

Obstructive Sleep Apnea in Poorly Controlled Asthmatic Children: Effect of Adenotonsillectomy

Dear editors:

I am writing on behalf the article “OSA in poorly controlled asthmatic children and effects of tonsillectomy” by Keirandish-Gozal et al. in this year’s April issue.¹

This is a very interesting field for research. But, I must confess: I wonder how this study managed to pass peer review!

It is unable to answer its own study hypothesis—it cannot say anything about the prevalence of obstructive sleep apnea (OSA) in poorly controlled asthmatic children due to the lack of a control group, and the comparison to “literature” made is vague. There is no comparison to well controlled asthmatics, e.g., and to the prevalence of obesity in the “literature comparison group.”

The second study hypothesis about effects of tonsillectomy and adenoidectomy (T&A) in children with OSA and with poorly controlled asthma cannot even be answered since there is no data on controls who did *not* undergo T&A!

The study design is highly questionable, with prospective identification of children undergoing a sleep study “as part of clinical routine” without having ethic’s approval prospectively, and then working in retrospect—at least the authors critically mention that in their discussion.

There is no information given on asthma and allergic rhinitis medication apart from stating “no difference,” no information on therapy adherence and on, e.g., presence of house dust mite allergy and its treatment—and a lot of children are reported as having allergic rhinitis!

No time frames are indicated—how long were children followed prior to T&A to make a diagnosis of “poorly controlled asthma,” what is the time gap between T&A and follow up? Authors have also not attempted to make a clear diagnosis of asthma, since only in 45—less than half—children spirometry results were obtained/given, although the age mean is 6.5 and more children should be able to undergo spirometry. No results about bronchodilator response are shown.

There are way too many children lost to follow up, as well in the OSA+ (23 = 40% out of 58!) as in the OSA—group—29%. And, again, most important: children *with* OSA *not* undergoing T&A are lost to follow up!!! And not a word about this in the discussion!

No control was performed for multiple testing—if doing so acc. to Bonferroni’s method for correcting for inflated risk of type 1 errors in multiple comparison procedures, e.g.,² counting 22 tests (at least!) lead to $P = 0.05/22 = 0.0023$ showing that all the results concerning asthma symptoms after T&A are not truly significant and leaving OSA symptoms in OSA + children as only “significant result”—that is redundant.

In the discussion authors talk about “moderate to severe” asthma, while the study dealt with “poorly controlled asthma”—a significant difference!

This publication leaves me wondering whether peer reviewed quality control does not apply any more when there are names of leading scientists in the field among the authors. . .

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