2nd August 2024

Dear Matthias,

Thank you for your letter and for reading the paper.

I regret having to ask for more of your time to read my response. I believe that feedback on referees is useful for editors, and that in this case your rejection isn't justified by the reports nor by the explanations in your letter, as you were misled by the referees.

In the cover letter I sent when I submitted the paper, I told you that this paper suffers from hostility from experts in the field of development.

For your convenience, the cover letter is here:
https://warwick.ac.uk/fac/soc/economics/staff/omoav/media/cover_letter_jeea.pdf

As I clearly described in that letter and demonstrated with the nine referee reports we have received to date (that I provided in a link in the letter), the reports are just wrong or petty.

In that letter, I asked you not to send the paper to experts in the field of development. I provided you with the nine reports in anticipation that another referee report from that field would not be informative.

Below I will show you that there isn't much in these two new reports to justify rejection. And thus, I think your decision letter that echoes some of the wrong claims of the referees is not well-founded.

Of course, as you are the editor, I cannot contest your subjective evaluation, but I do think that the paper addresses a very important issue that received a lot of attention, including the Nobel Peace Prize, and billions of dollars in loans to the poor that failed to alleviate poverty. So, the question of what went wrong with microfinance is clearly of interest to a wide audience in economics and beyond, and since there is no good explanation to date, perhaps our paper does deserve a place in the JEEA. I also believe that the theory is novel and surprising despite its simplicity, and perhaps an applied theorist who isn't part of the dev group, would agree. I strongly believe that if you read the introduction (that fully explains the theory) without the noise from the referees, you would see the beauty of the theory.

I know that the norm in economics is to move on and not disagree with referees and editors. Efficiency is important and we don't want to waste the time of the editor. The problem is that our system is very inefficient, as it allows referees to block good research for bad reasons.

I wrote about it in a thread that received very positive attention (and by coincidence Romain, who is on the "Committee on Improvements to the Publication Process in Economics" just wrote to me about this thread a couple of days ago).

I would be very grateful if you would read it:

https://x.com/Omer_Moav/status/1807700429951615292

Let me start with referee 1, the referee who complains about the quality of the writing of the paper, in a report that I didn't think was well written and was difficult to understand in many parts.

1. The referee states that: "The contribution of the paper as written is not so clear. The paper's stated contribution is (not stated until the last paragraph of the introduction):"

We do summarise the contribution in the last paragraph of the introduction, but the contribution is clearly explained in the abstract and in the second page of the introduction. We use the first page to explain the motivation and the existing explanations in the literature, as well as their limitations. We write at the top of the second page:

"We propose an explanation that suffers from none of these limitations. We show that risk aversion can explain why the poor do not borrow, invest, and escape poverty when they have access to microfinance. Our explanation does not rely on an S-shape production function, fixed costs, or any other non-convexity in the technology, or any limited rationality such as false beliefs."

Second, I disagree that the paper isn't well written. We invested in writing it well and engaged an experienced editor for the final version. I hope that based on your own reading you can agree. In any case, clearly the paper's writing isn't bad enough to justify rejection, and one can easily understand the contribution (e.g., the correct summary by the other referee).

2. The referee writes: "As stated in the first sentence, this is not a novel contribution: risk aversion alone could explain this, for example. (The authors are clearly aware of this, since the authors choose CARA rather than CRRA in order to negate this.)"

The explanation is novel. This is the first paper to offer an explanation based on risk aversion WITHOUT relying on non-convexities. This has nothing to do with using CARA to illustrate our theory. We also show (numerically) that it works with CRRA. We explain all of this in footnote 5 in the paper.

3. The referee writes: "If the motivation is empirical, so could the idea of heterogeneity in project quality, i.e., the poor simply do not have high returns. So the contribution would be to propose ANOTHER theory. But I think the second sentence is crucial in that the authors additionally want to propose a theory that might explain why

we don't see intermediate levels of investment, e.g., their result that “investment is discontinuous in the agent’s level of risk aversion: more risk averse agents invest nothing, while less risk averse agents invest the largest amount possible, and do so even if the technology itself does not have s-shaped returns to capital because they feel the evidence is against the latter. They want this bimodal distribution, rather than unimodal distribution. This would then relate to the well-known portfolio result that even a risk-averse agent would like to invest a small amount in a risky asset with positive expected return. I'm not sure they full succeed in this because although there is a potential continuous investment in improving the probability, the project itself is discrete/indivisible, which already has the feature of a fixed cost, s-shaped return (step function of output). That is they rule out the option (especially important for the poor) of investing with 100% success probability in a scaled down project.”

I’m not sure I understand the entire complaint here. The contribution is indeed to offer a theory. The experiment is designed to test whether individuals behave as the theory predicts. The existing evidence, which our theory is consistent with, is that the poor choose the corner solution of zero investment and some established businesses borrow and invest significantly (we are not claiming that they invest “the maximum,” which is clearly a feature of a simple model and not a claim about reality).

The referee states that “[the authors of the paper] feel the evidence is against the latter” regarding the s-shape production function. It isn’t a matter of feeling. It is a straightforward reading of the literature. We survey the literature and describe that evidence in the paper.

The final part of this paragraph suggests that the model has a feature of a fixed cost because the project itself is discrete/indivisible, and from this the referee concludes that we rule out the option (especially important for the poor) of investing with 100% success probability in a scaled down project.

There are two points here:

First, we address this in the paper on page 5:

“Investment in our model cannot be diversified among independent projects to eliminate the risk. This is, of course, standard in theories that are based on risk aversion. We propose that investment augments the expected productivity of an existing indivisible asset – the individual’s labor, which is, for poor people, their main productive asset. The investment could be in human capital that increases the probability of finding a well-paying job or investing in a small business in which the owner's labor is the main factor of production."

I would add here that this DOES NOT imply a fixed cost, s-shape, or any other non convexity. That's simply a wrong statement by the referee.

Second, the poor could perhaps invest in a risk-free asset but clearly this will not provide a higher return than the interest on borrowing. Imagine the magnitude of the arbitrage otherwise.

We add further explanations in the paper and in footnote 8 we have:

"We are aware that the poor do diversify to reduce risk, but, of course, diversification is limited, and cannot remove risk altogether."

4. Regarding the experiment the referee writes that: "The experiment is interesting, but again the contribution is limited. The applicability of a Czech sample to the microfinance literature they motivate with is small. Wealth levels vary dramatically from the motivating population, and the authors are aware this is a concern. Second, it would seem that proper execution would want the bimodality of the distribution to vary in a three dimensional plot over investment level AND risk aversion. Third, the last part of the experiment is simply testing whether risk averse people are consistently risk averse. I don't see the tight connection of this with the model theory. Finally, there is a question of what we might even POTENTIALLY learn. On some level, we might simply learn whether people behave rationally, but we learn nothing about the relevance of the theory to real world."

The point here, that is echoed in your letter to me, is the concern regarding the relevance of the Czech sample to the population in poor countries. Here is what we write in the paper on page 5:

"One might raise the concern that representative households from the Czech Republic are substantially better off than a typical microfinance borrower. However, the experiment was designed to test the prediction of the model regarding human behavior – investment choices under risk – regardless of its application to the question of the failure of microfinance. Thus, there wasn't a good reason to conduct the experiment with a sample of people from a developing country, where the ability to conduct online experiments is limited. Our experimental evidence supports our model's predictions at all income levels. Poor, risk-averse households in our sample choose to avoid high return investment, unlike the wealthier households that do invest. Thus, if poor households in our sample, which are wealthier than the typical poor household in a poor country, do not invest, then poorer households - such as those targeted by microfinance -would also avoid such an investment. In reaching this conclusion, we follow the conventional claim that the poor in poor countries are not fundamentally different than other people, they simply have less income.” (In a footnote we clarify that the experiment had to be online due to Covid.)

We agree that we do not provide systematic evidence regarding the relevance of our theory to the failure of microfinance, but the experiment wasn’t designed to do that. We do provide examples of investments that are consistent with our theory, and the last part of the experiment is related to that as it illustrates (as consistent with the theory) that when agents have the two margins of investment available (increasing the probability of success and increasing the income conditioned on success) they will prefer the former, controlling for the expected return.

5. The referee writes: “In the end, sifting through everything, the paper makes a nice interesting point, but especially given that there are other existing theories to explain the data, the contribution of the paper is minor.”

We survey the existing explanations. The referee doesn’t claim we are missing something important in this survey. So please read the first page of the introduction and judge for yourself. I find this claim - that the contribution is minor - groundless.

6. The referee complains again about the writing of the paper. I’ll ignore that here. Please read the paper and judge for yourself. On one point regarding the writing of the paper the referee is probably correct: “The introduction seems to be arguing with a previous set of criticisms received. It therefore loses focus.”

This is an outcome of the many, many wrong complaints we have received, and our attempt to pre-empt the groundless claims previous referees have made about what is, and is not, in our model.

In addition Ref 1 writes: “Related, the premise that S-shaped production functions lead to risk aversion but there is a need for a model with a non-S-shaped production function to also lead to risk aversion is unfounded. S-shaped production functions lead to risk-loving even when preferences are inherently risk-averse (see Hopenhayn and Veraschgina, 2009, for example). It is simply not true that the theoretical literature has ignored the idea that people can invest to influence the probability of success rather than payoffs. This is the case of moral hazard, something that was considered in credit markets as far back as Stiglitz and Weiss (at least!) The introduction seems to be arguing with an argument in Banerjee and Duflo's book that is targeted toward a general readership.”

First, we DO NOT claim that S-shaped production functions lead to risk aversion, and we DO NOT claim that there is a need for a model with a non-S-shaped production function to also lead to risk aversion. These comments strongly suggest to me that the referee didn't really bother to understand the paper.

Second, we DO NOT claim that the theoretical literature has ignored the idea that people can invest to influence the probability of success rather than payoffs. Moreover, we are explicit to the contrary! We write on footnote 4: "We are not the first, of course, to assume in a model that the probability of success is a function of investment, but rather the first to show that this could lead to corner solutions."

(I'm not addressing here the very minor comments of the referee, comments 2, 4 and 5).

The bottom line is that this referee makes several misleading claims about the paper and provides nothing much that can justify rejection.

The second referee correctly summarises the paper, mentions they were a referee in a previous round, and repeats some of the same wrong comments.

The referee wrote:

"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)."

Regarding the first point: this is a misunderstanding by the referee. We amended our paper to make it clearer. The referee saw the clarification in this version, based on her/his reaction, understood it is a misunderstanding, but kept this wrong argument in their current report. The referee wrote in this round:

"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"

So moving to the other papers the referee cites: first a paper that indicates that some poor households behave as if they are risk-loving in some specific circumstances. The large consensus, based on a lot of evidence, is that the poor are by-and-large very risk averse. We discuss the evidence in the paper. The referee clarifies that they agree and just suggests discussing this in the paper.

Second, the referee mentions that the evidence is consistent with microfinance facilitating exit from a poverty trap for existing entrepreneurs. That is correct and fully consistent with our paper! Our model predicts corner solutions - the poor who are very risk-averse choose zero investment and the wealthier individuals choose to borrow and invest, as they are less risk averse. This is clearly explained in the paper.

Third, the referee also claims that in some cases investments are lumpy. However, this reflects the failure of the referee to understand the claim in the existing empirical literature. We do not claim that there are no businesses that require significant fixed costs. The claim is that the existence of business opportunities that do not require significant fixed costs is sufficient to conclude that the evidence is inconsistent with technology-based poverty traps. This is explained in footnote 3 in the paper.

All the other comments of the referee, if I understand correctly, are minor, and mainly suggest some additional discussion.

So, in the end, there is nothing objective in this report to justify rejection.

I would appreciate it if you would reconsider the paper, and ideally send it to experts who are not in the field of development, since development economists appear unwilling to engage with what our model actually contributes to our understanding of microfinance. Of course, this is your call, and I would accept without further debate any decision you would take. I thank you again for your time.

Best wishes,

Omer

3rd August 2024

Dear Omer:

Thanks for your mail. I fully understand your frustration, as I have been on the other side a number of times. Nevertheless, there is a clear standard for revisiting decisions, which is that the decision was based in substantial part on a referee (or editor) comment or evaluation that turns out to be wrong. This is not the case here. It came down to an evaluation of whether the central insight of the paper is more appropriate for a field journal vs. a top general-interest journal, and all reviewers I consulted agreed on the basic point that the paper does not quite clear the general-interest threshold. Obviously, this is a judgement call and I understand why you disagree, but in the end I found myself convinced by the referees.

A bit more on the process and the reports. Given your letter, I sent the report to three referees from different fields. This includes one development economist (on the modelling side of development) who I trust and two others in adjacent fields (but who have work on development-related issues). I thought that the R2 report was fair and this referee generally likes the paper, but thinks that it is not quite at JEEA level. I relied less on the R1 report, and I agree with you that the paper is generally well written (as also stated by R2). I also communicated with R3, who was late and ended up not submitting a report but who read the paper. This referee (from yet another field) had a favourable impression overall but also agreed with the others' conclusion that the paper is more appropriate for a field journal.

I can certainly see why you disagree with specific comments made by the referees. But the overall message I got from the referees (and, based on reading their reports, I think the referees at the other journals had a similar view) is that the theoretical insight alone is not enough for a general interest journal, and that the experimental evidence is too remote from the developing-country setting to support the empirical relevance of the mechanism.

I hope this is helpful in clarifying the process that led to the decision. I hope you will be successful in publishing this paper soon, as I do think that the basic insight is quite interesting.

Best Wishes,

Matthias

4th august 2024

Dear Matthias,

Thank you very much for your prompt and detailed reply. I accept your judgment, of course. Many papers do not suffer from any significant problems and yet do not make the bar to a top journal, and this is a subjective judgment left for the editor to make. So please don’t understand my following short comment as an attempt to convince you otherwise.

The point that bothers me, and that is very salient with this specific paper, is the weight an editor should put on the subjective judgment of referees that demonstrate in their reports that they didn’t bother to carefully read the paper and understand it.

If your letter to me had indicated that you have read the paper and found that even though it makes an interesting point it isn't sufficient, according to your judgment, for a top general interest journal, I would have not asked you to reconsider. (and just provide you with feedback on the low quality of the reports that make many factually wrong claims).

But you wrote to me that you were convinced by your reading and by two excellent reports and mentioned some of the concerns raised by the referees in these reports. Since these reports are significantly flawed and indicate that the referees didn't understand the paper's contribution, I decided to ask you to reconsider.

Thanks again for your time reading the paper, my long complaint, and the previous reports. I sincerely appreciate it.

Best,

Omer