Reviewer's report

Title: Conceptualization of Common LR: A useful Tool for Clinical Diagnostic Reasoning

Version: 4 Date: 21 June 2010

Reviewer: Jean Karl Soler

Reviewer's report:

Dear Authors,

I believe a major re-write of this paper is necessary, as an opinion piece or comment.

This article is a way of looking at likelihood ratios for a collection or category of diseases (say abdominal conditions, or infectious conditions). The idea is good, and should be presented to a large audience. However, the execution is still not presentable, in my opinion.

The way the data are used in this paper does not consider all possible infectious conditions, for example, in a category. So, for the assessment of infectious conditions, the likelihood ratios of some but not all infectious conditions are considered, and the pooled (category) likelihood ratio is necessarily wrong, since it would change were all infectious conditions to be considered.

There is a formal way to approach this problem. The International Classification of Primary Care, for example, allows grouping of categories through its chapters and through special groups of diagnoses (e.g. malignant conditions, infectious conditions, congenital disorders, etc.). This way of conceptualising groupings of possible diagnoses allows the pooled likelihood ratios proposed by this article to be calculated in a more formal and precise manner.

Data are also available for many more conditions than considered in this paper. The Transition Project (of which I am a member) published data on all ICPC episode titles and all reasons for encounter for a large Dutch primary care population.

I am also concerned about the sophistication of the arguments, which in many cases have not been fully considered and followed through. For example, likelihood ratios allow refinement of probabilities, but do not allow one to "rule out" a diagnosis. The diagnosis may also be a symptom diagnosis, for example tiredness may be labelled tiredness (no other more specific diagnosis being appropriate) at the beginning of a new episode of care in a large proportion of cases. A diagnosis is not always made more "objective" if analysed using a Bayesian model. There is a wide body of literature on the accuracy of experienced clinicians making a diagnosis in a field where they have experience and expertise, whilst the LR of many of the symptoms they find useful may not
even have been published.

The statement, in the introduction, that likelihood ratios for UTI have not been published is not correct (a review on "Does this woman have an uncomplicated UTI" was published in the JAMA, for example, is in the reference list of this article).

The appropriate cutoff for a LR (say 2.0 for a LR+ and 0.5 for a LR-) is not considered.

The combination of LRs in one summary measure also violates the conditional independence needed for Bayesian models. Symptoms presenting together are not independent.

In summary, the arguments are interesting but are not mature for publication in the present form.

I hope that this paper may be further developed to include these arguments and present the strengths and weaknesses of this approach, in a more coherent manner, with an appropriate reflection on the published literature.

Sincerely

Jean K Soler

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'