

Dear Editor,

I am prompted by my recent experiences to submit, with this paper, a specific request. Peer review is a bedrock of our profession, but my experience with this paper is that some unrelated issues are getting in the way of a professional peer review.

Many participants in seminars and conferences have found the paper interesting and surprising. It answers a current and important question in economics, for which we don't yet have a good answer, which is why microfinance has failed to reduce poverty.

We offer a novel explanation that is consistent with the facts and supported by our experiment. Our theory is surprisingly simple and transparent. It relies on the assumption that investment is largely directed at increasing the success probability of a small business. With this structure (which isn't uncommon) we show that risk aversion leads to corner solutions. Our model has no fixed costs (or other non-convexities in the technology) or any behavioral elements. Removing non-convexities, common in existing theories of poverty traps, is important as they are inconsistent with the evidence (as explained in detail in the paper).

Unfortunately, where we have not been met with enthusiasm is among members of the development group. One would have hoped that experts in this field, if they find fault with the model, would be able to provide some professional justification for this view. But this isn't the case. The bottom line is that ALL comments are wrong or petty, excluding a few subjective assessments that I believe are far from convincing.

I provide examples below, but my reason for telling you this is to request that you seek the advice of applied theorists who are not part of the development group, and who are more likely to conduct an impartial assessment of the paper.

I'll add that perhaps a paper that raises so many unfounded objections by no less than nine experts is worth taking seriously. It appears that our paper has hit a nerve, as we achieved a result that others within the field failed to achieve.

A page containing all the reports to date, my responses, and my exchange with the editors is at:

<https://warwick.ac.uk/fac/soc/economics/staff/omoav/media/>. I invite you to read them and see the views of the experts from the field.

I provide below a few important highlights that prove my point and even support the paper – as one referee did write that a paper that generates our results should be published by the ReStud, but suggested rejection, nevertheless. The reason given for why it should be rejected was wrong. Factually wrong – not a matter of judgment.

The paper was first submitted to and rejected by the AER. I've written to the editor – Rema Hanna – a complaint against one of the referees, which I discuss below. I wrote in the letter that I am not asking for reconsideration.

This referee (Ref 2) wrote that the paper should be rejected because our results rely on two assumptions they don't believe (and provided no additional substantial comments).

In the words of the referee:

"The conclusions of this paper depend on several assumptions. Specifically, one key assumption is that the probability of investment success is not exogenous, instead more investments increase the chance of success. I'm not sure if this is true in reality. For example, poor households might work on agricultural related projects, which can be influenced by random weather or price shocks. Some other business projects could be affected by exogenous supply chain shocks, unexpected policy reforms, etc. Another assumption is that poor people believe the production function has an "S-shape", where the marginal return is only high at higher levels of investment. This is suggested in some existing literature but there is no evidence showing the study sample also hold such beliefs."

I hope I don't need to explain why thinking that in the real world, the success of a business doesn't depend on the investment (money and/or effort) of the owner, makes little sense. Furthermore, it is an assumption adopted in many papers, including in the field of development. Even worse: it is clear, if one reads the introduction of the paper, that we don't assume that the poor have false beliefs in an S-shape production function. We only mention this theory in the paper as one proposed by Banerjee and Duflo.

There was also **nothing** to justify the rejection by the other referee (Ref 1). The comments were wrong or minor. I explain here:

[https://warwick.ac.uk/fac/soc/economics/staff/omoav/media/aer\\_response\\_to\\_ref1.pdf](https://warwick.ac.uk/fac/soc/economics/staff/omoav/media/aer_response_to_ref1.pdf)

The next rejection was from the ReStud. I sent to the editor who handled the paper, Elias Papaioannou, a detailed letter showing that **ALL** the substantive comments by the two referees were wrong.

