Reviewer's report

Title: A comparative analysis of early child health and development services and outcomes in countries with different redistributive policies

Version: 1 Date: 6 August 2013

Reviewer: David Walsh

Reviewer's report:

Major Compulsory Revisions

1. This is a hugely important topic, and this paper, therefore, potentially has a number of important things to say. For this type of ‘overview’ analysis one would not want issues of data comparability and compatibility to obscure the main messages: to an acceptable degree, the former should be secondary to the latter. That said, however, the complete lack of detail on data sources, and lack of any assessment of such comparability etc. leaves the paper too easily open to criticism from those who might wish disagree with its message (and others besides). Therefore, it is in the best interest of the paper, and its authors, to add a basic level of scientific rigour to back up the points that are being made. At the very least, the paper needs to detail each data source (including all definitions) under each of the topic headings in the Methods section. Where there is evidence of, or questions regarding, issues of compatibility/comparability, these should be highlighted and addressed in those sections. In the Discussion section, an overall assessment of these issues is required.

2. The paper would obviously be stronger if data for more countries could be included in the analysis. With more ‘data points’, the data could be plotted and correlations highlighted in a potentially more convincing way. If data for more countries cannot be obtained, some further discussion about which other countries would be included under each of Navarro’s politico-economic headings, and what is generally known about similar indicators for those places (i.e. whether or not they would be likely to back up the authors’ findings or not), would be extremely useful and would again potentially strengthen the paper.

3. On the same topic, the strengths and weaknesses section should surely consider how appropriate the particular choices of countries are. One could argue, for example, that the U.S.’s population size makes it a difficult comparator; however, if that country’s relevant policies are developed, and indicators most influenced, by federal rather than state government, then that would make the inclusion of country level data more defensible, no matter its size. A similar discussion of the pros & cons of Cuba as a comparator country (e.g. given the fluctuations in inequality experienced over the last few decades) would also help. More generally, an assessment of the suitability of each country to the heading is needed.
4. As a more specific example of points 1 and 3 above, on p10 the authors state that the US has the highest level of inequality with a Gini coefficient of 0.38 (that figure taken, so the references say, from an OECD publication). However, Table 1 states that the highest coefficient is for Cuba with ‘0.5’. So (a) the text is wrong, but (b) this is a good example of the bigger point made earlier: this figure for Cuba has come from an edition of The Economist. Gini coefficients can be derived in different ways depending on the income data they are based on. Is this figure accurate? Is it comparable? More details, and an assessment of comparability, is required.

Overall, this is – as I have said – an important topic. But the paper needs ‘tightened up' to address all the above.

Minor Essential Revisions:

1. Could the authors insure that all terms are introduced and defined in the correct places? For example, the ‘Educa a Tu Hijo’ programme is defined briefly on page 13, but was mentioned (without definition/clarification) earlier in the paper (p. 12). There are a few other, similar, instances of this.

2. It’s generally very clearly written, but the authors should be careful with some language e.g. after emphasising again that this type of comparative analysis ‘cannot claim causation’ (top of p13), it then says further down the same page that ‘generous distributive policies result in higher maternal leave allowance…’.

3. In Table 5 (and earlier in a revamped methods section) the authors need to define and explain the measures of educational achievement. What units are these measured in? Table 5 generally is hard to follow and needs to be made clearer to the reader.

Discretionary Revisions

1. See the first part of point 2 above.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests