Reviewer's report

Title: Health worker preferences for community-based health insurance payment mechanisms: a discrete choice experiment

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Reviewer: Dorte Gyrd-Hansen

Reviewer's report:

This is a well-conducted study which focuses on an interesting research question. The manuscript is well written and easy to follow. The results are, as is also documented by the authors, very policy relevant.

Major compulsory revisions:

The participation rate (95%) is exceedingly impressive. The only caveat is that the study does seem to produce results that are not altogether intuitive. This may reflect that some respondents have failed to understand the task. The authors need to address this issue and critically discuss their results. In current manuscript lack a discussion of study weaknesses.

Attributes and attribute levels need to be further clarified. For example on page 13: “Payment levels for the capitation attribute #1 were determined based on ongoing policy debate on whether to subsidise child premiums”. It should be stressed that subsidisation is by fourth party. Also, regarding attributes 3 and 4, are we also here dealing with reimbursement and provision from fourth party? The authors need to provide a more precise description of attributes and attribute levels.

That some respondents may not have understand the attributes and attribute levels is indicated by Table 4, where large proportion of respondents appear to prefer attribute levels that are objectively inferior (according to my understanding). The authors need to discuss these results more explicitly, and reflect on results: For example, if we are dealing with fourth party subsidisation, why is there not 100% support for full subsidisation for relevant attributes in Table 4? For example, why do women tend to be more opposed to child subsidisation? I do not understand the argument (p. 23) that women should “fear that such an increase could in the long term reduce children’s access to health insurance and health care”. Please explain.

Moreover, it is strange that for capitation schedule is C is most popular in Table 4, whereas this result is not replicated in the DCE. This discrepancy should be discussed.

Page 14: “...empirical evidence shows that rarely more than 20 respondents per survey version are needed to estimate reliable models using discrete choice data”. Is this also true if sub-group analyses are performed. The authors should
reflect on whether they have sufficient statistical power to perform their analyses.

The authors should explain how the odd ratios and percentage changes are to be interpreted in Tables 5 and 6. What is the reference?

A more general point: the authors need to – in more depth – discuss the proposition that health workers preferences for payment systems will necessarily align health workers incentives with CBI objectives. While this may sometimes be true, this will not always be the case. Incentive systems such as penalties may be very effective but also very unpopular. In the present case a specific type of mechanism to determine the size of payment (in RBF) may be more effective, but less popular. Clearly in some cases if more risk is transferred to workers there may also be more reason to perform. What I ask of the authors is that they be more nuanced in their discussion.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare I have no competing interests.