ReStud

My Letter to the Editor. July 11. 2023

Dear Elias,

I am taking up your invitation to contact you if I have any questions about your decision, although my query is better described as a complaint and a request. I know that as an editor you are extremely busy and that the last thing you need is authors complaining about rejection decisions. However, I believe this is an extreme case of a rejection that is based on two very flawed reports – flawed in a way that goes well beyond what can be accepted in our field. The reports raise a few very minor comments and many complaints that are not valid. I explain in more detail below.

As I wrote to you in the submission letter, the development group is hostile to our paper, but they fail to come up with reasons to justify their hostility. The two reports illustrate my point. I hope you can understand my frustration. I really don't mind a rejection based on subjective judgment, such as claiming the paper's contribution, even if it has no significant flaws, is simply too small for a top journal, perhaps because of a subjective assessment that the relevance to reality is too limited. But this isn't the case. It is very hard for me to accept a rejection based on these two reports.

So I make a respectful and straightforward request: please read the introduction of the paper. It will give you a very clear picture of the theory, the contribution to the literature, and the experiment that supports the relevance of the theory (which isn't questioned by the referees). If, based on the introduction, you feel the paper should be rejected, please just let me know that this is the case. If not, please send it to a new set of referees, preferably taking into account the hostility we are facing from the development group.

Perhaps you can ask a theorist or an applied theorist to read my following response to the referee's reports and advise you on the validity of my complaints.

I'll start with the comments of Referee 1 (the numbers correspond to the referee's numbering of his/her comments)

A1. The referee states that the proof of Proposition 1 seems incomplete. This is a very minor issue, which we can fix with a simple clarification.

A2. Here the referee states that the way we write the expected payoff (utility) from the lottery is not justified. He/she is wrong. The utility function is perfectly consistent with our theory. The alternative formulation, which the referee claims is justified, is not consistent with our theory.

Our theory suggests that potential borrowers face two outcomes if they borrow and invest: the investment could succeed or fail. The investment is fully financed by borrowing. The cost of investment is *ap*, where *p* is the probability of success and *a* is a constant coefficient. If it succeeds, they have a high income: *H*. If it fails, they have a low income: *L*. In both cases they need to pay the debt: *ap*.

So expected utility from investment is:

$pU(H-ap)+(1-P)U(L-ap)$.

The referee, however, claims that since the investment is paid upfront it should be:

$pU(H)+(1-p)U(L)-C(ap)$,

which is inconsistent with our theory. Investment is made upfront, but it is money that was borrowed, and the debt is repaid after the state of the world is revealed. The entire point of the paper, as clearly explained in the introduction, is that poor individuals face the risk that if they invest and their business fails, they still need to repay the debt.

The referee suggests an alternative model, that is consistent with a utility cost of investment (effort), not with a monetary cost. This is clearly an invalid criticism!

A3. In our model section, we analyze both a model based on our theory and an alternative model. In the model that is based on our theory, investment increases the probability of success of a business. In the alternative model, which isn't based on our theory, but rather on some of the existing literature, investment is in the size of the reward (the income from a business) if it is successful with an exogenous probability. We test both models in the experiment and show that individuals behave as predicted by the models.

The referee questions the reasonableness of the model in which investment is in the reward, by asking: "What kind of applications correspond to the reward game? If microfinance is the leading motivating example, what kind of loans allow agents to boost their high-state payoffs conditional on the outcome of a risky project?"
As our theory isn't the "reward game", the fact that the referee has doubts about the reasonableness of the reward game is clearly not a criticism of our paper, but rather implicit support for (some of) our modeling assumptions.

A4. We have a proposition that the referee thinks is redundant as it follows (as we clarify in the paper) from second-order stochastic dominance. I'm not sure I agree, but obviously we can just remove the proposition and give this result an alternative title. The referee doesn't question the result.

