My reply to ADF – which perhaps I've never sent:

Andrew,
Thank you for your email and for reading the model. I am sure you are very busy, and I appreciate it. And I apologize for taking a few more minutes of your time.
If I understand correctly, you agree with me that the one reason for rejecting the paper mentioned by the editor – that the theory isn't general - is not a good one. You also do not disagree with me that there is no other reasonable reason in all three reports by the referees that justifies rejections. The many comments the referees raised do not point to any significant issue with the paper. They are minor, wrong, petty, or just interesting thoughts on how they would have written the paper or designed the experiment. So rather than justifying the rejection based on the referee reports, you propose an alternative reason.
You argue, if I understand you correctly, that one can obtain the same results with a simple model that has a non-convexity in the technology: a minimum scale of investment or an S-shape production function (e.g., a logistic curve). And thus, if I understand correctly, renders our theory rather useless.
You are correct a non-convex technology can lead to the same results. This is well known, and we mention this in the paper giving credit to Banerjee (2000). But your conclusion that this is bad news for our paper is wrong. The achievement of our model is that it doesn't rely on any non-convexities. This is an important achievement because the evidence goes against the alternative you propose, which is indeed very common in many models that explain the persistence of poverty. Kraay and McKenzie (Journal of Economic Perspectives, 2014) survey the evidence and conclude that ***"the evidence is inconsistent with technology-based (fixed costs) poverty traps."*** (We discuss the empirical literature in detail in the paper.)
The claim, to be clear, isn't that there are no businesses that require large investments. It is that there are high-return investment opportunities that do not. And the puzzle is why the poor avoid these opportunities when they have access to credit.
For that reason, Banerjee and Duflo (*Poor Economics*, 2011) propose that the poor do not borrow to invest, not even a small amount they could afford to risk, because they believe, despite the facts, that production functions are S-shaped.
So, it isn't a coincidence that none of the referees so far raised your argument as a claim against our paper. There seems to be a large agreement that explanations based on non-convexities are problematic and a theory that can do without them is valuable.

However, I do not necessarily think you are wrong when you propose that this may explain some of the reactions to our work among development theorists. Because I was so puzzled by the hostility to the paper and the many wrong and petty comments I received on it from referees, I consulted my colleague Sharun Mukand. He thought that the paper is too good, and the development guys rejected it to protect their inferior models that fail to obtain the same results without relying on fixed costs of s-shape production functions. Of course, this is just a guess and I do not know what stands behind the negative reports, but I do know that they are low-quality reports that fail to point out any reasonable reason to reject the paper.

So we are left with a rejection that is based on nothing of substance beyond the fact that three referees think the paper should be rejected but fail to explain why. Is this a clear mistake that justifies reconsidering? My judgment would be positive, but I respect your judgment here could be different.

I agree with you that one cannot ignore the fact that three respected readers of the Journal had similar reactions to the paper. All three, if I understand correctly, had a very clear recommendation to reject the paper. But the question is how much weight should an editor put on a recommendation to reject by a referee who fails to justify the rejection? I think zero is reasonable, but I might even argue that a negative weight is in place, as strong objections to a paper that cannot be justified might indicate that this is perhaps an important paper for the advancement of the science. It goes against the convention, or it is superior to the existing ones.

The bottom line is that we have an intriguing result (as argued by the referees and the editor) that addresses an important question. The paper proposes a novel and simple theory, consistent with experimental data, without relying on any behavioral assumptions or any nonconvexities, and could answer a very important question: why microcredit has failed to reduce poverty. So far this question has no convincing answers in the literature, and the current paper is perhaps not flawless but referees' attempts to find significant flaws in it failed.
Finally, you conclude your letter by stating "I do think there is something here and I would be open to submission of new work on this topic in the future."

I honestly do not understand what you mean here and I would appreciate a clarification. Is this a very weak reject-and-resubmit? What do you have in mind that is new work on the topic, that takes on board the "something that is here"? I agree that there is something here, but I think that using it to explain the failure of microcredit is the best use of this model.

I understand the inefficiency of dealing with complaints about the editorial process. That's why the norm in our profession is to move on to the next journal, rather than waste the time of the editor with feedback and requests to reconsider. But I'm far from convinced. If referees are accountable for their mistakes the outcome could be inefficient. Good research can be blocked. And not just inefficient, but also discriminatory. For me the stakes are low. Of course, I would like to see the outcome of several years of work published, but if the paper is blocked it will not affect my life much. But there are juniors (including two on this paper) and if they produce good research, it shouldn't be blocked for the wrong reasons.

My recent paper on the JPE (2022) is a great example of hostility and poor judgment by referees and editors. Although I believe there is a large agreement that this is a great paper that earned its place in a top-5 journal, this outcome was just luck – one editor who went against the referees.

I tweeted about the process, and as an editor, I think you will find this useful as many did. (It was reposted by many, including senior economists, who added a few words of agreement). I urge you to read it.

https://twitter.com/Omer_Moav/status/1592453480492699649

If you are not on twitter, it is available here, but without the great comments others added.
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