Reviewer's report

Title: Increased Standardized Incidence Ratio of Breast Cancer in Female Electronics Workers: Is it Related to Trichloroethylene Exposure?

Version: 2 Date: 19 December 2006

Reviewer: Al Ozonoff

Reviewer's report:

General

1) This paper is well-written and presents standardized incidence ratios (SIRs) for a cohort of Taiwanese electrical workers. Results are presented clearly, methods are described, and discussion was generally useful to place the results in context. Because of the lack of detailed individual-level information, the authors are correct to note the limits on inference possible from the data available.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

2) Table 2 shows SIRs for a number of cancer types. Four out of four significant associations are in the direction of protection for this cohort of workers. This seems counterintuitive, and might suggest important demographic differences between the cohort and the general population. This should be addressed.

3) The discussion includes many useful remarks to place the results in context, and to understand the limitations of the data used. The comments on confounding could be more complete. For example, authors provide a convincing argument that female workers are unlikely to have higher exposures of ionizing radiation, smoking, or alcohol consumption, and thus discount those exposures as potential confounders. This addresses temporal confounding (when comparing pre-1974 to post-1974 employees) but does not address the following question: are the female workers here representative (in terms of those and other exposures) of the underlying population, to whom the incidence rates are being compared? Confounding in this setting could potentially bias the estimate of SIR, so while the temporal comparison may avoid problems with confounding, the SIR estimates themselves may lead us to draw incorrect conclusions. Additional discussion of this issue would be helpful (and see item (2) above).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

4) The statistical methods section is brief and should be revised. The description of analysis for the SIR calculations are correct, although they are essentially lifted verbatim from the documentation of the NIOSH software LTAS, and provide little additional information to the reader. The second paragraph of the statistical methods section is unclear. On first reading, it appeared that the t-test was used to compare incidence between the two subcohorts (employed before/after 1974). In fact, the t-test was used to compare those cohorts on three continuous outcomes (age at diagnosis, age at first employment, and length of employment). Rewriting to clarify would be very helpful here.

Discretionary Revisions (which the author can choose to ignore)

5) There does not appear to be any formal statistical test of trend to compare SIRs across subcohorts. Such analyses require an investment in time and energy, but could potentially strengthen the paper considerably.

6) Since the question "Is TCE exposure related to increased SIR among female electrical workers" can not be addressed adequately with the data available, it seems odd to state the question as such in the title of the paper.

What next?: Accept after minor essential revisions
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests