Reviewer’s report

Title: Autism, Circumcision and Analgesia: an Ecologic Link

Version: 1 Date: 17 September 2012

Reviewer: Roger B. Newson

Reviewer’s report:

The authors have presented an ecological study involving a small number of subpopulations (11 or 13). This would represent a low position on the hierarchy of medical evidence, at the best of times, and the authors (to their credit) acknowledge that. However, I have reason to believe that the authors have not even done their statistics correctly. I would advise them to consult at length with a good statistician before any resubmission is attempted, to this journal or to any other journal. As a statistician, I am not really an expert in the biochemistry aspects of this article.

Specific issues are as follows:

Major compulsory revisions:

1. I have reason to believe that the authors have done their statistics incorrectly, by including the full USA subpopulation AND the white, black and Hispanic sub-subpopulations in the same sample of subpopulations used to estimate the ecological correlation coefficients. The authors should include EITHER the full US subpopulation OR the 3 separate US sub-subpopulations, but not both. I find that, when I re-analyse the data of Figure 2 (correlating ASD prevalences and circumcision prevalences), if I erroneously include the US subpopulation AND the 3 sub-subpopulations, then I compute a Pearson correlation of 0.950 on 12 observations (95% CI: 0.826 to 0.986) for "Studies with some of the cohort born post 1995", and a Pearson correlation of 0.690 on 14 observations (95% CI: 0.252 to 0.894) for "Studies with all of the cohort born pre 1995", using the Fisher z-transform to compute confidence limits. These correlations are the same as those reported by the authors, although the confidence limits are different in the first case. However, if I include only the 3 US sub-subpopulations and not the total US subpopulation, then the Pearson correlation is 0.943 on 11 observations (95% CI: 0.789 to 0.985) for "Studies with some of the cohort born post 1995", and 0.577 on 13 observations (95% CI: 0.039 to 0.856) for "Studies with all of the cohort born pre 1995". The corresponding Kendall tau-a estimates (not assuming that the subpopulation prevalences have a bivariate Normal distribution in the meta-population of subpopulations from which these subpopulations were sampled) is 0.709 (95% CI, 0.108 to 0.930) for "Studies with some of the cohort born post 1995", and 0.321 (95% CI, -.058 to 0.618) for "Studies with all of the cohort born pre 1995", using the delta-jackknife method with a Fisher z-transform and a t-distribution (Newson, 2006). So, the authors' conclusions seem mostly correct, but the calculations are not. I have attached my
version of Figure 2a (excluding the total US subpopulation) to clarify my remarks.

Minor essential revisions

1. The notation for a confidence interval should be in the style that I have used in the previous comment, not in the style that the authors have used.

2. In Figure 3, the label ">20 years" should presumably be "<20 years". And the labels ">30 years" and ">30 years" should presumably be "<=30 years" and ">30 years", or something like that. The authors probably need to see a statistician to advise them as to what the correct labels should be.

References


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

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:

I declare that I have no competing interests.