RE: your letter rejecting our paper "Why Not Borrow, Invest, and Escape Poverty?"

Manuscript Number: **DEVEC-D-23-01112**

Dear Aprajit

Thank you for considering the paper. I am happy to hear that you find it intriguing.

I'm writing to you because I wasn't convinced that the rejection was justified, despite the clear negative reports of the referees. I realise that I'm not the first author to think a rejection is unjustified, so I will explain my position in more detail and I thank you for your attention to this letter.

I believe that the non-accountability of referees, who can hide behind their anonymity, is a significant problem in our profession. Some referees might make petty and wrong claims and if authors do not speak up, editors will not discover that the referee did a bad job. Authors know when referee feedback is inappropriate, but they don't know who the referees are. And even if, despite a negative/hostile report, the editor asks for a revision, authors tend to just thank the referee for the "valuable comments" rather than exposing the poor refereeing. I consider it a service to the profession to provide feedback to editors. And in this case, I ask you to reconsider.

I've been in the business of getting negative reports on my work (no more than most others, perhaps even less) for more than 25 years. No previous paper of mine has attracted so many wrong/petty comments as this one. I don't know the reason for this hostility, but I do know that the comments are mainly wrong.

I understand that I will need to convince you that my position is not just based on my own biases, so I will tell you a little about the history of this paper. After the paper was rejected by the ReStud, I sent to the editor who habdeled the paper, Elias Papaioannou, a detailed letter showing that ALL of 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 referees 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 clearly 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 explanaton 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 make the wrong claim that we have behavioral elements and non-convexities. The model is so simple that you don't need to be a theorist to see 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 paper was also rejected by the AER, and I've written to the editor – Rema Hanna - a complaint against one of the referees (I also disagreed with the other referee, but it was more of a matter of judgement, and in any case, I didn't ask Rema to reconsider, I just wanted her to know how bad the job this referee did is).

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

> In the words of the referee:
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> "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 clear in 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.

I offer this background as context for my deep frustration with the editorial process in journals, in particular when it comes to this paper. But it is also important to us that you know that at least one reviewer thought that: the result of our paper is sufficiently important for publication in the Restud; that my complaints about the other comments of the referees are correct, and that in spite of these two favourable endorsements, the same reviewer unfortunately made two mistakes about the theory, which led them to recommend rejecting the paper.

The paper was also desk rejected by two journals: JPE Micro and the EJ. The claim was basically that the paper doesn't ask a question that is interesting to economic readers not specializing in development. I find this view astounding given that the failure of microfinance is a very big issue, and many papers documenting this failure were published in leading journals, including in the top-5 - so it stands to reason that an explanation for why it failed is of interest to a large audience. (I didn't push back in this two cases).

From your letter, I understand that you find the paper intriguing, but you are concerned that it is not clear from the paper to what extent the results depend upon particular functional form assumptions. Our formal model is indeed an example that takes a particular utility function. However, the logic behind our result is general to any risk-averse agent, as illustrated in the introduction:

> "To understand the logic of the U-shaped expected utility, consider a lottery with zero expected return: with probability p the outcome of the lottery is a prize of one dollar, and with the complementary probability 1 − p, it is zero. The cost of generating a probability of

> success p is equal to p dollars. That is, with no investment the probability of winning the prize is zero; with investment of one dollar the probability is one; if the investment is half a dollar p is one half, and so on. In this case, any individual is clearly indifferent between no investment and investing the maximum (p = 1), where with probability one the agent simply receives the dollar invested back. The outcome is certain and identical in both cases. For any investment strictly between the two corners, the expected return is the same as for the two corners, but the realization is risky. Therefore, by definition of risk aversion, any risk-averse individual would strictly prefer the corners over any other investment strictly between zero and one.
>
> To understand why the poor might avoid a high expected return investment that the wealthy would invest in, consider two changes to the lottery. First, the reward in case of a successful outcome is greater than one, so that the expected return of the lottery is positive. Second, the investment is limited to be strictly below one, so that success cannot be guaranteed by high investment. Risk averse agents, who would typically choose between one of the two corners, now face a tradeoff between avoiding risk (by not investing) and enjoying an expected positive return (by investing the maximum possible). If the reward is not too high and risk aversion declines with wealth, there would be a wealth threshold above which individuals invest in the project and below which they don't."

Moreover, I don't think that scientific advancement is based on the generality of a theory. This is just a cultural fluke, perhaps an entry barrier, in the field of economic theory. A good theory is judged in science by its predictions. And here we have a theory about human behavior that we take to the data and find that this is how people really behave. So even if the theory is true only for a specific case, it is a relevant theory because it predicts, to a large extent, how people behave in our experiment. Since we establish that this is the case, our theory becomes a useful one to potentially understand the important puzzle of the failure of microcredit (and more).

