

Peer Review File

Article information: <https://dx.doi.org/10.21037/lcm-21-18>

Reviewer Comments

The manuscript is linguistically well written and focuses on an interesting topic ie. incidence of GCA in China. However, the extensive referencing to a study which has been retracted seems odd and weakens the content of the current article.

I have the following comments:

Comment 1: The title "Unexplained preponderance of GCA in Lanzhou, China" is misleading. There is much reason to believe that the results from the Lanzhou-study is flawed. Considering that the article has been retracted suggests that even the original authors acknowledge this. Consequently, disseminating results from the Lanzhou study merely serves to spread erroneous information. Alternative title "Narrative review of GCA in China" is better but the manuscript in its entirety, as well as the title, should be stripped of references to the retracted Lanzhou-study.

Reply 1: This has been done. Thank you.

Comment 2: Method: What is a NARRATIVE review....as opposed to a review? Does narrative add anything to the methodology? If not - remove it from the methods description.

Reply 2: As per the direct request of the *Longhua Chinese Medicine Journal*, the editorial staff asked that I use the word “narrative” review and the journal’s accompanying checklist. A narrative review does not have the methodology statistical rigour of a systematic review/meta-analysis.

Comment 3: Abstract conclusion states that "it is difficult to explain the preponderance of GCA in Lanzhou, China". I disagree. There are obvious issues wrt inclusion criteria in the Lanzhou study and as already noted...even the authors of this study has acknowledged its flaws...hence retraction. I suggest that a retracted study should not be referenced to as this may give credit to its erroneous(?) results.

Reply 3: The Lanzhou article is no longer the primary focus of the revised article. However, the Zou reference was not removed to prevent possible “file drawer bias”. It was not clear if the Editor of the radiology journal retracted the article because of radiology concerns or epidemiologic errors. If the Lanzhou study was withdrawn from the radiology journal for radiologic reasons, the epidemiologic aspects may still be viable. As such it seems prudent to at least mention the retracted article, in the case future researchers also find discordant results.

Comment 4: Use of references: Whilst reference 1 (Ing EB et al. Does herpes zoster predispose to GCA) is an interesting comparison of (crude) incidence rates of GCA and zoster in 14 countries, it is not a robust reference wrt incidence of GCA worldwide. For instance: its incidence estimate for GCA in Norway is based on a conference abstract (ref 37) whose conclusion was never followed by a peer-review scientific publication. Similarly, incidence estimate for GCA in Finland is based on a small study spanning a short time period whereas a larger study spanning the same time period ++ shows a markedly different incidence (Rantala). The incidence rates presented by Ing et al in the GCA vs HZ article are crude. There are several comprehensive studies describing incidence of GCA in a more robust manner. Such a reference should be used in the introduction instead of the self-reference.

Reply 4: The “England B, Mikuls T, Xie F, Yang S, Chen L, Curtis J. Herpes zoster and the risk of incident giant cell arteritis. *Arthritis Rheumatol.* 2017; 69(12): p. 2351-2358.” was substituted.

Comment 5: Line 57-58 statement "sunlight, latitude, vit D and infections have been hypothesized to have a role in GCA", should be followed by a statement acknowledging the conflicting results of such studies, perhaps referencing De Smit E et al, *RMD open* 2017.

Reply 5: The De Smit reference was added. The “Carvalho J, Shoenfeld Y. High frequency of vitamin D insufficiency. *European Review for Medical and Pharmacological Sciences.* 2021; 25: p. 574-5.” reference was added.

Comment 6: Reference 8 (Pereira) is used to back up the following statement: "the incidence of GCA is perhaps twenty times less frequent in Asians than Caucasians".

However, limitations/specifics of the Pereira study should be noted, eg. that it is a very small study, n=38, and that it merely compares ethnic distribution of GCA in patients residing in USA, ie not patients actually residing in Asia. This twist may cause selection bias.

Reply 6: The small size of the study was acknowledged in the Introduction section.

Comment 7: Reference 20 is a web site, but there are several scientific publications describing the differences between GCA and TAK. I suggest referencing such publications rather than the website.

Reply 7: The reference “Stamatis P. Giant Cell Arteritis versus Takayasu Arteritis: An Update. *Mediterr J Rheumatol*. 2020; 31(2): p. 174-182.” was added. The website reference from the the Vasculitis Foundation was removed.

Comment 8: Line 74 "Tam estimated an incidence of less than...." should be deleted as the exact same text is repeated in the following sentence (line 75).

Reply 8: This was deleted. Thank you.

Comment 9: Results: Results from the Lanzhou study should be removed. This study has been retracted ie. found to be of insufficient scientific quality.

Reply 9: The Lanzhou article is no longer the primary focus of the revised article. However, the Zou reference was not removed to prevent possible “file drawer bias”. It was not clear if the Editor of the radiology journal retracted the article because of radiology concerns or epidemiologic errors. If the Lanzhou study was withdrawn from the radiology journal for radiologic reasons, the epidemiologic aspects may still be viable. As such it seems prudent to at least mention the retracted article, in the case future researchers also find discordant results. This was explained at the top of the Results section

Comment 10: Discussion: Same comment as above. Lanzhou-results are the focus of almost the entire discussion section. However, the Lanzhou-study has been retracted. In my opinion all results from the Lanzhou-study are there for uninteresting. I suggest a complete rewriting of the discussion section (and to some extent other sections of this review) by focusing on methods and results from the non-retracted articles reporting

incidence estimates for GCA in China.

Reply 10: The article has been completely rewritten. Changes are in purple.