Author’s response to reviews

Title: Parents of Healthy Children Assign Lower Quality of Life Measure to Scenarios Labeled as Cancer than to Identical Scenarios not Labeled as Cancer

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RESPONSE TO REVIEW

Ajay Risal (Reviewer 1): I suggest the authors to look into the following issues:

1. Title of the paper: Is this title understandable? I think we should try to have an understandable title of the paper.

   We have revised the title to be more clear for readers: “Parents of Healthy Children Assign Lower Quality of Life Measure to Scenarios Labeled as Cancer than to Identical Scenarios not Labeled as Cancer.”

2. Ethical aspect: Not sure why you say ethical approval and consenting process is not applicable. I think these are the important and essential aspect in any research.

   This was not clearly stated on our part. We did indeed receive IRB approval for the study, and obtained informed consent on all patients. We have revised the Declarations Section to improve the clarity of our consent process and IRB review.

Justin Newton Scanlan (Reviewer 2): Thank you for the opportunity to review this interesting paper.

The overall aim of the study is relevant and novel and overall, the paper is very nicely presented.
I have a few suggestions for revisions that I have outlined below.

The concept of "health utility score" is a tricky one to understand, particularly for those who are unfamiliar with it. If it is possible to give a little further explanation of this concept when it is first presented (page 4, line 4 to 32), I think that this would be helpful.

We have added additional clarification to page 4 line 4-10 giving further explanation for the concept of a health utility score.

Related to the point above, the scale from "perfect health" to "quick and painless death" doesn't really seem to be the full spectrum of health outcome from my perspective. Pain was a key feature included in the scenarios, so it seems like "a slow and painful death" may be a more relevant polar opposite to "perfect health." I could not see reference to "quick and painless death" as the "0" anchor point in the other studies using this approach. Perhaps it might be worth adding a little further explanation as to the choice of this anchor point.

This is an excellent point. Our past experience with this methodology indicates that a simple presentation of the “death” end of the spectrum produces more consistent results. Some other investigators use other measures of the “0” anchor, such as death from specific circumstances or merely stating “death”. We have added language in the methods section, page 6 lines 1-5 and citations (# 11 and 12 in references) to page 4 and 6 to provide additional rationale for our choice.

I was a bit confused by in the methods section in terms of the sentence: "The scenarios were prearranged in order of severity along with "perfect health" first and "a quick and painless death" last." Firstly, in a later part of the methods section, it was stated that the "worst scenario" was presented to participants first. Additionally, it is not clear how the anchors of "perfect health" and "a quick and painless death" were presented to participants. I think it would be helpful to clarify this a little more.

We have added additional language to the Methods section to clarify how scenarios were ordered on page 6 lines 1-8. Since the scenarios were stated as “responding to treatment”, “stable on treatment”, and “not responding to treatment”, we did not anticipate any dispute on the severity order and felt having the participant arrange the scenarios was an unnecessary step.

In the analysis section, Page 7, lines 38 to 48, it is reported that "We used univariate statistics to compare each arm ("cancer" or "serious illness") using the Student's t-test for continuous data and the chi-square test for categorical data. We then compared the median health utilities of each scenario and all scenarios in aggregate using the Mann-Whitney test for medians. We chose a non-parametric test to compare the two arms, since health utilities are unlikely to be normally distributed." It is not clear to me why different analysis methods were used to compare health utility scores across the different scenarios (Student's t-test) and then the aggregate (Mann-Whitney). If health utility scores are non-normally distributed, then wouldn't this be the case across all of the scenarios (therefore suggesting Mann-Whitney tests should be used for all analyses)?
We did not clarify this well. We meant to say we used t test for continuous demographic data, meaning when we were comparing whether there were significant differences in parameters such as age and number of children in family (continuous data) between cancer and serious illness subgroups. We used Mann-Whitney when comparing all of the health utility values, as you have rightly pointed out we should for non-parametric data. I’ve included the Mann-Whitney u value and z score within the results section and clarified language in the methods to better explain when each test was used (page 8 line 4).

When reporting results in the "health utilities" section, I think it would be helpful to include the Mann-Whitney / t statistic in addition to the p value).

This is an excellent point, and we have included the Mann-Whitney U test statistic in our results.

I think that the discussion could be revised to make the implications of this study clearer in the context of the existing literature. This study indicates that parents of healthy children are more likely make riskier decisions when faced with a hypothetical situation that their child has cancer that is not responding to treatment as opposed to a similarly serious illness not labelled cancer that is not responding to treatment. However, results from the other scenarios suggest that the influence of the word "cancer" is not as significant when the prognosis is better. However, the results reported from parents of children with ALL suggest that when parents actually do have a child with cancer, they are more informed about the prognosis and therefore seem to make more "reasoned" or "balanced" decisions. So this suggests that "cancer" may have an emotive influence on parents of healthy children. I think the authors are suggesting that this may influence parental decision making in relation to their children undertaking tests or other treatment that may carry with it a risk of cancer (page 10., Line 26 to 34). I think that restructuring the discussion might be helpful in more clearly making this point.

The reviewer is correct that this is our central point. We appreciated your explanation and have added that additional language page 12 lines 5-9 and clarified existing language throughout the discussion to focus on that main point.

On page 11, line 14, the authors state that "Significant differences were seen between health utility scores assigned to different scenarios within each set." No analyses were presented in terms of differences within each participant group in terms of health utility ratings for the different scenarios. Therefore use of the word "significant" should be avoided as this suggests that analyses were completed to analyze these differences (and if they were, this should be presented in the results section).

This is an excellent point. That discussion topic is not central to our study and was not backed by appropriate statistics. We have decided to remove this paragraph from the discussion to keep the focus on the material we have supported by statistical analysis.

Page 12, Line 51 to 53: there is a sentence that reads "We will also mention that false negatives are almost as bad as false positives (Peasgood et al., 2010)." This needs revision, as it is not clear what is being suggested here.
We agree. We have decided to remove this sentence, as it creates more confusion than clarification.

In the declarations section, I think that "not applicable" should be replaced with "The Institutional Review Board at Indiana University School of Medicine approved the study with expedited status." in the "ethics approval and consent to participate" section. It was also mentioned in the paper that participants were "consented", so perhaps a brief description of the consent process could also be included in this section (i.e., if the consent was written or verbal). When I initially read this section prior to fully reading the paper, I was concerned that no ethics review process was undertaken and that formal consent was not granted, which does not appear to be the case.

This was not clearly stated on our part. We did indeed receive IRB approval for the study and obtained informed consent on all patients. We have revised the Declarations Section to improve the clarity of our consent process and IRB review.