Response to Reviewer 2

At the outset, I would like to say that I am very grateful to the reviewer for having taken the time and trouble to read and comment on my paper. I hope and trust it will be understood that this is genuinely meant, despite the fact that I also have a genuine difficulty in understanding, or agreeing with, the referee's criticisms. In what follows, I shall make an attempt to respond to the reviewer's comments *in seriatim*.

[1] Review of the Paper entitled "The Focus Axiom and Poverty: On the Co-existence of Precise Language and Ambiguous Meaning in Economic Measurement" This paper is a kind of review paper centering on focus axioms in the measurement of poverty. This is a well written paper. However, it needs to be shortened a lot especially the introduction and concluding observations. It is not an essay; it is a scientific paper and should be written accordingly.

With respect, I must submit I do not quite understand why a 'scientific paper' may not also be an 'essay'! Of course, if the prose is too prolix, I can resort to some abbreviation, but I would urge a consideration of the possibility that there is a case for being a bit expansive in the treatment of the problem since it also addresses questions in philosophy and the use of language.

**[2] I do not know why maximality axiom should be given so much weight in the literature. Consider, for example, the following situations: x = (0,0), and y = (0,0,0,0,0,0,0,0,0,0,0,w), where 0 < z < w and w is very close to z.
The maximality axiom says that P(x:z) > P(y:z). It may be a bit difficult to accept. These types of examples along with the authors view should be given along with other examples.**

But the maximality axiom *is* (usually implicitly) given a lot of weight in the literature, and that, I suppose, is precisely the point of invoking it! By the way, in the example furnished by the referee, the maximality axiom is seen as implying a strict inequality, when actually it entails only a weak inequality. That is, in the example under consideration, $P(\mathbf{x};z) \geq P(\mathbf{y};z)$ by the maximality condition. Arising from this, for any two vectors **x** and **y** belonging to the set **D*** of distributions in which all persons have zero income (and irrespective of the dimensionalities of **x** and **y**) , it will be the case that $P(\mathbf{x};z) \geq P(\mathbf{y};z)$ and $P(\mathbf{y};z) \geq P(\mathbf{x};z)$, whence $P(\mathbf{x};z) = P(\mathbf{y};z)$. Virtually every known real-valued measure of poverty satisfies this property. The referee seems to suggest that poverty associated with the vector (0,0,0,0,0,0,0,0,0,0,0,*w*) ought to be judged to be greater than poverty associated with the vector (0,0); *a fortiori*, one would then require the vector (0,0,0,0,0,0,0,0,0,0,0,0) to display more poverty than the vector (0,0): the intuition underlying this judgment is that the aggregate headcount should trump the headcount ratio. But the aggregate headcount view of poverty is supported by the Population Focus axiom, while the headcount ratio view is supported by the Replication Invariance axiom; and the problem arises precisely because most extant poverty measures satisfy replication invariance while (inconsistently) denying population focus and endorsing income focus. The entire purpose of the somewhat simple-minded impossibility theorem presented in the paper is to highlight this fact.

[3] Modifications of the maximality axiom by inposing n(x) = n(y) seems to be difficult. Because in that case it contradicts income focus directly.

I have tried to look at this proposition from every angle, but I am genuinely unable to say why income focus is condradicted in any way. The weakening of the maximality axiom which is suggested parenthetically in the revised version of my paper is best explained in the words of Manimay Sengupta, who sent this to me in personal communication. I greatly hope that his very clear elaboration resolves any confusion there may exist in the matter. I quote from Professor Sengupta:
"As far as the formal point is concerned, ...a weaker form of Axiom M, restricted to comparisons of poverty values in the same society, i.e. with a given population, could be used to prove the impossibility result. It seems to me that, if we add an axiom that for each given society the poverty measure is normalized (say) in the unit interval, with a value 1 when everyone is poor and has 0 income, that will allow using the weaker version of Axiom M, since this will give P(a)=P(a') even when a' is a larger vector of 0's than a. In the Proposition, P(c) > P(a) by PF and PG, but P(a') $\geq$ P(c) where a' has the same dimension as c, but then since normalization requires P(a)=P(a'), we get the desired contradiction."

[4] In the example 4, I do not see any difference between x, y and u. After all we are measuring poverty and not the potential for relieving poverty through redistribution. I think the author, while interpreting poverty through examples, takes poverty as the opposite of affluence. This is precisely what happened in interpreting in example 2, by saying that y is poverty wise worse than x. But poverty should not be viewed as opposite to that of poverty.

(I take it that the last word in the above comment should be 'affluence', not 'poverty'.) I do not deny that if one is unconditionally committed to the Income Focus Axiom, then it's just a matter of biting the bullet and insisting that , from a poverty point of view, one sees no difference between the vectors **x**, **y** and **u**! But the purpose of the examples in the Appendix is, precisely, to point to the possibility that our moral intuition on the status of income focus could reasonably be a variable function of the specific situation under consideration. I must repeat that any such possibility is naturally ruled out, *ab initio*, in the presence of an unyielding pre-commitment to the rightness of income focus.

[5] The author should revise his paper in the light of the above discussions and resubmit.

I suppose I could, in principle, undertake a revision, but it occurs to me that this would be neither (a) intellectually honest on my part, nor (b) compatible with the spirit of free discussion encouraged by *Economics E-Journal* and its unique experiment of public refereeing and response. Accordingly, I will now leave it to the Editor in charge to decide on the matter – in the hope, though, that my response will be seen to be a frank, but unfailingly respectful (though not deferential!), reaction to the reviewer's comments, the thought and effort behind which are much appreciated.

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February 24, 2012

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