Reviewer's report

Title: Risk of breast cancer and family history of other cancers in first-degree relatives in Chinese women: a case control study

Version: 1 Date: 6 June 2014

Reviewer: Catherine Schairer

Reviewer's report:

Review of “Risk of breast cancer and family history of other cancers in first-degree relatives in Chinese women: a case control study.”

1. Is the question posed by the authors well-defined? – Yes, but the study does not appear to be optimally designed to answer the question – no information on age at cancer diagnosis among first degree relatives, no information on number of siblings and number of children.

Major Compulsory Revisions (#s 2-6)

2. Are the methods appropriate and well-described? The methods are not well-described. Therefore, it is not possible to ascertain whether they are adequate. The reader should not have to reference another article (ref 25) in order to understand this article. Very basic information typically required in reporting of such studies is missing. More specifically, a) the types of benign breast disease in the control group are not adequately described – e.g., the number of each type, whether the types are related to increased breast cancer risk; b) the response rate for cases and controls is not described; c) it is not clear whether the information on family history and the risk factors was obtained from medical record review or in person interviews; d) it is not clear how complete the data on family history and risk factors are – was there any missing data?; e) was hormone receptor status missing for any of the cancers (it typically is)?; f) how many cancers were hormone receptor positive and hormone receptor negative?; g) how was positive hormone receptor status determined at the study hospital? h) how were the covariates defined and adjusted for (e.g. childbearing – is this ever/never gave birth to a liveborn child?)? i) why was age used rather than menopausal status to define subgroups – missing data on menopause status?; j) table 3 would benefit from number of cases/controls with family history of each type of cancer; k) family history of breast cancer should also be addressed – can help clarify the usefulness of the control group; l) statistical methods are not rigorously described.

3. Are the data sound? Not sure given the above deficiencies in the description of the methods. Number of children/siblings was not adjusted for. The results are at odds with the Negri study – could be due to chance, and definitely need replication.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? No, as described in point 2 above.
5. Are the discussion and conclusions well balanced and adequately supported by the data? The statement in the discussion that based on these data women with a family history of the above cancers should consider prophylactic surgery is inappropriate. The statement that “our findings...suggest that healthy lifestyles...should be kept in our country” seems irrelevant to the topic. More thoughtful discussion of the disparity of the results with the Negri study would be helpful in the discussion.

6. Are limitations of the work clearly stated? Some weaknesses are not cited, such as issues with accurate cancer reporting in even first degree relatives (e.g. Mai PL article, JNCI 2011) no adjustment for number of first degree relatives. The authors don’t adequately address points made in 2 above.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes.

8. Do the title and abstract accurately convey what has been found? Yes.

Minor Essential Revisions (#9)

9. Is the writing acceptable? The article needs editing for English proficiency. There are also too many instances of “and so on” and awkward phrases (e.g. old women and young women). The language is not scientifically rigorous.

Level of Interest
An article whose findings are important to those with closely related research interests.

Quality of written English
Not suitable for publication unless extensively edited.

Statistical review
Not necessary.

Declaration of competing interests
No competing interests.

Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions.