Reviewer’s report

Title: Response rates to a mailed survey of representative cancer patients: incentive and length effects.

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Reviewer: Timothy J Beebe

Reviewer’s report:

The authors are commended for embedding an experiment assessing the relative and interactive effects of incentive amount and questionnaire length on response to a mailed survey among cancer patients. Overall, the manuscript has the potential to add to the survey research and cancer epidemiologic literatures. However, a number of general concerns dampen enthusiasm for the paper.

First, the introduction and discussion sections ought to cover the relative merits and pitfalls of the various data collection modes in greater depth. As it stands, the advocacy for the mailed form appears one-sided as there is ample evidence that of the three main modes (mail, telephone, face-to-face), mailed surveys garner the lowest levels of participation. Also, much of the literature cited deals with populations and/or content unrelated to the study focus. For example, the coverage of the questionnaire length literature cites studies focused on adolescent populations, physician surveys or web-only modes. Few focus on general populations, patients (let alone cancer patients), or health surveys. More digging seems warranted.

Second, there are variables that appear in methods section or results that receive no coverage in the introduction. For example, is there no literature covering the potential impact of incentive amount or questionnaire length on willingness to participate in follow-up? How about for cancer stage? If information in provided in the methods or results sections, it ought to receive some attention in the introduction, otherwise it appears out of the blue. On a related note, the most interesting findings are the generally null results relating to the manipulated variables of incentive amount and questionnaire length. All of the other information seems extraneous, especially given that most of the results are statistically non-significant. The paper and the tables in particular would be more straightforward if the focus was only on the manipulations.

Third, is there anyway to pull some information off the cancer registry sampling frame that would support the assessment of nonresponse bias? As the authors indicate, nascent evidence suggests a very weak relationship between response rates and nonresponse bias. Most registries have quite a bit of information on its members that could support the comparison of respondents and nonrespondents. Also, did patient self-reported information vary by the conditions? Addition of such information to the manuscript would heighten its relevance as few studies attend to that information, especially in health surveys.
Without it, the manuscript strikes one as warranting publication as a short note rather than a full research article.

Some specific comments…

• Introduction: More references are needed to support some of the assertions in the first paragraph (e.g., “...average response rates [for web surveys] are not high compared to those for phone surveys.”). Also, some mention of how measurement error related to data collection mode should be offered (see Link et al. regarding web surveys vs. mail and telephone). Finally, casting the hypotheses in table form is atypical and distracting. These should be woven into the text. Also, they appear one-tailed but the statistical tests appear two-tailed.

• Methods: In the description of the mailing procedure, it isn’t clear what, “…return of the mail survey served as evidence of consent…” given that they had not received a questionnaire in the first mailing (unless I’m missing something). Also, a lot of real estate in the paper is devoted to describing the differences between AAPOR RR2 and RR4. This text should be cut down. The authors might also consider testing to see if the results varied by use of the two rates. If things don’t vary, then results (including tables) and discussion should focus on only one for parsimony and clarity.

• Results: The footnoted point at the bottom of p. 10 should be mentioned in the methods section and woven into the results. Formally including item nonresponse as a measure of data quality would increase the potential impact of the paper (see earlier point in general comments). For the tables, make sure that the reference category for each of the variables is clearly defined and noted for easy interpretation.

• Discussion: Reference 40 addressing the issue of average response rates for mailed surveys (55.6%) seems less germane than one done by Asch et al. focusing specifically on health surveys (see Asch, D.A., M.K. Jedrziewski, and N.A. Christakis. 1997. “Response rates to mail surveys published in medical journals.” Journal of Clinical Epidemiology 50: 1129-1136). Also, did the ACS study mentioned in that same paragraph really look at a mixed mode mail and telephone survey (the one where they got a response rate of 34%)? If so, that point should be clearer as it really doesn’t represent an apples-to-apples comparison with the authors’ mail-only investigation. Finally, the cancer-related findings are interesting. But, one wonders whether the observed differences in response across breast, prostate, and colon cancers hold after formally controlling for demographic (non-gender-related) differences in casemix across those groups.

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

**Quality of written English:** Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.