Reviewer's report

Title: Proportional odds ratio model generalizes comparison of diagnostic tests in meta-analysis

Version: 1 Date: 30 September 2004

Reviewer: Dirk Stengel

Reviewer's report:

General

This is a huge paper sketching an interesting approach to meta-analysis of diagnostic studies. The authors tightly follow their previous work, a meta-analysis on D-dimer tests to exclude deep vein thrombosis published in Clinical Chemistry, and a methodological paper on repeated-measures modeling that accounts for the use of multiple tests, published in the Journal of Clinical Epidemiology earlier this year.

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

While all the different components studied and developed by the authors may sum up to a whole, the present manuscript is simply too long. Although BMC Medical Research Methodology has no upper space limit, I like to advise the authors to limit themselves to around 40 pages (equating 20 pages after editing) to improve readability. For example, the last paragraph on p6 and p7, special model considerations on pp 10 to 14, large parts of the worked example and the discussion can be deleted without loss of information.

I feel some major points have been already addressed in the author’s previous articles. After checking the related work, I believe there is redundancy in the tables (not only in this, but also in the other papers).

I am confused about some discrepancies between the present and previous results. For example, referring to Table 4 in the original meta-analysis published in Clin Chem, the Auto Dimertest, the Dimertest Gold EIA, the Nephelotex, and others showed a relative DOR >1 compared to VIDAS, whereas in the current comparison, all of these tests fared significantly worse. Did I make a mistake?

The numerous symbols contained in complex graphs (for example, the funnel, Galbraith and L’Abbe plot) are impossible to distinguish. Figures transporting virtually similar information should be deliberately deleted.

The authors should add the graphical analysis of the POR assumption suggested on p12, which may help readers familiar with proportional hazards to keep on the track.

The notation of the given equations is unconventional, but suits the former provided in the Journal of Clinical Epidemiology. However, if I remember my statistics correctly, the general mixed effects model formula is notated as \( \hat{\beta}_i = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_k x_{ki} + u_i + e_i \), with \( u_i \) and \( e_i \) being normally distributed.

Yet, the assumption of a “general applicability” of the proposed methods is not justified (see title and conclusions). The authors stress the suitability of the POR for therapeutic meta-analyses, which has been demonstrated by Whitehead an co-workers (Whitehead A, Omar RZ, Higgins JP, Saavalny E, Turner RM, Thompson SG. Meta-analysis of ordinal outcomes using individual patient data. Stat Med 2001;20:2243-2260).
Conventional aggregation of a set of diagnostic studies by SROC fails in case of heterogeneity. Although I am not a promoter of the diagnostic odds ratio, I love the idea of handling the differences in LOR without making assumptions about OR0. There are valid points in this paper, that will become clearer if the authors get rid of the ballast.

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

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

Quality of written English: Acceptable

Statistical review: Yes

Declaration of competing interests: none