Reviewer's report

Title: The effect of out-of-pocket costs and financial rewards in a discrete choice experiment: an application to lifestyle programs.

Version: 2 Date: 24 March 2014

Reviewer: Eric Finkelstein

Reviewer's report:

The stated goal of this paper was to use a DCE to estimate the effect of out-of-pocket costs and financial rewards as applied to lifestyle programs. Unfortunately, I have little enthusiasm for this paper as written. This is partly because a recent paper (Effects of financial incentives on motivating physical activity among older adults: results from a discrete choice experiment., Farooqui MA, Tan YT, Bilger M, Finkelstein EA. BMC Public Health. 2014 Feb 10;14(1):141. doi: 10.1186/1471-2458-14-141.) addressed a similar research question and the design of that study appears superior. The authors need to review that study and see if they can make a case that their study fills a remaining gap. I am not convinced. I also have other concerns with this manuscript that further limits my enthusiasm. These are, in the order in which they appear:

1. It was not clear to me why two versions were needed. Finkelstein et al included a pay-in and potential rewards in the same DCE. That design allows for understanding the relationship between expected payouts and willingness to pay for an incentive program. It seems a superior design. Why did the authors put the two in separate designs?

2. I do not believe that financial rewards would reduce individuals' stated willingness to join a lifestyle program. I think this result must be due to a design flaw. It just does not pass the sniff test nor is it consistent with other stated or revealed preference data on the subject. We know from countless worksite wellness programs that incentives work to increase participation. This finding is almost surely spurious. The authors should work harder to understand what is driving this result. I wonder how many individuals dominate on a cost or other attribute, were not truly in the market such that they opted out of nearly all programs, got bored with 18 choice tasks and so did not take them seriously, or incurred other biases that would suggest the results are not reflective of actual preferences. I am almost certain that is the case, or at least the authors did not provide enough data to make me think otherwise.

3. I was not convinced that their target population was appropriate as many ‘may not be in the market’. As such their responses may be spurious or open to heuristics that would suggest bias. The authors need to consider this possibility.

4. To have 3 levels for all other attributes but 6 for costs may be a problem. It is generally better to have a similar number of levels for each attribute to avoid biases. It is also common not to have equally spaced levels (as you have) to
avoid recoding, another possible bias.

5. How long did the instrument take to complete. 18 choice tasks is a lot, many more than I tend to use.

6. The 27% response rate is very low. Suggests results may not be reflective of the broader population.

7. Page 12 – line 292 – lack of differences may be due to lack of power to detect differences.

8. I am not convinced by your discussion section. I think a more likely scenario is some form of bias in the estimates.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

none