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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Contribution of air-mass transport via the South Asia High to the deep stratosphere in summer" *by* Yu Liu

Anonymous Referee #2

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This manuscript is word to word identical to a previous submission in 2019 that I already reviewed. A new read did not induce me to change my report.

This work is based on assuming that the sole consideration of the distribution of a tracer allows to determine quantitatively the transport properties. It relies first on the miss-interpretation of the Eulerian-mean transport equation of Andrews et al., which is rewritten as equation (5) but then the ve and we in this equation are not the same as that defined on lines 143 and 144 and they are not either the effective transport velocity. The basic assumption that transport is just proportional to the mean gradient is usually wrong in the atmosphere and this is precisely what the Eulerian mean theory was aiming at showing. Recasting it as (5) is loosing the point and the paper is implicitly based on this wrong assumption. Then the most serious flaw is in interpreting equation

(10) with F defined in (9) as a transport equation while it is only an arbitrary identity among discretized quantities. This accident occurred due to the double replacement made in equations (6) and (7). Actually F is defined as a/a+b but the discussion is entirely based on (10) and (9). An other arbitrary identity is equation (13) with the additional wrong interpretation that G should be larger than 1 with no reason. There are other curiosities such as changing the sign of the meridional velocity as if it was not counted as positive along the oriented y axis. The "theory" is then applied to the HCN data from ACE, but no results can be considered as valid when the basis is unjustified. Figure 1 is just a redrawing of Figure 2 of Randel et al. (Science, 2010, doi: 10.1126/science.1182274) without mentioning it. Figure 2 a is also very similar to figure 1a of Randel et al. Therefore, the patterns are not surprisingly consistent with this previous work. It is not even discussed why a method which is derived as a zonal mean should apply to the transport by the SAH which is obviously localized in longitude. My recommendation is that the manuscript should be rejected.

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