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## PEER-REVIEW REPORT

Name of journal: World Journal of Clinical Pediatrics

Manuscript NO: 62778

Title: Prevalence of pulmonary hypertension among children with Down's syndrome: a

systematic review and meta-analysis

Reviewer's code: 00074323 Position: Editorial Board Academic degree: MD, PhD

Professional title: Assistant Professor, Medical Assistant, Senior Scientist

Reviewer's Country/Territory: Italy

Author's Country/Territory: India

Manuscript submission date: 2021-01-19

Reviewer chosen by: Ya-Juan Ma

Reviewer accepted review: 2021-04-01 06:31

Reviewer performed review: 2021-04-03 13:42

**Review time:** 2 Days and 7 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[ ] Grade A: Priority publishing [ Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [ Y] Minor revision [ ] Major revision [ ] Rejection
Re-review	[ ]Yes [Y]No
Peer-reviewer	Peer-Review: [Y] Anonymous [ ] Onymous
statements	Conflicts-of-Interest: [ ] Yes [ Y] No



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## SPECIFIC COMMENTS TO AUTHORS

The topic discussed in the article by Taksande et al is of interest for pediatricians and the analysis of literature is well performed. However the manuscript may not be so attractive in the present form. In the abstract, the authors talked about "increased prevalence" of pulmonary hypertension, whilst the actual prevalence in comparison with a previous one is not known or is indeed one of the outputs of this study. But the studies compared were very heterogeneous to allow making such epidemiologic considerations, and indeed the pre-analysis estimate was obtained from a recent study (ref #39). At best, the Authors should say "increased awareness". Yet this is also to be proven (for example showing the increasing studies on this topic in recent years). There is also some incongruence in the statements of the Authors: while in the abstract they say they aimed to determine the prevalence of PH, in the core tip they state that their objective was to address the lack of consensus on screening of PH. I guess that this inconsistency may be due to the real motivations upon the study. Maybe, the Authors themselves have faced the challenge of diagnosing one or more than one case of PH in their series with Trisomy 21. If so, I would suggest changing the form of the manuscript to "a case report and metanalisis of literature", with a final proposal to widen awareness on the problem and to develop recommendations for the screening of this challenging condition. In the introduction, in the list of factors likely contributing to the increased risk of PH in DS, the Authors might include the higher interferon-related inflammatory profile typical of the disease, which might favor the development of PAH in response to infections, due to increased interferon production (see PMID: 27472900). In the discussion/conclusion, based on the result of the study, the Authors should styress the importance of a multidisciplinary follow-up of children with DS, including cardiologic evaluations even in the absence of congenital cardiopathy. Minor comments: In the



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introduction, the Authors move from the use of Down's syndrome term to that of "trisomy 21" without saying that they are referring to the same condition. DiGeorge syndrome is mistyped as "di George" line 22: please say children with DS, not "children DS" In the discussion, it is not clear how "the prevalence ranges from various countries range from 42% to 54%"", when the overall prevalence is 25.5%. Please, define Persistent Pulmonary Hypertension in the Neonate (PPHN) at the first recurrence of the acronym.