Elias returned to me with two new referees, who also suggested rejecting the paper, but to our point, he wrote to me:

R4 wrote a detailed report that I attach. His/her assessment reads: *"Overall, while I perhaps agree with Omer that the referee's comments are not quite on point, I do think that the paper ought to be rejected as is."*

Ironically, while R4 agreed with my claim that the referees were wrong, the claim this referee made for rejection was wrong. Here is the main part of their report:

*"To my mind, the results of the paper are only really novel if they show a model that has neither a non-convexity nor a behavioral explanation but features local risk aversion. If this result is correct and indeed shows the existence of a poverty trap with neither a non-convex production technology nor a behavioral explanation, then it is a major result that deserves to be widely influential. It could help development economists to get clearer understanding of the types of interventions that can help to pull people out of poverty traps. So, if correct, the paper is suitable for RESTUD. The key question then is whether the authors succeed? Here I think the answer is no. The model features both a non-convexity and a behavioral explanation."*

So as you can see, at least this referee thinks that our result is sufficiently important for the ReStud. But then they make the wrong claim that we have behavioral elements and non-convexities. The model is so simple and transparent that one doesn't need to be an expert in economic theory to see that the claim is wrong, but I did consult with theorists who fully agreed that the model has no non-convexities and no behavioral elements.

The other new referee, as you can see, just wrote a very short message to the effect that they didn't put much effort into reading and understanding the paper and wrote that the paper is better suited for a field journal (specifically mentioning the JDE).

Here is my letter to the editor, his response, and the two new referee reports:

[https://warwick.ac.uk/fac/soc/economics/staff/omoav/media/restud\\_my\\_complaint\\_editor\\_response\\_and\\_new\\_referees.pdf](https://warwick.ac.uk/fac/soc/economics/staff/omoav/media/restud_my_complaint_editor_response_and_new_referees.pdf)

I was grateful to Elias for taking my complaint seriously and sending the paper with my detailed report to two new referees. I thanked him for that, without asking him to reconsider one more time, even though the rejection was based on a factually wrong claim.

The last rejection was from the JDE. You can read the reports and my detailed letter to the editor in response. The bottom line is that three referees wrote a lot of wrong and petty comments and nothing substantial and correct to justify the rejection.

My complaint was redirected to Andrew Foster, the EIC. Andrew didn't disagree with any of my claims, but still thought it doesn't justify reconsidering. In his view, if three referees recommended rejection the paper should be rejected, even if they failed to justify their conclusion. Interestingly, he speculated that the explanation for the objection to the paper by all the development experts is that one can obtain the same results with a simple model containing a non-convexity in the technology: a minimum scale of investment or an S-shape production function. And thus, if I understand correctly, it renders our theory rather useless.

Of course, the achievement of our model is that it doesn't rely on non-convexities. So rejecting the paper because non-convexities can do the job isn't reasonable, and none of the referees made this claim. But perhaps Andrew was spot on. Perhaps the negative referees, who failed to justify their objection, were protecting their own or their colleagues' models that rely on non-convexities. (I consulted my colleague Sharun Mukand, who is familiar with the field, and this was his interpretation of the hostility to the paper. In his words: the paper is too good – they have to reject it).

The paper has also been desk rejected by EJ and JPE-micro. The rejection letters are also available in the link above.

I complain to editors very rarely and only in extreme cases - when it is clear that referees did a very unprofessional job and that they base their rejection recommendations on wrong claims. Never before in my academic career (over 25 years now) have I experienced anything even remotely close to the experience I had with this paper, and, of course, I did receive unprofessional reports in the past.

I would be very grateful if you would read the paper's introduction and consider my request to send it to applied theorists who aren't part of the development guild. If our model can in fact offer an

explanation for the failure of microfinance, I believe it is a worthy complement to the literature that includes enthusiasm (and the Nobel Peace Prize) for the prospects of microfinance to reduce poverty, and the documented failure to deliver - but not yet a good explanation for what lies behind this failure.

Of course, like most theory papers, I don't know how relevant our explanation is to reality. I know that the theory is consistent with the available facts and with our experimental results, it provides a theoretical achievement (as argued by Ref4 for the ReStud), and if it is blocked, no one will consider further testing it.

Thank you very much for reading this letter.

Best wishes,

Omer