

The authors propose an explanation for the low takeup of microcredit grounded in risk aversion. They show that, when investment increases the probability of a payoff (keeping the payoff size fixed, such as binary success vs failure), investment choices tend to be all or nothing: invest little or nothing, or go to the other corner and invest as much as possible. Poor individuals, who may be more risk averse, will choose not to invest. The authors test the key predictions of their model using an online experiment with respondents in the Czech Republic.

I have reviewed this paper before. I think that it is significantly improved, but I still have some questions and concerns.

Comments

1. I had a few comments related to where we/readers should have our priors in terms of the likelihood of nonconvexities.

a. The authors cite Kraay and McKenzie and echo their argument that “the evidence is inconsistent with technology-based (fixed costs or S-shape production function) poverty traps.” Later they state “Non-convexities are commonly used in the related theoretical literature, but seem to be inconsistent with the facts.” These conclusions are not entirely consistent with several recent papers. One paper finds clear evidence of an S-shaped curve in the response to an asset transfer in Bangladesh (Balboni et al. 2021). Another uses an experiment in Uganda to show that a significant share of households are risk-loving – choosing a riskier lottery with a lower payoff – and that they use the proceeds to invest in lumpy land purchases (Kaboski et al 2024). Another paper finds that, in India, the long-term results of a randomized control trial are consistent with poverty trap dynamics for existing entrepreneurs, for whom microfinance is able to facilitate exit from a poverty trap (Banerjee et al 2024). And a fourth uses an experiment in Pakistan which suggests that nonconvex capital adjustment costs can give rise to persistent effects of larger loans to finance asset purchases (Bari et al 2024).

The authors cite Balboni et al. 2021, arguing that “the existence of ‘s-shaped poverty dynamics’ in the data (e.g., Balboni et al. (2021)) doesn’t indicate non-convexities in production.” However, I think engaging with the other two papers would be useful, as well.

Of course, a poverty trap may be present in some settings and not others – and, as argued by Banerjee et al 2024, for some individuals and not others – so this is not to deny the possible role of risk aversion as the authors argue. However, I think some discussion of these papers would be helpful.

b. Related, if the authors were to decide to carry out additional experiments, it might be interesting to “horse race” the predictions of a model with nonconvexities against those of the risk aversion model. If support for both models was found, the authors could then say something about the relative shares of “never takers” who appeared to be constrained by each channel.

2. The experiments carried out do not feel tightly tied to the setting of microcredit. There is a probability game, in which investing more raises the chance of a fixed reward, and a return game in which investing more raises the “good” payoff, holding the chance of the good payoff fixed at 50%. I would have appreciated some more discussion of if/how this maps into core features of entrepreneurship in low and middle income countries. Of course, lab experiments are necessarily abstracted, and this does not mean that we cannot learn from them, but I would appreciate more discussion of the mapping between them.

3. The sample is a representative sample of Czech households, who are substantially better-off than a typical microfinance borrower. The authors do discuss this, which I appreciated, and they show their results split by the income level of respondents. However, there are only 162 households in the lowest income bin, with average monthly income of about 19,782 CZK (~\$850) – which is still significantly more than the typical monthly income of MF borrowers in LMICs. So the sample seems somewhat limited in how much light it can shed on the decisions of low income households.

4. The sample also does not appear to be selected to contain a high share of current or aspiring entrepreneurs, which is the sample where both fixed-cost dynamics and risk-aversion dynamics might be particularly important.

5. The possible outcomes in the online game used to test the predictions range from 0 to 270 CZK. (The risk aversion elicitation has a higher possible payoff of 1000 CZK, but a lower chance of a payout.) Average monthly income in the sample is 35484.6 CZK. As such, these stakes seem too low for risk aversion to be at play. Of course this issue is not unique to this experiment (Rabin 2000) but nonetheless it is germane here.

6. Table 1 shows that risk averse households are more to invest if they play the probability game compared to the reward game, as compared to the non-risk-averse. This is useful; however, the magnitudes seem small. Perhaps I'm wrong in seeing the magnitudes as small, but some discussion would be useful: can the authors extrapolate how much of the low takeup rates of MF this channel can explain?

7. Since risk aversion is of course not randomized across households (and Table D4 shows that there are significant differences between those who are more vs less risk averse), it would be useful to discuss how much of this we can think of as the pure effect of risk aversion vs of other characteristics.

References

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