A5. The referee argues that there are models of risk aversion and no fixed costs in the literature and mentions Mookherjee 1997 as an example, but, as the referee states, these models make different assumptions and obtain different results. Clearly, this isn't a limitation of our paper. We are happy to cite Mookherjee and explain why the different assumptions lead to different results. But the referee's expectation that we use the Mookherjee model (that's the "justified" alternative he/she offers in comment A2) makes no sense.

B1. The referee states that the empirical results are consistent with our theory and that they are interesting. He/she asks if the results are also consistent with his/her preferred model in A1 (should be A2). We could address this comment but this comment clearly isn't a criticism of our paper.

B2. The referee suggests an alternative experiment that could test the claim that fixed costs are not important in many contexts, but risk preferences are, for the investment decisions of the poor.

This comment completely misses the point of the paper, which is very clearly explained in the introduction.

We do not argue that in theory fixed costs do not matter. On the contrary - with fixed costs it is easy to explain the fact that the poor do not borrow and invest. The theoretical challenge is to obtain these results without fixed costs because a large empirical literature claims that fixed costs are not a good explanation. The experiment that the referee suggests is a good experiment to test the theories that are based on fixed costs. This experiment is of no relevance to our paper.

C1. The referee states: “It is not the case, as the authors claim, that the literature has ignored the role of risk-aversion in studying the economic decisions of the poor, focusing instead only on fixed-costs or non-convexities.”

This statement is not true. We do not make such a claim. For instance, in the fourth paragraph of the introduction, we state: “Banerjee (2000) … proposes that the poor are too risk-averse to borrow and invest.”

The referee adds:

“Even in the theoretical literature, it is well-understood that without any non-convexities, strong income effects that work through risk-aversion or discounting of the future or how much people save or bequeth may cause poverty traps. The work of one of the authors of this paper (Moav, 2002, Economics Letters) is indeed an example of this, but even review papers like Azariadis, 1996, Journal of Economic Growth, and Ghatak, 2015, World Bank Economic Review discuss this.”

The referee misses the point, which we explain in the section “related literature” on page 11. One could create poverty traps (as I did in my 2002 paper) without any non-convexities based on the failure of the poor to save and accumulate wealth. But in the current paper we don’t just explain a poverty trap, we explain why, when the poor are offered credit that could overcome the lack of their own savings, they still choose not to borrow and invest.

C2. The referee writes:
“As to why microfinance did not succeed, the explanation the authors advance may well be valid but there are others that also hold, including those that focus on lumpiness of investments and the fact that the microfinance loans are too small in size. One paper that is particularly relevant here is by Banerjee, Breza, Duflo, and Kinnan, 2019, NBER Working Paper 26346) which shows that those loan-recipients that already had a business managed to improve their incomes significantly and permanently even though the average treatment effects showed insufficient gains.”

We address two of these points in the paper:

1. Yes, as explained above, and as discussed in great detail in the paper, it is easy to explain the failure of microfinance with lumpy investment, but this is inconsistent with the findings of many empirical papers.
2. We clearly mention the finding that some existing businesses do borrow and invest. This is in the second paragraph of the introduction (the paper by Meager (2022)).

And we can address the claim that microfinance loans are too small.

In any case, comment C2 doesn’t identify any limitation of our paper.

To summarize the comment of R1: among the issues raised in the report, those that identify a point that is relevant to the paper are very minor and clearly not sufficient to justify a rejection. The substantive claims are founded on a misreading of the paper or a misunderstanding of the model.

The report of R3:

This referee clearly understands the paper and describes it correctly in his/her report. But the reservations this referee raises are unjust, and to a large extent are in direct contrast to the report by R1.

1. The referee addresses the issue of portfolio theory and risk diversification. He/she claims that since individuals face many other risks, portfolio optimization tends to avoid corner solutions. However, what we are trying to explain is why individuals do choose a corner solution of avoiding borrowing and investment.
Indeed, portfolio optimization with exogenous success probabilities would lead individuals to diversify and avoid corner solutions, but the beauty of our theory is that it explains the facts by making the (plausible) assumption that investment increases the probability of success.
In summary of this point, the referee proposes a theory according to poor households diversify and avoid corner solutions. We offer an explanation for the fact that they don't avoid the corner solution of not borrowing for the sake of investing. Thus, this point of the referee is not a valid criticism of our paper.

