Author's response to reviews

Title: Factors prompting PSA-testing of asymptomatic men in a country with no guidelines: A national survey of general practitioners

Authors:

Frances J Drummond (f.drummond@ncri.ie)
Anne Elie Carsin (e.carsin@ncri.ie)
Linda Sharp (linda.sharp@ncri.ie)
Harry Comber (h.comber@ncri.ie)

Version: 4 Date: 11 December 2008

Author's response to reviews:

Dr. Scott Edmunds,
Senior Editor,
BMC-series Journals,
11th December 2008

Dear Dr. Edmunds

Thank you for considering our manuscript ‘Factors prompting PSA-testing of asymptomatic men in a country with no guidelines: A national survey of general practitioners. Frances J Drummond, Anne Elie Carsin, Linda Sharp and Harry Comber (MS: 1062348605210018) for consideration for publication in BMC Family Practice.

The reviewer’s comments were very useful and insightful. We hope that we have addressed them to your satisfaction (please see below).

I look forward to hearing from you.

Sincerely,

Frances Drummond, PhD

Reviewer’s report

Title: Factors prompting PSA-testing of asymptomatic men in a country with no guidelines: A national survey of general practitioners

Version: 2 Date: 29 October 2008
Reviewer: Melina Gattellari
Reviewer's report:

I believe only one issue remains unresolved. The authors state that they tested for multicollinearity in their modelling but did not specify how. The Hosmer and Lemeshow test referred to in the authors' response I think is a reference to the Goodness of Fit test, which is not diagnostic of multicollinearity. Instead, variance inflation factors and conditioning indices are usually calculated to test for multicollinearity. I have difficulty accepting that that number of sessions worked is not highly correlated with being full-time or part-time. One simple test for working out whether confounding is operating is to stratify the analysis by, say working part-time or full-time and calculating the odds ratios with in each strata for gender. If the odds ratios are different, that would confirm confounding. My main comment is that the authors should more fully describe the techniques they used to assess collinearity in their modelling.

We thank the reviewer for the useful comments.

We would, first of all, we would like to apologise for making slightly confusing comments in our last response. We did not mean to imply that we had “tested” for multi-collinearity using the Hosmer & Lemeshow test which is, of course, a test of goodness of fit. We carried out careful checks for multi-collinearity in our model fitting strategy. These included cross-tabulating candidate variables to assess inter-relationships; inspecting effects on risk estimates and CIs of adding variables to the models; and conducting stratified analyses. We also used the "collin" command in STATA to explore collinearity and found tolerances of acceptable levels. We used the Hosmer & Lemeshow test to confirm that the final models had adequate fit.

As regards the issue of sessions worked and full-time status, although they are related, there is not a complete correspondence between the variables. This is because the full-time status variable relates to whether the individual is a full-time principal GP in the practice. Some GPs are full-time but are not the principal GP in the practice, and these GPs would are coded together with those who work on a part-time basis.

In the previous draft of the paper we included numbers of sessions (GP workload) in Table 5 because one of the other reviewers specifically asked us to show it categorised with 3 levels. In fact it was not statistically significant in the multivariate analysis. In view of this, and its relationship with full-time principal status, we have therefore completely removed it from the final model and the table. We feel this should clarify matters.

As suggested by the reviewer, we have also calculated the variance inflation factors (VIF). None of the variables in the final model had VIF greater than 2.

We have added a description of how multicollinearity was assessed (VIFs and tolerance) towards the end of the statistical analysis section.
The authors have been very thorough in their response to comments from all the referees and the paper is greatly improved. Given that one other referee commented on the approach to the regression analyses and issues of confounding and interactions, I think that the authors should include a sample size estimate. It is important that they should have had an a priori hypothesis and estimated the type of size of effect that they could study with their study sample size. The issue of confounding is very important. The fact that some interactions were non-significant may in fact be a function of the sample size and number of variables included.

We have inserted a statement on the statistical power of the study, based on a priori assumptions about the response rate, frequency of willingness to test asymptomatic men, frequency of explanatory/risk factors, and the variance inflation factor of the other variables in the models.

We accept the point that some interactions may not be significant because of sample size, even although this was a large study. This is the case in almost every study when interactions are explored. It is further exacerbated by the fact that we almost never know, a priori, what form of interaction we might postulate and the statistical model that we use assumes interactions on certain scale. We would note that the sample size calculations - which were based on quite conservative assumptions - suggest that the power of our study to detect modest main effects, after adjusting for other factors in a multivariate model, was high.