Referee Report for: "On the source of risk aversion in Indonesia using micro data 2007" (MS 955)

This paper aims to add to our understanding of the determinants of heterogeneity in individual risk preference using the Indonesian Family Life Survey (IFLS). It fits within the literature on the determinants of heterogeneous preferences, which breaks from the traditional view in economics that preferences are stable as in Stigler and Becker (1977). The author finds that some individual socioeconomics characteristics and shocks in the form of natural disasters are significantly correlated with individual risk aversion as measured in the IFLS. The paper's main contribution is the use of a large, almost nationally representative sample from a developing country. I think the author's findings on the relationship between natural disasters, *arisan* participation, household transfers, and risk aversion are the most interesting part about the paper and should be its focal point. The rest of the report addresses the paper's main weaknesses. The more serious weaknesses, in my opinion, are listed first.

I fail to understand how the author combines Game 1 and Game 2 to construct a single risk aversion measure for each individual for the main analysis. Page 4 needs a much more detailed explanation of how this was done, in an appendix if necessary. It is clear from the IFLS instructions that the two games were completely separate. Two other papers that use the same IFLS dataset -- Cameron and Shah (2012) and Ng (2013) -- did not combine both games. Pending a detailed explanation of risk aversion measure construction, the main analysis cannot be fairly assessed. However for the purpose of this report, I will address the paper at face value.

The author uses ARA representation of risk aversion in the main analysis, claiming that its continuous nature improves over a simple rank ordering of risk aversion. However, the author's

ARA risk aversion measure is itself really just a rank ordering, as equation (1) on page 5 seems to show. Now, if the author had included own wealth in the derivation of risk aversion, the risk aversion measure would indeed be continuous, although whether this bestows any great advantage over an ordinal measure is debatable: For one, econometric methods to analyze discrete outcomes are readily available and well developed.[i] Second, the choice of ARA seems to me highly arbitrary. The author should address why he chose ARA over other functional forms such as CRRA or HARA.

On page 2, the author claims that the paper contributes to our understanding of exogeneity of risk preference. However, nothing in the paper substantiates this claim. Wealth being a statistically significant explanatory variable for risk preference does not say anything about direction of causality, as exogeneity would imply. In this regard, the paper is no different from the vast majority of other papers that examine risk preference whether on the left hand side or right hand side, in that causality is not established.

On page 9, this sentence is unclear: *"In order to minimise the potential impact of omitted variable for education, I included abilities in the robustness check."* Later on page 19, the author explains that he is referring to cognitive ability and numerical ability, which are really two measures of intelligence. This should be made clear on page 9. A problem with these measures (which the author acknowledges) is that they only cover respondents between the ages of 15 and 24, a highly selected subset of the full IFLS sample. What's more, there is probably much overlap between numerical/cognitive ability and education attainment. The author might want to use a different measure of intelligence for which fewer observations are lost and which identifies a dimension of intelligence other than education.

On page 3, the preview of results is confusing to follow. A suggestion would be to summarize the main results: state which explanatory variables are significant and in which direction. The author should explain more clearly what is meant by *"a policy that can increase the access for a natural disaster related insurance"*: what exactly is the policy and how exactly is it relevant to their findings?

Table 5 (page 13) would benefit from a pooling test to determine whether the coefficients estimated over the female subsample are equal to the coefficients estimated over the male one.

Finally, I suggest the author show the estimates of all explanatory variables in Table 10 (page 20). At the very least, the main effect of disaster should be shown so the reader can see whether the full effect of disaster is significant.

REFERENCES

Cameron, Lisa A. and Shah, Manisha. 2012. Risk-Taking Behavior in the Wake of Natural Disasters. IZA Discussion Paper No. 6756. Available at SSRN: http://ssrn.com/abstract=2157898

Ng, James. 2013. Essays on the Empirics of Risk and Time Preferences in Indonesia. Dissertation. Available at http://digitallibrary.usc.edu/cdm/ref/collection/p15799coll3/id/280742

Stigler, George J. and Becker, Gary S. 1977. De Gustibus Non Est Disputandum. *American Economic Review* Vol. 67, No. 2, pp. 76-90.

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[i] In fact, in section 3.3, the author re-estimates equation (2) using various other measures of risk aversion, including a simple ordinal one. The robustness checks yield broadly similar results as the main analysis (compare Table 7 to Table 4). ARA is not as important a contribution as the paper makes it out to be.