Letter to the editor

Letter to the Editor about the Article: "Performance of different imaging techniques in the diagnosis of head and neck cancer mandibular invasion: A systematic review and meta-analysis"

Dear Editor,

The Oral Oncology journal recently published a paper by Qiao et al., titled, "Performance of different imaging techniques in the diagnosis of head and neck cancer mandibular invasion: A systematic review and meta-analysis" has caught our attention [1]. This systematic review and meta-analysis is unique in its approach, as previous such studies have been limited to single imaging technique, or have not focused on mandibular invasion in Head and Neck Cancers (HNC). Therefore, this study has great value for both, informing the choice of diagnostic tools in the clinical setting, as well as informing research pursuits. For this very reason, we would like to comment on this systematic review and meta-analysis study, so as to further the robustness of this study.

Registration of systematic review and meta-analysis protocol before commencing

There have been systematic review and meta-analysis protocol registration established that help to maintain a level of homogeneity and quality across all meta-analyses and systematic reviews being conducted. PRISMA, Cochrane, JBI and MOOSE are a few examples of such guidelines [2–4]. Therefore, the lack of any mention of the use of such instructions, even though study itself follows the core principles of a systematic review and meta-analysis established in such statements/guidelines, raises the question of whether any such guideline was not used or just not mentioned.

Study search in Chinese BioMedical literature databases

We have also observed that the authors used multiple unique Chinese BioMedical literature databases, such as the China National Knowledge Infrastructure and the Wanfang database, over and above the staple bibliographic databases such as MEDLINE and EMBASE [5]. Therefore, the number of included studies from China (n = 3) being inordinately low, compared to studies from other countries, needs to be explained. We believe that this disconnect between the databases used versus the studies included should be elaborated upon and discussed as a limitation of this study.

RCT or non-RCT?

Furthermore, in the quality assessment of included studies, the authors mention that whether a consecutive or random sample of patients enrolled, was one of the quality assessment criteria. By this, we infer that studies were included regardless of whether they used a Randomised Controlled Trial (RCT) or a non-RCT approach. Considering the significant differences between these approaches and its effects on the overall study, we believe that important to mention this point in the study selection/eligibility criteria, as well as include it as a column in the table describing the characteristics of included studies [6].

CBCT is the best imaging technique, but may not have sufficient evidence

Another interesting point that needs to be considered, is the concluding statement by Qiao et al, where they state that, "Our meta-analysis showed that Cone Beam Computed Tomography (CBCT) had the best diagnostic efficacy among all modalities, with an AUC of 0.9461." followed by, "We also recommend CBCT as a priority choice in the mandibular invasion diagnosis, because offering a number of advantages over other modalities such as lower costs, easier handling and particularly smaller radiation exposure". Though each statement is individually correct, the overall notion of CBCT being the best imaging technique may not have sufficient ground. This is primarily because CBCT cohort has only five studies being included, which is relatively low in comparison to studies included in other cohorts. We agree that CBCT is likely to be the better imaging technique, however, at this point, it is still a hypothesis, with further analysis using a more significant number of studies being required to prove this hypothesis.

Additionally, we would also like to highlight a recent study, focusing on the significance of bone invasion in staging of small-size OSCC, which may be taken into consideration for similar future reviews [7].

We firmly believe that these comments will be carefully considered in future studies as the study is a valuable addition to existing scientific literature, and the suggested remarks will only help elevate the quality

of the study.

Declarations

Ethics approval and consent to participate

Not applicable.

Consent for publication

Not applicable.

Availability of data and materials

Not applicable.

Competing interests

The authors confirmed that they have no competing interests.

Funding

No funding support received to conduct and prepare this study.

Authors' contributions

RJ is primarily conceived of this critical review and led the development of the letter to the editor. Both RJ and CK wrote the first draft of the letter, and critically revised and edited successive drafts of the manuscript. RJ, and CK, read and approved the final version of the manuscript

Acknowledgements

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