2. The referee addresses the connection between the household and the business. First, he/she claims that the business of the household can be affected by shocks to the household (sickness or a need for cash). Therefore, the referee correctly argues that if microcredit availability helps household consumption smoothing, it could support the decision to invest in a business. This comment in fact supports our theory, as the referee proposes that microfinance should increase investment in businesses by poor households, but in fact it does not. Our theory provides an explanation for this.

The referee also mentions the possibility that "if poor people are close to consumption floors or asset thresholds, then risk-averse individuals can at times exhibit extreme risk-taking behavior." Perhaps this is true, but the fact is that the poor avoid taking microfinance loans to make risky investments. We provide an explanation for this fact. Clearly, this point cannot be a valid criticism of our paper.

3. In this comment the referee echoes the portfolio reasoning from above and makes a more general complaint: the model is simple, and the real world isn't. Thus, the referee questions the relevance of the model:
"It is thus very hard to see how the toy model in the paper corresponds to actual business decision-making. That is, the paper illustrates a potential mechanism through a stylized model and corresponding lab experiment set in this stylized model, but then does not discuss well how seriously we should

consider this as indicative of real-world behavior when facing real business decisions."

I guess there are two points here: the relevance of the model to the real world, and the brevity of our discussion of that issue. The first complaint can clearly apply to any model: all models are a simplified version of reality. The relevant question is, of course, whether the assumptions of the model are simplifying assumptions or assumptions that are crucial for the results.

Here the referee argues that in our model the outcome of the risky investment is binary (fail or succeed), whereas in the real world, outcomes are more continuous.

This does not appear to be a reasonable criticism. Not just that, as stated above, any model is simpler than reality: specifically, the binary outcome is very standard in this literature. (R1 for instance didn't propose this is a problem). Moreover, if one argues that an unrealistic assumption (binary outcome in this case) isn't a simplifying assumption but is crucial, one should explain why he/she reached this conclusion.

If the referee understands all of this and his/her comment is just about the discussion of the assumption, then we can easily add more detail about it.

Finally, the referee emphasizes the simplicity of the model ("toy model", "stylized model") and it might sound as if this is a disadvantage, but the opposite is true. We obtain very strong results despite the simplicity of the model, which illustrates its robustness. The Solow model, for instance, is also a "toy model" and so are many basic building stone models in economics, judging by the standard of the referee.

I hope I've managed to illustrate, that the recommendation of the referees to reject the paper cannot be justified by their reports. There isn't a single criticism in their reports that is valid and significant.

Once again, I do apologize for taking your time, and hope you do understand why I am frustrated with the quality of these referee reports.

Moreover, while I know the norm is to move on when papers are rejected based on incorrect criticism, I think it is time to change the norm, so I'm happy to do my share to that end. We are in a bad equilibrium in which referees could write negative reports based on some incorrect claims without being accountable for it. Their mistakes are very rarely revealed. The authors see the mistakes, but they don't know who the referees are, and most likely just move to the next journal following the rejection, without offering any insight to the editor about the quality of the refereeing. Even if the editor, despite the recommendation of the referee to reject the paper, offers an RR, most authors would not write what they really think for obvious reasons.

Thank you.

Best,

Omer

July 20, 2023
Dear Omer:

I'm writing about your appeal on my decision to turn down your exciting paper on micro-finance. Mistakes do happen; and as I want the *Review of Economic Studies* to stand out as a fair (and speedy) journal, I had a close look again at your paper.
In this regard I liaised with the more theory-inclined reviewer of the initial round, sharing your respone. Besides, I liaised with two additional reviewers, asking them to have a close look. The initial reviewer continues to be negative, arguing that the theory is not up to the *Review* standards and the empirical part is not that relevant. More importantly, the two new reviewers (who are not from any hostile camp), praise the paper but argue that you over-state the paper's novelties and contributions. To my regret, they both recommend <<rejection>>. So in the end I have four referees, all top-notch scholars (with no major stake in the literature to be hostile), who recommend <<reject>>. Given such unanimous recommendation I cannot proceed.

Here is the feedback I got from the two new reviewers, let me label them R3 and R4.

R3 sent a brief report recommdening <<screen reject>>. He/she writes: "*The paper is not suitable for the Review. While the paper makes an interesting point, it is not that novel – there is a relatively large literature showing the importance of missing insurance markets in affecting poor's investments decisions. The paper's contribution is to apply this idea to explain the low take-up of the standard microfinance product which may not be an appropriate way for the poor to invest in "reducing risk". While that is an interesting point, theoretically it is not that novel. Moreover, the evidence from the lab experiment has limitations due to its context, which is not the typical microfinance setting, limiting its external validity. Therefore, I feel the paper would be better suited for a field journal such as JDE*."

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

Thanks again for considering the *Review* for your nice work. [Below you can find the report of R4.]

Elias

A *major* theoretical result in the study of poverty is the finding that, in the absence of fixed costs or other non-convexity, the standard expected u(lity model does not allow people to sit at a corner solu(on of zero investment, even if they are very risk averse, or the investment is very risky. The result is important because it implies that the poor ought make risky investments, and be able to save and invest their way out of poverty, if they have access to a smooth produc(on technology. I will call this RESULT 1.

A related result is that -- in some specific dynamic models -- if a poverty trap is caused by a non-convexity then making a credit op(on available will allow people to escape a poverty trap. I will call this RESULT 2. RESULT 1 is to my mind very important. It is very generally implied by the standard EU model and guides the places that development economists look for policies to alleviate poverty. It also leads to puzzles that can help drive forward research, as is noted in the intro to this paper. The key to RESULT 1, which can be stated in a static model, is the well-known proposi(on that EU decision makers exhibit *local risk neutrality*, and so every risk averter takes at least some of any loIery with a posi(ve expected return. It is important to note that RESULT 1 does *not* necessarily imply that credit will allow a poor person to come out of the poverty trap. In the standard EU model with a fixed cost to take a loIery, the decision maker *does not want to* take up the loIery, and credit need not convexify the production technology. It is only if credit is modelled as providing a specific of insurance that it will remove the corner solution. The authors are silent on what credit does in this paper.

RESULT 2 is much different. It can be stated in a model without risk, does not rely on local risk neutrality and comes about in a seKng where a poor person would like to take an investment choice in principle, but is not willing to pay the short term costs in terms of lower utility to be beIer off in the long run. In this case, credit convexifies the choice set, means that the decision maker does not have to pay as large short term costs, and removes a poverty trap. This result, I believe, only applies in a dynamic model like, for example, Galor and Zeira.

My first comment in this paper is that it is a liIle hard to understand the author's claims. Their theoretical results are about a staic model in which there is no credit, and as I see it their main claim is to have a model in which there are no non-convexities, but local risk neutrality does not hold, so that very risk averse people may sit at a corner solution, choosing not to invest at all, even in a project that has a positive expected utility. However,
the paper's intro links to the literature on micro-credit and the authors claim one of their main achievements is to show that poor people may not take up micro-credit. I simply cannot see the link between their work and this claim. I see credit as being a mechanism to move costs across time, and it is hard for me to see how it can be understood in a purely static model. In any case, as noted above, we already have a very well-known model in which credit does nothing to change whether a very risk averse person takes up a risky lotery with a fixed cost – it is the standard expected utility model. Perhaps I am missing something, but I have tried hard and I don't understand, so either the authors need to clarify their introduction or their main claim should be about RESULT 1. For the purposes of this review, I will assume that they wish to show a seKng in which RESULT 1 is not true.

