Author's response to reviews

Title: Waiting for elective general surgery: impact on health related quality of life and psychosocial consequences.

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Author's response to reviews: see over
Dear Editor,
Please find attached a revised version of our manuscript. We thank the reviewers for their valuable comments and have revised the manuscript in light of them. Please find a point by point description of how we addressed the referee’s comments below. We hope the paper is suitable for publication in BMC Public Health.

Yours sincerely,
on behalf of all authors,
Jurriaan Oudhoff

Referee 1
Reviewer: Mats Lundström
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Discretionary Revisions
1. In purpose and conclusion there are no comments about comparing the three diseases. In results however many comments are about statistical significant differences between the three patient groups. If there was no purpose to compare these three diseases and make priority recommendations between the surgical procedures I can’t see any reason for making comments about statistical differences in estimated anxiety or whatever. The postoperative results are based on 20% of the original patient groups and we don’t know if the postoperative questionnaire responders are different for different diseases.

The reason for the comparisons between conditions was that in practise the three groups of patients are seen and treated within/by the same specialty/doctor and the comparison would thus have practical relevance. Although this comparison was a purpose from the start, we agree that this aim was merely implicitly mentioned in the purpose section and understand this might not have been self-evident. We have therefore now added a sentence to the purpose-section in which this sub-goal of comparing the three conditions is explicitly mentioned.

2. In the result section, 2nd paragraph I would recommend to add “Data not shown” in the end (page 9, line 12).

*We have added ‘Data not shown’ as suggested.*

3. Table 5 could use more explanations. How shall Odds ratio be interpreted when one makes a multilevel logistic regression analysis on the association etc. Why p and not 95% CI?

*We thank the reviewer for pointing this out. We have now changed the title and description of Table 5, hoping it would make the table clearer and easier to understand. We did not have a specific rationale for using p-values and agree 95%-CIs give more insight. We therefore now report all Odds Ratios with their 95% CI.*

4. Appendix A shows items in the homemade? questionnaire about negative emotional reactions. Some items seem very much alike and are probably redundant. Have other psychometric tests been performed except Cronbach’s alfa on this questionnaire? The questionnaire seems to overestimate negative emotional reactions. I recommend a comment about being careful when you interpret not fully validated questionnaires.
We indeed focused specifically on negative emotions in this scale and therefore included the word 'negative' explicitly in the name of the scale. The reason for this focus was that anecdotal evidence mainly suggested negative emotions to waiting, which we subsequently addressed in the study. Apparent redundancy of the items might to some extent relate to the issue of translating the scale from Dutch to English as care was applied to address different aspects. Reliability analysis also did not show that taking out one of the items would improve the properties of the scale. We did not perform other psychometric tests on the data though, and we agree that caution should therefore be applied with regard to the results of this scale. We have added a comment to this effect to the limitation section as suggested.

5. The non responders are analysed reasonably good, but as far as I understand only those who were eligible but not participating. One may wonder if there were any differences between those who answered the postoperative questionnaire and those who did not. In pre-op EQ-5D answers in Table 4?

In response to this comment, we have now added an analysis comparing the responses on the EQ-5D of the participants who did return the postoperative questionnaire and those who did not (see additions to methods, results, and discussion sections).

Reviewer 2
Reviewer: Takeo Nakayama

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Major Compulsory Revisions
1. It is necessary to make a brief explanation about the general system of the "waiting list" in Netherlands. This system must be somewhat different from those of the other countries. Is it possible to change the order when health status of a waiting patient worsens?

We have now added the suggested brief description of waiting list management to the introduction and the discussion.

2. Why are the three diseases chosen for this study? Is it impossible to examine the cases of the other diseases?

We have added a sentence to the introduction about why specifically these diseases were chosen. Whereas we focused on these three groups because they represent a large proportion of patients on waiting lists, the inclusion of other patient groups would naturally be technically possible. It would however require a larger study and the involvement of other specialties which was beyond the scope of our study.

3. Approximately 25% of the respondents to the preoperative survey failed to participate in the postoperative one. How about the consequences of this bias?