The specific relevance to microcredit notwithstanding, the theory is - as explained above - much more general. So clearly I disagree with reviewer #1 who states in comment 1:

> "Furthermore, as the authors suggested, the result in Proposition 1 is not generic, which becomes an issue when one wants to take theoretical predictions to the data."

The paper also includes Appendix B.1. and we refer to it in footnote 5 as follows:

> "Unfortunately, we cannot provide a complete answer to the question of how general our U-shape result is. We prove it holds for CARA utility functions. The intuition for our U-shaped expected utility doesn't depend on the specific form of the agent's preferences. In Appendix B.1 we provide a numerical example that shows that the result extends also to the case of a CRRA function, albeit with an "approximate" rather than an exact U-shape. This suggests that our qualitative results hold more generally, albeit in a weaker form. In any case, the result that agents' investment is discontinuous in risk aversion still holds in the example, and may well hold more generally."

(I wonder if the entire formal model is redundant, and perhaps including it is just a way to follow the norm, but it doesn't contribute to the scientific value of the paper.)

The referee's comment number 2 is also wrong: the experiment does correspond with the model and there is a cost function, but it is discrete, not continuous, for the sake of simplicity.

Comment number 3 of this referee echoes comment number 1.

I also disagree with the referee's last comment - number 4. I think that the experiment we did is a better way and a more practical way to test our model's prediction, but in any case, we did what we did and even if the referee thinks there is a better way, this doesn't point to anything wrong in what we did. This referee's comments do not point to any problem in the paper, but simply a subjective dislike of some of our choices.

I will refrain from a detailed response to the two other referee reports, except to say that while the very long report (it's not clear to me if this is referee #2 or #3) makes many comments that we could address, there are also some that are wrong or petty, and some that raise the bar to unreasonable levels. For instance:

> "Finally, they would need to examine in the field the investment opportunities of households to whom micro-credit is offered, and show that these investment opportunities are indeed of the kind proposed by the authors: changing the probability of success more than returns at a given risk."

We are not claiming that this paper closes the debate, it only contributes to it. The referee's comment illustrates that this is a useful model that could inspire and guide field work.

As for the third report, while I'm happy to read that the referee largely (or somewhat) likes the paper, I suspect the referee is missing the main point of the paper as indicated by the statement:

> "First, this paper has nothing to do with credit or credit constraints. It is merely about investment."

Yes, it is a paper about investment not about credit constraints. That is why it could explain why the poor do not invest even when credit constraints are removed by microcredit!

This misunderstanding also guides the second comment of the referee:

> "Second, to better link the model to problems in development, why not lean into the issue of credit constraints?"

The other comments of the referee propose some interesting thoughts, but they do not identify any flaws in the paper.

If you are willing to let us make the case that the paper should be published by the JDE, I am happy to provide a detailed response to all the comments of the referees.

We have an intriguing result that addresses an important question, and the referees do not point to any crucial mistakes in the paper. Our paper proposes a novel and simple theory, consistent with experimental data, without relying on any behavioral assumptions or any non convexities, that answers a very important question: why microcredit has failed to reduce poverty. So far this question has no convincing answers in the literature (as explained in the paper).

We believe that the readers of the journal deserve the chance to observe our results and judge for themselves how convincing they find it. Of course, if you are willing to go against the referees and seriously consider the paper, we can make the revisions you suggest and will respond to all the comments by the referees.

Many participants in seminars liked the paper a lot, and found it surprising that such a simple model succeeds where so many others fail: it creates a poverty trap that doesn't rely on behavioral elements or non-convexities in the technology, and is robust to removal of credit constraints.

In most cases, the few participants who took exception to the paper were development theorists, many of whom did their best to criticise the paper using wrong arguments. This has also been our experience with referees. I do not know the origins of this hostility from within the field of development.

If you prefer that we move on, that's perfectly fine and we understand. As mentioned above, I think it is important that authors provide feedback to editors when referees are wrong or unfair, and I would feel better if I were confident that our paper is being rejected on the merits, and not because referees are resistant to our model and results. .

I know that the convention is to take responsibility and to write that it is our fault that we were not clear and misled the referees. I am not doing it because if I would write a paper that pre-empts all possible complaints, mistakes, and other random thoughts, it will not be a short paper but a long boring book, and if referees "feel" (in your words, and perhaps correctly) that the paper should be rejected then they will still find a reason.

Sincerely,

Omer Moav