Author's response to reviews

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

Authors:

Johanna OP Wanders (j.o.p.wanders@umcutrecht.nl)
Jorien Veldwijk (jorien.veldwijk@rivm.nl)
Ardine de Wit (ardine.de.wit@rivm.nl)
Huberta E Hart (bhart@lrjg.nl)
Paul F van Gils (paul.van.gils@rivm.nl)
Mattijs Lambooij (mattijs.lambooij@rivm.nl)

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Author's response to reviews: see over
Dear Dr Jean Adams,

Thank you for giving us a chance to respond to the comments of one of the reviewers who was not entirely satisfied with the responses in our previous rebuttal regarding our manuscript entitled: “The effect of out-of-pocket costs and financial rewards in a discrete choice experiment: an application to lifestyle programs”.

Our point-by-point response to the comments of the reviewer and you is listed below. All changes that were made in the manuscript are highlighted in green in the revised manuscript.

The manuscript has been resubmitted to your journal and we hope that the responses to the comments will satisfy the reviewer and you.

We look forward to your response.

On behalf of all authors,
With kind regards,

J.O.P. Wanders

University Medical Center Utrecht
Division: Julius Center for Health Sciences and Primary Care
Huispost Str. 6.131, Postbus 85500
3508 GA Utrecht
The Netherlands
E-mail: j.o.p.wanders@umcutrecht.nl
Phone: +31 (0)88 75 688 74
Rebuttal

All changes in the manuscript are highlighted in green.
Reviewer: Eric Finkelstein

Comment # 1:
In response to my first comment, the authors note that the aim of their study is to disentangle possible differential effects of two different types of financial incentives. However, one ‘incentive’ is actually a cost, and not an incentive at all. Whereas the other is an actual incentive. Moreover, the authors claim that they chose to put the two in separate choice tasks because ‘in most situations people will either have to pay for a lifestyle program or will receive a financial incentive, but will not be exposed to both simultaneously’. I do not believe this statement to be true. Even in programs such as Jenny Craig, people pay for the program and receive discounts and other incentives for meeting goals. As 3rd party payers look to offset the cost of incentive programs, I suspect cost sharing is likely to increase. But even if I am wrong on this point, given that the authors are really comparing a pay-in (cost) to an incentive (benefit), as opposed to two incentives, I still do not understand why the two could not have been more appropriately compared in a single design.

Reply authors # 1:
In the first part of comment 1, the reviewer states that a cost is not an incentive. We thought an incentive can be positive, like a financial reward, or negative, like costs. However, using the word ‘incentive’ for both financial rewards and costs might be confusing. Therefore, we replaced all ‘incentives’ referring to costs by the word ‘cost(s)’ or ‘payment’. We replaced the ‘financial incentive’ referring to the DCE attribute by ‘money’.

In response to the second part of comment 1, we would like to refer back to our response given in the previous rebuttal. We wanted to compare the effect of out-of-pocket costs with the effect of financial rewards and we think that the design that we used is the best way to do so, since it rules out contamination of the opposite incentive. In addition, as mentioned in the ‘Background’ part of our manuscript, we describe that according to the ‘loss aversion theory’ we expect that people will respond differently to a (potential)
financial gain than to a (potential) financial loss when participating in a lifestyle intervention. We chose to operationalize financial gain in ‘reward’ and financial loss as ‘out-of-pocket costs’. Since much of the healthcare in The Netherlands was reimbursed until recently, Dutch are likely to experience out-of-pocket costs as a loss. The reviewer is correct in that an incentive scheme can involve buy-in and (partial) compensation after a performance, or other types of sophisticated mixes of payments and rewards. However, in this study we aimed to study a general tendency of people in their reactions to gains or losses when it was tied to health related behavioral change, therefore we chose to present the rewards and costs like we did.

Comment # 2:
In response to reply #2, I remain unconvinced that a financial reward ‘might have the opposite effect’ (the cosine story) and there is not a single empirical study I am aware that is consistent with this result. I appreciate the authors efforts to clarify some of the possible design effects, but I just do not find the result credible and believe it is likely driven by some unobserved bias.

Reply authors # 2:
When looking in the literature, articles can be found that also describe the drawbacks of financial incentives. Alfie Kohn writes for example: “Punishment and rewards are two sides of the same coin. Rewards have a punitive effect because they, like outright punishment, are manipulative. "Do this and you'll get that" is not really very different from "Do this or here's what will happen to you." In the case of incentives, the reward itself may be highly desired; but by making that bonus contingent on certain behaviors, managers manipulate their subordinates, and that experience of being controlled is likely to assume a punitive quality over time.” [1].