We have now added an analysis of those who did not take part in the postoperative part of the study. See also our response to point 5 of referee 1.

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Minor Essential Revisions
1. In Table 1, show the initial response rate in the column of percent. For example, it is preferable to show "40.5" instead of "100".
We have changed the table as suggested.

Discretionary Revisions
1. Concerning the three diseases examined, is there any common rule for surgical indication? Is general tendency of surgeons for early operation in the Netherlands positive or discretion? Are general patients active in making a decision of undertaking operation?

We agree that the above questions have some relevance especially with regard to the extrapolation of our results to other countries. However, unfortunately there is not much data available that can shed light on these issues reliably. We have added some sentences to the introduction e.g. with regard to the general decision for surgery, but we have deemed it appropriate to keep these to a minimum in the absence of proper information about how Dutch practise compares to that in other countries with regard to the issues at hand.

2. When are the waiting patients informed of the exact date of operation?

Unfortunately there is no common rule for when this information is supplied. For clarification we have added a sentence about this to the paragraph on 'measures' in our method section.

Referee 3
Reviewer: Alv Dahl

Major Compulsory Revisions
1. The relationship between the current paper and reference #32 should be spelled out. Is this a paper made from a dissertation or a master paper?

The study reported in this paper was part of a larger research project on the consequences and acceptability of waiting times. Reference #32 concerns a project report in Dutch that was written up for the body that funded the research project (Dutch Ministry of Health, Welfare and Sports). In particular it refers to a part of the project that looked at specific physical symptoms of patients on waiting lists. We have made this clearer in the specific reference.

2. The data were collected 5-6 years ago. I miss a comment on the long publication delay.

We presume the comment about the time of data collection refers to our sentence in the method section where we describe that the hospitals were visited in 2001 and 2002, i.e. the time the study started. Because data collection depended on the length of waiting times (which were lengthy in some cases) and the additional 3 month period after surgery, the data collection period stretched out over a period of more than 2 years. Given this we do not consider the publication delay of our paper as excessively long. As in addition there have not been many changes in the waiting list situation since our study finished, we deemed it appropriate to leave out a comment on the publication delay.

3. I miss explanations for the categorisations of various variables.

We thank the reviewer for pointing out this omission. We have now added information regarding the categorisation of responses on the EQ-5D to table 5. We also specified the categorisations of the variables on limitations in work and leisure activities in the text (results section) as the tables regarding the latter variables have been removed (see point 5).
4. I also miss a more in depth explanation for the quite low response rate.

We have now added some more explanation regarding the response rate to the limitations section.

5. Altogether there are 9 tables, some of them quite brief and could they be described simply as text?

We agree that the number of tables is fairly high. Although we preferred tables as they allow a better level of detail with regard to reporting numerical (regression) results, we deemed it possible to cut down the number of tables by 2 by removing Tables 8 and 9 from the paper. To make up for the associated loss of information we have added some sentences describing the results from those tables to the results paragraph. Obviously we are more than happy to supply the full tables to anyone interested, so if the editors prefer we could add a sentence to that effect to the paper.

6. I miss clinical/organizational implications of the results. These patients came from 27 surgical departments, which imply few patients from each department. Do each hospital operate in splendid isolation, or are there some sort of regional or national co-ordination. If not so, should that be a consequence of the findings of the study?

We agree that clinical/organisational factors play an important role in the issue of waiting lists and their consequences. These system factors were however beyond the scope of our study and we subsequently do not have reliable data that would allow for an analysis of these aspects proper. In absence of this data, we preferred not to draw conclusions in this regard as they would be quite speculative and we therefore decided against this.

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Minor Essential Revisions

1. The authors state that long delays represent a threat to the quality of care, both in the Abstract and in The Background. To me there are no associations between these concepts, and the authors should spell these associations out more clearly.

We have changed the specific sentences in abstract and background in order to make the connection between these concepts clearer. With timely care provision being an important aspect of high quality of care, waiting lists would subsequently pose a threat to that quality of care. Essentially, our study tries to look at the extent of (a part of) that threat, and we therefore deemed it relevant to put our study in the context of quality of care.