difference between investment decisions in the two games and risk aversion. The probit regressions performed are a way of considering the differences in the distribution of investment decisions between the two games (this is not very transparent - I have the feeling that a direct linear regression would give exactly the same result). The comparison is carried out on two groups of individuals: those who are risk-averse and those who are not. (The authors collect two measures of risk aversion, one declarative and the other derived from the choice that participants have to make. It's not clear which is used, but the sample is split into two groups: risk-averse and risk-averse.)

Apart from the fact that the dichotomy between these two groups is not well explained,

it is not clear what is being tested: there is nothing in the model to distinguish the two distributions. Authors don't explain why such a comparison make sense. I have the feeling myself that there might be situations in which, despite the propositions, the distributions of investment can be quite close (it is worth to note for example that there is a mass at maximum investment in both distributions). We wouldn't nevertheless be surprised if, for risk-averse individuals, there were an over-representation of low investments in the probability game and, for risk-averse individuals, an over-representation of high investments in the same game. Such a result is not obtained: while there is a larger proportion of risk-averse participants investing the maximum amount in the probability game, his same difference is also observed for the low-risk-averse participants. What makes sense in any case is the distributions of investment made in the two games for low- and large-risk-averse participants.

A more transparent analysis would be useful. It would be useful to see whether the bi-modal nature of the investment distribution in the probability game can be explained by a discontinuity in investment decisions linked to risk aversion. (Notice that bimodal is a weak version of the implications of proposition 2) It's true that the comparison with the case of the reward model is useful, but the model part doesn't describe how investment decisions vary with risk aversion, so the conclusion is limited anyway.

The authors do not follow through on the empirical implications of their models

Since the authors have the measure of risk aversion and the dependence functions of probabilities and payoffs $H(c)$ as a function of investment, there is nothing to prevent them from estimating structural models to validate the two models. This would make it possible to determine whether agents' behavior is in line with that predicted by the models.

Next, they would need to show that the two models generate different investment distributions. They would also have to show that for one model, there is a mass of investment in zero, but not for the other. This part is more or less done, but as discussed above, it's not really conclusive.

Finally, they would need to examine in the field the investment opportunities of households to whom micro-credit is offered, and show that these investment opportunities are indeed of the kind proposed by the authors: changing the probability of success more than returns at a given risk.