Given this, I take the main claim of paper to be that, when the standard model is changed a litIle, so that the choice is around the probability of success, rather than around the rewards to success, the local risk neutrality result breaks down, and there are poor (or very risk averse) people do indeed choose to sit at a corner solu(on of

no investment. Here it is worth being clear about what we already know about local risk neutrality. I think
there are two clear cases where the poor stay at a corner. The first is a non-convexity in the production technology, for example a requirement to purchase a minimum amount of a loIery, or a fixed cost to join a lotery. Second, local risk neutrality is a property of the SEU model, and there are many well-known non-EU models that do not display local risk neutrality, at least in some cases. For example, the MEU model of Gilboa and Schmeidler features kinked indifference curves, and so can generate local risk aversion, and could in principle generate a poverty trap. The same can be said of Koszegi and Rabin's well known model of reference dependence. I will call these *behavioral* explanations.

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 explana(on 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. The standard EU model proceeds as follows: there are a set of states of the world, which cannot be altered, a subjective probability distribu(on over these states, and then a mapping from the states of the world to outcomes. It is common to simplify the model by working with a binary set of states of the world, for example a high state and the low state. This non-convexity in the state space does not introduce a poverty trap, and seems to be without loss. From this starting point, the standard way to include the probability of success into this model would be to work with a continuous state space (e.g., (0,1) ranked from bad to good) that cannot be altered by the agent, and to assume that there is a technology that increases the range of states over which a project generates a positive outcome. In doing this, it is probably important to acknowledge that the world is likely continuous, so the production function should be continuous in the state space. This is just the standard model, and it does not to the best of my knowledge deviate from local risk neutrality.

Rather than taking this approach, the authors choose to depart from the standard model in two ways. First, they assume that the state space is defined not by things that happen in the world that are beyond people's control, but rather by the success or not of an investment or project. Second, they choose to use a binary (i.e., non-convex) state space of success or not success. For the record, this approach is used in lots of models, for example the standard moral hazard model in which effort increase the probability of success, but effort is not measured in money and effort costs are convex. Here we still get interior solutions, as was noted by one other referee. It may be without loss of generality to take this short cut, or it may be that the authors have inadvertently smuggled in either a non-convexity, or a behavioral explanation. If it is the later then the paper does not, in my opinion succeed in its task. Unfortunately, I think the answer is that the authors have smuggled in both a non-convexity and behavioral explanation. The behavioral explanation: it may be the case that people see the world as having unchangeable states of the world, or it may be the case that they code the world by discretizing the state space into good and bad states that they think they can manipulate. I do not know, but the laler is, in my opinion not the standard model, but a behavioral model. If this is the model that the authors want to push for, I think the evidence they need is not the evidence they present, but evidence on how people perceive the world. This would, in my opinion be a very different paper about perception.

The non-convexity: it is hard for me to think about what the production function is in the model presented, and whether or not it is convex (what space does it live in?). To help think about it, let me map it back into what I believe would be the standard model: the state space is not altered by decisions and is a con(nuous set (0,1). There are a set of states [s,1) for which the project yields a high outcome, and set of states (0,s) for which the project yields a low outcome. There is a clear discontinuity here in the sense that payoffs jump at the point s. I am prely sure that there are no production functions that look like this in the real world, and I am also prely sure that removing this jump would change the results of the paper, as I note above. Given this, again the only way I think the model makes sense is as a behavior model: even if the real world is continuous at s, people see it as discontinuous. Again, if this I the claim the authors want to make, then they would need to show evidence on how people perceive the world, rather than evidence showing that, if people perceive the world in this way, then we get corner solutions (which is what the current evidence shows).

In short, I am very sympathetic to the goals of this paper. I would love to read a paper that takes the basic insights of the model and tries to show behavioral evidence on bracke(ng of outcomes and hence corner solutions. However, I believe the authors overclaim on what they can show, and that the empirical evidence they present is not relevant to their main claim.