In addition, in Gneezy’s article ‘When and Why Incentives (Don’t) Work to Modify Behavior’, it is mentioned that “although people may not view certain incentives as a sign of distrust, they often see explicit control or monitoring in this way and many people experience control as a signal of distrust and react negatively to it.” [2].
In another article of Giné, Karlan, and Zinman a voluntary commitment product was created to help smokers quit smoking. Savings accounts were offered to smokers into which the smokers deposited funds for six months: if the participants quit smoking by the end of this period, their money was returned; otherwise, their money went to charity. The authors found that only 11 percent of smokers were willing to use this commitment device. And although these commitment devices showed some success for those who choose them, the majority of people in these studies and in life do **not choose to use such commitment devices** [3].

Finally, C. Allan looked at reasons why smokers failed to complete and incentive based smoking cessation scheme. In the results section the author writes: ‘Incentives appeared to introduce a potential change/reversal in the felt contractual relationship between service provider and client: the client was now the provider and being paid to quit. This led to an increased **sense of obligation** towards the service, and enhanced feelings of failure, guilt and shame post-relapse, and reluctance to continue engagement or re-engagement.’ [4].

Above mentioned studies show that financial incentives in various situations can be experienced as a way to control people’s behavior and this can cause people not wanting to be involved in financial incentives schemes. Another possibility is that people feel a sense of obligation to the people who are paying for the incentive to complete whatever they were paid for (reciprocity theory). A higher reward can increase this feeling of obligation, resulting in a lower participation rate.

We extended the discussion with the following section and added also the references that were used in this reply, to the manuscript.

**Page 16 – line 382:**

‘**Finally, it is possible that people feel like they are being explicitly controlled or monitored by a financial reward. People may react negatively to this feeling** [1, 2].’

**Comment # 3:**

In response to comment #3, the authors state that healthy patients would be unlikely to participate in a healthy lifestyle program and are therefore of limited interest. I disagree to both parts of this sentence. Healthy patients may be even more likely to participate in
these types of programs in efforts to maintain their health. Moreover, if one wants to understand the cost implications of offering incentives, it is important to include all potential participants in the design, especially if healthy active participants may be obtaining an incentive for doing what they would have done irrespective of the program.

Reply authors # 3:
We agree with the reviewer that healthy people are more likely to participate in lifestyle interventions and trials. However, we were referring to those people who think of themselves as people who are living a healthy life (perceived their lifestyle to be healthy) and do not want advice from health specialists. Those people will not participate in a lifestyle programme, because they think they will not need it. This group of people is different from the group of ‘true’ healthy people who are truly interested in health and lifestyle.
Due to this misunderstanding, we made some changes in the manuscript to clarify what we meant.

Page 17 – line 415:
‘On the other hand, information about other factors like income, social norm, self-efficacy and attitude towards lifestyle programs that could influence the choices of participants, were not available for non-responders. Therefore, the non-response is likely to be selective, in the sense that DM2 patients who are not interested in a lifestyle program were also less likely to participate in this study. It may for instance be that patients who perceive their own lifestyle to be healthy are not interested in lifestyle programs, and therefore did not participate in this study. However, these patients would in real life probably also not participate in an actual lifestyle program, since they think they have a healthy lifestyle, and are therefore of limited interest for this specific study.’

Comment # 4:
With a 27% response rate, the fact that non-responders and responders are similar on observables does not mean the results are unbiased. There is a reason that some respond
and others do not. I do not believe one can generalize beyond responders given the low response rate.

Reply authors # 4:
The response rate in DCE studies are extremely variable which is partly due to the selection process of responders (i.e., whether a preselected group of patients was used, whether questionnaires were given to patients during a doctor’s visit or sent by mail, whether reminders were sent, how much reminders were sent). Looking at some DCE studies involving diabetes patients, response rates are ranging from 11 to 77% [5-9]. Our response rate is comparable with response rates of other DCE questionnaires sent by (e-)mail [5, 6, 9]. We would like to emphasize that having a representative sample answering your questionnaire is more important for the generalizability of your results than having a high response rate. Since our responders and non-responders are comparable regarding important variables (age, sex, BMI and the use of medication), we believe that our responders are at least for an important part comparable to our non-responders and that we therefore have quite a representative sample. Of course one has to be cautious about statements concerning generalizability, however we do not believe that our results cannot be generalized at all beyond the responders.

As we already mentioned in our discussion, since people in The Netherlands are not used to paying high co-payments for health care, it may have been difficult for them to value this lifestyle program as offered within a health care setting. Therefore, our results may be less generalizable to countries with health insurance systems where people are used to high levels of co-payments for health care.

In summary, although we do not think that our results cannot be generalized at all beyond the responders, we do recognize that the generalizability is limited. We therefore added the following to our limitation section in our discussion:

Page 17 – line 401:
‘First, even after sending a reminding letter, the response rate of 26.9% was relatively low, resulting in a study population of 206 DM2 patients. However such a response rate is
comparable with response rates of other DCE questionnaires sent by (e-)mail [5, 6, 9]. In addition, most published DCEs include between the 100 and 300 respondents, so our sample size falls within this range. Nevertheless the relatively low response rate might influence the generalizability of our results.’

Additional references