Reviewer's report

Title: Clinical characteristics and management of patients with tuberculous pericarditis in Africa in the HIV era

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Reviewer: alison elliott

Reviewer's report:

General
1. This paper describes baseline findings from a multi-centre study of tuberculous pericarditis in sub-saharan Africa based on a registry set up by interested colleagues. It is well written and presented.

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

2. Establishing such a registry is an interesting idea and a lot of work, and presumably the authors have some future plans for collaborative studies? In order to justify the publication of baseline, descriptive findings, it might it be helpful if this were explained at the outset.

3. The authors state (methods, first paragraph) that the study was designed to examine the impact of HIV infection on clinical presentation.. etc. It therefore seems unfortunate that about half the participants did not have an HIV test. Rapid HIV tests are not difficult to perform, and for the numbers of participants involved would not cost much. Despite the arguments presented for working within the constraints of the health systems involved, might it be possible to make these available to the study centres in future?

4. Classification of HIV status by clinician’s impression strikes me as problematic. It seems probable that clinicians would be more likely to consider sicker patients, and patients with disseminated tuberculosis, as having “clinical HIV disease”. Indeed, the association between disseminated tuberculosis and HIV is well documented, so one could argue that it would be correct for a well-informed clinician to conclude so. This would mean that the association between worse disease and HIV was “caused”, to some extent, by the clinicians’ perceptions of HIV disease, rather than by HIV. Did the clinicians specify the “clinical grounds” (page 4) on which they based their assessment; or simply give their overall impression? I think it would be appropriate to present the analysis by serological status for the subgroup for which sero-status is known, at least in addition, if not instead of, the current analysis, to show whether the same conclusion would be reached. It seems likely that one would find that the factors of interest, particularly the evidence of myocardial involvement, would appear largely among those with most advanced HIV disease.

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

5. Methods, page 4: As well as defining criteria for haemodynamic instability it would be helpful if the New York Heart Association functional classification were defined. They have a website that could be referred to, also.
6. Discussion, page 6: The first statement may be a little sweeping; such a result would require a registry with less selection bias as to regions included.

7. Discussion, page 6. Is there some text missing after “with the exception of Cameroon”? Or perhaps that statement just needs a little more explanation?

8. Discussion, page 6, end of third paragraph. Is 17 the correct reference? I have only been able to access the abstract, but it appears to be about ECG findings rather than HIV prevalence. Prevalence much higher than 55% has been reported for tuberculous pericarditis in parts of Africa where HIV is more prevalent, including Tanzania as mentioned in the previous sentence, so I wonder about this statement in any case.

Discretionary Revisions (which the author can choose to ignore)

9. I think the idea of further trials of corticosteroids in HIV-positive patients with tuberculous pericarditis should be approached with more caution than implied in the final statement. Using a non-parametric test, Hakim and colleagues (Heart 200; 84:183-188) showed a marked, statistically significant benefit, most apparent early in treatment. In the review quoted (24), a rate ratio has been calculated instead for Hakim’s study, making the result appear less powerful, presumably because this approach assumes that deaths are spread evenly over the follow up period. A non-parametric test may have been more appropriate, given the uneven distribution of deaths in Hakim’s study, and an effect on early survival may be especially valuable in this group as anti-virals become increasingly available, and can be added to anti-tuberculosis treatment.

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: No

Declaration of competing interests:

I declare that I have no competing interests.