

Letter to the editor

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We would like to applaud the authors of the article “The worldwide epidemiology of acute rheumatic fever (ARF) and rheumatic heart disease (RHD)”¹ for highlighting this important disease which continues needlessly to kill people living in poverty while being largely ignored by the developed world.² We would however like to address some key issues.

The authors state that: the evidence of serologic infection is based on one of “a pharyngeal swab culture positive for GABHS, positive rapid GABHS antigen test or rising serologic antibody titers”. Very few countries in highly affected regions of the world utilize routine rapid antigen or microbiological testing due to the cost involved. Furthermore, the delay involved in repeat serological testing before a diagnosis can be made, will result in missed diagnoses and tragic consequences. We recommend therefore that elevated serological markers of previous GABHS infection, together with major and minor criteria, are sufficient for a diagnosis of ARF.

Another contentious issue is echocardiography and the diagnosis of ARF. Although the American Heart Association guidelines do not recommend that evidence of subclinical regurgitation be used as a major or minor criterion, readers should be cognizant of recommendations by the Heart Foundations of Australia and New Zealand developed in response to an extremely high prevalence of RHD in their indigenous populations.³ Admittedly, the use of echocardiography as a major criterion for ARF diagnosis does require expert interpretation, but is strongly considered by experts in affected regions to be pivotal in making this diagnosis, while also increasing the sensitivity of the Jones criteria.⁴

The third controversy stems from the treatment of GAS pharyngitis. Evidence for recommending oral penicillin as first line treatment is scant. Given that the few trials testing the efficacy of penicillin for preventing ARF were confined to intramuscular (as opposed to oral) penicillin,^{5,6} our recommendation is that intramuscular penicillin should be the first choice in keeping with the evidence. This will also result in better concordance.⁷

A final point relates to the use of aspirin. Aspirin is, indeed, particularly effective at rapidly resolving joint manifestations of ARF – so much so, that in areas of high endemicity, indiscriminate use of aspirin without due consideration of the diagnosis of ARF could lead to dramatic underestimation of the incidence of ARF.⁸ It is crucial therefore, to highlight the importance, in high-prevalence areas, of careful consideration of the diagnosis of ARF in any child presenting with arthritis,⁹ and to treat patients

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with paracetamol rather than aspirin or nonsteroidals until a definitive diagnosis is made.

In conclusion, we suggest that this review would have been greatly improved by a more robust systematic review design, incorporating other databases, as well as foreign language and unpublished data, in the search.¹⁰ As it stands, we believe that this review is most likely to have underestimated the problem of RHD, particularly in low- and middle-income countries. Given the availability of cheap and effective prevention, the time has definitely come to accelerate efforts to control this disease.¹¹

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First, we would like to thank the editors of *Clinical Epidemiology* for inviting this review of the published literature.¹ In addition, we would like to thank Zühlke and colleagues for their thoughtful comments. We offer a few points for consideration.

We agree with the use of an elevated serologic antibody titer on initial presentation as an indication of recent streptococcal infection, and support this for the clinical diagnosis of acute rheumatic fever.

While we presented the guidelines from the American Heart Association regarding the diagnostic criteria for acute rheumatic fever, we readily acknowledge variation in practice in other parts of the world.

As discussed and referenced in our review, the diagnostic use and utility of echocardiography in this disease is indeed controversial.¹

Regarding the use of intramuscular penicillin as a first-line treatment for pharyngitis, the increased cost and associated pain from an intramuscular injection must be balanced against the risk of incomplete treatment for acute pharyngitis. We recommend that clinicians adhere to local standards when determining the appropriate treatment course for their patients.

As referenced, this manuscript represents a review of publications from 100 countries, written in 9 languages. The study design, populations, definitions, and treatment options varied widely across time and between venues. While an attempt was made to maximize the contribution of each article, the limitations of meta-analysis in this setting were formidable. We recognize that the published literature to date describes a gross underestimate of the impact of this horrible disease, and applaud the editors of *Clinical Epidemiology* for seeking to shine a light on this heart of darkness.

Reference

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