Author's response to reviews

Title: Response rates to a mailed survey of a representative sample of cancer patients randomly drawn from the Pennsylvania Cancer Registry: incentive and length effects.

Authors:

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Author's response to reviews: see over
02/28/10

Melissa Norton, M.D.
BMC Medical Research Methodology
c/o BioMed Central Ltd, Floor 6
236 Gray's Inn Road
London, WC1X 8HL, United Kingdom.

Dear Dr. Norton,

Attached is a revised version of the manuscript, entitled: Response rates to a mailed survey of representative cancer patients: incentive and length effects.

Each of the reviewer comments has been specifically addressed and we have carefully edited and reformatted the paper.

The content of this manuscript has not been copyrighted or published previously and is not now under consideration for publication elsewhere. The contents will not be copyrighted, submitted, or published elsewhere while acceptance by this journal is under consideration.

We appreciate your consideration of these revisions (specific comments to reviewers can be found below)

Thank you,

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Responses to reviewer comments

Reviewer 1
Reviewer’s report:
Major Compulsory Revisions
1) The authors state that they were unable to obtain current health status data, this limitation is acknowledged; however, how confident is the team regarding marital status, especially when in this population many individuals are likely to become widows/widowers?
Response: The sample consists of those reported to the cancer registry within the last year. Thus, given that marital status data is only one year old, we were not concerned that this had changed for many people. However, we did ask marital status on the questionnaire, so were able to compare marital status data from the PA cancer registry to self-reported marital status. Ninety-three percent of those who were married according to the PA cancer registry, reported being married one year later when asked on the survey. Five percent had been widowed and less than 1% each had been separated or divorced. We do not have parallel change in marital status data for the non-respondent population.

2) The authors list among their limitations the fact that little differences were observed with incentive amount. While it is speculated that this may be due to the fact that not enough separation existed (which could definitely be true), another reason is that cancer survivors tend to be more adherent, which is probably one of the reasons why the response rate was better than in most other populations. This should be included in the discussion.
Response: This was discussed in the conclusion, page 16, but has now been moved to the discussion about incentive amount.

3) The authors state that they are unable to determine reasons for refusal and list precancerous lesions as a potential reason; however, they should be able to discern refusal by stage 0 disease - this should be done and reported.
Response: We have conducted this analysis and added appropriate language (see page 15).

4) Rates for Hispanics and Whites are listed in the tables - why not other minorities? It would seem that, if anything, African-Americans comprise the largest minority population in PA, thus listing Hispanics seems odd.
Response: We apologize for the confusion, but it is not rates for whites and non-whites that are listed in the table, but the % of respondents in each group that are of white race (vs. non-White) and the % that are of Hispanic ethnicity (vs. non Hispanic). These are two different variables. Language has been added to the table to clarify that point. In table 4, response rates are listed for Whites and non-whites (which includes African Americans).
According to the U.S. Census, African Americans and Hispanics are the two largest minority groups in the state of Pennsylvania (10.8% and 4.9% respectively). Asians comprise only 2% of the population and American Indian/Alaskan Natives less than 1%.

5) Table 3 would benefit from supplying confidence intervals - the p-value is superficial, though if it is listed, it should be placed in the last column.

**Response:** 95% CIs have been added to table 3 and the p-values moved to the last column.

**Minor Essential Revisions:**
1) The authors stated in their letter that they changed the term "mail surveys" to "mailed surveys;" however this was not accomplished either in the abstract or text.

*We apologize for this oversight. It has been corrected.*

2) Suggest that the term "representative" is omitted from the title and the text until it can be defined.

**Response:** We think the title ought to reflect the unusual nature of the sample for such studies of cancer patients. While we thought the existing simpler title with the term representativeness would capture this idea for readers, we will accept the reviewer’s advice and now offer the longer but more precise title: Response rates to a mailed survey among a sample of patients randomly drawn from the Pennsylvania Cancer Registry: incentive and length effects.

3) The text should be reviewed by a scientific writer and the tables reformatted so they are of the quality typically seen for scientific publications.

**Response:** The text has been further reviewed and edited and the tables have been reformatted.

**Reviewer 3**

**Reviewer's report:**
1) The introduction and discussion need to be tightened up and could benefit from further editing and a deeper review of the extant literature (there is a lot more out there dealing with the issues of incentive and length in the health survey literature). The introduction still comes off as a bit meandering. It might help if the research questions were dropped or summarized in a single paragraph. The discussion should weave in the results of existing research better to contextualize the current findings. The discussion of telephone surveys in the intro should mention the problem of cell phone-only households as a weakness of this mode. Finally, I disagree with the other reviewer’s suggestion to morph the incentive amounts into current denominations based on inflation-adjusted rates. It is also atypical and distracting.

**Response:** Research questions have been moved to a single paragraph. We agree that the addition of current denominations is atypical and distracting and have, thus removed them. Language regarding cell-phone only households has been added. We have added some articles related to survey length and response rate. However, while we acknowledge that there are other studies in the literature, we had eliminated those on
physician surveys and populations that were less relevant to the cancer patient population in order to satisfy comments of another reviewer in an earlier round of revisions.

2) While I understand the rationale for retaining both RR2 and RR4, inclusion of both is distracting and unnecessary, from this reviewer’s perspective. I would recommend choosing just one and sticking with it.

**Response:** We respectfully acknowledge the reviewer’s point of view. However, it is our judgment that if we merely presented the response rate that could be applied across analyses (RR2) we’d end up leaving readers with a misleading overall response rate impression. Therefore, as the reviewer has acknowledged our rationale, we have left both response rates in the paper. For most purposes, the RR4 rate is the appropriate one to take away; our use of RR2 was required for one set of internal comparisons.

3) As indicated in my last review, the thrust of the paper should be on the manipulated variables (incentive and questionnaire length) and one non-manipulated variable (cancer type). The other results, interactions and non-significant findings, can be summarized briefly in the text. This will make the contribution more apparent, cut down on tables, and make the whole manuscript tighter.

**Response:** We have eliminated blocks 2 and 3 from Table 3; we agree that those (null) results are adequately summarized in the text.

4) Although referenced indirectly, more formal mention of Groves’ leverage-saliency theory should be included. It is likely explaining the response rate differences across cancer types.

**Response:** A more direct reference to this theory has been included, see page 15.

5) The “willingness to participate in future studies” element is still mentioned in the statistical analysis section although the authors dropped it from the balance of the paper.

**Response:** We apologize for this oversight. It has been deleted.

6) I don’t see a need for the item nonresponse analyses to be weighted/adjusted for differential nonresponse by stage 4 cancer and African American respondents.

**Response:** We apologize if this was unclear. Our references to oversampling and weighting had to do with follow-up data collections and analyses. There was no weighting for non-response in the analyses reported here. The point being made was simply that the fact that response differed by group in the study reported here, led to a modified subsequent sampling strategy. The lower response rates for those with stage 4 cancer and African American respondents prompted us to oversample these two groups in the larger study in order to have sufficient samples of each to allow for analyses.

**Response:** We have eliminated the reference to weighting in the text since this was unclear, and of no relevance to the current study.

**Reviewer 4**

**Discretionary Revisions**
1) The authors provide a good summary of some of the literature on the effects of incentives and questionnaire length on response, but miss two published meta-analyses of these effects. The authors say that they have added these two references but they are not listed in their references (perhaps they did not find them useful to their discussion?).

**Response:** We apologize for this oversight. Both meta-analyses were useful to the discussion and have been added to the text and the reference list (see pages 4 and 5).