

Referee Report #2: for Guilt Aversion and Redistributive Politics: A Moral Intuitionist Approach

In my first report I said

A brief look at the paper reveals that the author has spent a fairly large amount of time on it and surely he cares about his argument and results. However, I must say that I don't think the paper is publishable in the present form. Also, I believe that the modifications that are necessary to make it publishable would be so many and so deep to mutate the paper into a different object. That's why I'm not asking for a revision, but rather encourage the author to write another version of the paper. What follow are my constructive comments the author may consider in case he decides to follow my advice.

The other referee seemed to have the same opinion, although he/she was more positive and suggested to write a revision. From the reply the author wrote to me and the other referee, I have the impression that he underestimates the amount of work that needs to be done before the paper becomes publishable. So I reiterate: just write another paper.

Consider the following points.

- *First*: to my skepticism about the mathematical structure of the paper and what it really delivered, the author replied that

except for its confused presentation, the model in the current version is really standard in the economic literature and its mathematics not too difficult. Proof of proposition 1 is mostly the resolution of a second order equation! In the revised version I will also try to make the proofs easier to read and more convincing.

Well, this was exactly my point. The paper in its present form looks like a straightforward exercise in social preferences. This view seems to be shared by the other referee, who correctly remarks that the author seems to be confused about guilt-aversion (which requires psychological game theory, and so it's not standard, properly speaking) and inequality aversion (which is basically straightforward game theory). Now the author proposes to write another version in which he removes the reference to the papers on guilt aversion, but the referee insists that his/her "comment is more general than what the author claims: it is not just about removing a reference." This is exactly what I mean when I say that a different paper should be written.

- *Second*: if the model revolves around a second order equation, what is the point of 20+ symbols? Also, I think that the dynamic part of the paper requires more than a "comment" to Figure 3. What seems to be lacking is the *proof* that the equilibrium is stable. (But I may be wrong) It follows that the mathematical part of the paper should include more stuff and at the same time be much simpler. I don't think the author can do this in an afternoon.
- *Third* and most important: if the paper is pretty standard in economics (and I think it is), what is the point of all the quotations from psychology and neuroscience? Just to have a term of comparison, in the Bisin and Verdier paper that the author seems to like, there are 33 references, about one third of the references in the present paper. I repeat myself: most of these references are just redundant. Some of them are misleading: Bisin and Verdier quote Boyd & Richerson and Cavalli Sforza & Feldman on cultural evolution, because they are the authorities in this field, not Bisin. In fact, the author seems to have missed the reason behind my reference to the literature on cultural evolution. I did not intend to encourage him to include more references, but less. The same applies to Kahneman and Tversky's work on economics and psychology. In replying to the other referee the author mentions that Kahneman got a Nobel prize for economics, which proves that his works now belongs to the toolkit of the "mainstream" economist. The author is right on this point, but I think it makes the wrong inference from it. In fact, it would be natural to conclude that economists need not to be persuaded that psychology is relevant for economics, because they already are. And in any case a paper like the one we are discussing is not the right place to persuade the skeptics.

To sum up: I think that a paper with approximately the same content, written in a reader-friendly way and with a more conscious theoretical foundation (keep guilt-aversion separated by inequality aversion, remove the emphasis on voting...), could be read by any economist interested in social preferences, independently from any reference to K&T's *system 1* and *system 2* or any other cutting-hedge research in neuroscience. I would be glad to read such a paper, but the author needs to be conscious of the fact that it will be very different from the version we read. Basically another paper. I do not expect to see the new version before a couple of months.