Reviewer’s report

Title: Analysis of patient preferences in the drug treatment of Attention-deficit hyperactivity disorder (ADHD): A discrete choice experiment

Version: 3 Date: 8 December 2008

Reviewer: Terry Flynn

Reviewer’s report:

- Major Compulsory Revisions

The article has failed to acknowledge any of the main DCE literature (particularly key methodological issues that pertain to this study) published in recent years. A bibliography of 6 papers suggests little effort.

Page 7: DCEs are not derived from conjoint analysis. Although in North America they are frequently referred to as choice-based conjoint analyses, elsewhere it is widely recognised that DCEs are based in a well-tested psychological theory of decision-making (random utility theory) whilst CAs do not (and include a variety of methods with no robust theory of decision-making, such as rating scales).

The authors seem to believe that DCEs all involve choices from pairs. This is not so. See the book by Louviere, Hensher and Swait (2000) and numerous journal articles by those authors in and outside of the field of health.

Pages 7 and 11: DCE design. The foldover method (to produce ‘right-hand side’ profile from ‘left-hand side’ one) is appropriate. However, it appears from the first paragraph of page 11 (which is confusing) that the authors may not have obtained an Orthogonal Main Effects Plan (OMEP) in 8 to start with, but generated these 8 profiles randomly. If this is the case there is a very real possibility their design is inefficient and unable to estimate all main effects independently of each other. Therefore they should input their entire design into the Street web-based design generator (http://crsu.science.uts.edu.au/choice/choice.html) so that they may quote the efficiency of their design (which will be 100% or close to it, IF they used an OMEP but otherwise might be MUCH lower) and that all main effects can be estimated independently of each other (or not). They should also make it clear to the reader that this design is (only) an OMEP, which assumes all interactions to be zero. There are no references to Street and Burgess in this paper, which is strange, given that the seminal work on design (including the use of foldover techniques) has come from those authors.

The rating-scale part of the study should be de-emphasised as generally applicable method of eliciting attribute importance. In this particular instance it appeared to correlate well with the DCE results (which, being based on a choice-based psychological theory, should be regarded as the gold standard). The authors should refer to the following papers which deal with the issue of
attribute importance/impact and revise their paper accordingly: Lancsar E, Louviere JJ, Flynn TN. Several methods to investigate relative attribute impact in stated preference experiments. Social Science and Medicine, 2007; 64: 1738-1753


- Minor Essential Revisions

Many grammatical errors (missing apostrophes for possessive case for instance)

- Discretionary Revisions

The authors may wish to compare the DCE results with those using methods put forward by Lancsar et al (see above).

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Declaration of competing interests:**

I declare that I have no competing interests